TEACHER, RESEARCHER, AND ADMINISTRATOR

Vern O. Knudsen

Interviewed by James V. Mink

VOLUME I

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VOLUME I

INTRODUCTION

When Vern wrote to me "expressing his joy" that I would write some introductory words to these oral tapes, and so "associate my name with his," that joy was mutual. I took it as a sign of our long friendship. Vern was remarkable in his taste for rare old wines, and for his loyalty to old friends. To those who worked with him in science, that attachment was especially apparent. In the case of Leo Delsasso, whom Vern met in 1922 in his first days at what was then the Southern Branch, University of California, and with whom he worked for nearly fifty years until Leo's death, the two became almost as brothers. This ability of Vern easily to make and permanently to keep friends catalyzed his cooperative accomplishments in many diverse types.

I told Vern that my little report would have to be personal and somewhat acoustical, unsupported by historical chapter and verse. I knew how dedicated and effective was the support he enjoyed from his partner Florence in all the important aspects of the world of music and art. But I, as one who had started his career in acoustics, with his first job at Submarine Signal Company, Boston, proposed to stick chiefly to the narrow technical field. As a good friend, Vern agreed to this interpretation of my competence, a sort of plumber working in the vineyards. We have worked or played together for thirty-four years, and in this case it is difficult, and pointless, to distinguish the two categories. In fact, in retrospect, it has almost all become fun. The association started propitiously and with a bang, engineered by Max Mason, an old friend of Vern, and an even longer time friend and great teacher of mine.

The horror of war sometimes is modified a little by the generation of special friendships. At the front, "war buddies" is the term. Even far behind the lines, the urgency of the shared crises may accelerate and weld friendships. This force certainly was operative in our case. We were thrown together as members of a small committee, appointed in the autumn of 1940 by the National Academy of Sciences, to investigate for the Navy the question of the effectiveness of its methods for detection of submarines. The committee consisted of E.H. Colpitts, chairman (formerly vice-president of Bell Telephone Laboratories); William David Coolidge of General Electric Laboratories (who now is in his 101st year, and the oldest living member of the National Academy of Sciences); Vern Knudsen; and myself. I had worked with Max Mason in the previous war, when he developed the Navy's most successful submarine detector, and so was a bridge between the two wars.

By 1940 Vern was already a leading acoustical physicist.

He had published his basic paper showing how the absorption of sound in the atmosphere depended so critically upon the humidity of the air. He was a charter member of the American Acoustical Society and its president in 1933-1935. He had been chairman of the UCLA Department of Physics, and was then six years into his twenty-four-year term as dean of the UCLA Graduate Division.

The field work of this committee began in late December, 1940, at the Navy's Submarine Base, Key West, Florida. Luckily we were able to conclude this investigation with satisfying finality in about a week. Before I left Cambridge, the Dewey-Almy Chemical Company supplied a quantity of rubber weather ballons, of diameter three to four feet when inflated. An inflated balloon, tied to the periscope of a submerged submarine by a long line, marked its position for all to see, except the blindfolded attack team. At the command "Fire depth charges!" the blindfolds were removed, and the distance and angle to the balloon recorded. These distances were always great, sometimes enormous. The report was definitive, demonstrating a clear need for substantial improvements. Within six months, by July, 1941, the Navy with the aid of the National Defense Research Committee (NDRC, later called the OSRD) had initiated at least three new anti-submarine research laboratories--one at New London, one at San Diego and one at Quonsett, Rhode Island--for research in airborne detection methods. Vern was placed in charge of the Point Loma, San Diego, research program. This was basically a project in the propagation of underwater sound.

As the war was drawing to its close, Vern and his colleagues--Harald Sverdrup at La Jolla, Joseph Kaplan, Jacob Bjerknes, and others at UCLA--were imbued with the idea of creating a unified, all-university effort in geophysics. They planned an Institute of Geophysics in which research in meteorology, oceanography, solid earth geophysics would be supported throughout the university, and they obtained President Sproul's approval of such an institute, with headquarters on the Los Angeles campus. Suffice it to say that another chapter in my close working friendship with Vern commenced with scarcely a lapse of a year, when in 1947, I became director of that institute. Vern for many years was a member of my advisory board of the institute. Obviously as an old friend, and as one who had spend many a night caring for the infant UCLA, his advice was sought and found sound in guiding the young institute among the surrounding shoals.

After Vern's so-called retirement, or "jubilation," in Delsasso's Spanish rendition, his new freedom to pursue his main interests in architectural acoustics was an evident joy. His friends then rediscovered the relaxed good cheer of Vern's company at the frequent occasions of lunch at the Faculty Center.

A suitable melding of the beauty of architectural line with the fidelity of sound reception in a concert hall is in each case an individual problem. How does one achieve, for each one of thousands of customers within the walls of the enclosure, essentially a uniform auditory perfection, when the sounds heard may be spoken, or sung, or orchestrated? This is the problem to which Vern devoted his special gifts of imagination, of patience, and hard work. The two hundred concert halls incorporating Vern Knudsen's acoustical designs are his legacy to those who enjoy the beauties of music. Vern has said that the most important musical instrument being played in a concert hall is the hall itself. It has the dangerous power of altering the intended sound. Vern's last work, completed as he was approaching the age of eighty, was the Edwin J. Thomas Performing Arts Hall, Akron, Ohio, which opened on October 10, 1973--"a building of which any world capital could be proud." Actually the Akron complex consists of <u>three</u> quickly interchangeable halls, each designed acoustically for respective audiences of 3,000, 2,400 or 900 people.

In 1968 at the International Congress of Acoustics in Tokyo, the two pairs--Knudsen and Delsasso, and Delsasso and Knudsen--presented two papers on the subject, "Diffusion of Sound by Helium Filled Balloons." This rather academic sounding subject represents in fact an imaginative idea in the measurement of room acoustics. The merits of helium filled balloons as efficient diffusors of sound arise from both the spherical shape, and the high reflection coefficient of the helium-air surface. The balloon scatters incident sound by reflection, and transmitted sound by refraction. In the UCLA reverberation room, twenty helium filled balloons were randomly placed, which by test achieved the desired uniform sound field and "enabled the use of standard reverberation-time formulas for determining practical values of coefficients of sound absorption of most acoustical materials." The final sentence reads: "We are currently investigating other diffusive agents; the results will reveal, we believe, whether the diffusive effects of helium filled balloons are of more than academic interest." In other words, Vern was still looking for other paths for improvement.

On April 13, 1973, Vern Plane and Vern Knudsen presented a paper before the Acoustical Society of America on control of motor vehicle noise by design of building shapes, or of roadside barriers. The subject of "noise pollution" in its deleterious effects upon humans is also one pioneered by Vern. He demonstrated how seriously injured was the hearing of the young who persisted in the generation of the high intensity music of rock bands. Sound was indeed a sacred subject to Vern, and he could and did present the clear sad facts concerning the deafening of ears abused by high sound intensities.

When the energy shortage was finally recognized by the public and politicians, Vern, with typical concern for assisting public understanding and cooperation, made a study in his home of the effect of lower thermostat settings on extending the intervals of the "off" times of his gas furnace, and in reducing the "on" times. These experiments were properly summarized and received publicity by the press. Vern handed me a copy of this report, saying that he was "hoping to save some gas for the benefit of his grandchildren," and asked for comments. I had expected to see him soon at a regular luncheon meeting; but he entered the hospital, and the opportunity did not arrive. But to the last he was applying science in the public interest.

I have tried to indicate Vern's genius for friendship, his enthusiasm, and his skill in applying science to the service of the many. He was indeed a good man, who found the means in science to multiply his influence to the benefit of thousands. He has left many a fine monument, but of his last work, the Thomas Performing Arts Hall, he could say, as a mark of a lifetime of progress, "This is my best work."

Perhaps it is futile to try to understand, or to explain, the abilities of great men. Perhaps the ancient Greeks, living among their human-like gods, understood such mysteries as well--and explained them better than we do. René Dubos writes, "The preclassical and classical Greeks symbolized the hidden aspects of man's nature, in particular the forces that motivate him to perform memorable deeds, by the word _entheos_--a god within. From _entheos_ is derived 'enthusiasm,' one of the most beautiful words in any language."[1]

We have reason to believe that this god within still thrives, and continues to inspire the memorable deeds of men like Vern.

Louis B. Slichter

Los Angeles
June, 1974

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[1]René Dubos, _A God Within_ (New York: Charles Scribner and Sons, 1970), p. 4, citing a quotation of Louis Pasteur in Dubos's _Louis Pasteur--Free Lance of Science_.

In Memoriam

Dr. Vern O. Knudsen

UCLA's Schoenberg Auditorium

Thursday, May 30, 1974

CHARLES E. YOUNG: Florence, Vern Knudsen's family, and ladies and gentlemen: We are gathered here this afternoon to pay loving tribute to one of our company who was associated with UCLA for more than fifty years as a dedicated teacher, a gifted researcher, a great and far-sighted administrator and public servant. If one can use the poetical phrase "movers and shakers" in a way that truly fits any human being, Vern O. Knudsen surely was one of that company. And yet, if the appellation "gentle man" fits anyone, it was Vern Knudsen. He had the courage to join the Department of Physics at UCLA in 1922, when this campus was young and struggling, a little-known institution that was then called the Southern Branch of the University of California. But by the early 1930s, Vern Knudsen was a powerful force in this university and the leading figure in the establishment of graduate study at UCLA--a basic ingredient of a major university. And he was named the first dean of the Graduate Division in 1934. During World War II, he investigated anti-submarine warfare problems for the United States Navy. In the mid-1950s he became vice-chancellor, and in 1959 the regents named him chancellor of UCLA, to serve until his retirement a year later. But he did not retire--at least, in the usual sense. He continued his research in acoustics over a wide range: from medical problems of the ear, to noise pollution in our cities, to serving as president of the Hollywood Bowl Association and cochairman of the California Symphony Society. All the while, he continued to act as an international consultant on the acoustical design of more than five hundred auditoria and lecture halls. I will not attempt, here today, to detail his many contributions to his students, to the university, and to society. But after a musical interlude by the UCLA Madrigal Singers, of which Vern Knudsen was especially fond, five of his close friends will speak of that facet of Vern that they knew best. [Singing follows. Selections: "Hope of My Heart," by John Ward; "Je pleure," by LeJeune; "Bitter for Sweet," by John Chorbajian.]

DAVID S. SAXON: Mr. Chancellor, distinguished guests, friends and family of Vern Knudsen: It is my responsibility to tell you something of Vern Knudsen, the physicist; and by so doing, inevitably, to shed light upon Vern Knudsen, the man. _Inevitably_, I say, because all that Vern did in his distinguished and far-reaching career was deeply rooted in his physics. And his physics was in turn deeply rooted in his character. Vern always emphasized that he was an applied physicist rather than a pure one. Because his life began on the farm, he said, he had early learned that although man does not live by bread alone, without bread, life is dominated by the struggle to survive. He believed that creative intellectual activity, pursued diligently by feeling as well as knowing men, in the students' seats as well as the professors' desks--and I've used his own words--should and must be applied to the everyday needs of the community, to serve and uplift humanity, to serve the good and the aesthetic. (And again, I have used his words.)

He began with a doctoral dissertation on his kind of topic, on hearing difficulties, at the University of Chicago in 1919-1922. In order to do that kind of a problem, he took on not merely his thesis topic, but also his mentor, the formidable, opinionated, and already famous Dr. Robert A. Millikan. (I'm going to interject a parenthetic remark, because that interaction was only a prelude to a later struggle with Millikan over the proposed introduction of graduate education at UCLA. For Millikan, at least at that time, believed that such education ought to be the exclusive prerogative of private institutions such as his own.)

When Vern Knudsen joined the faculty at UCLA in 1922, he continued his pioneering work on hearing, in collaboration with local physicians, and also began his equally pioneering work in architectural acoustics. In the course of these latter investigations, he made his most important contribution to physics.

Shortly after the move from the Vermont campus to Westwood, Vern found that the absorption of sound, measured in the new reverberation room at Kinsey Hall, varied from one day to the next. He soon realized that sound was absorbed in the air much more strongly on dry days--during a Santa Ana, say--than on humid ones (contrary, I think, to what most of us might have guessed about the behavior of sound). In any event, by studying the dependence of this effect on the humidity and on the frequency of the sound, Vern and his students and collaborators were able to demonstrate that the phenomenon involved the fact that oxygen molecules in the air are set into internal vibrations by sound waves of appropriate frequencies, which causes the absorption; but that collisions between oxygen and water molecules, present in the air when the air is humid, are very effective in de-exciting these vibrational states of the oxygen molecules and, thereby, in diminishing the absorption of sound at the frequencies in question.

Now, this work was enormously influential, and, in a sense, all modern physical acoustics begins with it. Its transcendent importance came through the fact that a classical phenomenon--sound--could be used to study microscopic processes at the atomic and molecular level, processes which involved the newly discovered ideas of quantum mechanics. Vern Knudsen has always been recognized as the pioneer worker in this field, and his achievements earned him a number of medals and prizes and made him famous.

His architectural acoustical work continued to flower and remained his dominant life-interest, however. In that connection, as Chancellor Young has said, he assisted in the consulting capacity in the design of more than five hundred buildings. I would like to name a few of the things that he was involved in: the sound stages for MGM, Paramount Pictures, Fox, Universal, Warner Brothers, during the period 1928-1933 when sound first came into the film industry; KNX studios and CBS, all in Hollywood; the University of Washington Music Building, 1946; the General Assembly, United Nations, along with others, in 1948-50; Schoenberg Hall--this hall--at the University of California, Los Angeles, in collaboration with his student, Bob Leonard, and his longtime colleague, Professor Delsasso; the Dorothy Chandler Pavilion at the [Los Angeles] Music Center; the Atlanta Memorial Fine Arts Center, 1968; Zellerbach Hall at the University of California, Berkeley, 1968; University of Akron, Ohio, Performing Arts Hall, 1973; Centre College, Danville, Kentucky, Performing Arts Center, 1973. Those are just a few of the architectural acoustical activities that Vern was involved with.

I think you will note that his activities reached a crescendo after his retirement as chancellor in 1960. And despite a long administrative interlude, which began in earnest twenty-six years earlier, in 1934, when he became the first graduate dean at UCLA, those postretirement years, spent working in Knudsen Hall in close collaboration with his old friend and colleague, Leo P. Delsasso, were golden, happy years for Vern. He was a joy to be around.

Perhaps I will be forgiven if I conclude on a personal note. Vern played a decisive part in my own life, and I want to tell you about it. For it was he who interviewed me in New York in 1946 when, under the prodding of Alfredo Baños, who is the physics department's newest and youngest emeritus professor, the matter of a position at UCLA for me became a matter of serious discussion. I was very young at the time, and I was as green as grass and as nervous as a cat. But Vern set me at ease in thirty seconds. In his gay, jovial, unpretentious, friendly and totally engaging way, he simply said as we met, "I don't know about you, but I want a drink."

VICTOR GOODHILL: Chancellor Young, Florence, members of the family, ladies and gentlemen: Vern Knudsen, educator, physicist, acoustician, also made many contributions to medicine. The doctoral thesis, to which Dr. Saxon referred, in 1922 was entitled, "The Sensisibility of the Ear to Differences of Intensity and Frequency." This was a landmark paper in psycho-acoustics. It was followed by studies in paracusis, diplacusis, and one of the most horrible problems of mankind, tinnitus. He collaborated with otologists in other studies of pathologic problems in hearing. He was a pioneer in the field of medical audiology long before the word "audiology" was even conceived. He brilliantly applied the newly developed technology of vacuum tube circuitry to an audiometer, which could be used in speech tests with selective frequency amplification, and with special switches for tests for malingering. He did fundamental investigations on massing and on bone conduction threshold measurements.

A rigorous scientific approach to basic problems in acoustics was combined with an enormous desire to help humanity. In 1971, in an address on noise pollution, he stated--and I quote--"What we see and hear, how we react to these sensations, largely determines what we are and what we will become."

His ivory tower was the petrous pyramid of the human temporal bone, that extraordinary acoustic device which makes possible all human communication. He was one of the very first pioneers in the study of the effects of noise, not only upon the human ear, but upon the human being as a whole, as well as upon society.

He designed an ear protector which has served to protect millions of servicemen and industrial workers for decades from the hazards of noise exposure.

At UCLA he played a very important role in the founding of the School of Medicine. Retirement from the post of chancellor suddenly did not stop his interest in human hearing. He was keenly interested in the development of otology research in the medical school; and he was instrumental in our starting the very first research audiology program in the Department of Surgery, in temporary quarters in Knudsen Hall.

When Harvey Fletcher spoke at the dedication of Knudsen Hall ten years ago, he reminisced of his very first impression of Vern Knudsen as a child--and I quote Harvey Fletcher's words--"A romping little boy, always so jovial and smiling." Indeed, a jovial happy person, Vern Oliver Knudsen was a source of cheer to all. He loved music and all of the arts passionately. He hated only one thing in the world--noise. This kind and loving humanist was known as the number one noise hater in America--a worthy reputation which he thoroughly enjoyed. He led the acoustics and medical professions in the study of noise as a serious human problem.

Vern Knudsen was a wise and distinguished scientist who knew that wisdom without action is like a tree without fruit. He applied his wisdom very actively, and his trees were indeed very fruitful. I shall cherish the memory of our close personal friendship. The extraordinary legacy of his scientific and humane contributions will be a blessing for all mankind.

GUSTAVE O. ARLT: Florence, Margaret, Morris, and the many friends of Vern Knudsen: I want to say a few words on that aspect of our friend's life that affected me most deeply and which I know the best.

On February 12, 1963, at the anniversary of the celebration of the first doctorate at UCLA, Chancellor Knudsen confessed from this platform that he had never aspired to any position, any post, as he did to the graduate deanship. In fact, he said he coveted it, and he wasn't sure that he had not broken the Tenth Commandment. He called the day in 1934, when he was appointed, one of the happiest in his life. But it was also a happy day for UCLA, because it saw the metamorphosis of a distinguished scientist into an equally distinguished administrator. Under his thoughtful, humane guidance, and with his almost visionary foresight, the Graduate Division flourished in excess of all expectations. The long-range plans that he formulated in 1936, when doctoral candidates could be counted on the fingers of two hands, were just as valid twenty-five years later, when they were counted by computer.

In those early, simple days, Vern Knudsen endeared himself to the graduate students by his warm, personal interest in each one of them. He endeared himself to the faculty by his wise, patient leadership of the Graduate Council and its committees. He endeared himself to the upper administration and the regents by his unswerving loyalty and faith in UCLA and in its future. And he endeared himself particularly to those of his colleagues who, like myself, were privileged to work with him--never under him, but with him. What he built--the Graduate Division--stands as his monument, more lasting, more permanent than the stones in the building that bears his name. His service to graduate education on the national level is rarely mentioned here, but the records of the Association of Graduate Schools and the Association of American Universities attest to his untiring efforts to bring a young, regional university into national focus and into the national limelight. Here was a man of many facets and many talents. But they were all brought into focus by his unvarying dedication to the highest principles of graduate education. Vern Knudsen left his indelible imprint on many aspects of UCLA, but none is deeper and more permanent than his imprint on graduate education. He stands as a model for those who succeeded him and for the generation of graduate deans still to come.

MURPHY: Florence, members of the Knudsen family, and friends of Vern Knudsen, all of us: Occasions like this inevitably involve sadness. Fortunately, though, we can leaven that sadness with happy and indelible memories. We all remember--I certainly remember--the tall, erect, balding, vigorous figure of a man striding, with great energy, across this lovely campus. Who can forget that quick smile, often dissolving into infectious laughter, giving a lift to all? And who, of course, can forget his continuing, abiding and really fierce love for this university and his colleagues at UCLA? These are the kind of memories, and others as well, that sustain us as we come together on such an occasion.

I've been asked to talk a little about Vern and his relationship with the broader world and broader community. My memories of Vern go back to

1960, when I arrived here, a new boy on campus and a new boy in town. Happily, I was wise enough to know that I knew nothing and that I should quickly turn to those who did know something. And it became quite apparent that no one knew more about what I had to do, and what I had to understand, than Vern, who, by that point in his life, had forgotten more about UCLA, the University of California, and this community than most remember. I talked to him often, for, as you know, he was easily, quickly, happily, constructively available to anyone who sought his advice.

We talked about many things, and I got a good deal of advice--all of it, I must say in retrospect, sound. But one bit of advice relates to my mission today, which was his observation that this was a great university, UCLA. It was an important and even critical resource for this community of greater Los Angeles; it was, with increasing vigor, participating and contributing to the growth and the evolution of the Los Angeles community; and it was the responsibility of the one who held the title of "chancellor" to participate in building these bridges--effective, constructive bridges--between campus and community.

Now, you're not surprised that that would have been a strong bit of advice and suggestion on Vern's part. For, as you know, he was no hermit of the laboratory or of the classroom or of the library. He was a man not only of the campus, but of the world as well. And he did contribute mightily to those aspects of this community which, you would understand, he might logically find of interest.

You will recall, those of you, that in the early, struggling, difficult days of the Hollywood Bowl, he was the central figure--along with Mrs. Norman Chandler and others--in seeing to it that this very important community resource survived; and then, contributing of his skill, wisdom and time, in making of it, I think, one of the acoustically perfect kinds of large outdoor amphitheaters in the United States.

He was, as a result of his logical interest in music, president of the Los Angeles Symphony Association. Mrs. Chandler was telling me, just the other day, how critically important it was that he participate in the design of the Dorothy Chandler Pavilion; and how, whenever there was an argument between Mr. Welton Becket, on a matter of aesthetics, and Vern Knudsen, in terms of sound, everybody came down on Vern's side. Vern would have it no other way, as you will remember. And you all know that, by virtue of his interest in architectural acoustics, he was all over the United States--both in and out of universities, in and out of many other kinds of civic auditoriums. He was, I repeat, a man not only of the campus, but of the world.

I, along with some of you here present, had the happy opportunity to take to the regents, not so many years ago, a recommendation that the new physics building be called Knudsen Hall in honor of his long, devoted and enormously effective service to this campus. And we do have Knudsen Hall. And I must say I was enormously happy that Knudsen could have, in fact, worked in Knudsen Hall and, in fact, in a literal sense, knew there was a Knudsen Hall. But I think we should all remember, although they are not all so called, that there are many "Knudsen Halls" across this country, and the country is better for it.

HENRI TEMIANKA: Dear Florence, Margaret, Morris and friends: As we are here on this stage, the four of us, with my three associates from the California Chamber Symphony, so we have sat many an evening in our home, or in Chancellor Knudsen's home, or in the homes of mutual friends, and made music happily, because nothing made Vern happier than the beautiful music of the great classical composers. And these are the joyous memories that will remain with us, as we were together and played these wonderful quartets and trios. Vern believed in the ennobling influence of music. He was, above all, a good human being. I know how great a scientist he was and how brilliant an administrator; but above all else, he was such a wonderfully good man, and this came out in everything he did. He was, with all his heart, with us; in the California Chamber Symphony he honored us by being vice-president for many years. He was always there, not only with advice, but with the most generous deeds. And I think there is no more appropriate way to honor his memory and to express our deep gratitude, love, and admiration to him and to our beloved Florence than to conclude this meeting by playing the slow movement from Beethoven's "Harp" Quartet, opus 74, with my colleagues: Frances Steiner, Polly Sweeney, and Sven Reher. [music follows]

CHARLES E. YOUNG: In my introductory remarks I was, I think--by nature of the event and my role--cast in a somewhat impersonal position in regard to Vern. Before bringing this to a close, however, I would like to pass along one other personal recollection about another "Knudsen Hall" (as Franklin Murphy would put it), a personal relationship with Vern which, again, I think is indicative of the man and the debt that we all owe him.

Some ten or eleven years ago, when a building on campus--which is not called Knudsen Hall, but rather, Pauley Pavilion--was under construction, and I was then vice-chancellor, my predecessor, Franklin Murphy, was out of the country. His predecessor called me one day and said, "Chuck, if Pauley Pavilion is completed with the seats unupholstered, it will be an acoustical nightmare. And I won't have my name associated with it in any way." Knowing that Franklin, were he here, would not want that building to be completed without Vern's stamp of approval, I foolishly went ahead and authorized the expenditure of $150,000 or $200,000 to have those seats upholstered.

We will all be in Knudsen Halls of one kind or another from time to time; but many of us here have [spent] and will continue to spend many hours in Pauley Pavilion, and we should all do so knowing that our comfort and the acoustical attractiveness of the facility has been enhanced by Vern's wisdom.

This has been a symbol and a token of our remembrance of Vern Knudsen. I hope it is also an indication of our rededication to the great goals which we shared with him. I know we will continue to hold him dear and recall the great blessings he has brought to us and to our university. As many of you may know, there was a portrait which was hung in Knudsen Hall and which mysteriously disappeared some time ago. Fortunately, we were able to have the painter of that portrait, Mr. Charles Cross, do another before Vern became ill (shortly before that time), and that painting is in the lobby of Schoenberg Hall, to your right as you leave the auditorium. Thank you very much.

INTERVIEW HISTORY

INTERVIEWER: James V. Mink, University Archivist & Director, UCLA Oral History Program. B.A., M.A., History, UCLA; B.L.S., Librarianship, University of California, Berkeley; Certificate in Archival Administration and Preservation, American University, Washington, D.C.

TIME AND SETTING OF INTERVIEW:

Place: Vern O. Knudsen's office, Knudsen Hall, UCLA.

Dates: December 20, 1966-June 3, 1969.

Time of day, length of sessions, and total number of recording hours: The interviews took place in the midmorning; each session lasted about two hours. A total of approximately thirty-eight hours were recorded. The sessions were not always held at consecutive intervals because of special consulting projects that often took the respondent out of town. Additionally, no interview sessions were held between October 1967 and February 1969, due to other commitments on the part of the interviewer.

Persons present during the interview: Knudsen and Mink, except during two interview sessions (Sections IX, X), when Professor Leo P. Delsasso also participated in the discussion.

CONDUCT OF THE INTERVIEW:

The interview was designed to obtain a full biography of the respondent with special emphasis on his career at UCLA as professor of physics, dean of the Graduate Division, and vice chancellor and chancellor of UCLA. The respondent prepared extensive notes for each session and made frequent reference to his office files. The interviewer prepared questions and introduced documents from the University Archives intended to elicit additional recollections of significant activities.

EDITING:

Editing was begun by the interviewer and completed by Winston Wutkee, Assistant Editor, UCLA Oral History Program. The verbatim transcript was checked against the original tape recordings and edited for punctuation, paragraphing, correct spelling, and verification of proper and place names.

Due to a lapse in record keeping, it cannot be said with certainty that the final manuscript totally reflects the order in which the interview sessions were held. However, within each session the material is presented as recorded. The subject material of pages 1-1058 of the transcript is arranged chronologically but may not represent the actual sequence of the interview sessions. Pages 1059-1301 do represent the correct sequence of sessions.

The respondent reviewed and approved the edited transcript. He made many corrections and also supplied spellings of names that had not been verified previously.

The index was prepared by Joel Gardner, Editor, UCLA Oral History Program, and the introduction was written by Dr. Louis B. Slichter. The other front matter was prepared by the staff. The manuscript was reviewed by Bernard Galm, Senior Editor, UCLA Oral History Program, before being typed in final form.

SUPPORTING DOCUMENTS:

The original tape recordings and edited transcript of the interview are in the University Archives and are available under regulations governing the use of noncurrent records of the University.

Records relating to this interview are located in the office of the UCLA Oral History Program.

PART ONE

December 20, 1966 - October 3, 1967

[As there is confusion concerning the actual dates of the interview sessions of Part One, only the inclusive dates have been indicated. Each section represents one side of a tape recording.]

SECTION I

KNUDSEN: My father, Andrew Knudsen, was born near Bergen, Norway, in 1854. My mother, Chesty Sward, was born in Sweden--Malmo, Sweden, in 1855. My father, at the age of nine, migrated to the United States with his family and then on to Utah. The Mormon trail was the route that he took at that time, and it began at the Missouri River and continued to Salt Lake City. This was one of the Handcart Companies in 1863. This was during the Civil War. On the trail, he helped his parents with the handcart. His little sister, less than two years old, eighteen months or so, died as they were approaching Echo Canyon, which you know is really pretty near Wyoming, so it was near the end of the trail. He assisted his father in the burial of his little sister there, and of course it was an unmarked grave. Later in life my father made several trips back there in hopes that they could locate the body of his sister and bring her to a decent burial. These things meant a great deal, as you know, to religious people, who believed in the literal resurrection, and so on. They wanted a respectable burial for even a child that had died along the trail that way.

My mother came to Utah at age seventeen. That was somewhat later. Both parents migrated to Utah because they were converts to Mormonism, and there were economic matters, as you know, as well as others that prompted them to come.

MINK: Do you know when they were converted to Mormonism and how?

KNUDSEN; Well, you see, they were both juveniles, and their parents were converted to Mormonism, so my father came with his family. He had two brothers and maybe three sisters, and the entire family had accepted the Mormon faith in their hometown. It's a little place outside of Bergen, between Bergen and Oslo, Loyten Station. This name is significant today among Norwegians because it's where they make aquavit. The potatoes there are supposed to be unusually good for making this beverage, and the potatoes are allowed to just "peek" out of the ground a little; so the Loyten aquavit differs from the Danish, which we know in this country. I've asked for Loyten aquavit many places in the United States, and you don't find it. But whenever I go to Norway, I bring back two or three bottles of the Loyten aquavit, and when my Norwegian friends come to the home, I bring out this aquavit that was produced in the birthplace of my father. My father never knew that I would think of his hometown as the place where they produce an alcoholic beverage, because alcoholic beverages were pretty much taboo in my family as a part of our religious training.

MINK: Did the Mormon Church send missionaries to Norway?

KNUDSEN: Oh, yes.

MINK: Where did they come from?

KNUDSEN: Well, they were converts to the church, and, as you know, the missionaries are lay people. My father later returned to Norway as a Mormon missionary, and my three elder brothers all served as Mormon missionaries in Norway. I was the renegade of the group. I didn't want to go abroad. I wanted to go someplace where I could do some studying, and I fulfilled a mission later in life.

I was the seventh child in the family. I had three brothers, older than I am, and three sisters, of course, all older than I am. My mission was somewhat delayed. In fact, I did not go until I had completed college. I wanted to go to Chicago, or to New York, or someplace where there was a university. I really intended to steal a little time from the missionary duties and try to improve my mind at the same time. As a matter of fact, I was allowed the privilege of going to the University of Chicago during the last six months I was in the mission field in Chicago. We'll come to the mission experience a little later.

Well, my parents came to Utah, and the Scandinavian immigrants of the Mormon faith were in general encouraged to go either to Logan, Utah, in the north, or south to Provo, or even further south into Sanpete County. But at the time of my father's arrival, Provo was just being settled. I'm not sure whether father received a call from the first church presidency; it was not unusual for these immigrants. He said, "Well, you're going to Provo," or "You're going to Logan," or "You're going to Ogden," or you're going some other place in the state. This is the way the settlement was performed. The church was everything then and there. It was the government really, as you know. This was the territory of Deseret, and of course it was just Brigham Young and his associates that governed before it became a territory of the United States. I don't even remember the date of the territory, but it was several years after Brigham Young arrived there.

So my father's family was directed to go to Provo and to help settle that community. This was certainly about 1863 or 1864 that he was in Salt Lake City, a relatively short time. So in 1863 or 1864, he, with his family, his parents and his brothers and sisters, came to Provo, which was then an Indian fort. Provo had been named, but the fort where the Indians lived was simply called Fort Field. It was down where the farm land is. The Utah County, where Provo is the capital, or what you call county seat, was a farming community, strictly a farming community. Farming was everything, as you may know, at that time. It was possible to irrigate this land by water from the Provo River.

The Knudsen farm, which my father's family acquired, I don't know how, was probably free at that time. They were assigned to this land. If not free, it probably was [acquired] for almost nothing, because these immigrants arrived really with almost nothing. They had saved up their money to pay for their fare to Utah, and it was the ambition of nearly all converts in those days to come to Zion, as Utah was called, in those days.

And so my father's life: he was still a small boy, nine or ten years old, when they began reclaiming this land. The land was pretty much covered with willows and other growth, and it became necessary to remove willows and trees and the natural growth there to make it fertile land. It is among the most fertile land in the State of Utah. Utah County has very rich soil, and this soil, where my father's farm was established, was right near the mouth of Provo River that empties into Utah Lake. The Provo River, as a matter of fact, formed the north boundary of our farm land. The farm that belonged to my father's family was probably oh, 300 to 400 acres. It grew somewhat because they acquired land later, and earlier than I can remember, but I have been told about it. I was probably two or three years old. My father was then old enough to inherit his part of the land. He had one brother, Herman. The farm was divided into two parts, and as is usual, it was in various segments, and they drew lots for the segments they got. This was the usual way of transmitting from fathers to sons. In general, all the sons stayed on the farm in those days because the farm could be divided and still be big enough for each of the sons.

So the farm which my father inherited that way consisted probably of 150 to 200 acres, something like that, some of it pasture land, which since has become an airport for Provo, Utah, but most of it very fertile land, which grew sugar beets and potatoes and grain and alfalfa and a large variety of crops of that sort. But the money crop was really sugar beets. I'll come to that a little later because this is how I got my introduction into farming and into hard labor, and probably one of the reasons I deserted the farm a little later in life.

Well, as you know, the sons of farmers in those days were normally predestined to remain farmers, and it was expected that I would be a farmer, as my three brothers were. There was quite a gap between my birth and my next eldest brother because my father spent a mission in that interval, which delayed things somewhat. I think maybe there were some miscarriages in the process, and so there's a gap of some ten years between my birth and my next eldest brother. Mine was December 27, 1893, and so I got my initiation on the farm in the summer, six years later.

I was six years old when in summers, I began working on the farm, and I remember the first employment was thinning sugar beets, which was the real crop. The sugar beet patches were generally in about ten-acre lots, and that means that the rows were forty rods long. I was just estimating that here; you see, that's 220 yards, or 660 feet. In thinning sugar beets, everything was manual. The hoe goes along first in what they call block hoeing; someone, an older man, goes along and chops out the beets about every eight inches, leaving a little clump of beets between chops that are an inch or an inch and a half high. The thinning process is done on your hands and knees. You get down on your hands and knees, and you come to each of these little clumps. You select the best looking one, the biggest and healthiest one of the clump. Maybe there are five or six altogether there, and you pull away the runts and leave the best one, and push some dirt around the survivor and go on to the next clump. Well, in roughly 660 feet, there would be approximately a thousand of these clumps on a row. You could thin one row of sugar beets, if you worked very, very hard, in about thirty or thirty-five minutes, so you could do twenty rows, then, in a long day, ten or eleven hours, something like that. That was a normal working day on the farm in those days, preceded and followed, of course, by the usual chores.

MINK: Was there a particular time limit in which that [beet thinning] had to be done as far as growing seasons were concerned?

KNUDSEN: Yes. This was critical, quite critical. The planting was done usually in early April, and the sugar beet thinning came beginning about the middle of May and ending about June 1, so that nearly all of the thinning had to be done in two weeks.

I began thinning beets (in the late afternoons) when I was in the first grade at school, Maeser School in Provo, Utah. My father would come by the school every afternoon at two o'clock, when the classes were out for the younger grades, and so six, seven, eight, nine year olds, something like that--as many as six of us--would be taken into father's conveyance. It was a surrey with a fringe on the top. I can still remember that. It was a very attractive means of conveyance.

We had two homes, one on the farm at what was called Fort Field, and another in the city of Provo, called Provo City. It probably had a population of 3,000 or so at the time. The farm was three miles from our city home, towards the mouth of the river. There was a house on the farm also, where much of the time, my three older brothers lived. They stayed almost continuously there, except in the winter. When they were in school they would come to the city, but one or two of them had to be on the farm even in the winter time, because they had twenty-five or thirty cows, and these had to be milked morning and night, and there were no milking machines in those days. During the summer months I often participated in this ceremony of milking the cows at about 5:30 in the morning, in the harvest season, and then you put in ten or eleven hours in the field, and then it's milking time again. By the time comes supper it's probably 7:30 or so. By the time supper's over, you're so tired and sleepy that you roll into bed and you sleep, sleep very well.

Well, this work didn't please me very much. It's amusing, I think, in terms of the value of labor then and the value of labor now. The going rate for thinning sugar beets was five cents for each forty rod row, so that if you worked eleven hours, you might do twenty rows and make one dollar. It was a good, a very good day, when you earned as much as a dollar thinning sugar beets, and you've crawled then, you see, a total of twenty times forty. That's what? That's eight hundred rods. That's a long way, and you've thinned some 20,000 clumps of beets on the way. Forty rods is an eighth of a mile, so you've crawled two and one-half miles, I believe, if my arithmetic is right.

Well, this was my introduction to the farm, and naturally I didn't regard it as a salubrious form of work and life. It was a toilsome life. In the summer months we worked definitely on the farm. There were sugar beets to be weeded throughout the summer and the potatoes to be planted and later weeded, and the grain to be harvested, and the three crops of alfalfa to be cut and winnowed, you know, into rows and placed by pitchforks into a cocking, cocking the hay, we called it. So these were all activities that I grew into. The first few years I didn't do the heavy work, but I did such things as thinning beets and weeding beets and weeding potato crops and things of that nature.

Summer was always a wearisome thing, even though we had many pleasant diversions. We lived near the river, and the river was a very fine swimming hole, deep water there, eight, nine, ten feet. There were fish, mostly trout, in the streams, and my brothers taught me how to fish. My life-long interest in fishing probably dates back to when I was six or seven years old and learned to fish and swim in the swimming hole there. There were nice pools for trout above and below this swimming hole, which was right adjacent to our farm. In the summer time, when it was very hot, it was customary to have a swim practically every time we had made one round of weeding sugar beets; that would take maybe forty minutes. Then, our costume consisted of a pair of overalls, and we would run to the river and strip and have a little swim, then put our overalls on again. We'd never think of having a towel to dry yourself. Put the overalls on again, back and do another round, and then have another swim. So probably we would have four swims, anyway, during the daytime. This was not at all unusual. It was one of the compensations, really, for living on the farm.

My mother and father often took my two elder sisters, just older than I am, the three youngest of the family, on camping trips during the summer. Usually, this would be for a period of at least a week. I recall one such trip, especially, that was to where the Strawberry Reservoir now is. It was Strawberry Creek then. But as an indication of travel: we began, I remember, one of those trips late one afternoon. We made six miles for the first afternoon, and then we camped that first night up at the mouth of Provo Canyon. It took two more days to get to Strawberry Creek. Now it's forty-five miles, and you go by automobile in about forty-five minutes. But it was a two-plus day-journey in those days. I remember going with my father to fish in the creek the first night we arrived there. Father had just a dry fly on a line. It wasn't a very fancy line, but every time the fly would hit the creek, I guess there were twenty trout rushing to grab it. They were brook length, but in a half an hour we had all the fish the five of us could eat for dinner that night. That's the way fishing was in those days. These were all native, of course. They'd never thought of anything like planting at that time.

But in spite of these pleasures, the farm was not for me. I realized that. During the summer months, I would always count the days since I was in school and the number of days when I'd be back to school again, because it was a sort of an emancipation. I loved learning in those days. The school meant a great deal to me, and my parents recognized that and soon began to recognize that the farm was not for me. By the time I was eleven or twelve years old, I think they realized, "Well, Vern's going to go to high school, and he's going to go to college. He may not necessarily stay on the farm." They were determined that one in the family, at least, should have a complete education. They helped a great deal, all the way along the line.

I remember when I was in high school studying geometry, among other things. In the harvesting season, especially the harvesting of the sugar beets, which took place from about the first of October until the middle of November, it was customary for young boys of my age, even through high school, to withdraw from school. This was standard practice in my life. There was a period every fall, I'm sure, until my second or third year of high school that I was out for a period of six weeks. Then, one of my sisters would find out what the lessons were for the next day and bring them to me, and I would work on those at night after this strenuous work in the beet fields. But when I was studying geometry, I was given a great favor. I was permitted to herd the cows on the old alfalfa field. That is, you'd cut three crops of alfalfa, and then the fourth is allowed to become pasture. The cows pasture there, but there's no fence between the alfalfa field and the beet field. I was permitted to be on this boundary between the two, to keep the cows from getting into the beet tops, because that was almost poison. It gave them diarrhea and also made the milk taste bitter, which was something, so we had to keep the cows out. But this allowed me time, and I really worked out plane geometry, very much of my plane geometry, watching these cows and working geometry theorems at the same time. I do remember that I got an A in my geometry, in spite of having to work on the farm under those circumstances.

Well, high school was finished in normal time. My elementary school began at the Maeser school. Now Karl G. Maeser was a very important man in the Mormon Church. He had become a convert and was a teacher in school in Germany and had learned about the Mormon faith and came to England, really, to investigate it, because there were no Mormon missionaries in Germany at that time. Maeser subsequently became a member of the Mormon Church, converted by missionaries in England. Even while he was there, he was one of the first missionaries to go back to Germany and open up what was called the German Mission. He was an educator and finally came to Utah after having served, I think, some time in Baltimore, also in education there. He called on Brigham Young, and Brigham Young told him, "Well, you know, we'd like to start a school down at Provo. Would you be interested in going down there and starting it? There isn't much money. You'll have to take script from the farm, really, in compensation for your services, but it's an important mission that I would like you to undertake." Maeser came back the next day and said he was eager to go down there. He was the founder. Really, it's called The Brigham Young University, but Maeser had been appointed by Brigham Young to head this school. It was a very primitive thing, naturally, in those days the money was very scarce.

The first day I went to elementary school, the first grade, was at the Maeser School. It had been named in his honor. Dear old Maeser, we revered him at that, came to the classrooms in the Maeser school. This was its first year, and he wrote a gem on the blackboard of each room, and I can remember him writing--one of my first memories of school--this gem on the upper left-hand corner of the blackboard, and it was framed with glass. It may be there to this day. I regret to say I haven't been back to see it, but I must see it sometime because it was something that was important in the life of a youngster. He believed, of course, very much in the religious aspect of training, and one of the things Maeser said that I remembered a long time, [something] he said, Brigham Young had told him, [was this]: "You must teach mathematics, even mathematics, with the spirit of the gospel."

MINK: Do you remember what he wrote on the board?

KNUDSEN: I must find out sometime. I would like to know. I'm sorry, I don't have it now.

Well, where do we go from here? The church was really everything in the early days of Utah, even in my early days, although we had city government, and we were then a state, had been a state for quite a while, became a state at almost the time of my birth.

Ours was a typically religious Mormon family. My father had been on a mission, and my three elder brothers subsequently went on missions, as I remarked earlier.

The usual ceremonies in the family were strictly observed, family prayer, always in the morning, always at night. Father was very zealous to see that all members of the family were present for family prayers, and in the morning for a long, long time, the family prayers were preceded by singing of appropriate Mormon hymns or Christian hymns that were sung at that time. My father was a musician. He played both the violin and the clarinet and later taught me to play the clarinet. I played the clarinet during my youth. But the prayer in those days was very definitely a format that they blessed, prayed blessing upon everybody, beginning with the constituted authorities of the church, the first presidency and the twelve apostles of the church, and then the constituted authorities of this land, from the president on down to the least and last called of those who were in governmental positions.

The morning prayer was usually a pretty long formal affair and the children knew pretty much about how long it was going to be. I had a second sister. She has always been very close to me throughout my life and has followed it very, very carefully. She died just after this building [Knudsen Hall] was dedicated in May of 1964. She was a little more restless, I think, than the others, and very often during family prayer, she had her breakfast on the kitchen stove, cooking. Family prayer was always in the dining room around the dining table. She often felt that, well, she had to turn the eggs over, or attend to something in the kitchen, and so she'd slip away from family prayers and go in and do these duties in the kitchen and return in time before the amen was finally pronounced.

At night, again, this ceremony was performed. During a part of my youth, and a very pleasant part, the entire family would gather around the table after supper. We had supper, and the sisters, I guess, washed the dishes, which was a routine ceremony. Then in the winter months, we always had apples, or fruit of some kind, or nuts. We had spent from seven to nine doing our lessons around this table, and this was a quiet period. Father might read a little scripture, or translate some Ibsen, toward the end at nine o'clock. We'd usually get the scripture reading, and then we had our family prayers at nine o'clock, and then everybody was off to bed, immediately after nine o'clock.

Well, my father at that time, during my early youth, from the time of my birth until I was six years old, was a bishop of the Provo First Board, and as such, there was strict adherence to the ordinances of the Mormon Church, which meant no alcoholic beverages, no smoking. Even coffee and hot drinks in general were taboo, although the one hot drink that was tolerated was barley coffee, which was made by browning ordinary barley until it was almost burnt, and then we stewed a brew from that, which was a substitute for coffee. My mother and father both loved coffee. My mother did especially, and in her older years, she departed from that part of the Word of Wisdom, as it's called. The Word of Wisdom, among other doctrinal pronouncements, regulated the diet somewhat. Even excessive eating of meat was not considered "good for the belly," that's the wording that the doctrine and covenants used in referring to that.

Well, I was brought up, of course, under this strict religious regime, and beginning at the age of six you become a member of what's called the "Primary Organization." And so once a week, I would go to these primary meetings for youngsters. This was not especially doctrinal. It had more ethical content, moral content, than religious. It was designed to amuse as well as instruct youngsters. They were accompanied by pleasant parties. We had quite frequent parties, primary parties, and by the time you were nine or ten years old there were even dancing parties that were held in the afternoon.

Dancing had never been frowned upon by the Mormon Church, although the church had been very strict in many other things. My first association with girls really began while I was in this Primary Association, and in those days, if you were going to escort the young girl to the dance, it was a bold boy that would dare go knock at the door. He would usually whistle on the outside for the date to come out and join him for this primary dance.

MINK: Why was it not considered right to knock?

KNUDSEN: I don't know. I think it was a timidity of some sort. You didn't want to meet the parents. That's my recollection. I know on many occasions I called for dates by whistling instead of knocking at the door. Maybe by the time I was eleven years old I began knocking at the door, but at the younger age it was the whistling, and this was probably not too unusual for very small children. It was customary. If you didn't whistle, you called the name out. This was a rather unusual way of notifying the person that you were there.

Well, I was certainly indoctrinated in this religious regime, and as I reflect on it now, it's too bad there isn't more of it in the world today. It had its place, and especially, it had its place in the bringing up of children. The fear of God was implanted in your heart, but also the ambition to make something of yourself was implanted in those ceremonies.

My father was a very hardworking man. He was, although he had probably no more than a grade education, a great reader, and he was always interested in improving the mind. He was an accomplished musician. He was a first-rate violinist and a rather good clarinetist. As a matter of fact, he supplemented his farm income by playing with orchestras and playing for the Provo Opera Company, that is, with the orchestra for the Provo Opera House, which was everything. It's a multipurpose auditorium, but it was the opera house, and traveling theatrical groups and musical groups came.

These things were accompanied with our strict religious upbringing. We were required always to be home at an early hour. That is, we were expected to be home, I know at ten o'clock and if it was later, eleven o'clock, you were reminded that it is late. Midnight was always considered, even when you were in your upper teens, as the time when you were supposed to be back and give an account of where you'd been.

Once, in my early youth, I learned that religion and politics and many other matters really were very closely intertwined. The first political speech I ever heard--I was probably six, seven, eight years old--was a Scandinavian, barnstorming politician, who was pleading for the Republican cause. My father had become a Republican. The politician begins: "Brothers and sisters, I want to tell you. Democracy is hypocrisy but Republicanism is the word of God!" And this was so typical. You see, he was earnest about this.

MINK: And this was predominantly a Republican area?

KNUDSEN: Yes, it was. For two reasons: sugar beets required protection, so they felt; it was to keep the price of sugar up so they could earn a profit on their growing of sugar beets. And also, it was a grazing country and a sheep country, and the protection of the price of wool was also an incentive for a tariff. These two things, I'm sure, had a lot to do with the growth of the Republican party in Utah, and it was pretty strongly Republican in my youth. I think that most of the farmers were Republican.

MINK: And so it's continued?

KNUDSEN: It's continued a great deal. Although it's been Democratic at times, it's Republican again now. Well, do you want to shut it off a moment? [tape off]

The training that one received in church matters began very early. As soon as you could hear, you listened to these prayers, and you were taught to participate in the family prayers. Members of the family were called upon to pray. My father didn't always do it. In the morning he always prayed; at night this was a matter of rotation. And so, certainly, by the time I was six years old, I was supposed to participate in the family prayers. Of course you always said your private prayers before going to bed.

But the first training in the church that was of a formal nature, after this primary experience that I reviewed briefly, is the ordination into the priesthood. This takes place in the Mormon faith for all faithful male members, beginning at age twelve, when they are ordained to what's called the Aaronic Priesthood named after Aaron. That's the lower priesthood. There are three steps in the Aaronic priesthood, the deacon, the teacher and the priest.

The duty of the deacon is primarily to assist in the gathering of fast offerings. In a rural community, as Provo was in my youth, this consisted mostly of collecting food the day before fast day. Fast day was the first Sunday of each month, and you were supposed to eat no food from Saturday night, when you had your dinner, until after fast service that normally ended about three o'clock Sunday afternoon. That is, there was Sunday school in the morning and a fast meeting at which testimonies were borne. It was very much like a Christian Science testimonial meeting now. This service was given over to the passing of the sacrament and to the bearing of testimonies by the lay members of the church.

Of course, all the officers in the church are lay members. There's no paid ministry and all officers serve without pay. The twelve apostles and the first presidency, of course, have allowances. I don't know just how that's arranged now, but they live good lives. They have good Cadillacs, and it's not a hardship life any longer to be an apostle or one of the first presidency of three. The trinity has a very marked effect, you see, on the organization. There's the first presidency of the church, which is made up of the president and his two counselors. The church is divided into stakes; a stake is a group of wards, each of which would correspond to a parish in our general understanding of church organization. Probably six to eight or ten wards would comprise a stake. At the head of the stake again is a first presidency. The presidency of the stake is made up of a president and two counselors. This trinity goes into the ward; there's a bishop and his two counselors. And in the other organizations: in the Sunday school, there is the superintendent and his two assistant superintendents. In the Mutual Improvement Association, which is the young men's organization that continues after primary, there's the president and the two counselors, and the same for the young women's [organization]. And it's this way, really, in all of the church organizations. This trinity is outstanding.

Well, as a deacon, we had these two responsibilities. One was the gathering of fast offerings on the Saturday before the first Sunday of the month. This was for the benefit of the poor. The fast offerings usually consisted of gathering things in a little red wagon. So the boy at age twelve goes with another boy at age twelve or thirteen. Maybe he's accompanied by one a little older, because there's normally a three year period in which you serve as a deacon. You had the equivalent of a block, maybe ten or twelve families, that you would call upon. You'd call with your little red wagon and you'd knock on the door then and say, "We're deacons calling for the fast offering." We always had a flour sack because most of the fast offering consisted of flour, maybe ten pounds or fifteen pounds of flour. We'd have two or three bags of flour, and I know we collected more flour than anything else. This was taken to what was called the Stake House, which was a repository, really, for all of the city of Provo, and was dispensed then by church authorities, relief society, and so on, to the families of the poor. This was good training, you know, for boys twelve years old, to spend a Saturday afternoon once a month in going out and thinking of the impoverished in their city and their community and getting donations of that sort for the benefit of the poor.

The other duty of the deacon is to participate in the administration of the sacrament, in the passing of the sacrament. The sacrament consists of eating bread in remembrance of the body of Jesus Christ and of drinking water--they don't use wine--in remembrance of His blood that was shed for mankind. The deacons pass the sacrament. This is a duty they begin, again, at age twelve, and they're trained quite strictly. It's done, really, with good deportment and good, almost military, order. These boys are always dressed up and look neat as they appear in church. Even in those days, we were warned to have clean fingernails and to have our hair trimmed and to be dressed in our very best clothes, when we helped with the passing of the sacrament.

This is followed by three years in the second step of the Aaronic Priesthood, in which you are a teacher. I had performed faithfully as a deacon, and so I was promoted in due time to a teacher. The teacher, beginning at age fifteen, goes with an adult, who is a member of the higher priesthood. This is called ward teaching. Once each month the ward teachers--in my youth this was made up always of a teacher who was age fifteen to eighteen, in that bracket, with an adult who was more accomplished in understanding what the gospel was about. You probably had a dozen families altogether that you were to call on each month. This again was a religious ceremony, which ended again by bringing the family down on their knees and having prayer with the family and leaving a message with the family, a timely message, which had been directed by the higher authorities of the church. This message came down to the ward teachers each month. Often, it was a special message which had to do with the observance of the law of tithing, which, you see, is a rather strict law in the Mormon Church.

I'm sure that Governor [George] Romney is still (1968) a very faithful tithe-paying Mormon. That means he pays quite an amount. He did when he was president of American Motors, at any rate. Maybe I'm surmising a lot, but I've been told that he's a tithe-paying Mormon, and I believe it because I know many of the Romney family myself. Incidentally, I know he's a very good man, and the family is very good. It was a polygamous family, initially, and therefore there are many Romneys. I can name at least ten Romneys that I knew in my youth that would now be relatives of George Romney, who is being talked about a great deal as a possible candidate of the Republican party for president of the United States.

Well, as a ward teacher, this again had an educative value. I was supposed to lead in prayer half the time; that is, the elder man would usually do it the first time. The next time I was to lead the family in prayer and ask the blessings of the Lord upon them and for the forgiveness for the sins they committed and to admonish them to be faithful in their observance of the laws of tithing, fasting and prayer, and the moral requirements of the church, and so on. This was routine; and it took at least three nights each month to perform your duty as a ward teacher. This practice is still in vogue in the Mormon Church.

Occasionally, we have ward teachers call at our home; it's mostly a friendly call, followed by having a printed but timely message. I'm not a good practicing Mormon at the present time. My Sunday church service consists primarily of listening to the Salt Lake Mormon Tabernacle choir and organ program, with Richard Evans's timely sermon. It still means a great deal in my life, the Salt Lake Mormon Tabernacle. I value the training I received as deacon and teacher in the church.

Well, the third step in the Aaronic Priesthood is that of priest. The priest continues some of these duties as a teacher. He is still expected to participate in the ward teaching, sometimes as the senior partner, but he then has the authority to participate more actively in the sacrament. He actually says the ceremonial prayers in the blesssing of the bread and the blessing of the water. And the ceremony goes something like this: "Oh God, the Eternal Father, we ask Thee in the name of Thy son Jesus Christ to bless and sanctify this bread to the souls of all those who partake in it; that they may do it in remembrance of the body of Thy son who died for them," and so on.

I forget the rest of it. But that's the way the sacramental prayer goes. The officiating priest has the printed prayer before him so that he wouldn't forget the end as I did in just relating this to you, and a similar one for the blessing of the water. Then the priest hands the trays of water, the trays of bread to the deacons, who pass them out to the audience in the sacrament service; there's a sacrament service every Sunday. This is still a practice in the Mormon Church, sacrament comes regularly at a ward service.

MINK: In the Tabernacle?

KNUDSEN: No. It's in the ward chapel.

MINK: I see. At Salt Lake, where would it be held?

KNUDSEN: We had wards all over Salt Lake. I imagine that today there may be fifty wards in Salt Lake City. As a matter of fact, at the Brigham Young University they now have stake and ward organizations, and the students participate in the regular church activities, but much through the administration of the next phase of the priesthood that I will describe briefly now. That's called the Melchizedek Priesthood, named after Melchizedek, a name you'll find in the Bible.

There are three levels or ranks in the Melchizedek Priesthood: elder, seventy and high priest. I successfully passed the priest level and was, in due time, ordained an elder. Normally, the time you're ordained an elder is about the time you're likely to be called to go on a mission.

MINK: Do you remember how old you were when you were ordained an elder?

KNUDSEN: Yes, twenty-one. That's the usual time, three years as a priest, eighteen to twenty-one. As a matter of fact, sometimes the ordination will come a little earlier, if you're called on a mission. You're always ordained an elder when you are called on a mission, even if you're not twenty-one years old. As a matter of fact, in my college years, after two years of college, I received what is referred to as "a call on a mission." This is a formal request that comes from the first presidency of the church. It always comes from the headquarters in Salt Lake City, and it says, "You have been called to perform a mission for the church in a certain place." This first place, I actually forget now, but this was two years before I finished college. If one has circumstances that would make it difficult to go at that particular time, it's considered ethical and proper to write to the first presidency of the church and say, in my instance, "I would much prefer to finish college before I go on a mission." This is not usual. It's customary for most young men to go even before they complete college, but I wanted to complete college. I had become interested in mathematics and physics and engineering somewhat at that time, and I felt that I was entitled to that leave. Furthermore, I felt that I would be better qualified to be a missionary, which also is true because, during the last two years of college, I was quite active as, well, an itinerant preacher, if you allow me [laughter] to use that word. It amounted to that.

Young men, especially if you were engaged in college work, might be invited, or requested even, to participate in the preaching services of the church. I know, beginning at age nineteen or twenty, I did have calls to participate in such work, and I went to neighboring wards, usually outside of Provo. They would usually get you a little way from home base, to places like Springville and to Lake View and other communities around Provo. I probably visited a dozen wards during the last two years I was in college. I preached sermons, and one of them I used in quite a number of places. It was based on the thirteenth chapter of the First Corinthians, you know, about faith and hope and love (or charity). This was a favorite topic of mine and I had carefully prepared the sermon, parts of which I read, and parts of which I had memorized. It was well-received in general and, I think, sort of marked the preparation for my missionary experiences. I knew I was going on a mission, as my three brothers and father had gone before me, and my call to a mission therefore was delayed until after my graduation from college.

Incidentally, when I graduated from the eighth grade, this was quite a stepping stone in those days. That's where education ended for most young boys, in my youth at that time. They finished the eighth grade, and the graduation exercise was really quite a formal affair, held in the Opera House. My father trained me in the playing of the clarinet, and my part on the program, when I graduated from the eighth grade, was to play a clarinet solo. I still remember it. It was Schumann's Träumerei. This is good music, and I know I got great pleasure, and my family got a great deal of satisfaction from this performance of the eighth grade.

Incidentally, the examinations at the eighth grade then were graded at a central place. There were probably five or six schools in Provo at that time, and I succeeded in being the number one scholastically in the eighth grade. So this gave me some encouragement too, that possibly education was something I ought to pursue more than the farm. I'm sure it had a bearing upon my parents making it possible for me to complete high school. There was no high school graduation because the Brigham Young Academy was both a high school and a college at my time. I spent seven years at the Brigham Young University, where I had all of my high school and all of my college work.

MINK: Oh, you did your high school work in the university?

KNUDSEN: It was not a university then. When I entered, it was called the Brigham Young Academy. Then it became the Brigham Young College, and finally, Brigham Young University. I graduated in 1915 at the head of my class--and therefore was the class valedictorian. The address was published in the school paper. I'm sorry, I tried to find my valedictory address. I thought maybe I had it and that I might quote a few things from it. If I can locate it I will. I saved it, I know, for a long time, but that's a long time ago. If I do, I'll let you have a specimen [laughter] of it. Well, that's maybe a stopping point for a while, here now.

[tape off]

Near the completion of my college work in 1915, I then received this formal call to go on a mission to what was then known as the Northern States Mission, with headquarters in Chicago. We were ordained elders then at that time so I became a member of the Melchizedek Priesthood, and was duly ordained as a minister and had the authorities of a regular minister.

MINK: What determined the mission? Was it something that somebody told them you would like to do, or did they just select these at random?

KNUDSEN: Well, I had indicated that I preferred to have my mission where there were cultural influences present, and I named New York and Chicago as two places that interested me very much, and London.

MINK: Oh, you get asked about this?

KNUDSEN: Yes.

MINK: At what point do you get asked about it?

KNUDSEN: Well, the first call was to the German mission. This was two years before I had completed college. When I asked for the delay, I said I would, if possible, prefer to go to some place where there was a university or a cultural environment, because I very definitely intended to pursue an academic life. I had known that then, when I was in college. I had one B in my college record. The rest were A's. The B was in a course in German, and I was dating a young lady at that time. Occasionally, I'd cut German to go out walking with this young lady, and I think that was largely responsible for this B grade. But physics and mathematics and engineering had appealed to me. In my youth I thought I wanted to be an engineer. In college I thought I wanted to be a mathematician. As a matter of fact, the vice-president of the Brigham Young--I think it was called a university then--Brigham Young University, spoke to me and said, "I wish you would work for your PhD degree in mathematics some place. There would be a post for you here at the Brigham Young University on the teaching staff." And this, of course, was a great inducement at that time to think that you might become a professor at the college where you were having your training. Then I met Harvey Fletcher--we'll go into that story later--which very definitely directed me into acoustics.

The call for a mission then, I realized, was quite a turning point in my life, the end of college, and it had been a joyous college. The last two years I had been relieved from any farm duties, even in the summertime.

Two years before I finished college--this is an interlude--I was persuaded to go with a group of book salesmen into Oregon, to sell Orson Swett Mardens's [book]. He had been the editor of <u>Success Magazine</u>, and he was the author of a two-volume book called Pushing to the Front, and Pushing to the Front was a favorite in those days, to be pushed by college students that were recruited to go out in the summer and sell these books. You strapped on the prospectus in which you had the little leaflets from the Orson Swett Mardens's Pushing to the Front. We spent this time up there in Oregon, eastern Oregon, a rattlesnake-infested country, where we were in rural communities, The Dalles and another place, Arlington, and along the Columbia River, where the railroad runs.

I lasted at selling books, I think, about eleven days, and I then knew that it was not for me. I wasn't succeeding very well and so I went into Portland, Oregon, where I got my first professional job. I had had one course in plane surveying and I called on the YMCA secretary to ask for advice about how to go about getting a job. I simply had to make some money during the summer. I didn't want to go back on my parents, and they'd recognize that I'd failed on this attempt to sell books. I knew that I was not a salesman. That was very, very apparent. And the YMCA man said, "Well, what training do you have?" I said, "Well, I know how to do plane surveying. I've had instruction in surveying, and I know how to use a surveyor's instruments." He said, "Well, then, you go to . . . " He gave me a list of names, and he said, "You call on them everyday and see if they've got an opening." The third outfit I called on was the Oregon-Washington Railroad and Navigation Company. It operated a railroad along the Columbia River, among other things. I had an interview with the man who was hiring for the technical employees, and I told him what my training was, in mathematics and physics and surveying, and so on. I had completed two years of college. And he said, "Well, we need a man to do some work along the Columbia River." He said, "Do you know how to do cross-sectional work?" I said, "Yes, I do." I just knew what it was about, but I actually didn't know the technique. He said, "Well, you come back tomorrow, then, and we'll put you to work on doing cross-sectional work along the Columbia River." Well, I spent all day, until the library closed, at the Portland Public Library reading everything I could find about what cross-sectional work consisted of. It was really a method of measuring how much earth has been washed away by the high waters of the Columbia River, and so they had to know that in order to make plans for repairing the damage that had been done by the high water that year. Well, this is just an interlude, but it indicates that I was away one summer during my college career and was very glad to get back again to Provo. It was then that I met Harvey Fletcher.

MINK: How did you do with the work, once you had that one day of preparation?

KNUDSEN: Well, I got along all right. Yes, I stayed on it. It was temporary employment, but it lasted until I was ready to come home at the end of the summer, so I was at that probably two months, or a little over two months.

MINK: In that time how did you live? Did you live in a work camp?

KNUDSEN: No. We were in Portland, Oregon. It was headquarters. We were out Monday and returned on Friday night. It was a five-day week, even then, for this kind of work, at any rate. There was a party of us. There were three, or sometimes four in the party conducting this survey. And on most of our travel, we'd go by train, say, to Arlington or to The Dalles, or some place like that and then go off on a branch line and there we'd usually go with a handcar. [tape off] [Dr. Knudsen was asked if there was any talk among the survey crew of his limited experience with surveying in college.] Yes, well, this, I think, had a bearing on professional work. I realized that I could do work of that sort, having had only one semester course in plane surveying, and go out and pose as a surveyor. I got away with it all right, and they kept me on until I had to leave for college again at Brigham

Young University, which opened sometime in September.

I see we were talking mostly about this call on the mission, so we'll jump ahead two years, after I left for my mission. Having been ordained as a minister of the gospel, I arrived in Chicago. The first duties of a Mormon missionary are what they call tracting. That means two of you go in pairs. This is not three, but a missionary always has a companion. You are broken in by a more mature person, who has been in the mission field probably a year longer, so I had a married man, who was probably twenty-five years old, to initiate me into this tracting operation. You carried with you a few copies of The Book of Mormon and some small pamphlets. They are called tracts, and it's called tracting. You were assigned to a certain area. I know I was assigned to an area on the north side of Chicago, quite a way north, not as far as Evanston, but right in the city limits of Chicago, where there were, you know, flats, usually two-story flats. You ring the doorbell, and there's a speaking tube [through which] they would answer back at you, "Who's there?" and "We are representatives of a religious organization, and we have some printed matter that we would like to leave with you." This was the usual come-on. Well, nine times out of ten, you know, you're given the brush-off, but once in a while, they'd say, "Well, come upstairs," or "Come in."

You'd be invited in, and you'd have a little discussion. This was the beginning. This is a typical way [it was done] in my youth, and I think it's done quite similarly to that today. This is part of the missionary process, called getting converts to the Church of Jesus Christ of the Latter Day Saints. It's been a very successful operation. I may have told you the missionaries serve without pay. You pay your own railroad fare really to the mission field, and you pay all of your living expenses and other expenses while you're in the mission field. For a young man, it's usually a period of about twenty-seven months for missions in the United States and English speaking countries; it's longer in foreign countries where you'll have to learn a new language. It's as long as four years in Japan.

MINK: Immediately it comes to mind, where do you get the money to do this?

KNUDSEN: Well, it comes from the family sacrifices.

MINK: Is this part of the tithing?

KNUDSEN: This doesn't come out of the tithing. This is extra. You don't have a deduction from your tithing. Yes. This is another service that's called for in a religious family in the Mormon Church. Mother and Father paid my expenses, but my only problem [was] the living expenses in those days, 1915 to 1918. Prices were going up because we were at war before I finished my mission.

Thirty-five to forty dollars a month, though, was very frugal living. Usually a number of missionaries would live together, and they did their own cooking and their own housekeeping, and so on. Thirty-five to forty dollars was considered the usual amount that the parents would have to put up. It had been less than that in earlier years, but in this time, it increased up to about that amount.

Well, I frankly didn't enjoy tracting very much. I knew it was my duty, and I said my prayers faithfully so the Lord would strengthen my testimony and convince me that what I was doing was really the thing He wanted me to do and the thing that I should be doing. I wasn't quite sure about it.

Very soon, I was also permitted to speak on the street corners of Chicago. This is still a process that's used in many places. It's something like the Salvation Army, although not quite so formal and less music. There would be, maybe, two, three or four missionaries together, and, on the street corner--this was permitted--you'd begin by singing a Mormon hymn or two, and then you'd start speaking. Maybe there'd be one or two drunks, or one or two passers-by, who'd stop first of all. But little by little, you were able to attract a crowd. I found that I was pretty good at attracting a crowd, and this certainly was good training again. I don't know of any service that was more rewarding than the feeling that you had after you'd had a successful street meeting. These were called street meetings. You'd preach maybe a thirty minute sermon of some sort, and you actually had some attentive listeners. They weren't just hecklers. They stayed there on their own. But often they came up and wanted to know who we were, and we introduced ourselves. Two or three converts I know came from that type of encounter. This continued through the early winter.

I went on the mission in September of 1915, and in the spring of 1916 I was sent out on what's perhaps the most arduous part of the Mormon mission, which is called "traveling without purse or script." You go to the end of the trolley line. You were allowed to take that much money to pay your trolley fare to the end of the line, and then you're supposed to get your lodging and your meals by your missionary efforts. You are told the Lord will provide, and you go out with that feeling. You don't know if this is going to last two weeks, or a month, or until you're notified that you can return to headquarters.

I began my "traveling without purse or script" as a Mormon missionary somewhere between Chicago and Geneva, Illinois. Geneva is about twenty-five miles [from Chicago]. We went as far as we could go for the trolley fare, which, I guess, was five cents then, or something like that, and got off at the end of the line. We were soon in farming country, and our tracting there along the country road, was headed West along the old line of the Union Pacific. It was about a ten-minute journey between homes. You see, they were fairly large farms, so you wouldn't call on more than ten or twelve families in a day, and you'd hope to find a hospitable family when evening came, where you could have supper and bed as their guest, and breakfast the next morning. If you didn't succeed, then you slept out. We succeeded every night.

We were out four weeks altogether, and we never missed a meal and never paid for a meal because we didn't have anything to pay for a meal. We lived, and lived reasonably well. We were able to convince someone toward dark that we weren't scoundrels, that we were Christians, and that we had a message that we considered important. You almost always arranged to get them down on their knees before you retired for the night and preached a little sermon in your prayer with them.

We occasionally shaved, bathed and washed our clothes in a nearby river or stream. Some of our introductions to strange families were amusing. As we were opening the gate, we were greeted by an unfriendly dog that I was fending off with my umbrella. During this unpleasant encounter, the master of the house suddenly appeared at the front door and, in a hostile voice, demanded, "What are you doing?" I replied, haltingly, with our usual introductory sentence, "We... a...are...a...representing...a...the Church...a...of Jesus Christ..a...of...a..Latter Day Saints." "Well," he said in a friendly voice, "if you are Christians, come in." We were invited to stay for dinner, and then overnight with a comfortable bed and bath.

It wasn't easy for me. I know I'm always reticent about approaching strange people this way, and especially asking for food and lodging. This was not easy, but you were told that this is the way the missionaries did it in the time of Christ, and this is the true way of preaching. You were to give no thought about what you eat, or what you wear, or things of this sort, but again, it was very good training. There's no doubt about that. If you put up with this for four weeks, you really learn something about human nature. You learn to have a little courage, and you learn to do things that later pay off. I'm sure it did in my case.

Well, I think one of the most joyous announcements I ever received in my life was when we had been out four weeks, and one Saturday we called for our mail, and there was a letter there that simply said, "Return to Chicago immediately." That's all it said.

MINK: How did they know when to call you back?

KNUDSEN: This was their decision.

MINK: Did they just decide arbitrarily how long you would stay?

KNUDSEN: In this instance there was a reason for calling me back, though the message simply said, "Return to Chicago immediately." Well, this was great news, because even tracting in Chicago, where you didn't have to beg for your food and lodging, was really something to look forward to. So we got the first train back. We were sent in the letter enough money to pay for our fare back to Chicago. When I arrived back in Chicago I was asked (or called) by the president of the Northern States Mission to take over the presidency of the Chicago Branch Mission. I'd been in the mission field then just about seven months. This advancement to an administrative position was unusual for a young man who had been in the mission only that long. The fellow who was branch president was a missionary who had just been honorably released to return to his wife and family, but it was unusual for one who had been in the mission as little as I had to become the president of the largest branch in the Northern States Mission.

So I had other duties from then on. There were probably twelve to fifteen missionaries for whom I was responsible, and I had to assign their duties and I had to do more preaching at branch headquarters. They didn't have wards at that time. They were called branches. There were probably a dozen branches in the Chicago neighborhood, and I, as the president of the Chicago Mission, had to visit the missionaries in these various branches. On Sunday, I was always one of the principal speakers, so I got a lot of experience then preaching Mormon sermons in these branches and the neighborhood of Chicago.

MINK: Do you suppose you were asked to do this because you were so successful with your street meetings?

KNUDSEN: Well, it had something to do with it. The president of the Chicago Mission was released. He'd served his term, and he was the one that had instructed us to return to Chicago and when the Northern States Mission president asked me to take over the ...

MINK: Then he has the right to name his successor?

KNUDSEN: No. The Chicago Branch president recommends to the Northern States Mission who is over the branch presidents of all the Northern States.

So I served as the president of the Chicago Branch for approximately a year, and then I was named secretary for the entire mission, which was the number two [position]. The presidents don't have counselors. The secretary is the number two man, and this meant that I had to visit the various mission branches in the Northern States Mission. This included the states of Illinois, Indiana, Iowa, Wisconsin, Michigan and Minnesota, so I did a great deal of traveling for the remainder of my mission, for about fifteen months, which was until January of 1918. I'd been in twenty-eight months when I was released.

During this time, as I say, you perform many of the functions of a minister. I performed several marriage ceremonies. When the mission president was away, I was the top officer, and so I performed marriage ceremonies, I preached funeral sermons, I did duties that not many missionaries are entrusted to perform, and this had its educational value. There's no doubt about that. It also had a lot to do with preserving my faith in Mormonism for quite a number of years after I completed this mission.

I recall my first marriage ceremony. The groom was in his sixties, maybe sixty-two or sixty-three. I think I had just turned twenty-two. The bride was certainly forty-five or so. I had gone through the ceremony. You have your Bible before you, you know, in which you have the formal ceremony. It's not unlike other marriage ceremonies. I thought I was doing very well and was going to make the end without breaking down. So I came to the end, where it says, "You may kiss the bride." He says, "I'll do that when I get home." I didn't know the answer to that one. I said, "Well, I guess that will seal it then." [laughter] This was my first ceremony of this sort that I performed, but I performed many others after that, baptismal services, and so on.

MINK: Would you travel by train around the northern states?

KNUDSEN: Yes. When you became secretary, your travel expenses were paid by the church. They had an allowance for that. Furthermore, you lived at the mission home and did not pay room or board. So during approximately fifteen months that I was secretary of the mission, this relieved my parents, you see, of supporting me because I lived in the comfortable mission home.

MINK: This would not happen to every Mormon?

KNUDSEN: No. This was unusual. There were at least a hundred missionaries, and only one, the secretary, was privileged to live in the mission home, which was a nice home. The mission presidents lived a very good life, and they had a reasonable allowance. They were called like everybody else, and often they were called from gainful employment and often returned. There have been some tragedies about finding employment again for these mission presidents after they've served sometimes three or four years. They usually serve more than two years, or two-and-a-half years.

My mission president was a faithful and competent man. He served with the prohibition enforcement agency after his release as a Mormon missionary. My wife and I used to visit him in Washington where he had his headquarters. He was one of the men that was to see that the Volstead Act was observed. And so it was that he was more fortunate than many others that had difficulty.

I must add only one thing. It was in the Mormon mission that I met my wife. My mission president had preached a funeral sermon in a suburb of Ogden, Utah, in West Weber. The organist there appealed to him very much. He thought, "There's a girl we ought to have in the mission." He said, "I'd like that girl to meet Vern Knudsen, Brother Knudsen," he always called me. He arranged to have her called on the mission. Now you're asking how this was arranged. Well, he saw this girl and he said, "Well, I'd like to have her in the Northern States Mission." So he passed the word along, or wrote the letter that ultimately led to Florence Telford being called to the Northern States Mission. I was living in the mission home at that time, and it was my duty to receive missionaries when they came. She arrived, I know, on a Sunday morning, and I had been down stoking the furnace in the mission home. One of the members of the family, an elder, said, "Three missionaries are up in the living room and would like to meet you." It was my duty, really, to meet them and welcome them, and I saw this girl, and I knew this was it. It took about one-tenth of a second, and I know when I met President Ellsworth a little later, I said, "Where did you recruit this beautiful missionary?" I said, "I don't know how I'm going to observe the laws of the mission." You know, you're not supposed to do any courting or anything like that as a missionary. This is definitely taboo. Well, the wife of the mission president, I think, had connived with the president, and they allowed me certain privileges, nothing disrespectful or improper, or anything of that nature, I hasten to add. But we were allowed to sing duets together, and I was allowed to take her to the train when she was assigned to serve in southern Indiana. So I escorted this young lady to the train and had also taken her, with two other missionaries, on a visit of some of the principal parks of Chicago, while we were there. Then we began correspondence, and this led to engagement and to marriage, and this is how I won my wife. That's all.

SECTION II

KNUDSEN: I thought it might be of interest to discuss some of the early events in my childhood that led to my interest in numbers, or arithmetic, and science and technology because I believe they are significant in showing my interest or predilection to these things. I had a brother, Heber, who was really my mentor and protector in my early days, and he taught me to count. I remember, in the accounts I have been given by my family, I would count up to twenty when I was age two, and he had taught me how to tell time by the age three, which was a sort of a record, I guess, in the family. I was the seventh child, and they became interested in this. But my brother Heber especially was.

Soon after three, I don't remember how young, but certainly before I began school, I saw a survey party at work, and this interested me very much. I plied Heber with all sorts of questions about what they were doing, and he explained the transit and the level that he knew something about, and how the transit was used for determining the boundaries of lots and farms, and the level was used for laying pipes on the farm, and this interested me. I thought I'd like to be a surveyor. I know that was my first ambition. When I saw these people, I thought that's much more interesting than farming, or it's even more interesting than selling things in the store, or even in the semiprofessions that I knew something about at that time. This was, I think, the first evidence that I was going to be interested in science and technology, and it had, I'm sure, a lasting benefit in my life.

MINK: And this is when you were three?

KNUDSEN: Well, sometime between three and six. This actual survey event is certainly before I went to school, which I began of course at age six.

One of the other early things that interested me very much came from my mother. She had been in Sweden in her early youth, and the matter of sunset was a very important thing as the winter solstice came and was passed. They kept a record outside of the time of the setting of the sun. They had marks, she said, on one of the sheds there. They'd write down the time of the sunset; and one of the things that she taught me at that time was that the sun sets earliest, not on the twenty-first of December, which most people believe is the shortest day of the year, but on December fourth or fifth, which is the time of earliest sunset, and if you observe in the newspapers of today the [time of the shortest] sunset, it is usually the fifth of December, and by the twenty-first, the sun is setting several minutes later than it does on the fifth of December. This excited me very much because I had been told the shortest day of the year would be the twenty-first of December, and therefore the sun would set the earliest on that date. As I grew up, I learned the reason for this; it fascinated me even in those early days, throughout school, which I began at age six in the winter semester, January, 1900.

Because I began in the winter semester I was sort of out of step all along, you know, because you finish a grade at midyear instead of in the spring. But from the very first grade on, I diligently sought to be at the head of my class. This was ambition. I was afraid of my teacher, I remember, the first day, but I got over that scare soon, and very soon I felt that, well, I've just got to excel in spelling and reading and reciting poems and in the children's program. I was an activist in all of these things at that time but definitely ambitious to be at the head of the class, and generally I succeeded.

I finished the eighth grade in January, 1908. That meant, also, that I began high school at midyear. I enrolled at the Brigham Young University. Of course, that was the only place then. There was no public high school, so far as I know, at that time, although one came along very soon. But I think more youngsters who went on to higher education--and high school was considered higher education at that time--went to the Brigham Young Academy, as it was called then. It was both an academy and a college, and high school and college were continuous. In my case, it was a seven-year continuum, seven-and-one-half years, I think, to be exact, and no pause or exercises of any kind at the end of high school. Probably in my fourth year, I was taking as many college courses as high school courses, and if you planned to go on to college, you just didn't bother about getting a high school diploma, just as in graduate school today, many people don't stop to take a master's degree if they're going on to the PhD degree. So that college and high school together was a single event in my life, a continuing event.

It might be worthwhile to say a little about the background of Brigham Young University at this point. The first president of Brigham Young University was Karl G. Maeser, and Maeser was the name given to the elementary school which I first attended, as I have previously related. Brigham Young, at the time Maeser was asked to go down to Provo, or "called" to go down to Provo [Utah] is about what it meant, because the president of the church made official calls and these calls normally were obeyed, although in this instance Maeser, I know, was delighted to accept the responsibility of starting this new institution of higher learning, which was to be the prime church school of the Mormon Church. Brigham Young warned him to teach all subjects, even mathematics, with the spirit of God, and so this spirit really pervaded Brigham Young University throughout the seven-and-one-half years that I attended high school and college there. During that time, you were required always to take a course in theology or religion. This was a standard requirement, and it always followed devotional exercises, which would correspond to chapel in most other institutions, and chapel was every day.

During my days there, the president was George H. Brimhall, who played a very important role in my high school and college education. Brimhall had a little sermon, or as he called it, sermonette, for each of these devotional exercises. We were always called together at these devotional exercises, and prayer was usually pronounced by someone who was called with no advance warning. And even students were called to the podium to offer the opening prayer.

I recall one such incident that was rather amusing. The call came one morning to the president of the college student body, Einar Anderson by name, and a physics major, a person that I knew very well. He was caught, I think, somewhat unprepared and he came to the podium and his prayer went something like this: "Oh, God, oh, God, oh, God, . . . I can't pray." This is the only instance which I recall during my seven-and-one-half years there that anyone really made a failure of his attempt when called on to pray. I, myself, was called two or three times during my college years to offer the opening prayer at devotional exercise.

MINK: It was a custom of the Mormon Church to have extemporaneous prayer?

KNUDSEN: Yes. They were always extemporaneous prayers.

MINK: How many people were enrolled in the university at this point? I imagine rather a small group.

KNUDSEN: I remember definitely my graduating class at college made up of only twenty-nine students who graduated that year. College had been going only maybe five or six years. It was the high school that was called Brigham Young Academy, and it was just a high school. College began, my guess would be somewhere around 1910 or 1911, maybe a year or two earlier than that, so that the devotional exercises, which would have all students present, was held in a room that would hold five hundred at most, and it was approximately filled. So altogether it was of that order of size, with most of the students at the high school level. And by the time I graduated, twenty-nine seniors got their degree, so that there probably were not more than, oh, 150 college students altogether, maybe 200, but certainly not more than that. In contrast, the present (1968) student enrollment at BYU exceeds 20,000 at the college and graduate levels.

MINK: Do you want to stop? [tape off]

KNUDSEN: Did I tell you in our first interview about my studying of geometry?

MINK: Somewhat. I think you said that you had studied geometry while you were herding cows.

KNUDSEN: Yes. Well, I think we won't go into that incident again then. But from the time I began my high school work, it was quite clear that I wanted to pursue a career in engineering or mathematics or science. I didn't know precisely. The first desire seemed to be in engineering, but the vice-president had, I believe I told you, invited me to go on for a degree in mathematics with the assurance that there would be a position for me, a chair in mathematics at the Brigham Young University, and that, of course, inflated my ego very much. But when I enrolled for the first year of college physics, Harvey Fletcher had just returned from the University of Chicago, where he had won a PhD degree in physics working with Professor [Robert A.] Millikan. At the time Fletcher was at Chicago, which ended in 1911, so he probably was there from 1908 to 1911, Millikan was working on two of his most important projects: one, the measurement of the elementary electrical charge, the electron, and the other, Brownian movement, which you know results from collisions of molecules against the little particles of tobacco smoke or oil drops, things of that sort. The oil drop was used in both of these experiments, the determination of the elementary charge, for which Millikan received the [1923] Nobel Prize. Fletcher was the assistant to Millikan on both of these projects and played an important part as an assistant. And sometime, I hope, the story of Fletcher's contribution to the measurements will be better known. Fletcher's own account has been dictated by him in a record that is now in the Niels Bohr Library of Contemporary American Physics in New York at the American Institute of Physics, that maintains such collections. And I happened to interview Fletcher about his contributions to contemporary physics; Fletcher went into great detail in describing just how he became interested in the oil drop experiment and the parts he contributed to it.

Well, in 1911, Fletcher came to Brigham Young University in charge of both mathematics and physics. I think he was the whole staff for both mathematics and physics, although until 1911 there was another man in mathematics, Chester Snow, a brilliant mathematician, under whom I learned differential and integral calculus. But all of my upper-division mathematics and all of my lower-division and upper-division physics were taught by Harvey Fletcher. And that first course in physics, which is a freshman-sophomore course, by Fletcher convinced me definitely that it wasn't to be engineering, it wasn't to be mathematics, but it was to be Fletcher's type of physics.

He invited me in my senior year to be his research assistant, and also invited me as a senior to be a teaching assistant, which is rather unusual for an undergraduate to have that kind of experience. So in my final year in college, I was both a research assistant and a teaching assistant, and with Fletcher's eminence in that field, this had a definitive effect in settling my future. I knew it was going to be physics from then on, but I had to pause for a while to fulfill this Mormon mission that I spoke about in our last session.

MINK: When you say, "Fletcher's type of physics," would you go into that just a little more?

KNUDSEN: Well, he worked on the measurement of the elementary charge and on the Brownian movement, and the work that he was doing at the Brigham Young University was on the Brownian movement. The technique was very interesting, and it was certainly fascinating to me. The purpose of Fletcher's Brownian movement experiment was to determine how many molecules there are in a given volume of gas. It's called the Avogadro's constant normally, or the Germans refer to as the Loschmidt number, and this was quite exciting, watching a little droplet of oil. The experiment consisted of a sensitive technique that was used in measuring the elementary charge on the electron. There were two plates, big, massive, cylindrical disks about ten or twelve inches in diameter, and they were about two inches apart. An oil droplet was sprayed in there with an atomizer, the regular kind of atomizer, the rubber bulb of which you push, and some small droplets of oil go in between the plates. And one of these droplets of oil is focused with a telescope so that you can see it, and it's illuminated from the side, and it looks almost like a star in the firmament. It's just as brilliant as that. And the plates are charged with a difference of potential, an electric potential, of the order of 700 volts. I believe it was 700 volts that Fletcher used at that time. I know it was enough that when I accidentally got my arm in touch with it, it knocked me off the stool. It was the first electric shock I had felt, and it was a violent shock. It was that hard, and my arm hurt for a long time after that, but the shock actually knocked me down.

MINK: It was the first electric shock you ever experienced in your life?

KNUDSEN: It was the first shock, and it made me cautious, I assure you [laughter], thereafter in handling things of this sort. This potential was supplied by little lead cells of the type you use in ordinary storage batteries, but they were small ones too, because all it had to supply was the potential and not a current. But then, X-rays were shot in there, and these X-rays ionized the air and electrons would be knocked off from the air molecules and the oil droplet would capture one or two or more electrons; and then you had an electrical charge on this oil droplet, and it moved under the electrical field of this 700 volts across the condenser plates, and you watched this droplet as it dropped. It didn't drop uniformly, but in a zig-zag path. And the experiment consisted in measuring how many seconds were required to go across each little division of distance, a millimeter, see. And so you made, maybe, a hundred such time measurements. One might be 3.5 seconds, the next 4.2 seconds, then 5.3 seconds, and so on; these irregularities in time were the result of bombardments of randomly speeding air molecules against the little oil droplet, and that made it drop with an irregular speed. It was pulled down by the force of gravity and the electrical field, in combination. But once it was charged, you could turn off the electric potential, and then the thing simply fell down under the force of gravity, but it fell in this zigzag path with difference in time for going the same distance toward the lower plate. By measuring these deviations from the average time, you can determine how much momentum the air molecules are contributing to this much larger oil droplet, and from that you can determine, knowing the mass of the air molecules, how many are colliding. And from this, you determine how many air molecules in a cubic centimeter, or in a gram-molecule. We calculated that there were some 6.0×10^{23} in a gram-molecule. And the result was in good agreement with other determinations that had been made from chemical analyses, and so on, of the number of molecules in a cubic centimeter, or in a liter, or 22.2 liters, which is a gram-molecule. [tape off]

MINK: I wanted to ask you something about the apparatus and the method of assembly and when it was necessary to improvise.

KNUDSEN: Apparatus, in those days, was always much of a homemade affair, and the apparatus that Fletcher used for determining Avogadro's number in these Brownian movement experiments was really the same kind of apparatus that Millikan used for measuring the elementary charge of the electron, and almost all of it was improvised. Parts of it had to be made in the shop. These disks that were used for the condenser plates, something like ten inches in diameter, and so on, would have to be made in a precision shop. And the voltage which was used had to be a DC voltage, made with little lead cells, but they had to be purchased. They were small cells not much bigger than your thumb, of two volts per cell, so you'd have to have say 300 to 350 of these. This was the one item you'd have to buy. But that wouldn't be very expensive, and certainly less than one hundred dollars for all of those, maybe fifty dollars. The rest was pretty much improvised from equipment available in the laboratory. The oil which was used, of course, was inexpensive and so was the atomizer. Most every place had some means of generating X-rays at that time, so this was something that was available. If you didn't have X-rays, you might have a little specimen of radium. Either one would supply the means of ionizing the air so that you would get an electron free to hop onto the oil droplet. The viewing telescope was usually a simple piece of apparatus, and this simply had to have in it a little glass scale divided into millimeters or tenths of millimeters. So this, essentially, was the type of apparatus that Millikan and Fletcher had devised, and I'm sure the whole thing didn't cost more than fifty or one hundred dollars, not counting the labor. And it was with this kind of equipment that the first measurements were made of these very important things in physics, in all of science today.

MINK: You mentioned the shop. I've been told that we have one of the finest shops here in the Physics Department, one that will manufacture just about anything you need for an experiment. Did you have such a shop at Brigham Young, or did you have to go out into the town to have equipment made?

KNUDSEN: I'm quite sure these plates were not made in the physics shop. The shop was just a repair place, at that time, to keep up the primitive equipment. There was no air conditioning equipment, of course, or no elaborate equipment, and even the electrical equipment for lighting was very simple. And for heating, we did have a central heating plant at the Brigham Young University in those days, but throughout most of my elementary school, it was the coal stove that supplied the heat. So there was certainly no [elaborate] shop, and these plates had to be made in some factory in the community. I just don't know where Fletcher had them made, but certainly they didn't have shop facilities for doing even so simple a thing as making two cylinder plates, ten inches in diameter. [Tape off: Professor Knudsen was asked to elaborate further on Professor Harvey Fletcher's influence on his early years.]

Harvey Fletcher, beginning with that first course in physics that he taught, had a very significant influence on my whole life. My professional career, and even, in many respects, my moral and ethical affairs were all influenced beneficially by my association with Harvey Fletcher, which began then, and has continued on an increasingly friendly and intimate basis, and especially on a professional basis, throughout much of our lifetime. Fletcher is approximately ten years older than I, which means he became eighty-four years old last September (1968), and he still is active in research at Brigham Young University. He taught at Brigham Young University, only from 1911 until 1916, and so, fortunately, he was there during all of my college work. And I believe I told you that all of my college physics [courses], lower-division and upper-division, were taught by him. Among the upper-division courses he taught, there was one in kinetic theory, which followed very closely the course that Millikan had taught him in kinetic theory, kinetic theory of gases, more specifically, which included also molecular physics; electricity and magnetism, a theory course and a measurements course; a course in physical optics, followed by a course in spectroscopy and experimental optics. I remember, particularly, that he had managed to get a budget large enough to buy a Michelson interferometer. This was the instrument that [Albert Abraham] Michelson had devised, you may know, and used so successfully for measuring the velocity of light, and it has become a very important instrument in optical physics. It was work on the interferometer and related things that led to the Nobel Prize that was given to Michelson, the first Nobel Prize in science to any American. Michelson and Millikan had influenced Fletcher very much, and later, also were to influence me very much. But we're talking about Fletcher now, and his influence continued. He also taught courses in mathematics. Vector analysis, advanced calculus and differential equations were three courses in mathematics that Fletcher taught, besides these courses in upper-division physics. Fletcher was a superb teacher. He taught four or five courses everyday, five days a week. This was a normal teaching load. He lived very near the campus and liked to get started early in the morning. His earliest course was usually at 6:45 [A.M.], even in the winter months. I lived about a mile from the Brigham Young campus. I usually rode my bicycle, often in the snow and the dark of the winter mornings. It seemed very, very early, but Fletcher was always there on time; however, he'd go home for breakfast after his 6:45 to 7:45 [A.M.] class. But he got started that way, in spite of the early hour, and, he was a very inspiring teacher.

I believe he was the only PhD on the faculty at the time he was recruited to Brigham Young University, so he was conspicuously the intellectual person on the faculty and at faculty meetings. I was permitted to attend them when I was teaching assistant. He was often arguing for higher standards of instruction and higher standards of scholarship at Brigham Young University. His service there from 1911 to 1916 had a salutary effect on the upgrading of the scholarly standards at BYU. He was an ardent pleader at these faculty meetings that we must have more PhDs on the faculty, that we must upgrade the work that's being done on this campus. Some of the old faithful members were talking about the day when this would be one of the great universities of the world. It didn't seem like it would be at that time to students such as myself, although I was extremely loyal, and I don't believe I could have had a better professor of physics anywhere than I had at Brigham Young University in Harvey Fletcher.

Well, his course in differential equations was a tough one. It was the most difficult course in mathematics that I had as an undergraduate, and Fletcher piled on the problems. He was a great believer in solving the problems, and for the final examination I was a bit piqued because he assigned twenty-five problems that were to be solved in a period of one week, just before graduation. It was a tradition at Brigham Young University at that time that seniors would be excused from their final examination at the graduation time. This was a reward for your faithful services over the years, and you were allowed to have fun during this last week or so. But Fletcher didn't believe in that. He believed in final examinations, and he gave these twenty-five equations. I know I was disturbed a bit because I wanted to have fun with the other students this last week of my college days because I realized this was quite a turning point in my life. I had accepted a call to go on a Mormon mission, and I knew that this would be followed by more serious work of one sort or another. This, in a sense, was the end of a pleasurable part of life, of youth, of dances, of hikes, of meetings and things of that sort. But as Fletcher said in his dedication of this building [Knudsen Hall], "Knudsen somehow found time to have a good time along with his devotion to his studies." I did manage to pull down A's in all the classes I had under Harvey Fletcher, and he paid a very fine tribute to me in the dedication speech that he gave on the occasion of the dedication of this building.

MINK: Well, I take it the twenty-five equations didn't bother you, from the standpoint of solving them. It was just the fact that you didn't like the idea of having to solve them.

KNUDSEN: I didn't like to have to solve them in that period, and so in my anger I set to work at them, and

I worked continuously until the next morning, all through that night. I brought them to Fletcher in the morning, and I was just a little bit peeved. I said, "Here are your problems." He said, "What?" I said, "Here are your problems." He said, "Aren't you going to solve them?" I said, "I've solved them." He said, "When did you solve them?" I said, "I solved them in the last twenty-four hours." He said, "I don't believe anyone can solve those in twenty-four hours. They're to take an average of an hour for each one. When did you sleep?" I said, "I didn't sleep. I'm going to sleep tonight." [laughter]

And that was that. He always reminds me of this, this anger that I had when I brought those twenty-five problems, and his amazement that I had been able to solve these twenty-five problems. In differential equations you know when the answer is right, because you can just work backwards a little way, you see, integrate them, and you know if you're right. I knew I had all twenty-five right.

Well, Fletcher left Brigham Young University to become a research worker for Bell Telephone Laboratories in 1916. I had begun my Mormon mission a year before, and Fletcher stopped off at Chicago to have a talk with me on his way to New York. We had a very pleasant reunion there, and he encouraged me to go on again with my advanced work in physics. But toward the end of my mission, Fletcher urged his boss at the Bell Telephone Laboratories, a Dr. R. L. Jones, to interview me with the possibility of my joining what then was called the Western Electric Research Laboratories but later became the Bell Telephone Research Laboratories.

MINK: Why did Fletcher decide to leave Brigham Young and go back to the Bell Telephone Laboratories?

KNUDSEN: Well, I'm sure salary had something to do with it. The University of Chicago and Millikan had a very important role in the staffing of the research laboratories of Western Electric, really Bell Telephone Laboratories, we shall call it hereafter, because that's what it became. Millikan was advisor to the Bell Telephone Laboratories in staffing matters. He had recommended as the president [director of research] of the laboratory a man by the name of H. D. [Harold DeForest] Arnold, who had earned his PhD degree at Chicago a year or two before Fletcher, maybe one year before, but who knew Fletcher very well. Arnold knew that Fletcher was the first person to receive a summa cum laude at the time of his PhD degree and that, therefore, he was an outstanding man to have at the Bell Research Laboratories. Arnold persuaded Fletcher to accept this position at the Bell Telephone Laboratories. I don't believe Fletcher was dissatisfied with the Brigham Young University. He was more attracted by the opportunities at the Bell Telephone Laboratories.

MINK: He was in no way beholden to the church?

KNUDSEN: No. He was not. The influence that dominated Karl G. Maeser when he was told to teach mathematics with the spirit of God was quite as dominant at the time Fletcher was there. Fletcher continued in his devotion to the church and his loyalty to Brigham Young University, a loyalty which has persisted throughout all his life. He returned there again to become Dean of Engineering and built up their Engineering School, after his retirement from the Bell Telephone Laboratories. After his retirement from the Bell Laboratories he was professor at Columbia University, and when he reached retirement age at Columbia, he returned to Brigham Young University. He had received many honorary degrees around the country by the time he had returned to BYU for his latest career.

Well, R. L. Jones came to Chicago and interviewed me, probably two or three months before I had finished my term of twenty-seven months as a missionary. I remember this interview was a very pleasant one. He had been told by Fletcher that I was a good recruit for the laboratories. He interviewed me and more or less indicated that he was satisfied with my qualifications and wanted to know if I would be interested in coming to the laboratories. I was delighted at such a prospect, and he said, "What salary do you think you should receive?" I hesitated. I didn't expect anything like that, for me to suggest the salary I should receive. I remembered that I had received, maybe, two dollars a day for the work I had done before. "Well, I'm worth more than that." With, really, my heart way up in my throat, I said, "Well I think I ought to get $100 a month." He said, "Well, that's very fair. We'll give you twenty-four dollars a week, which figures $104 a month." And so I began my work at the laboratories and my close association with Fletcher, although I didn't work in his department.

There's one other matter I wanted to mention in my days at Brigham Young, and that has to do with the former president of the Brigham Young University, George H. Brimhall, who also exercised a great influence on my life, morally, religiously and in other respects. Maybe one or two of the anecdotes of Brimhall's, many of which have stayed with me through life and have been really important things in guiding my life, are worth recalling. These anecdotes or sermonettes were always original. Sometimes they were in the form of poetry. Usually they were in the form of a parable or story of some sort. One had to do with race horses.

Brimhall was much interested in horse racing. The only kind of horse racing we had other than on the Fourth of July, when we had real horseback riding in the races, was usually in a sulky. You know, the drivers were there with these little sulkies, and this was the kind of race I saw in my youth. The race track was next to our farm, so it was quite customary, on occasion, to go to the race track. Brimhall went there very often. One of his parables had to do with two of the horses on the track. One was named Black Eagle and the other Ginger. Black Eagle was the type that just couldn't keep the track. He would bolt for the grandstand, which was a favorite expression used in that day, which means you break, say, from the normal trotting pace, the pace that's allowable in the race, and the horse goes into a gallop in the end. And Brimhall said:

> Black Eagle and Ginger were matched against each other. They were two of the finest racing horses on the track at Provo in those days. Black Eagle, though, just couldn't resist the temptation at the end to bolt for the grandstand. But the driver of Ginger--his name was Spicer, an appropriate name--would keep his tight rein on Ginger, and as he was approaching the final gate he'd say, "Keep the track, Ginger. Keep the track, Ginger," and he'd tap him gently with the whip. And Ginger would keep the pace all right, but Black Eagle broke pace as he was approaching the gate, bolted for the grandstand, and therefore disqualified.
>
> Years later I returned to the race track, and I saw a beautiful horse that reminded me very much of Ginger. "What's that horse's name?" There was

> a stallion then at the race track, and I found his name was Ginger. He was beautifully groomed, you know, and the tail was bound up in ribbons, and so on, a regular display horse there at the race track. I was walking through the town, and I saw a black horse hitched to [a grocer's delivery wagon]. I said, "Well, I think I recognize that horse." I went up to the delivery boy and I said, "What's the name of that horse?" He said, "That's Black Eagle, he used to be on the race track."

This was the lesson. He ended up by saying, "Don't bolt for the grandstand." Now, you see, stories like that stay with you. It's a simple story. At another devotional exercise his sermonette was an original poem of his that maybe I can recite. It was called, "The Aim of My Existence." And it's one that has stayed with me more than fifty years. I haven't repeated it for a long time. Let's see:

The aim of my existence is that I may have
    more joy than sorrow in the sum of life
That I may seek and find the truth and in
    the search be glad
Be much more moved by love of good than by
    the fear of bad
To freedom gain and ne'r forget that others
    too have rights
That mine turn in where theirs begin, no matter
    what's my might
To go the road that God has gone who once was

mortal man of perfect type
And says to me, "Be ye like me, do ye
as I have done
If God had not intended that I divine
might be
Then why confer the image of divinity
on me
Thus making my appearance here a necessary
fraud
A being in his likeness here that never
can be God?

Among my favorite professors during my years at Brigham Young was the professor of English, Professor Alfred Osmond. Osmond was a great admirer of Shakespeare. I had been introduced to Shakespeare, I believe I told you, in the seventh grade, when our teacher read and explained Macbeth for us. Osmond really had memorized many of the favorite speeches in the works of Shakespeare, and he regaled us in class, and even at devotional exercises. He was often called upon to give the gravedigger's scene. He was superb at the gravedigger's scene in Hamlet. He was also very good as our teacher in English 1, which was freshman English. He hammered grammar into us very, very effectively.

I recall, this was the sleepy hour of the day. It came just after lunch, but he managed to keep us awake in this manner: for example, I know of one occasion he had a girl reading something from Shakespeare, and she came to the word "belly button." She hesitated, and he said, "belly button. Yes, belly button. That's a good word. Use it often." And so he had this way, you know, of putting this girl at ease on a word of this sort, which might be a little embarrassing to her. I recall his hammering into us the use of descriptive clauses and restrictive clauses; and he'd reel off something: "Is that a descriptive clause or a restrictive clause?" And we'd say, "Well, that's a restrictive clause." And if we were right, he'd say, "Yes. And what do you do with a restrictive clause?" "Well, we'll have to set it off with commas," we'd reply.

"That's right. And anyone that says you don't will be a son of perdition." [laughter]

And so you remember lessons, you know, that are taught that way. Years later, quite a few years later, after Osmond was near retirement, Fletcher and Fletcher's brother-in-law, Carl F. Eyring, who was professor of physics at Brigham Young at that time, and I met together at Strawberry, Utah. Osmond was our host on this fishing trip at Strawberry Lake, which was a favorite place for fishing, and was where I had my first lesson in fishing from my father. I believe I told you about it. Osmond had kept a comfortable tent up there, and the four of us went up this time in, I think, Fletcher's automobile, and it took an hour and fifteen minutes instead of two-and-one-half days, as it had taken by wagon on my first trip there.

Osmond was entertaining us. We'd had our dinner, and we were playing various simple card games, which ended up pretty late in the evening with a card game called smut, because the loser would have his face blackened with smut. Osmond, recognizing that Fletcher and I were the two visitors from out of town, insisted that Fletcher and I have the bed in the tent, and he and Eyring were to sleep outside. We retired quite late, at probably 11:30 or so, and we were to get up at 3:30, at the crack of dawn, to go fishing. I don't think we'd been in bed more than an hour, when we heard a shotgun outside, and Osmond said, "Damn that rat! I almost got him." A little later we heard this voice of Osmond out there calling to us. "Doctors, oh doctors." He called us to attention, and then asked us how we slept. He was telling us about his dreams. He said, "You know, I was dreaming last night. Dreamed about women, nothing indecent, I'd have you understand, just certain indiscretions." And so when we were out fishing a little later that morning, Osmond reviewed his lessons in English 1. He had taught all three of us our English 1, and we got his same old stories. He recited the gravedigger's scene for us, as we were fishing, and the story about the descriptive and the restrictive clauses, and all the rest of his lessons on grammar. So we got a good review of our English 1, maybe ten to twenty years after we had had his courses. [tape off]

[George] Brimhall was interested in all students. His door was always open, and I know on several occasions I went to his office with problems that had to do with courses or with personal problems, and he was always a very wise counselor and deeply interested in his students. He taught a course in theology, which I was fortunate enough to take, and this was really a liberal course, not a doctrinal course. It had to do with the immortality of the soul. It had to do with should the Jew and Christian intermarry. He was a well-read man. This course really dealt with contemporary religion more than with Mormon theology. A lot of people thought most of our courses were strictly orthodox. Some of them were, but Brimhall's course, especially, was of a liberal nature. He was, of course, a fundamentalist, but I guess he also believed in the theory of evolution. This theory was taboo among orthodox Mormons. A few years before, three faculty members had been dismissed from the faculty because they taught evolution. By the time I was a college student, belief in Darwin's evolution [theory] was in transition, and although Brimhall didn't proclaim that he believed in evolution, it was quite apparent that he was having struggles with his orthodoxy at that time. He was a very well read man, and while I never heard him preach evolution, it was quite apparent that these three professors would not have been discharged with Brimhall's sanction at the time he was president of Brigham Young University.

Well, Brimhall bade me farewell. I called to see him before I left for my mission, and he said, "I'll write to you from time to time." I thought, oh, that's a kind gesture, but he never will. But I know I received at least two letters from him, and these letters were intended to bolster my faith in the work I was doing as a missionary. I think he realized that one would have moments of doubt. I remember he referred once to the testimony that Jesus gave Thomas when he showed Thomas the hole prints in his hand and his feet, where the spikes had been used to nail him to the cross. And Brimhall's letter said, "Even Thomas required this convincing evidence, so look for evidence everywhere for the principles you are preaching as a young Mormon missionary." His letters to me and the lessons in religion and theology which I had from him were important in my missionary experience. I also used them extensively in later duties that I had in the Mormon Church, teaching

Sunday school classes, mutual improvement classes, and matters of that sort. Brimhall, I'm sure, had more influence on my religious life after leaving college than anyone else.

MINK: I think you were saying the other day that especially in the first stage of your mission work in Chicago, where you were sidewalk preaching, you felt that he gave you ammunition which made you able to do this better than you might have.

KNUDSEN: Yes. Very definitely. His letters were models of evidence that would bolster your faith. He realized a young missionary might have moments of doubt, and he didn't want me to lose my Mormon faith. But he wanted me also to reach out for evidence in all directions and, again, he encouraged me in these letters to go on with my higher education.

SECTION III

KNUDSEN: Last week we referred to the interview I had with R. L. Jones that led to my employment in January 1918, as research engineer at the Western Electric Research Laboratories, now the Bell Telephone Laboratories. In that interview I was somewhat concerned about my draft status, and I reminded Mr. Jones that as a Mormon missionary, I had been exempted because I was classified as a minister of the gospel. R. L. Jones assured me that the people who were working on war problems at their research laboratories were exempted, and he was sure that my status would be exemption, classification five, I believe it was called then, which meant essential war services. And as a matter of fact, I was given that classification, and one of the earliest projects I worked on at the Bell Telephone Laboratories had to do with the development of a loudspeaker system that could be carried by airplane and used for giving orders to troops from the air. Such a loudspeaker system was actually field tested.

Loudspeakers were relatively new at that time, and my part was to help develop an instrument that would be loud enough that it would overcome the noise of the airplane and the battlefield, so that the airplane could actually communicate orders to troops. This was used, but I wouldn't say that it had much to do with the winning of World War I. This is typical of the type of work that I did in my early months there and during the war, which ended in November of 1918. I was working definitely on problems that related to the war.

MINK: What would have been the requirements of such a loudspeaker and what did you have as a basis to go on in perfecting it?

KNUDSEN: Well, this was quite soon after the development of vacuum tube amplifiers, which would have to be used for a power amplifier to actuate a powerful loudspeaker. The Bell Laboratories had been interested in the development of loudspeakers at that time, and we really worked on developing a loudspeaker which was somewhat more powerful than one that was useful at that time, and a corresponding vacuum tube amplifier that would actuate this loudspeaker. But it was a Western Electric horn-type speaker of those days. In testing it we mounted it on the top floor of the Bell Telephone Laboratories in a window and tested it down in the street, where there was a lot of street traffic noise. This was a very loud reproduction of speech at this time and did actually override the noise of the airplane, and it was loud enough that it could be heard in certain aspects of battle. But when the attack became furious, the level of the gunfire noise would mask the speech. This was the first project I worked on that had anything to do with the war.

MINK: Did you try to simulate battle noise in testing it?

KNUDSEN: No, we did not. We didn't have any simulation of battle noise, but we had some experienced people who knew about the battle noises that we would have to contend with.

MINK: How many decibels would there need to be, and so on?

KNUDSEN: Yes. Decibels didn't exist at that time. Decibels came very much later. I'll maybe tell you something about that when we come to the organization of the Acoustical Society of America.

MINK: I'm still interested in this loudspeaker, though. What was the amount of input, and how did it operate? Did it operate off a battery, or did it operate off the generator in the plane?

KNUDSEN: Everything was battery operated. We had to carry a battery to energize it at that time, and there was a microphone. The Bell Laboratories had been instrumental in developing high quality microphones. Condenser microphones had been developed in 1915 at Bell Telephone Laboratories. It was a Western Electric condenser-type microphone, connected to what we now call a preamplifier and a power amplifier, to actuate the loudspeaker, not unlike loudspeakers that are used for public address systems today.

The next problem I worked on at the Bell Laboratories was in connection with the noise generated in vacuum tubes by reason of the vibration of planes. This was at the beginning of communications between ground and a plane in the air in guiding operations of the airplane for bombing and attacking and things of that sort in World War I. The planes were rather primitive. The vacuum tubes also were primitive at that time, and, especially, they were subject to defects by vibration. Certain tubes would be more defective than others. I participated in the devising of a shaking apparatus that would simulate the vibration of the airplane, because very often the tubes would fail because the vibration of the plane would actually damage a tube to the extent that it was no longer usable in the plane.

MINK: What was it that was damaged in the tube?

KNUDSEN: Normally the filament. Sometimes the filament would actually break as a result of the vibration, and often the vibration of the filament, grid or plate would cause excessive tube noise. We devised isolating structures that would prevent vibration, but we also had to test all tubes before issuing them for use. I remember for a period continuing almost thirty-six hours--all of one day and all of that following night, and all the next day--a crew of us were just testing tubes by tapping them in a special vibrating holder we had devised, and discarding those that we knew would fail. This was a service for the Air Force that we conducted in the laboratories at that time.

MINK: Was there a great pressure to get out the tubes?

KNUDSEN: Yes, indeed. That's the reason we worked night and day to get usuable tubes out as early as possible.

MINK: Did they have Air Force inspectors on the scene at this time, or was this something that came later?

KNUDSEN: Well, there was one officer, a major, I believe his rank was, from the Air Force, who came to assist us. Primarily, this thirty-six hour session vibration test was a matter of discarding tubes that we knew would fail.

Now the third project I worked on before the war ended in November of 1918 was in connection with cable noise or interference resulting from so-called earth currents, that is, currents of electricity that vary with time, somewhat cyclically, in such a nature that they caused an interference on the transatlantic cable. I was assistant to the engineer in charge of the project, Harry Hitchcock. There were two transatlantic cables at this time, one of which, the St. Pierre Cable, had broken some 1700 miles east of Orleans, Massachusetts, which was the United States terminal of this cable. This 1700 mile segment was available for the study of these earth currents, and the earth currents were serious at that time, because they limited the speed at which you could send telegraphic messages. The volume of messages, among others, that President Wilson had to transmit to Lloyd George and Clemenceau and others there was clogging the line, and they were in desperate need of a speedup in the one operating cable. Our investigation of these earth currents on the 1700 mile segment of the St. Pierre Cable was done to in some way limit these earth currents or to find out the nature of them, so that we could hopefully speed up the transmission of telegraphic signals over this transatlantic cable.

We arrived in Orleans [Mass.], I remember, one early morning before sunrise. Our apparatus had come by truck, and we had gone by train to Boston and then continued with the truck to Orleans, Massachusetts, which is near Chatham, Massachusetts. Incidentally, Chatham was the one site in the United States which was hit by a German missle. A submarine offshore had fired at the United States [coastline], and the missle hit in Chatham, Massachusetts. At the time we were there, we visited the site. There was a fence built around the projectile, which had partially embedded itself in the ground. A prominent sign near the site claimed that this was the only missile from a German submarine, or any other German weapon, that reached United States soil.

MINK: It did no damage.

KNUDSEN: It did no damage. [laughter] It hit in an unoccupied portion of Chatham, but that's not very far, you know, from where the Pilgrims landed.

Orleans was a rather interesting community. I think the population was between 500 and 600 in the wintertime. It was a resort center in the summer, and, among other things, the facilities for [board] and lodging were very primitive. We lived at the boarding house that also accommodated the school teachers, two or three "schoolmarms," who were teaching in Orleans. Among the facilities that were in the community at that time, we were told there was only one bathtub, and it was certainly not in the home where we were lodged. Saturday night we took turns in a round washtub, to have our weekly bath. And I'm not sure that all others in the community had that facility, because as the season progressed, the olfactory sense at the weekly dance became worse and worse.

Well, to go on with the research, these earth currents that we were investigating are very feeble. It was necessary, therefore, to have a very high gain amplifier to magnify these feeble earth currents to the extent that they could be recorded in any way. We wanted to know their frequency, their amplitude, how they depended upon the time of day and on natural electrical and magnetic disturbances, such as aurora borealis. That was known at that time to be a very serious interference, as it is today. It interferes with radio communication also, as you know. The amplifier that was necessary turned out to be one that we developed, somewhat, right on the job. It certainly had a record at that time for the extent of its amplification. It had, in terms of decibels, a gain of 130 decibels, which in power means that it increased the power ten to the thirteenth fold, and so it was a very high gain amplifier. It was mounted in a cabinet [that] looked more like a coffin than anything else, and altogether, it was as heavy as a metal coffin with a heavy corpse in it.

MINK: What was its size approximately?

KNUDSEN: It was about six feet long, a foot and a half wide and a foot and a half high. It had metal shielding in it, and it had batteries associated with it: the so-called A battery that operated the filaments, the B battery that maintained a plate voltage on the plates of the vacuum tubes--they were all three-element vacuum tubes, consisting of a filament, a grid and a plate--and also the grid [C] battery. Those batteries were all essential parts of the amplifier, but they were mounted outside the coffin, which contained the vacuum tubes, condensers, inductances, resistances, et cetera.

The high gain of the amplifier meant that it was extremely sensitive to all sorts of disturbances, and after our work had been progressing for a few weeks and a rainstorm had occurred, the amplifier became inoperative. The output of the amplifier, which actuated a telegraph siphon recorder, was not the characteristic oscillatory earth currents but a lot of "hash" that completely masked the earth currents. That is, there was noise in the system, internal noise that we call system noise. It became necessary to find out the source of this noise. We worked on this frustrating problem at least three weeks before we found the cause of it, and other experts from the New York laboratory came out to take a try at it. Finally, we found that the source of the difficulty was leakage through the floor, the wood floor, of the cable house terminal that we had our apparatus set up in. The rain and the salt in combination had made the wood floor sufficiently electrically conducting that there was a leakage from the B battery, which was high voltage, 150 volts or so, over to the C battery, the grid battery, which was only a few volts. This fed a spurious voltage onto the grid which made the vacuum tubes inoperative. Once we made the necessary correction of insulating the B battery, we were on our way again.

Incidentally, we were working on this project, trying to get the amplifier in proper condition, at the time of the first but false armistice of World War I and then during the real armistice, that ended in the celebration in Orleans, Massachusetts. The celebration itself, I think, was rather interesting because both of the men who could play a musical instrument had been recruited.

MINK: And that was barring you?

KNUDSEN: Yes, that was barring me [laughter] probably, but the only instruments and players available for them were a bass drum and a big bass horn. The only thing they could play was "As We Go Marching Through Georgia," so the celebration consisted of this two-member band that marched up and down the street with followers carrying an effigy of the Kaiser which was properly burned. This was part of the celebration of the victory that I remember. The victory was also, of course, tempered very much by the flu epidemic, which hit Orleans, Massachusetts, probably as hard as anyplace. They were hard pressed for medical help. My older brother had once told me that a doctor should "double" as an undertaker. I had never believed him, but at this time in Orleans the only doctor was also the undertaker. The death rate from influenza was very high at that time, but fortunately no one who was attached to our team got the influenza during our stay there.

As a matter of fact, within a week or two after the war was over, we were told that the work that we had conducted was no longer as high priority as it had been, and we were called back to New York. But we had conducted enough work at that time to indicate the principal source of the earth currents we were investigating. Among other things, we suspected that they were associated with magnetic disturbances in the earth that might be associated with extraterrestial, magnetic variations, and so we compared the siphon recorder signals that we got from the St. Pierre Cable with those we got from a very large magnetic loop, that is, a single loop of conducting, insulated wire that was laid out in a square loop, approximately a mile in length. I remember it was square, on level ground outside the cable station where we were working. We had only one siphon recorder, so we were not able to make simultaneous records, but we did obtain records in close time sequence. The records that we got from the electric currents induced in the single loop were quite similar to the earth current records we had obtained from the St. Pierre Cable, and it was reasonable to suppose that the variable currents induced in the loop resulted from similar variations in the earth's magnetic field. The variations of the magnetic field fluxing through the loop would cause a current to be generated in the loop, and it was this current that actuated our amplifier and the siphon recorder that we also used for recording the signals on the cable. Well, the similarity was close enough that it was probable that the main source of the earth currents that disturbed the transatlantic cables were variations in the earth's magnetic field. But other things were contributing as well. It was obvious that this variable magnetic field was not the whole story, but it was part of it, and a significant part.

MINK: Did your investigation lead to any developments to overcome this?

KNUDSEN: So far as I know, no. I was invited to stay on at the Bell Laboratories, when in September of 1919, I had decided to go to the University of Chicago to complete my doctorate program. I was to go with a team to Havana in the neighborhood of Havana where there were electrical storms of a considerable magnitude, as you know, in the fall months. My decision to go to Chicago prevented my going there, and I know very little more about what happened there.

One expedition to investigate earth currents that two of my colleagues and I made here, Dr. Leo P. Delsasso and Dr. Arthur H. Warner, who were members of the [UCLA] Department of Physics, was made probably in 1924 or 1925, during the Thanksgiving recess. We went out to Twentynine Palms because Twentynine Palms was freer from any electrical disturbances than anyplace we could find in Southern California at that time. We strung out a long wire there to see if it responded to earth currents; we were able to detect only a DC component that others had observed. Our system with a high gain amplifier, as we had it then, was not sensitive enough to pick up any variable earth currents that we could identify. Of course that's quite different from the cable 1700 miles long. But having obtained negative results on this trip, we abandoned any further research on investigation of earth currents.

MINK: You said that the St. Pierre Cable had broken.

KNUDSEN: Yes, cables sometimes break at sea, you know. I don't really know what was the cause of the break, but it was a segment 1700 miles long, and it was broken at sea.

MINK: Was it inoperative?

KNUDSEN: Yes. It was inoperative, it was just a segment. The remaining segment extended over to the other side of the Atlantic.

MINK: And they never repaired this?

KNUDSEN: I do not know. I have heard nothing further about that. It was not repaired, at any rate, during the time I was at the Bell Laboratories and no steps had been taken to repair it. But often when a cable does break--or we say, "The cable is parted"--attempts are made, and usually they succeed in joining the cable together again. This may have been done to the St. Pierre Cable. This could be ascertained, but I haven't ever taken the trouble to determine whether it was done.

MINK: Well, the fact that it was broken provided this opportunity for the study.

KNUDSEN: Yes, that is right. [tape off]

The third significant problem of a research nature I worked on at the Bell Laboratory had to do with the vacuum tube circuits for the generation of alternating electric currents. Vacuum tube oscillators were relatively new at that time. The vacuum tube had made the oscillator a possibility. Not very much was known about the characteristics of the oscillator at that time, although one of my bosses had worked out the theory of why a tube oscillates. He was the second removed from me up the scale. His name was R. V. L. Hartley, and his name is well known among electronics workers, especially of a generation ago, and was at the Bell Telephone Laboratories a first-class mathematical physicist. This was a beginning point of the research. I was assigned to work on certain factors that affect the vacuum tube oscillator--the conditions necessary for it to oscillate, its frequency, its amplitude, and waveform. If I simply list a few of the headings here, I think that will give an idea of what this dealt with. It, in a real sense, was the first research that I carried out on my own. The results were described in two memoranda for file I wrote during the summer of 1919 and which I later transcribed for instructing students in my classes at UCLA.

MINK: What is the document that you're now quoting from called?

KNUDSEN: The document is my lecture notes based on a "Memorandum for File." As a matter of fact, there were two memoranda: one dated July 10, 1919, and the second one dated September 22, 1919. A rough indication of its content will be indicated from the five subtitles or sections that these two memoranda deal with. The first is introductory and deals with the problem of the vacuum tube oscillation generator, the general nature. The second part is analysis of the oscillator equations for the frequency and the plate resistance, resistance of the plate of the vacuum tube, a three-element vacuum tube. The third section deals with the verification of the formulas, and that was the experimental research project that tested these formulas that had been derived, the ones I had derived in connection with one of the typical vacuum tube oscillator circuits. The next section deals with applications to the design of oscillation generators. That's the conclusion of the first memorandum.

The second memorandum deals with solutions of present (in 1919) standard vacuum tube oscillator circuits. There were eight circuit possibilities at that time, depending upon the tuning circuit, which normally consisted of a condenser and an inductance. The frequency of oscillation is determined by the inductance and the capacitance, and as you know, in most tuning circuits you vary the capacitance of a condenser. You have a dial on the condenser, and usually what you do is tune to a certain frequency. Besides the inductance and the capacitance, which determine the frequency of oscillation to a close approximation, but other circuit elements, especially resistances, affect slightly the oscillation frequency. I might show you sometime the equations that predict the frequency, and control the amplitude, of oscillation. They get a little involved. Well, here is one equation, for example, that gives the frequency for one of the eight circuits. It takes up practically a full page to write the equation.

MINK: And that's on page...

KNUDSEN: According to my lecture notes it's on page sixty-one of the second memorandum. The two memoranda together comprised some sixty-seven pages and some twenty-nine figures that were with it. It's a fairly comprehensive report on this topic. I'll paraphrase the first two paragraphs because I think it might clarify the nature of the problem. "In the absence of a satisfactory knowledge of the behavior of a vacuum tube oscillator, the investigation described in this and the succeeding memorandum has been worked out. It is known that the condition necessary for oscillations is that the input of the (vacuum) tube be coupled with the output of the tube by such a circuit as will impress upon the grid of the input of the tube a voltage equal to one over the amplification constant of the tube, and in phase, or very approximately in phase, with the voltage developed in the output or plate circuit." This is typical of a howling telephone or radio circuit; that is, sometimes when you hear your radio set or telephone howl, it means that enough of the output has been fed back into the input to make the circuit oscillate. For example, if an amplifier has a voltage gain of, say, a thousand fold, then if one-thousandth of that voltage is fed back to the input, and is in phase with the voltage in the input, the circuit will howl or oscillate. And this is the principle utilized in the vacuum tube oscillator. There must of course be a local source of energy to maintain this transfer of energy from the output to the input. This energy is supplied by the A and B batteries. The A battery supplies the filament current and the B battery the plate voltage. Furthermore, the frequency of oscillation will be approximately given by the inductance and capacitance coupling the output to the input of the vacuum tube, but the internal output resistance of the tube, the external resistance in the plate circuit, and probably the current in the grid circuit, all have a bearing upon the frequency. All of these circuit elements control the amplitude and also the waveform, but they have not been adequately understood, or at least they were not understood by my associates and me in 1919. The lack of such information was responsible for several undesirable features in the design of vacuum tube oscillators. For example, the frequency of a standard audio frequency oscillator is not independent of the plate potential, the filament temperature, the resistance in the plate circuit, and the amplitude of the oscillation. Further, for a fixed inductance in the tuning circuit the amplitude decreases and the oscillations ultimately cease as the capacitance is increased. Or as the capacitance is decreased, the other impedances in the external circuit remaining fixed, the amplitude may increase to the extent that the waveform becomes very poor. The purpose of the investigation, therefore, was to determine the causes of these undesirable variations and how, so far as was possible and practical, they could be remedied.

MINK: As far as you know, had anyone else undertaken research on this same subject?

KNUDSEN: As I mentioned at the beginning here, R. V. L. Hartley had initiated studies of this sort. He was my mentor, and it was he who suggested that I really carry on this problem.

MINK: This was at Bell, of course, that he had done this work. I was thinking of some other research organizations.

KNUDSEN: I think the Bell Laboratories at that time were ahead of the game. The only other place where research of this nature was really carried on in a serious nature at that time, at least in this country, was at the General Electric Research Laboratory, and even when I went to the University of Chicago later in 1919 I doubt that anyone there knew very much about vacuum tube oscillators or the factors which affect their operation. As a matter of fact, I was invited by Professor Millikan to give three lectures on vacuum tube circuits in his course in electron physics, during one of his absences from Chicago.

MINK: Well, this was a very important thing in radio, taking the howl out and so forth.

KNUDSEN: Also, it was important in determining what controls the frequency, precisely, because we were able to show just what effects these various circuit impedances had on the frequency besides those of the main tuning impedances, the inductance and the capacitance. These other circuit elements all affected not only the frequency of oscillation, but also the amplitude and waveform of oscillation, all of which were significant.

Incidentally, this research led to two patents at the laboratory, and they were the first patents issued in my name. But I didn't realize any financial benefit from it. You know, when you are employed by an organization of this sort, you assign your patent rights to the organization for one dollar.

MINK: Well, I think there's something that you can be justly proud of, and I wonder if you can tell me, as best you can, what was the organization of the laboratory where you worked and who were some of the people who were well known that were working there at that time?

KNUDSEN: The president of the Bell Telephone Laboratories was H. DeForest Arnold. Dr. Arnold had been a student of R. A. Millikan's at the University of Chicago, and had worked with Millikan on preliminary problems that led up to the measurement of the elementary charge of the electron. Millikan, at that time, was an adviser to the

Western Electric Company, and, I'm sure was responsible for recommending Arnold to take over the new position of organizing a research laboratory, which was then called the Western Electric Research Laboratory.

MINK: Well, then this was a very new organization you went to.

KNUDSEN: Yes, it was. Arnold had been there, well, probably from 1910, certainly not before 1910. So this organization began at about that time with H. D. Arnold at its top.

Among the top men at that time was Edwin H. Colpitts. Colpitts is like Hartley, a name that's associated with vacuum tube oscillators. There were Colpitts circuits and Hartley circuits. Probably I should have said it in reverse order, because I think the Hartley circuits were better known among electronic engineers than Colpitts circuits. But they are two of the persons who were near the top at that time. This R. L. Jones who hired me was also one of the vice-presidents at that time. Harvey Fletcher, who had been my professor at Brigham Young University, was in charge of the section on speech and hearing. There were other sections. One section I know dealt entirely with radio. The man in charge was Raymond A. Heising. His name is well known in early radio history in the United States. Irving B. Crandall was in charge of the work in phonetics, and his work in phonetics was extremely basic at that time. I have Crandall to thank for changing the pronunciation of my name, which was then K̲nūdsen. I had difficulty getting that version heard over the telephone. Crandall told me Nūd-sen could be heard better. It was, and that is why I made the change.

There were probably not more than, oh, 100 or 125 men in the research laboratory at that time. I worked closely with another man who dealt principally with vacuum tube amplifiers, and I dealt principally with vacuum tube oscillators, after my first year there.

MINK: Who was it that you worked with?

KNUDSEN: Well, the man who worked on vacuum tube amplifiers was Harry Reed, but my immediate boss's name was Robert C. Mathes, who was in charge of circuit research, and who retired a number of years ago and now lives in Santa Barbara. He visited me at UCLA a few years ago just after his retirement. I have learned much from Mathes. R. V. L. Hartley, who no longer lives, was one of the prominent men there. E. H. Colpitts, for example, retired before World War II, and I'll have more to say about him later, because Colpitts and W. D. Coolidge, who was then vice-president of General Electric was in charge of research, and Louis B. Slichter, who is on our faculty now in geophysics and Max Mason, who was in charge of the 200-inch telescope and I were members of the committee appointed by the National Academy of Sciences in late 1940 to investigate the inadequacy of the anti-submarine warfare program in the U. S. Navy. I'll come to that.

MINK: But while we're on the subject, I wonder, because all of these people were so important, if you could just give us a little sketch of your impressions of them at this time.

KNUDSEN: Yes. I should name one other, Frank B. Jewett, who was very important at that time. He was then chief engineer of the Western Electric Company, and in 1925 became president of the Bell Telephone Laboratories. Jewett, at the beginning of World War II, was the President of the National Academy of Sciences, and he is the person who appointed the committee that investigated the anti-submarine warfare technology at that time.

The laboratory was relatively simple. Besides the sections which I mentioned, there was one that dealt more or less with hardware, that is, the actual elements that go into the telephone system, the microphones, the earphones, the circuitry, and things of that sort. Halsey A. Frederick was in charge of this section. He later (1932-34) was vice-president of the Acoustical Society of America, at the same time I was president. And then there was, beginning at that time, a section or division of chemistry, which was very important, you see, in exploring all sorts of crystals, and later, all sorts of alloys, mostly magnetic alloys, which came to be very important, you know, in the whole development of inductances and circuitry and in many other things.

MINK: Well, couldn't we begin with Hartley? Tell just something of what he was like, as a man.

KNUDSEN: Well, Hartley was really in the genius class, a very shy person, but profound in his ability at analyzing complicated physical problems. He enjoyed the respect of all members of the staff, and I would say that he was among the top two or three investigators at the Bell Laboratories. Certainly, his memorandum, which preceded mine, dealing with the vacuum tube oscillator is a model. It was called "The Approximate Solution of the Vacuum Tube Oscillator," dated December 19, 1918. It will give you some idea about him if I just read a paragraph here: "The purpose of this memorandum is, one, to describe an approximate method of dealing with vacuum tube oscillation generators which, within certain limits, should give accurate enough results for practical purposes: and (2) to describe some rather simple experiments which should serve to test the accuracy of the method for conditions favorable to its application and indicate the degree of its inaccuracy for less favorable conditions." So this deals with a critical analysis of the vacuum tube oscillator--why and how it generates alternating currents having frequencies encompassing the entire ranges of the subaudible (below 20 cps), the audible (20 to 20,000 cps), and on up and through the megacycle radio and television broadcasting ranges. Without Hartley's work, I could not have done what I did. That is, Hartley's pioneer research made it possible for me, really, to do a significant piece of research on my own, which was my first attempt at research, and which convinced Hartley and others, I think, that I had research possibilities. As a matter of fact, they wanted me to come back to the laboratories after the completion of my PhD degree. We'll come to that story later, because I decided to come to [laughter] a little junior college [UCLA] instead of going back to the Western Electrical Research Laboratory, which was just about ready to change its name to the Bell Telephone Research Laboratories.

MINK: What can you say about H. D. Arnold?

KNUDSEN: Arnold was an exacting and sophisticated executive. I can best describe him in terms of his reaction at an early meeting of the Acoustical Society of America, when one of his staff members was delivering an important paper but wasn't speaking loudly enough to be heard by his audience. Interrupting the speaker he said, "One of the things we're concerned with at our laboratories is communication, and if you whisper, you can't be heard in this auditorium." He was really a meticulous person. You could always say, "Well, he looks more like a banker than a research engineer." But he was a very good organizer and really set the tone for the organization of the Bell Telephone Laboratories. He had more to do with its initial progress, perhaps, than any other one person.

MINK: Was he a cordial person?

KNUDSEN: Very. He was the first man who interviewed me when I began work at the Bell Laboratories. I had been hired by R. L. Jones, but when I arrived there, I was told to go to H. D. Arnold's office. In this discussion with him, he told me I was to work with vacuum tubes. I didn't know what they were, but I didn't dare ask him. I kept quiet a little while and he said, "Well, first of all, I want you to get acquainted with vacuum tubes." He said, "You should read the memoranda that have been prepared by H. B. Van der Bijl." That is another character I should include among the outstanding men at BTL. Van der Bijl, as the name implies, is a Hollander, and he had written more memoranda on the vacuum tube at BTL than anyone else. I was given two weeks time by Arnold to read them. He said, "First of all, I want you to read every memorandum that has been written by Van der Bijl. There's nothing like them in print anywhere. These are the original memoranda dealing with the development of the three-element vacuum tube. The vacuum tube made possible, in 1915, you know, long distance telephony (New York to San Francisco) and the first radio communication across the Atlantic. My introduction to the vacuum tube was three years later. Van der Bijl had written this series of memoranda, which later became the basis of the first book on the vacuum tube, The Thermionic Vacuum Tube, by H. B. Van der Bijl. Van der Bijl was invited to come to the University of Chicago for the 1921 summer session to give a graduate course on the vacuum tube. It was based entirely upon his book, which was just off the press at that time. Well, Van der Bijl was a very interesting person, socially as well as professionally. He decided to leave the Bell Laboratories while I was still there to accept a position in South Africa. A party was given in his honor, which was a rather gay one--at least it was for me at that time. This was during prohibition, but there were ways of getting around it. At this party, the guest of honor was feeling pretty gay. He gave his farewell speech to us from the top of a night spot table. He had been invited to become the equivalent of our Secretary of the Interior and Secretary of Commerce, where they were combined together in South Africa. He went to South Africa in 1919, and I learned nothing about his career there until recently. I therefore was amazed to read in the

December, 1966 issue of Fortune Magazine an article on South Africa, and H. B. Van der Bijl was mentioned as one of the pioneer workers, one of the formers of the commerce and development of South Africa. An octogenarian in 1966, he was still active and prominent in South Africa forty-seven years after he left the laboratories.

MINK: Well, we lost an important man.

KNUDSEN: We did lose a very important man.

MINK: He never continued his research on vacuum tubes?

KNUDSEN: No. I had the fortunate experience of spending my first two weeks at the Bell Telephone Laboratories just studying the Van der Bijl memoranda, original documents describing the epochal development of the vacuum tube, which has played such an important part in modern technology. I must say, those two weeks were among the most important ones of my entire education. This experience certainly oriented my thinking and my future work very much, especially my work on my thesis at the University of Chicago, and my early researches in architectural acoustics at UCLA were made possible because I had spent these two weeks reading these memoranda. I became an expert really, [laughter] in those days, on designing and assembling vacuum tube circuits, because I had read these memoranda by Van der Bijl.

MINK: Did you get a chance to talk to him about them?

KNUDSEN: Oh, yes. I had several conferences with him about vacuum tubes. There were occasional seminars at the Bell Laboratories, and we neophytes were invited to participate in them. I was just a listener, but I remember Van der Bijl was a very good teacher and, foremost, a very good research man.

MINK: Did you get together off the job to talk about your work?

KNUDSEN: Well, often at lunch, but not with Van der Bijl. I often joined other colleagues at lunch away from the laboratories. We were not far from Greenwich Village [New York City] and one of the joys of working at the Bell Laboratories was to be able to spot a new place to eat lunch. We'd say, "Well, we found 'Three Steps Down'; we found 'The Black Cat'; or some other novel place." You know, even fifty years ago these peculiar names were commonplace for little eating places in Greenwich Village. Very often three or four of us would go together, and sometimes we'd talk shop, but more usually we'd talk something else. But among my closest luncheon associates at that time was a man who later left the laboratories to go to the International Tel and Tel [Telephone and Telegraph]. His name eludes me now. But I remember that he had an inventive mind, and we both liked good food, which we often found at Wanamakers. He had his own private laboratory at that time and was developing selenium cells, which became of interest to the Western Union. They were used in their telegraph operations, and this later led to his accepting a position with the research laboratories at International Tel and Tel. Closkey is the name. Closkey and I became good friends, and I had lunch with him more than with anyone else. He was married and their first baby was born while we were working together. I recall now that one of his promises to me was that the baby carriage, which was a rather fancy one, would be sent to me on the occasion of the birth of my first child. I had forgotten all about it when our first child came, during the graduate student years at Chicago, and we got another carriage. That's how bad my memory is on some things, but I just happened to recall a short time ago that Closkey had promised to give my wife and me our first baby carriage.

Two other persons I should mention. One is Dr. Mervin J. Kelly, who later became president of the Bell Telephone Laboratories, and also like Arnold, Fletcher and myself, had his graduate work at the University of Chicago. Mervin Kelly was out of graduate school (PhD Physics) at Chicago a year or so before me. I remember that I joined him in a group he directed that after the close of World War I, went to Washington, D.C. to participate in demonstrations of equipment that had been developed at BTL for the benefit of World War I.

MINK: Was this a request that was from the government?

KNUDSEN: Yes. It was a request from the government. And various other corporations that had developed instruments that were useful in World War I were on exhibit. I was with this Dr. Kelly at the Washington exhibits, who later rose in the ranks to become president of the laboratories; he is a very well known scientist-engineer in the world today, although he's been retired now for a number of years. I think that his selection, and also mine, to go with this small party, there were probably eight altogether, but as I recall, I was his right-hand man on this interesting assignment.

MINK: He was in charge?

KNUDSEN: He was in charge, and this was one indication, if I may digress again, that I was being spotted, I think, for advancement, and possibly administration, in the laboratory. One of my duties in the laboratory was to conduct guided tours for important visitors who came there, a recognition, perhaps, that I had some personal traits in meeting people. My experience as a Mormon missionary, of course, had something to do with that [because] I'd served in administrative capacities there. And in a personal series of tape recordings of this sort, you think of these things and call attention to them. But I'm sure that these early missionary and

BTL experiences, had something to do, later, with administrative tasks that I was called upon to perform in the Acoustical Society, the formation of the American Standards Committee on Acoustical Measurements and Terminology and other tasks besides the administrative duties I've had here at the University of California at Los Angeles. But Mervin Kelly rose in the ranks, as I indicated, and I suspect that his example exerted its influence on my own career.

The other man I should say a few words about is E. H. Colpitts, who was a precise thinker and a very precise administrator with watchdog characteristics. These characteristics I observed, particularly, later when he was in charge of Division Five [Subsurface Warfare] of the National Defense Research Committee during World War II. I had very little to do with Colpitts during the time I was in the laboratory, but I came to know and respect him later very well. Colpitts retired in 1937 from the laboratory and his retirement is significant, I think, because it has a bearing on the effect of retirement on many persons. Colpitts, I suspect, didn't want to retire, and he knew the Bell system had conducted a survey on the effect of retirement on longevity. This survey had indicated a discontinuity in one's life expectancy, namely, that the day after retirement your life expectancy drops several years. I forget exactly how much, but there was a discontinuity of about four or five years in this curve of life expectancy that occurs abruptly after retirement. And Colpitts really went into decline, in matters of health and outlook, and so on, from the time of his retirement. But in 1940 he was named as chairman of the committee that was appointed by the National Academy of Sciences to investigate the technology, really, of the U.S. Navy in connection with its anti-submarine warfare potentials. It was in that capacity that I first really became acquainted with Colpitts, and as a matter of fact, working with him, I learned to admire him very much. As a matter of fact, the first draft of the report, which this committee submitted to the National Academy describing the deficiencies in our anti-submarine [program] and making recommendations for correcting these deficiencies, was pretty much written by Colpitts and me on the train from New York to Los Angeles, which was, you know [laughter], three days and four nights. So I really got acquainted with Colpitts on this writing job. We struggled over this, phrase by phrase and word by word, really, in writing this report, which we realized was a very important document. But Colpitts--we'll come to that somewhat later, because I think I should describe the work of that committee at a later date--then was named as the vice-chairman of this division of the National Research Defense Committee to supervise the civilian work in the anti-submarine warfare.* This division became a very large organization, as you might know. Radar was more important than sonar, but the research of this division was really significant in the development of sonar. Colpitts, in watchdog fashion, was in charge of the New York office. I was later a member of his staff. We were supposed to be at the office at 8:30. Colpitts was always there at 8:15 to find out who was late. Every morning he made the complete tour of the sixty-fourth floor of the Empire State Building, which we occupied, and you could always expect Colpitts to come in. If anyone was a little late, you could see the wrinkled nose of disapproval break out and spread over his face. But once or twice that had happened to late comers, they were there on time. So Colpitts was really a meticulous fellow on punctuality and fulfilling your obligations. He was also competent and very sharp. That is, he knew what the score was in connection with the problems of the anti-submarine warfare program, and the United States was fortunate in having a man like him at the operating head of the committee. Drs. Tate

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*The Chairman of Division V of National Defense Research Committee was Professor John T. Tate of the University of Minnesota [V.O.K.].

and Colpitts were a splendid team, and much of the success of our subsurface warfare was attributable to their leadership.

MINK: Would you go back just a moment and tell me something about your trip to Washington to demonstrate the technological developments? Who did you see? Where was this exhibition held?

KNUDSEN; Yes. It was held at the [building] where the National Academy and the National Research Council [were then located]. It was not their present building, but it was near the present site of the building, so it was held in an important building in Washington, D.C. And this was open to the public. It was a public demonstration. And the visitors that I remember more than any others, and I think significant for this record, were the Japanese. There were usually a half a dozen or more Japanese going around from exhibit to exhibit with their notebooks, making complete notes on everything they saw and asking all sorts of questions. And there was no doubt in the minds of those of us who were there that this was the beginning of preparation for military supremacy on the part of Japan, and in 1941, they felt they could pretty nearly beat the United States in conflict over their interests in Asia.

MINK: Well, did the President ever come to see the exhibit?

KNUDSEN: No. It wasn't that important. But there were important industrialists and engineers, and we had many visitors there. This continued, I believe, a week or longer, maybe two weeks, and it was an interesting display of what various corporations had contributed. The General Electric, I know, had a good exhibit, and I believe RCA.

MINK: In your work at the laboratory, whatever research was done became the property of what was later Bell Telephone.

KNUDSEN: Yes. That is right.

MINK: And you didn't receive or reap any benefits from it?

KNUDSEN: That is correct, except for, of course, the reward for having been at an important laboratory and for doing interesting work, and being well paid for it. I'm sure it had a beneficial influence on the lives of all of us who worked there. I am greatly indebted to BTL and so is our country, because again in World War II, the United States was mighty fortunate that there was a Bell Telephone Research Laboratories. They were organized and able to make faster progress than could be made in many of the emergency wartime laboratories, and BTL made very important contributions to the anti-submarine warfare program, for example. The first acoustical torpedo was largely a development of the Bell Telephone Laboratories, and it had a salutary effect, you know, in our winning the Battle of the Atlantic, and then in completely wiping off the Japanese fleet, and also destroying its submarine fleet. These acoustic torpedoes played a crucial role. I'll go into that a little later when we talk about the war years, because I played a part in determining the range within which the acoustic torpedo would have to be dropped to seek and find its target--an active submarine.

MINK: Yes. Well, maybe this is a good place to stop for now. [tape off: While the tape-recorder was off, Dr. Knudsen was asked if there was anything more he could add about the Bell Laboratories and particularly about the buildings.]

KNUDSEN: At the time I was at the laboratory, all of the laboratories were located at 463 West Street, and I was on the seventh floor. I think there were eight floors altogether, and the top executives were on the eighth floor.

MINK: Well, that showed you where you were, anyway!

KNUDSEN: West Street runs right along the Hudson and across the street from us was a cheese factory, and the cheese often was aged more than it should be for olfactory reasons, and we were often, I know, disturbed by these odors that came from there. You know, it's right along the dock on the Hudson [River] there. And the building was a good building in those days. It served as the main laboratories until probably sometime in the early thirties, when they moved out to Murray Hill, [New Jersey].

SECTION IV

KNUDSEN: I thought I'd talk today about my years at the University of Chicago. I regarded my missionary years, 1915-1918, at Chicago as an interlude, a normal phase in the careers of most young men and some young ladies of the Mormon faith. I think I've explained that my father and my three older brothers had fulfilled what's referred to as "honorable missions," all in Norway. In my senior year I had received from the University of Wisconsin's Department of Physics an inquiry regarding my possible interest in a graduate teaching assistantship.

MINK: How did that come about?

KNUDSEN: Well, I presume they make inquiries at various universities, send out form letters, and probably Harvey Fletcher advised them that I was a promising student worthy of an assistantship at Wisconsin. This is just my surmise. I advised them of my commitment to be on a mission for a period of some two years or more but indicated that I would be pleased to consider such a possibility following the completion of my mission.

During my tenure as secretary of the Northern States Mission, I had an opportunity to visit all branches of the mission, one of which was at Madison, Wisconsin.

I used this opportunity to call on Professor "Benny" Snow--he was known by that name everywhere, affectionately too--he enjoyed a most distinguished reputation among prominent physicists for his brilliant and fascinating demonstration lectures. There was one in particular for which he was famous, his demonstration lecture on snowflakes, in which he produced them in the presence of the class. This was a general course in physics, and many non-majors attended it. The course was very popular at that time and had come to my attention. I do not recall whether he was chairman of the Department of Physics, but he was probably the best-known man, and when I was in Madison on official missionary duties, I had a most pleasant conference with him. I found him a most cordial person. He told me about his celebrated demonstration lectures, including the one on the snowflakes. He guided me on a detailed tour of the Physics Department. I was very favorably impressed with what I saw and what I heard from this Professor "Benny" Snow and decided that if I couldn't get into the University of Chicago as an assistant--I had to have some financial help for my doctorate study--I would apply at Wisconsin. So I made no commitment; this was just preliminary, and it was really a year before I would be free to undertake any such work as that, and it was before I knew I would go to the Bell

Telephone Laboratories.

During the autumn of 1917, my mission president, German E. Ellsworth, who later became one of the top U.S. officials in enforcing the Volstead Act, granted me the unusual privilege of taking time off to attend the lectures of [Albert Abraham] Michelson at the University of Chicago. Michelson was, as you know, the first American scientist to win the [1907] Nobel Prize, and his lectures on the four subjects that he taught were well known among physicists. Fletcher had told me a great deal about what a splendid lecturer Michelson was and how much I would learn from taking these courses, so I was granted the opportunity to attend this one course. Nobody knew what Michelson's course was going to be until the first lecture. He gave four courses altogether, and he announced the subject at the beginning of the quarter--Chicago operated then as now on the quarter system. "The lectures this quarter will be," as he said this time, "on the electromagnetic theory of light. If any of you have taken this course, you may be excused."

MINK: It would not indicate the title of the course in the catalogue?

KNUDSEN: That's right.

MINK: Just general physics?

KNUDSEN: There were four courses that were listed always.

Besides the Electromagnetic Theory of Light, which was one of his most famous courses, he gave one on Electricity and Magnetism, one on Physical Optics and one on Mechanics of Wave Motion and Sound. Subsequently, I took the other three, but this fall term of 1917, while I was still serving full time as secretary of the Northern States Mission, this first course under Michelson was for me a great opportunity, and it had a lasting effect on my life. It was certainly one more experience that led me to believe the University of Chicago was _the_ place that I should go to work for my higher degree.

I came to know Michelson later through the courses that I subsequently took from him. All candidates for the PhD degree were required to take these four courses, and from the titles, it's apparent that they did cover pretty well the field of classical physics at that time, in contrast to modern physics, which I'll speak about later.

MINK: At the time you were taking this course, you had not been officially admitted as a graduate student at the university?

KNUDSEN: No. I discussed this matter with Professor Millikan, who handled these routine matters for the department. Michelson was head of the department, and that meant at the top, and Millikan was chairman of the department. They both had offices then; Michelson's office was primarily titular, but when really serious problems arose, he took over. But Millikan really was the man who made most of the decisions, and especially all those about taking courses, selection of dissertation subjects, and so on. Michelson didn't want to be bothered with administrative details. If it was something very important, he was ready to act. I'll describe an incident later in which he did act, and act very decisively; he probably learned how to act that way, first of all, when he was a midshipman at Annapolis, where he got his first academic training.

MINK: So you were entered as a special student?

KNUDSEN: I was entered as a special student, permitted to take this one course. But I did take it for credit, and it was put on the books. This, for me, and for most of the physicists I know who had these lectures, was tough going. But it was superb teaching. It was comprehensive, and it was penetrating, and it was faultless, and it was literary prose. Michelson would never begin a sentence until he had thought it through to the end. He would sometimes pause for a few seconds, and you knew he was forming that next sentence. But when the sentence was spoken it was in such style that it could easily go into print immediately without revision. This was characteristic of all his lectures.

MINK: It was good for the note-takers.

KNUDSEN: It was very good for the note-takers, although--I'll come to that--you had to scribble very fast. Each sentence was apparently thought out to its end, and was a model of precision, conciseness and elegance. It is unfortunate that there was not at that time either an oral history project at the University of Chicago or the recording facilities to make such a project possible. At that time there was only the primitive Edison phonograph, and as you know, the only power for cutting or engraving the speech or music on the record, was the energy formed by the human voice. And [with] the speaking voice, you had to speak pretty loudly to generate even 100 microwatts. That's, you see, one ten-thousandth of a watt, and that's the amount of power then that's available for cutting these records. As you know, they had to speak right into a horn and conserve the energy to make any kind of record. And this would not do, of course, at all, for recording the lectures of a professor such as Michelson. I don't think anyone ever thought of such a thing at that time anyway. If you do it, you go into a studio, where they have this big horn that you speak into. You're told to shout into it, in order to generate enough power, because that power has to drive the cutting tool that cuts the master sound record. So it was utterly impossible to do it, say, the way we do it today with a microphone on the desk and going on in ordinary conversation. It is unfortunate; maybe Michelson could have recorded many of his lectures later, because electromagnetic recording became commonplace before Michelson died. Many physicists, myself among them, would be better teachers of advanced physics if sound recordings of Michelson's lectures were available today, and, of course, if they were used, as models to emulate, because he, among all the teachers I had, would be certainly the best model of physics' lecturers. In addition to his lectures on the electromagnetic theory [were] those in the other three courses he taught at Chicago, the courses were those I mentioned, physical optics and so forth.

I've discarded or lost too many of my written documents, Mr. Mink, including most of the notebooks I filled covering the graduate courses I took at Chicago and the graduate and undergraduate courses I have taught at UCLA. But fortunately, I kept the notes of Michelson's lectures. The ones on electromagnetic theory, which were my first graduate student notes, I prize very highly today; and the lecture notes I took on his course on the mechanics of wave motion and sound enriched my early teaching duties at UCLA. I really cribbed a lot from these notes, but I gave Michelson credit. His method of teaching had a profound effect on my own method, and the notes I kept on these four courses were of great value to me in my teaching career at this university.

MINK: You mentioned that you used the notes from the one on sound here at UCLA. Did this course have any influence on your decision to go into acoustics?

KNUDSEN: I don't believe so. I wouldn't single it out especially that way. There wasn't very much on sound. It was mechanics of wave motion and it had application to all sorts of wave motion, whether they were low frequency (as in the tides), mechanical vibrations, electromagnetic vibrations or light vibrations. It covered the basic field of mechanics of wave motion, and the sound was incidental. Michelson wasn't especially interested in sound. A course I had later under the title of Acoustics by Professor A. C. Lunn, I think, had more influence than Michelson's. But Michelson's lectures on mechanics of wave motion and sound really were of great value to me in my course here that I called Mechanics of Wave Motion and Sound, [one of] the first upper-division courses. It was [Physics] 114 and it still is given in this department under the title Mechanics of Wave Motion and Sound. The first part of the course, as I gave it, followed closely that first part of the course as Michelson gave it at the University of Chicago. Michelson (also Millikan) never used a textbook and I'm sure that influenced me very much. I have never used a textbook in the courses I have taught, except once for Physics 1A. I always assigned a large list of reference books, which were placed on the reserve list at the library; the students had access to them. My form of lecturing certainly gained a great deal from Michelson's examples.

My first course under Michelson, that great American physicist and teacher, along with my reverence for Chicago-trained Harvey Fletcher, led me to the conviction that, come what may, Chicago was the place to work for that coveted PhD degree in physics.

Following the conclusion of World War I, I began preparation and plans, saving as much of my salary as possible, to enroll in the graduate physics [program] at the University of Chicago.

This was an easy decision to make in those days. There was no question about the superiority of Chicago as compared with other American departments of physics at that time. All the science departments were strong in Chicago in those days, but physics especially. No other American department of physics in those days could match the two men, Michelson, who already had the Nobel laureate, and Millikan, who was on the way to the Nobel laureate. Everybody at that time said, "Well, Millikan will win the Nobel Prize," and he did win it in 1923. That was a few years later, but undoubtedly he had been mentioned by the Nobel Prize committee in his early years of research at Chicago. Fletcher had told me, "There's no question about it; Millikan will win a Nobel Prize." And so I, at that time, closely associated with Fletcher at BTL, thought, "Well, there are two Nobel Prize men in physics at the University of Chicago; there aren't any at any other American university." When I talked about various graduate schools for physics with my other colleagues at the Bell Telephone Laboratories there was unanimity that Chicago was the place to go. It was just obvious that you would try to go there. [tape off: While the tape recorder was turned off, Dr. Knudsen was encouraged to contrast Michelson and Millikan.]

These two professors made really an outstanding combination. Michelson was superb in classical physics, the standard things. These courses I mentioned indicate the scope of his interest in classical physics, and they were really considered more important than anything else in preparing for the doctoral examination at the University of Chicago. In contrast, Millikan dealt with modern physics, with atomic physics, molecular physics, electron physics, and he was right in the stream of the research which was current in the major physics laboratories of the world at that time. He sensed where the "breakthroughs" would occur, and he knew how, in association with his students, to exploit them. He had spent time at Ernest Rutherford's laboratory at Cambridge, and continued to correspond and collaborate with the principal physicists at Cambridge, including Rutherford, Sir J. J. Thompson and other pioneers in the establishment of electron and atomic physics. So Michelson and Millikan made an unusual pair, probably each the preeminent American man in his field, one in classical physics and the other in modern atomic and electron physics. It would be interesting if some first-rate scholar on the history of physics would write a biography of these two men, a comparative biography, because they were so different in their characteristics, and yet they supplemented each other so very, very well.

Michelson's lectures, as I have indicated, were very formal, very thoroughly prepared. He had delivered them time after time after time, so they were probably almost memorized by the time he had given them in my days there, which were among his last days at the University of Chicago. He had been at Chicago a long time then. He was one of the original men that had been brought there, I believe in 1892.

You know, Chicago started with several highly distinguished scientists that were recruited thereto by President [William Rainey] Harper, to start the university. Michelson was among the most eminent of those. He was the first to receive a Nobel Prize, among any of them. I'm not sure whether any of the others received a Nobel

Prize; they were in the Nobel Prize class. Michelson was the most distinguished man on the faculty at that time and probably was considered the most distinguished physicist in the country, although Millikan was certainly the most active one, especially in research. Students took Millikan's courses because, well, he was Millikan, and he was the top American in his field. Furthermore, it was almost certain if you were a graduate student in physics at the University of Chicago, you would do your dissertation under Millikan. Michelson didn't welcome graduate students to do their dissertations [under him].

MINK: Why?

KNUDSEN: He was a lone worker. He had an excellent mechanical associate, who was his assistant throughout, well, I suppose more than twenty-five years.

MINK: Who was that?

KNUDSEN: Pearson was his name, Fred Pearson. Pearson made his interferometers and his diffraction gradings, and the equipment that he worked with, and was his research assistant in the laboratory, and was with him later when, at the Irvine Ranch, California, they made heretofore unattained precision in the measurement of the velocity of light. Michelson just didn't want to be bothered with graduate students doing dissertations under his direction. He didn't care to spend his time that way. He gave his famous lectures; the rest of his time was spent on his research and certainly, as a result, the world is much better off today.

The Michelson-Morley experiment (in 1887), you see, was one of the great contributions he made in the development of the theory of relativity. His work, with his special interferometer, you may recall, led to the measurement at Mount Wilson of the diameter of the biggest sun at that time, Betelgeuse, which had a diameter 300 times the diameter of the sun. I remember at the time--this discovery was made while I was a graduate student--and the headlines of the Chicago Tribune, I remember, were all the way across the front page: "Sun Twenty-seven Million Times the Size of Our Sun." You see, volume increases with the cube of the diameter, so you've got 27 million, isn't it? I remember it was 300 diameters, and that's 3 x 3 x 3 is 27, with 6 ciphers, 27 million times the volume of the sun. I believe it remains the largest sun that we have in our galaxy.

But if I may digress on that subject while I'm talking about it, the American Physical Society was meeting in Chicago at the time this announcement was made--I believe it was in November, 1921. When Michelson walked into the meeting, the audience arose as though the president of the United States had entered the room, and burst into applause such as you would never expect to hear among a group of physicists. That is, they just spontaneously applauded Michelson's work on this discovery, which was made at Mt. Wilson with the large telescope. It was a 100-inch telescope at that time which had attached to it the special interferometer that Michelson had devised for use with the telescope, and Fred Pearson, as usual, had been working with him. Yes, Fred Pearson was right there all the time working on it with Michelson. This is one of the spectacular discoveries that came from the researches of Michelson, and one to which I was a witness because I was present at this extraordinary meeting of the Physical Society. This incident is an indication of the reverence with which, really, Michelson was held by his colleagues and students.

Well, to go on with this contrast between Michelson and Millikan, Millikan's lectures were much more informal. They also were more historical. He paid a great deal of attention to the historical development of physics. He always gave a long list of reference books for each of his courses. He, like Michelson, used no text, but he made a copious list of references that we were expected to read; and also, he gave us a fine bibliography at the beginning of each course, outlining the work which had led up to the lectures he had prepared to give in the course. This, I'm sure, was a very valuable part of the courses Millikan gave. You got the background, and even when he quizzed candidates for the PhD degree, he would bore in very much on the history of the subject, going back as far as Newton or even earlier, in many instances. He expected the candidates for the PhD degree at Chicago to know their history of physics, which I think would interest you. It's an important thing that too often is neglected now, but to have these two kinds of instructors at one institution, I know, has had a profound effect on the development of American physics, Michelson as the refined, finished product that serves as a model for all good teaching of classical physics, and Millikan for the preparation, in the research aspects particularly, of a physicist who is going on for a university career, a teaching and research career.

Millikan's lectures were, as I say, quite informal but he emphasized the research that was in progress at the time, and also, he realized that his courses were a training ground for the research-dissertation problems that would follow in a year or two on the part of these students who were attending his lectures. There were others who attended them, but those who were planning to work for a PhD degree knew almost certainly that the lectures Millikan was giving would be the best kind of preparation for their dissertation, when they began that work, and for his questions at the final doctoral examination. He kept a very close rein on the work of his students and was most helpful in the selection of dissertation subjects. A graduate student always had a conference with Millikan, and he'd bring out a little notebook that he carried in his pocket in which he had the titles of subjects that he considered suitable for a doctoral dissertation. I remember the wide range of subjects he had, even beyond those that were in his own field. At that time it was mostly electron physics, Brownian movements and photoelectric effects, and his work in stripped atoms. That's knocking the outer atoms off from molecules by very high voltage shocks, shock excitation. It was that kind of work he was doing, especially the last year he was at the University of Chicago, and which he continued at Cal Tech [California Institute of Technology] after coming there in 1921.

MINK: I'm going to ask you a wicked question. Do you think that he was trying to get people to work on things that would help him in his research?

KNUDSEN: I presume I'll have to say yes. Millikan was an egocentric person, very much, and so was Michelson in a different way. I think I've indicated that Michelson didn't want anything to do with the actual supervising of doctoral dissertations, or the research they were doing for their doctoral dissertations.

Millikan was right in it up to his neck all the time. Millikan was a harder worker, I guess, than anyone I had ever known, but he also used his students a great deal, that is, as he used Fletcher. They worked together. Very often Millikan published the results jointly with his students, but in the case of the work on the measurement of the elementary charge of the electron and the Brownian movement, for which Harvey Fletcher was his assistant on both projects, he made a deal with Fletcher that Millikan would publish the one on the charge of the electron, and Fletcher would publish the one on the Brownian movement, under his name. Many people at Chicago felt that the two papers should have been published under their joint names, and some people even believed that if the two papers had been under their joint names, that Fletcher might have shared in a Nobel Prize.

MINK: Which he never did receive?

KNUDSEN: No. I think it's significant in this sizing up of these two men. Millikan throughout his career, I think, was very much concerned about his own reputation, and this may be the time to report an incident that came later in his life. [tape off]

Well, as you know, Millikan later became very much involved in the study of cosmic rays, and there was quite a controversy between Millikan's findings and the findings of Arthur [Holly] Compton, who had been at Chicago and at the time, I think, had moved to Washington University, St. Louis, where he was chancellor. But the evidence was piling up, more and more, in favor of the views held by Compton, that the primary cosmic rays were mostly particles, protons and light nuclei. Millikan had held out that they were mostly waves.

SECTION V

KNUDSEN: The controversy between Millikan and Arthur Compton on the nature of cosmic rays really came to an end, so far as physicists were concerned, at a meeting of the American Physical Society that was held in Washington, D.C. It was the big annual meeting, probably twenty years ago or twenty-five.

MINK: And you were not in attendance?

KNUDSEN: I was not in attendance, and the report of the meeting was given to me by several physicists I know who were there. I believe the following description of what happened at this meeting is fairly accurate. Arthur Compton had presented a paper which summarized his latest findings on cosmic ray research. As you know, this was conducted all over the world, especially on high mountain peaks and deep wells and mines and things of that sort, because the cosmic rays depend a great deal upon absorptive effects and collisions that occur in the atmosphere, and, of course, by other objects that could absorb, disrupt, or otherwise change them. At this meeting, I was told, Compton's paper really was a clincher, so far as convincing physicists that the views he had held about cosmic rays being particles were true. Millikan was present at the meeting, according to the information I received from colleagues, and could have arisen on the occasion and said, "Well, gentlemen, the evidence that Compton has presented today convinces me that he has made the best possible interpretation of cosmic rays as we know them today." He should have congratulated him then, I think, as he did in a later publication. But instead of that, according to my informants, he lost an opportunity, really, to have established himself as a magnanimous person in such matters; whereas, [he acted] like so many other scientists and scholars [who] are much more concerned about establishing their own views than they are, in some instances, about establishing what's the truth. I wouldn't like to hang that label on Millikan, whom I greatly admired, but this instance is certainly discreditable, so far as Millikan is concerned.

There was a sequel to this, which I think is of local interest. The Los Angeles Times, in commenting upon the press accounts of this meeting, which indicated that Compton had really been vindicated in his views about cosmic rays, also had an editorial about the subject. I paraphrase the editorial and probably exaggerate a little, but this is the way I remember it. In my own words, the editorial said, "Who is this upstart Compton that dares challenge the great Millikan? If

Millikan says that cosmic rays are waves, by God, they are waves!"

MINK: This is more or less explained, isn't it, by the reverence with which the Southern California community held Millikan in that area?

KNUDSEN: Yes, it is. He was deservedly held in reverence, and I wouldn't like this to be interpreted as deprecation. This is probably one of the human frailities in a man. Who among us is free of them?

MINK: That's true. But just to have a man like this in the area did credit to the area, I think.

KNUDSEN: There's no doubt about it. He not only did credit to the area, he did credit to the United States. He did credit to the world. Millikan was certainly one of the most honored physicists the twentieth century has ever had. If you read his Who's Who citations, and note there his many, many honors, you'd be really amazed, because he was greatly honored. Besides his Nobel Prize, he had a string of prizes that takes up more than half a column of Who's Who. Besides his epochal achievements as a world famous physicist, he really made Cal Tech one of the world's foremost scientific institutions.

MINK: You'll be wanting to talk about him more later.

KNUDSEN: Yes. [tape off] I arrived in Chicago then to resume my doctorate program in the fall of 1919. I had the course with Michelson as everybody did. These four courses would continue, and the course I had that fall was Electricity and Magnetism. That was the second one for me. And Millikan's course dealt with atomic physics. It was an introduction to atomic physics as it was at that time.

My first quarter I was there as a bachelor, an unmarried student. During the several months following the end of World War I, I not only planned to begin my quest for the doctorate at Chicago, but I also worked hard on a much more personal problem, namely, the persuading of Florence Telford, the lady missionary, who had captured me at first sight in Chicago in December of 1917, when I greeted her at the mission home on the west side of Chicago. I was planning desperately that she should become my bride as soon as possible. She had just been released, in September of 1919, from her mission duties, and she felt that she should have a little time with her family and her hometown [Ogden, Utah] before she undertook three years of less than affluent life as the wife of a graduate student at Chicago. She was pursued by quite a number of others, and I had to play my cards skillfully in order to win this suit. I did win during that first quarter at Chicago. She had promised that she would marry me in December of that year, and as soon as my fall term was finished at the University of Chicago, I rushed to Salt Lake City, where we were married, as most good Mormons were, in the Salt Lake Temple. As a matter of fact, I don't believe my first quarter at Chicago was as successful as later ones, because my interests [laughter] were definitely divided, and I confess that my concern about this marriage received higher priority even than my studies at the University of Chicago.

Among other things, I had to find a suitable apartment. This was not easy following World War I, when the housing shortage was very acute in Chicago. When you were looking for an apartment of any sort, you'd read the advertisements in the morning paper and you would rush to the place of a possible apartment, hoping to be the first one there to make a deposit. I failed at probably the first seven or eight trips that I made in different parts of the south side of Chicago, within commuting distance of the university, but finally succeeded in getting one about two days before I had to leave to marry Florence Telford at Salt Lake City. [tape off]

One course I remember especially in that first quarter at Chicago, was Michelson's Electricity and Magnetism; another was Millikan's Atomic Physics as I have mentioned. There was a third one with [Henry Gordon] Gale, entitled Geometrical and Physical Optics. Gale, also, was a very interesting character at the

Department of Physics. [For] everyone who took high school physics over a period, I guess, from about 1915 until 1940, the standard textbook was A First Course in Physics by Millikan and Gale. They worked together in writing the book, initially, but most of the revisions were made by Gale.

Gale was a hale and hearty fellow, not in the class of Michelson or Millikan, but a first-class spectroscopist and had developed the spectroscopic program in the department. Gale was a profane person; you'd hear him loudly cursing up and down the hall, trying to locate Dr. Millikan. And you'd hear him shout, "Rob, oh Rob. Goddamn that Rob! Where is he? You can never find him when you want to find him." He was then revising A First Course in Physics. Later (1921), Millikan had left Chicago for Cal Tech, taking with him his assistant Ira Bowen, who later became director of the Mt. Wilson Observatory. Bowen had worked with Millikan at Chicago on "stripped" atoms, and continued working closely with him during those first years at Cal Tech. [Millikan], according to Gale and others, had boxed up the spectroscopic equipment that he and Bowen had been using, much of it equipment that Gale had acquired for his own use in spectroscopy. This was quite an incident there, and most of the graduate students friendly toward Gale felt that he had been "robbed." Gale was infuriated that this spectroscopic equipment was taken away, and I can recall his words, which were approximately: "Goddamn that Rob Millikan! He's so goddamned selfish, and he doesn't know that he's so goddamned selfish." That was his characterization of Millikan's taking away this equipment.

MINK: It was boxed up and taken off to Cal Tech?

KNUDSEN: Yes, it was taken to Cal Tech.

Well, returning to Michelson's lectures on electricity and magnetism, they were a model of conciseness. I remember this. The course began with electrostatics and magnetostatics and then electrodynamics, followed the classical treatment of circuits containing resistance, inductance and capacitance. One day, really, in a single lecture, he covered the whole subject of steady state and transient currents in circuits, comprised of inductance and resistance and capacitance. I remember the remark of a Jewish student who was walking out with me at the end of the lecture. He said, "My golly, he covered more ground today than we used to cover in a whole quarter." There was an immense amount of labor the students gave to writing up each lecture--many gaps to be filled in. It would require the average student four or five hours to write up a typical lecture, and there was good reason to do it promptly.

Michelson's courses consisted of two lectures and a quiz, and this was routine.

MINK: Throughout one week, two lectures and a quiz?

KNUDSEN: Yes, three times a week for the quarter. You see, this was about eleven weeks, so there'd be something like twenty-two lectures and eleven quizzes in the process, no final examination at the end. But these quizzes, I assure you, served in lieu of any other kind of examination. They were very formal, rigorous affairs, and the students normally prepared for them very, very much. Michelson's quizzes, again, were models of good teaching technique, because in the lectures he often sketched the derivations. He would begin very thoroughly, and the first few steps were worked out in detail, then he'd hit the high spots as he proceeded toward the end of the lecture. At the beginning of each course he always said, "Now, I don't want you to take notes of my lectures in class. Listen as intently as you can, and as soon after the lecture as possible, write up the notes as completely as you can."

MINK: That's a good memory trick in itself.

KNUDSEN: Indeed it is, but nobody heeded that advice. Everybody came armed with notebook and pencil or pen, and they scribbled as fast as they could to get down all he said and all he wrote on the blackboard.

MINK: Including you.

KNUDSEN: Including me, yes. That's how I happen to have my notes.

MINK: Then you could expand them later?

KNUDSEN: Then you could expand them; you <u>had</u> to expand them later. The expansion was left to your ingenuity. He wrote the principal equations of a derivation on the board, and he would go through the first steps with great care, and you got everything complete in the beginning. Then he'd say, "If you do certain mathematical operations, you will end up with this result." We were supposed to do those mathematical operations, and woe to the student who hadn't done them, if he was called at the quiz. It was always an oral quiz, and the order in which you were called at this oral quiz was entirely haphazard. That is, he had a pack of cards which contained our names and he'd shuffle them after each individual was quizzed, so you had the same chance of being called immediately after one quiz, as you did any other time, see.

MINK: If he called up somebody twice or three times during one quiz, would he pass him over next time?

KNUDSEN: I don't recall any instance in which the same man was called up twice in the same day, but I do recall instances, myself included, in which he was called for two quiz sessions in succession. His classes were not very large, not more than about fifteen to eighteen, and not more than three-to-five would be quizzed in one session.

MINK: This question is not meant to discredit Michelson. Would you say that the body of material to be covered in that course at that time was not as large as it is now, or was everything pretty well fixed by that time?

KNUDSEN: In these four courses that he gave, things were pretty well fixed at that time. They are pretty substantial and essentially complete even today.

MINK: There's not been much change in theory or in material to be covered then?

KNUDSEN: No. In the course in electricity and magnetism, he ended up with some relativistic findings of Einstein and others, following, of course, his experiments on the attempt to measure the motion of the earth through the ether. So we got an introduction to the special theory of relativity, and that, of course, changed very much after Michelson's time, especially by Einstein's general theory of relativity, which has greatly modified nearly all branches of physics.

MINK: Would that be covered in a course today on the same subject?

KNUDSEN: Well, in a course today, electrodynamics would be, of course, very much modified and developed, as compared to the days of Michelson, so would atomic and nuclear physics, relativistic mechanics, cosmology, and so on. [tape off]

Michelson was most gentle and helpful in dealing with students who were bright, who had done their homework, and were prepared to discuss intelligently his lectures, but he had no patience with the pretender, and little with the person who was actually dumb. The pretenders were put in their place, and the dumb ones really sought some other major than physics, after a disappointing failure before Michelson. His questioning was done very formally and in a precise and dignified manner, but with a very firm manner. He felt that it was his obligation to separate the boys from the men. This was the end result. That is, when they were really, essentially disgraced before the class this way, some of them dropped out, some of them settled for a master's degree, and others transferred to other departments at the university, or sought to get a degree elsewhere. But among the ones who succeeded well there, I remember such students as Ira [Sprague] Bowen, who came with Millikan here [to Cal Tech] and whose life you were telling me about recently, and Professor [William Ralph] Smythe at Cal Tech, and Mervin Kelly, whose names I've mentioned, are among the students who were there in my time. A Lloyd William Taylor became chairman of the Department of Physics at Oberlin, where Millikan really began his career in physics, and others became important professors, or in industry around the country. Carl Akley was the top student in relativity theory at Chicago at the time I was there and has continued in theoretical physics, and especially relativity theory, until his retirement a few years ago at the University of Indiana.

MINK: When all of these men got their PhD, they did their dissertations under Millikan?

KNUDSEN: Yes, except possibly Akley. There was only one PhD examination at Chicago. We didn't have advancement to candidacy or many of the series of oral and written comprehensive examinations that characterize our system. There was one big final examination. I'll describe that, probably in our next session, because this was an event, and Michelson and Millikan had very important parts in these final examinations. I think that's a separate story.

Just one more brief comment here. As I say, that first course that Millikan gave dealt with atomic theory, and especially kinetic theory of gases. It was a useful supplement to Michelson's lectures, and as I have indicated, it emphasized the current research aspects that were going on. Millikan's other courses there, less rigorous than Michelson's, were certainly informative and interesting and, as I said, especially designed to prepare his students for research in his special projects, which ranged over much of the advancing front of modern physics. That was in the fields of electron physics, of molecular physics, of cosmic rays. They were just beginning to be interested in that subject, but photoelectricity was a subject in which Millikan was very much concerned at that time.

Millikan's lectures that first quarter, I remember, had a great deal to do with the transition from classical physics, which in the last decade of the nineteenth century, physicists regarded as the end of physics. That is, they could add one more decimal point or one more significant figure, but there'd be only refinements, and there would be no important discoveries. His first lectures in this course really were very fine records of this termination of the old and the beginning of the new. You see, this complacency about the status of physics was followed almost immediately by the discovery of X-rays radioactivity, relativity, electron physics, the Bohr atom, quantum mechanics, nuclear and particle physics. These things just brought on the whole era of modern physics, which really began in Millikan's era. Millikan was a graduate student in Göttingen, Germany, and also with Rutherford's laboratory in Cambridge; he was there and at Chicago really at the beginning of the era of modern atomic, electronic and nuclear physics.

MINK: I was going to ask, did these three people [Gale, Michelson and Millikan] correspond a great deal with other physicists elsewhere in the country?

KNUDSEN: Oh, yes. Millikan, especially did. He corresponded a great deal with both American and European physicists.

MINK: Did he ever bring these letters to classes and read them, or would he read to you in private what others had written about their experiments?

KNUDSEN: Yes. Millikan would share much of his correspondence with his own students. I was not his student on my dissertation; I worked alone. Usually Millikan would be back at the laboratory every night, from probably 7:30 until midnight or later, and almost always with one or more of his graduate students. They were going over the project this graduate student was working on, or probably writing up the paper which would be the end result of a project or dissertation, which Millikan had guided and had participated in, in a very, very real sense. There's no question but what he deserved to be a joint author of the project, and this was standard practice at that time. It was co-authorship; it was the master and the disciple. These young men that worked with Millikan undoubtedly gained a great deal from this experience, and in those sessions, I'm sure, they had an opportunity to read letters Millikan received, because Millikan corresponded frequently with the leading physicists at that time. Most of them were not in the United States. They were in Germany, England, in Holland and in other countries. Nearly all of the people who came to visit Millikan, professors who came to see him, I know, were from European universities.

MINK: Do you ever recall any such visits?

KNUDSEN: I recall very definitely one by Professor [Hendrik Antoon] Lorentz of Holland, who was then a very distinguished and a very fine person. He'd worked a great deal on low temperature physics and on the beginning of relativistic physics. He was a theoretical physicist and knew the work of Einstein very well, but also was interested in its applications to atomic physics at that time. He came to visit Millikan when I was working on my dissertation, which was not in that field at all but was in the problem of hearing, application of vacuum tubes to the problem of hearing. Millikan brought Professor H. A. Lorentz into my laboratory to have him become acquainted with the acoustics research I was doing and probably to have his hearing tested. Lorentz was very much interested in having his hearing tested because he was quite an elderly man then, probably in his sixties. Almost always a person of that age does have a loss of hearing, and I remember how fascinated Lorentz was in the findings about his own hearing, and to learn that hearing loss could be measured with such precision, as we were able to do it, with these electronic instruments. And there were many other distinguished physicists who [visited Millikan].

MINK: How was his hearing?

KNUDSEN: His hearing was down, more than he thought it was, just as nearly everybody's hearing at age sixty is down, or is supposed to be down. Mine is down much less than it's supposed to be at my age, fortunately, and that's a great asset to me, because I use my ears a great deal in judging the acoustical properties of auditoriums. But Dr. Lorentz was much interested in the outcome of the tests of his hearing. We also were investigating at that time the hearing of people who heard the same sound of a different pitch in each ear. This is called diplacusis. If they were to listen to a violin [while] they closed one ear and then closed the other, the pitch may go ee-EE-ee-EE [pitch modulated]. It's the same physical tone, but it's heard with a different pitch in each ear. Now this is not common, but we had one or two cases of diplacusis. I'll go into that story later, because it was responsible for directing me to a thirteen-year research program with Dr. Isaac H. Jones, an eminent Los Angeles otologist, who became a most interesting and valued friend and colleague.

SECTION VI

KNUDSEN: I'd like to introduce a few of the anecdotes about Michelson, who was certainly a revered man at the University of Chicago and has, of course, played a very important role in the development of physics, and particularly the theory of relativity. In the early experiments of Michelson, to determine the velocity of light, it became of crucial importance to determine whether there's a difference in the velocity of light in, say, the north-south direction and the east-west direction because of the rotation of the earth. There should be such a difference of velocity of light, if light is traveling through a medium, which was then referred to as the ether and which had very definite electric and magnetic properties, which really determined how fast light traveled. [James C.] Maxwell's equations had indicated that you can compute the velocity of light in terms of these electric and magnetic properties. If the light is actually measured with sufficient precision, and if light is actually traveling through a medium through which the earth is rotating and actually traveling around the sun, then you should be able to determine the velocity of the earth, or the speed of rotation of the earth, with respect to this medium (ether), which was supposed to pervade all space. So Michelson was making precise measurements of the velocity of light. They have to be very precise because the velocity of rotation of the earth, and even its traveling around the sun, is very, very small, compared with the velocity of light. So it called for an unusual degree of precision, which was made possible by the system of rotating mirrors devised by Michelson's ingenuity. And so Michelson was setting up his equipment along a railroad right-of-way, and some curious passerby came to him and he said, "What are you doing there?" Michelson said, "Oh, I'm measuring the velocity of light." And the curious interrogator said, "Why are you doing that?" He said, "Oh, because it's such good fun."

MINK: Where was this done, around the university, or did he go out into the country?

KNUDSEN: Probably in Maryland. This work was done before he came to the University of Chicago, but after his graduation from Annapolis. He was an instructor at the Naval Academy, and it was during that time that he became interested in measuring the velocity of light and the required precision instrumentation. This extraordinary work revealed he was one of the men that Harper wanted when he started the University of Chicago.

MINK: Michelson did not have this assistant that you spoke of last week?

KNUDSEN: No. Fred Pearson was associated with him in Chicago, and later at the Irvine Ranch. I don't believe he was associated with him before then.

Another anecdote about Michelson: he was a superb lecturer, as I have indicated, but he, I think, more forcefully than anyone I know, didn't want to be interrupted during the course of his lecture by questions, and especially by questions that annoyed him. Probably the most annoying questions or interruptions that ever occurred would be when he was going through a mathematical derivation. On one occasion he was hesitant for two or three seconds about just what the next step was in a mathematical derivation. It was obvious that he was thinking and that the right step had not become fully clear to him, and while he was hesitating, a bright young student spoke up. "Professor Michelson, if you'd do . . ." He said, "Young man, if you leave me to my own resources, I think I can extricate myself from this difficulty." That silenced, not only this young fellow, but everybody else in the class, and no one ever interrupted Michelson during the remainder of that course.

MINK: Was this when you were taking the first course and then still on your Mormon mission?

KNUDSEN: No. This was later.

MINK: When you were a full-fledged graduate student?

KNUDSEN: Yes. I referred earlier to Michelson's decisiveness in action when he felt it was necessary. One instance that occurred during probably the last year I was at the University of Chicago resulted from the theft of a set of demonstration lecture notes that Professor Harvey B. Lemon had developed over a period of years. Dr. Lemon gave these demonstration lectures once a week over a period of three quarters, so there were something like thirty demonstration lectures covering the whole field of college physics. They were recognized around Chicago as unusually fine demonstrations. This was Lemon's principal service to the department, giving these demonstration lectures. Any graduate student, who was contemplating a teaching career, would place a very high value on Dr. Lemon's demonstration lecture notes, which had not been published and which Harvey Lemon, in general, didn't pass around to the students. That is, they were private property that he felt should be kept private. Following their disappearance Michelson and Lemon both surmised that the thief was a graduate student, who was contemplating the use of these for future demonstration lectures. Michelson's suggestion was that Lemon approach each of the graduate students in alphabetical order. You go to Mr. A, and you say, "Now, I'm asking your cooperation in solving this problem." Professor Lemon did the leg work, and he would say:

"Now it's necessary that you treat what I am asking you to do, as <u>entirely</u> confidential and private. No one else except you and I must know what I'm going to propose. I know, if you are innocent of this theft, you will be glad to cooperate, and I'm asking you to take me at once, now, to your domicile, and we'll inspect together the materials you have, the books and the notebooks and the relevant material you have in your room." That's an invasion, you see, of privacy, but everyone up to, as a matter of fact it was K-- I won't go any further than that, but it was in the early K's, and this means approximately half of the graduate students, then--had collaborated in this way, and there had been no protests about it.

MINK: Not including you, though?

KNUDSEN: No. It didn't get to me. The thief was apprehended before they got to me. When this K student was interviewed by Professor Lemon and he proposed that they should go [to his house]--this was in the summer months, the window was open--he said, "You don't go to my home. This is invading my rights." He had an automobile; he rushed to his automobile and went to his home. Lemon also went to his automobile and also rushed to the home where he was, and wasn't permitted to enter. But on the basis of this incident, they made other investigations and finally got a confession from this person that he had actually taken the notes.

MINK: Was that an end to his graduate career?

KNUDSEN: He was expelled at once. Michelson dealt very sternly. He was expelled immediately, even on the basis of the circumstantial evidence that he would not permit Harvey Brace Lemon to go with him to his room.

MINK: Did you attend any of these demonstrations and watch them as Dr. Lemon was doing them?

KNUDSEN: Oh, yes. As teaching assistants we were all required to attend them for one complete series, so I attended all of these demonstrations.

MINK: But you did not act as a teaching assistant for Dr. Lemon?

KNUDSEN: We were all under the supervision of Dr. Lemon. And this is an important lesson I learned later, when we were dealing with graduate teaching assistants at the UCLA campus. Lemon was in charge, not only of the demonstration lectures, but of all teaching assistants who participated in the teaching of these sections.

MINK: Quiz sections?

KNUDSEN: They were quiz sections; they were lecture sections. They were different from the sections as we know them generally around here. Lemon gave his demonstration lecture once a week and the teaching assistants did the instruction from there on through the [week]. The lectures, that is, not the demonstration lectures, but all other lectures, all of the quizzes, including the laboratory experiments, were conducted by the teaching assistant. So my responsibility as a teaching assistant at the University of Chicago was almost identical with my responsibility and duties as an instructor at UCLA. Professor John Mead Adams required us [junior staff members] to attend his lectures, but they were not only demonstration lectures, they were the formal lectures, and we had our quiz sections and our laboratory sections. So I was really doing a lower level of instruction during my first year or two here at UCLA than I did at Chicago as a teaching assistant. Lemon, for example, required all of the teaching assistants [to attend his lectures], and there were quite a number of us because there were something like 600 students in those years taking this general course in physics.

MINK: This was a graduate course?

KNUDSEN: No. This was undergraduate, at the freshman-sophomore level. But all medical students, for example, were required to take it, and undergraduate chemistry majors and physics majors and others. This was the only course in college physics that was taught. As I say, it was fairly large so that there were a very large number of sections and it required a large teaching staff of teaching assistants to do the work which was required of us at that time. But it was extremely important as a training experience to be under a master teacher, and Lemon was a master teacher of general college physics. He required all of us to meet every Saturday morning at a review and a training and instruction session, in which he told the teaching assistants the things they should know and the things they should cover in the following week, and a discussion of the fine points of the laboratory experiments which the students were going to do. There wasn't enough equipment for everybody to do the same experiment at the same time, so there was a rotation, usually, of six experiments going on at a time. So at one of these sessions, he would deal entirely with the next six experiments so that he would cover the laboratory work of all the teaching assistants over a period of six weeks. But we met on other weeks, the other five weeks, to receive instructions from him on how we should manage the teaching duties to which we were assigned.

MINK: Was he disposed to visitations to the class, something that I always feared when I was a TA here?

KNUDSEN: I don't remember his ever visiting a class that I taught, and I never heard any criticisms of him in that respect. He was a very cordial person with the teaching assistants. We were invited to his home.

On occasion, some of us were invited on trips out of Chicago to join him in his automobile. A little later I may describe one of the trips we took with several other professors at which we were introduced to the Bacon Bilateral Code, which had a bearing upon whether Shakespeare or Bacon wrote the works of Shakespeare. But I'll come to that later, if we have an opportunity. [tape off]

There were a few amusing incidents that happened during the courses that Professor Michelson gave. One in particular occurred one morning when he walked in very sedately, as he always did, into the class, and everybody came to attention the second the door opened and Michelson entered. On this occasion, it was noted that there was a necktie hanging from behind his coat, hanging down very much like an animal's tail. A snicker passed over the class, but it didn't last very long, and no one had the courage to tell Michelson that there had been probably a little accident in his dressing that day. He proceeded with the lecture, and he had a habit of rotating rather fast from the blackboard to the class. Each time he would rotate from the blackboard to the class the necktie would go through an arc. The first time this happened, again there was a little snicker, but no longer anymore. We were all concerned only with getting our notes on his lecture until the end of the lecture.

He walked out of the classroom, as it was customary, at the end of the lecture. And no one again was courageous enough to tell Michelson about this peculiarity of his habit.

MINK: To come back to Dr. Lemon's demonstrations, I can't help but think the graduate student could have gotten enough just by taking notes on the demonstrations and wouldn't have had to steal the notes.

KNUDSEN: That is a debatable question. Probably the person who stole them didn't pay as much attention to the demonstration lectures as some others. I know I felt I got the main value of the demonstrations from having seen them once. They were well calculated to stimulate interest in physics, as well as to demonstrate important principles, important experiments that had occurred in the development of history.

MINK: Did you have an opportunity to use them later when you came here to UCLA?

KNUDSEN: Yes. It was several years before I gave lectures with demonstrations. During the first years, it was largely quiz sections and laboratory sections. But I'm sure I used some of these demonstrations during one year that John Mead Adams, for some reason, was not giving the lectures in Physics 1A.

MINK: He was on leave?

KNUDSEN: He was on leave, and I gave the lectures on

Physics 1A. I'm sure I used some of Lemon's demonstrations at that time. I don't recall which ones now, but they had made a lasting impression upon me, and I know that I derived a great deal of benefit in style as well as content in giving demonstrations as a result of having been at the University of Chicago with Lemon.

Well, there are one or two other incidents about Michelson that I think should possibly go in the record. He was an athletic person even when he was sixty years old. His sixtieth birthday occurred, I know, while I was there. He was considered the champion tennis player in the entire faculty, and he kept up his tennis game several years after I left. He retired not many years after I left Chicago.

He was also an amateur violinist, although I never heard him play the violin. We were never invited to Michelson's home like we were to the homes of other professors.* Michelson was a person apart, in many respects, including fraternizing with students. We worshipped him, but we didn't really get to the throne of his home. He was reputed to be a good violinist.

He was also reputed to be an unusually able and popular demonstration lecturer, especially at ladies'

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*We were invited at least once a year to parties at the Millikan home--charades, costume parties, impersonations. Once Millikan, dressed in a regal robe, impersonated one of the popes of Rome. Staggering down the stairs, he sang, "The Pope's a jolly good fellow."

clubs. He had a lecture which was very famous in Chicago on changeable colors, which we talked about at that time. He had investigated the changeable colors in the wings of beetles, and the bodies of various insects, you know, that have changeable colors. This color change is a function of the angle at which you look at these materials, and the ladies were interested, because at that time, changeable color silks were very prominently used in the dresses that they wore. They rustled very much, and they changed colors as you looked at them from different angles. This, Michelson had demonstrated, is a layer effect of the silk itself. These various layers are separated by distances that are of the order of visible wave lengths, so when you look at these from different angles, you get interference and reinforcement of these reflections from the several layers. Michelson had beautiful demonstrations to show how these color changes occurred with the angle at which you viewed these insects or the changeable silk.

I think it's possibly worthwhile also to mention his last experiment, which was conducted on the Irvine Ranch, which now houses the [University of California] Irvine campus. Michelson had worked in the open prior to this time, but he felt that his measurements of the velocity of light could be improved, the precision could be improved, if this could be conducted in the long underground tunnel. A mile-long tunnel was constructed for him, according to his specifications, on the Irvine Ranch. His last experimental work (1931) was the measurement of the velocity of light in this long tunnel, which led to a higher precision than had ever been attained before, and has since then remained the standard value of the velocity of light, from the point of view of the experimental determination.*

Michelson was writing up a part of the results of this research. I am told by others at Cal Tech, who were closer to him at that time than we were over here, that he was writing up the results as death was taking its hold of him. It reminded one very much of the death of Socrates. The story told about Michelson's death was that he felt death creeping up from his feet. His feet grew cold, and it spread upward, and he knew it was going to hit the vital organs sometime and that would be the end. He was racing against death to complete this manuscript he was working on. The manuscript, according to my informants, was not completed at the time of his death.

MINK: Would he have come to Cal Tech from Chicago as a result of Dr. Millikan's coming?

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*The resulting velocity was 299,774±11 kilometers/second [V.O.K.].

KNUDSEN: Well, it made a cordial relationship, and certainly they had been very close collaborators in the development of the University of Chicago. Of course, climate and availability of a large tract of land at the Irvine Ranch and the facilities of Cal Tech, which were certainly made available to him at that time, made this a propitious place for him to work. Certainly he wouldn't come to UCLA [laughter] at that time. We didn't amount to very much in the way of a research institute. Cal Tech was among the top three places in the country at that time. I presume of the three or four, you would certainly place it in the class of Chicago itself, and of MIT [Massachusetts Institute of Technology] and Harvard.

MINK: Then he followed Dr. Millikan to Cal Tech?

KNUDSEN: He was retired when he came to California to conduct this final experiment on the velocity of light, and he was a guest, really, at Cal Tech. He was not teaching then. He'd been retired for some time. As a matter of fact, he was kept on at the University of Chicago a year or so after he reached the compulsory age for retirement, and this was an exception that was made for Michelson. I don't know whether it was made for any others or not, but I do recall it was made in his case.

Well, that's the end of these anecdotes. There are many others. As I said before, some historian should write comparative biographies of Millikan and Michelson. There's one man living that I think is better qualified than anyone else. He came with Millikan to Cal Tech when Millikan left Chicago in 1921. His name is Earnest [C.] Watson. He's written many articles on the history of physics, and he could do this. He was so close to Millikan that he would probably be much more acquainted with Millikan's career than with Michelson's. He was at Chicago. If there was someone who would have as much experience with Michelson and also with Millikan, as Watson has had with Millikan, that would be the ideal man. This would be a very valuable contribution to the development of American physics and, indeed, American science, because these two men did make an important contribution at Chicago, as I have mentioned earlier. They are so different in their detailed qualifications, in their human traits and in their habits, that it would be a worthwhile biography.

MINK: I don't think you talked at too much length about Dr. Gale in our last session.

KNUDSEN: I probably overemphasized a few of his profanities, for which he was very well remembered around the campus. Gale's course was primarily a course in geometrical optics, and he used the first hundred pages or so of the standard book on optics of that time, which was [Paul K.L.] Drude's Theory of Optics. I recall an anecdote similar to the one that I mentioned about Michelson. Gale also did not want to be interrupted in his lectures. In one lecture I recall particularly, Gale was hesitating a moment over a derivation in geometrical optics that puzzled him. I believe it was this same bright student that had spoken out of turn in Michelson's class, that said, "Professor Gale, Drude says so-and-so." And he turned to this [student]. He said, "I don't give a continental what Drude says. I've got a mind of my own. You don't have to tell me what Drude says."

This [course] was the principal one that Gale gave. It was the only course I had under Gale. He taught a course in spectroscopy, which I did not take. He was generally credited as the person who had built up the spectroscopic program at the University of Chicago, in which Millikan and Ira Bowen later became very much interested. I've spoken about that previously.

Gale became, really, the Dean of [the Division of] Physical Sciences at the University of Chicago. The structure there was somewhat different than it is here, as you probably know. They have these four divisional deans, and this extends from the college all through the graduate division. They didn't have strictly a graduate dean as such. But Gale represented the Chicago Graduate Division at the meetings of graduate deans I attended, when I was a graduate dean here. The meetings were those of the Association of American Universities, in which the presidents of some forty "high-hat" universities and the graduate deans of those universities met together. Gale was a fairly prominent member among the graduate deans at that time. He was always a character. He was one that enlisted very soon in World War I, and he was long known as Major Gale by many Chicago people. He was a very able collaborator with Millikan, and as I've mentioned, Millikan and Gale, <u>The First Course in Physics</u>, became a classic, and I have no idea how many copies were actually published and sold. It must be over a million. It was really used as a standard high school text almost everywhere. I believe that's enough about Gale.

MINK: Do you want to go on now and talk about your preparation for your doctoral examination?

KNUDSEN: Yes. The dissertations for the doctoral degree in physics at the University of Chicago at the time I was there were almost always directed by Millikan. Millikan was always consulted by the students, or actually invited by Millikan to come and discuss with him the selection of a dissertation. Most of the students did work on dissertations that were closely related to the research Millikan was doing in photoelectricity, the elementary charge on the electron, the Brownian movement and matters related to the emerging field, then, of atomic physics, of which he was undoubtedly the principal scholar-research man in the United States. I think it was generally conceded that he occupied that position. I had at least one conference with Millikan about a dissertation for me. I had met Millikan prior to my going to the University of Chicago, and he'd been very cordial to me, primarily because I was a student of Harvey Fletcher. He and Fletcher had had so much in common during the years Fletcher was a graduate student there. Millikan discussed with me various problems, but there was one particularly that he wanted me to work on, namely, the contribution of electrons to the specific heat of metals or conductors. This was a problem that had baffled quite a number of distinguished physicists. Einstein had made some progress about the contribution of all oscillators within a lattice. Peter [P.] Debye, who was later a Nobel Prize man, had worked a great deal on the heat capacity of vibrating particles in metals. This problem was of interest both to physical chemists and to physicists at that time, and was one of the really foremost problems that physicists were concerned with.

The room to which I was assigned for my research and for my headquarters as a teaching assistant--I had a private room, which was one of the luxuries we enjoyed at Chicago at that time--as a matter of fact, happened to be the room in which Fletcher and Millikan had worked together on the oil drop experiment. So it was a rather hallowed room, so far as graduate students were concerned at that time. This was not assigned to me as any special favor to Vern O. Knudsen, but simply because the timing was of such a nature that this room became vacant at the time I was ready to occupy a room, and I had this room exclusively for my own research.

Well, I couldn't become enthusiastic about the problem Dr. Millikan had suggested, not only because some distinguished physicists had worked on it and had been unable to make much progress, but [also because] a graduate student before me had worked two years on the problem and abandoned it because he couldn't really make sufficient headway to make it a satisfactory fulfillment of the requirement of a dissertation. I was a young married man at that time, actually expecting the firstborn a few months in the future. I didn't feel that I should commit myself to a problem the outcome of which was as greatly in doubt as this one was at that time. Furthermore, I was much more interested in acoustics than I was in electron physics at that time, or atomic physics or the specific heat of metals and what electrons would have to do with the specific heat.

I realized that this was a very important problem, but I had acquired, really, a technique in the use of vacuum tube circuits and oscillators in particular, as one of our earlier interviews would indicate, and so I wanted to work on a related problem. The opportunity came as a result of one of Millikan's visits to Europe. He traveled a great deal, and as I believe I have mentioned, he was very much in demand in Europe as a lecturer and as a scholar-researcher in electron physics in England and Germany and Holland, particularly those three countries. He was on an extended visit when I discussed with Gale my desires about my research and dissertation. Gale said, "Well, here's your opportunity, Knudsen." He said, "You get going on this problem you want to work on," which was the matter of adapting vacuum tube circuits to measuring certain properties of hearing.

MINK: While Millikan was gone?

KNUDSEN: While Millikan was gone. He said, "You get going on this and have it so far along by the time Rob Millikan comes back, he won't have the heart to take you off from the problem." And Gale was right. I followed his advice. I did work furiously. I had twenty hours a week of contact with students in my teaching duties, and I was taking a class or two besides. But I'm sure I worked at least thirty or forty hours a week during the time Millikan was away and got my apparatus together and was going great guns on the project by the time Millikan came back. He came in to see me, and he gasped when he saw what I had spread out on the table there. We all made our own experimental setups.. We had very little help from the instrument shop at that time. I had assembled the circuitry of the vacuum tubes, the capacitances, the inductances and resistances, and other essential equipment. Certainly very few people knew more about vacuum tube oscillators than I did at that time.

MINK: Was it purposeful or was it a financial consideration that you were not allowed help from the shop?

KNUDSEN: Well, my problem was one in which the shop couldn't contribute anyway. There were very few graduate students who had access to the shop. The shop served mostly Michelson and Millikan and a few other full-time staff members. There was a student shop where we were permitted to make our own things. We had a drill press and a lathe and some other useful tools and equipment. On Saturdays we usually used this shop. Saturday afternoons the married students were there, mostly to repair home appliances. Routinely, I had to repair a baby carriage. The rear axle would break almost every week, because we lived on the second floor, and you had to take the baby carriage with the baby in it upstairs backwards. Really, the strain on this rear axle was enough that it often would break, and I would mend it over in the student shop on Saturdays. But this was a shop where we made much of our research apparatus, though most of mine was a matter of assembling units that required relatively little shop work.

MINK: Well, Dr. Millikan gasped, but he didn't object.

KNUDSEN: He did not object at all. He became much interested, and when he had distinguished visitors, he almost always brought them to my laboratory, and we checked their hearing. The title of the project I was working on, which led to my dissertation subject is, "The Sensibility of the Ear to Small Differences of Intensity and Frequency," and this led to my first publication. It was published in the Physical Review in January of 1923, which you see was a matter of six months or so after I had left Chicago in the summer of 1922.

MINK: Could you summarize the results of this research?

KNUDSEN: Yes. I think I can. Much of it is of a nature that's not mathematical. There's some simple mathematical aspects of it, but this sensibility is a term that was used then to measure the smallest perceptible changes, and this is related to what the psychologists refer to as the Weber-Fechner Law. You probably have read about it in psychology textbooks. The Weber-Fechner Law, in general, is an oversimplification; but the law generally states that there is a constant value of the ratio between the smallest perceptible change in the physical characteristic of what you are sensing and measuring, whether it's smell, taste, seeing or hearing, or things of this sort, the ratio between the smallest perceptible change and the total magnitude of the sensation that you're judging. In the case of the hearing, sensibility of the ear to small differences of frequency and intensity means that you want to [determine] what is the smallest percentage change in the intensity of a sound that you can detect. As a function of its intensity, is this a constant, as the Weber-Fechner Law says it should be? Or does it depend upon the intensity? And in the case of frequency--that is, of course, a thing which is dear to the heart of all musicians and so on--how small a change in pitch or loudness can you detect? That's what I was investigating then with the new facilities of electron tube circuits. And how are these sensibilities, as we called them then, dependent upon the frequency or the pitch of the sound, and the intensity or loudness of the sound?

The methods that had been used in the past were relatively crude. That is, they used tuning forks and how far away you have to move the tuning forks, say, before you could recognize a change in loudness. They would change the length of a bowed string, or pluck this or that string, or sound two different tuning forks, to determine the smallest recognizable change in pitch. There wasn't good agreement between the results of different investigators, and it was apparent that there was need for an improvement of this Weber-Fechner Law in respect of hearing; to investigate the Weber-Fechner Law for hearing now with the precise tool that was available in the vacuum tube oscillator for actuating a telephone receiver. The telephone receiver then becomes a means of producing small and known, determinable values of change in frequency or change in intensity. The change in intensity can be very easily determined by the change in a resistance in the circuit. The change in pitch also can be very precisely determined by changing the amount of capacitance in the oscillator circuit. You have two capacitances in the circuit, one of which is variable, and that can be cut in and out.

The technique that I used is called a flucuation technique. When you hear the tone, say it goes "ah ah ah," it will have to keep the same pitch, but I can demonstrate it this way by rapping on the desk here. [raps on desk] I think you will recognize that the second rap was louder than the first. [raps on desk again] Maybe you can't distinguish the difference there. As a matter of fact, it was known generally that for changes in loudness, the intensity had to change of the order of 10 percent. My findings confirmed that. But my findings indicated that the smallest perceptible change of loudness was a function of the loudness itself. When you start near the minimal audible threshold of loudness, that is, when it's just barely audible, you'd have to increase the intensity as much as 30 percent. [tape off] That is, for feeble intensities, the change in intensity had to be as much as 30 percent before the ear could recognize that there had been a change. This change was observed in the following way: You listened to a sound which is fluctuating in intensity, about once a second. The person under observation (the subject) was to signal just when this changed from a fluctuating sound to a steady sound. This, you see, eliminates a time interval between the judgment of this [sound]. [raps on desk] They [the raps] are separated by a second or so, something like that.

All the measurements that had been made before were discreet [time-separated] presentations of loudness.

The technique that I had, using the vacuum tube oscillator with the devised circuit adjustments, allowed the intensity easily to be dropped down to the threshold of audibility. Work had been done previously by the Bell Laboratories, especially in developing the audiometer, which was known to me at that time. So we knew, really, a great deal about the threshold of audibility, and I was able to attain the threshold of audibility. We'd begin there, where it was just barely audible. Then we'd have to increase something like 30 percent before it could be detected. When we got up to what we would rate today as 40 decibels above the threshold--that means an intensity level at 10,000 times the threshold intensity--there it steadies off to this value, which is very close to 10 percent. From there on up to intensities that go up very, very much more, certainly up to 70 or 80 decibels above the threshold, it stays constant. In that range the old Weber-Fechner Law is pretty valid. That is, there is a constant value for the ratio of the smallest discernible change in intensity and the intensity of the stimulus. This value of 10 percent was very well established and has been confirmed repeatedly since then.

Now on the sensibility to difference of frequency or pitch, there again, I used that same sort of fluctuation. The smallest perceptible change of frequency has to be a bigger change for feeble intensities than for greater ones. At usual intensities (or loudness) the smallest perceptible change of frequency is about three parts in one thousand. [Dr. Knudsen here refers to his published dissertation, "Sensibility of the Ear to Small Differences of Intensity and Frequency," Physical Review, 2nd Series, v. 22, no. 1 (Jan. 1923), pp. 84-102.] This was my first scientific publication. The results I'm referring to are on pages 92 and 93. [I'm] referring now to these changes in frequency (or pitch), which is a rather surprising result. In the first place, there was the same dependence upon the intensity or loudness of the tones the subjects were listening to. We listened always to a simple, pure tone that was generated in the high quality telephone receiver by the vacuum tube oscillator. Then this tone was fluctuated by cutting in and out a little amount of capacitance--a small capacitance (consisting of a variable air condenser) could be added to or taken away from the major capacitance--which introduced a change in frequency. Simply by changing the magnitude of that capacitance, you'd change the pitch by any amount you want. You'd start out and it'd go: [Here Dr. Knudsen orally simulates fluctuations in pitch], and then they'd get closer and closer together until finally it became one tone. At that point you reach the smallest perceptible change under the most favorable condition, say, for recognizing differences. And here these smallest recognizable changes were those shown on page 100 of this reprint. You will note that you have to have a bigger change in the frequency of a low pitch tone than you do for a high pitch tone. For example, for a tone having a frequency of 50 cycles a second, which is a little more than an octave above the lowest frequency which the ear recognizes as a pure tone, the change has to be as much as 1 part in 100, 10 parts in 1,000; whereas, when you get up to about 500 cycles you reach a leveling off in which you can recognize the difference of 3 parts in 1,000. It continues at 3 parts in 1,000 up to as high as we were able to go in these experiments, up to about 4,000 cycles per second, which would correspond closely to the pitch of the highest note on the piano. So from an octave above middle C to the highest note on the piano, using the piano keyboard, under these most favorable conditions, the ear--I just say ear, not specifying it otherwise--can recognize a difference of about 3 parts in 1,000. This result, surprisingly, was almost entirely independent of the nature and training of the subject under test. This was a rather startling finding in this investigation; the musical aptitude, the musical talent, or even the presence of absolute pitch on the part of the subject who was under investigation, had no apparent effect on his ability to recognize when a change of pitch occurred.

This means that the process is basically physiological or physical, and that it depends upon the resonance characteristics of the cochlea which are essentially the same for all cochleas. The human cochlea is therefore a standard frequency analyzer, a physical instrument that is about the same for all hearing persons.

MINK: That's very interesting. Did you use graduate students and faculty subjects?

KNUDSEN: Yes. All of these light lines you see on this figure in the reprint are the individual results on different individuals. There were about twenty different subjects (mostly graduate students) who participated in these experiments. There's quite a spread, as you can see here, but the deviation from the average was surprisingly small. But there was no outstanding advantage [for] those who were musically trained or musicians, talented musicians, or even those who were able to identify the pitch. Now, this is another thing which involves memory. This does not.

Now we also measured how large a change you had to make in order to recognize a difference in pitch when there was an interval, time interval, between the two tones that were presented. And this separates, of course, the musically talented, and especially those with absolute pitch, from the others. That is, those who do not have absolute pitch would have to have a a relatively big change if there's an interval of only two or three seconds. This was enough to make a big difference. You can't detect anything like a difference of 3 parts in 1,000 when there's a time interval between them. And for some people, you may have to have as much a change as 50 to 100 parts in 1,000 (more than a semitone) to have them recognize a difference even when there's a time separation of only a few seconds.

MINK: So then did you purposely select people who were musically talented and those who were not?

KNUDSEN: We had both kinds there. Yes. We had them. There were a number of people from the music department there that were invited to come over and participate in the experiments. We didn't have an Isaac Stern or a [Jascha] Heifetz, or anyone like that participating in these experiments, but we did have some who were musically trained, and they were all amazed that they could not do better than the physicists over there [laughter] who knew nothing about music.

MINK: Well then, when all of this research was finished, then you had your final examination. You did not have a written examination?

KNUDSEN: I'd like to say one other thing about this, then I'll come to the examination.

The results of this investigation led Millikan to suggest that I should meet Professor George E. Shambaugh, who was the head of the Department of Otology at Rush Medical, which was then a part of the University of Chicago. They were on separate campuses then. Shambaugh was definitely recognized as the dean of otologists in the United States at that time, and his name is still very highly revered. He was a close friend of Millikan, and Millikan said, "Well, you should meet Dr. Shambaugh. Shambaugh should know about these results." And Shambaugh was very much interested in them. He said, "Well, I wonder what would happen if we take some of our patients who have impaired hearing." So Shambaugh began sending patients to me to determine how small a change in pitch and loudness they could detect. Well, if you got sound intensity up above their threshold, they could do as well as persons with normal hearing, with a few exceptions that I'll mention a little later.

This led to an association between Shambaugh and me, and we published two papers together that were based on our joint work which was done while I was still a graduate student during my last six months or so in Chicago. The one is entitled, "Sensibility of Pathological Ears to Small Differences of Loudness and Pitch," which was essentially doing the same thing as I described in the dissertation for patients that were referred to me by Shambaugh, who had marked hearing impairments or minor hearing impairments, all of which were of a cochlear or a nerve nature. And in general all of these patients, with a few exceptions, could do as well as persons with normal hearing. That is, they could detect a difference in frequency above 500 cycles per second of about 3 parts in 1,000. And they could detect a change in intensity of about 10 percent of the intensity of the stimulus.

MINK: And this was published where?

KNUDSEN: The one on "Sensibility of Pathological Ears to Small Differences of Loudness and Pitch" was published in the Transactions of the American Otological Society, in 1922. I note here, actually before my dissertation. I had always supposed it was a little later.

The principal finding was that their pitch and loudness sensibility was as good as that for people with normal hearing, even though they may have had very gross hearing losses, that is, the ones with, say, old-age deafness or "boilermaker's deafness," which means a large loss of hearing of the high frequencies. If you've raised the sound level above their threshold, they could still do as well as a person with normal hearing. I'll have occasion to refer again to this smallest perceptible change in intensity when I discuss some research I did here at UCLA, which indicated the finger tips, in sensing intensity of vibration, have the same characteristic that the ear does for recognizing changes of intensity. We'll go into that on a subject called hearing with the sense of touch, which is one of the subjects we investigated some years later here at UCLA.

The other paper that I did with Shambaugh, entitled "Report on an Investigation of Ten Cases of Diplacusis," was also printed in the Transactions of the American Otological Society, this in 1923. Now, diplacusis needs to be defined. It's not generally known in the popular mind. Dipl [Greek] means twos, you see, and diplacusis [hearing] two sounds. Diplacusis, then, means that you hear the same physical tone at a different pitch in each of the two ears.

Now Shambaugh would sometimes make a [note] that there was some evidence of this diplacusis among the patients he saw, and he saw many patients. Shambaugh was so prominent in this field at that time in connection with problems of hearing, and surgery associated with otology, mastoiditis and operating on mastoid bone, removing the tonsils and other things that were associated with hearing impairments at that time. He had--during the time I was there--eight assistants--mostly young doctors. These assistants were young, aspiring otologists, who came, many of them, from foreign countries and other places. It was considered an unusual opportunity to be able to work with the great Shambaugh in problems of otology at that time.

His own research had dealt largely with the function of the cochlea and what part of the cochlea is largely responsible for analyzing sounds into their various pitches, that is, their frequencies. Shambaugh was a proponent at that time that the tectorial, that means the roof, or the upper membrane in the cochlea, was really the one that did the job of analyzing, and his opponents and many others believed it was the basilar [basement] membrane. In the end both membranes were found to participate, but today the evidence favors the basilar membrane. The hair cells are between the basilar membrane, and the tectorial membrane. These hair cells are stimulated by a mutual vibration between the two membranes when the ends of these hair cells are impacted by the basilar membrane, which, as it vibrates, pushes up against the hair cells. Later research demonstrated that the basilar membrane is a much more suitable vibrator than the tectorial membrane. But Shambaugh was investigating the ear primarily anatomically. That is, he was getting the cochleas of young pigs and other young animals and examining the tectorial and basilar membranes histologically. He was much interested in how we hear, and just what part these membranes might play in the analyzing of sound into tones of different pitch. Diplacusis, of course, was an exciting thing to me, and when one of my associates at Chicago learned that I was investigating this problem he came to me. He said, "You know, I'd like to have you measure my hearing." He was an instructor in physics at that time, Munk by name, a spectroscopist. He worked with Dr. Gale. He said, "You know, when I hear a sustained tone on the violin I can close one ear then close the other. And he said, "You know, there's a change in pitch of about a semitone. It goes [Dr. Knudsen modulates tone vocally] this way." So we investigated his hearing, and sure enough, he had analyzed approximately that there was a difference in pitch in his two ears. We had the means of switching [the tone from one ear to the other]. You listen to this, and you adjust the frequency of the sound in one ear until both are judged to be of the same pitch. You then ask him to hum the sound as heard in each ear, which usually enables you to determine which ear is "off pitch." When they sounded the same pitch in the two ears, we measured the difference, and found in many cases half a semitone or more was quite a usual phenomenon in diplacusis.

It was always associated with chochlear or nerve impairment. There are in general two kinds of impairment of hearing. One is a mechanical defect that's usually in the middle ear, (although it can be a plug of wax or obstruction in the external ear canal), and that doesn't affect the cochlea at all. The other is in the internal ear, which does affect the cochlea. This phenomenon of diplacusis, whenever it was found, was associated with a cochlear impairment, that is, with internal ear or nerve deafness. And this investigation of ours was the first comprehensive study that had been made of this problem of diplacusis. The extraordinary thing about diplacusis was its prevalence. These ten cases were simply selected from the routine patients that came to Shambaugh over a period of a few months. We were working on this project not more than two or three months.

MINK: Well, I think I have a question here. You said that this graduate instructor in the University of Chicago had come to you, and this had caused you first to measure and discover this difference?

KNUDSEN; He knew that I was working with Dr. Shambaugh on this problem, when he came to me.

MINK: And Shambaugh, himself, had been aware?

KNUDSEN: Oh, Shambaugh was aware of this, but he didn't have an audiometer or any precise means at all for measuring such small differences of pitch. He knew the problem existed. We published these two things together as a result of my collaboration.

The only other thing I wish to mention about Shambaugh is that he was a very delightful person to work with, and my experience with him was an important chapter in my life. When he knew I was coming to California, he said, "Oh, there's one person you must meet. That's Dr. Isaac H. Jones. He certainly is the most important, outstanding investigator in otological problems, and particularly the problems of the hearing, and especially of the vestibular mechanism in man, that there is on the West Coast." And so it was my work with Shambaugh that led, less than a year later, to my calling on Isaac H. Jones and telling him about my work with Shambaugh.

MINK: And that began your association with Dr. Jones and your extended work with him?

KNUDSEN: Yes. This collaboration with Dr. Jones continued at least for fifteen years.

SECTION VII

KNUDSEN: The doctoral examination of candidates at the University of Chicago for the PhD in physics, as in most other departments, is a departmental affair, and there is no graduate division or graduate dean that specifies certain requirements as having to be fulfilled. These were handled entirely by the department. The foreign language examinations were conducted by the departments of foreign language. You would go to the German examiner in the Germanic languages department for the examination in German, for example, as I did, and to the French for the one in French. German was considered by us then as the most important foreign language for physics. As a matter of fact, in my own field, even now as then, there would be at least four or five articles in German to one in French. I had had some formal courses in German at the time I began my graduate work, a one-year course at the high school level and another one-year course at the college level, at the Brigham Young University. Then I had a course also in scientific German at the University of Chicago, and in one of the courses that Millikan taught the only book we used [was in German]. He didn't use a textbook, as I've mentioned previously, but he required all of us to obtain a copy of [Arnold J. W.] Sommerfeld's Atombau und Spectralinen and this was the first important book, really, that began with the Bohr theory of the hydrogen atom. Sommerfeld had done much of the early subsequent work, and this book was a must among atomic physicists and other physicists of my era. I really polished off my German by reading the entire Atombau und Spectralinen at the time in Millikan's class. It was good, good training for all of us, and a good preparation for the doctoral German examination. It was pretty easy to pass the Germanic languages' examination.

In contrast to this preparation, my preparation for the French examination consisted of cramming French during a one-month interval between the summer quarter and the autumn quarter. Then I appeared [before] the French examiner. You always brought with you two French books, and he would select something from these books. It was traditional for the physics students at that time to always take a book in geometry, and this was one of the books I took along with me when I appeared before the French examiner. He looked at me, I know, as he must have looked at many others, with a bit of contempt, and said, looking at the geometry book, "Hmph." He said, "The more technical the book is, and especially in a mathematical sense, the easier it is for the candidate and the more difficult it is for the examiner." But even with that one-month preparation, I was able to pass the French examination. Other students said you just breeze through the reading examination in French. That was the traditional thing at that time.

Well, the program of graduate work, as I say, was largely under the department. It was traditional for graduate students, if they were any good at all, to complete [the PhD program in] three years. This was more or less a standard practice, but there were three years of four quarters each so that there would be normally a total of twelve or thirteen quarters, if you stayed through the summers. Say you began in the fall and you did maybe a total of twelve or thirteen quarters; very few would go beyond that, that is, if they flunked the PhD examination. It came at the end. There was only one grand final PhD examination. We didn't have any for advancement to candidacy or the oral comprehensives, or our series of divisional examinations. We have examinations in five different branches of physics that every candidate must pass separately before he takes his final comprehensive examination for advancement to candidacy.

At Chicago there was one grand finale at the end in which you were quizzed by all the professors that had taught you. They were all invited, and you had to have a minor as well as a major. My minor was mathematics, which was quite customary at that time. So I know at my doctoral examination there were three men from the Mathematics Department, probably five or six from the Department of Physics. In general, the mathematics people would come in and do their stuff and then leave. The voting was usually done at the end by the survivors of this examining committee. Sometimes this examination would be in two half-days, in the morning and the afternoon, with a recess at lunchtime. Most candidates fortified themselves by taking a Thermos bottle filled with coffee to stimulate themselves during this ordeal, which was never less than four hours, and sometimes it was two sessions of three or four hours each.

MINK: That's a longer examination than is traditional here at UCLA.

KNUDSEN: Yes, it's long, and to prepare for it was really an ordeal too. I know in preparation I used brown wrapping paper in big pieces, and I had the walls of two rooms in our four-room apartment literally papered with these brown papers on which I had summarized really all the derivations of the important theorems and principles in physics and mathematics for all the courses I had taken at the University of Chicago. And I knew the night before that examination that I could derive any one of those equations I had gone through and had around there the last few days. I'd just go from one derivation to the other and see if I could go through these derivations in my mind. We were aided a great deal in this preparation by what was known as the "Bone Book," which was a book that was handed down from candidate to candidate, and the latter, as soon as his examination was over, was required to write in the book, as nearly as he could remember, all the questions that were asked of him by Michelson, by Millikan--Millikan had left at the time of my examination, so I was not examined by him--but by Gale, by Lunn, by Dempster, by Lemon, by the others who had participated in the examination and the men in mathematics. So the "Bone Book" at the time I came up for my examination must have had, well, at least twenty-five or thirty summaries of questions that had been asked of the candidates before me. And by that time, you saw a pattern of repetition among the questions of Michelson that you knew, the thirty or forty questions from which Michelson would select maybe three or four for your examination. So if you boned up on those thirty or forty special questions, some of which you knew Michelson would ask you, you were well prepared for that part of the examination. The same is true of the other courses, although particularly of Michelson. He had this routine use of typical questions. He gave his four courses over and over again, and if you had done your homework on them and prepared for the final examination, you could really breeze through them, and I know I did. The same was true, pretty much, of the others' questions, and for me it was not a formidable experience that day, because I knew I was prepared for it. I had worked harder than I had ever worked in my life, and I developed my first peptic ulcer, I know, as a result of this last semester's work. This was when I had my first symptoms of pain in the stomach that followed the cycle which I later learned to know as a duodenal ulcer. And I'm sure some other graduate students must have suffered the same way. We were under a very heavy load. I was teaching, as I said, twenty contact hours a week, which is a double appointment as a teaching assistantship. It paid double pay, and that was worth a great deal. Five hundred dollars plus tuition a quarter was what I was getting then, which was $2,000, plus tuition, which is nearly as much as I got my first year as an instructor here, at $2,400 a year. I'd been offered $2,000 a year to stay at Chicago.

Well, this is the main thing of the examination. Then you were dismissed from the room while they cogitated, and they would announce the outcome of your examination, whether you had just graduated, completed a satisfactory examination, or whether it had been worth a <u>cum laude</u>, <u>magna cum laude</u> or <u>summa cum laude</u>. I managed to pull down a _magna cum laude_ but not a _summa_.

MINK: Then you started work on your dissertation?

KNUDSEN: No, this comes after the dissertation. But I was not asked a question about it. It normally includes a very detailed questioning on your examination, but Gale, who was more or less the spokesman then, said, "No one here knows anything about his dissertation, so I suggest we not ask any questions."

MINK: You know, I was thinking when you were saying that this was final and irreparable, this meant that a person's dissertation would go out the window if he didn't pass the exam.

KNUDSEN: He could come up for a second trial at the examination. That happened sometimes, but in many instances, he was encouraged to settle for a master's degree.

MINK: Terminal master's?

KNUDSEN: Yes. Quite a few did, not, I think, by that time more than one or two in ten or twelve. I know two or three instances in which the consolation master's degree was awarded.

MINK: Would they just award them a master's degree and let it go at that, or would they have to take an examination?

KNUDSEN: Oh, I frankly don't remember. I think they had earned the master's degree; then it was issued at the next commencement.

MINK: Well then, you had completed your examination.

KNUDSEN: That last quarter, the spring quarter of 1922, I also finished the writing of my dissertation. I had this teaching duty. I was doing some private tutoring on the side to supplement my income and preparing for the final examination.

MINK: You finished writing your dissertation before you actually took the examination.

KNUDSEN: Yes. Everybody had to do that. It had to be approved, really.

MINK: And they never did ask you any questions about it?

KNUDSEN: They didn't ask me any, but normally if the man who would supervise the dissertation [is at the examination], he could ask questions about the dissertation there.

MINK: So it was in this period, then, that you were considering where you might go to teach?

KNUDSEN: That is right. We might just briefly review that in the remaining few minutes we have today. I had left the Bell Telephone Laboratories nearly three years before with the intent of returning to the laboratories, and the Bell Laboratory kept on my trail during this time. When they learned that I was going to graduate that year, they reminded me of the discussions we'd had about the possibility of my returning, and they wanted to know if I was still interested in returning to the Bell Laboratories. I discussed the problem with Gale. Millikan was away at that time. And Gale said, "I'll tell you what to do, Knudsen." He said, "You put the salary at which you're willing to go back there so high that they won't want you." Because I had made up my mind then that I would rather go in for a teaching career than return to the Bell Telephone Laboratories. So I cogitated about that and discussed it with my wife and decided we would ask for $4,000, which was quite a lot in those days, as compared with $2,000. Gale had offered me to stay at the University of Chicago at $2,000 a year as an instructor.

I had received an offer, also, from Portland, Oregon, Reed College, which was a reputable place even then, in teaching particularly. I had been interviewed by someone from [Reed], I guess it was the physicist there. I forget his name at this time. Then I had this visit from W[illiam] W. Campbell, who then was vice-president of the University [of California], and was out looking for an instructor, and had called on Millikan to see if there was anyone there. Millikan said, "Well, I think Knudsen is one you should interview." I don't know if he interviewed others or not. At any rate, Millikan brought Campbell to me, and I met Campbell for the first time. I may have told you earlier about this interview with Campbell. Campbell told me about this junior college down at Los Angeles. He said, "It's only a junior college now and it ought not to be anything more, but the Chamber of Commerce of Los Angeles and other boosters down there are determined to make it a real university." And he said, "I don't think anything can stop them." So this is the word I had from Campbell at that time. When I told Gale about this, he thought I had lost every scrap of sense I'd ever had and his comment to me was, "My God, Knudsen! It's only a junior college." And he said, "These people out there are such boasters. I don't see how you could ever be at peace with their boasting." [laughter] So this was the advance information I had. I took the physical examination for the laboratory job at New York and passed that, and it was then that I received a formal invitation from John Mead Adams by letter to accept an appointment here.

SECTION VIII

KNUDSEN: My appointment [at University of California, Southern Branch] was as instructor in physics. We had some of those. Maybe we'll want to refer to this letter to John Mead Adams.* I haven't seen it. I think I should see it, and maybe review my memory on that, because I'm sure I don't remember much of what's in that letter. You said there was some argument about salary, and I'd even forgotten that. But when you mention it, I know, I had been offered--I guess it was $2,000--by, whether it was by Moore or Adams I don't recall. But we had an offer to go to Bell Laboratories at $4,000 and it just didn't seem that I could quite afford to come out here at $2,000, and so I think I asked for $2,500 and they sent along an offer of $2,400, which I accepted. I don't know what else was in there, but the letter records events that led up to my acceptance of an appointment as instructor in physics at UCSB. There were four offers that I was considering at that time. [The year] 1922 was a very propitious time for a PhD because there had been a shortage of training of PhDs during the war years,

*Vern O. Knudsen to John Mead Adams, Chicago, May 11, 1922. Records of the Chancellor's Office Record Group A3, Series 1, box 30, Physics Department.

and this was pretty much the first crop of those who were trained following World War I. Therefore, there was good pickings for young PhDs, especially in physics.

I think it will interest you to describe a visit to the Riverbank Laboratories, which was really, I guess, the only privately supported acoustics research laboratory in the country, and that had an effect on my choice of architectural acoustics as a lifelong career. Also, that's where I was first introduced to this Bacon controversy. I'd like to review a little of that. I believe you agreed to have it in the record.

In that connection, I suggest you read the latter part of this Speak Out for Shakespeare, by Guy Endore. I think he gives a good, nontechnical review of the Bacon controversy. Finally, [he] has an old-timer come out and recite something from Midsummer Night's Dream and then it ends up here on "To be or not to be . . ." And this old-timer, he's unidentified as he comes up, and he's in rags and so on, but he's evidently given his life to Shakespeare. He is asked, "Well, what is your name?" He said, "Oh, just call me Shakespeare." So that's where the title came from, this old-timer. But he reviews the controversy involving Marlowe, Jonson, Lord Bacon, and all the others that have been mentioned. Guy Endore has the printer appear also and [he] gives convincing arguments. You know this five-year experience Shakespeare had had as a printer's apprentice. He learned the vocabulary there and he called attention to all of these printer's terms that he'd used there.

MINK: How was it that this came up, this visit to the Riverbank acoustical laboratory?

KNUDSEN: Well, I was invited to go with [Professor] Harvey Brace Lemon, who had an automobile, and Arthur Dempster, who was professor of physics, and Professor Lunn, who was professor of mathematics but taught courses in the [University of Chicago] Physics Department, and there probably was one other. I don't remember the other [one]. There were five of us. This was the time when automobiles were five passenger, and I know I was elated because Professor Lemon had invited me to join them on this trip, a Saturday trip to the Riverbank Laboratories, which was at Geneva, Illinois, about forty miles west from Chicago.

The Riverbank Laboratories were built by a fairly rich person, Colonel Fabyan, who was a cryptologist. He also was very much interested in the work Wallace C. Sabine had done, the pioneer work in architectural acoustics at Harvard University, which began in 1896 or thereabouts. Sabine had worked on this subject until World War I came along, when he did research with the radio people in the war. He died at the time of the flu epidemic--but not from the flu. I think it was from a surgical procedure that he'd delayed during the war. I've been told that if he'd had surgery early enough, it probably would have saved his life.

This laboratory was really a memorial to Sabine; and it was the first well equipped, or [only] equipped laboratory for experiments in architectural acoustics. Even today, it is one of the two principal laboratories for measuring the absorptive properties of acoustical materials or the sound insulation of various partitions. The other is the National Bureau of Standards.

MINK: Well, I imagine it has changed from what it used to be. Could you describe it as you saw it then?

KNUDSEN: Yes. There was a remarkable reverberation room and an adjacent smaller reverberation room, and they could build partitions between these two rooms, simulating different kinds of wall and floor-ceiling structures. They also investigated for the first time, in a suitable laboratory, the absorptive properties of materials.

But there were two other things that they were interested in. This Colonel Fabyan had quite a collection of electrical organs. They were the forerunners of the present-day electrical organs. He had several of them on display, which we listened to. Professor Lunn, who was with us, the mathematician, was a first-rate organist.

As a matter of fact, he served as organist for the University Baptist Church near the campus of the University of Chicago. And incidentally, I hadn't thought of this until now, but he had an interesting story to tell of his first playing the organ in the recital part of the church service. He played a Bach fugue of some sort, and the newly-appointed minister came to him after the service. He said, "You know, I'm sure your playing was very fine today, but you know, it seemed to me it had just a little tinge of jazz or modernness that's not quite appropriate in the church." And Lunn didn't say much, but the next week he thought, "Well, I'm going to have some fun with this old-timer who didn't recognize Bach. He played the Barcarole, the most luscious love scene from the Tales of Hoffmann. The minister called him aside after the service. "You know," he said, "that's the kind of spiritual music we need."

So Lunn had this musical background, and he was with us at the Riverbank Laboratories.

We were given, first of all, a demonstration of the Bacon bilateral code, about which Colonel Fabyan, an amateur cryptologist, was deeply engrossed. I'll describe that here a little today, because I remember how he convinced all five of us, four of whom I can remember. Well, we left there with the feeling this is it! Bacon really did Shakespeare. Well, I'll mention one thing which I later recalled was not verified, and that is that we had no opportunity to determine whether the Shakespeare folios that he presented to us and used for his demonstration were original folios of Shakespeare or counterfeits. I recall that literary critics later categorized them as counterfeits.

MINK: How did he come into possession of these folios?

KNUDSEN: Well, we didn't go into that. It was only a part of one day that we were out there, altogether, and a short day at that, because we had left Chicago at nine or so in the morning to go out there, and we arrived at Geneva at eleven or so. We also had an unusual gourmet's luncheon. They grew most of their own food; they had their own registered Jerseys, and such cream, I'd never seen before. We had choice beef and vegetables grown on the estate. It was an elaborate spread, I know, that we had. And then he put us through this demonstration, which I'll describe today.

This trip to the Riverbank Laboratories was an experience I have never forgotten. As I have told you, Colonel Fabyan, our host at the laboratory, had arranged for this unique building to be constructed for Wallace C. Sabine, the pioneer of architectural acoustics. You will recall that Sabine's monumental work began when the Fogg Museum Lecture Hall at Harvard was constructed, and they couldn't understand speech in it. So the problem was placed on the doorstep of the Physics Department and a young instructor named Wallace C. Sabine, who later became Hollis Professor of Mathematics and Natural Philosophy. Sabine not only solved the problem for Fogg Art Museum but he really [did] the pioneer work that led to the science of architectural acoustics. The Riverbank Laboratory was built according to his specifications to carry on his researches in architectural acoustics.

These researches consisted of two general types. One had to do with the measurement of materials that absorb sound, like acoustical tiles, acoustical plasters, acoustical boards, fiber glass, mineral wool, carpets, draperies, hard plaster, brick, wood and all the things that are used for the interior of a room. These materials, and the dimensions of the room, determine how much reverberation you have in that room. This, of course, is old-hat today and is regularly followed in the design of most rooms. But there are many rooms in which it's violated even today.

This Riverbank laboratory had a very large reverberation room. The dimensions, I'm estimating, were about 25' x 30' x 20' high. Like our reverberation room here, it was a room inside of a room, each a concrete structure which has become approximately the standard construction for a reverberation room. Ours is 24' x 30' x 19' high. The one at Riverbank I know is fully as big as ours, maybe a little bigger. When you shout in that room, it will remain audible on the order of ten to fifteen seconds, something like that, but the reverberation time diminishes greatly with the increase of the frequency (depending especially on the air humidity), so that at 8000 cycles it can be less than 1.0 when the relative humidity is very low. You brought into the reverberation room a sample of material that normally was of not more than 100 square feet, say, of an acoustical plaster or acoustical tile--tile really had not been developed at that time--but materials like hair, felt, flax, linen and other porous materials [that] were found to be absorptive (often with coefficients of absorption as high as 0.80) and therefore useful for controlling reverberation and noise.

Among other things, Sabine had much earlier measured the absorption of the theater seats that were in Sanders Theater, which he had used a great deal in his pioneer work on the Fogg Hall Lecture Room at Harvard. He also measured the absorption of hair-felt of different thickness, of upholstered chairs, and of people, which is very important. These are typical of the things which were measured by Sabine in his improvised reverberation room. At the time of his death in 1918 he was just getting started on similar but more precise measurements in this new splendid laboratory. His nephew, Paul Sabine, had been appointed to become the director of that laboratory and was director until his retirement in 1947, but he subsequently died. But the Wallace Clement Sabine Laboratories exists today, and is one of the two major laboratories in which investigations of architectural acoustics are being conducted, that is, nonuniversity laboratories. The other one is the National Bureau of Standards.

Adjacent to the large reverberation room was a smaller reverberant room in which there was a large opening on the order of 8' x 8', into which could be built typical panels of partitions, wall partitions, ceilings, floors, constructions, doors, windows and various things of that sort. The earliest measurements made on how much sound insulation these various structures would provide were those made in this Sabine laboratory.

Naturally, I was eager to see this laboratory because I knew this was the one place, the only place--this was before there was any work at the Bureau of Standards or any other laboratory--in this country, or any other country, conducting research on architectural acoustics. So this was really an exciting experience for me to go out there, particularly to go out with two of my professors from the Department of Physics and one from the Department of Mathematics. There was another graduate student with us, but I don't remember who it was. But the three professors were smart men, and they were interested in acoustics to the extent of going out there, because it was a new laboratory where a new branch of research was being conducted.

Among other things, there was this work on cryptology. Colonel Fabyan had been engaged as a cryptologist during World War I, so he'd had considerable experience in dealing with ciphers for the transmission of secret information. We were given a quick demonstration of the acoustical laboratories and the collection of electric organs which Fabyan had at that time; they were the first electrical organs I had ever seen. This was in the spring of 1922.

MINK: When had they been built?

KNUDSEN: Oh, these were new. They were certainly developments of only the past [few years], say probably not earlier than 1919 or something like that. They were new, probably after World War I. I doubt that the necessary electronic facilities were available before then. They were using vacuum tube circuits and things of that sort that had not been available a very long time. That is, the vacuum tube, the condenser microphone and similar electronic developments began really only about 1915, so it couldn't have been that early. My guess is that it began about 1919 or 1920. I'd never seen an electronic organ and I don't think the other men had ever seen one. Professor Lunn, who was a first-class organist, was much interested, of course, in these electrical organs because, as I told you, he played pipe organs at the First Baptist Church near the University and at other places. The rest of us were interested in these things, and also in the pleasant surroundings near the laboratory, in Geneva, Illinois. It was along the Fox River, a delightful place to spend a Saturday.

But the main part of the visit was really this demonstration that Fabyan gave us.

He wanted us out there, I'm sure, to indoctrinate us on the Bacon-Shakespeare controversy. He was an ardent Baconian. I've never met one like him before. This was a subject to which I had given no attention. I don't believe the other physicists had at that time. There, of course, was no English or literary scholar with us. We were just four or five physicists who had gone out there and, as was customary at the University of Chicago, we were thoroughly insulated from all other departments except the physical sciences and mathematics. Fabyan began his demonstration essentially this way: He said, "First of all, I want to introduce you to the Bacon bilateral code." The technique of doing this was to give us each a sheet of paper and he had us write down certain letters of the alphabet in five columns. In the first column, going down from top to bottom, we wrote down a, b, a, b, a, b, twenty-four entries in all, corresponding to the twenty-four letters of the alphabet at Shakespeare's time. In the second column it was: aa, bb, aa, bb down to the end of the twenty-fourth entry. The third column was four a's, four b's, and so on to twenty-four. The fourth, eight a's, eight b's and eight a's. And the final or fifth column was sixteen a's and eight b's. Then opposite this first row--you see, they would be all a's: a a a a a--we'd put a big capital A. Under the next one we'd put a capital B, and the A and B would alternate all the way down the column of the twenty-four letters of the alphabet, down to Z. And so this was the code we were given by Colonel Fabyan. Here you have the typical bilateral code, one designated by the letter A and the other by the letter B. He then explained to us that the type fonts that had been used in the original folios of Shakespeare used two fonts of type, which were distinguished from each other only by a very slight inclination of the letters of one font that distinguished them from those of the other font, which were vertical. A corresponded to a vertical, and B to the inclined font. I don't remember the exact inclination of the B's, but not more than five degrees, something like that. He then brought out to us what was reputed to be an original folio of Shakespeare's As You Like It. Before we opened this beautifully bound book, he explained, "I want to give you a simple example of how this code works." He asked us to print out here the following sentence: "What will the weather be tomorrow?" So we all wrote this out and he says, "Now, you mark this off in groups of five beginning with w - h - a - t - w, and the Colonel instructs us how the two fonts (A and B) can be arrayed to signify the letter R. Then you go over the letters i - l - l - t - h, and are instructed to assign the A font to all five letters, which turns out to stand for the letter A. So you continue supplying the code to "What will the weather be tomorrow?" and the code signified that it would be R-A-I-N-Y. This was Fabian's example for instructing on how to use the Bacon bilateral code for conveying secret information.

And it was then that he opened up this folio, which he said was an original of Shakespeare. The four or five of us together examined the beginning lines of this printing of As You Like It, and became convinced that there was a difference in the inclination of the type-letters. There were, sure enough, two fonts of type, identical except that one was vertical and the other inclined so slightly that it could be recognized only by the aid of a straightedge that was inclined the required amount. Then he directed us in applying the code to the text. And lo and behold, when we marked off the beginning lines of the text in sequential groups and five letters each, there were A's and B's together as in accordance with the code. We could recognize the difference between these two fonts by the inclination of the type-letters, and as we applied the code to the text, it began to tell us the story of Bacon in approximately the following text: "I, Francis Bacon, the illegitimate son of Queen Elizabeth, was required, in order to conceal my identity, to write under the name of William Shakespeare . . ." And so this is the way the story began; the code purportedly revealed the life of Francis Bacon. I don't remember the further details, but we were all startled, of course, to see that here was something that looked like a first folio to us, an original folio, and it was giving in code that Bacon really was the fellow that wrote Shakespeare.

All of us, I know, left that demonstration with the feeling that well, this is very convincing evidence. I don't believe we at that time suspected that there could be collusion of some sort, for example, that what we examined may not be an original first folio, but a counterfeit. When I asked a number of literary people about this in subsequent years, they always gave the answer, well, of course it was a counterfeit. I don't know the evidence for that. That's beyond my field [laughter] of competence. But I am no longer a Baconian. But I was impressed by that Bacon incident perhaps as much as I was by what I saw in the acoustical laboratory, but the latter incident had a more lasting effect on me. I said, "Well, I'm going to get hold of the Collected Papers of Wallace Clement Sabine," which were published just that year in 1922, by the Cambridge University Press. This book I practically memorized. I read it and reread it. This book really influenced my career, I think, as much as anything else. I could to this day tell you almost verbatim much of what's in the Wallace C. Sabine book. It has since been printed in a paperback edition (incidentally, following my recommendation), I have the paperback here somewhere behind me. But this book influenced me very, very much because it included all the papers that Sabine had published on architectural acoustics, a subject for which he was indeed the architect.

It is interesting to note that architectural acoustics was not a popular subject at that time. Most of his papers were published in non-scientific journals, such as the American Architect and even the Brickbuilder. [tape off]

My three years at Chicago exposed me to one of the major universities that at that time was following the quarter system, and naturally I became very much adapted to it. My undergraduate work had been on the semester basis, and the outstanding advantage of the quarter system, so far as I was concerned, was that it utilized the facilities of the campus as much during the summer, and at as high a level, as any other time. As a matter of fact, the summer quarter at Chicago was an enriched one, and if there hadn't been a summer session quarter at Chicago I would have missed some of the most important courses that I had during my three years there.

MINK: People visited the university and taught during the summer quarter?

KNUDSEN: Distinguished visiting professors were brought there for the summer quarter.

MINK: Could you tell me about some of those who did visit?

KNUDSEN: Yes. I think I have mentioned already the name of H. G. Van der Bijl, who was the author of the first book on the vacuum tube, the thermionic vacuum tube. He was brought to Chicago one of the summers I was there. A second and third summer we had a Professor Silberstein, who was one of the leading authorities on relativity theory. A distinguished theoretical physicist from Holland came one summer. His name eludes me now. But every summer there were one or two world famous physicists brought to the campus of the University of Chicago. My introduction to the theory of relativity was by two courses I had in two separate summers, two by Silberstein, and one by A. C. Lunn, a regular member of the staff there. If it hadn't been for the summer sessions, I would have been short-changed on relativity theory, which was really a very hot subject in physics at that time. So I came away from Chicago having had three courses in relativity, and when I arrived at Los Angeles, I probably knew more about relativity theory than anyone in the Physics Department, probably more than anyone at UCLA. I don't want to be boastful, but I had taken these three courses at any rate, and I'd worked through the derivations that [Albert] Einstein originally had made. I had a thorough grasp of the special theory of relativity, and I knew what the general theory of relativity was about and therefore could understand the lectures which Einstein gave in German on his relativity theory when he first gave these lectures at Leon Mandel Hall, which was the main auditorium and lecture hall on the University of Chicago campus.

Incidentally, the first Einstein lecture at this hall was filled. It holds probably 1,000 or 1,200 people. The second day, there were maybe 100 or 125. The third day there were eight or nine.

MINK: He was that far out.

KNUDSEN: That far. They just couldn't understand; they were there curious, you see, and came for the first lecture. This brought all sorts of people, the second, the ones who more or less felt they ought to be there, and the third lecture brought only those who really cared. And I was among the small group that attended all three of his lectures, and although I couldn't understand everything he said in German, I could understand much of it.

MINK: Did you have an opportunity to talk to him then?

KNUDSEN: I didn't even try to talk to him then.

MINK: Do you have any impressions of him, though?

KNUDSEN: Well, you knew he was a celebrity at that time, and he was a superb lecturer. That is, his lectures were given in superb style, and you knew he was the master as well as the creator of this subject, as he demonstrated later when he came to Cal Tech for several weeks, and at that time gave a lecture at UCLA, at one time after we'd moved to this campus. [tape off]

The visit at the Riverbank Laboratories had a very strong influence on my deciding that I wanted to work in architectural acoustics. And at about that time, I'd had a preliminary visit with William W[allace] Campbell, distinguished astronomer and the vice-president [University of California], about the possibility of going to the Southern Branch of the University [of California]. As a result, correspondence with me was initiated by Professor John Mead Adams, who was chairman of the [Physics] Department here. After a little haggling about the salary I was to receive here, I finally received official notice from Ernest Carroll Moore, the director of the Southern Branch at that time, that he would recommend to the Regents my appointment as instructor at $2,400 a year, and that I should receive formal notice of the appointment at some later date.

The decision really to accept a position at California was finally reached as a result of the health of our only child at that time, Marilyn, who was then about eighteen months old, and was not yet walking. We felt that among the various places we could go--New York, Chicago or Portland, Oregon--that Southern California offered an environment for our daughter that would be more propitious than at these other places. That was really the deciding factor in our acceptance of the offer to come to California, if they would offer as much as $2,400 a year, because Chicago had offered me a position as instructor at only $1,800 a year. I had the opportunity to return to Bell Laboratories at $4,000 a year, so it seemed to me that some intermediate salary between $1,800 and $4,000 was about the least I could accept, and so I held out for the $2,400 and was appointed at that salary.

MINK: Well, we've noticed in looking over the records, the correspondence concerning your appointment, that a number of letters are in the University Archives in the records of the Office of the Chancellor. We notice too that there were a number of people who were considered along with you. You looked at these letters, and I think you said you only knew one man, H. B. Wallen, who was a graduate student at Chicago, not in your class.

KNUDSEN: Yes. He'd finished one year before I completed my PhD, and I know subsequently he accepted an appointment at the University of Wisconsin. I followed his career for a long time, but I've lost track of him, probably for the past fifteen or twenty years.

MINK: When did you start to come out to Los Angeles?

KNUDSEN: Well, immediately after the completion of the 1922 spring term at the University of Chicago. This would be approximately the fifteenth or twentieth of June or maybe earlier in June; in June, at any rate, early in June. I remember we had to dispose of our furniture before leaving Chicago. We lived under fairly comfortable conditions for graduate students. I was receiving $500 a quarter for doing double teaching duty. I was doing some tutoring on the side, and so my income the last year at Chicago was close to $2,400. It didn't seem very lucrative to accept another position at that amount, but we learned that living would be cheaper in Los Angeles than it was in Chicago, and in many respects it was, especially at that time.

Both my wife and I came from Utah, and we decided to have a summer vacation with our relatives. We taxied to the train at Chicago. I remember how I watched the meter on the taxi cab as we were driven all the way from the University of Chicago campus down to the Union Station, and when it reached three dollars, my heart began to sink because I didn't have many dollars left at that time. But we reached there for a little more than three dollars. We then journeyed by the Union Pacific train, which was the standard mode of travel, to Ogden, Utah. We spent two or three weeks loafing and visiting in Ogden and Ogden Canyon with my wife's friends and relatives. There's good fishing in Ogden Canyon, and I was enjoying it immensely. We'd been under a pretty heavy schedule for three years, teaching twenty hours a week (plus some tutoring) besides doing the requirements for a PhD degree in three years. And so both of us were very tired, and we enjoyed very much the visits with our families and friends who celebrated a great deal at our return. A PhD in the family was quite a status symbol at that time, so we were regaled very much by our families and friends, many of whom we still cherished, in Ogden where my wife lived and in my home town, Provo, and its Brigham Young University. There was some very good fishing near Provo in mountain streams and lakes. Harvey Fletcher took me on one fishing trip, and my father and my wife and I joined in the annual trip up to the top of Mount Timpanogus, which is a summer feature at the Brigham Young University.

This was quite an arduous physical feat for my wife and me, but my father, who was then sixty-nine, managed very much better than the rest of us. The last 1,000 feet or so of the ascent is a glacier. You had to zigzag, of course, up a steep trail in which a pick had made footmarks so that you could safely climb up there, because we all had a hold of a rope as we went up, and the strong climbers sustained the weaker ones. But coming down, you were on your own. You were advised to carry a gunnysack, on which you would sit for swift and risky descent. You'd be let down (by the strong men), maybe fifty feet, by holding on to a rope. Then you'd let go of this rope and you almost dropped through free space at the beginning of this descent down the glacier. Then, hopefully, you come slower and slower and finally you come to your end. Well, my father used his own technique to go down the glacier, which is one that I have used since then. He said, "The way to go down a glacier is to ride a stick." And so he had with him a substantial stick that he'd picked up on the way up and which he used as a cane to help him during the long climb up. He straddled this stick after he was let down on the rope about fifty feet. He rode this stick in an upright and dignified position all the way down the glacier, and the stick served also as a brake--the more you pull up on the stick, the greater its braking action. I've used it since then on snowbanks, and coming down steep mountain declines. It's a great saver of your leg muscles for coming down. Only two years ago, I used this on one of my annual trips up into the High Sierras. We'd been after golden trout, and this was an easy way of getting down without injuring your legs from the steep descent.

Well, we had this pleasant Utah vacation and left for Los Angeles in late August to assume my duties as an instructor of physics. I had expected my first check to be waiting for me, which I needed and had been promised. You see, the July check should have been in the business office on the first of August. I was greatly disappointed when they had no record of it in the office. My financial resources were just about zero. I had a few silver coins in my pocket and that's about what I had to sustain life for three of us at that time. We had rented an apartment, which we had paid in advance, so we were secure there for one month. But the disappointment prompted me to see Ernest Carroll Moore. I was told at the business office, well, the only person who can do anything for you is Dr. Moore. And so I decided, well, I'll make my acquaintance with Dr. Moore, and I presented my problem to him. It was a very pleasant encounter, and he said, "Oh, don't worry about this. It'll be down in due time," and pulled out his checkbook and wrote out a check for $100, and he said, "Here, you take this to take care of your needs for the next few days, and don't worry about when you pay me back. Anytime will be all right. I'll trust that you pay it back."

And so this was my first meeting with Ernest Carroll Moore, and it led to many others. I admired the man very much. He was interested particularly in the Greek dramas. He and a Miss [Evalyn] Thomas, who was in charge of the Greek drama in the English Department at that time, were responsible for giving some first-class performances of the Greek comedies and tragedies, especially in the old Millspaugh Auditorium on the Vermont Avenue campus. That was the main auditorium. It seated, maybe, 1,200 people, and this place was filled, really, to see these performances of Greek drama. Moore encouraged activities of that sort. He was a professional educator, but he was also steeped in the philosophy of the old Greek philosophers. Plato and Aristotle and Socrates and Archimedes, and of course the Greek tragedians and dramatists, were men that he knew very thoroughly. This became an important part of the life at the southern branch as we resumed our duties.

Well, I must tell you about my first encounter with John Mead Adams. He was very apologetic about the shortage of space. He had written to me about that. He said, "Our greatest handicap is space, but more space will become available later." I learned that they not only had no space for research, utterly no space for research, but he apologized about the office space I was given. It was strictly a cubbyhole under a stairway and had, naturally, a sloping ceiling. This "office" I had to share with two other men. You may have a record of who was in that office. Do you know?

MINK: No, I don't.

KNUDSEN: I forget. Fred Poole was one of them. And maybe Harry Kirkpatrick was the other. But I know there were three of us, and we had little desks. They were certainly not more than, oh, maybe 30" x 27"--the smallest desks I've ever seen. There was room for these three desks and three chairs in there. Really, there was not room for a student to come in and interview us. When a student wanted an interview with one of the three, if we happened to be in the office at the same time, one would have to leave to make room for the student who had come in to consult one of the instructors.

MINK: How soon after you arrived did you go up to the campus?

KNUDSEN: As a matter of fact, very soon. The apartment that we had rented was on Heliotrope Drive, just south of Melrose, and was within walking distance. So I was on the campus, I know, the first day we arrived here.

MINK: To look it over?

KNUDSEN: To look it over, but primarily to collect my check, [laughter] because there was very little between us and the next supply of groceries, and so it was very, very important that I get the $200 check. There were no deductions at that time, so there was to be the full $200 check waiting for me, and a few days later there was to be the second one for the month of August. And this came, I foget how long, maybe a week or two after; but this $100 that Dr. Moore made available to me really saved our lives during that first week or so.

Professor Adams assigned the duties that I would have, and I inquired about research. He said, "Well, we simply don't have anything now." There was no space, so I often characterized my research space at that time and research facilities as S = 0, space equals zero, and E = 0, equipment equals zero. This was the southern branch status for research at that time, but I began searching around for places where I could investigate architectural acoustics.

Obviously, auditoriums in Los Angeles suggested themselves, and I soon learned that there were a number of high school auditoriums, and I visted the first one, which was at Belmont High School. I realized that this one was excessively reverberant and something ought to be done about it. I arranged an appointment with the Los Angeles Board of Education for the city school system, and discussed acoustics and told them I would like to have access to the auditoriums of the high schools of Los Angeles. And I was granted the authorization to conduct experiments in the five high school auditoriums. I think there were only five altogether. I know there were five in which I conducted research, and this was really the beginning of my research in architectural acoustics.

MINK: Did this begin right at the very beginning?

KNUDSEN: It began during the first year. And during this time I also began some experiments related to it in a classroom that we could use at night, investigating the effect of noise on the hearing of speech in a room. And I will possibly describe some of those results later.

MINK: I was going to say that in addition to the people I know here, and in addition to Adams, there's

Dr. Laurence Dodd, who was the assistant professor in the Physics Department; Hiram W. Edwards, who was an assistant professor; and apparently Arthur H. Warner, who was an associate in physics; and Leo Delsasso and John D. Elder, who were assistants. And that was the department.

KNUDSEN: I'm sure that's right. You have that in the record and my memory would confirm that the record is correct. Arthur Warner has since retired. Hiram Edwards retired about 1955. He was appointed Director of Relations with Schools in the early thirties, and moved from Los Angeles to Berkeley to take that position for the entire university. John Mead Adams died in 1945, just before World War II was concluded in Japan. John Elder got his PhD in mathematics somewhere, and I think ended up at the University of Michigan. Warner was in the radar program all during World War II, advancing from major to colonel, and stayed on with the Army as liaison field officer in connection with the further development of sonar. He served as an intermediate man between the scientific and engineering laboratories where radar devices were developed, and the armed forces where they were making field applications. Later he served as an assistant to President Lee A. DuBridge at Cal Tech. And Delsasso, of course, has been associated with the Department of Physics and me from the very beginning. He was no more than a sophomore in 1922, because that was the highest class there was. But as a sophomore he was also assistant, as this record indicates.

The record that you reviewed for me, Mr. Mink, also revealed an interesting thing to me: that my appointment, I suppose as were all other appointments in physics, was very much controlled, it would seem, or influenced at any rate, by the chairman of the Department of Physics at Berkeley. In all of these items to which you called to my attention, now, you've allowed me to peek into the [laughter] confidential files of this correspondence and the name of E. Percival Lewis--it's E.P.L., but I know that stands for E. P. Lewis--who then was chairman of the Department of Physics and whom I later came to know. But certainly, the Department of Physics or even the Director of the University of California, Southern Branch, had to clear through the chairman of the department at Berkeley. This was of interest to me, and I think it is an indication of the relationship between Berkeley and Los Angeles at that time. We were certainly not sister institutions. It was rather a mother-daughter relationship, and we were a pretty small daughter at that time. And the administration of this place was very carefully supervised by the Berkeley authorities.

MINK: I think these records indicate even courses to be added had to be cleared because there was very shortly an addition of Physics 5A, Mechanics.

KNUDSEN: Yes. The numbering of courses and also their contents were very closely patterned after the Berkeley system. There were exceptions in the laboratory, and I think I should pay tribute to John Mead Adams, who was responsible for some of those innovations. Adams developed what's known as the impulse balance. This consisted of a brass vane that had a transverse bar at the top that allowed the vane to be suspended as one arm of a balance, with a controllable counterweight as the other arm. The mass of the brass vane could be held in a vertical position by means of the counterweight. Near the bottom of the vane there was an attached funnel through which you could direct a stream of water. And this water would go through the funnel, hit the brass vane, then drop down into a glass for measuring the volume of water that flows in a certain measured time. By measuring the size of the nozzle you calculate the velocity with which the water hit the balanced vane and also the mass of water hitting the vane per second. The force on the vane was thus balanced by the force of the counterweight. And the experiment thus is a convincing demonstration that force is equal to the rate of change of momentum, momentum being defined as equal to the product of the mass times the velocity. So this was an experiment for investigating force in which the force was provided by this stream of water. The experiment repeated with three nozzles of three different diameters. These were experiments that enabled freshman physics students to verify definitely the law that force is equal to the rate of change of momentum. And it's a very neat and instructive experiment in beginning physics. The experiment has continued to this date, so far as I know. It was continued all the time I was teaching elementary physics here, and I think I see the impulse balance around here to this day. And many of the other experiments that were performed in the laboratory in those early years under the direction of John Mead Adams were done with instruments that had pretty much been devised by him and were made by Leo Delsasso. We should ask Delsasso sometime to tell us how this equipment was made, because he knows better than anyone else, and probably is the only living person now who was there at the time Adams really started physics at UCLA, in 1919.

MINK: It was interesting also among the other early researches that were going on in the department to note a request by Adams for John Elder to use the department at night. And apparently Dr. Moore was not too happy about this, that the university was not set he also was very democratic. That is, the young instructors, and even Delsasso who was a sophomore student but an assistant, and also Elder, and all others, had a voice in reaching decisions that affected the welfare of the department. And it was a great joy to me.

There were many disappointments in coming from Chicago, which was one of the great places of physics in the world at that time, to a place where there was almost nothing. My own research room at Chicago had been the one Millikan had used for his work on the electron. It was probably 16' x 20' or even 16' x 25'. And this was my research room, which was mine alone. Besides that, I shared another room of equal size with three other graduate teaching assistants. So we really had luxury there as compared to the skimpy things we had here. But one of the compensating things for me was the superb teaching and administrative qualifications that John Mead Adams had. He held the group together in a splendid manner. You felt that even though you were the youngest member of the staff, as I was at that time, you were really a part of the team planning for the future. And things developed fast.

I became acquainted with Delsasso, and Delsasso and I have been very close colleagues, friends, and collaborators during the years. Now, in 1967, after up to handle night work. But the work that Elder was carrying on was careful photographic work in which vibrations in the daytime made it impossible to do the research properly. Do you remember what kind of research it was that Mr. Elder was carrying on?

KNUDSEN: I'm sorry, I do not. He was as much interested in mathematics as physics, I remember, and I frankly don't remember what he was doing in research. Later, as I have told you, he got a PhD degree in mathematics, and his subsequent work was in mathematics rather than physics. But he was an intelligent student, and developed into a first-rate scholar.

MINK: Also, I know that among the early happenings of this first year that you were here that there was suddenly a new tariff on scientific instruments which required Adams to ask for extra money in the budget in order to make the budget equivalent to what was needed for the purchase of the instruments.

KNUDSEN: Yes. That is interesting. I might add one other comment about Adams as chairman of the department at that time. Departmental meetings were held quite frequently, I would say on the average of once every two weeks, and we met together at luncheon at a nearby restaurant in Hollywood. Adams, as the file here will show, was a very orderly person, and the arrangements that he made for these meetings were always perfection;

five years of retirement for him and nearly seven years for me, we are carrying on together our researches in acoustics that I began here in 1922. Soon he was helping me and has been helping me all the time since then. We are now equals, have adjacent offices, and work together in the acoustics laboratory, which was especially designed for research in architectural acoustics.

MINK: One man that we haven't mentioned who was there in your first year and continued there was Dr. Laurence Dodd.

KNUDSEN: Yes. Laurence Dodd's specialty was--and he was able to conduct research in that field because it didn't require any equipment--geometrical acoustics. And he was very ardently devoted to that subject. He published several articles, I know, on the subject of, well, complicated arrays of mirrors and prisms and things of that sort, in which you assume that light travels like a ray and is reflected regularly. He was very competent in that particular subject but could become interested in quite a variety of other subjects. I remember three typical research articles that he wrote during the years he was here. One dealt with the washboard effect in roads. He had a reasonable explanation on how these ruts develop along the course by the action of the wheels and springs of an automobile. I forget the details now, but at least it was a rational explanation of why you get a succession of ruts in a road, especially dirt or gravel roads. One of the other early things he did was to make counts of the dead birds and rodents he'd find along highways. He kept records of these counts and related them to the customs and hazards of the automobile. A third research article revealed the dependence of the sounds of crickets on temperature.

MINK: I notice that he also taught a couple of courses, one on the physical basis of music, for example, which included lectures and experiments and demonstrations.

KNUDSEN: Yes. This is rather interesting. There were two kinds of applied courses given at UCLA in 1922, or maybe for the first few years I was here. One was in household physics. Joseph Ellis, who came a year after I did, taught the course in household physics, and Dodd taught the course in musical acoustics. (Dodd had a good singing voice. He and I once sang a Romeo and Juliet skit at a faculty party. It was to the tune of "Long, Long Ago." Dodd was Romeo; I, Juliet.)

MINK: This course included lectures and experimental demonstration.

KNUDSEN: Yes. Well, today we'd call it musical acoustics or something of that sort. But these applied physic courses were short-lived. When the first dean of the Letters and Science, Charles Henry Rieber, came here from Berkeley, he said, "If students are going to learn physics, they're going to learn physics in the Physics Department. We're not going to have physics for home economics students and physics for music students. If they want to learn physics they can come over and take the solid courses in physics." And this had quite a salutary effect on the kind of courses that were offered, not only in physics but in other departments here. Dean Rieber's influence had a salutary effect on the upgrading of scholarly standards at UCLA.

MINK: Well, in setting up these household courses I should imagine for other than physics majors this was a holdover from the normal school aspect of the university.

KNUDSEN: Indeed it was. Home Economics and Mechanic Arts were the two principal departments--that is, most of the students at the Vermont Avenue campus, when I arrived there, were either women preparing to teach or they were returned veterans from World War I majoring in Mechanic Arts. And if you exclude those two groups there wouldn't be many more students at UCSB in 1922.

MINK: Then I take it that Adams wasn't too happy about offering these courses either.

KNUDSEN: I don't believe so. They were courses I guess he inherited when he came here. There'd been this tendency. The School of Education, the Department of

Mechanic Arts and the Department of Home Economics had a powerful influence in determing early policy here, within the regulations that Berkeley insisted upon.

MINK: Well now, that's an interesting point, because I note here that it was in this first year that you were here that Physics 5 was added to the curriculum, and this was to be equivalent to Electrical Engineering 1 as it was taught at Berkeley. Moore had to get the consent of President Barrows, and the dean of physics had to give his consent. Baldwin Woods asked for permission.

KNUDSEN: Woods was dean of engineering.

MINK: Right, and he asked Dean Clarence Cory and he gave his permission. And this course was then added in the second year. I think you did teach it at one point.

KNUDSEN: Is that so? I've forgotten. [laughter] But it could be, because I had had experience as an electrical engineer, of course, at the Bell Laboratories, and I knew especially vacuum tube circuits.

MINK: This is one very small minor question I was going to ask you. I know that in the catalogue for years you were the only instructor who appeared with just "V. O. Knudsen." Everybody else appears with their full name. [laughter] Can you explain why?

KNUDSEN: No. I can't. Probably my first letter to

John Mead Adams may have been signed "V. O. Knudsen." Was it?

MINK: Yes, it was. Did you prefer this?

KNUDSEN: No. As a matter of fact, I thought I normally signed "Vern O. Knudsen." Certainly, at the Bell Telephone Laboratories and as a Mormon missionary I always used "Vern O. Knudsen." I sometimes abbreviate, but this is unusual.

MINK: Well, the first year you taught General Physics 1A-1B, 2A-2B, and 3A-3B.

KNUDSEN: Yes, but I only assisted in the teaching of them. Physics 1A and 1B were taught by John Mead Adams; I assisted in the laboratory, and attended his lectures, from which I benefited. Hiram Edwards and Dodd probably taught one of the others.

MINK: You taught 2A-2B with Edwards.

KNUDSEN: Yes, if the record so indicates.

MINK: And then you taught Physical Measurements, 3A-3B, that year, too.

KNUDSEN: First year, Physical Measurements, I don't remember that. [laughter]

MINK: I also notice that there weren't too many courses above the basic courses. There was 4A-4B, Electricity and Light, that Adams and Dodd taught together. But what about mechanics of sound, heat, light, and electricity? Well, that was actually for home economics students.

wasn't it?

KNUDSEN: Yes. That was for home economics.

SECTION IX

DELSASSO: Well, I think I'd like to say first that I was one of the students who came here just after the First World War, together with many others who had been taken out of circulation for the duration of the war and were behind in their college entrance. There were about, I think, two or three hundred GI's who had been disabled who were here at the Normal School, even before it was made a junior college. The junior college, of course, started in September, 1919, and in that summer, I guess, the declaration that it would be a part of the university was made and the faculty was collected.

The three people who were picked for physical science were John Mead Adams to head the physics group, George E. F. Sherwood to head the mathematical group, and [tape off] William Crowell for chemistry. These three people practically killed themselves the first semester trying to do all the services--teach, make unknown chemicals, and build the equipment. But I came to the university because it presented a way of starting my education without paying heavy tuition--a condition that exists at the present time. And the group in physics, as I recall, was not larger than seventy-five at the very beginning. We had no equipment, as you probably have noted by the record, and the first experiments were done with a meter-yardstick combination and three bricks, which served as the laboratory apparatus A substitute for laboratory work was provided by asking the class to do special problems in a group under the supervision of the then existing and only professor, John Mead Adams. This was, as I see it in retrospect, not too bad. We really learned a great deal by having someone as thoughtful and as able as he was to supervise us in detail while we were making our calculations.

MINK: Yesterday you spoke about the meter stick which was the first piece of equipment and how you came to have it.

DELSASSO: Well, John Mead Adams bought it himself and he stole the bricks from a building across the street from the Vermont Avenue campus.

MINK: You were at that time a sophomore?

DELSASSO: No.

MINK: Freshman?

DELSASSO: Freshman. Just beginning. And we began then immediately to build some equipment. Adams had sketches and designs from his previous experience at Occidental College and in Canada, and had no money, little to pay for shop help. I was the first employee in that capacity, and we built the things you see in the photographs there: tangent galvanometers, wheatstone bridges, impulse balances, and the usual equipment for studying moments of inertia and things of that kind, magnitometers, and the rest of the equipment needed for an elementary laboratory.

MINK: It seems surprising to me that they would authorize the beginning of the study of physics without any laboratory at all.

DELSASSO: Well, this was a decision made in the summer, and it was just physically impossible to either get the money or to get the orders in and delivered by the time they needed to get started. Now I say again that I don't think this was a great loss over the years. We didn't suffer by this, but it did push us to build equipment for the next semester.

MINK: Where was the shop located?

DELSASSO: The shop was located in the machine shop. We just used the central machine shop that was available primarily for these GI's who were being retrained for other vocations because of their handicaps. And so we had the use of the general machine shop, if we'd keep out of the way of the other people using it. A year and a half from the time of opening, I made a trip on one of the battleships and found a lathe that seemed to me to be ideal for our purpose. It was a rivet lathe which cost $1,200 at the time, and we put in an order and somehow or other it slipped by all of the screening authorities and we got it. And we never surrendered it, but we were forced to put it under the central machine shop for supervision so that it wouldn't be mishandled. But this lathe and a drill press that I built in high school were the two pieces of equipment that we had to work with for, I guess, five or six years.

MINK: But Professor Adams, meanwhile, had to press for a shop for physics and later for what he called a "mechanician."

DELSASSO: That's right.

MINK: Now what does this mean? He makes quite a distinction here between the mechanics in the machine shop and what he calls a mechanician. Could you explain?

DELSASSO: There is a real distinction there. The mechanics that were available--I guess you could describe them as people who could put wheelbarrows together pretty well, but when it came to turning something on the lathe in a very precise way, or doing glass blowing, or some of these related things that are needed in any physics laboratory, they just couldn't handle it. The mechanician really did not do very much glass blowing, but he had to work with glass and with fine instruments. And he is a different scale of person from the ordinary mechanic.

MINK: Apparently, he was not able to get such a person immediately. He was pressing for this.

DELSASSO: No. I've forgotten just what date, but I think you can see that it was several years before we were able to get Mr. Jung, who was a well-trained mechanician who had been at the University of Chicago and, I guess, at the Carnegie Foundation where Dr. Barnett had been. He had been trained in building surveying instruments, things of that sort. That was his background, and he was an extremely able person.

MINK: Did he come as a result of Dr. Barnett's coming?

DELSASSO: He came as a result of Adams' early pushing for such a person, and Barnett happened to be chairman at the time that he came, or at least he was here and made the recommendation because he'd had experience with the man.

KNUDSEN: Jung came to the department probably about 1926 or 1927. The early negotiations had been made by Adams. Very favorable reports had come about the work that Jung had done as instrument-maker, both at the University of Chicago and at the Carnegie Foundation. I think it's very interesting to comment on the sophistication of Jung. For example, many people would walk through the shop of the physics department at that time--faculty members--and Jung was especially well informed about [Jakob] Wassermann, a prominent German contemporary writer at that time. Professor Diamond of the Germanic languages department had made a special study of him, and I overheard several conversations in the shop between Jung and Diamond on Wassermann. Jung could hold his own very well in discussing noninstrument making topics of that sort and was in many respects a real scholar to have in the Department of Physics. He understood, of course, enough physics to be a very useful instrument maker.

DELSASSO: Yes. Later on, he held informal seminars at noon hour almost continuously out on the lawn with students who would gather around to talk about all sorts of things. He would argue on any side of almost any subject that you wanted to bring up. He was really a very accomplished person.

MINK: Well, Dr. Delsasso, when was the first time that you met Vern Knudsen? Do you remember?

DELSASSO: Well, when he came here as an associate professor.

KNUDSEN: No, instructor.

DELSASSO: Instructor, excuse me. [laughter] In 1922, I guess was the date. And my first contact with him was, of course, when he taught the one course that we had in electricity. My impression of him was immediate and firm that here was a fellow to follow, and I've done that ever since.

MINK: Do you remember that, Dr. Knudsen?

KNUDSEN: Yes, I do. The course, I think, bore the label of 4A or 4B, and I probably taught only part of it. It was electricity or electrodynamics, and the lectures I gave were seasoned very much, of course, by the things I had learned at the Bell Telephone Laboratories where I had worked on vacuum tube circuits and especially on early oscillators and amplifiers. And I suspect part of that novelty--it was new material, really, at that time--attracted Delsasso into his interest. A year or two later, I think, we added the first course in acoustics; we simply called it Acoustics 114, and that later enlarged to 114A-114B, and, I think, started many of our students in acoustical careers. There were not many courses in acoustics offered anywhere in the United States. Delsasso, I'm sure, was one of the first to take that course in acoustics. He might wish to comment on these first two courses, because I think they did have quite an effect on his later interest, which has continued now for forty-five years.

DELSASSO: That's right, and I'll say something about my training in high school. I went to Polytechnic High School here in Los Angeles, and at that time they were providing college preparatory work. If you extended yourself just a little bit you could get all the shop and electrical measurements courses that were being given, and go out and at least get a job at the end. And I had worked from 1915 to 1919 with the Southern

California Edison Company in the test department and in the research department. So I was oriented in electrical work, and to extend this to the notion of vacuum tube oscillators and amplifiers was a great fascination to me. These had not started out here at that time.

MINK: I would like to ask about the caliber of instruction in the city school system at that time and at Polytech.

DELSASSO: I think they were doing a superb job and I'm sorry to see that it isn't continued, because, for one thing, they filled your day with things that were useful. You didn't do just a little bit of shop, a little bit of tin shop, a little bit of mechanics and mechanical work, but you took serious courses. You started by learning how to chip a piece of cast iron and file it flat. Now this, of course, in these days is certainly not necessary. But it gave you good training for those times. In addition, they provided these courses which had, as far as equipment is concerned, everything that a university would have in the way of electrical measurements. We had the galvanometer, potentiometers--all the rest of the equipment, far more than we ever had here for years. And there were competent people to do the teaching. And I think it's a pity that this hasn't continued, because it did three things: it kept you busy for longer hours than the kids are kept now, and it kept you busy at things that were useful, and it fired your imagination at the time.

MINK: Would you say that the kind of instruction that Dr. Delsasso received here, Dr. Knudsen, was given elsewhere in the country? Did you run up against this in your high school background?

KNUDSEN: Definitely not--that is, my high school was at the Brigham Young University, and I don't believe I had seen a drill press or a lathe or anything of that sort--certainly during my high school days. I had seen blacksmith's at work and I had seen the cruder operations of mechanics, but certainly nothing resembling the kind of work that Dr. Delsasso is referring to here, at the high school level in Los Angeles.

MINK: Dr. Delsasso, how do you explain this?

DELSASSO: Well, this was an experiment initiated by the superintendent of schools at the time, and he felt that this was the--well, it was the beginning of a trade school type of thing, but integrated with the high school in such a way that you could do both. You could go there just for the trade school aspect of it or you could take both college preparatory work and the trade school part of it, too. It was the beginning of the Frank Wiggins Trade School and all the rest of these schools.

MINK: In Los Angeles. I wanted to ask you now, Dr. Knudsen, your course in acoustics--were you pushing for this from the very beginning, or were you waiting to gather material through your studies here before you could offer it?

KNUDSEN: Of course, it was unthinkable that it could be offered then as a lower-division course because the lower-division work was taken up completely by these fundamental courses in physics--1A, 1B, 1C, 1D, which is as it should be. But I was thinking about it and soon upper-division work was authorized. It was authorized first in 1923.

DELSASSO: There was a gap of two years.

KNUDSEN: It was in 1924.

DELSASSO: It was in 1924, I think, a gap of two years, and I recall going down with a bunch of students outside of a regents meeting very much as they do now and shouting, "Third and fourth year! Third and fourth year!" [laughter] And this came about very shortly.

KNUDSEN: I wish to comment further about the kind of training Delsasso had in high school and the beginning work he had at the Southern Branch. It left its mark on him throughout his entire career. When we had, I think, our first military students come here for post-graduate work, a colonel (well, he was a colonel even at that time when he came here) made a remark to me at one time about Delsasso as he was patting Delsasso on the back. He said, "I declare, Dr. Knudsen, that

Delsasso can do more with a pair of pliers and a screwdriver than anybody I ever knew." When you had to deal with the design of apparatus, or the set-up for a critical experiment, he knew how to do it.

Delsasso's role in the department throughout the entire time we have been together here has been one in helping others in critical experiments. That is, he became more and more sophisticated in these things. His publication record is by no means an adequate index of the contribution he has made to the research contributions of this department. Delsasso has been the most unselfish person I've known in the university during my long time here, and one of the most beneficial for the careers of others, and particularly for their careers in research. That is, he has known what his colleague was doing in his research program. He was willing to help him design the necessary equipment, and to make suggestions as to the best way to do things, and then, if necessary, he would build it with his own hands. This happened frequently in my early work in acoustics. But I definitely was interested in acoustics when I came to Los Angeles, and I think Delsasso became interested in acoustics soon after, so there has been an unusually long and close partnership between Delsasso and me. Certainly my publication record wouldn't be what it is today if it had not been for the help I received from Delsasso. We didn't publish many papers together until recently, but we've been working together continuously since our retirement, and this is something I expect will continue as long as we can totter into our offices and our interesting laboratory.

MINK: Dr. Delsasso, since you did take the first offering of the acoustics course when it was offered by Dr. Knudsen, would you give me some impressions of it as you remember it?

DELSASSO: Yes. It, of course, opened my eyes to really fine teaching. The clarity with which the material was presented and particularly the type of material happened to be of considerable interest to me. To be able to see more into electrical circuits and understand more of what was going on in complex circuits, particularly in tube circuits as they came along, was a great challenge to me. I don't know of anything that gave me a bigger lift than this opening out of my vista of electrical circuits and their operation.

MINK: Dr. Knudsen, was UCLA the first institution to offer a course of this kind, or were these courses being offered, too, at Cal Tech and Occidental?

KNUDSEN: Nothing was offered at Cal Tech or at Berkeley. There was a course at the graduate level called Acoustics at the University of Chicago during the time I was there. It was taught by a mathematical physicist, although he was in the Department of Mathematics--Dr. A. C. Lunn. And it was very definitely mathematically oriented, but it was a useful course. Although one of Michelson's courses didn't include the word acoustics--it was Mechanics of Wave Motion and Sound--sound was very incidental to it, but it treated the mechanics of wave motion thoroughly. And as Delsasso could testify, much of the material that was used in my first course in acoustics was the mechanics of wave motion, and it was pretty much the same material that Michelson had taught us when I was a graduate student at the University of Chicago. And the treatment was, at that time, I think, rather sophisticated because it had so much of the label of Michelson on it, and it was better than anything I knew in available textbooks. I don't think I plagiarized Michelson. I told the students very definitely I was very much indebted to Michelson for this kind of instruction which I had received from him. I used much of the technique that I learned first from Harvey Fletcher and then from Michelson and Millikan in the kind of teaching I have done at UCLA. I may have told you that Harvey Fletcher preceded me, by about ten years, as a graduate student of Michelson and Millikan.

MINK: Well now, as we come along here we begin to come into departmental affairs, or do we want to discuss something of the departmental affairs as they began with, say, 1925 and going on up into the 1930s?

KNUDSEN: I'd like to comment on that a bit. It was, of course, quite a shocker to me to come from Chicago where there were the very finest facilities for research and the advancing front of modern physics at that time, and then to come here and find almost nothing in the way of equipment. But I wasn't disappointed. I realized that there was a certain advantage in starting from scratch. I was, of course, very much concerned about the dependence we had upon Berkeley, and I think this was an interesting element in the early development of UCLA. We were in the strictest sense of the word a branch of Berkeley and we were made to understand that by many events that happened. My own appointment, for example, I learned only after I saw the files here a few days ago, had been pretty carefully screened by E. Percival Lewis, who then was chairman of the Department of Physics at Berkeley. And subsequent appointments, I learned, also had been pretty carefully screened by Professor Lewis or others at Berkeley.

There are one or two items here in the early records that I think bear on that close relationship. Although we received very helpful advice and nourishment from Berkeley at that time, as later I did when I became Dean of Graduate Study, we were still very much dependent upon Berkeley, and that dependency, I think, has had a great deal to do with the distinction that later developed here at UCLA. I'm not saying that our dependency was all good or all bad. It was certainly a novel thing in the life of a person who came from the University of Chicago where the Physics Department "was all," almost completely independent of the university administration, except for budgets, housekeeping, admissions, registration, the collection of fees, and foreign language examinations. We just didn't ask any department or any higher administration what we could or could not do. All the arrangements for examinations, including the final one for the PhD, and the doctoral dissertation were just matters that were handled by the Department of Physics; I didn't see any other part of the administration at the University of Chicago. [It was disconcerting] to come here and find that we were so thoroughly scrutinized by Ernest Carroll Moore, who in turn was beholden to the authorities at Berkeley--to President Barrows, to Dean of Engineering Baldwin M. Woods, and even to the chairman of the Department of Physics. These were all at Berkeley. These were all individuals that Dr. Moore had to very definitely bow to. This was quite an authoritarian rule, quite foreign to my experience at Chicago. [tape off]

DELSASSO: When President Barrows was first appointed, he came down to speak to the entire student body and faculty and declared in no uncertain terms that we would never be more than a junior college. And this was straight from the president of the university before this two-year interval, and then finally, of course, this was broken down.

MINK: Was that before you came, Dr. Knudsen?

DELSASSO: Yes. I think it was before you came.

KNUDSEN: Yes.

DELSASSO: It was just after we had finished the second sophomore year and the pressure was on to get the third and fourth year. But the president wanted to make it very plain that he was not for it and would fight it.

MINK: No demonstrations, please. Dr. Knudsen has a telegram.

KNUDSEN: Yes. This telegram bears the date of January 22, 1923. It's from Ernest Carroll Moore to Professor Baldwin M. Woods at the University of California at Berkeley. The telegram reads as follows: "It seems from President Barrows's telegram that we may give Physics 5 if the Department of Mechanics, at Berkeley, concurs as to its equivalence with Electrical Engineering 1. I take it that leaves us just where we were until we secure a statement from the Department of Mechanics. As that is your department, may I not ask you to present the matter." End of the telegram, signed by Ernest Carroll Moore, and it's in his handwriting. It's an interesting document because it does reveal that even the chief campus administrative officer here had to obtain the concurrence of the professor in charge of mechanics on that campus, in which presumably Electrical Engineering 1 was taught, in order that I could be authorized to teach a course in electrodynamics here at the Southern Branch campus.

There's one other matter here that bears upon the smallness of the campus at that time and its dependence upon rules that had been promulgated from Berkeley. It's a letter from Ernest Carroll Moore, the director, to John Mead Adams. This is dated September 8, 1922, about the day I began my duties here.

> Dear Professor Adams:
>
> The university has been trying to work out a means of supplying stenographic and typewriting services to the general department which will prevent the waste which attends setting up separate offices in each of the departments. To that end we have employed two faculty stenographers and plan to increase that service as rapidly as possible. It is contrary to the established policy of the university to furnish typewriters to the several departments. Through error due to the confusion attending the reorganization of your

laboratory requirements, a typewriter was allowed you this summer. As long as you have it the university fails to carry out its own policy, and other departments feel that they are being discriminated against in not being given what you have. Inasmuch as this was authorized in error, please be advised that the university asks the Department of Physics to return it to the controller's office at the end of the year.

Well, this is typical of the centralized control of some of these things. This was the Berkeley policy that had been handed down here and had lasted a long, long time. As Dr. Delsasso remarked earlier here, whenever we had a letter to dictate or to have typed we had to walk over to the central stenographic service and have it performed there. And the same was true for a long time at the central shop for the campus, and Dr. Delsasso may wish to comment about that a bit.

MINK: Would you, Dr. Delsasso?

DELSASSO: Well, this was carried on in name. Fortunately, we were able to keep our equipment in the physics building, and to carry on here very much the same as if it had been our own, except that it legally belonged to the central machine shop, and the central machine shop had to be consulted whenever we purchased anything. But we had established by that time the need for this work. We were really not too seriously affected by this ruling because we had forced it previously.

MINK: The records that Dr. Knudsen has been reading from are from the Provost Office records and from the folders on the Physics Department. I think we should put that in for the record. [tape off]

KNUDSEN: During the first year that I taught the course in acoustics, which was the year of 1924-1925, Dr. Moore asked the Department of Physics, through my help, to determine what could be done to improve the acoustics of Millspaugh Hall, which was the main auditorium on the Vermont Avenue campus. And I note from the files which you have identified here as coming from the Department of Physics. . .

MINK: The Department of Physics within the Provost's file.

KNUDSEN: The students in this first class in acoustics were asked to participate in the experiment of determining just how well you could hear speech in Millspaugh Hall under two circumstances: one, as it was; and two, as it would be after our proposed treatment. We conducted what are called speech articulation tests. This is a technique that had been developed by telephone engineers at the Bell Telephone Laboratories, in which you call out meaningless but typical speech sounds for typical listeners. We used meaningless monosyllablic speech sounds, in groups of three. The listeners would write down what they heard, or what they thought they heard, and then you compared the lists of what they wrote down with the list of what was actually called. For example, if you had called out 1,000 meaningless speech sounds--there'd usually be that number in order to be statistically meaningful--you could determine how many were heard correctly. If they heard, say 750 of the 1,000 correctly, the articulation is rated as seventy-five percent. Well, we trained these students in the first class in this technique of speech articulation testing. Among others, I remember that among these students there was Pinker, who later became the chief scientist in the crime department of Los Angeles, who retired only very recently. There were others who attained distinction, probably W. A. Munson, and you were in the class, Delsasso, and several others. There were probably ten or so, I would say, who participated in those tests.

DELSASSO: Yes. Probably Gilbert Wright was one of them. The son of Harold Bell Wright.

KNUDSEN: It's likely that John Elder was one, so we remember at least half of the students who participated, and the ones we've mentioned were students who really have risen quite a lot, not because [laughter] they participated in this experiment, but they were on hand.

This was, I believe, the first time an auditorium had been analyzed this way--that is, to use speech articulation tests to determine quantitatively how well you can hear in various parts of the auditorium.

MINK: Dr. Knudsen, was that something that you decided to use yourself?

KNUDSEN: Yes, it was.

MINK: And this came from the Bell Laboratories?

KNUDSEN: The Bell Laboratory technique had certainly become familiar to me at that time. Harvey Fletcher, my life-long mentor, had used it a great deal in connection with his work on speech and hearing at the Bell Telephone Laboratories.

MINK: This was in 1925, but for the record I must say that I noted in the minutes of the administrative staff as early as April 4, 1923, that there had been complaints about the auditorium and that they were asking you and your committee to look into this.

KNUDSEN: I don't remember the circumstances. That well might be. I presume as early as the spring of 1923, Ernest Carroll Moore knew that this young recruit in the Department of Physics was also very much interested in acoustics and was going to make it his career. I had taken steps to get permission to use some of the high school auditoriums of Los Angeles, and Dr. Moore had served as superintendent of the Los Angeles Schools.

That was certainly in the spring of 1923, so that it's reasonable to assume that Dr. Moore had requested this study, knowing that I was conducting research on the hearing of speech in several high school auditoriums.

I had recommended the addition of some 6,000 square feet of an absorptive material--celotex wallboard--to the ceiling in order to reduce the reveration. We had made some measurements in Millspaugh Auditorium with equipment that Delsasso certainly had helped us put together for these other experiments in the high school auditoriums. We had used this equipment also in Millspaugh in measuring the reverberation. These tests revealed that the reverberation was so long that it was the chief defect in hearing speech in that auditorium. Much work had been done before these tests that were conducted in the spring of 1925, which I reported to Dr. Moore on March 7, 1925.

I think it might be worthwhile to read the report. It's a very brief report entitled, "Summary of Articulation Tests in Millspaugh Auditorium." It reads, "To determine the improvement in hearing conditions following the addition of 1,620 units of absorption (a unit is one square foot of a totally absorptive material) which consisted of 6,000 square feet of celotex added to the ceiling." And here are the results of the percentage of word articulation--the nature of which I have briefly described. "Before treatment, in row Z," which was the last row, "the articulation was 52 percent," which means you can't understand speech under those circumstances. In row JJ, which was in the middle, it was 60 percent. In the balcony, the rear row, it was 57 percent. It was a little better in the balcony than it was underneath the balcony, and that's something that's been borne out by many of our later tests. The average, then, before this treatment, was 56.3 percent, and you can understand why Ernest Carroll Moore and others would complain about the acoustics of this particular hall, because an articulation of 56.3 percent just means you're straining all the time and you're missing so much that it's a disappointment to be present in such a hall.

The simple treatment which we recommended here was based upon the findings of Wallace C. Sabine, whose works we shall review when we discuss our work in architectural acoustics, because he was the pioneer in the development of the science of architectural acoustics at Harvard University. We knew this treatment would improve the intelligibility of speech, but no one had ever conducted experiments like ours to determine quantitatively how much improvement would result. And these experiments, I think, may have been the first quantitative results that were determined by this technique for ascertaining just how well you can hear speech in an auditorium.

Now, after our recommended treatment, on row Z the articulation was 78.5 percent in contrast to the 52 percent before the treatment. In the middle row, JJ, it was 88 percent in contrast to 60 percent before the treatment. And in the balcony, rear row, it was 80 percent as compared with the former 57 percent. Or the average articulation was 82.2 percent after the correction as compared to 56.3 percent before the correction. And as indicated here, it required for barely acceptable hearing, 65 percent. So that initially the 56 percent was definitely below barely acceptable, and barely acceptable is far from good. We say, today, 75 percent is the least we should tolerate, and that 85 percent is a goal to shoot at, and which can be attained in most rooms if you use proper care in the acoustical designing.

I added here, which I think also is significant and which will be confirmed by later results we shall discuss, "Nearly all the errors after correction were final consonants, such as confusion of m, n, and ng, b and v, and f and th, f as in fin or th as in thin." They can easily be confused because there isn't enough phonetic or acoustical difference between these two sounds to recognize which is which. It's only by the context of what is being said that you know you're talking about a thin man instead of a fin man.

MINK: I noticed there that on the back of this letter is an acknowledgment from Ernest Carroll Moore of your report. On the back of the carbon is a carbon of the acknowledgment. And very promptly on March 10, 1925, he acknowledged this.

KNUDSEN: Oh, yes. "I have your letter of March 7 on the summary of articulation tests in Millspaugh Auditorium, which you were good enough to send to me. The result of your test seems to indicate that the prescription which you wrote for the auditorium has effected a cure. I congratulate you. I appreciate your service with the greatest personal keenness. Sincerely yours, Ernest Carroll Moore."

MINK: You can see that he was concerned then.

KNUDSEN: Yes. And this investigation, I'm sure, had been going on very soon after 1923.

DELSASSO: I'm sure. Very shortly after he came here he began to get worried about Millspaugh.

KNUDSEN: Yes, because this was one of some six or seven other auditoriums in which we conducted speech articulation tests, and a little later we'll refer to that because that work was the first important research work I did after coming to UCLA, the hearing of speech in auditoriums and the various conditions which contribute to the hearing of speech.

MINK: Before we go on, since Millspaugh Auditorium is no longer, and since you say the reverberation was so long, perhaps you could describe the auditorium and what caused this problem.

KNUDSEN: At that time everybody thought reverberation was all. This was the finding of Wallace C. Sabine at Harvard. We found that Millspaugh Auditorium empty, say, had something like 5 or 6 seconds of reverberation. When sound reverberates that long a vowel, say, which has 100 to 1,000 times as much acoustical energy as the consonant that follows--and the consonant follows maybe only 0.2 of a second or less after the vowel--the reverberating vowel is so very, very loud that it completely masks the consonant; and so this is one of the principal things. But as we shall show later, there are factors of shape and of noise--noise is very, very important, and we investigated that carefully here--and then there is the extremely important factor of the loudness of the speech. In large auditoriums, therefore, you have to amplify the speech. As we'll explain later, the acoustical power generated by typical speakers varies from something like only 4 microwatts, up to 152 microwatts. These factors in general are the principal things that determine how well you can hear speech, and which we will consider in detail at a later discussion.

MINK: Well, I think what I meant was why the shape of Millspaugh Auditorium made it have such a long reverberation period?

KNUDSEN: It wasn't primarily a matter of shape. It was predominantly a matter of materials. Hard plaster had been used for the walls and the ceiling, and the seats were wood seats, and it was almost entirely a matter of having too much reverberation because the interior materials didn't absorb enough sound. The shape was neither good nor bad. It was conventional, rectangular in shape, and there is nothing especially wrong about that. One of the difficulties of Royce Auditorium, in contrast, is that the rear wall is concave toward the stage, and sound, therefore, that is projected toward the rear is reflected back convergently and is heard in the front as an echo. We didn't have that difficulty in Millspaugh, but we did have the difficulty of excessive reverberation. We don't have excessive reverberation in Royce Auditorium, but we have a certain defect of shape. [tape off]

Referring to two letters in the file for 1924-25 that came from the administrative office, apparently the Department of Physics had volunteered to provide amplification of the radio broadcasts of Calvin Coolidge's inauguration on March 4, 1925. I note that the letters mention Delsasso--and I see two others here who later became interested in acoustics: W. A. Munson, who is a distinguished acoustician today in his career at the Bell Laboratories with Harvey Fletcher, and Gilbert Wright who later was much interested, you may know, in using the voices of a cow or a horse or a jackass or some other strange sound source that he would funnel into the mouth of a person so that the person spoke with the voice [laughter] of a jackass or any other animal or sonic source he wanted to impersonate by this. And this was a device that Gilbert Wright invented and maybe patented. It was used in the motion picture industry, and also for radio programs.

DELSASSO: Sonobox, he called it.

KNUDSEN: Sonobox, yes, sure. Well, there were Arthur Warner and Delsasso and these others. This was really a group of people who were much interested in acoustics this early (1925) and had proposed that it would be possible to broadcast this inauguration in Royce Hall so that it should be heard. This is commonplace today; but it's the first instance I know of for broadcasting a radio program in a large auditorium. It may have been done elsewhere--probably had--but we said we could rig it up and we did rig it up from equipment in physics, much of which we had made, and many things were done that way in the early days of the campus. The Department of Physics was often asked to do really an engineering job like this. This job was done, and Ernest Carroll Moore in his letter of March 10 to Professor Adams gave thanks to those of us who participated in this experiment, and Moore even praised "members of your department who have so contributed to the gloriously successful hearing of the inaugural address on March 4."

MINK: Well, I think probably Silent Cal was not silent. [laughter] Can you tell me how, Dr. Delsasso, you went about rigging up this apparatus? Do you remember?

DELSASSO: Well, it was of course radios. It was just a question of amplifying that, and I think this was after we had gotten a few loudspeakers from the Bell Laboratories.

KNUDSEN: That's right. We had a cone-type loudspeaker that came from the Bell Laboratories. I don't believe they were in common use at that time.

DELSASSO: It was one of the first. I know I saw one down at the Edison Company just about the same time. But our loudspeaker was just two BTL cones back-to-back, instead of a horn, and it didn't put out very much acoustical power, but it put out enough to be heard in Millspaugh Auditorium.

KNUDSEN: Delsasso, I'm almost sure they were ones that we obtained as gifts from the Bell Telephone Laboratories and which we used in our early experiments on the hearing of speech in auditoriums. When we were measuring the reverberation characteristics of these high school auditoriums and others, we needed a powerful loud-speaker. We had used it there for this research, and it was simply used for the broadcast of Calvin Coolidge's inaugural address in March of 1925.

MINK: This brings out a point which I would like to ask you, Dr. Knudsen. Through your association with Harvey Fletcher and with Bell Laboratories, were you able to get gifts of equipment, and if so, what other kinds of equipment did you get from the Bell Laboratories?

KNUDSEN: Almost everything we had for our early research here came as gifts from the Bell Telephone Laboratories and largely through letters of appeal that I had written to men I knew back there--not directly to Harvey Fletcher, but to others there who remembered that I had been at the Western Electric Research Laboratories. We got as gifts the vacuum tubes that we used for our first vacuum tube oscillators and amplifiers that we used in our experiments, not only in the investigation of the acoustics of auditoriums, but also in the work we later inaugurated, or almost simultaneously inaugurated, on problems of hearing and deafness. In the collaborative work we did with Isaac Jones, in the first audiometer that we built, we had Western Electric tubes that I think were gifts to us. For later audiometers we built we purchased

Western Electric vacuum tubes.

DELSASSO: They're still in the one audiometer we have in our laboratory.

KNUDSEN: Delsasso would buy the machinery he had, the inductances we used in these first units, and many of the other parts were homemade. One of these earlier models of the audiometers is being conditioned, I hope, as a gift for the Hope for Hearing Laboratory which occupies the sixth floor of this building.

MINK: Now, do we want to go into the discussion of the department and the coming of Dr. Barnett and the problems of Dr. Adams? Shall we begin now on that?

KNUDSEN: I think it's fortunate in the respect that Delsasso is here. I should say for the record that Dr. Delsasso and I both may have somewhat partisan views on these subjects because we later both became enemies of him--or he regarded us as enemies and treated us as enemies, or as persons not suitable for the responsibilities we had here in the department.

MINK: You're speaking of Dr. Barnett now?

KNUDSEN: I'm speaking of Dr. Barnett. And I think it's necessary to preface this, and this is the part that I think should be sealed for a long time and considered very confidential, the personalities of the two principals involved here, John Mead Adams and Samuel J. Barnett. They are very different personalities. We have indicated earlier that Adams had weekly, or at least two meetings a month of the department. At one type of these meetings the regular members of the faculty met in one of the rooms in Millspaugh Hall, I notice. At the other type of meetings, everybody--assistants, associates, irrespective of rank--met with the department at luncheon in a Hollywood restaurant on Vermont Avenue north of Hollywood Boulevard. And there was a democratic spirit that pervaded these meetings that Adams followed religiously.

Adams was perhaps the most orderly person in handling the affairs of the department that we have ever had. I think there's no question about that. We've said a great deal about Adams, and an examination of the files today would indicate how careful and how thorough and orderly he had been in all these things. Just his handwriting alone reveals this thing; you know from examining these records that you're dealing with a very orderly person. He kept these things in such good order that anyone could go into the departmental office at any time and find specific answers to most relevant questions. Among other things I know we received specific instructions.

When I came here I received a set of those written instructions about just how we were to record the grades and to enter them into the records in the chairman's office, so that whoever was sitting in the chairman's office could answer any questions. One of us had to be sitting there at all times to answer students' questions, who may have come to inquire about a grade or inquire about something else. But these things were handled more meticulously, I think, and more precisely than they have ever been since then. I don't believe that's an exaggeration. These were some of the characteristics of Adams as the chairman of the department. Dean Charles Henry Rieber said at one time that whenever he saw the signature of John Mead Adams at the end of a report, he knew that this report was true and accurate. He could put it aside and didn't have to worry about anything that John Mead Adams had written into a report.

Barnett was really a classical physicist and a classical scholar. I'm not sure where he was born,* but he had a New England puritanical nature about him. The matter of upholding what he considered was right was a sacred duty. Among other things he thought it was very dishonorable that one member of the staff, Art Warner, for example, married within a year, I think, after the death of his first wife, and presumably this first wife had urged Art Warner not to remain single.

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*Woodson County, Kansas.

I think this was the first evidence we had of Barnett's puritanical nature. He felt this was reprehensible for a member of the faculty of the University of California, Southern Branch, to marry within one year after the death of his wife. And this, so far as I can remember, was the first instance I saw of what I thought was a puritanical view. Even in spite of my early orthodox training, [laughter] it seemed unduly severe and unduly interfering in the private life of a man. Delsasso and I knew Warner very well. As the record would indicate, we worked together here very closely, and we had been very loyal supporters of John Mead Adams during this period. Professor Barnett came to UCLA with the very highest of recommendations, although the departmental and administrative records indicate that one offer had been made to Professor Harry Bateman at Cal Tech. The correspondence between Bateman and Adams, as the records that you obtained from the administrative office here indicate, show that Bateman had considered very seriously accepting appointment as professor of physics. The records, you will recall, Mr. Mink, indicate that there was at the top of the list of the names of the members of the physics staff, "blank blank, professor of physics." That continued for two years [1924-1925], and the next year [1926] the name of Samuel J. Barnett was there.

The qualifications of both Bateman and Barnett had been discussed in these departmental meetings that Adams called routinely, and the department had singled out these two men in that order--the offer was first made to Bateman and the second to Barnett. There were these two men that had been carefully considered, both of whom had very fine records as physicists. Bateman was a theoretical physicist and especially a mathematical physicist. And Barnett was primarily an experimental physicist who had done outstanding work in the field of magnetism--magnetization by rotation of a material or the converse effect, the rotation as a result of magnetization. These are two effects that Barnett worked upon during most of his life, and it's often referred to as the Barnett effect.

Barnett was very highly respected in the world of physics for the work he had done in this particular field. And at the time he came I think we all felt, "Well, we are really getting an outstanding research man." He knew the work of Einstein. He knew the work of Ehrenfest, and he went into the work of Hendrik Lorentz and other important men in the world of physics at that time. I think there was a real feeling that his appointment was a valued contribution and a step forward in the development of the Department of Physics, and I think it's fair to say that his high standards of scholarship and research did have a beneficial result on the upgrading of the Department of Physics. The two appointments that were made during Barnett's chairmanship, for example, Kaplan and Kinsey, I think everyone would realize, was an important step in the development of the scholarly standards of the department. Here were two men who had held National Research Fellowships, and were brought in, really, as recognized scholars. Their work had already indicated that they were very much research-oriented and this was going to very definitely upgrade the work in the department.

Barnett, in contrast to Adams, referred to himself as being flexible and Adams as being rigid. Some of the comments they made, and I know some of the letters that passed around, some of which were distributed to members of the department, emphasized this contrast. Barnett believed in flexibility, as he called it. But many of the rest of us felt that he did not exercise the same care in keeping the records of his correspondence with others and especially the office of the Director (Dr. Moore). There were certain, well, if not ambiguities, things that were not stated quite so clearly as they were stated in letters by Adams. Adams was concise and to the point and his sentences were short. Barnett's sentences often were a little involved, I know in one particular instance. And the one that really led to the first conflict between Adams and Barnett was a result, I think, of a misunderstanding that occurred between the director's office and the business office and the Department of Physics, because Barnett had indicated in a certain letter a willingness to sacrifice a certain appropriation for general assistance if another activity would be supported. You have the letters and you could document this.

MINK: This could be documented.

KNUDSEN: And as a result of this incident, during maybe the second or third year that Barnett was chairman of the department, we ran out of funds for general assistance and there were complaints all along. At a meeting of the department, apparently, Adams indicated that part of this was a result of this ambiguity in the dealings with the business and administration office over there, and that we were suffering because of a fault in communications between the department and the administration offices. Delsasso may be able to remember more details about this than I do at this time, but I know this was the beginning of conflict between Barnett and Adams.

SECTION X

DELSASSO: I'd just like to point out that John Mead Adams was a distinguished student, that he graduated with high honors from Harvard University in 1905, and that he had had considerable experience in teaching and in research, both at Occidental and in Canada.

MINK: We can get that for the record and put it in.

DELSASSO: He came here as an experienced and able person by 1919. In my opinion he was an exceptionally able administrator, a superb teacher, and quite a capable investigator, although as indicated his publication record was not very great. He had devoted himself exclusively to building up the department, and as indicated in the record had finally taken a little leave in order to do some work on snow flakes, which he had started but had not had a chance to complete. And I say also that when these two people, Bateman and Barnett, were being considered with the others--but these were the two main candidates--that the whole department was in on the discussion. The decision obviously was made by the senior people, but everyone, including the teaching assistants, had an opportunity to talk about the person they would want to bring in.

MINK: Dr. Delsasso, there's one thing that really perplexes me though. While the department was in on it, the decision to have a full professor in the department and not to promote John Mead Adams to full professor strikes me as something that must have been made higher up.

DELSASSO: I think this decision was made with Adams's consent and thorough support at the time.

MINK: You do?

DELSASSO: Yes, indeed, because we talked constantly about the need to bring in distinguished people in the department. I don't know whether he thought of him as a chairman, particularly. I think he rather thought of him as a person who would add distinction by the research that he would do and that the rest of us would take care of some of the housekeeping work. But I know of no time where Adams ever indicated before Barnett arrived that he did not expect this man to get the full cooperation and the lion's share of whatever we had to build up the department. As a matter of fact, I remember him saying, "Well, if Barnett comes over here, although I'm started on this research in snowflakes, if I can be of assistance in shifting over to his field I will do so." This was done before Barnett came, but he was looking ahead. He might be able to help strengthen this one feature that was going to be really important to the department. So that before Barnett came I'm sure there was every indication that he hoped that this man would be the big professor in the department and would help to strengthen the department.

MINK: Dr. Knudsen, you had said that before Dr. Barnett came to the department that Dr. Adams had been recommended for full professorship. Is this correct?

KNUDSEN: No. I don't believe I said that. If I did I certainly [should correct that].

MINK: Maybe I misunderstood you.

KNUDSEN: I would have to consult the record to know just when that occurred. Professor Franz was here, I know, and this is again a very confidential matter. Professor Franz had apparently been chairman of the faculty committee to act on the promotion of Adams. Now the record somewhere along should show what year.

MINK: This is Professor Shepherd I. Franz.

KNUDSEN: Shepherd I. Franz, yes, that's right. And Franz, I suspect, violated a rule of confidence, and the word got back to Adams probably through the route of the women--excuse me--but at any rate John Mead Adams had been told by a chain of information that the faculty committee had recommended him for promotion to full professorship, and that it undoubtedly would come.

It did not come; and this, of course, was a very great disappointment, I know, to John Mead Adams. But this very well could have come even after Barnett was here. I'm not sure about that. I doubt very much that Barnett ever recommended him for full professorship.

DELSASSO: I have no knowledge of this committee at all.

KNUDSEN: I think it's accurate to say that, whereas during the time Adams was chairman of the department, we did have these regular, frequent, democratic departmental meetings and all members knew what happened. There was not anywhere near that degree of democracy after Barnett came here.

MINK: How was this changed?

DELSASSO: Well, the meetings, mostly.

KNUDSEN: The chairman calls the meetings pretty much, and the meetings were called only occasionally, and I don't remember how often but certainly not often.

DELSASSO: No, not often.

KNUDSEN: I think we had very little voice in the selection of Kaplan and Kinsey. I think Barnett corresponded a great deal with Leonard Loeb, who was professor of physics at Berkeley and whom he knew, and Loeb, in turn, knew Kaplan and Kinsey, and it was pretty much a matter that had been decided without our concurrence. We didn't disapprove, of course, at all, but there was a lack of the same kind of democratic operation of the department after Barnett took over the chairmanship.

DELSASSO: I think this is shown in the correspondence, that it was largely the selection by Loeb of these two people which Barnett concurred in and finally reported to the department after it was practically sealed, signed, and delivered.

MINK: Professor Loeb at Berkeley.

DELSASSO: At Berkeley.

MINK: To go back just a moment, it seems to me that I had raised a question about Dr. Adams's failure to be promoted in terms of his research. And I had asked you, Dr. Knudsen, if it wasn't so that in this period teaching was one of the major criteria for promotion rather than research.

KNUDSEN: This should have been the criterion at the Southern Branch at that time, because there were no facilities for research, and without facilities for research in a department of physics it's pretty difficult to judge a man's competence in research. Certainly, the qualifications for promotion which do regularly involve teaching and research and service within the university--and extramural service as well--are the usual criteria. And at an institution which has no facilities for research in the physical sciences, particularly, it doesn't seem quite fair. But all of these promotions had to clear through Berkeley to be sure, very definitely. And Berkeley did have then the publish-or-perish criterion for promotion, and undoubtedly that was the real obstacle in the promotion of Adams to the full professorship.

MINK: In other words then, even if Shepherd Franz and his committee had recommended Dr. Adams, it might be turned down at a higher level at Berkeley.

KNUDSEN: Yes. Of course, that has been a point of controversy even during the last years of my administration here, and I presume it hasn't disappeared altogether even today, although it's improving all along.

MINK: Yes. Well, now to go on with Dr. Barnett's administration--it appears then that about this time Dr. Barnett wrote a confidential letter to President Sproul, which we have not been able to obtain as yet, which was then returned to Dr. Moore. And Dr. Barnett was asked to take the situation in the department up with Dr. Moore. Now I'm not sure exactly what this situation was.

KNUDSEN: I think I can comment somewhat on that. We have already indicated that the first incident that really came to the attention of the department, at any rate, was a letter that Barnett circulated to the members of the department in which he castigated John Mead Adams for what he considered an attack on him at the meeting of the department which was held just a few days before.

MINK: We have a letter about that.

KNUDSEN: You have a very long letter detailing these things and this had, I'm sure, been communicated also orally to Ernest Carroll Moore. The record would indicate that Barnett felt he was not getting what he was entitled to in his dealings with Dr. Moore, so he wrote directly to Sproul. And the file here that you obtained from the UCLA Administrative Office does have a copy of a letter from Sproul to Barnett of April 6, 1932. Things had been moving in the direction of a confrontation of some sort between Barnett and the department and the administration, beginning at this earlier date which you can verify.

In this long letter that Barnett wrote to all members of the staff, he almost demanded an apology on the part of John Mead Adams, because he felt that this was a personal attack upon him, and feelings undoubtedly had been very, very much strained. But Sproul's reply to Barnett--to this confidential letter that we don't have access to, at least as yet--says, "This is a late acknowledgement of your letter of March 25, but I wanted an opportunity to talk with Ernest Carroll Moore before I took this up." He said, "After talking with Dr. Moore, I cannot reach the same conclusions that you do about the difficulties in the physics department. It seems to me that there is something to be said on both sides of the unfortunate controversy which has arisen, and that we would not improve our position by taking drastic action at this time."

I'm not sure what that drastic action was at that time, but it was something apparently that Professor Barnett had recommended to Moore. I know there were many problems that Moore was worried about in the department here. On more than one occasion--two or three at any rate--both Dr. Kaplan and I were called into Dr. Moore's office. And I think during the last year that Barnett served as chairman, Kaplan and I were more or less requested to work with Barnett to try to eliminate some of these sources of difficulty. There was a succession of men who became subject to the censorship of Barnett. We mentioned Art Warner because of this marriage. Delsasso, I think, probably was a third person who became involved.

DELSASSO: Yes.

KNUDSEN: I think it's generally known. Delsasso may want to comment on this, that Barnett wanted to really get rid of these three men--Adams and Warner and Delsasso.

DELSASSO: As a matter of fact, I think that we got this directly from Moore when we went over to talk to him. He said, "I have recommendations to relieve you three people." And he indicated that he was reluctant to do this, but that this had been the recommendation from Barnett, I suppose.

KNUDSEN: Well, I know Kaplan and I in our conference with Ernest Carroll Moore indicated that this was entirely an unjustifiable act on the part of the chairman to dismiss three people. That's really what was the prospect at this time, that these three colleagues were to go. Adams and Delsasso, particularly, had been here from the very beginning and had laid the foundations for this department, and it seemed to every member of the department that they should stay. I know that Kaplan was with me, and he represented the view of Kinsey very definitely at that time and I represented the views of Dodd and Hiram Edwards at that time. There was unanimity of opinion among all members of the department, except the chairman, that these three men were valuable men in the department and there was no justification whatever for recommending that any one of them should go. Delsasso and Warner did not have tenure at that time, and so it would be possible without having, you know, the usual hearings and so on to terminate their employment. But John Mead Adams had tenure, and this was quite another matter, and all of us felt that John Mead Adams really deserved promotion rather than discharge from the university.

MINK: I guess I would like to ask one question about one letter that I noticed in the file, which perhaps you could explain. It seems as though Dr. Adams kept postponing his sabbatical at this time, which he wanted to take to continue his work on snowflakes. And I think finally he did take this sabbatical in 1926, I believe.

DELSASSO: It was somewhere around there.

MINK: And yet later there is a letter from Dr. Barnett to Dr. Moore and a reply saying that Dr. Moore does not wish to penalize Dr. Adams. [tape off]

KNUDSEN: I was enlightened, as I'm sure Dr. Delsasso was, yesterday when we learned some new facts for the first time by our perusal of the files for the year 1932-1933. This letter was dated July 21, 1932, from Barnett to Ernest Carroll Moore, and it dealt really with replacement of someone for Dr. Adams who was to be on leave. He says "Professors Edwards and Knudsen and others in the department have informed me that they have heard Dr. Adams will be away on leave." This is a peculiar way of putting it because apparently there had been some agreement between Moore and Barnett in conversations that they had had, which I certainly read into the letter, that Moore replied to this letter of Barnett's of July 21, which was simply dealing largely with a budgetary matter and in getting some help to make a replacement for Adams who would be away. And the letter of July 30 to Barnett was, as I say, extremely shocking to both Delsasso and me because we knew nothing of this, certainly. Moore's letter says, "The university feels compelled to discipline Professor Adams by requiring him to take a compulsory leave of absence for one semester. It is disciplining the department by requiring it to carry on without additional help. In accordance with our conversation Professor Knudsen will be the chairman for next year, 1932-1933. He will be notified at once, and I am asking him to arrange the program of the department." This, as I say, is a new bit of information to me, and although I knew of certain difficulties I did not know this.

DELSASSO: Apparently to Barnett, too, that you were going to be chairman.

KNUDSEN: Yes, it was new to him at this time. This was a decision, I'm quite sure, that Sproul and Moore had reached when they discussed the situation that Barnett had raised. Barnett had insisted that Adams should make an apology at one time. Then in a letter he wrote to Adams he said, "Let's forget the whole thing." But there was real rancor between these two people on that situation. I think that Moore and Sproul together decided that the department couldn't go on any longer under the administration of Barnett.

MINK: I don't quite understand what Dr. Moore meant by disciplining Professor Adams.

KNUDSEN: We don't either. It must be what Barnett had reported to him about what Barnett had considered reprehensible conduct when Adams had complained at the end of the departmental meeting about a fiscal matter. Now, normally a person can complain about a fiscal matter without incurring animosity and bitterness, but apparently this was engendered on the part of Barnett when Adams made this remark in the department. It was a logical thing for Adams to make it because he had been so meticulous about bookkeeping matters and things of that sort, and knowing just where we were financially and in all other respects, and I think Barnett had been a little ambiguous about some of these things.

MINK: Well, does this wind up the problem of what then happens? You became chairman of the department in that next academic year.

KNUDSEN: Yes.

MINK: Would you like to discuss this now?

KNUDSEN: I don't remember anything unusual at this time. I think we instituted again routine meetings of the department. That was the main thing I would say that I did. We had regular meetings of the department, and I think there was a return very much to the democratic type of administration that Adams had demonstrated to us as something we thought was a model to pattern after. I'm sure this disturbed Barnett. I became the fourth person [to feel] his bitterness or animosity--that is, I think he felt it was a disgrace that he should be relieved of the chairmanship of the department and that this young upstart should be named as chairman of the department. I'm probably imagining more than I ought to, but Delsasso might have something to say about that because I think he would know what the attitude of the department was. Certainly, I wasn't a dictator. I don't believe I've ever been that type. Maybe I've been a little compassionate, as Clark Kerr has sometimes been accused of being compassionate.

MINK: Would you like to comment, Dr. Delsasso?

DELSASSO: Well, as a matter of fact, as Dr. Knudsen has said, we returned to the normal procedure of putting the budget we had to work with on the board and having everybody slice it up in the way they thought that could best serve the department, and from then on they knew what they had to go on. We returned to normal operation of the department. One thing I might comment on is that the university has used this device from time to time, when there's disagreement, of having one of the members go someplace else in the university for a year or a semester to sort of cool off--for everybody to cool off. That's the only thing that I can think of, and Adams did take his leave and went to Berkeley, as I remember, for a semester. I don't think it was a year.. I think it was a semester.

MINK: Then this would explain possibly what Dr. Moore meant when he said disciplining Professor Adams by asking him to take compulsory leave.

DELSASSO: Yes. Well, I think he actually just transferred to Berkeley for that semester, or for a year. I'm not sure which it was.

MINK: Do you feel that this controversy had any effect upon the teaching in the department during this period?

DELSASSO: People do their job. They don't let things like that interfere with their real mission. I don't think the students suffered. I think certain staff members suffered from having this controversy going on, but I don't believe either the research or the teaching suffered from this. Did you, Vern?

KNUDSEN: No. I don't believe so. Certainly, my own role wasn't changed greatly. I was reaching the peak of my research productivity. I think the years 1930-1933 would be my best. Anyone who would analyze my years of productivity would say that's where the peak was in Knudsen's career. And this actual year of 1932-1933 was the year that Hans Kneser was here. The research work that I'm known for more than anything else was really consummated in this first year that I was chairman of the department, and things were organized in such a way that I had my afternoons free. I just wouldn't allow any meetings of any sort to interfere. We'd have our departmental meetings late in the afternoon or at lunch time. But after lunch the rest of the day and the evening I really was working at research, even though I was chairman of the Department of Physics and teaching, I guess, two courses at any rate. I think when I was chairman of the department, I'm quite sure in 1932-1933, the record would indicate that I was teaching two courses, one of which would certainly be acoustics throughout the year, and maybe another course.

DELSASSO: Maybe 1A or another lower-division course.

KNUDSEN: Yes, some other lower-division course. But there was nothing unusual about my administration. I didn't consider it a difficult task. It was difficult only because I knew Barnett was bitterly opposed to my taking over the chairmanship, and I would have no objection whatever to having in the record the letter he wrote to me, and I've looked for it in my file. I can't find it now, but if I find it I'll certainly make it available to you.

MINK: Perhaps you can state just the essence.

DELSASSO: I can add a little to it. I know that it was a very bitter statement to Dr. Knudsen saying that he had in the past been able to support him wholeheartedly in what he was doing, but to have him take over the chairmanship was premature, that sort of statement. To the extent that I knew the letter had arrived--I don't mind this being in the record--and because we were celebrating, I think, his taking over the chairmanship, something of that sort, with a little party. I didn't want him to be disturbed by it, so I kept the letter for a day or two until this party was over before I gave it to him.

MINK: You mean the party went on for two days?

[laughter]

DELSASSO: I don't recall now just how long I kept it.

MINK: Well, I know that you said, Dr. Knudsen, that you were shocked by what you saw here in the record in that you had no idea that Dr. Moore and Dr. Sproul expected you to be a disciplinarian.

KNUDSEN: I was told to bring about peace in the department. I think [laughter] that's about what I was told.

MINK: Were you told that in so many words?

KNUDSEN: I don't remember the exact words that were used, but, as I say, Kaplan and I together had several conferences with Dr. Moore, and the two of us at one time had been encouraged by Dr. Moore, almost commanded by Dr. Moore, to try to bring about peace in the department as it was at that time, when Barnett was still chairman. And apparently we failed in our efforts.

Certainly, I had no ambition to be chairman of the department. I was very much engrossed in my research, and the chairmanship wasn't a job I craved at all. I held it for six years and I was very happy to be relieved at the end of six years because I also had the office of dean of the Graduate Division (beginning in 1934) at that time. It was too much. But we got along I think democratically very well during those years. But from the time I was made chairman, I became a target of Barnett's vindictive attitude.

MINK: Dr. Delsasso, when he said in essence that he could no longer support Dr. Knudsen, was he talking about supporting his research activities?

DELSASSO: No. He meant supporting the move that the university had made of putting him in as chairman. And, of course, he was replacing him.

MINK: Now I have a question about Dr. Moore. Dr. Moore has been very controversial in information we've been able to glean on him in his own records. In fact, you might be interested to know that he has been the target of a good deal of study by graduate students and people working in seminars and so on, particularly in the period that is just upcoming here, having to do with the communist activities at UCLA. Would you say that Dr. Moore's attitude became more and more autocratic from the period, say, 1926 going up through into the early thirties? Did he tend to become more autocratic in his dealings with the faculty and with the chairmen of the departments?

KNUDSEN: I think that's a fair appraisal of a trend that was developing. This I noticed especially in the latter part of this period you refer to. I had relatively little communication with Dr. Moore until we moved to this campus. Then in 1929, I was on leave during that next semester, so it was really 1930 when I began having communications with him, and the difficulties were then going on in the department. I think Moore thought I might be able to be of some service in bringing the department into harmony again, because it was quite obvious that the difficulties originated from the time Barnett came here. This incident that involved Barnett and Adams in the first place and later involved these three men, was very well known in the department. Barnett was trying to really move them out of the department--that is, have them discharged from the university or at least removed from this department. Maybe he'd have been satisfied if Berkeley or some other place would take them, but at least he didn't want them in our department. You can imagine that all members of the department resented this very, very much. And probably I resented it more than anyone else. I'm not sure about that, but at any rate Dr. Moore got from Kaplan and me a story of this feeling that Adams and

Delsasso and Warner should <u>not</u> go, that they were very important. And this was primarily the issue on which Barnett was trying to bring about their discharge, and really Barnett knew that Kaplan and I had supported them. I don't think he aimed at Kaplan nearly as much as at me. But if Kaplan had been named chairman, I think Kaplan and not Vern Knudsen would have been the victim.

DELSASSO: Sure. It would have been the same thing.

MINK: I'm still asking about Dr. Moore.

KNUDSEN: Yes.

MINK: I'm wondering if you could give me some examples of this change of attitude. I think it's very important for this period in the history of the university that this be understood.

KNUDSEN: Well, I have an important incident to relate that comes maybe a little later in the record, when I first became dean of the Graduate Division, because this promoted me to cabinet status here on the campus. I met with the provost's advisory committee, which was made up of the deans and the business manager and the registrar. Do you want this story now?

MINK: Might as well.

KNUDSEN: Dr. Moore had these cabinet meetings of the administrative committee every Monday morning at nine o'clock. And there was a very definite format that he had for these meetings. He would first of all turn to Dean Helen Laughlin, dean of women. He inquired, "Dean Laughlin, have you any problems with the girls?" She would reply, "No, Dr. Moore, I have no problems with the girls." And if there was a, oh, a drive, say, for the Red Cross or some charitable organization of that sort, Dr. Moore was really interested, and he'd talk for a few moments about that, and then he would turn at once to the danger that the university was facing because of the uprising of communism on our campus--that is, he saw communists everywhere. There were probably not more than three or four of these meetings in the fall of 1934, which is the first year I was dean of the graduate division. At this third or fourth meeting we had that same format of inquiring about the girls and then gravely announcing: "I'm going to read to you this morning Lincoln's Emancipation Proclamation," and he read us the entire Lincoln's Emancipation Proclamation. This was followed by, "I am this day issuing an emancipation proclamation for this campus. I ask not for your advice but I ask for your support." This was the way it was presented to us at that time. And Moore's emancipation proclamation consisted of his decision to fire four students from the university, discharge them from the university, and he named them. One of them was the president of the student body and one of them had been very closely identified I know with the University Religious Conference, and one of them I think was probably a troublemaker of the kind you often have on the campus. She had been troublesome at USC before she came here.

DELSASSO: Celeste Strack, I believe.

KNUDSEN: That's right. Strack, that's the name, and I believe her first name was Celeste.

MINK: Hansena Frederickson spoke of Earle Swingle.

DELSASSO: Yes, I know Earle.

KNUDSEN: Yes.

MINK: Earle Swingle kept the minutes and I imagine he was involved in ferreting out the communists.

DELSASSO: Oh, sure.

KNUDSEN: He may have kept the minutes of this meeting I'm referring to.

MINK: Did you ever have anything much to do with him that you can remember?

KNUDSEN: He was the man you'd sometimes have to go through in dealing with Dr. Moore, and Dr. Moore had him sort of as a front man for certain things. I don't remember any real encounters with him. They were routine things if I had them, and I don't even remember what he looks like now.

MINK: Generally, how do you think the faculty regarded him?

DELSASSO: Oh, as an office boy over there.

MINK: Was Earle Swingle at UCLA? I believe he was.

DELSASSO: Yes, he was a UCLA boy. I know him.

MINK: I think he was the president of the student body.

DELSASSO: He was the president of the student body.

MINK: And Dr. Moore selected him to come into the office?

KNUDSEN: I think Hansena would know the details of that. She was very close and knew what was going on. I had almost forgotten that there was such a man as Earle Swingle, until you brought up the name, and I certainly agree with Delsasso that he was much of an office boy over there, not much more.

DELSASSO: This is just an incident in Earle Swingle's tenure over there in Dr. Moore's office. He was usually away attending to, oh, all sorts of details like pinning up posters properly, exactly in the right place, carrying messages or going for a cup of coffee or something. And Earle would, I'm sure, enjoy this as much as anybody, because he would go and leave the phone and it would ring constantly. Next to him was Dean Rieber's office, and Dean Rieber was one day taking care of a mother who had problems with her daughter and it was a serious conversation. Dean Rieber couldn't continue to tolerate this phone ringing, so he excused himself, got up, went into the other room, pulled the phone out by the roots, came back and continued his conversation. And I'm sure after that, that the phone was either turned off or Earle Swingle was in the office to answer it. [tape off] [interview of February 21, 1967 begins]

KNUDSEN: I read in the paper this morning that George Romney said if necessary he would break with the Mormon Church on the matter of racial matters.

MINK: Oh, is that right?

KNUDSEN: That is a rather strong statement, but it's typical of the feeling I have had on that subject for a long, long time. And it's incidents of that sort that really brought about a transition in my life from an orthodox Mormon who changed--one way of putting it--from a belief in Jesus Christ as the Savior, as the Immaculate Conception, and all of those things, to the more intellectual belief in Jesus as a great man--so great that I continue to worship him.

MINK: When do you think that started, approximately?

KNUDSEN: Well, the germs were there perhaps early in my life. In my early missionary experience I know I was often plagued somewhat by the feeling, am I really doing the thing which is honestly the thing I should be doing? And the momentum I know of my early life carried me along a long, long time in a strictly orthodox way, and I have no regrets about that, in fact, it has contributed greatly to my joy and success, and I am grateful to have been brought up as an orthodox Mormon. And I also have no regrets about the transition. And I recognize that the fundamentalism I was taught in my youth was good basic training. But even my father, I know, had doubts about the biblical account of the creation of the earth, and the doctrines of Charles Darwin began to take root in his life. I know often he talked with me, shaking his head, he said, "You know, Vern, I don't know about these things. Perhaps Darwin was right after all." And this was at a time when three professors at the Brigham Young University had been discharged because they taught evolution.

MINK: I think you said that when we were talking about Brigham Young.

KNUDSEN: Could be. And this I know disturbed my father. My father didn't believe that this was right. And I was just a young boy at that time. I know I often said that this would never have happened at the Brigham Young University even at the time I was a college student, because the biologists were teaching evolution at the Brigham Young University and it was recognized at that time. We at least weren't quite as far back as they were in the Scopes' case.

MINK: Yes.

KNUDSEN: In Kentucky.

MINK: Right. I think at one point I was going to ask you, as you went along and after you finished your doctoral work and you came here to the Southern Branch, then you began to actually practice your profession of teaching and your research, how did this change your religious life--if it did, and also would you say something about your religious life just after you arrived here in Southern California.

KNUDSEN: My religious life following the completion of my mission in 1918, continued for many years, really, in a fairly orthodox manner, and I continued primarily in the teaching aspects of things. As soon as I arrived in New York, in February 1918, I'm sure through the recommendation of Harvey Fletcher who knew me very well, I was asked to take charge of the adult Sunday school class in Brooklyn. Therefore every week I participated in teaching lessons there that were doctrinal in quite a definite sense; but I was much more interested in the moral aspects of religion than I was in the doctrinaire.

In reviewing the notebook that I kept during those days, I find that there was very definitely a gradual transition from the doctrinal to the religious, the moral, the ethical aspects of religion. Certainly, I had no regret that I had been brought up in an orthodox atmosphere. This, in youth particularly, I think has certain values, and I regret in a sense that I didn't provide for my children the same orthodoxical environment that my father and mother provided for my brothers and sisters and me as we grew up, because it was a strictly orthodox life. But even in my youth there were evidences of doubt appearing in my father, who was an intelligent person but had very little advantage of formal education. I think he had the equivalent probably of a fifth- or sixth-grade education, but he read all his life and he was really a learned man, a thinking man. He was plagued very much I know by the questions of, well, the six days of creation and by the stories of the Old Testament. And when evolution came along, I'm sure this troubled him very much. The evidence looked very convincing, and it was of course very convincing to me. From the time I began my college career I began having certain doubts about orthodox views in religion.

But to the extent of completing a mission, where you had to be orthodox, I was an orthodox Mormon through those years, and I continued in an orthodox fashion, but with certain doubts from that time onward. When I arrived in New York to take on my first professional job at the Bell Telephone Laboratories, I'm sure that Harvey Fletcher was responsible for saying, "Well, I think

Vern Knudsen would be a good man to have in charge of the Sunday school class," at the Brooklyn branch of the church at that time. It was a mission at that time. Later, it became a Stake of Zion, and wards, with the usual auxiliary activities in the Mormon Church organization. They had a very fine chapel in Brooklyn, and I had an adult class of probably fifty, seventy-five or a hundred people, and I was given considerable freedom in choice of subjects we would talk about, although we had a guide that had been prepared by the general authorities of the church in Salt Lake City which was made available throughout the church. I departed a great deal from the guide, and I enjoyed this work of teaching then, and I'm sure it had a beneficial effect on my later teaching career.

I think I told you earlier that as early as a senior student at the Brigham Young University I was sent out on preaching assignments. I especially used one text that I've used a great deal throughout my life since then, and it's been a very important influence in my life--the thirteenth chapter of Corinthians--which is the really choice thing in all of the Bible to me. I think I know the thing by heart because I've used it so many, many times. You often hear it now at funeral sermons and on other religious occasions.

Well, I continued this Sunday school teaching and teaching in another organization of the Mormon Church, what's called the Mutual Improvement Association, at Brooklyn. Then after a little more than a year and a half at the Bell Telephone Laboratories and with these religious activities at Sunday school and the Mutual Improvement Association at Brooklyn, I continued active church work during my graduate years at Chicago. And although I didn't preach as much or conduct Sunday school classes, I was made a--what is called an assistant superintendent of the Sunday school at Chicago, which was held at the same place where I had my headquarters as a Mormon missionary in Logan Square, and this was on the northwest side of Chicago. The University of Chicago, of course, is on the south side, and it was quite a ride. It was a trip of an hour's duration on the elevated train to go over there, but I don't believe we missed a Sunday unless there was illness of some sort in the family. Although I didn't do much teaching, I did some administrative work in the Sunday school during the years I was at Chicago.

I continued certainly as a faithful Mormon would be at that time, including the payment of tithing, you know, which is a doctrine. My tithing was paid faithfully during my years at the Bell Telephone Laboratories and during my years at the University of Chicago even, where the last year I earned fully as much as I did in my first year as an instructor at UCSB. But one-tenth of my income every month went to the tithing fund of the church. This is a pretty good test of orthodoxy in any faith, and I'm told that George Romney continues to pay one-tenth of his income as a faithful Mormon is supposed to do. This is a very important part of the Mormon Church, and it's one reason why it's a strong financial organization today, and it's becoming stronger, of course, all the time.

Well, the continuation of that work at Chicago during my three years of graduate work would normally be continued as a young instructor at the University of California, Southern Branch, in those days, and indeed it was. My sister in Los Angeles was an active member in what was known as the Adams ward of the church. That was on West Adams Boulevard in Los Angeles and was, I believe, the only Mormon Church in Southern California of any importance at that time. There were smaller branches in other suburbs; in San Pedro, I know, in Boyle Heights, in Glendale, and in a few other places, but there were probably not more than five or six. They were increasing, however, about that time, and within a few years there were many, and today I have no idea how many there are. I can't keep track of them, but there are probably more than one hundred, maybe two hundred, wards or branches of the Mormon faith in Southern California today. I believe there are more Mormons in the Los Angeles area than anyplace, except possibly Salt Lake City.

At that time [1922] we were still a mission--that is Southern California was a mission--of the Church of Jesus Christ of Latter Day Saints. And I began active participation and teaching in the Adams ward. I don't remember how it happened that I was first asked to be a teacher, but I know within a matter of weeks I was teaching a Sunday school class, and my wife was teaching a class in the Mutual Improvement Association, and we were very faithful church goers there.

At the same time I had a very active life here at UCSB, and I think I was more conscious of the career requirements of my life than anything else. I believe I told you that the room I inherited here was a cubbyhole under a stairway in which two others shared the tiny space with me. During those early years, the first two or three years, I not only taught Sunday school at the Adams ward, but when the Mutual Improvement Association was organized, this was within less than a year after I came here, I think in January following my arrival here, I was asked to be their president.

MINK: Did you have anything to do with that?

KNUDSEN: No, I didn't. It just happened that the mission in Los Angeles had grown to the extent that the church authorities decided it was time to have a Stake of Zion. They were referred to as "Stakes of Zion." And so it was organized, I would say, in January of 1923, because I know that I began stake work at that time, and I was asked to take on the presidency of the Stake Mutual Improvement Association. That is an organization of young people that normally begins at age fourteen and continues on up quite indefinitely thereafter. It is less religious than other organizations. As the name implies it's a young men and young women's mutual improvement association.

I think this name designates pretty much what its function is, and it's certainly been one of the most beneficial organizations in the Mormon Church. That is, they get these youngsters about the time of adolescence and they normally attend weekly meetings. These meetings are usually held on Tuesday nights, and there's much social life infused into the work of the Mutual Improvement Association. For example, they have at least a formal dance of some sort once a month and then the big annual dance is called the Gold and Blue Ball. Then there was basketball and other types of competition.

Each ward had its own basketball team, and once a year we assembled in Salt Lake City for the overall church organization of the Mutual Improvement Association, and this was a very big gathering. It's still conducted every June. During the years I was president of the Young Men's Mutual Improvement Association here, my wife (who was vice-president of the Young Women's Mutual Improvement Association) and I always went once a year to Salt Lake City to attend the Annual General Conference of the Mutual Improvement Association.

Certainly, this continued interest in religion had a lasting effect on my life. I discontinued teaching the Sunday school class because I became more interested in the Mutual Improvement Association. This office of the presidency of the Mutual Improvement Association involved setting up separate organizations in the various wards. We were responsible, really, for naming the presiding officers of the Mutual Improvement Association in such wards as existed at that time. There were probably seven or eight, mostly in the suburbs of Los Angeles. And every Tuesday night we would be meeting with one of these organizations, and usually on Sunday night we would be out again. So there were two nights a week.

The president of the young women's association here at that time was the sister of Marriner S. Eccles, who later became the chairman of the Federal Reserve Board. That is, Roosevelt appointed him as soon as he became president of the United States. The Eccles people had always been very ardent church people. The father of Marriner Eccles and of Vida Eccles Savage, who is the one my wife and I worked with here, was a polygamist. He had two wives. This was before the manifesto, so there were a lot of Eccles, and it's a very big and prominent family today. David Eccles, the father of Marriner and Vida Eccles, became a multimillionaire. They had all sorts of forest interests and lumber interests in Washington, Oregon, and Utah. The Eccles are a prominent banking people in the state of Utah today.

I know that Dean R. Brimhall, who was the son of my old famous George H. Brimhall, the president of Brigham Young University during my days there, and Stuart Chase were good friends. Dean Brimhall had meanwhile won his PhD degree at Columbia University and worked a great deal with James Cattell, who later started American Men of Science and these various things. Dean Brimhall and Stuart Chase decided that Roosevelt should name Marriner Eccles the chairman of the Federal Reserve Board, and they swung the deal. Marriner Eccles was one of the early appointees of Franklin Roosevelt, and you may remember that he was chairman of the Federal Reserve Board for a long, long time--I think even through the Eisenhower administration.

This is a diversion, but Vida Eccles Savage was a wonderful person, and in my wife's and my association with her and her husband we became very close friends, and our years with the Savages were very, very dear years to us. Our finances were nowheres as good as those of the Eccles at this time. They had two cars; we had none. And Vida Eccles' husband, Tim Savage, who was a grand old fellow, had a Ford with a Ruxle axle. He says it just isn't right for you to have no car. And so he insisted upon Florence and Vern Knudsen taking over their Ford car with the Ruxle. He needed it about once every two or three months, whenever he went to the desert. He was interested in mining claims in the desert, and I went with him on at least two of these trips. So, the first automobile we had was not our own but this one that Tim Savage insisted that we should use; we used it a great deal in going to meetings of the Mutual Improvement Association, and for all other personal travel.

Often, members of the Mutual Improvement Organization would come to interview me at my office at the Vermont Avenue campus. If three of us were in the office at the same time it became necessary to ask one of my colleagues to leave while I carried on the conversation with my caller. Otherwise my guest would have to stand, and there would scarcely be room even to stand. And I think this situation began to annoy me--that is, these casual callers felt that they had a claim on my time. I told them many times, "Well, now if you want to see me, you come and see me at home in the late afternoon or at night. You're not supposed to come here and interfere with my university duties." And I believe this experience was the first incident in my life that more or less withdrew me from active participation in the church. It was first of all an annoyance, and it grew more and more annoying in the coming years. Many church members felt that they could come to me to help them solve their personal problems. It might be divorce. It might be finances, and it could be other personal problems. This was interfering with my career, and I didn't want anything to interfere with my career at that time. I was becoming increasingly interested in teaching and research at the university, and my interest especially in research became so attractive that it really took precedence on many of these other things. I think this was the beginning, really, of my transition from a regular faithful church-going member of the Mormon Church to one that still believes in its basic religious principles so far as they are moral and ethical, but some of the orthodox principles, I have come to believe, are not what I was taught to believe in my youth. I continue to value my membership in the Mormon Church, and I especially value and support the high ideals and good works of my alma mater, the Brigham Young University.

SECTION XI

MINK: This morning I think you said you were going to talk about the establishment of the Acoustical Society of America.

KNUDSEN: Yes. That's a subject in which I am much interested. A little background material might be appropriate. Until about 1925 the American Physical Society and its journal, the Physical Review, were really the big show in all of physics, including spectroscopy and acoustics and atomic physics and relativity theory and molecular physics and quantum theory, which was just beginning to explode really at that time. And as a result of that interest in new fields--really, acoustics was somewhat in the condition that physics was supposed to be in about 1895 when, you know, the physicists of that day said, "Well, there are no fundamental discoveries to be made now. They have all been made and about all we can do is add one significant figure in refining these constants of physics and so on."

And then, of course, there evolved the work of [Wilhelm Conrad] Roentgen on X-rays and the work of the Curies, who were working with radium and the breakdown of heavy radioactive atoms, and the work of J. J. Thompson on electron physics, followed later by others with Millikan's work on measurement of the fundamental electrical charge. And these developments were really in the forefront in physics of that day.

This was on a crescendo really and had reached almost a triple fortissimo level by about 1925, and acoustics under these circumstances was not looked upon by physicists with much favor. It was considered that the work that Lord Rayleigh had done in his monumental two-volume Theory of Sound, which was published in 1876 and revised successively, one as late as 1921, in various editions, was the last word in acoustics. A lot of people felt, well, this is a has-been branch of physics, and there is not much point to working in acoustics; and quite a number of my friends and colleagues, Millikan included, thought that I was off the beam when I wanted to do a dissertation in acoustics at Chicago.

However, the papers that I did submit for publication or for presentation at meetings of the American Physical Society were printed in the Physical Review. But by 1924 or 1925, shortly after I came here, there was very definitely a feeling among acoustical physicists that--well, we are not too welcome at the American Physical Society meetings anymore. We felt that we were really a second-rate citizenship participating in their meetings.

At about that time a young man by the name of Wallace Waterfall, whose name you know, and who has been the secretary of the Acoustical Society of America from its beginning in 1929 until the present time--and he is still a vigorous man and will likely be secretary as long as he wants to be, or as long as he lives maybe--became much concerned about the development of architectural acoustics. Waterfall had a master's degree in physics under Floyd R. Watson, the father of our Norman Watson here, at the University of Illinois. And his relationship to Watson was very similar to mine to Harvey Fletcher, who had been my mentor. Watson and Waterfall had talked these things over at length, and felt it was time to start our own show. Watson's work had been exclusively--his research work--in the field of architectural acoustics, and he was consulting in this field at that time and quite definitely was the successor to Wallace C. Sabine of Harvard, who, as I have told you, had fathered the birth of architectural acoustics as a branch of physics.

Waterfall was at that time secretary of the Celotex Company and in charge of their program in acoustical materials. They had recently acquired a patent for what is now known as Acousti-Celotex, which is a fiber tile made of sugar cane fibers, with holes drilled into it to make it highly sound absorptive. It was the most successful of the early acoustical materials, and the Celotex Company was exploiting this product, and Wallace Waterfall naturally was interested in architectural acoustics because of his training with F. R. Watson and also his responsibility with the Celotex Company. He realized that about from 1925 to 1928, in those years, that there was a need to get together the people who were doing research in architectural acoustics and the manufacturers of acoustical materials, and possibly set up some sort of a society.

Waterfall and Watson happened to be on the West Coast here in the summer--it was late August or early September of 1928. I don't remember the exact date. Waterfall thinks it was the first week of September. I think it was the last week of August. But at any rate they were here at about that time and discussed with me the necessity of getting these various people interested in architectural acoustics to form some sort of organization that would foster both research and the exploitation of acoustical materials in the treatment of rooms. And accordingly I invited them to have dinner with me at what was then known as the Gables Beach Club. It was the second beach club to be formed along the Santa Monica waterfront down here. There had been the original, simply called The Beach Club, to which Isaac Jones and I repaired weekly, you will remember, to do our writing on problems of the pathology and especially the physics of impaired hearing.

Isaac Jones had told me about the opportunity to become a member of this Gables Beach Club. By paying an initiation fee of $500, you were promised life membership without dues. And this looked like the bargain of bargains in beach clubs. I had begun some consulting work then for the motion picture industry, and it was the first time at which I was affluent enough to even think about membership in a beach club. If I could get life membership for $500, I thought really this was made to order for the Knudsens and their three young children. And it was a very fine club. It is a landmark that no longer is there. It was a landmark for a long time, and its name was an architectural reflection of the shape of the building. It had very prominent gables and old-timers may remember it had a slate-covered roof, and it was a very posh club in those days.

MINK: Was it located just below the outlet of Santa Monica Canyon?

KNUDSEN: That is approximately right, but south of the canyon.

MINK: Right near the Marion Davies home?

KNUDSEN: Yes. But several hundred feet south of the Marion Davies home and south of The Beach Club and of Isaac Jones' beach house where we had our writing sessions.

MINK: And it was torn down.

KNUDSEN: It was later torn down.

MINK: In the 1930s?

KNUDSEN: It may have survived into the 1940s. But it did not survive as a beach club much beyond this meeting to which I am going to refer now, which took place in August or September of 1928, when Watson and Waterfall were my guests at the club and where we discussed the necessity of having an acoustical society. Watson and I were of the opinion that it might be advisable to extend the scope of it beyond architectural acoustics, and we suggested that Wallace Waterfall should at least discuss this matter with Harvey Fletcher, who was in charge of acoustical research at the Bell Telephone Laboratories, and which Waterfall subsequently did.

Well, as a result of our agreement that there should be a society of some sort, and following the meeting that Waterfall had with Fletcher, Harvey Fletcher said that the Bell Laboratories would be glad to act as host to an organizing meeting, or at least a meeting to discuss the type of organization, if any, that should be established. This meeting at the Bell Telephone Research Laboratories was set up, and it happened to fall on my birthday, December 27, 1928. And there were some forty persons who had been invited to this meeting.

There had been some correspondence between the time of the earlier meeting at the Santa Monica Gables Beach Club. Waterfall and Watson and I had all written to acousticians that we knew, encouraging attendance at the meeting that Harvey Fletcher had arranged at the Bell Telephone Laboratories.

At this organization meeting, I think Fletcher, Watson, and I particularly favored the inclusion of all branches of acoustics in the formation of a society, rather than just an organization with interest in architectural acoustics. The meeting was made up of both physicists and engineers, and there was a long discussion as to how much emphasis should be given to physics and how much to engineering and whether we should be affiliated with one of the physics societies or with one of the engineering societies or go our own separate way.

Fletcher and I, I know, were very strongly disposed to stay close to the Physical Society. There had been a good precedent for this in the establishment of the Optical Society of America, which was really the first splinter organization from the parent organization of the American Physical Society. And this it seemed to us was a good pattern to follow. And I think the subsequent events would bear that out. As a matter of fact, there was agreement at this meeting that we should seek to be affiliated with the American Physical Society in the same way that the Optical Society was. There was not agreement as to how much emphasis should be placed on physics and how much on engineering. It was agreed that all phases of acoustics should be represented in this new society and that an appropriate journal would be established.

At that meeting officers were named for the society. Harvey Fletcher was named as president. I was named as vice-president. Wallace Waterfall was named secretary. And Fuller Stoddard, the inventor of the Ampico piano, and who had made a million dollars from royalties on this invention, was the first treasurer. And he could very well afford to be the treasurer of this young society. He was a gay person. I believe he had two Rolls Royces at that time. I know he had one, and later two. I often was a beneficiary of his friendship and his Rolls Royce. He would always meet me (or Mrs. Knudsen and me) at the Pennsylvania or Grand Central railroad station, and later at the airports, when we came to New York.

We became close friends. As a matter of fact, later, I believe I told you, we measured his hearing here, and we found that he had a form of diplacusis for which in one ear the pitch didn't change at all as we changed the frequency over a range of at least an octave, which as usual was in the high frequency range. Well, Fuller Stoddard was a <u>bon ton</u> fellow in spite of his gross impairment of hearing. I believe I told you that one of our assistants here made one of our vacuum tube hearing aids for him which he carried around in a large woman's purse. It was a box that was about, oh, 10" x 3" x 8", something like that, and it weighed about 9 pounds. Fuller carried it with him wherever he would go. You always saw it with him at meetings of the Acoustical Society; it had an extension microphone on it, and although Stoddard would always sit in the front row, he would place the microphone up by the rostrum and this was his means of hearing well during these early meetings of the Acoustical Society. He was, as I say, a very pleasant person, with a delightful sense of humor. He was a rock-ribbed Republican and one of the most--is it appropriate to digress into this?

MINK: Sure!

KNUDSEN: One of the most virulent haters, I guess, of Franklin D. Roosevelt, and later of Eleanor Roosevelt. And an interesting incident happened. He had a lovely apartment on Riverside Drive in New York, and near there Eleanor Roosevelt had an accident.

MINK: An automobile accident?

KNUDSEN: An automobile accident--the report of this accident was that she had gone to sleep at the wheel.

And so Fuller used this as an occasion to write a letter to Eleanor Roosevelt, in which he said, "Dear Mrs. Roosevelt: You could make a very great contribution to safety in driving in the United States if you would simply imitate Lafayette and have a plaque erected at the place of your accident, which simply says, 'Eleanor Roosevelt slept here.'" [laughter] Fuller did not get a reply to this letter. But he doesn't give up easily, and so he sent a second letter; and following Mrs. Roosevelt's receipt of the second letter he did get an acknowledgement from her secretary which simply said, "Mrs. Roosevelt sees nothing humorous about your correspondence concerning her accident on Riverside Drive." Well, this incident is typical of Fuller Stoddard. He would always use any excuse to really have fun, and he intended to make fun out of this letter.

Well, I am getting a little off the trail, but this organization meeting took place, as I say, on December 27, 1928, and it was arranged that there should be the first meeting of the Acoustical Society of America in the following May. And that meeting was held, again at the invitation of Harvey Fletcher, who was then our president, at the Bell Telephone Laboratories in New York. The invitations, however, came officially from Dr. H. D. Arnold who was then director of research and vice-president of the Bell Telephone Laboratories.

So the first meeting of the Acoustical Society was held at the Bell Telephone Laboratories May 9-11, 1929. The first session was an evening session on May 9, held at the Arnold Lecture Hall at the Bell Laboratories. It was one of the earliest lecture halls really to be acoustically designed, and it became a prototype of good acoustical designing of lecture halls.

MINK: Who had done the work?

KNUDSEN: A Bell Laboratory man by the name of [Joseph P.] Maxfield, who not only made important contributions to architectural acoustics, but who really was the pioneer scientist-engineer in the conversion from mechanical to electrical phonographs. The electrical phonograph was made possible by the developments of electronics and the high quality condenser microphone, which had been invented by another Bell Laboratories man, [Edward] C. Wente. And Maxfield was very prominent in this particular field. [Harold] D. Arnold gave the first address on this evening session, which was a joint session with the Society of Motion Picture Engineers. As you will recall, sound was just coming into motion pictures about that time, 1928 and 1929, and I will discuss later my role in the design of the first sound stages.

MINK: May I ask, did you correspond with the gentleman who did the work on the Arnold Lecture Hall?

KNUDSEN: Yes, I knew him very well.

MINK: Did you correspond in regard to the design of the acoustics for that lecture hall?

KNUDSEN: Not specifically on that hall, but we had talked about it at earlier meetings. I visited the Bell Laboratories on almost every visit to New York; and I made many visits there before this 1928 meeting, so I was acquainted with Maxfield, and on several occasions we had discussed architectural acoustics. I had been an engineer there, you may recall, nearly two years. Well, this lecture by Dr. Arnold was the first official meeting of our new society. The title of his lecture was, "Acoustical Facsimile;" acoustical facsimile, as you might know, meant how closely can you reproduce sound so that you will not recognize any difference between the facsimile and the original? And the Bell Laboratories later, largely through the efforts of Harvey Fletcher and William Snow, developed stereophonic sound. That turned out so good, as an example of acoustical facsimile, that a meeting of the Acoustical Society that met jointly with the Eastman School of Music at Rochester, New York, was devoted to a very interesting demonstration. It was one of the earlier ones--the second prominent demonstration of stereophonic reproduction of sound. The high quality reproduction of sound was so good that when the audience listened to a segment of the Tchaikovsky string quartet, the andante movement of the Andante Cantabile, they simply could not recognize the difference between the original and the facsimile. For the original, the Eastman string quartet played behind a screen which was opaque optically so you couldn't see them, but it was transparent acoustically--<u>transsoundent</u>, we should say. The facsimile was the Fletcher-Snow stereophonic reproduction. The audience (made up of distinguished musicians and acousticians) was simply asked to raise their hands if the first playing was the original or the facsimile. And the audience divided just about equally. Half of them thought the original was the first playing and the other half thought the second was the original. And that satisfied Harvey Fletcher, who conducted this experiment, that they really had a facsimile reproduction of the string quartet.

MINK: You were there?

KNUDSEN: Oh, yes, I was there.

MINK: And which one did you vote for? Did you vote right or wrong?

KNUDSEN: I voted wrong. [laughter]

MINK: What did they do? Did they then raise the curtain and present the orchestra?

KNUDSEN: Yes. It was a string quartet--just a string quartet for the Andante Cantabile.

Well, the first session, then, of the Acoustical

Society alone was on the morning of May 10, and I mention that because three papers were presented at this first meeting. It was a symposium on methods of measuring absorption coefficients of materials that are used for the control of noise and reverberation in rooms. The first paper, delivered by Paul Sabine, was entitled "The Measurement of Sound Absorption Coefficients by the Reverberation Method." Paul Sabine was the nephew of Wallace C. Sabine and was the director of the Riverbank Laboratories. I believe I have referred to that already. The second paper was entitled "The Measurement of Coefficients of Sound Absorption by the Intensity Method and by the Oscillograph Method," by V. O. Knudsen. And the third paper was a tube method of measuring sound absorption coefficients, by E. C. Wente, the inventor of the electrostatic microphone and many other devices that played a very important role in the development of electro-acoustics. These were the three papers presented at that first meeting.

I have not referred to my paper in our previous discussions, but the absorption of sound by the intensity method was conducted in a laboratory (reverberation room) that was built for me on Central Avenue [Los Angeles] in the manufacturing district. It was built by what was then known as the Cal-Acoustic Plaster Company, affiliated with a plastering contractor by the name of Simpson Brothers. And this was a room about 16' x 17' x 14' high, a solid concrete structure. But it wasn't a room inside of a room as our reverberation rooms are here, and so it was necessary for us to work in the middle of the night. From midnight until about 4 a.m. was the favored time for making these sound measurements.

I believe the work we were doing here at that time was the first work in utilizing vacuum tube circuits and the new developments of electronics in the measurement of matters concerned with architectural acoustics, such as reverberation and sound absorption. And the intensity method for measuring sound absorption was developed in this laboratory. We had four high-quality telephone receivers that had been developed by the Western Electric Research Laboratories, as it was called at that time before the presently named Bell Laboratories had been officially adopted, and these telephone receivers were rotated by a phonograph motor that we used which was very quiet. These four, which served as microphone receivers, mounted on the ends of two thin strips of wood about 4' long, and crossed at right angles, were rotated, and we had them located at eight different positions in the room to measure the average intensity. The rotation of the receivers in a circle of a 4' diameter was necessary in order to average out space variations of the sound intensity in the room. And the method was sensitive enough that I could detect the presence of my felt hat in the room--that is, the sound intensity dropped just by a perceptible amount by this technique when my felt hat was in there. And we had made measurements of the absorption of Acousti-Celotex, type C-4, which had been measured at other laboratories. And this was one of the investigations I reported at that first meeting.

The other method for measuring sound absorption was by making an oscillographic record in a small room that we had on the Vermont Avenue campus. This was before we came to this Westwood building, you recall. And for that investigation we used oscillographic methods of portraying the decay of sound in a room. It was a converted men's room that we had in there, and therefore it was an inside room with all the surfaces being of hard plaster, and it made a fairly good reverberation room; it was our campus reverberation room for our studies in architectural acoustics until we moved to this campus. This paper which I presented at the first session of the Acoustical Society I thought may be of some interest.

Well, the early development of acoustics by the Acoustical Society, I think, can be well illustrated by calling attention to two things. In the beginning, as I mentioned, there was quite a tug-of-war between those who felt it should be engineering-oriented and those who felt it should be physics-oriented. We had an all-night session at an early meeting, possibly a year or two after this meeting of May, 1929--in 1930 or possibly it was 1931. But it was a meeting of the Executive Council of the Acoustical Society. We were dealing with a policy of just what direction we should move in respect to physics and engineering. And I know it was 5 a.m. the next morning when we broke up.

I think the physicists more or less won out in the argument and this I think is somewhat borne out by the naming, say, of the presidents during the first ten years or so. The first president was Harvey Fletcher, affectionately referred to as Uncle Harvey at most of the Acoustical Society meetings. The second was Dayton C. Miller, who was Distinguished Professor of Physics at Case School of Applied Science, Cleveland, who had not only the biggest collection of flutes in the world but bigger than all other collections combined, and the same was true of his literature on the flute and other woodwind instruments. He played the flute himself, and he made a number of flutes, one of solid silver, one of solid gold, and finally one of solid platinum.

MINK: Which sounded the best?

KNUDSEN: There was almost no difference. And there would be a big waste of platinum to make a platinum flute, although it was the most dense of them all. And a good, heavy, noncorrosive metal was a little better than wood. There was no discernible distinction really between silver, gold, and platinum so far as Miller's experiments were concerned. His entire collection of flutes and his entire library was comprised of something like 1,400 volumes dealing with the flute and the same number, I think 1,500 or 1,600, of flutes. Both the library of Dayton C. Miller and his collection of flutes are now in the Smithsonian Institution. He willed them to the Smithsonian. He died in 1941. In his later years, he repeated several times Michelson's famous experiments on the "ether drift," which was basic to Einstein's relativity theory, and which Miller's experiments, he believed, did not confirm Michelson's finding. But he was really the grand old man in acoustics at the time the society was organized, and he was revered as the society's second president, 1931-33.

The third president was V. O. Knudsen, 1933 to 1935. These three are not only physicists; they were, or had been, professors of physics as well. Fletcher had been professor at BYU before he went to the Bell Telephone Laboratories. Next, Paul Sabine, who I have mentioned, was the nephew of Wallace C. Sabine, who was also a physicist and director of this Riverbank Laboratory, which became the most important laboratory for measurement of the sound absorption properties of acoustical materials and also for the sound insulation through various kinds of doors and windows and walls and floor and ceiling structures, and so on, and is today the leading laboratory in that field.

The fifth president, which was 1937-1939, was Professor Frederick A. Saunders, who was then chairman of the Department of Physics at Harvard University and is known principally for two things. One, there is the Saunders series in spectroscopy. His early work was in spectroscopy. His later work was in acoustics, and for many years he was the leader in this country in investigating the acoustics of the violin. And you may recall that Scientific American of about five years ago, had an article dealing with the acoustics of the violin, which reviewed the research of Saunders. This work on the violin has been continued by his assistant, a Miss Huggins. And they have a society now that is known as the Catgut Society. The violin family consists of five sizes of stringed instruments, you know, for which catgut strings are used. And only last June at the Boston meeting of the Acoustical Society, a demonstration was made of the instruments that have been developed as a result of this research done by Saunders and now this Catgut Society.

The program was dedicated to F. A. Saunders, and they performed a concert at Harvard University in which they played on these facsimile instruments that were made as nearly as possible after the famous Stradivari and Amati and the other great Italian instruments of the sixteenth and seventeenth centuries. And Saunders had demonstrated that his facsimiles of these old famous instruments were so good that even professional violinists and cellists and so on, when they listened to them in the dark, didn't know which was the original and which was the facsimile. So here again is another instance in which acoustics had really developed the string instruments to the point where these facsimilies really compare favorably with the famous instruments of the sixteenth and seventeenth century Italian masters. They cost between $500 and $1,000 each, as compared with $25,000 or $50,000 or more for these famous old instruments.

MINK: You've spoken about the first presidents. Was there a concern during this early period with how well the organization was going, whether they felt that they were receiving opposition from the American Physical Society? Was there a concern with how well they thought the first meeting went, for example?

KNUDSEN: I know there was great satisfaction among the members of the Acoustical Society; furthermore the Society was welcomed into what was then soon organized as the American Institute of Physics. Three of the members were, of course, the parent organization: the American Physical Society, the Optical Society of America, the Acoustical Society of America, and about this same time there was a Society of Rheologists. Rheology is the science of flow. This was when plastics and things of that sort were coming into prominence. And this was more physics-based at that time than chemistry-based, and it was for a long time a part of the American Institute of Physics, which became largely a publishing holding company for the members of the physics fraternity, like the Acoustical Society, the Optical Society, and the Physical Society, and so on.

MINK: Maybe I want to phrase this question in a different way. Was there concern, for example, that the Acoustical Society was looked down upon and that they needed, you know, to raise their standards so that they would be regarded as more important?

KNUDSEN: Well, I think very definitely the affiliation with the Physical Society had all of the advantages that we had formerly had as members of the American Physical Society. It gave a place where we didn't feel that we were second-class citizens. And I'm sure there was a wholesome respect by the parent organization for the Acoustical Society. We added both numbers and strength to the Institute, and our society gained the respect of both the Institute and the Physical Society. Our society was made up of a mingling of many disciplines besides acoustical engineers and acoustical physicists; there were psychologists; there were musicians, otologists, phonoticians, and you name almost anything associated with acoustics, and there was representation there. So it made an interesting society; the meetings were certainly well attended, and the rapport among the members was splendid, delightful, and the envy of some other closely related societies.

The society prospered and grew very well. I think that I should enter the name of the sixth president, Professor F. R. Watson of the University of Illinois, of whom we have spoken. So these first six presidents of our society were all physicists, and five of them were--or had been--professors of physics at leading institutions throughout the country. From then on I should add there have been presidents from the other acoustical disciplines--in engineering, psychology, etc. But coming back to your question, there was definitely opposition, or at least dissatisfaction, on the part of engineers--those who were very strongly engineering-oriented. Another society was formed, as a matter of fact, which is the Audio Engineering Society, and it is presently prospering.

MINK: I see. These are what we would call sound engineers?

KNUDSEN: Yes. They are primarily sound engineers and from several arts and industries.

MINK: Such as we have in radio, television, or in the movies.

KNUDSEN: Yes, yes. And their chief concern, their chief interest, has been recording of sound, the making of phonograph records, the recording of sound in radio and television, and the acoustical problems associated with this recording and reproduction of sound. [tape off]

The Acoustical Society, as I have indicated, was, during its early years, physics-oriented--the first six presidents were all physicists. Since that time I think the presidency, for several years pretty much alternated between a physicist one year and an acoustical engineer the next, and more recently the other acoustical disciplines have been represented. It has been moving much in that direction, although there has been a preponderance of physicists even since the first six physicists, and among them, four had their graduate work in acoustics at UCLA, and two of the four were professors of physics here--Robert Leonard and Isadore Rudnick; the other two were Richard H. Bolt, chairman of Bolt, Beranek and Newman, and Cyril Harris, professor of engineering at Columbia University.

There were a number of people who felt that the Journal of the Acoustical Society of America especially was getting a little too much oriented toward physics, and that it wasn't paying enough attention to some of the practical problems that were associated especially with the development of stereophonic recording, hi-fi developments; and as a result of that a new society was formed that was definitely engineering-oriented, called the Audio Engineering Society. And that is a successful society today. It is not as large as the Acoustical Society, but it is a very fine organization. They hold annual meetings in the fall in New York and spring meetings in Los Angeles, and they are certainly well-attended.

I attended a meeting in New York two years ago last October, and I would estimate that there were 300 present on that occasion. I was given the Boggs Medal at that meeting for my work in architectural acoustics. Leopold Stokowski was one of the principal speakers at the evening program and spoke about his early interests in high-quality recording. He was closely affiliated with Harvey Fletcher and Bill Snow, as I have mentioned, in the first transmission of stereophonic sound from Philadelphia--from the Academy of

Science, which is their official concert hall, of course, to Constitution Hall in Washington.

On that occasion I happened to be in Washington, D.C., and was with Harvey Fletcher and Bill Snow in the afternoon when they made their final adjustments, and we were talking then about the importance of absorption of sound in the air, and that it might change at night owing to the change in humidity. And as a matter of fact, the presence of the audience did increase the relative humidity, and it was necessary to make adjustments in their high-fi equipment to compensate for this change, which changed the absorption and therefore the attenuation of the high-frequency components of their music.

Probably the development of the Acoustical Society can best be summarized at this point by referring to the twenty-fifth anniversary of it. And I refer you, for the record here, to volume XXVI, 1954, and on pages 872 and 873 you will see the program cover.

MINK: It doesn't look too acoustical to me.

KNUDSEN: There are twenty-five Rockettes from the Music Hall, and as a matter of fact at this twenty-fifth anniversary one of our meetings was held in the Music Hall in Rockefeller Center, and we were entertained by others including the Rockettes. And, further, one of the characteristics of the Acoustical Society has been really a very fine camaraderie, and a sense of humor especially at their evening meetings. At the evening meeting that comes up on April 20 of this year (1967) I am to receive the Gold Medal of the Acoustical Society. The citation will be read by Dr. Fletcher, but I know that there will be some horseplay in this presentation. It won't be all just serious; it will be that.

For example, at an earlier meeting of the Acoustical Society I was featured and was required to measure the sound absorption of Sally Rand with and without her fan. And a professional entertainer had been employed to dress as Sally Rand usually did [laughter] in which she had very little on except the fan. I hasten to add that she was clothed in appropriate tights for the occasion.

MINK: A G-string of some sort.

KNUDSEN: It was a bikini in those days. This was probably fifteen years ago. And the stage was set for the way Wallace C. Sabine made his measurements in which he used a wooden cabinet that covered him. And only his head projected through a hole in this cabinet so that his clothing and his body would not affect the absorption measurements he was making in his reverberation room. And he used this in the Riverbank Laboratory. So they made a cabinet very similar to the one Wallace C. Sabine was in, and I was required to sit in there with my controls outside the cabinet. That's all I had outside besides my bald head.

Then Sally Rand came up and sat on a chair in front of me, and I was to measure her sound absorption. We went through the simulations of, doing it first with the fan in place and then with the fan removed. This is quite typical of the kind of bawdy entertainment we sometimes mix in with our serious meetings of the Acoustical Society. I thought it might add just a little relief of humor here to the serious meetings we are going to talk about. This twenty-fifth anniversary I think does give an opportunity to indicate how the Acoustical Society has grown over the years. And besides the Rockettes we had some serious things.

I think it's worthwhile probably to review a few things here. The twenty-fifth anniversary celebration was Wednesday through Saturday, June 23-26, 1954. And the first paragraph of this report of the meeting deals with a quarter century of acoustical science, and it mentions here that, "Since we are committed to the decimal system of numeration while at the same time holding to the scale factor of 2"--the "2" is the octave, you see, doubling the frequency--"and since our unit for the measurement of long times is the period of the earth around its orbit, therefore, the Council of the Acoustical Society has declared this particular moment, a quarter century since our founding, as a time for high celebration of the success of the Acoustical Society in advancing our science and as a moment for assessing past progress and orienting future effort."

At this program, in contrast to the two short sessions we had on May 10, 1929, there were three simultaneous sessions. There were 527 registrants at the meetings. And the meetings, as you see, continued from June 23-26. And the sessions were all commemorative ones at which there were invited papers from European contributors as well as from here. The names of these sessions all bore the names of distinguished scientists most of whom were dead. They were all dead except one [laughter] in this list, here.

I think it would give an interesting review of some of the important people that have contributed to acoustics throughout the past two or three centuries. The first session was the Dayton C. Miller session on musical acoustics, who was the author of Science of Musical Sounds, which has been a classic textbook; and his work on the flutes I have referred to earlier today. The second was the Paul Langevin session on ultra acoustics. Langevin was the French physicist who in World War I first invented what was referred to later as the "Asdic" by the British, which was the beginning of sonar. That is, Langevin invented a transducer for generating an ultrasonic vibration in the water, which would send out a beam of sound and its echo from such a submerged object as a submarine would be recorded. Langevin was working on this project just as World War I ended, in 1918.

MINK: Did he ever visit this campus?

KNUDSEN: No, he did not. He visited one meeting of the Acoustical Society in New York. I met him there, and I met him in Paris. He is an important name in acoustics, and particularly in the inventing of this first radiater of sound, you see, of ultrasonics in water, which could be used for recording the echo of a submersed vessel or even of a surface vessel. The next session was the Thomas A. Edison session on transducers--the big transducer being, of course, the phonograph. There was the Marcus Vitruvius session on architectural acoustics. Vitruvius' early writings on architecture dealt with the acoustics of the Greek and Roman theaters, and called attention to the use of large vases that were used as resonators that were supposed to reinforce musical components that you would want enhanced.

As a matter of fact, it is a question of whether they actually enhanced or absorbed these components. A resonator similar to that with some absorptive material in the mouth of the resonator is used today primarily to absorb sound. They had been used in the history of the control of sound in churches and other buildings as far back as the tenth century. That is, the Scandinavian countries have found early churches in which similar resonators were used, probably included because advocated by Vitruvius for enhancing sound; but some acousticians believed they were actually used for absorbing sound in reverberant churches, a function it is known they do perform. And this, whether or not these early church builders knew the true function of resonators, is the earliest record of the use of absorbents in buildings. Several other countries besides the Scandinavian ones exhibit the use of these vases in the walls exposed to the room, possibly for the purpose of reducing reverberation.

There was the Hiram Percy Maxim session on noise, based on his Maxim gun silencer, who was thus honored on this occasion. There was the Wallace C. Sabine session (the second one) on architectural acoustics, the pioneer worker in that field. The only person alive honored at the meeting was Harvey C. Hayes by the second session on underwater sound and wave propagation. Harvey Hayes was for many years in charge of the program on underwater sound at the Naval Research Laboratory and had carried on in that capacity very much between

World War I and World War II. And there was the Hermann [Ludwig] von Helmholtz session on physiological acoustics, distinguished scientist and author of The Sensation of Tone, the classical volume, as you know, in that field. It has long been the great work on the hearing, especially because of his epochal investigations of the resonance properties of the ear. Then there was Georg S[imon] Ohm session on hearing. Now most people identify Ohm, of course, with our unit of resistance in his electrical work, but he also worked on the ear as a resonator. And so there was a second session on hearing, honoring Ohm.

There was the Karl Rudolf König session on instrumentation, a prolific acoustical investigator of instruments, the tuning fork, etc. Everyone who has had elementary physics at UCLA, and most other laboratories at colleges and universities have used König tuning forks, and know what the König rod is. It is used to set up stationary waves in the Kundt's tube. You have Lycopodium powder in the bottom of the tube and you stroke this rod with a cloth that has been roughened, say, with resin, and this makes the rod vibrate; and the rod sets up stationary waves in the glass tube. You get pileups of the Lycopodium powder, you see, at half wave-length intervals along the tube.

Then there was the Lord Rayleigh session on waves and vibrations. Of course, his two volumes are the bible of all acousticians even today. The [Jean Baptiste] Fourier session on noise was held. There have been some questions as to whether Fourier should be so honored. He is largely responsible for what is called Fourier analysis, for analyzing any kind of vibration into its simple harmonic components, and whether it should be identified with noise which is not very harmonic was controversial, but the controversial people were overruled, and the session was called the Fourier session on noise. Then came the Pierre Curie session on ultrasonics. There again most people would think that all of his work was in radium and the radioactive elements, dating back, you know, from about the turn of the century, or a year or two before, until the name is still a very well-known name in this particular field. But during World War I again, Curie was active in the underwater sound detection of submarines. Then finally there was the Alexander Graham Bell session on speech. I think it doesn't require any explanation. But these were the names, and they are among the most illustrious contributors to the science of acoustics.

MINK: Did you have anything to do with planning this meeting?

KNUDSEN: No. I was of course present, and I was referred to [laughter] both humorously and seriously.

I was made an honorary member of the society on this occasion. On page 885 of this same volume there is a facsimile of the certificate I received at this twenty-fifth anniversary celebration. It reads: "Vern O. Knudsen was proclaimed an honorary member by unanimous accord of the executive council." Then the citation brief says, "As a scientist, he has achieved eminence in acoustics. As a member, he has made manifold contributions to the welfare of the society. As a president, he has guided the society wisely during its formative period. As a man, he has endeared himself to his colleagues." At New York City, June 25, 1954.

MINK: Well, the reason I asked was because you said in one instance there was a question as to whether a session should so be named after one individual there.

KNUDSEN: Well, there were two sessions on underwater sound, and Langevin rightly should be the first one; but if the second one was named, it would be Harvey Hayes because of his administration of this work at the Naval Research Laboratory.

MINK: I was wondering whose idea it was to name these various sessions after the individuals.

KNUDSEN: Well, it was the program committee, and I was not on the program committee. [laughter] The program committee here is named. I don't know that it's important, but [Winston] Kock was chairman. I actually don't know who he is. And Richard Bolt did sit with the committee. Richard Bolt was the first to get a PhD degree in acoustics at UCLA. And he is now chairman of Bolt, Beranek, and Newman, which is the largest consulting acoustical outfit I guess in the world. They are located just out of Cambridge, Massachusetts.

There was another incident at this meeting that I think is worth reviewing. No, there are two incidents. On page 882 there is a photograph of some 36 of the 40 members who attended this organization meeting at the Bell Telephone Laboratories, December 27, 1928. And I might call attention to a few of the names here. I think it's interesting. On the front row starting at the left you will find F. A. Saunders of Harvard; then R. V. Parsons, who was in charge of research at the Johns-Manville Company; Dayton C. Miller of the Case School of Applied Science; Wallace Waterfall, the first secretary; Vern Knudsen, the first vice-president of the society; Harvey Fletcher, its first president; Fuller Stoddard, the treasurer; J. P. Maxfield, whom I have referred to for his important role in developing electronic recording and reproducing of sound; F. R. Watson, the beloved F. R. Watson; Floyd K. Richtmyer from Cornell University who was president of the Optical

Society and was there to guide us, you know, on how the Optical Society was formulated; and Professor C. R. Anderson from the University of Toronto. Well, these are the names of the persons in the front row and they have been important contributors and had much to do with the guiding of the Acoustical Society in its early years. And they were given this place of honor on the twenty-fifth anniversary. Dayton C. Miller is shown displaying his gold flute. I am sure you have seen his picture elsewhere. He is a good flutist himself.

MINK: On page 883.

KNUDSEN: Yes. Dayton C. Miller's photograph is there. He's a very splendid person besides being a very competent man in the field of musical acoustics, a distinguished professor at Case, a member of the National Academy of Sciences, and probably the best advisor we had in this group of 40 who met with us at the Bell Laboratories on the occasion of organizing the society. He also was responsible for the acoustical design of Severance Hall at Cleveland, which is recognized as one of the good acoustical concert halls in the United States.

At the evening entertainment (see page 886 of this same volume) you will see a photograph of Leo Delsasso at the extreme right and Vern Knudsen at the extreme left, and three other members of the society, all of whom were deans. And I think this is interesting, incidentally, because it's rather extraordinary how many members and fellows and officers of the Acoustical Society later became deans or vice-presidents, presidents, or chancellors of universities throughout the country. Well, Leo Delsasso's function at this meeting was, as the title says here, "Reversing the usual action, the deans are called on the carpet for an accounting by Dr. Leo Delsasso."

The photograph on this page shows, besides Delsasso and myself, from the left, Robert Lindsay, who was Dean of the Graduate School at Brown University; Harvey Fletcher, who was then Dean of the Science and Engineering School at Brigham Young University; and George Pegram, who throughout his lifetime, almost, was treasurer of the American Physical Society and was advisor to Dwight Eisenhower at the time Eisenhower was president of Columbia University, had served as acting president of Columbia University, and was Dean of the Graduate Division throughout much of his career at Columbia.

MINK: Did you know him well?

KNUDSEN: Yes. He visited here only a year or so before his death, and we had a good visit together. Delsasso and I showed him our experimental facilities in acoustics.

He was much interested in other things; he played an important role, you may know, in the development of atomic energy and the atomic bomb (Manhattan Project). He was very close to these first experiments, you see, which were guided very much by the University of Chicago and Columbia University. And so Arthur Compton and George Pegram were very close to that development during World War II. But he was always interested in acoustics as well, and he attended most of our early meetings. Is it worthwhile referring to some of these things?

MINK: Yes, I think so. I am interested to hear what you were called on the carpet about.

KNUDSEN: Delsasso said, "Well, I'm sorry to be the one person to bring a somber and sad note to this otherwise festive occasion. I hope that when the time capsule is opened"--the time capsule was to be opened on the occasion of the one hundredth anniversary, and all of the proceedings of this twenty-fifth anniversary were deposited in a safe in a bank in New York and are not to be opened until the one hundredth anniversary, seventy-five years from then--"they will forgive me. I do not feel duty-bound, however, to call attention to one of the occupational hazards that goes with membership in the Acoustical Society. This for the benefit of the young people who are here. I sat through the meetings today listening to some of these young expert folk. They are expert because they know about Hankel functions and a variety of other subjects of that sort. But in addition to that they are very able people and are likely to be called into administration even before they know it. I should like to point out that this is a real danger.

"And just to prove my case I want to ask some of the former members of the society, or some of the present members of the society, who have fallen by the wayside, to step up and say a few words as to how this all happened to them. I would like to read the list. Some of them are here and some are absent. The first one is Dr. Knudsen, dean of the Graduate School at UCLA. Will you please come forward?" (Which I did, of course.) "Dr. Robert B. Lindsay, dean of the Graduate School of Brown University--will you please come forward?" (Applause) "Dr. Harold K. Schilling," (Dr. Schilling was absent.) "the dean of the Graduate School of Penn State. I believe Dr. Schilling is not here." (He was not.) "Dr. [Harvey] Fletcher, dean of Science and Engineering at Brigham Young University." (Applause) "Dr. [Eric] Walker, dean of the College of Engineering, Penn State." (He later became, and now is, president of Penn State.) "Dr. [Charles Paul] Boner, dean of the College of Arts and Sciences, University of Texas." (He later became vice-president of the University of Texas.) "Dr. Elmer Hutchisson, dean, Case Institute of Technology." (He later became director of the American Institute of Physics.) "These gentlemen unfortunately could not be here." (That is, the four just named.) "Perhaps they no longer exist. Dr. [George] Pegram, dean of the Faculty of Applied Science from 1917 to 1930, then dean of the Graduate Faculty from 1930 to 1949, Columbia, President Emeritus and special advisor to the president." (And that was to Eisenhower, of course.) "All these folks have fallen by the wayside and I think, Mr. President, you should ask them for an accounting." And the editor of the journal broke in here and said, "Before Mr. Delsasso gets too far away, I would like to mention that he sometimes signs himself 'assistant to the dean, UCLA.'" As a matter of fact, Leo Delsasso was associate dean of the Graduate Division in charge of contract research at that time. Shall I give some of my reply here?

MINK: Oh, yes.

KNUDSEN: This is my accounting. "There is nothing extraordinary about physicists and more especially acousticians becoming deans, as this spectacle before you [laughter] and the other four gentlemen who as Delsasso said proved the point about the hazard of becoming dean. I know poor [Charles] Paul Boner, is at home boning over his budget, and I presume the others are doing the same." [More laughter] "The real problem, though--I think some of the younger members of the society might very well attack this problem--is the irreversibility that this process seems to possess. If the recent trend continues there may be a sorry celebration seventy-five years from now. We up here will be the majority and you down there will be the minority." [More laughter] "To dean or not to dean really is a serious question, that each of us tries to answer when we are called to this post. Each makes the resolve, 'I shall be a professor first and a dean second.' Soon he becomes a dean first and a professor second. Ultimately, he becomes just a dean. But the second law of thermodynamics will not be denied. I think this process <u>is</u> irreversible. There is nothing extraordinary at all about the movement in the other direction. Deans may become provosts or vice-presidents or advisors to presidents or chancellor or even presidents. But the process doesn't move the other way. One of our colleagues, we will all bear witness, tried the reversible process and he is now dead. The younger members, I say to you, beware all ye who enter here."

MINK: Was this spontaneous?

KNUDSEN: Well, yes, but I had essentially memorized it from former uses.

MINK: You said what came to your mind?

KNUDSEN: Yes. I said what had come to my mind.

MINK: You didn't have anything more to say?

KNUDSEN: That's all. That's the end of my remark. Now the others made their remarks here.

MINK: Now would you say that if you had been allowed to go on, would you have added anything to this?

KNUDSEN: Well, of course, I was just a dean at that time and later became a [laughter] vice-chancellor of Academic Affairs and then chancellor, so in that sense what I said was somewhat prophetic, and others of these I have mentioned have become presidents. I remember I spoke to the Navy's underwater sound group that meets annually. They met near San Francisco one year out at Del Monte. I was the evening speaker on that occasion, and I mentioned the number of research workers during World War II who were in the underwater sound program--that is, the anti-submarine warfare program--who had later become presidents or chancellors. And this was after my retirement, after I had served as chancellor, and so I was introduced as chancellor emeritus of UCLA. Well, there was Gaylord Harnwell. Harnwell was then and is now president of the University of Pennsylvania; he succeeded me as director of the research at the Underwater Sound and Radio Laboratory, which is now the U.S. Navy Electronics Laboratory. at San Diego. Eric Walker, who was in charge of one of the programs at Harvard in the underwater sound program, is president of Penn State, and two others, I know, were presidents at that time. Their names elude me now, but there were five members of the active research staff on underwater sound during World War II who later became presidents of respectable, major universities throughout the country.

SECTION XII

KNUDSEN: I think it is appropriate to say a few words about Professor Floyd R. Watson. I have said quite a lot about Harvey Fletcher, who has been my mentor very much through life. F. R. Watson has been a mentor to quite a few men who later specialized in acoustics; they had their start really from F. R. Watson. Among them were his own two sons, Norman, who is professor of physics here, and Robert Watson, who was for a long time at the University of Texas, Austin, and now is in charge of an important division of one of the naval laboratories in Washington. I think it's naval ordnance, not naval research. Both of these sons of F. R. Watson came to UCLA where they specialized in acoustics.

I first met F. R. Watson at a meeting of the American Physical Society at the University of Chicago. It must have been in the fall of 1921. It was an occasion on which F. R. Watson presented a paper dealing with the acoustics of the large auditorium on the University of Illinois campus at Urbana, and Watson had made a very careful study of this room. The outcome in acoustics was unfortunately very bad. It was mostly circular in plan with a huge domed ceiling, and the place was beset with all kinds of echoes and sound-focusing effects and non-uniform distribution of sound and nearly all the faults that you can attribute to a bad shape, and besides that it was excessively reverberant.

Much of the important work that F. R. Watson did in architectural acoustics was in connection with his very thorough survey of the acoustics of this large auditorium on the campus of the University of Illinois. I think it had something like 5,000 seats. It was a major auditorium in the country at that time, and certainly it was an example, like Albert Hall was in London, of the wrong things to do in the acoustical design of an auditorium. He used special high-frequency sources of sound for tracing down the echoes and multiple reflections. He used a large parabolic reflector and traced the echoes--that is, starting on the podium or the stage there, he would direct the beam at surfaces that he suspected and would find out where they were first reflected and then reflected the second time.

Watson's book is titled Acoustics of Buildings. After the Collected Papers on Acoustics by Wallace C. Sabine, the next book on this subject to appear in the English language was this book by F. R. Watson. I have both the first and second editions of it on the shelves here. And it was a very useful book to architects, and much of it dealt with the acoustical problems and how he solved them in this large auditorium. He described how he made extensive use of an absorptive material, which usually in those days was hair-felt, sometimes coated with a porous and plastic colored "paint" that would prevent the material from popping off but yet would be porous enough so that the sound could penetrate it and be absorbed by the hair-felt behind it. This was used judiciously in those places that gave the most offensive reflections, which would be much of the domed ceiling and some of the cylindrical walls. And the acoustics of this room were greatly improved as a result of Watson's studies and recommended treatment. He reported his work on this hall at this Chicago meeting of the American Physical Society at which, for the first time I also presented a paper. And this was in November of 1921.

MINK: And you still would have been in graduate school at this point.

KNUDSEN: I was a graduate student at the University of Chicago, and after the meeting Watson introduced himself to me, and we discussed further the two papers we had presented. Mine was on my dissertation at that time, "The Sensibility of the Ear to Differences of Pitch and Loudness," only partly completed at that time, but he was much interested in what I was doing, and I, of course, was greatly interested in his work. I took him to my laboratory where I was working, which, incidentally, was the room, I think I said, in which Millikan had done his work on the oil drop experiment for the measurement of the elementary charge on the electron. Watson and I became very close colleagues and friends from that day on, and this friendship has continued throughout the years. He encouraged me very much at that time to go on in acoustics, and I counted his counsel very valuable, and it probably had a beneficial influence in my choosing acoustics as my profession.

As I have mentioned earlier, that he and I were present with Wallace Waterfall, who had felt that there was a need in bringing the manufacturing people and the professional people together, which led to the organization of the Acoustical Society. Without Watson and Waterfall's efforts I'm not sure when the Acoustical Society, if ever, would have been established. But the time was opportune and Waterfall and Watson worked together very closely in arranging for the kind of program we would have at the Bell Telephone Laboratories that I have described here.

Well, Watson and I saw a great deal of each other throughout the years. We exchanged reprints, of course, all the time. We kept each other informed of the researches we were doing. And I think I owe a debt to F. R. Watson, because in 1934, December, when I reported at a meeting of the American Association for the Advancement of Science on the absorption of sound in gases, it was Watson, I know, who was chairman of the committee that reviewed the papers presented in acoustics. He and Harvey Fletcher and some others recommended that my paper be considered by the committee that made the annual award of $1,000 for what was considered the most noteworthy contribution at the session. And this involved, of course, all the societies of the American Association for the Advancement of Science. At that time, the AAAS annual award probably was the most coveted prize in science in the United States.

Well, I did not suspect at all that any award would come to me for that paper. As a matter of fact, I didn't know that there was such a committee, but I learned later that Watson was chairman of the committee and that he had been very favorably impressed with the paper I had presented. He and Harvey Fletcher and a few others, I guess some of my buddies and friends, [laughter] old Acoustical Society colleagues, said, "Well, let's see if we can't have this paper considered by the committee."

I heard no more about that until two days after

I had completed some appointments in New York following the AAAS meeting which was at Pittsburgh. I boarded a train in New York and transferred at Chicago, as was usual, and when we were coming through Wyoming I was in the lounge car reading the headlines of a Denver paper and it said, "California Physicist Wins AAAS $1,000 Prize." Well, a young lady was sitting beside me. I was astounded. I didn't know who she was. But I said, "Look at this!" She said, "What's wrong; what's funny about that?" I said, "I'm the guy!" [laughter]

Well, R. A. Millikan was on the train at that time, and he was occupying a roomette not far from mine. I couldn't resist the temptation to take this down and show it to Millikan. And Millikan was the first person I knew then to see this headline, and I received his congratulations. But I mention this because I'm sure F. R. Watson's friendly relation with me had much to do with the selection of this paper among those that were presented in acoustics. I think this was the first and only paper on acoustics that has been so honored by the American Association for the Advancement of Science, so it meant a great deal to me at that time, and I know it meant a great deal to F. R. Watson. F. R. Watson, like Harvey Fletcher, really regarded me as almost a member of the family. We were always the very best of friends, and Watson, as I mentioned, later wanted his two sons to come to UCLA to study acoustics, and both of them did very fine work in that field. Floyd Watson is still active, as you probably know. He is [almost ninety-five] years old.*

MINK: I believe he is active. Did he continue to teach right up to his retirement at the University of Illinois?

KNUDSEN: Yes, he did. The retirement age, I believe, was sixty-five at that time, and he moved to Southern California immediately after retirement.

MINK: Because his children were here.

KNUDSEN: Because of his children, no doubt--and, you know, he had been a Californian, a student at the Los Angeles Normal School. And Norman Watson was then on the faculty here and Bob Watson, I think, was still a graduate student here.

MINK: Norman Watson came very soon after you did, to become a student here, did he not?

KNUDSEN: He came very soon after the Graduate Division was established, before we offered PhD work. He was one of the first two to receive a PhD. Kenneth Bailey in history was the other. Watson did some of his

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*Note added 12/18/70. Yesterday I received FRW's annual Christmas card, with its cordial greetings and a recent photograph of him looking as young and vigorous as he did ten years ago. Under the photo, written in his splendid and youthful penmanship, was F. R. Watson--age 99.

preliminary work at Berkeley and got his degree from Berkeley because we were not authorized to give the PhD degree at that time while he was here. Although he had all of his course work in graduate acoustics and his dissertation work was done here in this laboratory, the degree actually issued from Berkeley.

F. R. Watson is worthy of further notice. He was present here at the dedication of this building, Knudsen Hall, and so was Harvey Fletcher and Wallace Waterfall at the dedication ceremonies. Fletcher and Watson and Waterfall and I were asked to stand together and take a bow by the invitation of Chancellor Murphy who was presiding at that meeting. F. R. Watson always was the person to pay thanks (and to say it sincerely with flowers) to the host institution where we were holding meetings of the Acoustical Society. So long as Watson attended the meetings, you could always depend upon a resolution being prepared and submitted by Watson before we adjourned. And it was always a very fine acknowledgement of the courtesies and the pleasures and so on that we had received from the host institution. We would always say, "Watson is the man to say it with flowers." And when the meeting came to a close we always looked around to see if Watson was there, and we would motion to him to arise--yes, and he had his resolution already written out which he presented and it was always, of course, unanimously approved.

MINK: I take it then that he was a good speaker?

KNUDSEN: He was a good speaker, a very clear speaker, and a very clear writer. He was an excellent teacher in the sense that he knew how to make fairly complicated things understandable and intelligible to students; he inspired many of his students, as I have mentioned, to continue on in acoustics. He is now living in one of these leisure world centers, or centers for the retired persons (mostly university folk) at Claremont. His wife died a few years ago. They were very close companions during their many married years. I spent part of a Christmas season with them one year, a Christmas when I was going to St. Louis. It was customary then for those who had won the AAAS Prize to repeat their paper at the following meeting of the AAAS to a general assembly instead of just to the acousticians. And I had been in New York.

MINK: Did they repeat the same paper?

KNUDSEN: That was customary. I had a better script, and I had prepared some demonstrations to supplement the lecture. I had a longer time to present it, so it was embellished somewhat for the occasion. And I had spent a day and a night and most of the next day at the Watson home in Urbana. They are charming people who believe in "early to bed and early to rise . . . ."

I remember F. R. Watson was up at 5:30 in the morning, which is an early time to be up, I thought, for December. But he always did his work in the early part of the day and retired early.

MINK: Sounds like you back in Utah.

KNUDSEN: Well, very much. He is a better Mormon than I am, I guess, so far as his habits--no liquor, no tobacco, no habits of this sort at all, and his two sons have grown up in this same tradition.

But his temperate life apparently is paying off. It is extraordinary that he continues his consulting work for some of the architects for whom he consulted while he was at the University of Illinois, and they send their plans out to him here at Claremont and he performs his usual and competent acoustical services. When I talk with Norman Watson about his father, he will almost always say, "Oh, you know, father's working on another acoustical consulting job at the present time. It's for some firm in Chicago or some firm in Urbana or some firm out there that has relied upon his advice now since the early 1920s."

MINK: One other question I did have was this: in referring to some of the departmental records in an earlier session, we noted, did we not, that there was a joint meeting of the Acoustical Society, and, I believe it was, the American Physical Society held here on the UCLA campus in the early period. And I believe that Dr. Warner was asked to make arrangements for that meeting. Is that correct?

KNUDSEN: Yes. I believe that is correct.

MINK: Could you tell us something of that meeting?

KNUDSEN: Yes. This meeting was rather early in the history of the Acoustical Society. I recall that it was soon after I had been in Europe for the first time in 1930. I know I got back in time for this meeting. It was a joint meeting of the American Physical Society and the Acoustical Society of America. One of the things I think that struck the eye of physicists who were attending the Physical Society, as well as acousticians who were attending our society meeting, was that we had as many people at our meeting as they had at the Physical Society, and we were just a young society at that time, but we were confident and thriving.

I remember that one session of the Acoustical Society, although it may have been a joint session with the Physical Society, was addressed by Professor Emeritus Robert W. Wood, of Johns Hopkins University. Maybe this story should be stricken from the record.

MINK: Oh, no.

KNUDSEN: All right. R. W. Wood was a gifted experimental physicist, and always a punster. One of his chief ambitions was to embarrass headwaiters at prominent restaurants and hotels, and I can't refrain from telling one story when I was his host. Mrs. Knudsen and I were his hosts during the early stages of the war when I was director of research at the San Diego laboratory. University of California Division of War Research was the official title of our organization at that time. And R. W. Wood, who was one of the leading men in optics and spectroscopy, and had had a lot to do with the training of Joseph Kaplan and Lee Kinsey when they were graduate students at Johns Hopkins, had been invited to our laboratory at San Diego to ascertain whether there was any possibility of using optical methods for detecting submarines. And so he was much interested in the transparency of ocean water and was out conducting researches along that line.

He had two physicists from Berkeley who were working with him, Professor F. A. Jenkins and a Dr. Wise, both specialists in optics. I think Dr. Jenkins had been the host to R. W. Wood the night before at this famous hotel, you know, the old Coronado Hotel. And this is an American Plan hotel, and we walked in and I was going over to the desk to get our tickets because we were not registered at the hotel. And R. W. Wood said, "Oh, don't get your tickets. I'd like to have some fun with the headwaiter." And so I obeyed and we went in without our tickets.

R. W. Wood had explained that the night before, when Jenkins was his host, he had also insisted that they go in without their tickets, and when the head-waiter came and asked for their tickets, R. W. Wood spoke up ahead of Jenkins and said, "Tickets? Tickets? This isn't a motion picture house." And the headwaiter was flustered and said, "Why, this is an American Plan hotel and it is necessary that the guests show their registration card or that you have special tickets that you got at the registration desk." Wood said, "The better hotels of New York don't do this." And, so, R. W. Wood got away with it. That's at least the story that he told me as we went into the room that night.

Well, during the course of our meal we overheard the headwaiter instructing our waiter. He says, "Don't ask these people for their tickets. Their guest is from New York City and they don't approve of our plan of having tickets. We'll work this out some other way." Well, R. W. Wood was always looking for an excuse of that sort.

Wood addressed the joint meeting of the Physical Society and the Acoustical Society on this campus in what is now Kinsey Hall. His introductory story had a bearing upon the work that he had done in ultrasonics.

Although his major work was on spectroscopy, he authored (or coauthored) a book that dealt with--I think the subject title was Ultrasonics. He had done a lot of the early work on some of the extraordinary things you could do with ultrasonics, including the killing of small microbes and bugs, and even fishes. I don't think he got quite as far as rats and mice, which were killed some years later with ultrasonic radiation or even with sonic radiation, of high intensity. But he had early performed a lot of these ultrasonic experiments. He had actually patented certain ultrasonic things; we all knew that he was an inventive fellow and that he had many patents to his credit.

He was introducing a paper in which a question of priority was of some consequence, and so, he said, "Whether this work I am reporting is the prior work in this field reminds me of an experience that I had in my youth when I worked on a farm. We had a Negro in our employ, and we were transporting a load of manure. And we had to pass through a number of toll gates with this load. We got it in a city and we were taking it out to a farm. And when I came to the first toll gate, the gate master said, 'What do you got there?' I said, 'A load of manure and a nigger.' And when I came to the second gate and again the same question, 'What do you got there?' 'A load of manure and a nigger.' As I approached the third gate the Negro rose up and says, 'Say, boss, next time would you mind mentioning my name first?'" So R. W. Wood was full of stories of this sort, and this was appropriate at a meeting of the Acoustical Society where we were accustomed to departure from the more or less staid formality that characterizes most of the meetings of the American Physical Society.

MINK: I could imagine that this probably was one of the first meetings to be held here on the campus of a very large society.

KNUDSEN: It was the first meeting of the Physical Society on this campus. The first meeting of the Physical Society at UCLA was on the Vermont Avenue campus. I believe this was the first meeting of an important scientific society on the present campus. Los Angeles was a logical place for such a West Coast meeting of the Acoustical Society because of the interest in sound coming to the films, and radio also was beginning to be prominent in Los Angeles; but particularly there were quite a few acoustical engineers who were concerned with the making of motion pictures in these new sound studios in Hollywood. [tape off]

MINK: I understand that you have [April 25, 1967] just returned from a trip in which you received a gold medal from the Acoustical Society.

KNUDSEN: Right. Today we were going to talk about my consulting work. But the Bruin this morning has a report on the award of this gold medal from the Acoustical Society.

MINK: And that's the issue for Tuesday, April 25, 1967.

KNUDSEN: Tuesday, April 25, which is the date we are discussing.

MINK: Where is that? It's on page 3.

KNUDSEN: It's on page 3 of the Bruin. The first two paragraphs, it seems to me, in a sense introduce the subject we are going to talk about today. It says, "The country's top award in acoustics has been presented to Vern O. Knudsen, physics professor and former chancellor here. The Gold Medal of the Acoustical Society of America goes to Knudsen for nearly fifty years of path-breaking research. During this period"--and this is the important sentence that deals with our subject today--"he helped design the acoustics of some 500 auditoriums and concert halls, including the first movie sound stages, the Hollywood Bowl and the Los Angeles Music Center."

Inasmuch as we are talking about the consulting work I have done in connection with some of these, the 500 is not exact, but I know it is not an overstatement. For example, in my file here I was just noticing I made a brief count, and these are things that have been going on only in the last four or five years. There are 150 items in this three-drawer file here. I won't attempt to give you a complete list [laughter] of all of these 500 auditoriums, but a little later today I will mention some of the major things with which I was concerned.

But I think we should have a little background information on this subject, and I have made a few notes that I will use in dictating this. I have already referred to my Bell Telephone Laboratories and University of Chicago background which I think indicates my interest in architectural acoustics. I believe I may have mentioned even the economic aspects of that, that the offer I had refused at the Bell Telephone Laboratories had certain financial advantages that any married man at that time who was just getting out of graduate school would look at rather hopefully as contributing to the more abundant life, at least those things that can be made abundant by the dollar. So when I accepted the position to come to Los Angeles, as I have indicated previously, I felt that there was a real opportunity to do consulting work in the design of auditoriums and other buildings in respect to acoustics.

MINK: In deciding this, in coming here, there was never any question in your mind, or it never did arise, that this wasn't perfectly within the province of what you wished to do as far as your work here at the university was concerned.

KNUDSEN: I think I said one of the chief reasons at the time for coming out here was the health of a daughter, our first child. But I did feel that because of the rapid growth of Los Angeles, which was talked about a great deal at that time, even at the University of Chicago, that there was especially an opportunity to introduce the subject of architectural acoustics out here and to actually help design the buildings. And probably the Bell Telephone Laboratories experience again had something to do with that. As I have indicated in my research, it was largely the utilization of these vacuum tube techniques, oscillators and amplifiers, that led to the early research here, which made it possible to refine the pioneer work that had been done by Wallace C. Sabine at Harvard University. [tape off]

There has always been somewhat of a question as to just how far a professor should go in doing consulting work, and I discussed this matter first of all with Ernest Carroll Moore when I came here and indicated what my plan was. He said, "Well, if it improves your work in teaching and research and has a bearing there, and is not interfering really with the established professions here, it is all to the good." And that was the advice I had from him.

Later, I discussed this matter with President Sproul, when I was really getting more active in this field, and Sproul was always very encouraging in his reply about questions of that nature. "By all means," he said, "this is doing a lot to improve the status of the university throughout California." Most of my work was limited to California, so that I didn't feel that this was really sacrificing my teaching and research responsibilities here, and I think that the record would indicate that I did not really step beyond the bounds of propriety in the matter of the kind of research and consulting work I did.

MINK: Had there been people in the department that you felt ever did?

KNUDSEN: No, not in our department. I think there have been people in the University of California; I can't cite names right now. I don't think I should. But I frankly don't know anyone on the UCLA campus. I have heard one or two rumors about people at Berkeley who had perhaps exceeded the bounds of propriety in this matter.

MINK: Well, then, "propriety" really means when this works to the detriment of your class work?

KNUDSEN: That is right.

MINK: If you don't meet your classes . . . .

KNUDSEN: I certainly never have cut a class because of any consulting work. As a matter of fact, when I was asked if I would consult on a building, I always said, "Provided this does not interfere with my basic teaching and research obligations to the university. I can help you on Saturdays. I can help you on certain afternoons and evenings, when I am not busily engaged in my university duties." But I made it clear all along that these duties came first.

MINK: Well, this would have to be, wouldn't it, because people couldn't be promised that they might expect something just like that.

KNUDSEN: That is right. Well, as a matter of fact, there has been no hard-and-fast line of separation between my research and my consulting work. That is, they have supplemented each other, and as a matter of fact, in this first book of mine, Architectural Acoustics (I think in the last paragraph--it is just a very brief one), I set forth really the way I felt at the time in 1932. I'm sure it expressed the opinion of Ernest Carroll Moore and Robert Gordon Sproul, with whom, as I told you, I discussed this matter on a number of occasions. It said, "The author wishes to make one significant claim about this book, namely, that he is not presenting untried theories which are only of academic interest, but that he has had occasion, in connection with the design and construction of numerous buildings, to test and verify the correctness of the fundamental principles set forth in the following pages." This, I think, is an honest, factual statement of the relationship of the consulting work I have done to the research work I have done here at the university. And I think you will find probably in any curriculum vitae of Vern O. Knudsen, written by people in the university, that it was this work I did outside as much as the work I did inside that contributed to my competence in the field of architectural acoustics. The honor we referred to this morning I know is based on the consulting work I have done as well as on the work I have done in the laboratory and the publications that I have produced.

MINK: Well, most of the buildings that you were working on--there wouldn't be a rush, would there? Because the buildings would be up. It would be a matter of modifying it, and they could put up with the bad acoustics until you could come up with a recipe for fixing it up.

KNUDSEN: Well, in the early career of my consulting work certainly corrective measures for existing buildings played a more important part than designing in advance of construction. More and more, in later years, the emphasis was on the design of new buildings.

MINK: Well, then, you really might be in the soup unless you could come up with something immediately, or the people just would have to accept the fact that you would get it when you were able, provided it didn't interfere with your other work.

KNUDSEN: Well, with those restrictions on the time I could devote to projects of this sort. Of course, if you can devote every Saturday, you are not going to delay people very long in doing consulting work of this sort.

MINK: How long does it take, actually, to come up with a solution to an auditorium in terms of time that you have to put in on Saturday?

KNUDSEN: Well, it didn't take nearly so much time in those days, because about all we thought about was to insulate it against outside noise and to correct the reverberation, get the reverberation close to the optimal time. I'll refer a little to that later here today. But then probably a matter of ten to twelve hours on a high school auditorium, something of that nature, would be the time required to go through the detailed calculations of reverberation and to examine what they've done in the way of sound insulation. But even sound insulation didn't receive very much attention in 1922 to 1930. It began to be more and more important, and very little attention was given to shape except the avoidance of--well, obviously bad shapes like domed ceilings or barrelled ceilings or circular plans and concave surfaces, large concave surfaces. Acousticians knew then and knew even before the work of Wallace C. Sabine at the turn of the century, that there were certain bad shapes that would give rise to focusing effects of the type in that large auditorium at the University of Illinois, where F. R. Watson did his outstanding work.

MINK: Well, now, when you are talking about twelve hours, you are talking about corrective measures. You're not talking about design prior to construction.

KNUDSEN: Well, even for an auditorium in advance of construction, you usually got the drawings after the architect had prepared his working drawing. This was customary at that time.

MINK: Then you would have to fight it out with him.

KNUDSEN: Then you would at least reason it out with him. If the shape was bad you would say, "This is impossible," and in some instances I said, "I can't have anything to do with it." If it was an impossible shape, and I knew it was impossible, I'd say, "Unless you change the shape, I'll simply have nothing to do with it."

MINK: Well, would they continue to be recalcitrant or were they willing to compromise with you?

KNUDSEN: Well, some of them were recalcitrant, and some of them had rather peculiar notions. I believe I should, at this point, read into the transcript here two or three introductory paragraphs of, I think, the first professional article I wrote on architectural acoustics. And it appeared in the <u>Architect and Engineer</u> for October, 1924. This was really at the very beginning of my consulting work and the beginning also of my research work. I was just getting started. But I think this is a sort of answer, and it sets the stage for some of the consulting work that we will talk about later today. This is called, "The Acoustical Design of Architectural Interiors." It begins on page 93 of this journal that I have just cited. And the first three paragraphs read like this. (I may paraphrase parts of it.)

> The acoustic outcome of an architectural interior is usually a source of grave worry and uncertainty to the architect. He awaits with hope and faith, yet mingled with fear and doubt, the verdict that his first audience will render regarding the acoustic properties of his latest creation. The writer hopes in the present paper to dispel some of this doubt and uncertainty by setting forth the fundamental principles of architectural acoustics in a simple and practical manner. These principles have been worked out with scientific precision.

I think "precision" is a little optimistic.

MINK: Well, as you said, you never really can approach an optimum, can you.

KNUDSEN: No.

> And if correctly applied, will assure good acoustic quality in any architectural enclosure. The success attending the application of these principles to architectural enclosures during recent years has fully demonstrated that we can be both optimistic and certain regarding the outcome of this important problem. It is about twenty years since the more fundamental principles of architectural acoustics were worked out and published by the late Wallace C. Sabine of Harvard. Generally, architects, builders, and the public, have been very slow to appreciate and apply these principles. In fact, there was no evidence of any application on the West Coast, when I came here in 1922.

MINK: Did the architects know who Sabine was?

KNUDSEN: I don't believe so. Some of his papers, I believe I told you, were published in the American Bricklayer.

MINK: A very low level at which to be published.

KNUDSEN: The name changed and it later became a more distinguished journal. I believe I mentioned that. "However, there have been many architects and builders who have applied these principles with satisfactory success during the past few years,"--not in the Los Angeles area but in the East where Wallace C. Sabine was, for example, the Boston Symphony Orchestra and the Fogg Art Museum where he did his pioneer work. And Sabine had been called in on the design of several New York and other theaters and auditoriums, and so Sabine was very active all the way from buildings I know in Michigan to the New England states and New York and as far down as Philadelphia.

MINK: Did Sabine have anything to do with the acoustical properties of the Harvard Museum?

KNUDSEN: Only the Fogg Lecture Hall.

MINK: I see. There is a record, you know, of E. Power Biggs performing in the Harvard Museum. It seems to have admirable acoustical properties.

KNUDSEN: Yes. Well, this was not true, of course, of Fogg Lecture Hall which was not used for music; but as the name implies it was a lecture hall there, and they couldn't hear lectures.

MINK: It was foggy.

KNUDSEN: [laughter] It was foggy, certainly, in an acoustical sense. Then it says, "In Los Angeles"--this was in 1924, remember--"the Board of Education is now having the acoustic designs of its new school auditoriums worked out in advance of construction." And

I believe I told you that I had been called in by the board of education after they learned of some of these researches that I was conducting in these five acoustically poor auditoriums in the high schools.

MINK: This was totally responsible for their changing their policy on this?

KNUDSEN: Yes. And at this time, October, 1924, I was then working on the correction of the acoustics of these five auditoriums in which I had conducted speech articulation tests. I had not made any special study of the acoustical properties, but I had to measure the reverberation times of these rooms. Nearly all of them were of the order of 4 to 6 or even 7 seconds, empty. And this means, you see, when you are listening to one syllable, if you are pronouncing, say 5 syllables a second or only 4 syllables a second, you are listening to 1 syllable while the preceding 25 or 30 are still audible, and they are masking the syllable you want to listen to. So reverberation has that kind of effect, and we had demonstrated, you see, that when you had 7 seconds of reverberation the speech articulation was as low as 50 percent and, you remember, we have said 75 percent is the minimum at which you can get by.

MINK: There are certain terms, I think, that are used to refer to the acoustics of buildings.

KNUDSEN: Yes.

MINK: For example, I was thinking about your mentioning of the muddy type of building where you have a long reverberation. What you were trying to do was get a crisp kind of a sound in the building. Now, are these words that grew up out of this corrective sort of thing? When did this vocabulary come into existence?

KNUDSEN: Certainly "crisp" didn't come into the vocabulary that early. This came later with the emphasis on the design of music halls, concert halls, and so on, and "crisp" is often used there. For example, many people--we don't use this term in the acoustic language--but the popular people will sometimes refer to the Boston Symphony as being more "crisp," for example, than the Academy of Music at Philadelphia. And our own Music Pavilion in Los Angeles has sometimes been likened more unto the Boston than the Philadelphia hall.

MINK: Then you people as acoustical engineers did not use these terms. These were terms that lay persons coined?

KNUDSEN: That is right.

MINK: Like "rain barrel effect," for example.

KNUDSEN: Yes. Yes. This "rain barrel effect" is excessive reverberation but especially at the low frequencies, and it is also associated with resonances in the room. When there is a peculiar resonance, the room certainly gives the impression that you are listening to sound with your head in a rain barrel. And it is like when you are speaking with your head in a rain barrel or any other similar resonator. You get the resonance very prominently induced there. And many auditoriums did have this rain barrel property.

MINK: Or "dry."

KNUDSEN: Sometimes "dry," which is descriptive of music rooms with too little reverberation. "Dry" and "crisp" are typical examples of terms that came more from the psycho-acousticians and the musicians who were using these terms, and who didn't use such terms as we used, such as "resonance" and "reverberation" and balance between "low-pitch sounds and "high-pitch sounds" and "echoes" and "flutters"--these are the terms that are current in the acoustical parlance, but often other terms are used by the popular groups.

MINK: When did you first hear these terms, would you say, what, about the 1940s maybe, do you remember?

KNUDSEN: Well, it's a little earlier than that.

MINK: Did you understand what they meant?

KNUDSEN: I think so. I wrote my first article on acoustics of music buildings about 1932, and quite a few of these terms were available then. I had made a survey of the leading concert halls of Europe and several in the United States at the time I wrote that paper; and so I was familiar with the fact that there were a lot of terms, and "dry" and "crisp" were familiar. "Mellow" was another, and one we use now is "definition." In a music hall you would like to have the reverberance, but also definition. And "definition" means, in terms of what we were studying about the hearing of speech in auditoriums, that you can understand the separate words of speech or you can recognize and hear distinctly the separate notes of music.

MINK: Of the different instruments?

KNUDSEN: Of the different instruments, or even the different notes of the same instrument. This is "definition." We use that term more than any other as equivalent to "dry." Sometimes the word "clarity" is used in the acoustical notation. But these are acousticians' terms. And one of the other terms that is used a great deal today is that you want to feel that you are "immersed" in the sound field, which requires that the sound reflections which comprise reverberation should come to you from all directions. One of the criticisms of Philharmonic Hall in Lincoln Center, New York--and this is also characteristic of most of the auditoriums that have been designed in the United States in the twentieth century--was that the emphasis had been to project the sound from the stage to the audience. And this has been overdone. It was overdone in Philharmonic Hall; it is overdone slightly even in Royce Auditorium; but in general, in the fan-shape auditorium, and with the ceiling often approaching parabolic shape, the auditorium becomes a sort of a megaphone, a huge megaphone, that directs sound out towards the audience. And the criticism that results was voiced by Leonard Bernstein when we were investigating the acoustics of Philharmonic Hall. He said, "My God, this doesn't sound like a concert hall. It sounds like a motion picture house, with the sound coming from loudspeakers behind the screen up there on the stage."

Well, obviously if you emphasize the direction--we say "the directivity"--of sound toward the audience, there is going to be too much flow of energy from the stage to the audience. And for good concert hall acoustics the listener wants to feel that he is immersed in the sound field, that he is surrounded by sound, that reflections are coming from overhead. They're coming from the right. They're coming from the left. They're coming from the rear as well as from the front. And that hall in which too much comes from the front is likely to be compared to motion picture acoustics instead of concert hall acoustics.

Well, I'll finish reading this brief paragraph here. "The writer is familiar with scores of instances in and around Los Angeles where during the past two years the principles of architectural acoustics have been applied successfully to churches, theaters, school auditoriums, music halls, offices, and other interiors."

As far as I know I was the only one in California that was working on acoustics at that time. As a matter of fact, when sound came to the motion picture industry about 1928, the motion picture people had no other person to turn to out here except Vern Knudsen. He was the only one on the entire West Coast who was doing anything about acoustics at that time. So I sort of had a monopolistic control of any acoustical consulting work that was done on the West Coast, and I don't know of any others, certainly before about 1930.

The Acoustical Society, you see, was organized in 1929, and then there were people who had been brought in from the Bell Telephone Laboratories--ERPI. It was called Electrical Research Products Incorporated, which is affiliated with the Bell Telephone System. They were the subsidiary unit of the Bell Telephone Laboratories that guided the motion picture industry in the selection and operation of the ERPI sound recording and reproducing equipment. But they became also interested in the acoustical environment in which these pictures were produced, and also in the theaters in which these sounds were reproduced.

So beginning about 1929 or 1930 there was really a resurgence of interest in acoustics on the West Coast, and many of these people that were called in to help, especially on the recording and reproducing equipment for sound in the motion picture industry, were building up very rapidly. The total interest in architectural acoustics on the West Coast [was on the rise].

MINK: When all of this breakthrough occurred, did you go to them and offer your services, or were you contacted first?

KNUDSEN: I was contacted. I think I have already explained, I had utterly no research space on the Vermont Avenue campus, and looking about I recognized that there were some pretty bad auditoriums acoustically in the high schools of Los Angeles, and so I did seek permission to carry on my research in these auditoriums. I frankly don't know now whether in the back of my head I felt that, well, this might lead to some consulting work. I wanted to do the research anyway, and if it did lead to such work, why, I wasn't going to say no to such invitations. And as a matter of fact, my initiation into consulting work came as a result of that work in those five auditoriums.

I think it was Susan Dorsey who was superintendent of the Los Angeles schools at that time. I believe she succeeded Ernest Carroll Moore who had been superintendant.

MINK: Yes.

KNUDSEN: Dr. Moore had told Mrs. Dorsey about the work I was doing, and she told him, "Oh, yes, we know about it. He is making a survey and we gave him permission to occupy these five auditoriums." And so I was invited to discuss the acoustics of the five high school auditoriums particularly, but the acoustics of auditoriums in general, at a board of education meeting in Los Angeles. And very shortly after this meeting--and this was at the time we were still conducting our researches in these five auditoriums--I gave the results of what would happen, because we had already conducted tests in Millspaugh Auditorium and had introduced corrective measures. I've referred to that already. So I had the results of what that correction did to the hearing of speech in Millspaugh Auditorium. And this impressed the members of the board of education, and I was soon retained by them to recommend corrective procedures for these five auditoriums in which I was carrying on my experiments at that time.

MINK: Well, when the sound came into the movies, and you say they came to you, who came to you?

KNUDSEN: Well, I'll discuss that a little later. I do intend to go into that. But let me just read one more brief paragraph here, then we'll go into some of the other matters, because this sets the stage for the consulting work that we will be talking about.

> There is nothing mysterious or extraordinary about the acoustics of interiors, although many weird and naive theories and principles have been proposed by unknowing persons. For instance, not long ago a rather prominent commercial and technical man attempted to explain to the writer how the acoustics of an interior was determined by three things: humidity, temperature, and differences of temperature.

MINK: That would be George Kelham, wouldn't it?

KNUDSEN: That would be Kelham--you guessed it. [laughter] God bless his soul. I guess he's no longer here. But Ross Robertson in chemistry told me the story that when someone told Kelham about this young assistant professor at UCLA, Vern Knudsen, who was doing some outstanding work in the acoustics of auditoriums, Kelham said, "Well, he's just a young fellow, you know. He's trying to make a reputation some way, but he doesn't know very much about it." And it was then that he said, "We know that the acoustics of a room depend upon three things: the humidity, the temperature, and the difference of temperature." And of course this is a ridiculous statement for anybody to make. That is, the velocity of sound depends upon the temperature, but the change in temperature and the resulting change in sound velocity are so small in an auditorium that practically these things have nothing to do with its acoustics. He guessed right in one respect, namely, humidity had more to do with acoustics than we suspected at that time, because you know, later, we did show that the humidity of the air was a very significant factor in determining the reverberation of sound at high frequencies.

MINK: Here on the Westwood campus?

KNUDSEN: Here on the Westwood campus. But at that time not even physicists suspected that the humidity of the air had anything to do with room acoustics. I was of course referring to our research on the absorption of sound in air and other gases, which was the reason the paper on that subject won the $1,000 prize of the American Association for the Advancement of Science. Kelham was correct in one little respect there, namely, that high frequency reverberation is affected by the humidity. But I think this was an accidental conclusion on his part. And sound is refracted either up or down if you have a vertical temperature gradient. Sound travels faster in warm air than it does in cold air. So if the air is warmer, say, high up in the room than it is down low, the sound will bend a little downward. But in the distances you have in a room, the amount of bending that you would have would be at most, of the order of 1 degree of angle. And this, of course, is utterly irrelevant so far as any effect it would have on the acoustics of the room.

"Another architect recently said he always attained good acoustical quality if he maintained a particular ratio of length to width to height. Another said his auditoriums were always good if he used wood floors." These were typical [statements] by architects in Los Angeles.

MINK: Well, maybe it's all right to say who those architects were.

KNUDSEN: Frankly, Kelham is the only one I remember. I do remember that one. But these others were architects who made statements about the characteristics of the auditoriums they design. I frankly don't remember their names now. Some music critics, for example, Albert Goldberg, for a long time often said--you know, he was formerly music critic for the Los Angeles Times--"If it's wood it's good; if it isn't, it isn't." And that's been a very strong feeling on the part not only of architects, and I probably could name a lot of architects who have said, "Well, if they use wood, but not just wood floors alone." They usually want wood walls and even wood ceilings; and it's quite true, because wood is much less sound reflective than hard plaster or concrete or brick. It was with the advent of hard plaster, concrete, marble, glass, and other similar materials that reverberation became one of the dominant sources of acoustical failures. Well, there's just one more sentence: "The acoustics of any enclosure depends upon the fundamental characteristics of sound, and upon its transmission, reflection, refraction, diffraction, absorption, and resonance." These were the things that I said in 1924.

MINK: You actually were one of the first people to begin to construct a vocabulary of acoustics then. Really.

KNUDSEN: Yes, but I was only one of many others. I was contributing on the West Coast, but this had applications elsewhere.

MINK: Well, now, you said that you were going to say something about some of these architects who were real tough. I would like to hear about that.

KNUDSEN: Hear about--I'll have to think about this. [tape off]

Do you want to state your question again?

MINK: The question I think that I had really is, how do you thrash out this whole business in a church?

KNUDSEN: Well, there have been a number of instances in which I have been called in early in the design of a church, and often this begins at a conference involving the architect, the minister, the organist, and the chairman of the building committee. And there will almost always be a diverse set of opinions that will come out in such a meeting. The organist invariably will insist upon having a very long reverberation time. He will cite many buildings that have reverberation times of 4, 5, 6 or even 8 seconds of reverberation, and for certain kinds of organ music a long reverberation time is admittedly good; but as we have already demonstrated, this is deadly for speech. You just have great difficulty handling the problems of speech if the reverberation time is too long. And this was especially true in that day.

There are special means of amplification today that they call "delayed loudspeakers" or "delayed speech," in which they'll have a whole series of loudspeakers down the nave, say, of a long church. The best example is probably St. Paul's in London, in which they have a series of loudspeakers about one-third of the way and two-thirds of the way from pulpit to rear. And this for St. Paul's is long, 300 or 400 feet, you see, from the pulpit to the rear of the nave. And they can largely overcome the effects of reverberation on speech (and also preserve the illusion that the speech comes from the pulpit) very effectively by having the amplified speech that comes from the loudspeakers nearest the audience arrive after the speech that comes from the pulpit, or from the loudspeakers near the pulpit. Then you get the illusion that the sound comes from the front, but you also get the advantage of having the loudspeaker source near you so that the direct sound is much louder than the reverberant sound. And an important criterion for hearing speech well is what we call "the signal-to-noise ratio." That is, if the direct (and the early reflected) speech you want to hear is louder than the reverberant sound, then the hearing of the speech is pretty good. So even with rooms of a high reverberation time, it is possible by this type of amplification, which we call "delayed speech amplification," to get by.

Certainly until recent years such corrective measures in the electro-acoustical equipment were not possible. Especially in my early days of consultation on matters of this sort, there was always this conflict between the minister who wanted his speech to be heard intelligibly and the organist who wanted his organ to sound the way he wanted it to sound--big, cathedral-like, that is, reverberant. He had the idea that it would sound very much better if it was in a very reverberant space.

In that connection I recall the opinion of Alex Schreiner who was for seven years organist at Royce Auditorium, and who was greatly admired by Ernest Carroll Moore (and me too), and presently is top organist at the Salt Lake Mormon Tabernacle. Alex Schreiner said he liked to play in Royce Auditorium on the organ there very much because the reverberation <u>wasn't</u> too long. And he could play the Bach tocattas and fugues and things of that sort in which there were very rapid movements. If you perform that kind of music in a very reverberant auditorium (church or concert hall) the separate notes overlap and confuse and you don't have good definition; in extreme reverberation the music, like speech, becomes a hodge-podge. And so there is much to be said about a reasonably low reverberation time to promote good clarity and definition even for organ music in a church. Dr. Schreiner tells me that he receives the best compliments for his playing in the Tabernacle when the audience numbers two to three thousand, resulting in a reverberation time of a little more than 2.0 seconds.

MINK: Well, what would the architects usually say?

KNUDSEN: The architects usually didn't know what to say.

MINK: Did they care?

KNUDSEN: Yes, I think they cared. An intelligent one was Mr. Carlton here. He has a son now who designs churches, and Leo Delsasso has helped him with the design of a number of churches, and I helped the father with two or three small churches in the Los Angeles area in early years. And both architects, father and son, had a very sympathetic understanding of the necessity of reducing the reverberation adequately, but at least keeping it reasonably long for music. David Allison, architect for Royce Auditorium and Kinsey Hall, also cared--and listened.

Well, at these early discussions you often would have almost irreconcilable opinions on the part of the minister, on the one hand, and the organist, on the other, and so the acoustical consultant usually was called upon to arbitrate. In general, I succeeded quite well in bringing these two persons, with their conflicting views, together. And one way you can accomplish that in the actual auditorium--it's a church we're talking about--is to have a relatively reverberant chancel and sanctuary and a choir space in the front, and then use more absorption in the rear of the church.

MINK: Fine.

SECTION XIII

MINK: I had asked you whether you tolerated off-color stories or not.

KNUDSEN: Well, I presume I tolerate them more today than I did when I was really an orthodox, practicing Mormon. It was somewhat shocking to me. And I think this can be illustrated somewhat by a story that was told of former Utah Senator Reed Smoot who almost lost his Senate seat. That is, you remember it was somewhat like the present case with the Negro congressman.

MINK: Adam Clayton Powell?

KNUDSEN: Adam Clayton Powell. But Reed Smoot was suspected of being a polygamist. He was not, and I think the record would very definitely demonstrate that--I knew it very well because I knew the Smoot family. In a town the size of Provo you know whether a man has two wives or not. This would have been in violation, of course, of what's known as the manifesto, which outlawed polygamy, and most good Mormons obeyed the federal law when the manifesto was issued. Well, about this matter of stories, I always liked to tell this one about Reed Smoot in connection with listening to salacious stories. He was in the company of some men, other senators and colleagues at Washington one time, when someone looked around the room and said, "I see there are no ladies here so I presume it will be appropriate to tell an off-color story." Reed Smoot bristled to attention. He said, "Well, there may not be any ladies here but there is one gentleman." And he didn't care for stories of that sort, and in general they've been looked down upon in the Mormon Church. If you have listened to Richard Evans on the Sunday morning sermons that accompany the Salt Lake Mormon Tabernacle Choir--you like the organ so I know you often listen to this program--and Richard Evans very definitely would indicate the kind of stories that would be wholesome. I presume Abraham Lincoln liked some.

But there was one other story about Reed Smoot that I think should be in the record because it was a choice one. This one was told to me by a regent of the university who also was the feature writer for the San Francisco Chronicle--Chester H. Rowell. I knew him well and admired him greatly. He was for many years a distinguished regent of the university and, like Edward A. Dickson, a powerhouse. I was sitting beside him at a formal dinner of the University Affiliates. He had just learned that I was a Mormon and he said, "You know, the first cub reporter job I had was covering the Smoot case."

Smoot's seating in the Senate was contested, and the Senate had a formal hearing as to whether Reed Smoot should be allowed to assume his seat as senator from Utah. As a matter of fact, he was vindicated, and he served I guess as long as anyone. I believe it was forty-two years. He ended I think the wrong way, favoring the Smoot-Hawley Tariff which even orthodox Republicans today say was the greatest mistake Reed Smoot and Hawley had ever made, and I certainly can verify that. I was in Europe in 1930 on my first trip [abroad], and I heard widespread castigation of Reed Smoot and Hawley because of the Smoot-Hawley Tariff at that time. People I met in Germany especially felt that it had a lot to do with the rise of Hitler. But that unfortunate mistake doesn't detract from Smoot's other commendable qualities and contributions. Smoot was the watchdog of monetary affairs in the Senate during most of his forty-two years there.

The story that Regent Rowell told me that night is as follows: "You know," he said, "one of the favorite stories that I heard at this time, I heard in my barber shop. The barber was summarizing his feeling about the findings in the Smoot trial. He said, 'Well, you know, after all, I think I'd rather have in the Senate a polygamist who doesn't polyg[amize] than a monogamist who does polyg[amize].'" [laughter] Well, I thought maybe those two stories would just [be good for the record]. Smoot was a man I admired very much, except for this Smoot-Hawley Tariff, and he was a very fine senator, highly respected by his colleagues and the public he served. [tape off]

I'd like to discuss some of the things that had to do with my choice of a research program when I arrived at UCLA in the fall of 1922. I recognize that my decision to choose architectural acoustics and physics of impaired hearing would classify me really as an applied physicist rather than as a pure physicist. And most physicists at that time probably would regard that as stepping down at least one rung on the ladder. You know, the mathematicians often think they're all at the top, and they look down on the theoretical physicists, who in turn look down on the applied physicists, the applied physicists on the engineers, and so on. I didn't worry too much about that.

Millikan, himself a pure physicist, had really told an interesting story in one of the courses I had with him there at Chicago, that always impressed me very much. He said that physics, like most other sciences, has a natural progression which is very much like a man walking. He takes one step forward with the left foot and another step forward with the right foot. And the left and the right feet could correspond to pure and applied physics respectively. He cited many examples to show that the whole history of science is replete with this alternating nature of progress between theory and practice, between pure and the applied physics. And so I have shared this opinion with Millikan, even though most physicists I had known believed that the transition from pure to applied science is an irreversible process--that is, this is quite a general feeling on the part of pure physicists. I won't name names, but there are some very prominent physicists who quite definitely would regard this as an irreversible process.

MINK: Is this such a sacrosanct thing, that you don't talk about the people who are applied physicists and you don't talk about the theoretical physicists?

KNUDSEN: Well, certainly there was one here; and I'm sure that this was one reason Barnett looked down on my work. He was very definitely a simon-pure physicist, dealing with research problems that had no immediate application or no prospect of application at that time. And I think he gloried in that kind of research, and I believe what I've related in the oral discussions here would indicate that I have always had a practical bent. I wanted to do things that had beneficial relationships to people. My research always had a bearing on human values, and architectural acoustics, and the physics of impaired hearing definitely have that slant.

MINK: It's difficult for me to understand what value there is in the pure part of this. If it doesn't have any practical implications, what value is physics or any other science?

KNUDSEN: Well, I always have been inclined that way, and you know I didn't want to work on the problem Millikan wanted me to work on--the contribution of electrons to specific heat of metals. This didn't excite me. It was more difficult, for one thing, than I wanted to undertake. I didn't feel that it was within my grasp. I honestly recognized my limitations and felt that if scientists like Walther Nernst and Albert Einstein and Peter Debye and some of the top theoretical physicists in the world had been struggling with this problem and had not made significant headway, that this was no suitable problem for a young PhD candidate to undertake. And one of my colleagues had wasted two years, really, on this project at Chicago, and so I was pretty much oriented toward these more practical things. My experience at Bell Telephone Laboratories also, of course, had a bearing on making me really an applied physicist rather than a pure physicist. I think even if I had had the talents to become a pure physicist and had pursued mathematics--I believe I could have become a pure physicist, although I don't think I would have been as successful in that field as I have been in applied physics--I couldn't have gotten the enthusiasm, for one thing, for it, that I have for these subjects that stirred my imagination and my enthusiasm.

MINK: Well, when we're talking about a pure physicist, what are his contributions? The theories that he develops and publishes, these are his contributions?

KNUDSEN: Well, one of the finest examples we can name right now is Robert Oppenheimer, who just died a few days ago. And he, I think, perhaps more than anyone I know, would really typify in a glorious way the greatness of the pure physicist. Yet he was tolerant, you see, of Ernest O. Lawrence who was very definitely an applied physicist. Lawrence had this interest in the adaptation of the cyclotron to the things which would be very useful, and he was much concerned with the question, can we develop a radiation treatment of some sort that will cure cancer, that will cure leukemia, things of this sort. Ernest Lawrence was very much concerned about these applications of the cyclotron to the problems of human welfare. And I'm sure that Robert Oppenheimer also was concerned very much about human welfare problems, but in a broader, more philosophical manner. Of course, he had his cross to bear--he was a victim of McCarthyism of that time, and this was a heavy burden to him, and it was a great loss to people who admire such fine people as Robert Oppenheimer was.

Well, history is replete, of course, with examples of the transition from applied to pure research, and I was aware of these. For example, it's generally known, widely known, [laughter] of course, that [Louis] Pasteur was working on the fermentation of wines and things of that sort, and this led to the germ theory of disease and the entire science of bacteriology. Newton himself was very much interested in applied optics, in the defraction patterns he was getting with prisms and so on. This really led to the establishment of the entire science of optics.

The story I shall relate of my own research will give another example, if only a very minor one, in which applied research actually led to an important discovery. This one paper that I shall refer to later entitled "From Architectural Acoustics to Molecular Physics" describes definitely an instance in which this happened. When I began my work in this field--calibration of the new UCLA reverberation room--no one would have ever suspected that such work or anything in architectural acoustics would lead to developments in molecular physics, but that's another story. We'll go into it a little later, because it is another example in which applied research actually opened a door into a new field of pure physics. I was comforted very much when this development occurred, because I had somewhat lived under the stigma that, well, this is just applied work. This is engineering. This shouldn't be physics. But it can, if it's done in the right way, lead to basic discoveries in science.

MINK: Well, now, when you say "a stigma," you probably refer again to Barnett. Were there other people in the department, in the university even, who felt this way?

KNUDSEN: I don't know of any others here. They're rare people. I think maybe one or two physicists at Berkeley felt, well, it's too bad Knudsen's working in this field. He could really do more important work than he's doing. Probably Leonard Loeb, who was also at Chicago at the time I was there and had worked very closely with Millikan, felt that way. I think Millikan would have been happier if I had worked in atomic physics.

MINK: What about Dr. E. P. Lewis?

KNUDSEN: I don't think Lewis knew me well enough to have an idea, unless he got it from Millikan, Loeb or Barnett. I met him, but I have never heard him speak. But I'm sure that both Millikan and Loeb, who were two pure physicists with whom I became well acquainted at Chicago, when I did this work on molecular collisions which grew out of architectural acoustics, felt well, after all, Knudsen has shown that he can do something in pure physics as well. So I was pleased about that. It led to my receiving the $1,000 prize of the American Association of Advancement of Science at their annual (1933) meeting, which prize normally is not given for applied work. This came to me as a great surprise, but it came also as a great consolation because here was work that grew out of applied physics that led to something that the AAAS felt was important enough to grant that prize. And at that time, this prize was probably the principal one given to American scientists--locally, here. Of course, it wasn't a Nobel Prize, [laughter], but it was sometimes referred to as "the little Nobel Prize" in United States circles at that time. The prize received much publicity in major American newspapers and also in leading Scandinavian papers.

There's still one more example of moving from applied to pure science. It happens to be one very close to my own field of work, namely, acoustics. As you probably know, acoustics owes its origin to the scientific study of music, which at least in the Western world began with Pythagoras more than 2,500 years ago. Pythagoras was experimenting, you know, with the monochord and found that when the two lengths of the string were in the ratio of one to two you got the perfect interval of the octave. This was really the beginning of the science of acoustics--these early attempts by Pythagoras to find some number relationship between music and mathematics.

These attempts have persisted, of course, to this day, although there have been some conflicting views between those of Pythagoras and a school of purists who at that time and even much later, didn't believe very much in experimentation or in anything except logic. We often think of Galileo as starting this important epoch of experimental physics. But here is a nice example that goes back to 2,500 years ago in which experimentation with quantitative relationships between music and the length of the monochord led, really, to the science of acoustics, so I don't have to apologize, I think, for supporting the field of applied physics. If for no other reason, it's more interesting to me than pure physics.

Probably I should give one other reason here that we can't very well escape in this practical world. I had been offered in 1922 almost double the salary at the Bell Telephone Laboratories that I was offered out here. I knew if I accepted this position we couldn't possibly live the kind of life that we had been accustomed to as a graduate student even in Chicago, unless I supplemented my income some way. [Through] architectural acoustics, even then in the fall of 1922, it seemed to me, there would be an opportunity to help design auditoriums. And there would be fees associated with the consulting work in architectural acoustics. I would be less than honest if I didn't name that as one of the other factors that led to my prime interest in architectural acoustics at that time. And so my work really began with: what to do about architectural acoustics? Well, as a guide, there was the monumental and pioneering work of Wallace C. Sabine that I already have referred to. I was reading his collected papers in the spring of 1922 when I visited one auditorium in Chicago that did suffer from excessive reverberation.

MINK: What was the name of that one?

KNUDSEN: I believe it was McKinley High School. Sabine's work had indicated that the principal difficulty with auditoriums was too much reverberation. And too much reverberation means there's too much reflecting of sound by the boundaries of the room with respect to its volume and with respect to the number of people that are in it. And Sabine's work had really solved that problem very neatly, and the importance of controlling reverberation hasn't changed a great deal, although we'll discuss later on some rather extraordinary departures from the Sabine formula for calculating reverberation when we refer to the work we have been doing here since Dr. Delsasso and I retired. That will come later.

It occurred to me that there must be other things besides reverberation. In making up my own mind about these things that affect the hearing of speech in auditoriums, I realized that besides reverberation, certainly the loudness of the speaker's voice is very important, and how can we get at the amount of noise that interferes quantitatively? If you're listening in a noise, you're certainly not going to hear speech well. And so it soon became obvious to me that there were four factors that affect how well you hear speech in a room. One is related to the loudness of the speaker's voice, and that in turn is related also to the size of the room. My voice is adequate for a fairly large room. It's more than adequate for a room this size (12' x 16'). We'll go into this matter in some detail later, because it is basic. The four principal factors, then, that affect hearing of speech are: one, loudness; two, reverberation; three, noise; and four, shape.

It seemed to me that the Bell Telephone Laboratories and the experience I had had there and the program that

Harvey Fletcher, particularly, was developing at that time, which he called articulation testing, offered a means for evaluating the effects of these four factors. Articulation testing consists of calling out meaningless speech sounds, and determining how well they are heard. As applied to the telephone, they call the speech sounds out into the transmitter and a typical listener or a group of listeners at the receiver end will write down what they hear. If they had called out a thousand meaningless speech sounds, for example, and they had heard 750 correctly, they'd say the percentage of articulation is seventy-five. That is, the articulation is 75 percent.

I decided to apply that technique to the hearing of speech in auditoriums to see if we couldn't get quantitative factors for the intensity or loudness of the speech (which depends on the speaker and is related also to the size of the room); for the reverberation; for the background noise; and for the shape. Well, this research project turned out to be a long and arduous task; the work continued, really, for about five years, and much of it required complete absence of noise, which meant after midnight hours. The investigation culminated in 1928, and was published in 1929 in the first issue of the Journal of the Acoustical Society of America. It was called, "Hearing of Speech in Auditoriums."

In the intervening years I had published two or three minor papers in the Physical Review, which was the only journal available to physicists at that time. But it was not as suitable a medium of publication as was the new Journal of the Acoustical Society of America, and there'll be a separate story that we'll discuss later dealing with the establishment of the Acoustical Society of America and its new journal.

This research program broke down into several parts, one of which dealt with the effects of noise. And this we could do quite neatly (beginning in 1923) with the facilities that we had at the old campus, including the apparatus that was given to us by the Bell Telephone Laboratories--which was known as Western Electric Research Laboratories in those days. Our noise research consisted of determining the effects of noise and pure tones on the hearing of speech. The subject listened to the speech in the presence of noise which he heard with a pair of earphones that were held out an inch or so from his ears. We could produce in these earphones either a typical noise, such as you have in a room--and we used several typical recordings of noise that we would reproduce in these earphones--or we would use pure tones. We determined how low-pitched tones, medium-pitched tones or high-pitched tones interfere with the hearing of speech, and of course also how typical noise interfered. And our first study on the effects of noise was published in the Physical Review. It was entitled "The Effects of Tones and Noise on the Hearing of Speech in a Room." This research enabled us to calculate how much typical room noises interfere with the hearing of speech in a room. This was the first phase of our research.

The more important phase was, how did reverberation interfere with the hearing of speech? Sabine had demonstrated that you don't hear speech well at all if the room is excessively reverberant. But it was not quantitatively established: how does the hearing of speech actually depend upon the reverberation time? If the reverberation time is two seconds, how much does that interfere with the hearing of speech? If it's six seconds, how much does that interfere with the hearing of speech?

Well, we had no facilities at the university to investigate that problem very far, except in some small rooms. But we did have these six high school auditoriums around Los Angeles that had excessive reverberation--reverberation times that varied from something like four seconds up to seven seconds. And so we conducted speech articulation tests in these auditoriums. We made all of these tests at night. They were made usually between eleven o'clock and two or three o'clock or four o'clock in the morning, because the auditorium was quiet at that time, and we wanted to be sure that the thing we were measuring was affected only by reverberation and not by background noise. So we had control of the reverberation in the experiments to the extent furnished by the six high school auditoriums, but this control was satisfactory, and we were accumulating useful data. We were given permission to use these auditoriums any time of the night that we needed, following of course scheduled meetings. We obtained very interesting data, for example, that as the reverberation time at mid-frequencies increased from four seconds to seven seconds, the speech intelligibility became poorer and poorer. We had obtained quantitative results showing how harmful excessive reverberation actually is for the hearing of speech in auditoriums. Soon after these experiments were completed I was called in to help Metro-Goldwyn-Mayer design its first two sound stages. Both of these had volumes of the order of 200,000 to 400,000 cubic feet, which is about the range of volumes we had in the high school auditoriums. We were permitted to conduct speech tests in Stage A, which is one of their first-rate sound stages. (We'll go into its acoustical design later.) But this was another extremely useful auditorium for our research because it had only 0.6 of a second reverberation time.

It was a very dead room--4 inches of mineral wool on the walls and 2 inches of mineral wool on the ceiling; only the floor was reflective. We thus obtained results in a room practically free from reverberation--and we found that if the listeners were not too far from the speaker, the articulation was very good.

We also had on the Vermont Avenue campus two halls that served as our laboratory. One was Berkeley Hall, which had a volume of, oh, probably 60,000 or 70,000 cubic feet, and Millspaugh Auditorium which had a volume of about 200,000 cubic feet. I've already described some of the results we obtained in Millspaugh, and you will recall that we got different percentage articulations there before and after absorptive treatment, and for different sizes of audience. And thus the size of the audience was another means we utilized for controlling the reverberation time. You may recall that we had added 6,000 square feet of fairly absorptive acoustical material to the ceiling. So we had results in Millspaugh under different conditions, with and without this absorptive material in the ceiling, and with different sized audiences. Millspaugh and Berkeley Hall on the old campus more or less filled the gap from four seconds down to about one second, which we had in the Berkeley Hall.

Then we had a small room. It had formerly been the men's room, but it was an inside room on the old Vermont Avenue campus, and we carried in different amounts of hair felt and other absorptive material to control the reverberation in that room. I had the help of students for much of this research, as we did in the tests in Millspaugh Auditorium where I used my first class, 1A physics students.

Little by little we had acquired a team of four or five students who were really experienced as assistants, especially trained as both callers and listeners in our speech articulation tests. Among them were Louis Delsasso, who is the brother of Leo Delsasso, who later got his PhD at Cal Tech in modern nuclear physics, followed by a distinguished career at Princeton and in national defense research. He died a few years ago. And there was W. A. Munson, who was just a sophomore student at the Vermont Avenue campus, and who later became affiliated with BTL and especially Harvey Fletcher. The Fletcher-Munson curves are even today widely accepted and used as the standard curves indicating how loudness increases at different pitches--that is, the rate at which the loudness increases with the intensity of the sound is a function of frequency. Their curves are contours of equal loudness as a function of frequency. Well, Munson got his start here as one of the assistants in these experiments on the hearing of speech in auditoriums. And of course there was always and foremost Leo P. Delsasso, who became my life-long colleague and collaborator. Both of the Delsassos remained very, very helpful assistants of mine throughout this early period here at UCLA from 1922 to 1930. We'll speak about them again in connection with the work that I did with Isaac Jones on audiometry and impaired hearing, because they were here at that time.

MINK: May I interject just one thing here, and that is this: at this point now, in this period, they were beginning to talk about the new campus.

KNUDSEN: Yes.

MINK: They were beginning to plan and to build the buildings. The buildings came along in 1928 and 1929 and were completed and ready for occupancy. What part did you play in the acoustical development of the buildings?

KNUDSEN: Yes. First of all, about 1925, I was the beneficiary of the first reverberation room in Los Angeles, which was not on the UCLA campus, but was down on Central Avenue, which was in the center of the black belt of Los Angeles at that time. The room, built to my specifications, was located at the headquarters of the Simpson Plastering Company. They were interested in the development of acoustical plaster to the extent that they were willing to build a reverberation room for me so we could make more precise measurements of the acoustical properties of acoustical plaster than we could make in the inadequate facilities we had on the Vermont Avenue campus. And this reverberation room became a sort of a prototype for a reverberation room that would later be designed at UCLA on the new campus, and it served our research very well until we moved to Westwood.

MINK: Was it designed after the one in the laboratories back East?

KNUDSEN: Very much. It wasn't as big as the W. C. Sabine Riverbank room, but it was designed much like that one. It was about 14' x 16' x 12' high. It was bigger than any room we had on the Vermont Avenue campus, but by no means as large as the Riverbank Laboratory, which I described in an earlier recording. Well, Dr. Moore believed during the planning stages of Westwood campus that the work I was doing in acoustics was of sufficient importance that good facilities for research in architectural acoustics should be provided in the new Physics-Biology Building that was to be on the new campus. Baldwin Woods, I know, also had much to do with the authorization of the new acoustical laboratory and respected the work we were doing. Ernest Lawrence also was respectful; he encouraged me very much to go on with my work in acoustics; and so these two people at Berkeley, I know, influenced the choice that we should have an acoustical laboratory to facilitate the acoustical physics that was initiated at Los Angeles. So as early as 1926 or 1927 the decision had been reached to establish an acoustical laboratory on the new campus here. It was at that time, the facility we designed, certainly the best acoustical laboratory in any of the universities of the United States. And there were not many throughout the world at that time. The U.S. Bureau of Standards had an acoustical laboratory comparable to the one provided for us here on the Los Angeles campus.

MINK: Well, I was thinking also about the acoustics of the first lecture hall, of Royce Auditorium, and the lecture halls in the Chemistry Building and in Moore Hall a little later.

KNUDSEN: Yes. For two of these, certainly, we made calculations of reverberation in advance of construction. One of these was Royce Auditorium. Dr. Delsasso and I also made a study of its shape by means of spark photography in a small model of Royce Auditorium. This was at a time when very little was known about the shape of an auditorium. The materials were selected for Royce Auditorium to reduce the reverberation to about 1.5 seconds, and we got very close to that--1.55 seconds, I think, was the value we obtained from later measurements. So we at least got in Royce Auditorium the kind of reverberation we aimed to get. This 1.5 seconds was a calculation that I made as the acoustical consultant to Allison and Allison, the architects for Royce Auditorium.

There are some acoustical defects of shape in the auditorium and some others on the stage, but from the point of view of reverberation, the knowledge we had available at that time was, for the most part, successfully utilized in designing the shape and selecting the materials which were used to optimize the reverberation. You may recall that above the wood wainscot it was acoustical plaster. It was an acoustical plaster that had been developed in Los Angeles and one that our tests helped develop. It was not the acoustical plaster that was developed at this laboratory made for me at Simpson's, but it was another similar Los Angeles-developed acoustical plaster--Calacoustic. The coffers in the ceiling were also cast of an acoustical plaster and this was cast by the technique that had been developed in the Simpson laboratory. The casting of the large ceiling coffers was done there. But these two acoustical (absorptive) materials, together with the upholstered chairs, brought a reverberation condition in Royce Auditorium that was very close to the optimal, and in this respect the acoustical outcome in Royce was highly satisfactory. The principal defect in Royce is the convergent reflection of sound from the concave rear wall, which, as was customary, followed the contour of the rear row of seats.

MINK: The upholstered chairs, then, in Royce Hall, you would say came about more for acoustical purposes than they did just to have comfort.

KNUDSEN: That's right. We urged the use of upholstered chairs in Royce even that early, even though they were not used much in auditoriums at that early date. They were considered a luxury--too good and costly for educational purposes.

MINK: In theaters, yes.

KNUDSEN: Yes. Almost always.

MINK: But auditoriums, no.

KNUDSEN: Well, even in the Pauley Pavilion they were going to have wood seats until Drs. Delsasso and Leonard and I were called in by our campus architects to help them on acoustical matters. We made calculations of reverberation and our recommendations included upholstered chairs as an indispensable acoustical requirement. And we were advised that upholstered chairs could not be used because the public would accuse the university of being extravagant and going in for luxuries. I wrote a strongly worded letter to the chancellor, declaring that the public will criticize us very much more if we have faulty acoustics in Pauley, and concluded the letter by asserting that we would publicly declare that we had nothing to do with the acoustical design of Pauley Pavilion if they do not use upholstered chairs. This letter was written after I had retired as chancellor. The letter, I am told, was handled by Bill Young, who was the vice-chancellor in charge of building at that time. He realized that we weren't talking through our hat, that this was really a prime requirement. Well, today as you know, it's not only better acoustically for that reason, but it's a much more useful place altogether, and I haven't heard a single criticism about the expense of those upholstered chairs.

MINK: To come back to Royce Hall Auditorium, when it was being designed as a shell structure, was it known--I don't think it was--that there would be an organ in there? Now, did this figure into your calculations?

KNUDSEN: No. It did not. This came later; and I did know later that they were going to install an organ. They had to use such space as was available, and it is not the best space for an organ. It's not only above the stage but it projects out over the audience for the first two rows of coffers, and as you probably know, that's at least twenty feet. I think those coffers are approximately 10' x 10', at least that size, so that when you listen to the organ there now I think you often feel--especially if you're sitting in front--that the sound comes down at you too much and not toward you, or surrounding you. The directional effect is not all it should be, although it's a rather good organ, and Alexander Schreiner, the organist, enjoyed the organ.

MINK: Very much, I understand.

KNUDSEN: Yes.

MINK: Were you happy with the shape of Royce Hall? Did you feel that the shape was fixed by the way that the classrooms were designed around it? Did this limit you in your ability to provide the kind of acoustics you would like to have provided for that auditorium?

KNUDSEN: Well, the ceiling was higher than we wanted. For one thing, we therefore decided a coffered ceiling, such as we have, was necessary. Otherwise the reflections from the ceiling would be delayed enough to produce an interfering reflection, almost an echo. And so this dictated the design of the ceiling. The coffers are deep, as you know. They are rather prominent coffers. And this feature was for an acoustical reason, to facilitate diffusive rather than "specular" reflection. The principal fault acoustically was, and is, as I said, one that was perpetuated not only at that time, but has continued until relatively recent years--the rear wall is concave toward the stage. The center of curvature of that rear wall is near the rear wall of the stage, so that sound originating in the front of the auditorium, and especially on the stage, will be reflected convergently from the rear wall and brought to a focus, or near a focus, in the front part of the hall. It's delayed enough to be heard as an echo, so if you clap your hands on the front of the stage, you hear a sharp echo that comes from the rear wall.

MINK: Was there any reason why they had to make that wall concave?

KNUDSEN: Visually, yes, and architects have long been prone to place sight above sound, and therefore the rear wall conforms to the rear row of seats. This practice was based on a long, long tradition of having the rows of seats concave toward the front. It gives a little better visual effect--that is, the sight lines are a little better for the side seats.

MINK: You could still have the seats concave and have the wall straight in back.

KNUDSEN: That is right. We often do that now, but since 1960 I have persuaded many architects with whom I consulted to use a series of large convex surface (usually three) for the rear wall.

MINK: Why didn't they do it then?

KNUDSEN: The acoustical emphasis was on reverberation at that time.

MINK: Still then?

KNUDSEN: To get the right reverberation was then the acoustical requirement of highest priority. Shape was something that really developed later.

MINK: But you had already indicated through your research that reverberation shouldn't be as important.

KNUDSEN: Well, I had shown the effects of reverberation, noise and the loudness--these three things. I had not investigated shape.

MINK: I see. You told me something about the acoustical properties of room 39 [Haines Hall 39]. I think it's 39, the big lecture hall in the old Chemistry Building.

KNUDSEN: Yes.

MINK: And what was used there?

KNUDSEN: Yes. This was the principal lecture hall on campus, and I was consulted about the materials they should use there. There was a plaster that had been developed in Salt Lake City, Utah. It's called nephi acoustical plaster. Nephi is a good old Mormon name. Nephi and Lehi are names of two cities in Utah, named after two important characters in the Book of Mormon, and these names are familiar ones in Utah. That has nothing to do with this acoustical plaster, except that it bore Nephi's name. The principal aggregate in this plaster was a dried and fibrous thistle weed that grew on salty soil. But it also was a rather porous weed, which was mixed into the binder of lime or gypsum--whichever was used. I think it was a lime base that was used in this nephi plaster. But the fibrous weed made the plaster somewhat absorptive. I think we used to call this hall room 29. Maybe it's been named 39 now, but it seems to me it was Chemistry 29.

MINK: We can get that.

KNUDSEN: All right. That isn't important. But at any rate, it was the main auditorium in that building, and it was somewhat bigger than the main lecture hall in the Physics-Biology Building here. The Chemistry lecture hall was the room we used a great deal here, for faculty meetings and public lectures, as well as the largest campus lecture hall.

But we had more to do with the design of the physics lecture hall. It had acoustical plaster but not nephi plaster. I think it was calacoustic plaster. We also gave considerable thought to shape in our own lecture hall here, in respect to both seeing and hearing. For example, Dr. John M. Adams designed the seating so that the top of the instructor's demonstration table was visible from all seating locations; those good sight lines also made good sound lines. The two different acoustical plasters had been tested in the laboratory we had over on Central Avenue.

MINK: I take it you weren't so much worried about the chemistry people then?

KNUDSEN: There were different architects for the buildings. Allison and Allison were the architects for the Physics-Biology Building and George Kelham of Berkeley, who was the chief campus architect at that time, was the architect for the Chemistry-Geology Building. And Kelham didn't listen to Vern Knudsen nearly as much as Allison and Allison did. I was told, I believe by Professor G. Ross Robertson of Chemistry, that Kelham regarded me as just a young upstart at that time.

MINK: Did you talk with Kelham and discuss this problem?

KNUDSEN: I think by letter.

MINK: By letter?

KNUDSEN: Yes, correspondence; at least I don't remember talking with him. But he did consent to use this nephi plaster. The texture of it happened to please him.

MINK: What was it that Kelham said about acoustics?

KNUDSEN: Well, we were talking about making some acoustical adjustments in Royce Auditorium after it had been completed. We realized that we were having some difficulty there, and this came to me secondhand, as I said, I believe from Ross Robertson. Someone had told Ross Robertson. Kelham had said, "You know, that young Knudsen doesn't know anything about acoustics. The acoustics of the room are determined primarily by temperature, humidity, and differences of temperature." And, of course, these have almost nothing to do with acoustics. Humidity later was found (here in our laboratory) to have a little something to do with acoustics, but certainly not the crucial role that Kelham was thinking of at that time. Probably he had heard of refraction of sound in which a negative temperature gradient would cause sound to bend down, but the actual amount of temperature gradient you have in a room is so small that it can't have any appreciable effect on the acoustics of the room.

Room 29 or 39 in Chemistry did have this nephi acoustical plaster on the upper walls and on the entire ceiling. And the reverberation was not bad there, but this room suffered very much more from the noise in the ventilating system than from anything else. I have at least a half a dozen letters in my file in which I complained about the noise from the ventilating system. We had conducted these experiments on the effect of noise on the hearing of speech, and it was so obvious that the difficulty they were having in this lecture hall in the chemistry building was attributable to the noise from the ventilating system.

MINK: Was it ever corrected?

KNUDSEN: I think maybe twenty years later.

MINK: It must have been, because I had lectures there where I could hear Dr. Bjork very well.

KNUDSEN: Well, yes. It was finally corrected.

MINK: Some of the other large buildings on the early Westwood site, large rooms I mean, the library, the reference room and the rotunda in the library--now I don't suppose that much was ever done in regard to those?

KNUDSEN: Nothing. We were not consulted at all about them. And when the library was completed we did go over and make some measurements of reverberation. As I recall, the large reference and reading room had between 11 and 12 seconds of reverberation.

MINK: In the large reference room?

KNUDSEN: Yes, in this large reference room. It was very, very reverberant.

MINK: Because you have the rotunda outside there acting as sort of a. . .

KNUDSEN: Well, the rotunda, somewhat, but it was largely a matter of the huge volume--the very high ceiling and its great length--the rotunda had something to do with it; but the principal "boner" was that the materials were all hard. There was no absorptive material used at all in this very large room.

MINK: Much glass?

KNUDSEN: A lot of glass and a lot of hard plaster and tile floor and hard plaster ceiling. There were those little paintings in the ceiling, you know, an interesting collection of famous bookmarks. They were painted by a professional artist on concrete. So it was a very, very reverberant room, and this aggravated the noise in the room a great deal. And a fifteen minute calculation in advance of construction would have predicted this baneful outcome in acoustics, and also would have indicated how much absorption was required to correct the excessive reverberation.

MINK: By this time had you begun in any way to collect fees for outside consultations?

KNUDSEN: Yes, I had. I think as early probably as 1923 the Los Angeles Board of Education not only made these rooms available to me, but when they heard of the results, they asked me to speak before a meeting of the board. They had a number of other people present, and I discussed some elementary principles of acoustics and how reverberation was interfering with the hearing of speech in their high school auditoriums. And they asked me, "What can you do about it?" And I said, "Well, we can make the calculations of what should be done to improve the acoustics of these rooms by reducing the reverberation." And so the first fees I received were from acting as consultant on acoustics for five of these six auditoriums. I don't remember about the sixth, but I know I gave a contract price. I think my fees ranged from $100 to $200, something like that, per auditorium for these first ones.

MINK: That's very modest.

KNUDSEN: In terms of fees today, yes. But in terms of what $100 or $200 would buy then, it was at least as much as five times as much would buy today.

MINK: That was the going rate?

KNUDSEN: Well, almost. Many of my friends and associates told me I didn't know how to charge.

MINK: Now the first building that you ever collected a fee for actual advice in its design before its construction--not improvement of the existing structures--was the sound stage that you worked on?

KNUDSEN: Apart from these five Los Angeles high school auditoriums--no, I had consulted on probably twenty or twenty-five school auditoriums, churches and theaters in other parts of California. There were several churches, as my record would probably show; but there were as many as twenty or twenty-five new or existing auditoriums for which I had consulted at the time I was called in at Metro-Goldwyn-Mayer when sound first was introduced to the motion picture world.

But let me go back a little further to the quantitative aspect of my consulting work and research, because this led to a formula which is used even today for calculating the percentage articulation for any auditorium. And this formula is PA = 96 $k_l k_r k_n k_s$, in which "PA" stands for percentage articulation. I'll explain the "96" a little later. The PA is 96 x 4 reduction factors k_l, k_r, k_n, k_s. These subscripts were l for loudness, r for reverberation, n for noise, and s for shape.

Research during this period beginning in 1923 until about 1928 culminated in the publication of the article entitled, "The Hearing of Speech in Auditoriums," which was published in the first issue of the Journal of the Acoustical Society of America. It was based on the data and the results obtained in many of these rooms that I have mentioned; but my research and consulting work brought together my findings in the formula for calculating the percentage of articulation in auditoriums. Well, why the number "96?" The best you can do under the most favorable listening circumstances is 96 percent. If I'm talking to you from a distance, say, of two feet and about as loudly as I am now in this room, and I called out meaningless speech sounds, the best you could do would be to hear correctly 96 percent of them. You would still miss some if you weren't watching my lips; and in all of these tests we did not permit the listener to watch the lips because you can see the difference, say, between "f" as in "fin" and "th" as in "thin."

One reason you hear so well in the motion picture theater is that the projection on the screen often "blows up" the face to a very large size, so that you see all the facial expressions, and you can recognize these consonant sounds--they're called the unvoiced consonants--that have almost no acoustical energy compared to the energy of the vowels. The vowel preceding the consonant may have more than 100 times as much energy (or intensity) as the consonant which follows it. And in an auditorium the reverberation of these vowels will almost always mask the succeeding consonants. I'll give you a reprint that you can review if you wish further information. It'll give you more of the details. The first one, as I mentioned, is called "The Hearing of Speech in Auditoriums," published in volume I of the *Journal of the Acoustical Society of America*. I think that'll do for the reference now. The second one is my faculty research lecture which was called "Modern Acoustics and Culture," and there's a table there that indicates the kind of errors of speech that occur in an auditorium. The table, for example, is given here on one page of this reprint.

MINK: That was what year?

KNUDSEN: This lecture was delivered May 6, 1936. It came soon after I won the American Association for the Advancement of Science prize. One of the members of the Faculty Research Lecturer Committee told me, "At last we are able to appoint you the Faculty Research Lecturer." Certain members of the committee wanted me earlier than that.

MINK: So this more or less represents the culmination of all this early research in the acoustical properties of auditoriums?

KNUDSEN: Yes. Pertaining to the hearing of speech, this early research culminated in a means for calculating the effects of reverberation and noise, and of the loudness of the speech (whether amplified or not). The 96 factor in the formula had been determined by Fletcher at BTL, and was confirmed by our own experiments. It is less than 100 (percent) because there are imperfections in our spoken language. We could design an esperanto of some sort in which you wouldn't have sounds like the unvoiced consonants that are responsible for nearly all the imperfections.

MINK: That's discussed on page nine.

KNUDSEN: Yes. On page nine of the faculty research lecture of 1936; the table there shows the percent of errors for the different consonant sounds, and how these errors occur; how many times "m" is heard as "n," how many times it's heard as "ng," how many times "f" is heard as "th" and so on; and so the typical errors of speech are listed here. If anyone is going to design an esperanto of some sort these data would be useful for that purpose. And they came out of this original investigation--just which sounds are missed. The vowels are almost always heard, but it's the final consonants that suffer most in the hearing of speech in auditoriums.

MINK: Well, when you come to getting the factor 96, this requires a great deal of testing, of dictating speech sounds, and having people listen to speech sounds.

KNUDSEN: Among our other tests, we did these in the desert. We went out where it was perfectly quiet, and we conducted our tests when there were no noise making insects. We did a lot of work like that. The Bell Telephone Laboratories had done extensive work of that sort, and so they and we both found that under the most ideal listening conditions the PA for typical English speech is 96 (percent).

MINK: This was a joint agreement?

KNUDSEN: We arrived at it independently. They use it now in telephony, and we use it in architectural acoustics--this 96 factor.

Now there's the problem of the k_1 factor. This was, I think, rather interesting and maybe we should discuss that just a moment. Different speakers generate different amounts of acoustical power in their speech. And to investigate this we used both Millspaugh Auditorium and Berkeley Hall on the old campus. I had a microphone hung up in the room unknown to the lecturers in the room. The microphone was connected to some outside listening apparatus, potentiometers and so on, with which you could match the loudness of their speech against a calibrated reference speech. And by this means I could determine just how much acoustical power (the average power in watts) they generated in speech.

We had a total of sixteen different speakers. There were six in Berkeley Hall and ten in Millspaugh Hall. I could hear the lectures, and I'd adjust the loudness or intensity level so it would match the standard reference level that I had in an anteroom. The average acoustical power for each of these sixteen speakers ranged from 4.2 to 152 microwatts.

Now think first of all how small that acoustical power is. One speaker generated only 4 microwatts, another 152 microwatts. Well, these were data we needed to know. They gave us average values, and we used these average values to determine this factor k_1.

Fletcher's work at the Bell Laboratories had indicated how the PA depends upon the sound level, and that a level 70 decibels above threshold is the optimal loudness. If it gets louder than that you begin to get some distortion because you overload the ear. If it gets lower than that you fail to hear because the consonants become inaudible. That's the thing that goes out first of all. And so we wanted to know, well, how many decibels above threshold does an average speaker go? Well, it turned out in large auditoriums that it was more like 50 instead of 70 decibels, so we knew we were greatly deficient in the amount of loudness of the average speaker.

The 152 microwatter can be heard pretty well, but I predicted that the 4 microwatter could not be heard beyond the fourth row in Royce Auditorium. This was before we had a sound amplification system. And at a later date this person spoke there. I'll keep the name silent; there's no point to introducing that now. But he gave a lecture in Royce Auditorium, and I don't believe he was heard beyond the third row. I had made a slight mistake in predicting that he could not be heard beyond the fourth row. But our finding was characteristic of different people's voices. Some people (former President Robert G. Sproul, for example) can speak in Royce Auditorium so that you can hear distinctly at all seats, with the use of sound amplification. I was in there yesterday during part of a super-senate meeting, you know, representatives from the nine campuses. If a speaker was on the stage and he faced the audience--I was sitting more than halfway back--you could hear satisfactorily, and it's because the reverberation is right there and there's no special noise, and for that particular use it's all right. But for other uses it's not so good.

The point here is that there is an inadequate amount of sound energy available in the typical voice to hear in a large room. Normally this led to the conclusion that if a room is larger than about 25,000 cubic feet you need amplification if you're going to get up to where the speech is adequate.

At the time I began my research in 1923 speech amplification was just beginning to be used for supplementing the poor acoustics of halls, and it is, of course, necessary in large halls, especially if they also are noisy. You must get well above the background in order to hear adequately the consonants of speech.

Well, the work on reverberation led to the conclusion that there's an optimal time of reverberation. This optimal is different for different sized rooms. For a small room it's as low as 0.6 or 0.7 of a second. For a large room, and for the unamplified voice it may be as high as 1.5 to even 1.7 seconds, because the increased reverberation results in an increase in the loudness of the voice. And so the optimal time of reverberation is that time for which a decrease in the reverberation time decreases the loudness or intensity so that the two effects are compensating. And so these curves in the faculty research lecture show this effect.

On pages six and seven we have curves showing how well you can hear speech in rooms of different size with different times of reverberation, when there is no noise present. And on page seven the curves also show the effect of amplification. The curve at the top on page seven is when the speech has been amplified up to the optimal loudness, and the curve shows that in a typical room, free from excessive reverberation, you can get an articulation of 90 percent. But, you see, when the reverberation time is 7 or 8 seconds and the speech isn't amplified, the speech articulation is between only 40 and 50 percent, which means you just couldn't understand speech at all in these high school auditoriums that had reverberation times of that order of magnitude.

MINK: So that's why they were calling on you.

KNUDSEN: That's why they were calling on me, but they called on me long before our early research on the hearing of speech in auditoriums culminated in these curves we have been discussing today.

TEACHER, RESEARCHER, AND ADMINISTRATOR

Vern O. Knudsen

Interviewed by James V. Mink

VOLUME II

Completed under the auspices
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Oral History Program
University of California
Los Angeles

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VOLUME II

SECTION XIV

MINK: You said that you were going to speak about your early research and coming to the Southern Branch.

KNUDSEN: Yes. This really supplements what we recorded a year ago, particularly in the beginning, and then I'd like to say some things at the end which we didn't have time to record.

MINK: This is the recording that you and Dr. Delsasso made?

KNUDSEN: No. This is the one of last week, on the hearing of speech in auditoriums.

MINK: Our own recording?

KNUDSEN: Yes, our own recording. I'd like to supplement that by what I had intended to say last week, but I didn't have my notes before me, and I therefore omitted some details, and also some summary comments regarding how my past experiences influenced future choices and accomplishments in teaching and administration, as well as in research.

First of all, one's choices in life--indeed, one's future--are almost always a projection or an extrapolation, as we would say in physics, of the past. Certainly my associations with Harvey Fletcher as my professor at Brigham Young University and later as a colleague of his at the Bell Telephone Laboratories, where he was in charge of the entire research program in speech and hearing acoustics; my experience with vacuum tube circuits at the Bell Telephone Laboratories; and the PhD dissertation utilizing the then new vacuum tube techniques--these were all, of course, things that led to research with Dr. George E. Shambaugh at the University of Chicago and the two papers we published: "Sensibility of Pathological Ears to Small Differences of Pitch and Loudness" and the "Investigation of Ten Cases of Diplacusis." Of course, I've also referred to the Collected Papers on Acoustics [1922] by Wallace C. Sabine. And in the forefront of my past had been, as I mentioned earlier, the guiding influence of religion and evangelism. And later in life, of seeking a cure and a rationale for my duodenal ulcer, which had plagued me from 1922 until 1950, in the course of which I sought the therapy of a psychoanalyst. I had, I think, three or four sessions--that was about all I could take. Her conclusion was that I had a "Jesus complex"--whatever that is. I'm not sure. She thought I wanted to be a perfectionist after she had thoroughly been convinced that this wasn't a sex problem that had brought on my duodenal ulcer and one or two of the other things that psychoanalysts pursue. She said, "Well, your difficulty is that you have a Jesus complex." I wasn't convinced or cured. About ten years later, in 1950, surgery by Dr. William P. Longmire, chief of surgery at UCLA, resulted in a cure that probably enabled me to continue as dean of the Graduate Division, to take on the additional tasks of vice-chancellor, and ultimately to serve for one year (which was long enough) as chancellor.

MINK: Do you remember at what point in time it was that you visited the psychiatrist?

KNUDSEN: Yes. This was in the late 1930s, probably 1939.

MINK: Well, maybe this was after you had begun with the graduate division, which we'll be talking about later.

KNUDSEN: Yes. There was the graduate division, which began in 1934 and continued until 1958; there was the writing of Architectural Acoustics, my first book, published in 1932; there were six years (1932-38) serving as chairman of the Department of Physics, at a time when I was really working harder than ever in my life. I was on a schedule of at least sixteen hours a day of work, much of it at research. There's no doubt that these things had something to do with the ulcer. I didn't believe the conclusion of the psychoanalyst that I had a Jesus complex, although I believe that my choice of a teaching career, and the type of research that I have conducted throughout my career, were greatly influenced by my interest in man. And probably my religious and evangelical activities [laughter] had something to do with that. The contributions of electrons to specific heat of metals couldn't interest me as the subject of a doctoral dissertation even if I had been capable of doing that work. It lacked the human appeal I had found in probing the problems of how we hear and especially the problems of the hard of hearing.

So, the foregoing supplementary comments furnish a little background that I thought we should probably add to the things I said last week. [tape off]

In regard to the discussion of last week on the hearing of speech in auditoriums there are three figures in that 1929 article that I want to discuss for the record. The entire article may serve as a supplement or an appendix to this discussion. There are three curves and a table in the article that have such wide application in the field of acoustics and even in the entire use of the human voice as it is heard in lecture halls, theaters, or even in ordinary conversation, that I think it should be emphasized.

On page 71 of this article, figure 6 shows a group of curves that give the probable percentage of articulation in auditoriums of different sizes and with different times of reverberation. And these curves indicate that there is an optimal time of reverberation for hearing of speech in auditoriums of a certain size. These curves include auditoriums that vary in size from--well, it would be a small lecture hall of 25,000 cubic feet up to a very large auditorium of 1,600,000 cubic feet, which is about twice the volume of the Dorothy Chandler Music Pavilion at Los Angeles. And an interesting thing about these curves--this is for unamplified speech in these rooms. Most of this work was done before amplification was used in auditoriums.

The optimal reverberation time, I believe I mentioned last time, increases with the size of the auditorium. It's only of the order of .6 or .7 of a second in a small room, but it goes up to around 1.5 seconds in the very large rooms. The reason for that is that you tolerate some reverberation in a very large room because the person has to speak louder, and the voice is down so much even though he speaks louder that the boost that you get from an additional reverberation means better reflection from the boundaries of the room. Therefore, the optimal time of reverberation, which is a very important thing in controlling the acoustics of a room, is a function of the size. And that gives rise to a series of curves that I reproduced in this article on page 75, figure 8. There are three curves there that deal not only with the optimal reverberation for the hearing of speech for rooms of different size (volume), but also the optimal reverberation for music rooms, and for rooms that are used for both speech and music. Although this publication was dated 1929, you'll still see these three curves that are shown on page 75 in almost every standard work that deals with architectural acoustics in any language.

There are three curves here. The upper one gives the optimal time of reverberation for music, an average for music. It's different for different kinds of music, as we may discuss later. The bottom one is for speech, and the bottom one for speech was based entirely upon the results obtained in this five-year investigation of the hearing of speech in auditoriums. And that curve has stayed pretty much as it is throughout the years since then. This curve gives the values of the optimal time of reverberation in rooms that are not equipped with sound amplification systems. The middle curve on this figure I'm referring to here is for speech and music or multipurpose auditoriums.

These values have guided the acoustical design of many rooms. You can calculate the reverberation time of a room in terms of its volume and the reflective and absorptive properties of the materials that comprise the wall, the floor and the ceiling, and especially the audience itself, whether you use upholstered chairs and things of that sort. But these are routine calculations, and whenever an auditorium is designed, if it's for speech alone and it's a room where they're not using amplification, then this lower curve would be used. If it's a multipurpose room for speech and music, as are most school and civic auditoriums, including even our music center downtown, the reverberation values that are given here in the middle curve are pretty much the ones that are used today. On page 76, figure 9 shows a group of curves indicating how the loudness of a speaker's voice affects the hearing of speech in auditoriums. Five of these curves are for unamplified speech, for five categories of speakers, depending on how loudly they speak. The weakest speaker in this series that I reported last week generated only 4 microwatts, and he could be heard, I predicted, only on the third or fourth row in Royce Auditorium. This was nearly verified later approximately when he gave a lecture in Royce Auditorium. And then there's a classification for a moderately weak speaker, for an average speaker, for a moderately loud speaker, and for the loudest speaker in this series. I think I might mention his name, Frederick C. Leonard, a professor of astronomy who died sometime ago.

MINK: Oh, I thought it would be Robert Gordon Sproul.

KNUDSEN: [laughter] I didn't get him in this series, but no doubt, he would be the winner--I'd wager he is at least a 200 microwatter! My subjects were just faculty lecturers on the old UCLA campus--in Berkeley Hall and Millspaugh Hall. This was before Sproul was president.

The interesting thing here is how much this varies with different speakers. For example, for the weakest speaker in this series, and this is for an auditorium having a volume of 400,000 cubic feet, the best he could possibly do is an articulation of 70 percent, whereas Dr. Leonard, who was the astronomer who had the strongest voice in the series that we investigated at that time, would be able to maintain an articulation of 83 percent, which is pretty good. The range here from 70 to 83 percent is really very significant, and it shows how important the loudness of the speaker's voice is in being heard in an auditorium.

Then there's this table here. Table 4 on page 81, or its equivalent, really should be used in the teaching of language and phonetics and how to use the voice so that it will be better heard, because it shows just which speech sounds are missed most frequently in rooms. The table was based upon many thousands of speech sounds that were called out in a typical auditorium, and the table gives a classification of the errors. For example, how many times "b," say, is heard as "ch" or as "d" or as "f" or as "g" or as "h" or as "k" or as "m" or as "p" or as "s" or as "sh" or as "v." Now "b" is very often heard as "v." The letters "b" and "v" are phonetically very much alike. They are confused quite often. The letter "d," for example, was misheard as "g" more frequently than any other consonant. That was the most common mistake there, "d" into "g."

I'll go down the line. The most frequent error of all was the confusion of "ng" into "n" or the reverse, the confusion of "n" into "ng." These stand out. This, of course, has a significance on the use of language. The public speaker or the lecturer should know how frequently these few sounds are misunderstood, particularly the final consonant, and a deliberate effort should be made on the part of the speaker to distinguish between "n" and "ng," to distinguish between "d" and "g," to distinguish between "b" and "v," and between the soft "th" and "f." These are the ones that are most frequently misunderstood. Or if we were going to establish a language in esperanto of some sort, then the information that we have in this table would be a useful aid in determining which sounds we should abandon and replace with suitable substitutions.

MINK: While we're talking about big auditoriums, I think you said, off the record, something about Pauley Pavilion, and I wondered if you wanted to put that on the record. At what point were you consulted in the design of acoustics for Pauley Pavilion?

KNUDSEN: Well, I was consulted at first very casually and very informally, during the early phase of its design. The whole problem of the acoustical design of Pauley Pavilion was a very casual affair, I think, on the part of the architect in the designing of the pavilion. Paul Veneklasen, a former student of mine, did quite a lot of consulting work for Welton Becket and Associates--the architect. Veneklasen told me that just as he was leaving the Becket office one day, someone asked him, oh, a few questions about Pauley Pavilion. Veneklasen made a few casual replies about what should be done, and he briefly told me about this casual incident. Much later our own local office of architects and engineers did ask Delsasso, Leonard, and me to more or less review what had been planned for Pauley Pavilion.

MINK: Will you tell me what your student suggested ought to be done?

KNUDSEN: He recommended absorptive treatment for the ceiling and the upper walls--the walls above the balcony there.

MINK: Is that all he recommended?

KNUDSEN: That's primarily what was recommended at that time. The shape for Pauley Pavilion, which is approximately rectangular, is better than most stadiums of that sort. It's much better, for example, than the Sports Arena downtown.

MINK: It's a large oval shape.

KNUDSEN: That's right--oval in shape--and the resulting concave surfaces, as we shall show later, are often very great offenders. The principal contribution that Delsasso, Leonard and I made--we recalculated the reverberation times for Pauley Pavilion and decided that even more absorption was needed than the architects' plans had called for. Different types of absorptive material were being considered then by our own Office of Architects and Engineers, materials that had different absorption characteristics. And we made calculations on the basis of these different materials and indicated which materials we thought would be best. And they had to do with, oh, choices between one kind of acoustical tile and another or one kind of acoustical board or blanket and another, protected by a "transondent" facing of some sort that would allow the sound to penetrate into the absorptive material (such as mineral wool) behind it.

But our biggest contribution, as I have related, had to do with the choice of upholstered chairs. And there had been a decision at the time we were called in on this project that upholstered chairs could not be used because the public would accuse the university of going in for luxury. We knew that Pauley Pavilion would be defective acoustically, especially when small audiences are present, if they did not have upholstered chairs. And I was so determined that they should have upholstered chairs in Pauley Pavilion that, as I told you earlier, I wrote a letter to the chancellor's office at the time and said, "Unless upholstered chairs go in this auditorium, I'll make a public declaration that Knudsen, Delsasso and Leonard had utterly nothing to do with the acoustics of this hall, and if our advice is disregarded we can assume no responsibility for the outcome in acoustics." And I also said in this letter, "The university will be criticized much more for faulty acoustics if we go ahead without upholstered chairs and with just wooden chairs, than it would be for spending money for upholstered chairs."

I cited large auditoriums around the country where upholstered chairs had been used and where they had not been used. They were in trouble everywhere they had not used them, and they were in not so much trouble where they were used.

Chancellor Murphy was absent at that time, but Vice-Chancellor Young, who was in charge of campus development and the building program, recognized the importance of our recommendations. They were based, in part, on this hearing of speech in auditoriums that would show how poor the actual hearing of speech would be with only a small audience. With 1,000 people present, with 5,000 people present, and even up until you get to 10,000 people, it would be quite a serious defect not to have upholstered chairs, because when you have as many as 12,000 or 13,000 upholstered chairs, they contribute a great deal of sound absorption, and therefore contribute very much to the optimal reverberation for all sizes of audience.

MINK: And you're talking about the use of a loudspeaker system here.

KNUDSEN: Yes. That's right. The loudspeaker is indispensable, but not sufficient. This auditorium was around 4 million cubic feet in volume, as I remember. I'm sure it's not less than that. Maybe it's 5 million cubic feet. And you just couldn't get by at all without amplified sound. You see, from the curves I showed you, we couldn't get by in a

400,000 cubic feet auditorium without good amplification. And when you have adequate amplification, as our data showed, the optimal reverberation time is below 0.1 second.

MINK: And what is the revereration [time] in Pauley Pavilion?

KNUDSEN: It's a little more than 1.0 second, about 1.3 or 1.4 seconds, but the cost of pushing it down below one second would be prohibitive. As I explained, the curve for how well you hear speech as a function of reverberation time is pretty flat between 0.7 of a second and 1.5 seconds, and it's only when you get above 1.5 seconds that you would really begin to have a significant loss of the hearing of speech in that room.

MINK: One other auditorium in Los Angeles is the Shrine Auditorium, which was built, oh, in the late 1920s, I believe, or early in the 1930s. Did you have anything to do with the acoustics in that auditorium?

KNUDSEN: No, I did not. Thank God, I did not.

MINK: Have you anything to say about your involvement with the auditorium?

KNUDSEN: Well, I dislike the place very much. Although I love opera very much, for the last five or six years Mrs. Knudsen and I have refused to go to opera at the Shrine Auditorium. We go to San Francisco occasionally to hear the San Francisco Opera Company. We go up there at least once nearly every fall. And we have opera in the Music Pavilion in Los Angeles. We're interested in going there, and we are familiar with the opera houses in Europe and New York. But for music, visually as well as acoustically, the Shrine is not a good hall. It's not quite so bad when you have a large audience. But the criticism there is primarily a matter of shape. The floor is so flat that you can't see or hear very well unless you're sitting in the front few rows; seeing and hearing became increasingly worse as you move toward the rear of the house. And so you're always ducking from one side to the other to see what's going on. You scarcely see a ballet at all. If it's ballet, you miss seeing the feet, unless you're sitting right on the front rows or up in the balcony. The balcony people can see the floor of the stage, but most of the people on the main floor cannot. And even for opera, that's visually an undesirable thing.

It's, of course, altogether too big for the best production of opera, especially without sound amplification. They've never had a good sound amplification system there. They've had inadequate little cone speakers set up sometimes on the sides of the proscenium, but I've never known of an adequate sound system in Shrine Auditorium. They could improve this large auditorium for opera if they'd use high quality amplification. I have no objection whatever to large halls. I think it's far better to have amplification in a large hall to see opera if that will pay for the opera, than to try to crowd into a small auditorium. But if the auditorium is larger than about 100,000 cubic feet, as we have indicated in the last two discussions, you must amplify the sound somewhat.

This is necessary in the Los Angeles Music Pavilion. When they did Salomé there some time ago they didn't use amplification, and we missed many words. And in the Hamburg Opera House, which is much smaller, you don't need sound amplification. I happen to have heard Salomé twice over there. I could hear the words even in German. My German isn't very good, but I could understand more of the words when I heard it in Hamburg than I could in the Dorothy Chandler Pavilion where it was sung in English. The singing on the stage was masked so much by the orchestra in the pit that you lost many of the sung words, especially in the scene where Salomé is singing down on the floor. That's the worst possible position for projecting the unamplified voice out into the auditorium.

MINK: You meant in the [Los Angeles] Pavilion?

KNUDSEN: In the Pavilion. [tape off]

The work on the hearing of speech in auditoriums that I have reviewed was published now nearly thirty-six or thirty-eight years ago. We talked about these various reduction factors that interfere with the hearing of speech--reverberation time and noise and so on, but we used single values for the reverberation time. This was the reverberation time at a frequency near 500 cycles per second, then we used 512 cycles per second. And we spoke of noise as a single value. Now reverberation is a function of frequency, and so is noise. And much work has been done in other parts of the world introducing these desirable improvements. We have not pursued these improvements here, but in South Africa, in Poland, in Czechoslovakia, in Germany, and in other parts of the United States, extensions of this early work of ours have been carried on by others. In these later researches, instead of using a single value for the reverberation or for the noise, the investigators considered the effects of reverberation and noise, as they vary with pitch.

It is known from the work the Bell Telephone Laboratories and others have been doing over the years just how much interference, say, a reverberation time would have, because it acts like noise, and so you want to know how much disturbance there is from noise at low pitch, at medium pitch, and at high pitch. And the work that has been done in recent years has been particularly in that field.

I've been in communication with a Dr. Kolmer who is at Prague, Czechoslovakia, and he and a group have been working on the hearing of speech in auditoriums now for two years. We will soon receive a publication from him, and he has already indicated that it makes only minor modifications in the results that we had published way back in 1929 based upon using a single value for the reverberation time and a single value for the noise. We knew that we were making approximations at that time, but single values (those for 512 cycles, which we used) turned out to be rather good ones. A lot of work has been done in these various laboratories throughout the world to make the calculations of how well you can hear speech in an auditorium when you know and use these better characteristics about noise and reverberation. We haven't talked much about shape; we'll talk about it when we discuss acoustical designing for music, because it will be more significant there.

MINK: You are finished, then?

KNUDSEN: Yes. [tape off] In the research work I did in connection with my doctoral dissertation at the University of Chicago on the effect of the sensibility of the ear to small changes of pitch and loudness, as I have related, Dr. Millikan made me acquainted with Dr. George E. Shambaugh, who was then regarded as the dean of otologists in the United States. This collaboration led to the publication of the two articles that we have already reviewed. And when Shambaugh knew that I was coming to Los Angeles and to the Southern Branch of the university, he said, "By all means, you should meet Dr. Isaac H. Jones. He is the leading otologist in Southern California. I think you will like him, and you should form a partnership with him."

I knew at that time that there had been some work between physicists and otologists--people that I knew as acousticians and had heard of as otologists, had worked together. For example, at the University of Iowa, Dr. Lee Wallace Dean, a distinguished otologist, had worked with a Dr. Bunch, a physicist, and they had developed a type of an audiometer which consisted of a saw-toothed disk that rotated in a magnetic field, and this was an improvement, of course, on the use of the tuning fork which otologists previously had used for testing hearing. This was a very slow and cumbersome and inadequate way of testing hearing, and I think this collaboration between Dean and Bunch at the University of Iowa was perhaps the beginning of this kind of collaboration.

And then a Dr. John C. Minton, who was also a former Bell Laboratories man and was at the University of Chicago at the same time I was, worked on problems of hearing also. Minton made the acquaintance of another otologist in Chicago by the name of Gordon Wilson. Wilson and Minton did some work on charting hearing with essentially an audiometer of the type that had been already developed under the direction of Harvey Fletcher at the Bell Telephone Laboratory. And Fletcher himself had collaborated with an otologist in New York by the name of Edmund Prince Fowler, who was certainly the most distinguished otologist in the New York area. And Dr. Fowler continued to be distinguished in this field throughout his life. He is not living any longer, but his son Edmund Prince Fowler, Jr. is also an otologist, a distinguished one in New York, just as George E. Shambaugh, Jr., the son of the George E. Shambaugh I worked with, is now a distinguished otologist at the Rush Medical facility at the University of Chicago.

Well, there was this background of collaboration, and Shambaugh and I, of course, had worked together on these two projects which I have reviewed. He felt that this was the type of collaboration that should be continued and that Jones and I would make a good partnership to carry on work of this sort. So soon after I arrived here I called on Dr. Jones. I don't have a diary, and I can't give the exact month, but it was probably no later than November of 1922 and no later than January of 1923. I know we were going strong early in 1923. I had called Dr. Isaac H. Jones by telephone, and he invited me to come over to his office. It was near MacArthur Park now on Wilshire Boulevard, just a block east of MacArthur Park, and this was the beginning of a very pleasant association.

I was attracted to Isaac H. Jones at once, and he was attracted to me. He's a charmer of the very first order and also a humanist of the first order, and so there at once was a very cordial relationship between Issac H. Jones and myself. He invited me to come down and see the way they were testing hearing then with tuning forks and so on. He had hoped that there would be some way of actually conducting the tests in a more scientific manner, and I know he welcomed very much the opportunity to have a young physicist indicate his interests in this problem, so he proposed that we work together. And that work led to a partnership that continued about fifteen years, during which we did some interesting work, published several papers, and developed an improved audiometer. It tapered off toward the end, largely because I became more and more involved in architectural acoustics and in other duties at the university that prevented my carrying on that work.

But the early years, beginning in 1923 and continuing at least for the next ten years, were productive years in the measurement, diagnosis and correction of impaired hearing. We soon formed the custom of spending every Tuesday afternoon, Tuesday night, and early Wednesday morning at his Santa Monica beach house. This beach house was located adjacent to the original Beach Club, and which is one of the few survivors today. It still operates as the Beach Club, and it was the original club underneath the [Pacific] Palisades and was very close to the--well, as a landmark, I guess it's known there now. It was the [William Randolph] Hearst home that he built for Marion Davies.

MINK: I think it's now been torn down, hasn't it?

KNUDSEN: It could be. I haven't seen the palatial place for several years. It had some rather unfortunate occupancies, I think, after Marion Davies left the place. I don't know the reason for her leaving.

MINK: Was Marion Davies living there when you were going there?

KNUDSEN: At the latter years, yes. This was rather interesting because probably within a year of the time we began our work there early in 1923, and before construction of the house was under way, the neighbors knew that William Randolph Hearst had purchased about 300 feet of beach front that was to be used for the

Marion Davies house, at a fabulous price. I forget the price, but it could have very well been $1,000 a front foot. That's the figure that I recall now--$300,000. It's much more valuable than that today. But the choice site was considered of course a very costly piece of land at that time. There were pretentious plans for the building, and there was nothing like it, of course, along the waterfront there under the Palisades near the Beach Club. And as a matter of fact, Dr. Jones's home was just the second one south of the Beach Club, and the Hearst place was just to the north of the Beach Club, and so we'd walk along the sand and watch the progress that was being made in the development of this building, because it was a very interesting structure. There had been nothing like it along the beach at any time, and the neighbors began to devise names for this building. Have I mentioned this?

MINK: No. What did they call it?

KNUDSEN: There were several names that I recall were proposed. One was Marion Davies's Swan Song, and Palais d'Hearst, and one of the others that I guess the more malicious people proposed was the Wages of Sin. These are three names at any rate that I now remember. There were many others, but probably less interesting. But we watched this building as it developed. It was a pleasant diversion to relax from our writing and observe the progress of this fabulous building. You see, there was lavish use of costly marble from Italy, and the very finest and rarest of other materials and workmanship went into the construction of this building. The outdoor swimming pool was a very elaborate affair with these beautiful marbles, rare artistic tiles, and so on. It was truly an extraordinary place as you would expect it to be. Anyone who has been to San Simeon and seen the other palaces that Hearst designed and built would know that he really went in for very lavish things, and in many respects they stand up high in the art world.

Well, these research and writing sessions down at the beach were both pleasant and productive. Dr. Jones would drive his automobile down one week and I would drive mine the next week. I think maybe a few of these ulterior things may be of interest. We would get to work probably at 2:30 or 3:00 in the afternoon (almost always on Tuesdays), and we would have a session until just before dinner time; then we'd go for a stroll, and if it was not too cold we would have a dip in the ocean. As a matter of fact, I swam in the ocean during those years every month, including December and January, but not very long in January, [laughter] because the water was pretty cold. But we made it a point to at least dip in once during January so that we could say we had been in the ocean in every month of the year down there.

These trips to the beach home of Dr. Isaac Jones [were wonderful]. It was a very comfortable beach home which had a private pier that extended out over the ocean, I guess at least 100 feet. We were going there regularly while they were driving the piles to support this pier; the entire setup was a very delightful place in which to spend the better part of a day and night. And our meals were usually at the club, especially during the winter months when there were no guests.

Then we were guests of Pierre, the Basque chef, [who was] always in the kitchen. Pierre was himself a character, and Isaac Jones and I had a fond association with him; Isaac Jones regarded Pierre really as one of his close friends. As a matter of fact, when Pierre had decided to retire and return to his Basque country and marry, Isaac Jones drove to the beach and got Pierre, with trunk, baggage, and so on, and took them to the Los Angeles Union Station and bade Pierre farewell on his return to his Basque country. And even after Pierre returned to his native Basque people and was married, he often wrote to us about his new life and to inquire about what we were doing.

He was especially interested in the dictaphone with which we dictated the papers we were attempting to write at that time, because all of our writing really was done at the beach. We didn't attempt to do any writing in his office or my laboratory. We had clinical research going on in his office and occasionally some of it in the UCLA laboratory, either at the Southern Branch at Vermont Avenue or here after we moved to the Westwood campus. Pierre was much interested in the dictaphone, and so each year Dr. Jones and I dictated our Christmas letter to him on a wax cylinder, and mailed him the wax cylinder. He would find someone with a dictaphone, and we'd always get a nice letter back from him, telling us how appreciative he was of this personal letter which he was able to hear and to share it with his people.

Well, during the first part of our work, Jones indicated to me his desire--to use his phrase--to test the cochlea directly. He repeated that over and over again. He said, "I would like to have an instrument that would do some of the following things." They are listed in our first publication entitled "Functional Tests of Hearing," (published in the Laryngoscope, September 1924). The article describes an instrument we called an audioamplifier. It was really an audiometer, but it had an amplifier associated with it, so Dr. Jones was really insistent that we use this name, audioamplifier. And the audio part was intended as the abbreviation for audiometer, and the amplifier was intended to describe the feature of the instrument that "tested the cochlea directly," among other things.

There had been some audiometers before that, as I mentioned, particularly the one that had been developed by Bell Telephone Laboratories. These audiometers were vacuum tube oscillators--that is, the ones after the Dean and Bunch one that was a rotating disk in a magnetic field. As soon as the vacuum tube was available it was reasonable that there should be an audiometer. There had been especially this one that had been developed by Dr. Fletcher and his associates at the Bell Telephone Laboratories. Isaac Jones was familiar with these audiometers and so was I, but he especially felt that they didn't provide all that the otologists needed. And he was especially concerned not only about testing the cochlea directly but also using the amplified voice or whisper, for which the loudness could be controlled, instead of the usual voice and whisper tests otologists had long used--you know, they whisper "sixty-six," "fifty-five," "thirty-three," and the patient is asked to repeat what he hears; this is the whisper test. And then there's a speech test. They'll call out numbers again, and the patient repeats what he hears. This was the standard technique that was used at that time.

Jones described what he wanted in the instrument; we talked several times about these things, and it was decided that I would try to design and make an instrument for his routine office use and also for our joint research that would test and determine the usefulness of the instrument. First of all, he wanted this means of talking or whispering into the person's ear, the patient's ear directly; and so the feature we felt should be included in the instruments that we were going to design and build should be the use of the second feature of a vacuum tube, namely, amplification of sound. The audiometer had simply used the vacuum tube as an oscillator to generate tones. He wanted it to be used also so that he could speak directly into the microphone of the instrument and have the speech amplified so that the patient could hear, if possible, the amplified voice. And we'll discuss why in a moment.

He also wanted to be able to use the instrument in testing for malingering--that is, he wanted to be able to know when someone might lie about his hearing. He might say, "Well, I can't hear anything in my left ear." And so our instrument's speech amplifier could be connected to either of two earphones which the patient held, one on each ear. And the operator had a little switch with which he could direct his conversation to either ear. The patient always had his back turned to you as you were making this examination, and the operator would carry on a conversation and then flip to the left ear or the right ear. Well, if he heard as well in the ear that he said couldn't hear at all, then you knew he was lying. The doctor is often called upon, you know, to determine whether a man is malingering about his hearing.

MINK: Insurance cases?

KNUDSEN: Insurance cases and so on. This was another feature we included in our audiometer. The feature I wanted especially at this time was what we called selective amplification, which means that you place emphasis on the high frequencies or the middle frequencies or the low frequency in different amounts.

MINK: Why were you particularly interested in this?

KNUDSEN: Because people have different kinds of hearing loss--the loss often was different for different frequencies. It was very well known then that persons who have nerve deafness, for example, or cochlea deafness, have the greatest loss for the high frequencies. There are people who have hereditary deafness, otosclerosis, which makes its appearance usually in the twenties, sometimes earlier and sometimes later, but usually it first manifests itself about that age, and it's definitely hereditary. It's primarily, especially in its early stages, a mechanical defect; it begins as such between the middle ear and the internal ear, in which there is a flexible membrane attached to the stapes--that is, the little stirrup that fits into the internal ear. This membrane becomes bony, ossified, and thus impedes the mobility of the stapes, and the resulting hearing loss is greater for the low frequencies than for the high frequencies.

I wanted some means here of selectively suppressing the low frequencies or the high frequencies, and we called this "selective amplification." So we would not only test the hearing of speech by the patient with undistorted sound, but also with the low frequencies suppressed and with the high frequencies suppressed. This is accomplished much as you filter-tune your radio with an increase or decrease of the bass or an increase or decrease of the treble. And this feature was not in the audiometer at the Bell Laboratories, or others that developed, so we decided that this feature should be in our audioamplifier.

We also felt that it was necessary to have a means for testing the hearing, not only by air induction in which you have the receivers over the ear, but also by bone conduction. The bone conduction had been conventionally tested by the otologists in striking a tuning fork and placing the shank of the tuning fork against the mastoid bones in the process just behind the ear.

MINK: Was this Jones's idea to do this?

KNUDSEN: The testing of bone conduction with tuning forks had been routine in otological practice for many years. Dr. Jones and I wanted our instrument to include an improved means for testing by bone conduction. Dr. F. W. Kranz, whom I mentioned, was a graduate student at Chicago the same time I was, and also a former Bell Laboratory physicist, and was working with Dr. Gordon Wilson, [who] had developed a type of bone conduction receiver to be used with the audiometer, so this idea was not novel with us. Dr. Kranz had developed one that he and Dr. Wilson had used. As I say, we had the experience of others, especially of Dr. Kranz, in designing our bone conduction unit. We designed a slightly different kind of bone conduction testing device, which included a controllable noise-making device that could mask one ear by testing the other. Our bone conduction unit and technique was one which Jones particularly felt was a suitable means for testing the cochlea directly.

Let me explain. When you generate the sound by the earphones, the sound is transmitted by air conduction, first through the external ear, which is a sort of a funnel, the pinna; and then it conducted (again in air) through a little tube, the ear canal, which is about an inch and a quarter in length. Then you have the drum membrane at the end of the canal. On the other side of the membrane you have three little bones, the hammer (malleus), anvil (incus), and stirrup (stapes); and that's the normal route by which you hear by air conduction. In hearing by bone conduction this normal route is essentially bypassed; the bony skull is set into vibration, and these vibrations are communicated to the internal ear.

The method of testing bone conduction, which had been in use at least 100 years, making use of tuning forks, was to put the tuning fork against the mastoid bone and measure how many seconds it remained audible after it had been given a standard strike. And you do this with maybe two tuning forks, one at low pitch and one at intermediate pitch. But the audiometer furnished the means of measuring the hearing acuity (throughout the entire audible range of frequencies) by air conduction and by bone conduction. Now when you measure the hearing by bone conduction, you bypass, as I said, the external ear and the middle ear. The bony walls of the internal ear are a part of the skull in which the cochlea is embedded, and therefore when you hear by bone conduction, you've stimulated the cochlea without using the external ear or the middle ear.

This is what Dr. Jones wanted our instrument and testing technique to show, because then you have determined the hearing acuity of the cochlea. And if you make a hearing chart (audiogram) of how much hearing loss there is at low-, at medium-, and at high-pitched tones, both by air conduction and by bone conduction, the air conduction curve measures the total loss, and the bone conduction curve measures the loss of the cochlea and/or the auditory nerve beyond the cochlea. And if you subtract the latter--that is, the bone conduction hearing loss from the air conduction loss--then the difference between the two curves is a measure of the hearing loss of the middle ear, which is of a conductive or mechanical nature. So by this means you obtain very useful diagnostic information. If you obtain the same hearing curve (audiogram) by air conduction and bone conduction, then you conclude that the entire hearing loss is in the cochlea or the nerve. If, however, the patient doesn't hear low frequencies well by air condiction and hears by bone conduction very well at these low frequencies, then you can conclude that the cochlea and auditory nerve are normal for low frequencies--and you suspect otosclerosis or some similar conductive lesion. And so by measuring the hearing acuity--that is, making an audiogram by air conduction and also by bone conduction--you have a powerful means of making diagnoses.

MINK: Once you have a diagnosis, would you attempt any sort of remedial work?

KNUDSEN: Yes. Often there would be remedial work. Well, for one thing, if the bone conduction is really good for a wide range of frequency, you can confirm this by the test of speaking into the microphone; if the patient hears the amplified speech, the hearing loss is largely conductive, and a hearing aid would be useful. If it's all nerve deafness, they will often squirm when you step up the amplification, because they say it hurts; and it does hurt because then they get the full force of the vibrations in the cochlea, and they've got a diseased cochlea to begin with. It's a defective cochlea. The hair cells (the sensory nerve endings of the auditory nerve) may be destroyed and various other pathological changes occur inside the internal ear. The nerve-deafened are extremely sensitive to noise. They hear sounds but have difficulty understanding speech. They don't hear anything until the sound level reaches their threshold. Then when you exceed their threshold and go to just a little more intensity, they say, "Oh, it's painfully loud."

This again is a diagnostic aid when they respond that way. So the matter of speaking into the cochlea, or speaking into the person's ear directly this way, if they hear distinctly when it's amplified, that again is confirmation that there's not much nerve impairment, that it probably is otosclerosis, which consists of a fixation (sclerosis) of the stapes. And otosclerosis later became a surgically corrective type of hearing impairment, first by what's known as the fenestration operation in which they make a new oval window by scalping away through the bony wall of the cochlea until they have only a thin membrane left, and that serves as a new oval window through which the sound can penetrate. Because it is so thin, it doesn't offer very much impedance to airborne sound getting directly into the cochlea. Or in a more recent surgical technique which is used for otosclerotic types of impairment--Dr. Victor Goodhill, UCLA otologist, is probably the leading surgeon in the Southland for this technique--is called mobilization of the stapes.

They proceed with this operation by using an audiometer. The audiometer's receiver is attached to the patient during the operation and the hearing loss is measured as the surgery proceeds. What the surgeon does is to scrape away the excess bony tissue surrounding the footplate of the stapes, which has developed and thereby has immobilized the stapes. This is the usual lesion that causes otosclerosis--an ossification of the fringe that surrounds the stapes at its entrance to the internal ear. Victor Goodhill and others who perform this operation very delicately scrape this away, this bony fringe, while the patient is only under local anesthesia, and therefore can hear the test tones of the audiometer, and the surgeon observes the change in the patient's hearing acuity as he performs the operation. It seems almost a miracle, of course, to the patient. He's there observing his hearing improve as the surgery proceeds. He is aware that the surgeon is scraping away at something in his ear, but it's very much like a dentist working on an anesthetized tooth. The patient doesn't feel any pain. The surgeon must operate skillfully and delicately, with the aid of a microscope. The stapes and the surrounding bony tissues are very, very small--a millimeter or two in diameter--and so you're dealing with very, very delicate surgery, and you're scraping down a hard bony tissue until it becomes so thin that it becomes flexible. When it's ossified, it's very much like clamping the stapes in a vice. It can't move back and forth, and so when you scrape away the ossified tissue and reduce its thickness to a thin membrane, you metaphorically replace the iron jaws of the vice with something like rubber, something which will yield, and thus allow the stapes to vibrate normally, or nearly so. And this will often increase the hearing for a very wide range of frequencies by as much as 40 decibels, and that's a lot.

You asked me to define decibels sometime. Maybe this is a good time to do it, because we'll often be talking about decibels.

MINK: Well, I was asking not so much to define it as to tell how the concept was developed.

KNUDSEN: Yes. Well, the decibel really in a sense goes back to the Weber-Fechner Law of psychoacoustics. In general, most sensations are a logarithmic function of the stimulus, and this is certainly true of hearing. The name itself, "decibel," should be pronounced "des-i-bél," not the way it was last week over a TV program, "dis-a-ble," because it's named after Alexander Graham Bell. The "bel" is a unit, of course, that is just 10 times as large as the decibel. "Decibel" means 0.1 of a bel. And this is based on the common system of logarithms. It's to the base 10. The easiest way to explain it to a nontechnical person, someone who hasn't had logarithms, is that the logarithm to the base 10 of 10 is 1. It simply means 10. You start there. The logarithm of 100 to the base 10 is 2, which means 10 x 10 is 100. The logarithm to the base 10 of 1,000 is 3, which means that 10 x 10 x 10 is 1,000. The logarithm of 1,000,000, for example is 6, which means that 10 multiplied by itself 6 times is equal to 1,000,000. Now this is the basis, then, when you talk about decibels. This is a unit that is 0.1 that size; so 6 bels is 60 decibels, and that means that the intensity has been increased a millionfold. Then 10 decibels means tenfold, 20 decibels means a hundredfold, 30 decibels means a thousandfold.

Hearing is always referred to as the threshold of hearing for the frequency which is most sensitive. It's approximately that which is at 1,000 cycles, and that means the pressure variation of the sound that is just barely audible at 1,000 cycles is two ten-thousandths of a dyne per square centimeter. Now that's very, very feeble. In terms of amplitude, this will be a little more meaningful; it's of the order of not much more than a billionth of an inch. This is the amount the stapes is moving back and forth for a sound that is just barely audible. If you had a more sensitive ear than you have, then you would certainly hear the blood coursing through one of the little blood vessels that flows under the basilar membrane in the cochlea. You would actually hear the impact of molecules hitting against each other and against the hair cells (receptors) of the auditory nerve.

Nature has gone just about as far as it is possible to go in developing the cochlea for its organ of hearing. It probably initially was a piece of skin.

Even today if you investigate the tactile sense, you'll find that in one respect it resembles the hearing sense. (Later we'll talk about hearing with the sense of touch.) The fingertips can recognize as small a change in amplitude of vibration or intensity of vibration as the ear can. For most of the range, I think as I indicated for hearing in one of our earlier discussions here, discussing my dissertation, I said the intensity has to increase 10 percent in order to give a barely perceptible increase in loudness. It also has to increase 10 percent to be recognized as an increase if you're feeling vibration. So if you're listening, as Helen Keller does, say, to the vibration of the throat or to the sounding board of a piano or any other vibrating body, as the cone speaker of your radio set, you can recognize a change. At the very feeble intensities it has to be more than that, maybe 30 or 40 percent, but the sensitivity increases--that is, the sensibility increases. The amount of change in amplitude that can just barely be detected follows the same curve for the tactile sense as it does for the ear. That's the thing which we determined in our laboratory some years later. I'll speak about that at a later date.

MINK: The equipment which you put together for Dr. Jones, you did in the laboratory of the Southern Branch, right?

KNUDSEN: Yes. That was over on the old campus.

MINK: Using again equipment that was given to you by the Bell Laboratory?

KNUDSEN: That is right. The first apparatus we had for our acoustical research was made out of equipment that really was begged from the Bell Telephone Laboratories--mostly vacuum tubes, which were then not available elsewhere. Many of the parts, anything that we could make ourselves, such as winding the inductance coils, binding the resistances, we provided by our own labor. But principally the vacuum tubes were the indispensable items we did not have. They were beginning to appear on the market at about that time. I was familiar with the Western Electric vacuum tubes, and I wanted to use their V-tubes for the first stages of the amplifiers because they had a high gain. This tube gave a voltage gain of thirty, and at that time no other vacuum tubes had that high a gain factor. And also we wanted to use Western Electric L-tubes for the oscillators.

Furthermore, the work I did on oscillators at the Bell Telephone Laboratories, and which I briefly reviewed in one of our sessions here, was the basis of the kind of oscillator we designed for this first

first audiometer. This utilized the use of the proper amount of resistance in the plate circuit of the vacuum tube. You know, you have a high positive voltage on the plate and low negative voltage on the grid, in general, in a vacuum tube circuit, and we decided on a Hartley circuit. That's one in which you have two inductance coils, the midpoint connected to the filament of the vacuum tube, and one end goes around through the resistance and to the B-battery and then to the plate; and the other end goes to the C-battery and then to the grid, to maintain the grid at a negative voltage as it is supposed to be. And then there's a variable condenser across the two windings of the inductance coil. It's largely the capacitance and the inductance of this circuit that determines the frequency, but everything else in the vacuum tube circuit has an effect on the frequency. And the purity of the tone that you generate can be controlled very much by the amount of resistance you have in the plate circuit. And so we controlled the purity of the tone we were generating by the proper choice of resistance. And for each frequency that we generated we had the appropriate amount of resistance to make the sound a pure tone, instead of an impure tone.

MINK: Well now, when was it possible to get this equipment into operation so that Dr. Jones could use it on his patients?

KNUDSEN: Well, not very long. I know we had this going certainly by the summer of 1923. The first paper, you see, was published in Laryngoscope for September, 1924. It was read in St. Louis before the American Oto-Rhyno-Laryngological Society, in May, 1924.

MINK: By Dr. Jones?

KNUDSEN: By Dr. Jones. Dr. Jones was probably invited to be there.

MINK: Did you go along?

KNUDSEN: No. I did not go with him for the presentation of our first paper. But there are one or two related things here that I think we might add for the record. Jones wrote pretty much the first part of the paper. I listened, and then I wrote the second part and he listened. And then we criticized each other's contribution a great deal, and we really spent a lot of time together. We began: "This paper involves the points of view of an otologist and a physicist; it has seemed best to approach it first from the standpoint of otology and then from the standpoint of physics." And so in the first part of this paper, for example, Jones says essentially,

> The practicing otologist feels the need of some method that is precise and yet requires a

minimal amount of time. Much time and patience is necessary to conduct complete functional tests of hearing with the present methods. It would be highly desirable if one could attain simplicity and speed as well as accuracy. Only too frequently the busy practitioner finds himself making only a cursory examination. He would also like to be able to measure progressive changes in hearing. He would like to determine whether any beneficial effect is being produced by his treatment of any given ear condition. We need a yardstick of measurement. We have all felt at times that perhaps our main standard is the temperament or disposition of the patient. If the patient is an optimist, he will tell us that he is much improved. If he's a pessimist, he will tell us that he is no better or worse. Even in prolonged observation of a deaf patient, unless gross changes occur either for better or for worse, it is difficult to draw any exact conclusions as to the status of the hearing defect.

This more or less sets the tone of the article here for what the otologist wants, and I mentioned some of these things that we wanted to unite, if possible, in a single instrument. We decided to call it an audioamplifier, for reasons I have explained.

MINK: Did you get any of your students from the Physics Department to go down and be tested by Dr. Jones?

KNUDSEN: Oh, yes--Leo Delsasso, my colleague here, his brother Louis Delsasso, who later got a PhD degree in nuclear physics at Cal Tech, and W. A. Munson, who later became associated with Harvey Fletcher and helped devise the Fletcher-Munson equal loudness curves that are used throughout the world. Munson now is training porpoises to speak at Miami, Florida. He has retired from the Bell Telephone Laboratories. Louis Delsasso didn't stay in acoustics, but Louis Delsasso, the brother of Leo, and Munson were my research assistants for several years. During about two of those years they built the audiometers based upon this first design and they were made only for otologists that were approved by Dr. Jones as competent investigators who would use the instrument for research purposes. One of these first audiometers will be put in working order again, we hope. But it is proving to be difficult. We can't find Western Electric tubes anymore, and so we really have to redesign the interior to make it adaptable to the kind of vacuum tubes that are available today. It is our intention to give this audiometer, made in 1924, to UCLA's Hope for Hearing laboratory--in working condition, if feasible, otherwise as a historical exhibit.

SECTION XV

KNUDSEN: The audioamplifier was in an oak box. There's a photograph of it on page 10 of this reprint of Jones and Knudsen's first paper on "Functional Tests of Hearing." This and one more copy are all I have left of the supply of 50 or more copies I had at the time of publication in 1924. The instrument was in an oak box that was approximately 28" x 14" x 15" in height. And anyone who might be interested, can see this photograph of it, or they can see an actual model which we have in this building.

MINK: How many did you make?

KNUDSEN: We made a total of, I believe, twenty-six, and, as I said, only for doctors that Isaac Jones would approve. They were doctors who would use the instrument for research. We weren't in the business of making commercial audiometers, but among the physicians in this neighborhood who had them, were well-known, distinguished otologists, such as Simon Jessburg and Anna Loefler, a woman; it's Anna Loefler's instrument, recently given to us, that we have here for the Hope for Hearing Laboratory. We're trying to make it usable so that it will be not only something to look at but something that people can actually use to test their hearing. Among other otologists for whom we made audio-amplifiers I remember Dr. Wesley Oakes and a Dr. Merrill. Dr. Merrill was then in Salt Lake City and later in San Diego. Dr. Oakes was in Provo, Utah, my home town. Other otologists who had our new instrument were in Boston and in Philadelphia. There were other otologists that Dr. Jones knew who had found out about it. Isaac would say for those he approved, "Well, it's all right to make one for this doctor." Louis Delsasso and W. A. Munson made the instruments in a simple shop we set up in the back of my lot where I lived at that time, on Hyperion Avenue, and so this became the headquarters for the making of these twenty-six audiometers that we made over a period of about two years. There soon were so many requests for these instruments that I decided to terminate this type of enterprise.

MINK: You sort of supervised the work?

KNUDSEN: Yes, oh yes, very much. I still have a journal at home in which we have a record of the calibrations of each of these instruments before they went out, and instructions on how to use them, and then we were in correspondence with these people. And many of them published papers later in the journals on data that they had accumulated. And this had a bearing on upgrading the practice of otology, especially in connection with the measurement of hearing impairments and the diagnosis of hearing impairments.

MINK: How much of an impact would you say it had, in terms of the progress in this field?

KNUDSEN: Well, it was enough at any rate that Dr. Victor Goodhill persuaded the Hope for Hearing Foundation, of which he is research director, to establish in 1964 the Vern O. Knudsen Graduate Fellowship in physics. It is awarded annually to a graduate student working in acoustics. And Victor Goodhill on the occasion of the dedication of Knudsen Hall made this announcement at a special session that he had arranged in my honor, held in the Hope for Hearing laboratory here. I'd rather he had answered your question. I know he felt that our audiometer plus my work with Dr. Jones had a very great impact on the development of audiology, and so did Dr. Joel Pressman. We made one for Joel Pressman, who is in the medical school here now.* You probably know who he is. He is best known, I guess, for his wife. [laughter] What's her name? She's a prominent movie star. It's Claudette Colbert.

Well, we continued this work; and the first two patients we tried this out on in Dr. Jones's office were two elderly ladies. Jones describes these two old ladies, about equally deaf. They were the first

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*He was when this was dictated in 1967. A year later he died of cancer.

ones tested with the new audioamplifier. This was Jones's comments: "It was found that one of them heard perfectly the ordinary conversational speech, and even the whisper, with the use of the amplifier. The other one not only did not hear better with the instrument, but heard better without it. Careful functional tests, auditory and vestibular"--Jones was an expert on vestibular tests. You know, that's for testing motion sensing, equilibrium, vertigo and other similar vestibular functions. The ear has two parts, the part for hearing and the part for maintaining balance and sensing motion--"Careful functional tests, auditory and vestibular, showed that the first (lady) had good internal ear function"--that's the one that heard speech very well--"the second one, impaired internal ear function. A series of patients was then tested. In these patients the nature of the hearing impairment was carefully studied by the usual functional tests. It seemed that the following tentative conclusions could be drawn. Those with conductive lesions can hear very well with the use of the amplifier. Those with perceptive lesions cannot hear nearly so well, and in many instances are much annoyed by the use of the amplifier."

This was the result (the utility of the amplifier) that delighted Isaac Jones more than anything else. He could speak (with his amplified voice) just a few sentences to the patient, and he was almost sure of the diagnosis simply from that. If their face beamed up and they said, "Oh, I hear you very well. I can just understand every word you are saying," then you knew pretty well you were dealing with a case of conductive deafness. If, on the other hand, the patient showed displeasure of some sort, especially when the spoken voice was greatly amplified, you'd see a frown come on the patient's face or even the jerking of the receiver away from the ear, and the patient shouts, "This hurts." You know [then that] you are definitely dealing with a cochlear or a perceptive type of deafness. It's the cochlea or the auditory nerve or both that would be involved in these perceptive cases.

But on carrying further the studies on the amplifier we thought it would be desirable to provide selective amplification, that is, selectively amplifying certain frequency ranges (e.g., high or low) more than other ranges. I was especially interested in this feature of our audioamplifier, and therefore included filters for giving different kinds of selective amplification. The patients who had hearing loss especially at high frequencies, in general, had perceptive impairments. If you controlled things carefully and had the gain (amplification) a little more where the loss was greatest, they actually did hear a little better, and you thereby could suppress certain frequencies (not so important for speech) that they would hear too loudly if you amplified all frequencies alike. So a suppression, say, of the low frequencies for them and a little gain on the high frequencies enabled them to hear a little better than they could hear with uniform amplification.

We made a few (four or five, I believe) hearing aids for some of our hard of hearing patients. They were usually built into a woman's purse, or equivalent carrying case, that was about eleven inches by eight inches high and three inches thick, and I think we got the total weight down to about nine or ten pounds. We made only these few hearing aids for a favored few patients of Isaac Jones and one for a friend of mine, Fuller Stoddard, inventor of the self-playing piano (Ampico). Louis Delsasso and W. A. Munson made these "portable" hearing aids with selective amplification in this little laboratory shop that I had back of my home on Hyperion Avenue. Fuller Stoddard and Professor Grace Fernald (UCLA) prized these instruments very highly.

MINK: These were not the first hearing aids that were made anywhere, were they?

KNUDSEN: No. But they were the first, I believe, with selective amplification, and among the very first that used vacuum tubes. We made one, as I mentioned, for Professor Grace Fernald, who was in our Department of Psychology.

MINK: Grace Fernald?

KNUDSEN: Yes. She had one. She said, "I don't care if it weighs a hundred pounds. If I can hear with it I want it!" [laughter] And so she carried this with her everywhere on the campus. She would smile and push the microphone toward the speaker. The microphone was detachable with a cord on it, and so it could be moved several feet. Fuller Stoddard had an even longer cord for his microphone. He was the first treasurer of the Acoustical Society of America. I often saw him at Acoustical Society meetings, sitting on the front row, with his hearing aid on his lap and its microphone at the speakers' lecturn. He had a nerve impairment, with a marked hearing loss at high frequencies, and a diplacusis lesion in one ear. He attached the earphone to the other and better ear.

MINK: Was he examined by Dr. Jones?

KNUDSEN: No. He was examined by me with an audiometer we had in our laboratory here at the Westwood campus. I did not meet Fuller Stoddard until the Acoustical Society was organized in December, 1928. My work with Dr. Jones had progressed quite a way by then, and Fuller soon was deeply interested in my work on hearing, audiometry and hearing aids. He very much wanted us to make him one of our vacuum tube hearing aids, so he came to our UCLA laboratory so that we could determine just what kind of selective amplification would be best for him. We didn't want to get in the business of making hearing aids, but we wanted to make enough to at least test them out by qualified users. The five we made convinced us they were beneficial.

Some hearing aids on the market today have selective amplification but its use has not been adequately developed. Later I may refer to a paper that Norman Watson and I published, based upon a more intensive study we conducted here, on hearing with selective amplification. In our investigation we used speech articulation tests with a number of subjects that we tested exhaustively. And our results supported the conclusion that selective amplification was beneficial. I know if I had hearing difficulty--which I am supposed to have at my age but I'm one of the lucky ones that hasn't yet lost much hearing at the high frequencies--I know I would incorporate high frequency selectivity in the instrument, giving a little more amplification for that. I would also devise a binaural aid, one for each ear. We may talk about that later.

But to return to my work with Dr. Jones, and specifically how to chart the results of audiometer tests. Two ways had been used for charting the hearing, but the principal method charted the hearing loss in decibels as a function of frequency. Jones was very much preferred the charting of how much you can hear, instead of how much you can't hear. He was an optimist in that respect. So we chose the second way of charting how much the patient could hear, expressed as a percentage of normal hearing. We would convert hearing loss in decibels into percentage of hearing. On the chart we plotted, the patient had, say, 80 percent of hearing at low frequencies, maybe 60 percent at mid frequencies, and only 40 percent at high frequencies. Because decibels frighten or at least don't enlighten a lot of people, and also because Jones thought, well, average people are much more concerned about what percentage of hearing they do have than what they don't have, we decided in favor of converting hearing loss in decibels to percentage hearing. This is how this conversion was made: if the total range of hearing, say, at 1000 cycles is 120 decibels, which it approximately is, and the patient has a loss of 60 decibels, then we say he has 50 percent of hearing at that frequency.

MINK: Would you say that Jones was the only one that worked in this way, or did the others work that way, too?

KNUDSEN: He was the principal one; he was really responsible for initiating this method of charting the hearing, and we used this all along in our publications, although this first publication I am reviewing for you doesn't show any charts, but in subsequent publications we show charted audiograms and they always show the percentage of hearing as a function of frequency throughout the useful audible range.

MINK: I see. I notice there's a picture here. Who's this?

KNUDSEN: I'm the operator in the picture.

MINK: It's on page 13 of this same report we've been discussing. And who is this gentleman you are testing?

KNUDSEN: I don't remember his name now. He was not a student; he was a patient in Dr. Jones's office, but he was a cooperative one and willing to spend day after day. We spent many hours on some subjects, like the one shown in the picture, who were willing to devote time. And so this paper, "Functional Tests of Hearing," was based on an intensive investigation. I was down at Jones's office at least twice a week. Here's another picture of the same patient showing how the test tone is made. You see, the finger is held up while the test tone is heard and dropped down while it is not heard. See?

MINK: Yes.

KNUDSEN: I thought maybe one of these would show how we tested for bone conduction. But the pictures show the way we tested for air conduction. The bone conduction receiver is shown, I thought, in one of these. It usually stands on the back there. It's a telephone receiver that has a strip instead of a circular diaphragm, and then it has an aluminum rod attached to it with a little button on the end which fits comfortably against the mastoid bone. Then the patient is told to vary the pressure back and forth, this way, so that you get the pressure which gives a maximum. And that's the way the bone conduction test was performed. And you then plot this percentage hearing curve for air conduction and for bone conduction. This together with the speech test makes a really infallible test for determining how much loss there is in the internal ear and the auditory nerve, and how much loss there is in the middle and external ear.

A good example of the utility of this information is its relevance to the fenestration operation. The fenestration operation should _never_, _never_ be performed on a person who has exclusively nerve deafness. It can't _possibly_ do any good for cochlear or nerve deafness. And yet one of my daughter's sorority sisters had an impairment of hearing, and her parents were determined that she should have the fenestration operation. She had had tests made by a competent otologist--I won't mention the name--and he rightly advised against the operation. She came to me for an audiometric examination because she was my daughter's sorority sister. I measured her hearing by both air and bone conduction. It was 100 percent perceptive. There was no trace of otosclerosis, and no trace of any other conductive or mechanical defect. And to go in and make a new window into the internal ear--well you see just how absurd that would be. You have _no_ mechanical defect--you have entirely a nerve or cochlear defect and you just can't do anything by making a new hole through the cochlear wall, which is very bony tissue. As a matter of fact, you would almost always suffer an additional hearing loss if there was no fixation of the stapes. But the parents insisted upon the surgeon performing this operation, which he should have refused to do. I measured her hearing again later and she had a greater loss of hearing after the operation than she did before. And, of course, they were greatly disappointed about this.

This indicates the importance of making these tests and then not yielding to any kind of pressure. No otologist who understands these things should under any circumstances be persuaded to perform the operation

anyway, even though the parents say, "Well, I insist upon your doing it." Because in those days, too, there was some risk even of death. You see, you might get infection if they happen to scrape away too much in getting this membrane so thin that it perforates. They might introduce infection in the fluid in the internal ear, which is called endolymth, and this fluid flows also through the spinal canal. And so an infection there could actually result in spinal meningitis, and it has in some instances. It may cause death, so that this is a type of operation that should be used only as a last resort. And today the mobilization of the stapes of the type I described that Victor Goodhill (among others) does, is ordinarily done first, whether they are dealing with cases of otosclerosis or other causes of fixation of the stapes. The mobilization of the stapes is a much safer operation, and if you can accomplish hearing improvement that way it's far better than taking the risk of making a new window in the bony wall surrounding the cochlea.
[tape off]

I would like to document one item that we referred to in an earlier session in connection with the cosmic ray controversy involving Professors Robert A. Millikan and Arthur Compton. In the first place, I would like to make it clear that there is plenty of original high quality work done by both of these investigators, so that they can share the glory even if there were some differences of opinion. As a matter of fact, when I was a graduate student at Chicago, the similarity between light waves and atomic particles was so close that I remember one of our professors there, A. C. Lunn, proposed the name "wavicle," which combines, you see, the aspects both of a particle and a wave. Professor Lunn said, "These phenomena of the Compton effect had definitely demonstrated that waves are reflected from various materials, atoms and so on, very much like particles that obey the law of conservation of momentum--that is, they act very much like billiard balls hitting against the cushion or other billiard balls. There's conservation of momentum there; or when two bodies collide, they have certain velocities of approach and they have velocities of recession, and the momentum (the product of mass times velocity) before impact is equal to the momentum after impact. Well, Compton demonstrated that waves behave this same way--that is, the wave length changes in accordance with the law that the energy is proportional to this universal Planck constant, h, times the frequency of the radiation.

Lunn said these waves and particles, which he proposed to call "wavicles," act like a wave on Tuesdays, Thursdays, and Saturdays, and like a particle on Mondays, Wednesdays, and Fridays. And so this controversy between Millikan and Compton, after all, is somewhat a matter of semantics. We aren't quite sure when these wavicles are particles and when they are waves. But Millikan and others, incidentally, called them cosmic rays because they believed they were waves, and this name has stuck through the controversy. They were named at a time when Millikan thought these rays were definitely waves, and his early work indicated that they were of the nature of waves. And as the work progressed he had evidence that he considered quite conclusive.

However, German investigators, besides Compton and others, had been gathering data that supplemented and modified the early work. Millikan was really the great pioneer investigator in this field, and he tried to explain how the cosmic rays varied in intensity with altitude in the atmosphere or going down into lakes or water. These media absorb the radiation. He was able to explain the nature of the attenuation curve, or the way these things attenuate as they pass through the atmosphere and then through water, by assuming that there were three types of radiation--that is, three different wave lengths. They're often referred to in terms of their energy. Million electron volts was the way of characterizing them. And I remember the lowest one was something like 26 million electron volts, and the other two were somewhere around 110 and 120 million electron volts.

Millikan seized onto the idea that if we assume that interstellar space is filled with dilute hydrogen, then these three radiations, which in combination he was able to show, would explain just the way cosmic rays were attenuated as they passed through the atmosphere. He postulated that the radiation that corresponds to 26 million electron volts represents hydrogen being converted into helium. And the other two represent conversions of these lighter elements, hydrogen or helium or both, combining into oxygen or nitrogen. Since these three elements are the three most abundant things, this was a rather plausible explanation. And Millikan referred to this phenomenon that he thought explained the cosmic ray information as "the birth cries of creation." That is, you see, helium was being created out of hydrogen, and oxygen and nitrogen were being created out of lighter elements.

Some of the people who were getting later information challenged this view. Millikan's view, you see, was fusion. But the proponents of the later views concluded that it looks more like these cosmic radiations result in the separation of the elements, and therefore cosmic rays are signaling the death cries of this process that's going on in nature. So Millikan's view, you see, was the optimistic one--here we have the birth cries of creation. Some of the opposition had taken the view that, well, it may be more accurate to say that these are the death cries of annihilation. And so a lot of people liked to seize on Millikan's views there because they did have this more cheerful aspect about the outlook on nature.

It was later demonstrated, though, as a result of the work that Compton and others had done that, really, this was a fortuitous series of combinations that Millikan was able to explain these things, and that they could not possibly be accounted for when they measured how the cosmic rays varied, for example, with latitude and with longitude (the east effect and west effect) and so on; and it became more and more clear that they could not be accounted for by waves at all, but rather by secondary particles. And it was when Compton had presented the evidence that convinced essentially all physicists that this was true that this incident that I referred to occurred at the meeting of the American Physical Society. So, I want the record also to make it clear that I was not present at this meeting, that I am not a cosmic ray physicist, and unfortunately I don't remember the name of the person who told me about the incident in which Millikan might have stood up and said, "Well, Compton has removed all doubt about what these rays consist of; they are primarily secondary particles that make up the radiation, at least as we know cosmic rays in the atmosphere." At least I wanted to get that much background, because I greatly admired both Millikan and Compton. [tape off]

MINK: I had asked you whether there was intercourse back and forth between Cal Tech and the Southern Branch and whether Millikan came over to Vermont Avenue.

KNUDSEN: There was a good relationship between Millikan and certainly the Department of Physics at UCLA. For example, I recall one occasion when Niels Bohr was at Cal Tech, primarily as the guest of Millikan; but Millikan was good enough to bring him over personally one afternoon to address a physics symposium that we had here on campus.

MINK: Was this at Westwood?

KNUDSEN: Yes. Millikan had attended one meeting of the American Physical Society at the Vermont Avenue campus, but his visit with Bohr was after we had moved to Westwood. Throughout the years Millikan was certainly always cordial in his relations with me. The Millikans and Knudsens had one common interest in UCLA, namely, Sir John Adams, professor of education, and his wife.

MINK: Adams?

KNUDSEN: Sir John Adams and Lady Adams. Lady Adams was a very good friend of the Millikans, and she was also a very good friend of the Knudsens, and we met the Millikans on a number of occasions as a result of this friendship. We interchanged views about the Millikans a great deal, because Lady Adams admired the Millikans very much. She was often entertained at our home, especially after Sir John died, and we were entertained at their home; there was no doubt about the closeness in the relationship between Millikan and some of the physics people over here, although Millikan was very strongly opposed to the development of graduate work at UCLA.

MINK: Oh, he was?

KNUDSEN: He was. On one occasion he was invited to give the Charter Day address here. I remember it was held in Kerckhoff Hall. And really the burden of his speech was about as follows. He first referred to the establishment of the University of Chicago at a time when people felt it was going to greatly damage the University of Illinois. This was going to be a very serious thing. But Millikan indicated that the interplay between the University of Illinois and the University of Chicago as it developed worked to the advantage of both. And he used as a summary statement of this interplay that "the key arch of our educational system is the interplay between privately endowed and publicly endowed institutions." But Millikan's relation with California taxpayer's associations and others here would interpret that summary statement to mean that the University of California at Los Angeles should train them up through the college level, but the private institutions would take over at the graduate level. This was his interpretation of this key arch as the interplay between the privately-endowed Cal Tech and publicly-supported UCLA. And it probably was a matter of preservation of the importance of Cal Tech that led him to this view, because he was one of the most ardent opponents of the establishment of and the growth of graduate work at UCLA.

MINK: I understand that there was some interplay between Ernest Carroll Moore and Millikan on the subject of whether or not the University [of California] at Los Angeles should be the recipient of private gifts, of money and funds, that did this not sap away this source from the private institutions in the area?

KNUDSEN: I can't document that. I do know that Millikan was outstandingly successful in attracting private gifts to Cal Tech, and that's very much to his credit. That is, Cal Tech certainly attained much of its stature by reason of these early gifts that

Millikan was able to attract to Cal Tech. And certainly I agree with Millikan's thesis here that the interplay between privately supported and publicly supported institutions is a wholesome thing, and I'm sure that this relationship between Cal Tech and UCLA has been a great advantage in the development of UCLA. And the present president, Lee DuBridge, certainly has been a very fine supporter of that view, and we have had excellent relations with Lee DuBridge, and as a matter of fact, with the entire Cal Tech staff, in the interchange of information and in research collaboration. As a typical example, the late Professor Paul Epstein was much interested in our research on the absorption of sound in fog and smoke, and published a plausible theory he had developed that supported our experimental findings. I'm sure we supplement each other, and there's no feeling at all that one is destroying the other. On the contrary, I think there is a feeling that they support each other.

MINK: To come to Sir John Adams, there was also a very close relationship between Ernest Carroll Moore and Sir John.

KNUDSEN: That I can document--that is, Sir John was brought here very definitely through the efforts of Ernest Carroll Moore. And Ernest Carroll Moore, I know, felt very much that Sir John had done for public education in England what had been done in our country by--you supply his name.

MINK: Horace Mann?

KNUDSEN: Horace Mann, yes, absolutely.

MINK: I see. Did these social gatherings include Ernest Carroll Moore as well as yourself?

KNUDSEN: Not at these private dinner parties. They were usually very small affairs, often just Lady Adams. Most of our acquaintance with Lady Adams occurred after the death of Sir John. She was especially fond of my wife Florence, and our three children. But during all that time she was a very close friend also of the Millikans. [tape off]

A second research project that Dr. Jones and I worked on that I think should be reviewed in this record has to do with hearing better in the presence of a noise. In the otological profession this goes by the name of paracusis and so the title of this reprint here is called "Paracusis," by Vern O. Knudsen and Isaac H. Jones. The reference is here in the Laryngoscope, I guess, for September, 1926. And it deals with our investigation of this subject: do you actually hear better in the presence of a noise? For example, on page 18 of this reprint is a photograph of W. A. Munson, who later became associated with Harvey Fletcher. He was then an undergraduate student at UCSB, [laughter] and this is a photograph illustrating some of our studies to indicate whether a person with a conductive hearing impairment actually does hear better in the presence of a noise.

MINK: Is that a gong there in back of him?

KNUDSEN: It's a loudspeaker, a Western Electric cone-type loudspeaker. That's the kind of loudspeaker that was used in the 1920s. This work, you see, was published in 1926. The work was probably done in 1925 or thereabouts, so it was relatively early in our work. Well, we investigated many subjects here. I forget the number, but there were fifteen or twenty that are listed here, and we show the audiograms of how well they hear in the noise and how well they hear out of the noise, and in every instance they hear less well in the noise than they do in the quiet.

MINK: Can I interrupt you just a second to ask how you got started on this particular aspect with Dr. Jones?

KNUDSEN: Yes. Because the audiometer, which we described last week and which is used for making a diagnosis of hearing, offered a means of investigating this hearing in the presence of a noise. There's a long history in the otological profession that attempts to explain _why_ certain people do hear better in a noise. In making a diagnosis, there was a standard question that the otologist would ask the patient: "Do you hear better in the presence of a noise?" And if the patient said, "yes," this was an index, so the otologist assumed that you were dealing with a case of conductive deafness. This type of deafness was a mechanical defect of some sort. And also some theories were advanced to account for the assumed beneficial effect of the noise.

At the time Dr. Jones and I were investigating this subject, the radio industry was just emerging. The kind of receiver you had then used a crystal, you know, and you had a "cat's whisker" (a fine wire) that made contact with the crystal and you'd have to move the "whisker" to find a live place on the crystal, which served as a little rectifier to convert the radio waves to audible sound. But this radio simile was used, and it was said that in some way the low frequency sounds energize the middle ear--that is, they mobilize the ossicular chain (consisting of the hammer, the anvil, and the stirrup) that connects the drum membrane into the internal ear. The low frequencies which are not heard very well produce a relatively gross vibratory motion of the ossicular chain, including the stapes that fits into the internal ear, and this actually makes the conductive mechanism of the middle ear more sensitive for the more important high frequencies. They said it's very much like finding a sensitive spot on these crystal receivers that radio receivers depended upon at that time. The phrase that the otologists often used was "the little ones (vibrations) creep in on the big ones." The big ones were the low-pitched sounds which caused a gross movement of the ossicular chain, and the little ones were the high frequency ones with a correspondingly smaller amplitude of vibration of this ossicular chain.

And so the most prevalent assumption, and one that was believed by most otologists, was that in some way these low-pitched sounds of noise do make the ear more sensitive for speech frequencies and for that reason persons who have a conductive type of impairment, derive a benefit from the noise. If you can get the stapes moving in its vise-like enclosure, that you essentially have in otosclerosis where the ligament fringe which surrounds the stapes is ossified, the gross low frequency sounds will force a limited mobility of the stapes and make the conductive mechanism more sensitive. This was the notion they had.

Well, we recognized that the audiometer offered a means by which we could determine whether the audiogram of a person with conductive impairment actually improved in the presence of a noise. We used low-pitched noises, we used high-pitched noises, we used street noise amplified, and we also used pure tones of low pitch and medium pitch and high pitch. The result was always the same: no matter what kind of sound we used, typical noises or pure tones, the patient under test always heard less well in the presence of a noise, or any audible sound. The result could be accounted for rationally by the masking effect of the noise.

The true explanation, which we definitely verified, and which had been proposed by Pohlman and Kranz, was that when a person with normal hearing and a person with conductive hearing talk to each other, the person with normal hearing is bothered by the noise whereas the person with conductive hearing is not bothered by the noise. Therefore, the person who has normal hearing talks louder to talk above the noise. The person with conductive impairment does not because he doesn't hear the noise, and therefore doesn't raise his voice. And so the explanation of paracusis was the effect which the noise has on raising one's voice rather than any peculiar effect which would be very difficult to explain of how noise can in any way make the hearing mechanism more sensitive. [tape off]

A popular account of this hearing better in the presence of noise, I note that I published somewhat earlier in the July 31, 1925 issue of the weekly magazine <u>Science</u>. The reference to this article, which is entitled "Hearing Better in the Presence of a Noise" begins on page 109 and ends on page 111. And it confirmed the findings of the earlier work I had done in the laboratory at UCSB with the help of Munson, who was our subject of a normal person at that time, and we had one or two persons who had impairments of hearing. This work there indicated that, really, no matter what kind of noise you had it interfered with the hearing of speech. [tape off]

A third paper that Dr. Jones and I published together--I can't find the reprint of it here--was entitled "Investigation of Tinnitus." Tinnitus is head noise--that is, it's a subjective sound that bothers people, but there's no objective outside source to give rise to that. I think everybody has experienced tinnitism of one sort or another; its often referred to as ringing in the ears. And I have induced tinnitus in my own ears as a result of overstimulation by sound on two separate occasions. In the investigation of hearing better in the presence of a noise we did use some very loud sounds, and I remember on one occasion I was listening to a very, very intense sound through earphones that were held a little distance away from the ears. This caused the noise, and then a person who doesn't hear the noise at all six feet away calls out these speech sounds and you write down what you hear, for example. And you find that you don't hear as well no matter what kind of noise you introduce here.

But we were using at the time a frequency of about 2,000 cycles per second, which is about one octave below the highest note on the piano, and the loudness of this tone was stepped up progressively, and the louder it was, of course, the more it interfered with the hearing of speech. But in the course of these experiments, I noticed that I had developed tinnitus in my own ear--that is, when I removed the earphones, I had a ringing sound, especially in my right ear. And this persisted for more than two days. We attempted to match the frequency of this tinnitus; we assumed, well, inasmuch as the stimulating tone had a fre-quency of 2,000 cycles probably it would cause most damage at that frequency--that is, the tinnitus probably would be of the same pitch. But the tinnitus was nearly an octave higher.

We recorded in one of our papers a brief account of this self-induced tinnitus that I experienced and which stayed with me for two or three days after this overstimulation of the ear. I'm sure many others have had tinnitus of that same sort. But the significant thing here was that we observed that the tinnitus occurred at a considerably higher pitch, and it was in the vicinity of 4,000 cycles per second. Well, it's pretty well known today that any kind of noise that impairs hearing produces the greatest damage for frequencies around 4,000 cycles per second. This has been more or less borne out by other types of tinnitus.

Furthermore, I induced a tinnitus in my right ear a second time when I was investigating the effect of explosive sounds. It is very well known that fire-crackers have caused deafness and gunfire causes deafness. Artillery men particularly have been victims of impaired hearing by reason of the excessive explosive sounds to which they are exposed. And I wanted to know how much I could tolerate and I said, "Well, I'm willing to do damage to my right ear, but I'm going to keep my left ear. If somebody is on the telephone I need one ear. And I needed it for my work in architectural acoustics. But I wanted to know. And I had to get a 38 caliber blank pistol so close--the way I described it at the time--it was so close that the flame that came from the firing almost burned the hairs protruding out of my right ear. I was holding the gun almost against my ear before I could get a measurable loss of hearing. But when I got the gun that close to the right ear, it not only produced a loss of hearing, which fortunately was temporary, but also my initial hearing loss went down something like 20 to 25 decibels. Again, this maximal loss was at 4,000 cycles--in that general neighborhood.

There is abundant evidence today that has come from many, many investigators that no matter what kind of noise you are exposed to, the greatest hearing loss occurs at this frequency range, usually between 4,000 and 6,000 cycles per second, and it's often referred to as the 4,000 cycle notch. That is, if you make an audiogram of a person with noise induced hearing loss you'll find pretty good hearing until you get up to around 2,000, then it drops down very sharply at about 4,000 cycles, then it rises again at higher frequencies. And this is characteristic of the early stages of impaired hearing as a result of exposure to noise. Well, here again I induced a tinnitus. I had not only a hearing loss that was in the 4,000 cycle range, but the tinnitus again was also in the 4,000 cycle range, induced by gunfire.

MINK: And how long did it last?

KNUDSEN: It lasted two or three days. And fortunately, as the tinnitus improved, became less and less loud in my right ear, the hearing also returned, and in a matter of a week my hearing was back to normal. And so this was just a temporary loss. But if I had repeated that day after day, I'm sure that ultimately there would have been permanent injury. That's the experience of the effect of noise on hearing. But this tinnitus in general, as it's found in the otological profession, is usually not induced by noise. Noise is only one way. It very often is a result of--well, today, it could result from Aureomycin. We did our work before the days of Aureomycin; but many people who have been subjected to Aureomycin treatment, which was quite standard you know, to prevent infections after surgery, have contracted tinnitus and deafness, and also vestibular disturbance. The VIII Nerve is especially sensitive to Aureomycin and to some of the other similar drugs that are often used to prevent infection. But childhood diseases--mumps, measles, scarlet fever--all of these things may cause not only impairment of hearing but sometimes they also will cause tinnitus. A blow on the head, an impact--prizefighters for example can sometimes be hit hard and this will induce tinnitus.

There are many forms of tinnitus, and very often this becomes so annoying to the person who suffers from tinnitus that it becomes intolerably loud, and he says, "I would rather lose all of my hearing than continue to have this tinnitus." Well, we investigated quite a number of cases of tinnitus. Frequently it is of a single pitch, sometimes it sounds like escaping steam. In one instance we found that it was associated really with the Eustachian tube. The Eustachian tube was actually opening and closing and making a little click, and when we listened with our own ears--Dr. Jones and I--to this person who came to Dr. Jones because of his tinnitus, when we put our ear against his ear we could also hear the clicks. And so this meant that it was not in his internal ear or brain; this was not in any auditory nerve. This was something that was happening some other place, and when Dr. Jones carefully examined the patient he noticed there was a peculiar twitch in his Eustachian tubes.

MINK: Spasm.

KNUDSEN: It's very much like a snap sound, and you could hear it: [snap, snap, snap] almost like the ticking of a clock. And he likened it then to the clicking of a clock because he didn't know where it was. Apparently it wasn't violent enough for him to be able to localize it, but when Dr. Jones examined his throat, he could see this thing clicking down there. It's very much like a little kissing sound or something of that sort.

MINK: Could he do anything for him?

KNUDSEN: We had only one means. Tinnitus has defied treatment. If it's an infection and you got rid of the infection you got rid of the tinnitus. But it's one of the most intractable of all otological defects to deal with. The most practical way of dealing with it, we have found, and it has been used in a number of cases since then, is to introduce in the room in which the patient sleeps--he's bothered most of all by his tinnitus at night and it keeps him awake--a sound that masks the tinnitus. This masking sound may be something that would simulate, say, the sound of a ventilating system. In the 1920s when we were dealing with this subject, we simply rigged up an electric buzzer device, with a cord which could be plugged into the 110-volt outlet. It had a transformer in it to reduce the voltage suitably, and it also had a little rheostat on it with which the patient could control the loudness of the buzzing sound. He'd simply turn up the buzzer noise until it just barely masked his tinnitus. As a rule, the patient was much more content to listen to an objective sound than to one that is generated within his own nervous system--from the VIII Nerve.

MINK: Now, I suppose they might use a tape recorder.

KNUDSEN: You could use a tape recorder today to simulate ventilating sounds or other things of that sort. It's used somewhat, but not widely; but we advocated this in our paper on tinnitus. I think Munson or Louis Delsasso, the brother of Leo, rigged up three or four of these for patients of Isaac Jones who complained very much about their tinnitus keeping them awake, and we got favorable reports from those who used this device.

MINK: And the work on mechanical devices that were used in this research was done down on Hyperion Avenue at your home, or was it done in a laboratory?

KNUDSEN: No. I think these were probably made in my little Hyperion Avenue shop. It was at the time we were making audiometers there, and this continued for a period of two or three years. I believe I have told you that we made a total of twenty-six. After that I closed the shop and gave its equipment to our Physics Department.

MINK: Is that place still there?

KNUDSEN: I don't know whether the shop is. We sold our Hyperion home in 1927 or 1928 to Tom Watson, who was then in the Mechanic Arts Department.

MINK: We talked to Floyd Watson; I believe he's now at Claremont.

KNUDSEN: That's someone else.

MINK: That's Norman.

KNUDSEN: That's Norman's father. Tom Watson died quite a while ago, but he was on the staff of the Mechanic Arts Department at the old Vermont Avenue campus and moved to this campus when we had, well, when we offered training in Mechanic Arts before we offered courses in engineering. Well, Tom Watson was here for a number of years.

MINK: As long as I have brought up Norman Watson's father, Floyd Watson, I know that in our interview with him which went on last year, I believe, we found that he was rather reluctant to discuss his work in acoustics. We could only get him to go so far with it. And he said, "Well, I think this is pretty well known. I think this is understood."

KNUDSEN: I'm afraid that may be an evidence of senility. But F. R. Watson really was one of the early workers in acoustics. After Wallace C. Sabine, he probably would come as number two among the early American workers on architectural acoustics. And his work at the University of Illinois was very important. It dealt with a number of practical problems. Possibly the most important had to do with the improvement of the acoustics of a large auditorium on the University of Illinois campus at Urbana. This was in a very faulty shape. It had a huge dome and curved walls, circular in plan, and therefore they were bothered very much with echoes and sound focusing effects and difficulties of that sort. Floyd Watson really analyzed this very carefully and recommended remedies--the use of sound absorbents in the right places to absorb the sound waves that were giving rise to these echoes and focusing effects. It greatly improved the acoustics of that place, and it was one of the early examples in matters other than reverberation--that is, you had to deal with the shape of the hall, and Watson did this very satisfactorily there.

I recall that he reported his research on this large auditorium at a meeting of the American Physical Society, probably as early as 1921, when I first met him. It may have been as late as 1922 because I know I was presenting a paper at the American Physical Society at the same time on my doctoral dissertation--"The Sensibility of the Ear to Small Differences of Pitch and Loudness." And Professor Watson made it a point to meet me at the particular meeting; we've had a close friendship since then. His two sons, Norman and Robert, came to UCLA as graduate students to specialize in acoustics. And his own work in architectural acoustics, I probably should add, had some bearing upon my becoming interested in this field. I am afraid I omitted mentioning that at the time. But later he did participate in the establishment of the Acoustical Society, and I will have more to say about Watson and his work at that time.

MINK: Yes. While you were here in the early days of the Southern Branch, did you have much association with him?

KNUDSEN: We corresponded. And I occasionally went to meetings of the American Physical Society, and I met Watson at some of these meetings. And we both were publishing in the field of architectural acoustics, so we kept in touch with each other. [tape off]

There are two other research projects in the field of hearing that were carried out at the Vermont Avenue campus. One of these dealt with "Hearing with the Sense of Touch." That is the title of it. It was published in the Journal of General Psychology for April, 1928, and these pages here are from 320 to 352, so it's a fairly comprehensive investigation of this subject.

MINK: How did you come to undertake this particular investigation?

KNUDSEN: There was interest in this subject, first of all, because Helen Keller was supposed to use her sense of touch. She'd hold her fingers on the throat of her teacher to feel the vibrations through the tactile sense, and it was generally known that hard of hearing children often were asked to put their fingers on the sounding board of the piano to sense the vibrations. And particularly at this time, Professor Robert H. Gault at Northwestern University was actually trying to train deaf children to hear using their tactile sense.

MINK: And you corresponded with him?

KNUDSEN: There's one or two exchanges of letters. I think I have one or two letters from Gault, and I wrote to him inquiring about the work he was doing there. It seemed to me that if the kind of work Gault was doing was to get anywhere, we should know more about the capabilities of this tactile sense, the sense of touch, to respond to vibrations, say, sound vibrations, which could be converted into mechanical vibrations. You can feel the cone of a loudspeaker vibrate, for example. You can feel a vibrating string if you can hold your finger gently against a string; similarly against a tuning fork, or against the belly of a violin, or the wood of any other of the string instruments. These can be readily detected by the sense of touch, and it seemed to me if Gault's work was going to succeed, we must know what the possibilities of this tactile sense would be for the hearing of speech, must know specifically what is the sensitivity of the sense of touch for sensing and analyzing sound vibrations. How large an amplitude is it necessary to attain before the sense of touch is aware of it? And how small a difference in frequency or in amplitude of vibration can the ear detect? This turned out to be an interesting part in my investigation because of my doctoral dissertation.

MINK: Had Isaac Jones been interested in this, too?

KNUDSEN: No. I think not. I gave the date of this, didn't I?

MINK: It was in 1928 that this article was published.

KNUDSEN: April, 1928.

MINK: Yes.

KNUDSEN: Well, then the major work was done at the old campus. Yes. I had forgotten about that. Well, I know he was inspired by it; but this was an independent study that we conducted at the old campus of the university and the brother of Leo Delsasso was my assistant on this project. We worked together, and Louis (probably with advice from Leo) built our vibrator. For example, on page 323 of this article, we show the circuit diagram that we used and the means of measuring the amplitude of vibration and the actual vibrator here is shown somewhere. I thought we had a photograph. It is shown in this same figure 1, in schematic form. It was essentially a vibrating reed that was activated electromagnetically, and the subject simply placed his fingers on the vibrating "reed" or lever. We usually used the index finger, although we found that there is essentially the same sensitivity for all of the fingers, and it didn't differ very much for other parts of the body's skin. This was one of the significant findings. But inasmuch as you had five fingers it might be possible to use all five fingers on five different vibrators and thus do something about analyzing the pitch.

But let me review. The investigation was concerned with an experimental determination of the sensitivity and the sensibility chracteristics of the sense of touch to a vibrating body. The sensitivity means how small an amplitude can you just barely detect at a given frequency, and the sensibility, as we used it then, meant how large a change in amplitude and how large a change in frequency is necessary to just be barely discernible. And you see, we can compare these sensibility values for the sense of touch with the values I previously determined in my doctoral dissertation for the sensibility of the ear to differences of pitch and loudness. So we were concerned here with a comparison of these characteristics, comparing the sense of touch with the sense of hearing. For example, you can hear sound vibrations throughout a range of approximately ten octaves. We know that in the case of the ear, from 20 cycles per second to 20,000 cycles per second, and he can distinguish small differences in amplitude and frequency which we reviewed when I discussed my doctoral dissertation.

But we wanted to know what we could do here, based on a determination of the lower and the upper frequency limits of a vibrating body. How low a frequency can you detect, as compared to the 20 cycles per second for the ear? How high a frequency can you detect? What is the sensitivity of the sense of touch to a vibrating body as a function of frequency? How does it vary? How wide a range of amplitude or intensity? For example, for the ear the intensity can be diminished very, very much as you go up in frequency from 20 cycles per second. You have to have a large amplitude to be detected there to be heard at all, whereas the greatest sensitivity is in the range of 1,000 to 4,000 cycles per second. We wanted to know, just what is this sensitivity curve for the feeling of the tactile sense of vibration?

MINK: Again, I suppose you used students from the campus as subjects.

KNUDSEN: Yes. We did. The actual photograph of the vibrator that we used is shown on page 325, figure 3. It's a photograph of the vibrator, showing the mechanical arrangement of the armature, the vibrating lever, and the micrometer U-frame for bringing the finger into gentle but optimal contact with the vibrating lever; so that you didn't vary the pressure against this vibrating reed or lever that we have there.

SECTION XVI

KNUDSEN: The experimental results, I think, are of interest here, because I think this is the first time the attempt had been made to determine the frequency range and the intensity range that the sense of touch is responsive to, to the extent that it might possibly be used for hearing of speech and music. When the frequency of this vibrator was less than about 15 per second, what you were aware of was the separate impact, just like separate hits against the ear. At about 15 to 18 vibrations per second, which is pretty close to 20 or so, which is normally associated with the ear, you were no longer aware of the separate impacts, but it was a sensation of vibration then. You felt the vibration as a single stimulus instead of a series of separate impacts. And, to explore the upper limit we found that it certainly was as high as 1,600 vibrations per second. Above that it fell off very much, but also our vibrator was not very sensitive at frequencies above 1,600 cycles per second; so that we could say at any rate the limit is certainly not lower than 1,600 cycles per second, and it's probably somewhat higher. But when we investigate the way this sensitivity varies with pitch, we plotted a curve here for the frequency range of 32 cycles per second to 1,600 cycles per second. That was the upper range. And the maximum sensitivity occurred at a frequency of 256 cycles per second. We obtained the relevant data for four subjects; the four subjects differed somewhat in their minimal threshold for sensing the vibration. But for three of the four subjects, the range for minimal threshold was about 10 decibels, which is a factor of 10 in intensity, or about 3 in amplitude. The average minimal threshold amplitude was about 0.0001 cm at 32, 64 and 128 cycles per second, and only 0.00001 cm at 256 cycles. At higher frequencies this sensitivity decreased--e.g., at 1024 cycles the minimal threshold was about 0.0001 cm. The upper (painful) threshold at 64 cycles occurred at an amplitude of 0.235 cm, which is approximately 2500 times the minimal threshold amplitude.

MINK: And that table is on which page?

KNUDSEN: These curves from which I am quoting are on page 328.

MINK: Page 328 of "Hearing with the Sense of Touch."

KNUDSEN: Yes. The sensitivity increases as you go from 32 vibrations per second to 256, where it is possible for our best observers, three out of four, to detect a vibration the amplitude of which is as little as .00001 of a centimeter. It appears, therefore, that the sense of touch is most sensitive to vibration at about 256 cycles per second, and it then becomes less and less sensitive as you go to higher frequencies. We are not able to obtain with our primitive vibrator enough intensity or amplitude above 1,600 cycles per second to actually give a sensation. But with a better vibrator it probably would be possible to feel higher frequencies than 1,600 cycles--I guess at least an octave higher. I don't know whether anyone has pursued this subject further.

The next thing we investigated was: how much do you have to change the amplitude or intensity of vibration in order to recognize that there has been an increase? And we have some curves on pages 332 and 333 that answer that question. The most significant thing about these curves is that the shape of the curves is essentially the same shape as you have for the ear in sensing the smallest perceptible change of amplitude or intensity. Furthermore, when the intensity becomes great enough, the sense of touch can discriminate as small a change in amplitude or intensity as the ear can. And I think those who are interested in the evolutionary theory of hearing were interested in that finding, because I believe evolutionists in general regard that the ear did develop from a skin, a tactile type of sensor, and that probably our own cochlea as we have it today, our hearing mechanism, did begin as a sensitive tactile sensor, and it subsequently improved over its frequency range and especially to distinguish differences of frequency. And inasmuch as the skin can still detect the same percentage change of amplitude and intensity that the ear can, for its limited frequency range, this I think was the most significant finding in our investigation.

Now this was, of course, very encouraging--the possible use of the sense of touch to substitute for hearing for those who had lost their hearing. But we were disappointed when we came to investigate the sensibility to differences of frequency. For example, we had to change the frequency at 64 vibrations per second by as much as 12 to 15 percent to recognize a change in frequency. In contrast, for hearing, the ear can recognize a change of frequency of less than 1 percent. And this, you see, is a serious limitation for hearing with the sense of touch. And at 512 cycles it was getting worse. The best frequency sensibility was at 256. We had just two subjects for these tests. Louis Delsasso could detect a change of frequency as small as 8 percent at 256 cycles, and the smallest change I could detect was 10 percent. Probably as many as 25 to 30 gradations of frequency can be detected by the sense of touch between 16 and 1,600 cycles; the ear can detect as many as 1,000 gradations of pitch between the same two frequencies. And so, it was disappointing to find out that the sense of touch was relatively crude in that respect. Probably, about the best you could accomplish by utilizing the sense of touch for hearing would be to have 10 vibrators at 10 different frequency ranges and use your 10 fingers, one finger for each of the 10 ranges. Some work has been done; but at any rate this part of the finding was of such a nature that it meant there were severe limitations on how well you could understand speech. And we actually conducted quite a number of tests, and obtained some oscillograms (reproduced on pp. 343-347) of different vowel sounds and simple words to see whether they could be detected. And we did get the rhythm, of course, and some sense of pitch, but in our limited experiments, we could not understand speech.

MINK: Through the sense of touch.

KNUDSEN: Through the sense of touch. It was rather disappointing in that respect. And I think our findings had an effect on diminishing activities in trying to regard touch as a substitute for hearing. I continue to believe however, that it can be an aid in many instances to supplement lip reading; I called attention to this possible aid in the paper, but so far as I know, nothing of consequence has come of it.

MINK: I think there is a question that I have been thinking of, and that was this: as you went along in this research and other research, was Dr. Moore interested? Did he stop you on the campus and ask you how you were coming along?

KNUDSEN: Oh, yes. Yes, Dr. Moore was much interested in my research. And so was President Sproul when he became president. As a matter of fact, during many of Sproul's public speeches he referred to the work that Ernest Lawrence was doing on the cyclotron at Berkeley and the work that Vern Knudsen was doing in acoustics at Los Angeles. And Moore on many occasions-- I had a note from him, I know. Maybe it's in the file there. I guess maybe he was the first person to whom I gave a copy of my first book, the Architectural Acoustics, which came out in 1932; and I had a most cordial and congratulatory note from Ernest Carroll Moore in which he referred to my 619-page opus. I remember that.

MINK: I suppose in making points in academic work it is very important that you see that all your publications are sent to the provost and the president.

KNUDSEN: Yes, but it can be overdone. I don't think I sent the reprints; but I did give Ernest Carroll Moore my first book. He was a good friend of mine.

This was before I was dean. In 1932, the book came along, in the summer of 1932, and I'm quite sure there is in the file somewhere here a copy of Dr. Moore's letter to me.

MINK: I was thinking in terms of this, that we will be talking, and maybe this is not the important time to bring it up, but do you feel that this research that you carried on in this decade had an important bearing on your appointment as dean of the Graduate Division?

KNUDSEN: Yes. I think it had. Certainly they wanted someone who was research-minded and who had a reputation. It probably was as important as anything else.

MINK: But weren't there any other people on the campus that were doing research too?

KNUDSEN: Yes, yes. Shepherd Ivory Franz was certainly outstanding in psychology as was Charles Grove Haines in his [field].

MINK: In political science.

KNUDSEN: Yes. They certainly were doing research which had received as much attention as mine. Sproul had called attention to mine on a number of occasions, and I'm quite sure that must have influenced Sproul in finally making the decision. I don't know how many people might have been recommended by a committee that

Sproul had appointed to recommend someone for the dean of the Graduate Division. My appointment came at a time before I had published my most important papers, two of which came out in the next three or four years, after this date of 1933.

MINK: Did you expect that you would be appointed dean?

KNUDSEN: No.

MINK: You did not.

KNUDSEN: No. I did not. It's the only administrative position I've ever had any ambition about. I was delighted when Sproul called me in and said that he wanted me to take over the deanship of graduate work in the Southern Section of the University. And Ernest Carroll Moore stopped me soon after, and he said, "You have been appointed to the most important position on this campus, including my own."

MINK: Is that right?

KNUDSEN: Yes. He apparently regarded the dean of the Graduate Division as being that important. As a matter of fact, I had no ambition at that time or later of ever becoming chancellor or provost or anything of that sort. I dismissed it entirely from my mind. But I knew I was among several that were being considered for the deanship, because I was a member of the committee that had been appointed, oh, about two years before. For one year, this committee over which Ernest Carroll Moore presided, had as members Charles G. Haines, Shepherd I. Franz and Marvin Darsie, who was dean of education, and I think Gordon Watkins and John Parish or Waldemar Westergaard and Sigurd B. Hustvedt. I'm not sure about Westergaard. Parish was on that, I believe, at that time. And obviously those who had been named to this committee would be among possible candidates for the deanship, because they had been guiding the work of the early planning for the Graduate Division. So it wasn't a complete surprise to me, but I certainly expected someone like Shepherd I. Franz or Charles Grove Haines to be asked to take over the job. They were considerably older, and it may be that they were considered too old, that it would be better to have a young man in this post. I was quite young at the time, thirty-nine. But, we'll come to that later.

MINK: All right. I only have one other question, and that is that I think that you did say, didn't you, in the speech that was given at the Twenty-fifth Anniversary Observance, this was one position which you had always wanted.

KNUDSEN: Yes.

MINK: Is that right?

KNUDSEN: Yes. There's one other paper I wish to review briefly. I won't take very long.

MINK: Let's go ahead with that.

KNUDSEN: There's one other paper here which Norman Watson and I published in--I see it is as late as January 22, 1940. No. That's when it was submitted. This was published in the April issue of the Journal of the Acoustical Society of America, 1940. That's volume XI, and the pages are 406 to 419. The title is "Selective Amplification in Hearing Aids," and although this was published as late as 1940, the germ of this research really dates back to the first paper by Isaac H. Jones and myself that we reviewed a week ago, and there had been really quite a controversy among acousticians regarding the possible value of selective amplification for the hard of hearing.

MINK: I think you said, did you not, that as a result of that initial research you did with Jones you did develop a number of hearing aids?

KNUDSEN: Yes.

MINK: These were given to patients, including Grace Fernald and Fuller Stoddard.

KNUDSEN: That is right. There may be a total of, oh, five or six that we made for a number of people. Grace Fernald is one I remember very well. Fuller Stoddard, who was the inventor of the self-player piano, you know, the Ampico piano was another. And they were both persons I knew very well. Fuller Stoddard, I believe I told you, was the first treasurer of the Acoustical Society of America and we saw a great deal of each other. Incidentally, he had a very peculiar diplacusis. I don't know whether we mentioned that when we discussed diplacusis before; because when we examined Fuller Stoddard here at this campus, we had good facilities for measuring hearing phenomena, and he was not aware that he had a diplacusis until we made this audiometric examination of his hearing here. We were going up the scale on high pitch, with the audiometer, and he said, "The pitch isn't changing! It isn't changing!" Then suddenly when the pitch got higher, "Oh, it came back again! But it's much higher than it was." And so there was a big gap (or "island") in his hearing in which all he heard was a buzz of some sort. He said it wasn't a tone; he said it sounded like a buzz in this range, for this peculiar sympton of diplacusis. This was only in one ear, and so we made the hearing aid to fit his other ear, the better of the two ears. That was always the case when we made hearing charts, audiometric charts, of how well they hear as a function of frequency or pitch; then this was a guide of the kind of amplification we should introduce in the hearing aid, and in general if they had a greater loss at high frequencies than at low frequencies, we suppressed the low frequency somewhat and emphasized the high frequencies.

The study which Dr. Norman Watson and I reported in 1940 was based on a more careful examination and investigation of a few selected subjects. They were mostly graduate students at UCLA, but one of them, an Ed Fricke by name who later got his PhD in acoustics here, had a very marked high frequency hearing loss of the perceptive type. It was a nerve deafness; and it was largely for these that we felt that selective amplification was justified, and for whom you should modify the frequency characteristic of the amplification to best fit their hearing. The criterion Norman Watson and I found was most satisfactory was what we called the most comfortable loudness criterion. We would find first what was the most comfortable loudness level at a pitch of 1,000 cycles per second. That's more or less a standard frequency in hearing problems because it is about in the middle of the range between the low pitch and the high pitch. And so the criterion we used would be first of all to find out what is the most comfortable level?

With persons with impaired hearing, they don't hear at all until you reach their threshold. Once you reach their threshold and you then increase the intensity a little, and you find that the loudness increases very fast, much faster than it does for a person with normal hearing, and that is also a diagnostic means for determining that the impairment is in the internal ear or the brain rather than in the middle ear or the mechanical part of the hearing mechanism. And then we produced an equal loudness contour, based on our finding for the 1,000 cycle tone. When we found the most comfortable loudness for this middle frequency, then we would adjust the gain of the amplifier so that it was the same loudness at both lower and higher frequencies. We would use that particular shape of the amplification curve because for persons with cochlear or nerve-impaired hearing, if a tone or any sound gets very much above their threshold, it's unpleasantly--they say, "painfully loud." This is the phrase they use: "Oh, it's painfully loud." They will shout that, so that you have only a very narrow range between a sound that is just barely audible and a sound that is so loud it's unpleasant. And therefore we adapted the amplification curve to conform to this most comfortable equal loudness contour. Subjects such as [Edward] Fricke could hear definitely better with that criterion than with any other kind of amplification, including especially uniform amplification. [tape off]

The results of our findings are illustrated in figure 13 on page 417, and especially in table 5 on page 418, in which we record what the percentage articulation was. We defined that before, as you know, as how many typical speech sounds you hear correctly per 100 speech sounds. And for 7 observers that we studied very exhaustively--I think in one case (Ed Fricke) we called out as many as 5,000 speech sounds, using three kinds of selective amplification. One of these was based on our "most comfortable loudness curve," one with less emphasis, and the other with more emphasis on the high frequencies. With the high frequencies over-emphasized for one subject, we got an articulation of 47 percent; using the best criterion--that is, the most comfortable loudness criterion, 61 percent; and for under emphasis 56 percent. For another subject the highest score was obtained with, again, our criterion, 87 percent; and with uniform amplification the score was 80 percent. In one other case (a mild hearing loss) we got 97 percent with the most comfortable loudness criterion. But we got 95, 91, and 90 percent by other types of amplification, which were characteristic of typical hearing aids. And for a fourth case we got 86 percent with our most comfortable criterion, and 82 percent with uniform amplification--that is, for all frequencies amplified the same amount. And in one other case--I won't go over all of them--the highest score of 79 percent we got was with our criterion, and the closest we could get with any other kind of amplification was 73 percent. So there was a difference between 79 percent and 73 percent, and when you remember that 75 percent is considered satisfactory hearing, then the difference between 79 and 73 is significant. And in still one other case, we got 96 percent with the best criterion, the one we were using; and the next best to that was 91 percent.

So these findings, we were convinced, gave justification for the use of selective amplification. The effect is not as big as one might expect or hope for, but it is in the right direction, and it always was significant. I know if I had a cochlear or nerve hearing impairment I would use this criterion for prescribing my hearing aid, and if my hearing was about the same in both ears I would also use a binaural hearing aid.

MINK: Oh, yes. I have a couple of questions. One of the things I was wondering, I know that when we talked earlier about the publication of research with an advisor or the person under whom you worked, that when you published something, their name always appeared with yours. But what determines whose name goes first on a research paper?

KNUDSEN: Well, on this one Watson's name came first, because I felt he had done more of the work than I had.

MINK: This would be in general the way that you've tended to go in submitting your research work that's been in collaboration, too?

KNUDSEN: Yes. I have never associated ny name as co-author of a candidate's dissertation for the PhD degree. He has always published under his own name alone. This 1940 paper was for research after Watson had got his PhD degree; he was probably an assistant professor here at the time we did this work in 1939 or 1940. He had got his degree two or three years before that. He was an instructor or an assistant professor when we did this work here. It was one of the early works we did together. We later did a second project in which he did most of the work, and that was also published by Watson and Knudsen. That was on ear defenders. We'll discuss that later.

MINK: I take it, then, that when you say you've never associated your name with a doctoral candidate in a publication--did you resent the fact that this was necessary when you were taking your degree? I think you said something that made me feel that you perhaps thought this was not fair.

KNUDSEN: Well, I have always felt that the doctoral dissertation should stand on its own feet, and that the candidate himself should be the sole publisher of it. Now, however, with the way physics has developed, especially in nuclear physics, four, five, or even six people may be working together, and usually under the supervision of a man like Ernest Lawrence, for example, or his successors at the radiation laboratory in Berkeley, and also in several branches of physics at UCLA. There, I think, it's almost impossible to avoid joint publication.

MINK: Now the other question I had was, inasmuch as you no longer have your laboratory down on Hyperion, could you try to describe what it was like, because so much of the work was done there.

KNUDSEN: Yes.

MINK: The work we have been talking about.

KNUDSEN: Yes. Much of the work we have been talking about, and that with Isaac Jones, utilized the facilities that we had at this little shop or laboratory at the back of my Hyperion Avenue home. Incidentally, Isaac Jones put up the money for building the little house. It was a single frame structure. I don't believe that it cost more than $1,000, maybe less than that in those days. It was about I would say 12' x 16'. It had a lathe, a drill press, and other shop equipment, mostly small tools, the total cost of which would add up to about $500.

MINK: Cement floor?

KNUDSEN: Cement floor, yes, concrete floor. And there was a bench across the entire length of one side, which faced the south. We had good light there, but we had awnings over the windows to keep the sun out when the sun was shining through too brightly. But it was nothing elaborate at all. It was just wood studs and shiplap, you know, overlapping wood boards on the outside, and simply a tar and paper roof covering. But it was still there in 1927 when we sold it to Tom Watson which was about eighteen months before we moved to this campus in 1929. Jones had given me title to the shop and all of this equipment which we had there. I gave all of the equipment and tools to the Physics Department and it became the beginning of the student shop. It's still here, the lathe, the drill press, and many of the hand tools. It was my first gift to the university, the apparatus and machinery that we had for making these audiometers and hearing aids in the Hyperion Avenue place. I wanted to get out of the business of making things for sale.

MINK: I think another question that I had--to sort of fill in on this--in regard to Millikan, you said that you suspected that there were some people who looked down on this applied research as opposed to pure research. Do you think that Millikan looked down on what you were doing?

KNUDSEN: I can't answer that categorically. He was always very cordial. He sometimes sent people to me that had hearing impairments. He would want to know about such matters. No. I think he was tolerant in general, altogether. I think he felt at one time, at Chicago, that I should have worked on modern physics instead of on acoustics or classical physics, and on a pure problem instead of on an applied problem.

MINK: The one that he had in mind for you to do for your dissertation, which you did not want to undertake?

KNUDSEN: Yes. I think he got over that pretty well. There was a good feeling, I'm sure. Millikan was always very kind to me, and I've had some letters from him about matters that would indicate that he approved of my teaching and research at UCLA.

MINK: But you seemed to feel, did you not, that some of the people at Berkeley felt this?

KNUDSEN: Yes. I think there was a general feeling among physicists that the work I was doing was of applied nature. Barnett here, I'm sure, also held that opinion very strongly. And possibly E. P. Lewis did up north; I'm not sure about that. I gave an invited lecture on acoustics at Berkeley in the early thirties. Ernest Lawrence was always very cordial and was enthusiastic about the kind of work I was doing. We talked with each other a great deal. He came to Berkeley a little while after I did, but we met each other quite often, socially and professionally. When we come to the antisubmarine warfare program, I will have something to say about Ernest Lawrence's help on our project down at San Diego.

MINK: Now as far as Ernest Carroll Moore was concerned, I guess it was just the opposite. He thought this was fine.

KNUDSEN: Yes, he did.

MINK: He was more of a humanist. He wanted to see some tangible results coming out, you would say?

KNUDSEN: He could understand my research, and I think he was sympathetic, because it did have a human relationship. And I think Sy Ramo in the speech that he gave on the occasion of the dedication of this building, sensed that the key to my life's success had been this combined interest in science and in humanity, the tying of these two things together.
[tape off]

I would like to say a few more things about Dr. Isaac Jones, because his relationship to UCLA and to the otological aspects of acoustics were of deep concern to him and to me during the years that we worked together, and it was at that time that he organized, first of all, what was called the Research Study Club at Los Angeles. It began really in 1922 when he invited eleven otologists from throughout the country to come to Los Angeles to meet with Robert Barany, who had received the [1914] Nobel Prize for his work on investigating the vestibular part of the internal ear, and had invented what is known as the "Barany chair," which Issac Jones and a few other otologists used a great deal, even before World War I, in testing the vestibular part of the ear. It's a rotating chair. In a typical vestibular test, the person under test is rotated in the chair 10 times in 20 seconds, and then the examing otologist watches the eyes of the person under test. This turning normally induces nystagmus, which is a motion of the eyeball, and you can see it move back and forth very conspicuously. The doctor keeps his eye on the patient's nystagmus to see how long it continues in motion. And for a normal person the average duration after this kind of turning in a Barany chair is, provided the head is very definitely in a vertical position, about 24 seconds. With the head in this vertical position, you are testing especially the horizontal canals of the vestibular mechanism, because with the head in the vertical position the rotation produces the maximal flow of fluid through the horizontal canals.

MINK: The semicircular . . . .

KNUDSEN: The semicircular canals. There are a set of three such canals, and they are at mutually perpendicular angles so that they would correspond, say, to the three planes of a rectangular room--the horizontal, the transverse, and the vertical--these three directions. By holding the head either in a vertical position or in a horizontal position, you test selectively the horizontal and vertical canals. The sensations produced by the motions of the endolymth fluid through these canals determine how you are moving. That is, motion is very much determined by the flow of this fluid through the semicircular canals, and excessive flow, as occurs sometimes when traveling over rough water or air, can induce "seasickness." It is the same fluid that flows also through the cochlea.

But returning to this nystagmus, for a normal person it should persist approximately 24 seconds. And this was one of the criteria that Dr. Jones and a few others had used for selecting aviators who were "fit to fly" in World War I. If the nystagmus persisted much less than 24 seconds, this finding was usually a symptom of impaired vestibular function. Well, the Research Study Club, as I say, was first set up to meet and learn from Dr. Robert Barany, who was the great expert on the vestibular part of the ear, and in describing this first meeting Isaac Jones said, "We invited eleven distinguished otologists and thirteen came." And that was the beginning of what has developed into a very important medical activity which now has the name of the Mid-Winter Clinical Course. It now is not only otology, but it is otorhinolaryngology, these three--ear, nose, and throat, and also ophthalmology. So it's now a clinical two-week course for the eye, ear, nose, and throat specialists. Ever since 1931 it has been an international organization. Dr. Jones was the guiding and working force that fashioned it and kept it growing. It meets regularly each January, and it was of interest to me because from 1932 on I was invited to give lectures on the physics of impaired hearing, the development of audiometry and hearing aids, each year for some eleven years, consecutively, except during 1941-45 when I was away on war research.

MINK: May I ask you if the proceedings of the meetings were published?

KNUDSEN: Yes. Many of them were. Many of the proceedings found their way ultimately into print. Those that dealt with the VIII Nerve which is concerned both with hearing and with motion sensing and equilibration--that is, the auditory and vestibular parts of the VIII Nerve--were described in detail in a book which

Dr. Jones edited; it contained, among other features, the papers that I have already reviewed, also the other work that Jones and I did together. The title of the book implies that it does include both vestibular and auditory aspects. The title of the book is The Eighth Nerve; it was published by the Research Study Club, as it was called at that time, which is now the Mid-Winter Clinical Course. This book is of interest to UCLA and my part in it, first, because at the time I terminated my annual lectures to the group, and because I became so involved in the graduate division and in my duties here at the university, and my preferred research in architectural acoustics, that I decided to discontinue further work in that field. Besides, Dr. Norman Watson was specializing in the field of hearing and was well qualified to continue the research and lecturing in this field. The book and the annual programs I have in my file will be turned over to the university sometime. They describe the meetings of the Mid-Winter Clinical Course, held usually at the Elks Temple in downtown Los Angeles, but during the early years, they always reserved one-half day to come out to UCLA, to learn about what we were doing in research on hearing.

MINK: Well, then, in the period when you were at the Southern Branch, they came to the Southern Branch, too?

KNUDSEN: Yes. Some of the members came there once or twice, but the formal visits were to Westwood, to the hearing laboratory, beginning in 1930. In 1930, 1931, 1932, in those years especially, I remember, we were hosts to the Mid-Winter Clinical Course here at UCLA. Incidentally, Isaac Jones had an ulterior but laudable motive in that. He wanted, even then, UCLA to establish a post-graduate school of medicine. He felt that the way to begin medicine at Los Angeles was to have it affiliated with UCLA and to start at the post-graduate level, and he began advocating that as early as 1930. I would like to refer . . .

MINK: May I ask you . . .

KNUDSEN: Yes.

MINK: Pardon me for interrupting. What about the Barlow School of Medicine which was in existence, and of course the USC School? I believe Barlow at one point merged into the USC School. Was Jones involved with these people at all?

KNUDSEN: To a certain extent, he was. The Barlow institution was a postgraduate institution, but it dealt largely with, oh, very specialized problems, some of which did deal with problems of hearing and vision, and to that extent Isaac Jones was interested in it. And later this institution became the Post-Graduate School of Medicine at the University of

California. But it terminated later. Bennet M. Allen, you may know, was a liaison person between the UCLA campus and this North Broadway Institute. We always referred to it as the North Broadway Branch of the Medical School of the University of California. It did not enjoy a high reputation at that time, especially as a postgraduate school of UC.

MINK: And it was, of course, the earliest UC department in Los Angeles, having its offices here as early as 1893 or 1894, I think, being called then I think the University of California Medical Department, Los Angeles.

KNUDSEN: Yes. I'm quite sure you're right. I can't verify the date, but you probably have already verified that.

MINK: So we see, then, that the relationship of medicine to the University of California here in Los Angeles goes back even before the Southern Branch.

KNUDSEN: Yes. You see, Isaac Jones's closest friend probably was Edward A. Dickson. They together established the Los Angeles Tennis Club, which continues to this date, as you know, and it's an important sports activity. Isaac Jones and Edward Dickson played tennis together on Isaac Jones's court for a number of years and Jones as well as Dickson was interested in the development of UCLA. One of the ambitions Isaac Jones had was to become a regent of the

University of California, and he would have been a very useful regent, I must say.

MINK: Do you know whether Regent Dickson ever proposed him to become a regent?

KNUDSEN: That's private information between the two close friends. I do not know for sure, but I believe Edward Dickson did, because he was a great admirer of Jones.

MINK: He worked behind the scenes?

KNUDSEN: Yes. I have no direct evidence, but I know that until Isaac's death day the Edward Dicksons were very close friends of both Dr. and Mrs. Jones. They were inseparable friends, the closest of friends, and Edward Dickson had often sought the advice of Isaac Jones and vice versa. They talked over many subjects together, and Isaac knew what was going on at the University of California, and he had this very intense hope about postgraduate medicine at UCLA.

One of the Mid-Winter Clinical programs in 1933 was a rather interesting one. My talk for the convivial evening dinner program was "A Son of Knute, by Knudsen," (title by Isaac). Another invited talk was by Ernest Hall, who was also a Mormon, and was professor of pathology at USC at the time. He had formerly been professor of pathology at Stanford University. He was an orthodox Mormon, much more than I was. His title was, "Mormons and Morons." And Isaac commented in a note that he sent to me following this evening entertainment, "Hall said Knudsen and Jones were Mormons and surely not morons." Ernest Hall surprised us with a really charming talk, which he had carefully prepared; he was a distinguished pathologist and greatly admired. On the back of this program Isaac Jones sent me was a copy of a letter that he had just sent to Ernest Carroll Moore. It said, "Knudsen, how's this? Send on this program to provost E. Z. Moore." You see, it's E. C. Moore and he just put this E. Z. Moore," and he means he had been easy on working up interest in a medical school at UCLA. That's what he meant by "E. Z." He had been easier than he ought to be in establishing this postgraduate medical school at UCLA that Jones thought should be established, and so did Edward Dickson. Remember, this was 1933.

MINK: You mean, then, Moore was for it?

KNUDSEN: Not necessarily. No. No. Jones meant that he was too easy on this subject.

MINK: Oh, he didn't say enough about it; he didn't push it enough. I understand, yes.

KNUDSEN: Yes. And I think you'll get it because I think this brief letter is worth quoting. "The Research Study Club would appreciate it if you would express to Professor Knudsen and so many members of the faculty the pleasure that our distinguished visitors had in meeting them, as well as catching a glimpse of their scientific work. As you know, in our intensive course we carefully saved an entire afternoon for our university. We want these doctors to go back to all parts of the United States with an appreciation of what you are accomplishing. Cordially." Then he said (on the note to me) "Wow! I hope it cracks him in the spot it's aimed at." And the spot it was aimed at was to get him active on the establishment of a school of medicine on the UCLA campus. Isaac had a special way of prodding people into action.

MINK: Well, how active did he really become? Did you ever talk to him about it?

KNUDSEN: Well, Isaac Jones?

MINK: No, to Moore.

KNUDSEN: Yes, oh yes. I talked with Moore about this several times.

MINK: Did he get the message?

KNUDSEN: I think he did. He knew the time was not quite ripe. You know money was tight at that time. Also there certainly was no great sympathy statewide for the establishment of a medical school at Los Angeles. There had appeared about this time one of the first reports about higher education in California, and this report definitely said there's no need for another medical school. And it also said there was no need for another architectural school. And so these professional schools for UCLA were long delayed. The report didn't say the same thing about engineering, so engineering came sooner than medicine, and architecture came last of all.

MINK: He probably wouldn't have had a very sympathetic ear in Sproul either, would he?

KNUDSEN: No. Sproul moved about as fast as he thought he could, and of course the medical school at San Francisco very definitely was not in favor of an early establishment of our medical school. Later, when the committee (regarding a medical school at Los Angeles) was set up--I happened to serve as chairman of that committee--we had had the finest help from such northern medical men as Langley Porter who was then dean of the medical school at San Francisco and from Howard Naffziger, the great brain surgeon who later became a regent of the university, and they were then 100 percent with us in planning the program for our medical school which was finally underway, as you know, in 1946.

Well, I want to say a little more about Isaac Jones, because this Mid-Winter Clinical Course has continued as a post-graduate international affair, and each year they have four or five of the top men in the world come here to lecture and to give demonstrations. It's both clinical and didactic. This had a very little beginning in 1922, and now I have no idea how many are attending. I haven't had anything to do with it since 1947. I believe that was the last year I appeared there. But during Jones's lifetime, he often referred to it because it was very definitely his creation more than the creation of anybody else. [tape off]

For the record, there is a fairly complete biography of Isaac H. Jones, at least on matters affecting his relationship to UCLA and his work with me in problems of hearing. It briefly describes the work he accomplished in the establishment of this Mid-Winter Clinical Course which we have already reviewed, but it also refers to another very important thing in which he participated, namely, the establishment of what is now known as space medicine, but then as aviation medicine.

MINK: Now this item is in memoriam to Mr. Jones?

KNUDSEN: I'm referring to this "In Memoriam," the title of this memorial pamplet for Isaac Hamster Jones, who lived from 1881 to 1956. One of the eulogies at the memorial services was given by one of his colleagues, whose name is not listed here and his initials are simply H.S.M.

MINK: You don't know who that is?

KNUDSEN: I tried to find out and I was not able to. I tried to call Austin Jones, the son of Isaac, but I was not able to find out. But I'm sure of the document. This will be among my collected papers here and will be available for the UCLA Library in time. But respecting his work in space medicine, in 1916 Jones had already become a leader in otological research and had been invited to address the Academy of Medicine in Washington, D. C., on the subject of the ear in aviation. You see, we were just getting deep into World War I at that time. And in the audience he addressed was Col. Theodore Charles Lister, distinguished ophthalmologist, whose name now is memorialized in the Los Angeles County Medical Library (near MacArthur Park), which is known also as Lister Medical Library. Lister and Jones and Eugene Lewis (otologist) were really a team in developing this field of aviation medicine, as it was called in those days. Lister was impressed with Jones, and after the lecture he invited him to join him in work he was doing at this time on devising physical examinations for selecting qualified aviators, which Lister was heading at that time as a colonel in the Air Force. And under Col. Lister, who was then chief surgeon of the aviation section of the Signal Corps, as well as the United States Army, Drs. Jones and Lewis were appointed to organize boards of medical examiners for candidates for commissions in the new flying service.

Jones traveled from coast to coast and made several trips to Europe during the war years. It was during that time that he went over when the ship had to be blackened as a precaution against U-boats. You know, the lights were out. As they safely reached the French harbor, he sent a telegram back to his wife Emily. He intended to say and had written in the telegram, "France, lonely without you." And when it was transmitted to Mrs. Jones it read: "France, lovely without you." Jones used this story later in a book that he wrote entitled Flying Vistas, which was a more popular account of the work that Lister, Jones, and Lewis did during World War I in establishing aviation medicine.

The formal and technical book which he, Lister and Lewis prepared on this subject is entitled Aviation Medicine. It often is referred to today as the fore-runner of space medicine. Later, Isaac wrote a more practical medical book that has the title Equilibrium and Vertigo; this treatise for many years was a standard reference book on the vestibular part of the internal ear and its functions pertaining to equilibrium, vertigo, motion sensing, and all these things that, you know, are related to the semicircular canals and the otoliths (tiny "plumb bobs"), which are attached to the vestibular branch of the VIII Nerve. These functions have a lot to do with maintaining balance in aviation, and, of course, they are very important in selecting air pilots and space explorers. [tape off]

Among the staff that Lister, Jones, and Lewis brought together to do this work in developing aviation medicine, was George E. Shambaugh to whom we have already referred, and with whom I published two papers, you will recall. And it was through Shambaugh that I met Isaac Jones. And the work which Dr. Isaac Jones had done on the vestibular mechanism was really a classic at that time, and is embodied very much in this book which bears the title Equilibrium and Vertigo. Dr. Jones, during our years of joint activity, really carried the vestibular part, and I carried very much the auditory part, and so the papers that we wrote always called attention to both the vestibular and the auditory aspects.

Among other things, these vestibular and auditory tests were very useful in the field of brain surgery. And Carl Rand, who is the father of Robert W. Rand who is at our medical school at the present time, and also a brain surgeon, would often refer patients that he suspected of having a brain tumor of some sort to have this examination, which Jones and I together would make. Jones would make the vestibular tests, and I would make the auditory ones. Very often the findings in these two tests would be of value in advising Carl Rand, the neurosurgeon, just how to operate to get at this tumor. That is, if the vestibular part was destroyed most of all, [as shown] in the tests which we had conducted, Dr. Jones could say, "Ah, this brain tumor must be very close to the vestibular branch of the VIII Nerve. If it was the auditory part that was impaired and not the vestibular part, it's closer to the auditory branch. If both were impaired, as they sometimes were, seriously, and the hearing loss and the vestibular loss pointed to a tumor, it meant that the tumor spread to both branches of the VIII Nerve.

And so part of the work which Isaac Jones and I did together was of this diagnostic aid to brain surgery, for removal of brain tumors. And one of the criteria that Jones used was referring to this 24 seconds of after-turning nystagmus, which was characteristic of normal vestibular function. [If] the nystagmus was only 12 seconds, most otologists in those days, Isaac Jones included, said, "You have 50 percent function of the vestibular mechanism, as far as that test is concerned." There were other vestibular tests besides the Barany turning chair test. For example, there's a caloric test in which they squirt either warm water or cold water into the ear canal, and that sets up a thermal syphon flow of liquid through the semi-circular canals, and this brings on vertigo and dizziness and even vomiting if it's carried to extreme. And you may know that fraternities sometimes use this trick for initiating their [laughter] new members. They'll squirt cold water in the ear until the boy vomits, and then they quickly squirt warm water in it and it cures him instantly. You stop the flow, this thermal syphon flow, you see, in the semicircular canal. When the warm water hits against the ear drum that's enough to change the flow of liquid through the semicircular canals. This vestibular effect is much the same thing, you see, as dizziness from turning, or seasickness from up and down motion.

So I learned a great deal about vestibular function and impairment at that time, so much so that I have on occasion diagnosed my own vestibular disturbances. I have had two severe vestibular reactions following major surgery, the symptoms were similar to those characteristic of the Ménière syndrome. You have probably heard of that; it's a vestibular disturbance that hits you very suddenly. That is, you're feeling perfectly normal and all at once the room you're sitting in or the surroundings begin whirling around, or you're whirling with respect to them, and you break out into sweat, into pallor, into nausea, and sometimes even vomiting. It can go that far from a Ménière syndrome. I've had these symptoms occur four or five days after each of two major surgeries. In general, the medical profession now attributes these after surgery symptoms to antibiotics. At least, the symptoms are very much like those that occur from a ruptured, tiny blood vessel that's in the vestibular part of the ear. And the rupturing of a blood vessel near the end organs of the vestibular nerve, to be sure, causes an irritation there, and any irritation with these nerve endings will set off these vestibular disturbances. And so this is part of the teamwork which Jones and I worked on. I haven't discussed the vestibular part before, and that's one thing which he very definitely helped to establish. [tape off]

It is really ironical that Isaac Jones died of cancer. As early of the late 1930s, several of his colleagues, including myself, noted that a common wart on his left cheek began growing. And we warned him that it should be removed. Like so many of us, including our distinguished physicians and surgeons, he postponed surgical removal; but at last when it was too late it was "removed." But apparently it had begun its malignant spreading. He then went to the Kettering Institute in New York for radiation treatment. According to Isaac's report to me, he was given a higher dosage of radiation treatment than had ever been given anyone at that time. He said, "Give me the full works;

give me the full treatment." That was his feeling at that time, because he then knew that it was malignant, and although this radiation treatment left an ugly scar on his cheek it did not metastasize in the usual five-year period. He was told, "Well, if this doesn't metastasize in five years, you can assume that it is cured." It did not. But in seven or eight years, approximately that time, it did metastasize into his spine. And they operated again at that time, and this was probably about 1950, or thereabouts, and he survived until 1956. The last two or three years he just slowly wasted away from this spreading malignancy. The last time I visited him, which was probably a month before his death, I could hardly hear him speak.

I'd just like to get this one thing in. Jones in his prime and even mature years would want no tears or sadness on the occasion of his funeral. Austin Jones, the son of Isaac, asked me to prepare the eulogy for his father. This eulogy, with others, is printed in this little reference I have already given you, In Memoriam to Isaac H. Jones. But there are one or two comments I thought should get into the record. Isaac Jones certainly wanted no tears or sadness on the occasion of his funeral. He would remind us that solace has an old meaning: to make cheerful, to entertain, to amuse; and this was really the spirit he wanted at his funeral. I think Dr. Jones was indeed a master at giving this kind of solace.

I remember that he gave that kind of solace to me when Mrs. Knudsen and I lost our first child when she was not quite five years old. He was at our side, conferring with the pediatrician, and comforting us, during all this trial, and later he gave me his kind of solace. He said, "It is what we can do to cheer you up. That's what solace means." And so Jones did a great deal to really save me from morbidity at this time because I think I have already mentioned that the loss of our first child was a deep and lasting sorrow to me and especially my wife, and also was a great blow to my faith, because we had exercised all the usual prayers and ministrations that devoted Mormons resort to in the case of serious illness. Dr. Jones would have us meet death as Socrates did, with courage and a cheerful countenance. And I recall that when I made my last visit to him, he spoke to me in a few words that were, as I said at this time, only a decibel above a mere whisper. But they were in story form. I regret that I have forgotten that particular story, but he was still barely able to whisper, and yet it was not about his illness. It was not about anything sad. It was an appropriate funny story about his illness. And so he met death in this fashion. I think this is an appropriate way to conclude my remarks about my friend and colleague Isaac Jones.

SECTION XVII

MINK: We're now going to begin to talk about absorption of sound in gases and in air, and Dr. Delsasso is joining us for this part of the interview.

KNUDSEN: I think it's very appropriate that Dr. Delsasso be here for this discussion, because this is typical of much of the research work that has been going on here at UCLA. Delsasso and Knudsen together really worked out many of these problems, and I'm sure I discussed with Leo Delsasso the things I am going to report now on the absorption of sound in air and other gases. It really began with the calibration of the reverberation room and the new laboratory here at the campus when we moved here in 1929. The first thing to do was to calibrate the reverberation room, because the room was designed primarily for measuring the sound absorptive characteristics of materials that were used for the control of reverberation and the reduction of noise in the acoustical treatment of rooms. This laboratory had been very carefully designed. Dr. Delsasso had a great deal to do with the design of the laboratory. It was, I think, one of the best acoustical laboratories in the country at that time. It was very well isolated against both airborne and earth-borne vibrations with--

oh, it consisted of a room inside of a room, and the inner room (with its heavy concrete slab floor) rested on a deep layer of sand and then on cork on top of the sand; so that the actual inside room was very well isolated against transient and steadystate vibrations from the outside. It had heavy concrete walls, ten or twelve inches thick, inside of each other so that the total sound insulation was comparable to that which we have in the modern new reverberation room in this building--sufficient to silence all outside noise and to exclude all disturbing external vibrations.

MINK: Very shortly after the move to the campus came the earthquake. Did it have any effect, Dr. Delsasso, on the new laboratory.

DELSASSO: Oh, not on this particular room and equipment we're talking about now. There were a few things shaken off the outside of the building.

MINK: But it didn't hurt the reverberation room.

DELSASSO: No, no. This was on the cork and sand. It certainly didn't hurt the inside room at all, and we didn't notice any cracks on the outside that were serious.

KNUDSEN: The calibration of the reverberation room at that time meant that you determine just how long a time is required for a pure tone of a standard sound level, and that standard level is 60 decibels above the threshold, to die away to inaudibility. Have we defined the decibel?

MINK: Yes, we have.

KNUDSEN: All right. [laughter] So the time required for the sound to die away 60 decibels, which means the sound dies away to one millionth of its initial intensity--that's what the 60 decibel decay means in this particular instance--was a measure of the reverberation time. And this measurement is done for tones of a low pitch, of medium pitch, and high pitch--at octave intervals, at least, from 125 to 4000 cps. This work, as I say, began in 1929, and as I have remarked on many occasions, the calibration of a reverberation room in the new Physics Building at that time would never have been associated in the minds of American physicists, or other physicists, to have anything to do with molecular physics. This subject is quite adequately treated in a publication "From Architectural Acoustics to Molecular Physics," that appeared in the Journal of Sound. It was simply called Sound at that time. It's in volume 1, number 4, pages 27 to 35, and number 5, pages 17 to 22, in July-August and September-October, 1962. The article is a description of this work which we began in 1929 and continued intensively until 1939, and occasionally since then, especially by Dr. Delsasso and his graduate students.

In the first paragraph I said,

> I doubt that any physicist at that time would have anticipated that this or any other research in architectural acoustics would lead to studies of the absorption of sound in gases. But it did. Such is the course of research. No one knows where its paths may lead. The fascination, excitement, and satisfaction of pursuing these paths into the unknown or the unsuspected are the real incentives and rewards that keep the scientist ticking. The example under review also led to new problems and often to their solutions in molecular collisions, relaxation phenomena, chemical kinetics and the quantum theory. It even invoked one of Einstein's theoretical papers on the propagation of sound in a partially dissociating gas.

And we shall say more about these things later in this discussion, but for the time being we will refer really to the experiments we conducted in the reverberation room. And I think I should discuss these experiments, probably in a form that's pretty much given here in this article.

The weather, you know, is blamed for many, many things. For nearly three years it was the <u>bête noire</u> in our life here, the life that Leo and Louis Delsasso and I were doing together in trying to calibrate this reverberation room.

MINK: That was a very rainy period, wasn't it, that period of the early 1930s.

DELSASSO: We had some rainy years, but 1938 and 1939 were very rainy, and I think some of the earlier ones were, too.

KNUDSEN: Well, the reverberation time at a given frequency was supposed to be dependent only on the dimensions of the room and the absorption coefficients of the boundaries of the room. The weather is supposed to have utterly nothing to do with it. The absorption coefficient, at least for a material like concrete, was assumed to be dependent only on the frequency. But the reverberation times at frequencies above about 1,000 cycles per second were found to depend upon the weather and particularly upon the humidity of the air. Thus, a test tone of 4,000 cycles per second in the room gave a reverberation time of the order of 4 or 5 seconds, when the room was filled with moist air from the ocean. But the reverberation time was only on the order of 3 seconds or so when the room was filled with air from the Santa Ana winds, desert air. And so, very definitely, the weather was calling the tune of the way these high-pitched sounds would die away. And this was a big difference--the difference between 3 seconds and 5 seconds, you see. You could even hear that by ear. You go into the room and sound this 4,000 cycle tone that corresponds to about the highest note on a piano--"ping"--and it dies away in 3 seconds on one day when the air is dry, and the same "ping" dies away in 5 seconds when the humid air from the ocean filled the room.

Clearly, the weather was affecting the absorption of sound by its influence either on the boundaries of the room or on the air in the room or on both, and to find out which was our major problem. And naturally we suspected that it had to react upon the boundaries of the room, because all of the classical physicists who had worked on this subject, such men as Lord Rayleigh [John Strutt] and [Gustave-Robert] Kirchhoff and [Sir George] Stokes and other famous men in classical physics of the nineteenth century had all indicated that the absorption sound in air is determined principally by two things: the viscosity of the air and its heat conductivity. The viscosity, you know, is the frictional forces between layers of air, and the heat conductivity, insofar as sound is concerned, is radiation from the compression which is warm into the rarefaction which is relatively cool. And these two factors had been assessed very carefully (mostly theoretically) by these classical physicists. The result was that temperature and humidity, for such changes as occur in the atmosphere, have so little to do at these frequencies that you couldn't detect it experimentally. At much higher frequencies, the heat conductivity and the viscosity both do have a significant result, and as a matter of fact the classical Stokes-Kirchhoff attenuation or absorption increases with the square of the frequency. But at frequencies below 4,000 cycles, there should be no measurable absorption. At 4,000 cycles the formulas that were extant at that time said the attenuation of audible sound in the air is negligible, and so knowledgeable [people] said you can just as well forget that. Normal changes in the humidity and temperature of the air cannot account for such large changes in reverberation time as we were observing. And we got all sorts of advice from physicists here and elsewhere that the humidity was probably affecting the absorption of the wall. So we put successive coats of varnish and enamel on the walls of the room, but this didn't change things at all.

About that time (September 1930) I attended my first international congress--it was the Twelfth International Congress of Architects in Budapest, at which I was invited to present a paper on acoustics. One of the acousticians who was there from Zürich, a Dr. Oswald, said, "Well, you know, we had this same trouble in our reverberation room at the University at Zürich and we lined it with bathroom tile, and that corrected it." Well, we considered lining the interior of our room, as Delsasso knows, with bathroom tile, but the estimate of the cost was $2,000.

MINK: What did you do, go out to Gladding-McBean?

KNUDSEN: Well, we got estimates that were pretty expensive, and before we asked the university administration for $2,000, which was a lot of money in those days, we thought we'd be very, very sure of ourselves. Well, it was about at that stage that the thought occurred to me, "By Jove, we've got two rooms here of different size but with the same kind of concrete boundaries." We had this reverberation room which was 19' x 20' x 16' high, and we had another room that was built the same way, a room inside a room, the same kind of concrete, ten-inch thick walls, and we said, "We're going to perform a two-room experiment. This is the critical experiment to tell us whether the absorption is at the boundaries of the room or in the air itself." You see, in the small room which is only 8' x 8' x 9.5', sound which was traveling about 1,125 feet per second makes many more encounters with the boundaries of the small room in a given time than it does in the large room, so that if the decay rate is dependent only upon the absorption of the boundaries of the room, then the decay rate ought to be correspondingly faster in the small room, but just by the amount of the change in the dimensions of the room. How many reflections per second do you get against the boundaries of the rooms in the two rooms? And this we can calculate very neatly in terms of the dimensions of the room and the velocity of sound. But if, on the other hand, part of the absorption is in the air, then, inasmuch as you've got a two-room experiment, you can set up two independent equations. One is for the decay rate of sound in the big room, in which you assume two factors are contributing to decay: the attenuation in the air and the absorption at the boundaries. For the small room, you set up a similar equation based on these same two factors: the attentuation in the air and the absorption at the boundaries. And so we assigned what we called a coefficient m to the attenuation in the air, and this letter m is used to this day to represent the attenuation of sound in air. We first proposed it when we began this work.

MINK: Oh, then I suppose you were praying for fog and then for Santa Ana winds?

KNUDSEN: Well, we depended on Santa Ana for dry air, but we increased the humidity by evaporation. I'll come to this in just a moment. Yes. We actually had the Santa Ana [winds] and fog at that time as well.

But we then had two unknowns: alpha (α) which we used for the absorption coefficients of the boundaries of the room, and m for the attenuation coefficients in the air. Each of the two equations, one for each room, has two terms in it. One is an exponential term involving m and the velocity of sound, and the other term involving α and the dimensions of the large room. The other equation is a similar one but with the different dimensions for the small room. If you measure the rate of decay of sound in each of these two rooms under the same conditions of temperature and humidity, then you can solve separately for the attenuation in the air, if there is any, and for the absorption in the boundaries, which are the same for both rooms.

MINK: You have two equations.

KNUDSEN: You have two independent equations and therefore can solve for these two unknowns, m and α. So this was the critical experiment. We knew, well, "By Jove, we've got it now; we've got it right by the tail this way, and it can't get away." And indeed it did not. It was this experiment then that solved this dilemma that had been troubling us for three years in this process of finding out why the weather called the tune of the way sound dies away in the reverberation room. And it was a very significant finding.

When we made our separate calculations alpha

and for m as a function of frequency, we found the absorption coefficients of the boundaries almost entirely independent of frequency. It had been assumed up to that time that because the reverberation time is much shorter at high frequencies than it is at low frequencies, that concrete is much more absorptive at high frequencies than it is at low frequencies. But we got about the same value--that is, only about 1.7 percent of the sound energy incident at the concrete boundary walls is absorbed; the rest is reflected. That is, this means that 98.3 percent of the sound energy is reflected.

But the absorption coefficient m for the medium, the attenuation constant in the air, behaved in a most extraordinary manner. For perfectly dry air there was almost no absorption in the air at all. Then as the humidity was increased, the attentuation increased very, very steeply for high frequencies. We were working mostly at 3,000, 4,000, 6,000, 8,000, 10,000 cycles per second. That's the range of frequencies at which we were working. At relative humidities at normal room temperature of the order of 16 to 20 percent, the attenuation turned out to be as much as 100 times as much as had been predicted by classical theory.

One hundred times as much as the Stokes-Kirchhoff-Rayleigh formulas would indicate you should have. Then, as the relative humidity increased (above about 16 percent at 3,000 cycles and 20 percent at 10,000 cycles) the attenuation decreased gradually, steeply at first, then gradually it tapered off to a relatively low value again at high humidities. Dr. Delsasso has done a lot of work on the attenuation of sound in the open air, high in mountains and at sea level, which he will describe in a moment, indicating that the results he obtained in the open air were close to the results we obtained in the two rooms. Maybe this would be a good time for Leo to describe the kind of work he did.

MINK: May I ask one question?

KNUDSEN: Yes.

MINK: For example, at 50 percent humidity, what was the air absorption?

KNUDSEN: It's come down very much from the maximum at about 16 to 20 percent. It goes up very steeply as the humidity increases from perfectly dry air. Now you ask how we control the humidity and the temperature. The humidity was controlled by passing air through water.

MINK: Oh, you set up apparatus.

KNUDSEN: We had apparatus which bubbled the air through water, and that increased the humidity in the room. We of course measured the humidity by means of the wet bulb-dry bulb temperatures, that's the usual method, but we also made humidity measurements by weighing of the air with different amounts of water vapor in it. These two methods were used for determining just how much humidity we had in our rooms, and we measured the temperature by the usual way. But we later had a means of heating the air so that we could determine the affect of temperature on air absorption. But I think it's more appropriate now to discuss how these results were confirmed by measurements in the open. You might discuss, first of all, our trip to the desert when we did listening.

DELSASSO: Shouldn't we just finish here by saying that this was done in more refined fashion by repeating the experiments in two steel cubical chambers.

KNUDSEN: Maybe I should describe that research. In order to avoid the necessity of changing the temperature and humidity in these huge rooms, one 19' x 20' x 16' -- that's a lot of air to change the temperature and humidity--we built two steel chambers. That is, they were made of steel plate a quarter-inch thick, thoroughly braced with L-beams to make sure that they were similar and that we had the same kind of bracing for the small chamber, a two-foot cube, as we did for the large chamber, a six-foot cube. These smaller chambers didn't require nearly so much time to change the temperature and humidity; furthermore, later, the need arose to work with nitrogen gas and oxygen gas separately. But I think at this stage it might be well to pursue the value of absorption in the air, then we'll come back to how we made these measurements in the two-foot and the six-foot cube, because after we had discovered this large and peculiar effect of humidity, we especially wanted to repeat, refine and extend our measurements of the effect of humidity and temperature on the absorption of sound in air, and this was done in these two steel plate chambers.

DELSASSO: Well, the early experiments were made out on the desert because it contained a flat plain. You can get a dry lake that would present very few bumps and hills. I think also that the challenge of going out and having fun on the desert was partly behind this because we took a group of students with us and we did our own cooking and had good fun during the outing.

KNUDSEN: Incidentally, we had the city councilman from this district, J. Win Austin, who was then a city councilman.

MINK: This was in the early 1930s?

KNUDSEN: Mid 1930s.

DELSASSO: Mid 1930s, yes.

MINK: Where did you go?

DELSASSO: Rosamond Dry Lake. This is a good place to perform such experiments, and the councilman and the students were placed in circles around a central source of sound that could be elevated from the ground; the source consisted of a long tube with a loudspeaker at its lower end so that it sent out essentially spherical waves from the upper end of the tube. And then the attenuation measurements were made by ear and having our group of listeners (there were 10 or 12) distributed around the source. First, for a fairly faint tone of high pitch and the listeners would move out radially until each reached his threshold of audibility, and we recorded their distances from the source. Those with the most sensitive hearing had moved out about 50 feet. Then we increased the sound level 10 dB, and though the listeners moved out until each observer ceased to hear the tone. This process was repeated for further increases in the sound level of the source--and this was done for at least three frequencies, for example at 3,000, 6,000 and 10,000 cycles per second. And this experiment not only served to make measurements on the absorption of sound in the air, but revealed the directional properties of sound propagation because of wind that Dr. Knudsen, I think, discussed some time back.

MINK: Did you have to wait for a perfectly calm day?

DELSASSO: Yes, for the experiments in which we were trying to determine what the air did to sound without the effect of the wind.

MINK: So to do this did you take a machine to measure the wind velocity so that you could tell when you were at just exactly zero velocity?

DELSASSO: Yes, usually a lighter-than-air balloon with strips of thin paper attached to the balloon's string could detect any slight wind motion. And for measuring the actual wind velocities when we were concerned with the effect of the wind, we used an anemometer. But these early experiments were made entirely by listening, and I don't know if you want me to continue on the other measurements--objective ones.

KNUDSEN: Yes, well, but first let me explain further that all listeners didn't go out from the source the same distance for the tone to become just barely audible because there were slight differences in their hearing. But we had accurately measured their hearing acuity in the laboratory before they performed the listening tests, so we knew just what their threshold of audibility was for the various tones we used in the experiment. So, by observing how much we had to increase the sound level as the observers move out in successive steps until they could just barely hear the tone. For the most feeble tone, the listeners were out about 50 feet and for the loudest tone, oh, about 1,000 feet or more. If there was no attenuation in the air it would be necessary to increase the sound level of the tone 6 dB for each doubling of the distance, but because of attenuation we had to increase the sound level considerably more than 6 dB. From the observed increase we could calculate the attenuation. So by this simple experiment we were actually determining (approximately) the attenuation for the existing temperature and humidity. When we compared the values of attenuation that we got in the desert air with the values we had obtained in the laboratory, we were pleased to find that there was good agreement. This was a cruder method of measuring attenuation than the one we used in the laboratory, but we at least knew that we had the same kind of dependence upon the humidity and temperature of this desert air, which was in a quiet location suitable for this kind of experiment. This was our first confirmation of the laboratory results. But later Dr. Delsasso set up other experiments, with Dr. Leonard helping on part of the investigation. I think you might describe those experiments.

MINK: I have a question.

KNUDSEN: Yes.

MINK: In getting the people to tell when they did or didn't hear it, it wasn't a group. It was one person at a time, right?

DELSASSO: Well, they all put their hand up if they heard the tone and put it down if they didn't hear it. This process was followed until all listeners had moved out or in until each had determined at which distance he could just barely hear the tone. They were not all at the same distance when they reached their threshold. You see, they moved out to the distance required to match their threshold.

MINK: I see, because you had the measurements on their hearing acuity, you would expect them to differ.

KNUDSEN: Yes, but you could record the distances for each of the observers at his threshold. There were about ten or twelve listeners I think that we had in the group. We knew the threshold at different frequencies, for each observer, and this helped us evaluate the results of our field measurements.

DELSASSO: Well, I think even in these early experiments the indications were that the values, although there were fluctuations, were at least as high and most of the time a little higher than the measurements made by the two-room experiments in the laboratory.

And this is borne out by more refined experiments that were done later.

MINK: How do you account for that?

DELSASSO: Well, the turbulence of the air and the slight wind shear and various things that interfere.

KNUDSEN: Also, back scattering.

DELSASSO: Backward scattering from inhomogeneities in the air, temperature essentially, bodies of warm air floating around in the cooler air--all of these things came in so that the somewhat more controlled and objective experiments were made at a variety of places. A typical one was down here at the beach in Venice, taking advantage of the oil well towers that provided a place to put microphones and also to put a small cannon fired with an electric spark. We had a lot of trouble with people who had a duck pond down there. We were scaring their ducks away and the neighbors didn't like it very much, but we managed to get many good readings over a period of about a year and a half.

MINK: And this was at the same time when you were carrying out the experiments in both the dry lake and the laboratory?

DELSASSO: No. It was after that. It was a little before and after the war years. But the scheme there was simply one of firing one of these cannons, recording the sound that passed a microphone relatively close, and then at two or three hundred feet from the cannon, and then at a considerable distance away, and by recording the sound pulses automatically on film recorders, you could later measure the amplitudes of the sound pulse--first the single pulse near the cannon, and then at the two more distant places. By proper correction for the inverse square law that would hold if there were no attenuation, the observed amplitudes between the two removed microphones would enable you to determine rather precisely what the air attenuation was. In these experiments, all the attenuation values turned out to be in excess of those measured in the laboratory. For absolutely quiet conditions, they'd come down very close to the laboratory values, but you'd never have the air as quiet (non-turbulent) as you had it in the laboratory. And so in some cases the attenuation coefficient m was two or three times the value that you'd measure in the laboratory--probably because of the turbulence, the wind, temperature refraction, and other changing air conditions.

KNUDSEN: We might pursue this a little further. These results, you see, that indicated for rather dry air, that's usually about the desert humidity, namely, 16 to 20 percent, in that range, for frequencies at the upper audible range, 3,000 to 10,000 cycles, the range that we had investigated, the humidity of the air then had quite a bearing upon calculating the reverberation time of all rooms. So the reverberation formula that had been used up to that time was modified to include another factor--the absorption of sound in the air.

This adds a term in the reverberation formula which had not been included before involving this m coefficient we speak about, the attenuation constant in the air. The term itself as it is used in the reverberation formula involves a factor 4 and the volume of the room, and so the $4mV$ now is a standard part of the reverberation formula. The $4mV$ corresponds to the total amount of absorption in the air. The various surfaces of the room times their absorption coefficients all added together, would be the total absorption by the boundaries of the room. And at frequencies above about 4,000 cycles, the absorption in the air is often much greater than the absorption by the boundaries of the room. And this places, therefore, a limitation on how much reverberation you can get in a room at high frequencies. At 10,000 cycles per second, even in our reverberation rooms, we have only a little over 1 second of reverberation. The sound dies away that fast. So sound absorption in air is very significant.

Well, these values m have quite a number of practical applications, as you might suspect. The distance at which you can hear speech in the desert, for example, if you happen to be at these desert humidities we're talking about now, sound is muffled very much, whereas, in the very cold arctic, you're down practically to zero humidity. There's almost no water vapor in the air at all there, and as I indicated earlier, that's when you have the least absorption. Furthermore, as we will show a little later, the temperature is also significant, and therefore the explanation of the great distances at which sounds can be heard in the artic is not alone a matter of refraction, which had been supposed heretofore. You not only have the benefit of refraction which bends the sound rays down so that you can hear barking dogs and crowing roosters maybe 10 miles away; but you would not hear them except that this air attenuation is so very low for the perfectly dry and cold air you have in the arctic.

Well, this attenuation effect is so pronounced that I can readily detect the effect on the quality of music in the Hollywood Bowl when we have the dry Santa Ana wind. If you're sitting 550 feet from the stage, where you would be if you are sitting on the center line at the extreme rear, you have an attenuation loss of approximately 20 decibels at high frequencies when the Santa Ana prevails, as compared with the relatively small loss that you have when the air is humid. So you see this is not a small effect. A 20 decibel loss means that only 0.01 of the energy gets there when the air is from Santa Ana. [laughter] We shouldn't blame Santa Ana, I know, for all this, but I think we know what we mean by speaking of this desert air.

MINK: I was very interested Sunday night to watch the Bell Telephone Hour and see the Garden of the Gods. Now in such a place as that I suppose the acoustics would be much better all of the time. Is that correct?

DELSASSO: When it's quiet, it's wonderful. When the wind is blowing gustily, then it's very difficult.

KNUDSEN: Well, normally for the acoustics of rooms it's advantageous, of course, to have air conditioning, especially if you are living in Arizona or other places where there's often desert air. And therefore we normally specify that the relative humidity of an auditorium, especially if it's an auditorium for good concert hall acoustics, that they control the relative humidity of the air in the range of 50 to 60 percent. That happens to be a good value for comfort, so it's fortunate that the air doesn't absorb very much more at 50 to 60 percent than it does at much higher humidities. But if you allow the humidity to go down toward 20 percent, then you would really attenuate these high frequency sounds so much that it does affect the quality of music. Well, these are just a few examples. When you are concerned about signaling in the air, then, of course, you need to know the temperature and humidity of the air. This gives a good index of how far you might expect to hear sound signals of a given frequency, because the air attenuation is so highly dependent upon humidity and temperature. Another good example is the noise from aircraft, especially jet planes. When such planes are near you, the whining sound (about 3,000 cycles) is predominant, and such high-pitched noise is very annoying. As the plane recedes, the whine gradually fades out because high frequencies are attenuated so much.

Maybe this is an appropriate time to go back to the two-chamber experiments. The problem which next presented itself to us was what causes this high attenuation in the air? Is it attributable to the oxygen or the nitrogen, to the water vapor acting on the oxygen or nitrogen, or the water vapor or some other interaction involving water vapor. The two smaller chambers, the six-foot cube and the two-foot cube, after we had them calibrated, from then on we could do the experiments only in the two-foot chamber, because we knew what the absorption of its boundaries were and the rest of the absorption was due to the medium. So this made a convenient chamber, you see, to work in. You could readily change the temperature and humidity.

When we first used this chamber for further work, we wanted to know whether it was oxygen or nitrogen or both that was significant. When we filled it with nitrogen, and then added water vapor, nothing happened. When we filled it with oxygen and then added water vapor, we observed that the absorption at the peak was five times as great as it was for air, and that perfectly dry oxygen was no more absorptive than air. Now, oxygen contains five times as many oxygen molecules as air does, and therefore this was pretty convincing evidence that the absorption resulted from the interaction of water vapor on oxygen. We further found that by changing the temperature of the air the absorption increased with increasing temperature. We changed the temperature down to 0 degrees Centigrade--this would be freezing temperature--and then down I think to -4 degrees Centigrade, which was the lowest temperature at which we could conveniently keep the temperature constant. We were able to chill and maintain the whole chamber at this temperature. The absorption practically disappeared. There was almost no measurable absorption in the cold air. I mentioned that the arctic was a very propitious place for long-distance transmission of sound. When we heated the air to 55 degrees centigrade, the absorption was double what it was at 20 degrees centigrade. So both temperature and humidity were having a marked effect upon air attenuation, and neither temperature nor humidity, in the range at which we were working, should have had a significant or a measurable effect at the high frequencies we were working at.

But at this stage of our work we fortunately had a distinguished visitor here, a German from the University of Marburg by the name of Hans Kneser. He spent the year 1932-1933 as a Rockefeller fellow at Berkeley and Los Angeles. It was just at the time Hitler was coming to power that he came to UCLA. We could go on to that, but I won't now because we often talk politics as well as physics here. But Dr. Kneser had worked on the attenuation of sound in carbon dioxide gas. And I said earlier today that Einstein had investigated the possibility of an attenuation of sound in a partially dissociating gas, a nitrous oxide gas. And carbon dioxide had been known to have excess absorption, and it was attributed to an interaction between the carbon dioxide molecules. The collisions between the molecules excited the atoms within the molecules into vibration, and the process extracted energy from the sound waves. And Kneser suspected, when he saw our results, that there was five times as much absorption (at the frequency of maximum absorption) for oxygen as for air, that this must be associated some way with the internal heat capacity of the oxygen gas which is attributed to the vibrational energy of the gas. Possibly the water vapor molecules, in colliding with the oxygen molecules, he suspected, can excite these vibrations of the oxygen molecule. The oxygen molecule is a diatomic molecule, and diatomic molecules are often referred to as dumbbell molecules. It's a dumbbell shape and it vibrates along its axis connecting the two atoms of oxygen; it's a vibration something like that.

Kneser was familiar with these phenomena, and spectroscopy furnished a means of calculating how much vibrational energy there is in a mole of oxygen gas. Starting with the spectroscopic and molecular absorption information he had, he was able to predict that the oxygen molecules, when colliding with water vapor molecules, would change translational energy into vibrational energy and back again, and that this process was responsible for the excessive and peculiar attenuation that we were getting for air at different humidities and temperatures. And this process involves what we call a relaxation time which is a measure, really, of how many collisions between--in the case we were working with--oxygen and water vapor molecules are required before one is energetic enough to excite the oxygen molecule into vibration.

And so as a sound wave passes through a gas, when a compression comes along, the temperature is increased, with the result that the speed of the molecules is correspondingly increased, and there are more energetic collisions between water vapor and oxygen molecules, so more oxygen molecules are excited into vibration. In the rarefaction phase of the sound wave, the reverse is true. During the compression, energy is extracted from the sound wave to go into this vibrational energy of the molecule. During the rarefaction phase some of the vibrational energy is returned into translational energy. And so it is this interchange of energy between the internal or vibrational energy and the external transport motion of the oxygen molecules that apparently gives rise to this attenuation, and making use of his special theoretical physics and unusual insight Kneser was able to predict the kind of curve that we obtained from experiments. The Kneser theory doesn't agree completely at high humidities, but it agrees very well at low humidities, and the general shape of the experimental curves are approximately the same as the theoretical curves that Kneser calculated at that time, and the magnitudes of the absorption we obtained in the laboratory for both air and oxygen were elegantly confirmed with Kneser's theory.

MINK: He was a visiting professor for one year?

KNUDSEN: He was a visiting scholar. He was the equivalent of an instructor at that time.

DELSASSO: He was a relatively young person. He was an instructor, I believe, at the time, and he had a half a year at Berkeley and then came down here for a half year.

MINK: Who made the arrangements for him to come over here?

KNUDSEN: He was a Rockefeller Fellow.

DELSASSO: And he circulated . . .

KNUDSEN: He initially intended to stay at Berkeley. He was there the first semester, and when he learned of the work we were doing here on absorption of sound in gases, he came to work with us at UCLA. He became very much interested in our work. When he writes to us and when we meet at international meetings he often says this, the half year he spent with us at the University of California was one of tbe most pleasant periods of his life.

MINK: Was he in favor of Hitler?

KNUDSEN: He was in favor of Hitler at that time, as a former member of the Hitler Youth movement, but he wasn't in favor of him very much longer after Hitler came to power.

MINK: You didn't know that though when he came, did you?

KNUDSEN: Oh, yes. We talked about it, and he gave reasons why he joined the Hitler Youth movement. This was a time, you know, of great economic difficulty in Germany. The youth were taught that the reason the graduates of medical schools who were Jews got positions is because the Jews controlled these things, and Kneser probably believed this view at that time. I think he was cured within a matter of a few months after he left us in the summer of 1933. He returned to Germany (at the University of Marburg). He told our Dr. Joseph Ellis, with whom he also worked at UCLA he had to shine the boots of [laughter] his superior officer, a Nazi. And Kneser soon became opposed to Nazism. I don't know how, but he probably had to give lip service for a time. But in communications we had with him when Professor Ellis, who was in our department of physics, visited him over there [were to the effect that he was unhappy with Nazism]. Kneser worked with us on the problem of absorption of sound in gases, but he also worked with Professor Ellis on a problem in spectroscopy at that time, and so we got word about Nazi Germany whenever any of our colleagues visited Kneser over there. We learned that he was fed up with Nazism within a matter of months after his return. And you see this was the first year of Hitler's power in 1933.

MINK: Yes. It didn't take him long.

KNUDSEN: Well, while Kneser was here we thought it would be interesting to investigate other gases, so we worked together furiously for two or three months while he was here, and we accomplished a great deal during that short period. It was certainly the time of greatest excitement in my entire career at the University of California, because we were right on the threshold of learning new things about sound absorption in gases and molecular physics in that short interval. His coming here was a great boon to the work we were doing at that time, and we probably would not have found the explanation with our own resources; but he had the special theoretical background and experience that was necessary to solve this problem as it developed at that time.

We investigated the effect of other impurities besides water vapor and found that hydrogen and all of the alcohols had similar effects, and the addition of any of these impurities and hydrogen and some of the alcohols were much more effective than even water vapor--that is, small traces of alcohol would shift these absorption peaks very much to higher frequencies. And this has significance for molecular physics. This is where molecular physics begins to come into this picture, because the frequency at which the absorption is the maximum is significant, because the reciprocal of that frequency gives a measure of the relaxation time, and this is important in the entire field of chemical kinetics. And so molecular collisions, kinetics, quantum theory, and now even some of these relaxation problems, are of significance in solid state physics. Work is going on in our own laboratory here now where they have frequencies as high as thirty thousand, million--is that about correct, Dr. Delsasso?

DELSASSO: Well, they're way above the kilomegacycle now. [tape off]

KNUDSEN: These experiments in different gases were extended to quite a range of other gases--helium, heavy water, as compared with standard water, and with carbon dioxide with all sorts of impurities, water and hydrogen and quite a range of alcohols again. These are all effective in changing the absorption in carbon dioxide, shifting it to higher frequencies. A comparison of carbon dioxide and carbon disulphide and

H_2S and many other gases has been carried on by many other students who worked for their PhD degree under Delsasso. And while I was in the graduate division, I also worked on some of these other gases.

DELSASSO: This started really a chain of significant experiments where the chemistry of the things go on very rapidly, in liquids and solids as well as in gases.

KNUDSEN: Yes. In respect of liquids, Dr. Robert W. Leonard of our laboratory here and one of his students, O. B. Wilson, investigated the attenuation of sound in sea water and in various impurities. I think they were the first to definitely establish that magnesium sulphate has a quite similar effect on the water molecule that water vapor molecules had on oxygen molecules in our experiments in air and in oxygen and in nitrogen and so on. And so relaxation phenomena occur also in liquids, and now a group working in solid state physics here--Dr. Hans Bommel and associates are working with frequencies that are as high as 50,000 megacycles per second. That's the range I was checking with. It was 25,000 megacylcles when I wrote this paper in 1962. Bommel said now they work as high as 50,000 megacycles, and they are investigating relaxation phenomena there which have quite a bearing on reestablishment of equilibrium between the matrices, say, within the molecule. The relaxation times that we worked with are relatively long and slow. But in the Bommel research they go up to these extremely rapid phenomena in which the acoustical wave lengths are even shorter than the wave lengths of light, and they are using such high frequency sounds to bring about these transitions between different states of energy. And so this has become a powerful tool for investigating quite a number of problems in molecular, atomic, and even in nuclear physics.

MINK: I was going to say that during the time when you were working with air and the gentleman from Germany . . .

KNUDSEN: Kneser.

MINK: Kneser came, it must have been sort of a hectic time, because that was just about the time that you were involved, Dr. Knudsen, in the establishment of the graduate division.

KNUDSEN: Well, it was really before.

MINK: A little bit before in 1933.

KNUDSEN: I became dean in 1934.

MINK: Right. But you were serving on the committee before that.

KNUDSEN: Yes, I was.

MINK: The committee that was established to look into this . . . .

KNUDSEN: Yes, I was actually on it.

MINK: But that didn't take up so much of your time.

KNUDSEN: No. No, it didn't. The committee didn't hold many meetings, and we were supposed to have only 125 graduate students that first year. And Dr. Moore really was chairman of the committee that year. I was just a member of the committee. So I attended a meeting maybe once a month or at most twice a month at that time, and so this wasn't a very trying experience, and we had a <u>very rigid</u> schedule here. We did all of our teaching, things of that sort, in the morning, and I simply wouldn't allow myself to go to ordinary meetings in the afternoon.

MINK: If the meeting was in the afternoon, you just didn't go.

KNUDSEN: That's right, except for a few exceptions. The afternoons and evenings were the time for research. And especially during this period that Kneser was here, we met at one o'clock and we would work all afternoon. We'd have supper together someplace or meet after supper and work again at night, and we were really living with this problem night and day for a period of months while he was here. And we made more research headway, I think, during that period than at any other time. This in a sense was sort of my swan song for my research, because [laughter] a year later I was made dean of the graduate division and I think I've already confessed that the research suffered very much from then on. Our stream of research was ultimately reduced to a mere trickle. And Delsasso and I, since our retirement, are trying to make up some of the damage that resulted from our administrative duties, so we work together again now as we did before 1935.

SECTION XVIII

KNUDSEN: These early experiences that I had with the elimination of mechanical vibrations, of solid-borne vibrations that were a disturbance, had been based on an early piece of work I did even over at the Vermont Avenue campus on insulation of vibrations. That's what I called it. And you asked me if I ever applied for a patent. I didn't apply for a patent on this, but I did have a patent attorney make a search on this system of isolation of vibration that I discussed here in which you can change the compliance and/or the mass in such a way as to reduce or even isolate objectional mechanical vibrations. And the patent search indicated that a man by the name of Dr. Carl Soderburg, who was with the Westinghouse Company at that time, but who later became professor of mechanical engineering at MIT, had taken out a patent essentially on the same thing; and so Soderburg beat me to the patent. If the search had indicated that there were no infringements or no patents, applications, or no prior art in the literature or in the patent office, I would have applied for a patent.

Incidentally, this was the same principle that was first used by the Chrysler Motor Company in isolating motor vibrations from the car bodies. The Soderburg patent or principle is one that has been used very much. Chrysler and others began mounting the motors in rubber. This was really the beginning of the isolation of vibration in motor cars, and they've done a good job of that, as you know. Each time as the piston is thrust out there's a reaction that thrusts back, resulting in a vibration that was objectionable in these motor cars of the early days. I often rode in a model T Ford, vintage of 1911, that was the first automobile the Knudsen family had, and I was permitted to drive it, and I remember how much it vibrated and rattled. Well, my investigation of the isolation of vibration was one instance in which I did think of applying for a patent and had a patent search made.

Once again I had a patent search made for control of variable acoustics in rooms. This was based on a scheme I proposed for the control of reverberation in a music rehearsal and recording studio in the Music Building at the University of Washington, at Seattle. I had devised a system of rotating cylinders that were installed in the ceiling. The entire ceiling consisted of these cylinders, having a diameter of 4 feet. One-third of each cylinder--that is, 120 degrees--would project through the solid structure of the ceiling. And the other two one-third sections would be above the ceiling. Each of the three sections would have a distinctive kind of absorptive-reflective material. One of the 120° sections would be highly reflective, for example, a steel plate surface. A second section would be one which is very absorptive for low frequency sounds, and becomes decreasingly absorptive at higher frequencies. The third would be just the reverse of that--that is, very little absorption at low frequencies, but increasingly absorptive at higher frequencies.

The installation in this music room at the University of Washington, comprising the entire ceiling, was made up of three arrays. The front and rear arrays have the axes of the cylinders in the transverse direction, and the middle one at right angles to the front and rear arrays; the three arrays can be motor-rotated. If you think of these three materials as A and B and C, then you can have all of A exposed, you can have all of B exposed or all of C, or you can have any fraction of A and B or of A and C or of C and B. There's no system I can think of that can give you such a flexible control of absorption (and therefore reverberation) as you can get from this particular scheme.

A search for a patent indicated that it probably was not patentable.

MINK: May I ask about this ceiling at the music building in Washington, was it just sufficient to do the ceiling? Could the walls have been done?

KNUDSEN: Well, we had acoustically designed walls. They were diffusive and there was some sound absorptive panels on the walls, but they were fixed. It was only the ceiling for which we had variable control of absorption and reflection, but that was adequate. If you had all the steel surfaces of the cylinders exposed, you'd have a very reflective ceiling, and as much reverberation as is desirable for lively music. And with other combinations of the A, B and C surfaces you could obtain optimal conditions for different types of music. Also the protruding cylinders are good diffusive elements so that the system introduces beneficial diffusion of sound.

MINK: So this way it would be possible to perform and/or record music with different acoustical environments.

KNUDSEN: Yes. This system is described in the Knudsen and Harris book, Acoustical Designing in Architecture. Anyone who wants to look it up would find it under the index there--variable absorption.

I am reminded of another consultation I did in Washington. It had a humorous aspect and yet it was tragic. It was a high school music building. I think it should remain anonymous, except that it was in the state of Washington. I don't know who the architects were, and I wouldn't want to embarrass them too much by referring to what I found there.

Part of what I found is a result of misuse of the word "insulation." Many mineral wool materials--blankets and boards--are referred to as "insulation materials," and they usually are truly good insulators for heat. But they should be referred to as "absorptive materials" for most acoustical purposes. Sound absorptive materials are useful in certain places. In an air space between two solid partitions, they would have some benefit for sound insulation.

But the architect had grossly misused so-called mineral wool "insulation" blankets in the walls of the small practice rooms in the music building I was retained to examine. I was taken to one of the practice rooms by the authority of the school who told me: "We just can't use these practice rooms at all; when they're playing the piano in the next room, it sounds like they're playing in this room. It's just that bad." And then he said, "Now I'll go in the next room and I'll talk to you, and you listen to me in this room." Well, he and I conversed with each other and it sounded almost like we were both in the same room with nothing between us. I asked, "What does the wall consist of?" He said, "Well, the architect said it was a sound insulation blanket, and as such it is rated as having good sound insulation." And I said, "Well, let's look into it somewhat." My first guess was, well, all they've got between the perforated hardboard facing is just a blanket of mineral wool of some sort. And I said, "Have you got a darning needle?" And he said, "Well, we'll find something like a darning needle." They found a piece of wire that simulated a darning needle. The perforated facing you saw was what we often refer to as "peg-board." The perforations were about a quarter of an inch in diameter and were spaced slightly more than one-half inch on centers.

MINK: Sort of like Formica?

KNUDSEN: Something like that, but a perforated, wood fiberboard, as Masonite. They are used a great deal as pegboards, and you hang up displays on them. But they're also used for acoustical facings of absorptive material. Well, I said, "The way this sounds they have used nothing except maybe a one-inch blanket of mineral wool between two facings of pegboard." And sure enough, you could push the piece of wire we had through the entire wall. You'd poke a little at it and it'd go through the mineral wool blanket and then through a hole on the other side. And so it's very much like putting up just an ordinary blanket between you and me here. Well, an ordinary blanket is about the same as a layer of one-inch mineral wool blanket of low density. It was a low-density blanket--not more than one or two pounds per cubic foot, so it was very open and porous, and so the sound "insulated" wall was no more effective than an ordinary bed blanket. I've never had such a shock in my life, because I couldn't believe any architect or builder would perpetrate such an error as that. And yet the partition had been approved by a technical person for the school board. Well, this discovery and demonstration was one of the amusing incidents I have encountered during my fifty years of acoustical consulting. But I am now reminded of another occasion similar but of an even more comedy-tragedy nature where a light-density mineral wool blanket served as the ceilings of two adjacent rooms--one a boys' toilet and the other a girls' [toilet].

I thought it would be of some interest to give a contrast between the relatively simple things we considered in acoustical consulting work and architectural acoustics in my early years here in the twenties and early thirties. There's been a gradual evolution, of course. In the mid-twenties everybody swore by the work in acoustics of Wallace C. Sabine at Harvard. It began at the Fogg Lecture Hall at Harvard University about 1897, and led to the importance of reverberation in controlling the acoustics of rooms, and these Los Angeles high school auditoriums that we have already referred to here were examples of excessive reverberation. We reported on how the reduction of reverberation would improve the intelligibility of speech in these rooms.

There were a few general principles that used to avoid domes and cylindrical shapes and matters that would give rise to echoes. These things had been studied somewhat and we considered them in preliminary designing of these rooms and auditoriums. But there wasn't much else. Then gradually noise became paramount, especially when we found these air conditioning systems that were beginning to find their use into auditoriums. It became necessary to consider the insulation against outside noise as well as against inside noise. The noise problem has become more and more complex as other things in technology have, and I thought it might be worthwhile to contrast these relatively simple things that put the emphasis entirely on reverberation. You'd calculate the reverberation--how much absorption you must add then to reduce the reverberation to what was called the "optimal reverberation time," which for a speech room of small size might be as low as 0.5 or 0.6 of a second. For a large auditorium, it might be a multipurpose auditorium, say, for a high school or a college or a junior college and so on, it might be 1.3 or 1.4 seconds for mid-frequencies. The Sabine formula was used for making these calculations and making the adjustments. And that plus a little attention to avoiding shapes that we knew would give rise to echoes was about the extent of it.

I have a list (in Knudsen and Harris, <u>Acoustical Designing in Auditoriums</u>) of some things which we do in acoustical designing. On page 172, in the chapter entitled "Acoustical Design of Rooms," is listed the number of things we normally have to do, and I think it might be worthwhile to mention these and maybe discuss one or two of them.

First of all, there's the selection of the site, preferably in quiet surroundings, but consistent with other requirements. It's not necessary to build a church, say, near a busy freeway or on Wilshire Boulevard. It would be better to choose a site a block or so away that's reasonably quiet. Always when I'm consulted early I give attention to the site: where is the site? And, second, there's the making of a noise survey (which I usually make, or have a competent acoustician make) to determine how much sound insulation must be incorporated in the building to meet noise level requirements on the inside. That is, if you make a sound survey at the site and you find out that, well, every five times an hour the level exceeds 95 decibels, and you can tolerate not more than 35 decibels on the inside, then you've got to provide sound insulation between the outside of 95 and what you've tolerated inside of 35--that is, 95 minus 35, or you must design 60 decibels of insulation into the structure.

MINK: It was impossible, say, in the 1920s, to make a good sound analysis.

KNUDSEN: That's right. There were no sound meters at that time. However, I improvised a sound meter for making the measurements of the sound insulation of the Metro-Goldwyn-Mayer sound stages.

MINK: Oh, you did improvise?

KNUDSEN: Yes. We used electro-acoustical devices. We of course had a sensitive microphone. We had the ingredients of what later became a sound level meter, but we had it in a cumbersome cabinet that resembled a coffin. Our equipment that we'd have to take out to measure these sound transmissions or insulations altogether weighed more than 500 pounds.

MINK: Where did you improvise this, in the laboratories on the Vermont Avenue campus?

KNUDSEN: Oh, yes. This was done at the Vermont Avenue campus.

MINK: I see.

KNUDSEN: The amplifier we used was in what I said we called a "coffin." It was about as big as a coffin. And the technique we used was based on what I had learned at the Bell Telephone Laboratories--Western Electric Research Laboratories then.

MINK: And now it's such a little meter that you can hold it in your hand.

KNUDSEN: Oh, yes. I can hold this model. It weighs only three or four pounds, so I carry it around with me a great deal--in buildings wherever I go, streets, subways, autos, busses, and airplanes.

Well, the second thing we normally do in the acoustical designing of an auditorium, church, or important building is to make a noise survey. If it's for a concert hall or other music building, we make a complete noise survey. For example, a careful survey was made near the site of the Santa Monica Municipal Auditorium. I didn't make the survey there. Donald Loye, who is an acoustical engineer, made a very thorough survey of aircraft noise, street traffic noise, and so on. I have made similar surveys at Huntsville, Alabama, for what is called the "civic art center" there, and at the site for the Grady Gammage Auditorium at Arizona State University, and scores of other sites. I spent a day at Arizona State making noise measurements before we decided how much sound insulation we would require to exclude the aircraft noises and street traffic noises and other noises that you might have. Similarly, I spent five days making noise measurements at the site for the Performing Arts Hall at Akron University. The site was near a railway and therefore sound insulation is very important. So this is a routine part of acoustical designing of auditoriums today.

MINK: When you get to your site, is it already chosen? Are you stuck with the site?

KNUDSEN: Sometimes you are.

MINK: Sometimes you can say, "This site is not good. Let's get ourselves another one."

KNUDSEN: Yes. I can say, "It'll cost an extra $100,000 if you want to build here. If you had a quieter site you could save this $100,000"--something like that. Because if you sound insulate against another 10 decibels, the cost can easily go up maybe 10 or 20 percent.

Well, the third problem we try to solve is the arrangement of the rooms within a building. In a school building it is especially important to make sure that you don't have, say, a very noisy band room adjacent to a recording studio, or lecture room. Very often school buildings have a band room with practice rooms opening into the band room hoping that they can use all these rooms at the same time. Well, if any design like that is brought to me, I say, "This is ridiculous. You must have these practice rooms moved away from the band room, or from the orchestra room, or the choral room."

You see, often, especially in smaller buildings, the teacher who's teaching the band or orchestra wants to watch these practice rooms. From the point of view of convenience, they have little windows in the practice room doors, so they can see what's going on inside these [laughter] practice rooms. So they often don't like to move these rooms. But this is an example of what you need. If you're going to have rooms that require a great deal of quiet, you wouldn't want them adjacent to a shop where you have a lot of noisy machinery. And so the arrangement of the rooms within a building is very important. And they're important, say, in a building like the Los Angeles Music Pavilion. You have an orchestra rehearsal room. There's a lot of sound level in that room, so you have to have good sound insulation between it and adjacent rooms; therefore, you'd locate it normally in a place where you don't have to spend too much for sound insulation. So the optimal arrangement of rooms in a building is dependant on the amount of sound insulation you have to build into the walls, the floor, and the ceiling of each room. That's a third acoustical consideration.

Then, there's the selection of the proper overall sound insulation construction--based on the findings of the noise survey and the location of the various rooms and the functions they are to serve. This information, together with economic considerations, lays down the ground rules for determining what kind of sound insulation structures you should use. These are matters that must receive early attention. Then there is the control of noise sources within the building, including especially the noise from the air conditioning system, but also from transformers, from elevators, or from other mechanical or electro-mechanical equipment throughout the building. And there must be adequate sound isolation against air-borne and also solid-borne noise from these sources.

Then there's the design of the shape and size of each room that will insure the most advantageous flow of properly diffused sound to all auditors and that will facilitate the hearing of speech and enhance the aesthetic quality of music. In the last ten years, this especially has been more and more emphasized, and so I now insist on sitting down with the architect, and the client, or the owner, at the initial stage of designing, to discuss shape, dimensions, and things of this sort. For music rooms, you want good diffusion of sound for all frequencies, a pleasant flow of sound from the stage to the audience, but you want early reflections, especially from the side walls.

There mustn't be a time gap in which there are no early reflections as there were in Philharmonic Hall in New York or in the Berlin Philharmonie. In both of these modern halls, there was a lack of early reflections. In order to feel that the reverberation is really what it is calculated to be--the reverberation is defined as the time for the sound to die away 60 decibels--it's more important to know the rate of decay during the first 15 decibels of decay, because after the first 15 decibels of decay, especially in rapidly flowing music, the next note has sounded and you don't hear the residual reverberation of the preceding note. So it's <u>how</u> the sound dies away during the early phase of its decay that determines our subjective sense of reverberation. Thus if a musical tone dies away 15 decibels during the first quarter of a second and then 45 decibels during the next 1.25 seconds, we would have the impression that the reverberation time is only 1.0 second, whereas it would take 2.0 seconds to die away 60 decibels.

MINK: I wonder now if in terms of perhaps what is the most controversial building--the Lincoln Center--could you discuss this?

KNUDSEN: Well, would you like it discussed at this stage?

MINK: Well, I think so.

KNUDSEN: Well, yes. I worked on this project throughout the entire year of 1963. The Philharmonic Hall at Lincoln Center, New York, opened in September of 1962. I was present at the opening and was aware of some of the defects, because at once in rapidly flowing music it just didn't seem like it had 1.9 or 2.0 seconds of reverberation which it was calculated to have. And if there was a long sustained tone at the end of a chord you could hear it reverberate all right for two seconds. That was all right. But some of the early complaints, and certainly one that I observed early, was that it didn't seem like it had as long a reverberation time as it was calculated to have.

There is a difference between what we call "subjective reverberation" and "objective reverberation" time. The objective reverberation time is the actual time in seconds for the sound to die away 60 decibels. Well, if the sound dies away at a uniform rate, say-- if the optimal time is two seconds, then it's dying away 30 decibels a second, or 60 decibels in 2 seconds. Or it would die away 15 decibels in a half a second. Well, now, if there are not adequate early reflections, then the sound dies away faster at first than it does subsequently. What happens during the first 15 or 20 decibels of decay is the important thing for determining your estimation of reverberation, especially when you are listening to rapid flowing music.

The acoustical designers of Philharmonic Hall attempted to introduce early reflection by a system of ceiling panels placed about 30 feet above the floor level, whereas the ceiling was about 60 feet above the floor. These panels, about four feet wide and eight feet long, are sometimes called "acoustical clouds" and sometimes "surfboards," because they had somewhat the shape of a surfboard. They were elongated and hexagonal in shape. There were some sixty-five or so of these panel reflectors suspended from the ceiling. They were spaced so that the openings between the panels had about the same area as the panels. The panels extended over the stage and out over about the first half of the main floor of the auditorium. Their function was to provide early reflections. They do provide early reflections for high frequencies, but much of the recent research that we have done here and that has been done elsewhere shows that low frequencies aren't reflected. We've investigated reflections from suspended panels here beginning four years ago. I have a manuscript in these two folders here, By Drs. Delsasso, Leonard, and me, that's just about ready to go to the publishers. The title says, "Room Acoustics: The Effects of Absorptive Floors and Suspended Ceiling Panels on the Location of the Source and Receiver on Growth and Decay Curves in a Room." And this research paper brings the subject of room acoustics pretty much up to date. It will be published in the Journal of the Acoustical Society of America. In a few days we get the final drawings; we check them and then the article is on its way. This is the last work on which Dr. Robert W. Leonard collaborated with us. You know, he died not long ago.

Well, this paper completed our investigation of one of the shortcomings of suspended ceiling panels. It demonstrated that an array of panels reflects frequencies for which the wave length is short compared to the sizes of the panels and the spaces between them. When the wave length is long compared to the size of the panels and the openings between them, the sound isn't reflected at all. We've made many measurements of the reflection and transmission characteristics of panels that are one-third the size of the panels in the Philharmonic Hall. Therefore, in order to simulate the panel array in Philharmonic Hall, we have to use frequencies that are three times as high--that is, if we want to simulate what happens at 1000 cycles for the full scale panels, then we experiment with the experimental panels at 3,000 cycles. Or if we want to know what happens at 100 cycles we use 300 cycles, because the experimental panel array was about one-third of the size of the panel array in Philharmonic Hall.

We found in our experiments here that the reflection coefficient was actually zero for wave lengths that are long compared with the size of the panels and the openings between them. This, translated to Lincoln Center, would mean frequencies below about middle C, or about 260 cycles per second. And so you do not get early low frequency reflections from such a ceiling of suspended panels and this lack of low frequencies introduces a frequency distortion because, you see, the high frequencies are reflected and the low ones are not. It fails to give for low-pitched sounds the acoustical intimacy that you would like to have. If you measure the rate of decay of sound in Philharmonic Hall, as the Bell Telephone Laboratory people (Dr. M. L. Schroeder et al.) did, it was found that the initial rate of decay was about 80 decibels per second for the first 20 decibels. That would correspond to a reverberation time of three-fourths of 1 second instead of 2 seconds. Furthermore, for rapidly flowing music, cultivated listeners had the impression that the reverberation time was much less than the design criteria it called for, which was close to 2 seconds, which would correspond to a decay of 30 decibels per second.

MINK: Well, you said you worked on the design of this building.

KNUDSEN: No.

MINK: You did not?

KNUDSEN: No, I must set the record straight there.

MINK: Well, excuse me.

KNUDSEN: I happened to be present at the opening concert because I was in New York, coming back from the Copenhagen international congress on acoustics in September of 1962, and I made certain observations in Philharmonic Hall at that time. But in February of 1963 I was retained as chairman of a team of four consultants who were charged to investigate the acoustics of the hall and to submit recommendations. The other three were Paul Veneklasen, who is in Los Angeles and a former student of mine; Dr. Manfred Schroeder who was assisted by three of his colleagues at the Bell Telephone Laboratories in making the necessary measurements and working up the data; and Heinrich Keilholtz of Hamburg, Germany. The work of the team continued throughout all of 1962. My work terminated at the end of 1962 when we completed our survey and submitted our recommendations for stage one of the improvement program.

MINK: Then what you've been telling me for the past ten minutes is what your findings were in terms of the [needed corrections].

KNUDSEN: I am leading up to our findings and our recommendations. I made thirteen trips to New York in 1963 to collaborate with the team and others in connection with the acoustical investigation and correction program. We listened to symphonic rehearsals and programs; we measured early reflections and reverberation for typical seating locations; we recorded echoes from the rear walls. The BTL members, Veneklasen, and I were acoustical physicists; Keilholtz was experienced in making symphony and opera recordings for one of the big gramophone companies in Berlin. He had not studied acoustics but he played it by ear and had the support of conductors George Szell and Joseph Krips, for whom he had made recordings in Germany and Austria simply by his sense instead of his calculations and technology. But this was the team that I worked with during the entire year of 1963 in making certain changes in the acoustics of the Lincoln Center Philharmonic Hall, which after our investigation consisted of raising the ceiling panels and filling in the openings between them, modifying the shape of the side walls, making the rear wall very absorptive, and making other minor changes.

MINK: Now I suppose that it was all done in good faith to begin with.

KNUDSEN: Yes, it was. The man who was responsible for the acoustical design had spent many years investigating the acoustics of the most prominent opera houses and concert halls throughout the western world.

MINK: And this man was who?

KNUDSEN: He was Leo Beranek, president of Bolt, Beranek and Newman. He's one of the most capable scientist-engineers we have in the field of acoustics. I have a very high regard for him. His five-year research led to the publication of a book. I have the book here somewhere entitled Music, Architecture and Acoustics. It's a very fine book. It's probably the best compendium of the acoustical characteristics of prominent concert halls and opera houses that's ever been put together. In this investigation he found that the ones that had the highest ratings subjectively, by the conductors, by music critics, by competent listeners, and so on, were those in which you had early reflections, and he referred to this as "acoustical intimacy." The concert halls with the highest rating actually had a delay of not more than 25 to 30 milliseconds between the first reflected sound reaching a typical position in the audience and the direct sound that goes unreflected.

Well, it's normally not possible to attain such short time delays as that in a large auditorium. In the prominent concert halls of Europe this was usually attained in relatively small, narrow halls, and the earliest reflections were horizontal (from the side walls) rather than vertical (from the ceiling). Because the ears are located horizontally, not vertically, they give more prominence to horizontal than to vertical reflections. In your stereophonic systems, as you know, you don't line up the loudspeakers vertically; you line them up horizontally so they're parallel with your two ears. You locate the origin of a sound by rotating your head to the right and left more than by nodding your head up and down.

Beranek planned to obtain acoustical intimacy by ceiling panels suspened about 30 feet above the orchestra and the front half of the audience. There were two acoustical shortcomings inherent in the panels. First, for reasons I have explained, they did not reflect low frequency sounds, so there was a bass deficiency, that is, a loss of low-pitched sound. Furthermore, as I have briefly indicated, the intimacy was for vertical rather than for the preferred horizontal reflections. I'll go into that in more detail a little later, if not today. When you design a hall for 2,650 seats, as you have in Philharmonic Hall--they had that many initially but they have a few more now because they rearranged the seating on the main floor--it calls for a large auditorium. And in order that the audience be not too far away from the stage, the large seating capacity usually is accomplished by spreading out the side walls; Philharmonic Hall does spread out quite wide. Furthermore, the side walls for the stage were far apart. As a result, side wall reflections were delayed too much.

MINK: It isn't very deep.

KNUDSEN: No, it isn't very deep. You'd like to get a lot of reflections in the first fifty milliseconds after the arrival of the direct sound. And if you don't, you will have the condition that for rapidly flowing music the room doesn't give "life" or sustaining reverberation to the hall. The hall sounds like a dead room for presto music and not like a concert hall having a two-second reverberation. That's a complaint that Leonard Bernstein repeatedly made. It sounded more like a hall with one-second reverberation. As a matter of fact certain measurements that the Bell Laboratory members of our team made, and to which I already have referred, revealed that the early phase of the reverberant sound corresponded to a reverberation time of less than one second. Their measurements were both precise and exhaustive. I recall that at one time (before their measurements were completed and evaluated) the man in charge of the BTL work, Dr. Manfred Schroeder, said if the Philharmonic people would have to pay just for the computer time that we've used in analyzing our data it'd be about $75,000. This is some indication of the extent of the work that the Bell Laboratories generously contributed as a public service to the New York Philharmonic. And their work has led to several publications which have made important contributions to the design of concert halls.

MINK: Now this has all been corrected?

KNUDSEN: It has been improved. The main improvement which was made as a result of the survey conducted in 1963 was to fill in the spaces between the ceiling panels and to raise this suspended ceiling. Later, an acoustical shell was provided for the stage which enabled the orchestra members to hear each other better, and it also furnished much-needed early reflections for the audience. Normally you don't have a separate shell for smaller concert halls, because the width of these halls is small enough to furnish early reflections, but we recommended changes for narrower and more complete enclosure and they now have what is essentially a shell. Much of these later improvements resulted from the so-called "second and third stages" that I didn't work on.

We recognized that certain changes were necessary on the stage, and they have subsequently been made, and certain changes have also been made on the side walls to introduce more diffusion there. The observable echoes came from the rear wall surfaces. These rear wall surfaces were slightly concave towards the stage which resulted in the kind of echo we have in Royce auditorium. As you probably know, if you sit especially in the front of Royce Hall you get an echo that comes from the rear that's quite disturbing; similar but less disturbing echoes came from some of the rear wall surfaces in the Philharmonic. One other thing we did was to add very thick absorptive treatment to these rear wall surfaces.

MINK: So has the reverberation time gone up to two seconds?

KNUDSEN: Well, it's more nearly that than it initially was, although I haven't seen the latest measurements that the Bell Telephone people made. It now is better than it was when it initially had a bass deficiency and the equivalent of only three-fourths of a second for presto music.

MINK: Well, it's just hard for me to understand. I know you have explained how it is that you say you are going to build a concert hall. It's going to have these properties and then it just doesn't turn out to have them.

KNUDSEN: Well, there are two concert halls of recent design in which mistakes were made--at Lincoln Center and at West Berlin. They happen to be two of the most prominent concert halls--and both were lacking in early reflections. Both halls are larger than older halls that have better acoustics. Both halls have employed suspended ceiling panels to provide early reflections, but, as I have explained, because of the openings between the panels a ceiling of suspended panels does not reflect low frequencies. This was not recognized until recently.

MINK: Well, it does seem to me, though, that in the basic structure of the building, this concave back wall would have been avoided because, as you have pointed out earlier, this was one thing that was cautioned against early in the game.

KNUDSEN: Yes. We learned it here in Royce Hall, and since then I have avoided them in my acoustical designing of auditoriums. Later I shall refer to Grady Gammage Auditorium at Arizona State University, in which we have good diffusion, early reflections, no echoes, and other desirable features. Sometime I'll take you downstairs to show you some of the acoustical models we have in the laboratory. These models are one-twenty-fourth of full scale. By scaling up the frequency of sound twenty-four-fold for experiments in these models we can predict how the full scale auditoriums will respond to both speech and music, and thus we can avoid the mistakes we have just discussed, and design auditoriums that will have optimal acoustics.

The favorable outcomes in acoustics of the Dorothy Chandler Pavilion and the Grady Gammage Auditorium at Arizona State University demonstrate that even large multipurpose auditoriums can be designed to have very good concert hall acoustics. Both have received very fine reviews, and we constantly get reports from conductors and others who use them that they are among the finest auditoriums they have ever performed in. And we've learned a lot from the experiences of Philharmonic Hall, and from the work that the Bell Telephone Laboratory people did there in analyzing its acoustics, the results of which have been published. And similarly, there have been publications about the Berlin Hall and the changes made there that are in the right direction.

Well, I was discussing the importance of shape in acoustical designing. We were talking about how acoustical designing has changed. The matter of shape now has high priority. We'll later have occasion to refer to Grady Gammage Auditorium because it differs from acclaimed conventional shapes more than any concert hall I know. It turned out unusually well, so we'll spend a little time later discussing that Arizona monument to Frank Lloyd Wright, the exterior design of which was among his last works.

After the shape of the auditorium has been determined, usually with the aid of optical and/or acoustical modeling, the selection and distribution of the absorptive and reflective materials for the auditorium must be worked out, because they contribute to the optimal conditions of the growth for the decay, and for the steady-state distribution of sound throughout the auditorium. Now that's more than reverberation time. How do impulsive and sustained sounds grow? We haven't talked about growth, but that's important also. How does the sound build up, in the stagehouse as well as throughout the auditorium? Your ears have an estimate of what's desirable there. You want the sound sensation to build up rapidly, sustained by early reflections, and then die away at the optimal rate, especially during the early part of the decay.

And you'd like the steady-state to be nearly uniform--that is, if it's a sustained tone you'd like it to be as loud in one location of the room as it is in all other locations. I'll indicate later how nearly we attained that uniformity in Grady Gammage Auditorium.

Then there's the supervision of the installation of acoustical materials and plastic absorbents and especially materials that depend upon the workman, or the good journeyman. If you're using acoustical plaster you have to be very, very cautious, and we require that specimens be submitted to us before approval of the material for use. In many instances we actually require them to plaster one room with the type of acoustical plaster they propose to use and then make measurements in that room to determine what the absorptive properties of the acoustical plaster are. And if approved, they can go ahead; if not, they have to try again.

This was done, for example, in the Los Angeles County General Hospital. We helped the architects with the design of that building, and acoustical plaster was used throughout in the ceilings of all patient rooms, corridors, and most service rooms. It was the biggest building in Los Angeles at that time, so the amount of acoustical plaster was very large. There were four competitors for the acoustical plaster contract, each of whom had prepared a room. We took our equipment over there and measured the effectiveness of the plaster to reduce noise and reverberation. The award was based upon the absorptive properties and the maintenance and on which one could be cleaned most readily, and so on. I believe there were something like 3,000 rooms or it may have been that many beds.

We made measurements in a total of fifty finished rooms selected at random, to see if the contractor had actually met the specified requirements. When acoustical plaster is used, that's very important. You have to watch and supervise from beginning to end. It must be applied the right way. There mustn't be too much water in it. There must be good drying conditions there when the plaster is curing so that the pores remain intact. If the conditions are not propitious, an impenetrable film may form at the surface of the acoustical plaster and then you don't get any porosity from it, and it's no better than hard plaster. There have been many failures of acoustical plaster.

Then there's the installation of the sound amplification equipment. Usually, that's designed by someone else, but the acoustician usually consults with the architect and contractor regarding the choice of loudspeakers and microphones, and especially the location of the loudspeakers, especially the low frequency loudspeakers which may require considerable space.

Then there is the inspection of the finished building, including tests to determine whether the required sound insulation, reduction of internal noise, the sound absorption, and the other acoustical requirements have been met. We do that routinely and we now have commitments to test the outcome of several other auditoriums now under design or construction. Finally, you leave some maintenance instructions with the owner of the building, particularly if there is material like acoustical plaster that could be ruined if it were painted with lead and oil.

We were at La Rue's Restaurant for dinner not so long ago on the Sunset Strip. We were with some rather important people that had reserved what they considered the most suitable location. It was on the circular part around there where you look out across the east, and there was a ceiling which is lower than this ceiling. I'm sure it is not over seven or eight feet. Lew Alcindor almost might bump his head on it. And you can see from the texture that initially it had been an acoustical plaster but presumably it's been painted with lead and oil. And the guests at our table all complained--there were eight of us altogether--that they could hear the people at the other end of the room as loudly as they could hear each other; it was almost impossible to carry on a conversation. You were annoyed frightfully because you had this sounding board above you, you see, which does transmit the sound from one part of the room to the other.

Well, you need maintenance instructions even in our administration building. The original administration building used the same kind of acoustical plaster that was used in the Los Angeles County General Hospital. And it did a very good job. But one morning, when I came to my office in the graduate division there, I found the painters applying lead and oil paint to the acoustical plaster. Well, I hit the roof, as you might expect. But already the corridor leading to my room had been covered over. Later, they applied acoustical tile which is more permanent. You can paint that material even like you can this acoustical tile in my office. This is a fissured tone type of product. I would say this is made by U. S. Gypsum Company and is called "Acoustone." You see, it's fissured and it's possible to use a light spray paint or even a light brush paint, if it's very thin, so that it can be decorated four or five times without destroying its value. Well, the foregoing will give you some idea of the steps we follow now in the acoustical designing of auditoriums and other rooms for which acoustics is a prime requirement.

SECTION XIX

MINK: [What about your work on the Wilshire Boulevard Temple?]

KNUDSEN: At the time it was built and for many years it was called the B'nai B'rith Temple, where Rabbi Edgar F. Magnin has preached and served Los Angeles as long as I can remember. You know, that's a well-known Jewish name, B'nai B'rith Temple. The temple, including the date of construction, is rather interesting. It's acoustical design, one of my earliest church buildings and the first synagogue; it is described on pages 478 to 483 in my Architectural Acoustics, which was published in 1932. This book went to press in 1930, so it had to be before then. (Unfortunately, my early files, containing reports and correspondence on this and other matters are missing, but my consulting on this important building began as early as 1925.)

I find here that the architects and the consultant on acoustics worked in close cooperation from the beginning of the rough sketches for the temple, which began in 1925, and continued until the building was completed, furnished and tested in 1929. So these dates mean that as early as 1925 I was entrusted with important commitments in acoustical designing. At that time I had done some work with Allison and Allison.

The principal architect for the B'nai B'rith Temple was A. M. Edelman, but Allison and Allison, who were the architects for Royce Auditorium and a number of other buildings on this campus, were the consulting architects. And it was largely through Allison and Allison, with whom I had done some work then, that I was drawn into this.

We had one conference, I recall, with the organist. I don't believe Dr. Magnin was present at this conference, but there was someone, probably Dr. Magnin's assistant, who represented the importance of speech in the temple. And I know at that conference the organist wanted a long reverberation time and was advocating about 4 or 5 seconds. But the person who represented Dr. Magnin on this occasion wanted it good for speech. And so we had this somewhat conflict of interest then on their part. But the representative of the church, I think, very sensibly in that case, said, "Why don't we leave this to the architects and the consultant. Let them thresh things out."

And so we did make a compromise between the requirements for speech, which required a relatively short reverberation time, and music, which required somewhat more. I notice the design criterion here that I recommended and which was accepted at that time was that with no audience present we'd plan to have

2.2 seconds, and with a capacity audience of 1800, 1.75 seconds. And to accomplish that we had to have not pews but very heavily upholstered chairs. That was one way, an important way, in which we solved this problem for both speech and music, and also for small audiences and large audiences, and this has been more or less a standard acoustical requirement, at least in my practice, since then. It was a requirement I insisted upon for the acoustics of our Pauley Auditorium.

MINK: Right away I see that that building has one feature which you have been talking about--a dome.

KNUDSEN: That is right, especially as a feature of the exterior design. It has a very prominent dome. But in the very early conferences we persuaded them not to use a circular shape for the main auditorium, including the base or spring line of the dome. This would have been very bad. A circular shape was used for the Temple Emanu-El in San Francisco, which is a prominent Jewish synagogue there, probably corresponding to this Los Angeles temple. It's the main synagogue in San Francisco. B'nai B'rith has a pretty good shape. It's auditorium has an octagonal shape. In our early discussions we agreed upon the octagonal plan, with the dome springing up from its octagonal base, but this dome was approved only provided this domed ceiling was made very absorptive and deeply coffered. And the interior photograph on page 480 of my book shows the coffers in the dome, which are very deep, and the inside panel of each coffer is treated with very highly sound absorptive material. At that time the best choice was a one-inch perforated acoustical tile.

MINK: I guess this is something you insisted on, isn't it?

KNUDSEN: Yes, very much. The deep coffers in the ceiling were to overcome the focusing effects of the ceiling reflections, and in order to minimize those reflections we made the inside smooth surfaces of those coffers as absorptive as we could possibly make them.

MINK: What material was used there on the interior?

KNUDSEN: It was an acoustical tile. It was probably Acousti-Celotex, the thickest and most absorptive acoustical tile that was available at that time.

We also recommended tests in an acoustical plaster for some wall surfaces, and for other rooms in the temple. Before approving the plaster we insisted upon their preparing a room with a kind of acoustical plaster the architects contemplated using in several of the other rooms. And so, during construction, one of the rooms was prepared for our testing--the entire ceiling and the upper three feet of the walls were plastered with the preferred acoustical plaster they were going to use in some of the other rooms. The acoustical plaster satisfactorily met our acoustical specifications, and accordingly was used for the ceilings of the large recreation room and several classrooms, besides the synagogue itself--that is, the main assembly area of the synagogue. The size of the assembly is pretty large--100 feet wide; 100 feet deep; and the ceiling at the soffit of the dome, slightly more than 100 feet, depending on the slope of the floor.

MINK: It's a large building.

KNUDSEN: It's a rather large building; the volume of the assembly is over 800,000 cubic feet. This was the first large and important building for which I was called in as consultant on acoustics, thanks largely to Allison and Allison with whom I'd had pleasant relations as consultant for one or two other buildings. We were beginning to talk about some of the buildings on the campus here at that time, the physics building particularly. They were the architects, and we were working with them on the design of our lecture rooms, especially for the physics building over here, which is now Kinsey Hall.

MINK: Were Allison and Allison pretty easy to work with?

KNUDSEN: Yes, very. One of the Allisons, David Allison, is the father of the present Allison of Allison, Rible and Company. They have done some work for this campus and they do a great deal for other educational institutions. Allison and Allison, incidentally, had designed the buildings on the Vermont Avenue campus, and I met them first in connection with the remodeling of Millspaugh Auditorium. David Allison, whom I came to know quite well, lived near the university here. He later became a social friend of ours. We've met them quite often. That relationship helped in working out the problems of the acoustics of the Wilshire Boulevard Synagogue, which has received high acclaim for its acoustics. At that time, Rabbi Magnin often greeted me when we would meet, and comment upon what he considered a fortunate--I thought it was deliberate--outcome in the good acoustics of the synagogue.

MINK: What do you mean you thought it was deliberate?

KNUDSEN: I knew it would be good if our recommendations were followed--and they were.

MINK: Oh, you knew it would be good when you designed it?

KNUDSEN: Yes. But the architects and, of course, the Rabbi and others are never quite so sure. I was a little more cocksure perhaps than I had a right to be.

MINK: Well, what did they think was going to happen?

KNUDSEN: Well, I don't know. The element of luck or caprice was always supposed to be a factor, so that you just didn't know what the outcome in acoustics was going to be until it was completed.

MINK: So they sort of looked at you like they look at the man with the hazel wand.

KNUDSEN: Somewhat, yes. There was a pretty general feeling at that time that, oh, there is no science of acoustics. Acoustics is just guesswork and you don't know what's going to happen. But my early work on the hearing of speech in auditoriums showed just how noise, reverberation, and the loudness of speech determined how well you could hear speech in different acoustical environments, and how you could qualitatively predict the percentage of speech articulation--for example, it could be as low as 55 percent or as high as 90 percent. Thus, if the reverberation time was about 3.0 seconds and the noise level 4.5 dB, the speech articulation would have been about 55 percent for average unamplified speech; but by reducing the reverberation time to about 1.8 seconds, the noise level to 35 dB, and by amplifying the speech adequately the articulation could be as high as 90 percent. An articulation of 75 percent is tolerable and 90 percent is very good. We therefore could predict what the outcome would be for the hearing of speech, and it was generally recognized that for music the reverberation time should be about 2.0 seconds. You see, it takes the hearing of speech quite definitely out of the realm of guess-work, and I'll talk more about music later. It's much more critical and difficult to design an ideal concert hall.

MINK: What do you think made this feeling of risk or uncertainty about it? Is it simply that no attention had ever been paid to this before?

KNUDSEN: Well, architectural acoustics certainly had not received scientific study until Wallace C. Sabine undertook his epochal research at Harvard in 1895. I have a chapter in my book Architectural Acoustics dealing with the early history of that subject. You, as a historian, may be interested in glancing through that chapter. It shows that there were some sensible things about acoustics that were qualitatively understood a hundred or even two hundred years ago. The evolution of the theater brought improvements in acoustics. I summarized most of these important developments in architectural acoustics, and I think I may have told you that one of the early qualitative improvements was due to Joseph Henry who about 1856 was unhappy about the excessive reverberation in the lecture hall at the Smithsonian Institution. And following his recommendations, some sound absorptive hangings were introduced, and with beneficial results. At approximately the same time, Brigham Young was designing the Mormon Tabernacle. Just before the opening (1862) he requested the women of the Relief Society to bring to the Tabernacle their skirts and overcoats and various other clothing of that sort, which were draped on the walls to reduce the reverberation. Later, the side and rear balconies were added to the Tabernacle, which improved the acoustics, and as is well known the Tabernacle is famous for its acoustics.

MINK: In your research for the book, had you been interested in the history of acoustics, or did you feel it was just necessary to include such a chapter?

KNUDSEN: Well, I had read some, of course. I had read Vitruvius, for example, who wrote the well-known five books on architecture. This was always cited in the literature. Most of my reading, much of it in the British Museum, was done very carefully. The first book I had read carefully, and practically memorized, was Wallace C. Sabine's Collected Papers on Acoustics, which was published in 1922, shortly after I had completed my PhD and Sabine's book determined my career.

MINK: And he had been interested in the history himself?

KNUDSEN: And he'd been interested somewhat in the history.

MINK: Did you go out of your way?

KNUDSEN: Oh, yes. I did quite a lot of research to write this first chapter here that deals with nineteenth- and twentieth-century acoustics.

MINK: I should imagine that the literature would be quite elusive.

KNUDSEN: It was, but it was interesting, and it stimulated my desire to add to the history.

MINK: It must have been only in general pamphlets.

KNUDSEN: It was, mostly. There'd been two or three books written on acoustics, including sections on acoustics of buildings. They're referred to in the first chapter of this book. It was source material. I didn't spend years at that subject--my own experimental research had higher priority. I may have spent fifty hours or a hundred hours, something like that, looking at the literature, and I looked up what we had in our meager library here and in the downtown library, and also the Crerar Libary (a technical library in downtown Chicago). Those are the three libraries, besides the one in the British Museum, I made use of in investigating the early history of architectural acoustics.

MINK: Well, now, do you want to talk about one other example of a building in which you were involved?

KNUDSEN: I'll mention one right on the campus. I think that brings it very close to home. [laughter]

Pauley Pavilion is an interesting auditorium and illustrates one of these points on which there will be differences of opinion, and the position you sometimes have to take in order to convince people that the right thing should be done. Very little had been done about the acoustical design of Pauley Pavilion, even at the time the drawings were pretty much completed. Paul Veneklasen, a former student of ours, had made some brief recommendations to the Becket office. It was designed by Welton Becket and Associates. Fortunately, it was rectangular in shape instead of elliptical and therefore it was relatively free from objectionable echoes. Veneklasen had recommended some acoustical treatment for the ceiling and the upper parts of the walls, using acoustical tile or other acoustical material of a suitable nature, such as Fiberglas covered with a perforated membrane. [tape off, Dr. Delsasso arrives]

The campus office of architects and engineers asked Dr. Delsasso, Dr. Leonard, and me to have a look at the plans. We made some calculations of the requirements and recommended some further treatment of the interior and the kinds of materials that would be most suitable to control the reverberation.

DELSASSO: This was well after it had been underway and almost completed.

KNUDSEN: That is right. It was almost too late for herpiside! It was getting pretty late. [laughter] But we did, then, at the request of our local office of architects and engineers here, make some calculations on reverberation and a number of relevant things. But the principal defect was that the design called for just wooden seats throughout. This meant that the reverberation would be very much in excess of the optimal value when only a few thousand people are present. And the Pauley auditorium was to be the next size above Royce, so if you have to accommodate more than 1900 people here for almost any kind of indoor exercise, you're obliged to go to Pauley Pavilion. And the only way you could make that large hall suitable for audiences, say, of a few thousand, 3,000, 5,000 or even 8,000, is to have upholstered chairs which are nearly as absorptive when they're unoccupied as they are when they are occupied; and for properly upholstered chairs the difference isn't very great. You can get four units of absorption in an empty chair, and when it's occupied it's not more than five, depending on how the occupants are dressed. So you're doing a pretty good job of simulating a person in a seat with upholstered chairs, whereas a wood chair is only about 0.2 of one unit. So with wood seats we would have had an excessively reverberant pavilion.

We had been told that we could not have upholstered chairs here because the university would be criticized that we were going in for luxury. But we insisted that the university would be criticized even more if there was a failure in the acoustics of this hall. The three of us were pretty much concerned about our reputation and pride in the matter, so I wrote a letter to the chancellor's office. I think Bill Young and Chuck Young probably got the letter. The chancellor was away at the time. In the letter I said, "Unless you use upholstered chairs in this auditorium we will publicly declare that the three of us had nothing to do with the acoustical design of this auditorium."

Well, whether that swung the deal or not I don't know, but we received word later that Bill Young, I guess, and Chuck Young got their heads together in the absence of Chancellor Murphy and decided that there would be upholstered chairs. We meant what we said, that we would very definitely withdraw from having anything to do with the place unless they installed these upholstered chairs. (I have used the same tactic in several similar situations, and it usually succeeds.) Well, that's one example. There are many interesting ones, but I don't think we should go into any more today.

MINK: Do you have anything to add, Dr. Delsasso?

DELSASSO: Well, except that it was, as I said, almost too late to really do anything without great expense.

KNUDSEN: The acoustical outcome was quite satisfactory, and I am sure that even the spectators of basketball approve of the comfortable seating. I was there yesterday, sitting on the side, about the fifth row up in the balcony, just to the side of the cluster of loudspeakers, and although the sound was a little too loud for the band initially, this wasn't a fault of the acoustics. The sound system performed, I thought, satisfactorily. Dr. Murphy talked a little louder than was necessary into the microphone. But they certainly needed it as high as they had it for Emperor Haile Selassie--I won't use the rest of his title--and for the other persons who performed there. But the intelligibility of speech was good, acoustically. I didn't miss a word. It just wouldn't perform that well if it had been a circular or elliptical shape, and so the shape at least gave us something to start with that was right, very much like the Wilshire Boulevard Synagogue that we talked about earlier today--and the upholstered chairs are performing well their important acoustical function.

MINK: The meeting to which you referred was the Ninety-ninth Charter Day exercises which were held on Monday, April 24, 1967 here in the Pauley Pavilion.

KNUDSEN: That is correct. Emperor Haile Selassie was the Charter Day speaker.

MINK: And the recipient of an honorary degree.

KNUDSEN: And the recipient of an honorary degree. And also our colleague Jacob Bjerknes received an honorary degree. That's the one we scientists were very pleased about because he certainly is one of the top two or three of living meteorologist, if not the top. I attended a conference in 1958 in Bergen, Norway, which is his birthplace and also very close to the birthplace of my father, so I was happy to be there. But Bjerknes was singled out for special honor on this occasion. It was commemorating the twenty-fifth anniversary of the discovery of the polar front by Bjerknes and his father. This discovery led to the development of the dynamics in the entire atmosphere which has revolutionized meteorology. Bjerknes was really feted as the top meteorologist in the world at this particular commemorative world congress. And so we were delighted that Pauley Pavilion was used yesterday, for conferring an honorary degree not only on Haile Selassie but also on our colleague, Jacob Bjerknes. I think that's a good place to stop. [tape off]

There are a number of interesting incidents that occurred in connection with my consulting work. One that I recall is dated July 12, 1927. I know the exact date because it was the date of the birth of our twins, Morris and Margaret, by Caesarean section at the Good Samaritan Hospital. The surgery took place at just about the same time that I was supposed to be consulting in the office of Allison and Allison, who designed some of the early buildings on this campus.

This operation was an emergency, as it often is, and it's a matter of getting the twins out in a hurry, so the surgery was done very hurriedly. I was permitted to be in the room in a white gown and watch the fascinating procedure. I always say that Morris was born with the surgeon's right hand and Margaret with the left, and it was almost as fast as you can say "right-left." They just barely got Margaret out in time. Her body was blue when they extracted her and the surgeon pounded her in the usual fashion; and I really saw her come to life. As soon as she began breathing I rushed off to keep my appointment at the office downtown.

In my excitement I ran through the red traffic signal at Seventh and Figueroa, and I was immediately apprehended by the cop, and the cop said, "You know you ran that traffic signal and damn near got killed?" I said, "My good friend, if you'll call up the Good Samaritan Hospital, you will find that one Mrs. Vern

O. Knudsen, whose name corresponds to the license here, gave birth to twins by Caesarean section just thirty minutes ago." His jaw dropped this way, and he said, "Good God, drive on."

Well, I won't describe the session with Allison and Allison. I know I was excited at that time.

A year later in 1928, I received a phone call from Eddie Mannix, business manager at Metro-Goldwyn-Mayer. This was at the very beginning of the designing of sound stages to accommodate the addition of sound to motion pictures. There was no one else on the entire West Coast who had done anything in acoustics, so I was virtually the only person they could turn to. And there were a number of interesting things that happened in connection with this consulting work at MGM. It was the beginning of the acoustical designing of stages for the recording of sound in motion pictures and later in television and in the making of phonograph records and magnetic tapes.

But the nature of the problem was indicated in this first conference, which was a rather significant one. Present at the conference table that day in the private office of Louis B. Mayer, who was the president of MGM at that time, was Irving Thalberg, who was vice-president and, of course, a well-known figure to those of us who can remember back that far.

He was the husband of Norma Shearer, you may remember. Eddie Mannix was really at that time the manager of MGM. He ran the works. There was no doubt about that. Thalberg and Mayer deferred to him on all things. He had a battery of telephones on the desk before him. This conference was interrupted frequently. I remember one instance in which he pushed a button, and a good-looking secretary came in, and he said, "Make out a check to Sid Grauman for $3,400. It's what I lost to him at poker last night." Then he went on with the discussion. Maybe there was a telephone call from New York about some new release of a motion picture; things like that were interrupting us frequently.

But Louis B. Mayer got in the first comment. He said, "We want these two stages, stages A and B"--which were probably the first two sound stages designed anywhere for motion pictures--"to be insulated from each other so well that you can have gunfire on one stage and record chamber music on the other stage."

MINK: That must have really been a blow to you.

KNUDSEN: Well, I said, "This calls for a very costly type of building." He said, "We don't care. We want that; that's the requirement. That <u>must</u> be the requirement." And, of course, this was a very high requirement, and so I set the target then. There were no such things as decibels. We called them "sensation units" at that time. And I said, "Well, this will mean really that the stage must have 70 sensation units of sound insulation, which really will require a building inside of a building." And that is what we designed. There were no noise meters to use for measuring the sound insulation of these two sound stages, but we knew how to measure the intensity of sounds and we used our homemade equipment with which to check the sound insulation and reverberation as we progressed during the design and construction of these sound stages. The description of the stages you'll find in Architectural Acoustics on pages 574 to 577, and I won't go into the details here, except to say that we did meet the design objectives.

Basically, there were ten-inch thick concrete walls and a very heavy concrete ceiling slab, and then a separate structure built inside of this concrete structure. So that it conformed to my recommendation of a room inside a room. And these two stages remain today as probably the ones with greater sound insulation than any others. The inner structure was designed to completely absorb sound on the inside, so that you could control the acoustics for recording sound by the acoustical design of the set.

This is a brief account of the principles we utilized in these first sound stage designs; the principles and characteristics are described in Architectural Acoustics. I won't go into them here, except to say that stages A and B were too expensive to be duplicated, and I was called in by MGM again to consult with them on stages that would not have as high a sound insulation, but which would be more economical. And that did lead to new and satisfactory designs in which we used a California-type construction--that is, wood, stud and plasterboard structures. But again, it was a room inside of a room, and this type of procedure became the approximately standard technique, not only in California but largely throughout the world. That is, the design of these stages at Metro-Goldwyn-Mayer, I think, set the acoustical standard for later designs. They were pioneer designs in sound stages.

MINK: Well, then, Louis B. Mayer wanted the best, but when he found out how much it was going to cost him, he decided to settle for something a little more economical.

KNUDSEN: Well, during the second conference we had there, he rushed in and said, "I just got word from somebody that would not make it necessary to go to all of this expense in the design of stages A and B. I'm told that there is a wire screen device of some sort that will keep inside the sounds you want to record and keep outside the noise you do not want."

MINK: Did you ever hear of such a thing?

KNUDSEN: No. I said, "I'm sure that your information must have come from someone who is confusing electrostatic phenomena with acoustics." The Faraday in-a-cage experiment indicates that you can conduct experiments inside a wire mesh cage--a fly screen or any metallic conductor of electricity would do--in which you could have violent discharges of electricity outside this cage, but they wouldn't disturb the inside at all. But I said, "Sound doesn't behave that way, Mr. Mayer. Have you tried to talk through a fly screen? "

MINK: What did he say when you said that?

KNUDSEN: Well, he was unhappy about it, but he deferred, and we had no difficulty with him after that. My dealings were pretty much with Eddie Mannix thereafter.

MINK: Can I interrupt just a minute?

KNUDSEN: Yes, of course.

MINK: The first conference, when he told you he wanted one where he could gunfire in one stage and another where he could have chamber music, what else did he say at that conference?

KNUDSEN: That was his main contribution at that time; he wanted the best, and he was willing to pay the price. I told him they would be very expensive, and Mayer and his associates realized that. Mannix had an

MIT-trained engineer who worked very closely with him, Fred Pelten by name; much of my detailed work at Metro-Goldwyn-Mayer was with Fred Pelten.

MINK: So this first conference was a pretty short one.

KNUDSEN: Yes, it was. Louis B. Mayer didn't have much time for conferences of this sort. I imagine it lasted thirty minutes, anyway, which is a fairly long time when you have the president and the vice-president and the manager all together there in this same conference, so it was a high-level conference, and I was an insignificant assistant professor of physics then.

MINK: Were they pressed for time, or did they have commitments in the way of sound pictures that they wanted to get started on?

KNUDSEN: Well, of course, they wanted to be first in sound. You see, they did the movie with Al Jolson.

MINK: The Jazz Singer was the first.

KNUDSEN: Was that the title of it?

MINK: I believe so. It was one of the first talking pictures that I remember.

KNUDSEN: Yes, I think it was. But I know it was Al Jolson, and I think it was the Jazz Singer. I was present while parts of that sound-and-light film were recorded, later, after these first two sound stages were completed. They turned out to have the required sound insulation, and to have the acoustical characteristics that were required, but there was one serious shortcoming, namely, the noise from the air conditioning system. That was a problem that had not been thought of. Carrier Engineering Company that installed the system was the leading outfit in the country in air conditioning equipment, as it is even today. And they, of course, were called in and required to do something about reducing the noise.

The noise situation was so bad that when they had this air conditioning equipment on they couldn't record. When they turned it off, the Klieg lights that they had for illumination generated so much heat that the actors' makeup melted and ran off their faces. And so this was a very serious condition; you can't imagine how disturbed the MGM people were that they were confronted with this condition. They were obliged to cool the studio down, record for fifteen minutes or so, then they'd have to stop recording, let the air conditioning take over again, and then stop it while they recorded again. This was the condition they had.

Well, Carrier studied the problem pretty much, and in effect said, "Well, this is a baffling situation." You see, normally there had been no buildings that were as quiet on the inside as these sound stages, and so therefore the air conditioning noise seemed to be unusually loud. Complaints of air conditioning noise hadn't been widespread, at least at that time, because it was masked by other noises, and Carrier didn't know what to do about it. Eddie Mannix told Carrier, "You'd better retain Vern Knudsen and have him help you with these problems." And I was immediately retained by Carrier. This came in 1929, which, because of on-coming depression, was a handy time to be retained.

MINK: I should say so.

KNUDSEN: I had a two- or three-year contract with Carrier, with a retainer, I remember of $3,000 a year plus $100 a day. For a young assistant professor who was getting probably $2,700 or at most $3,000 a year at that time, this, my first retainer, was a real windfall. The retainer may sound very high for a professor, but the retainer and per diem were suggested by Carrier themselves. I worked for them three years (probably five to ten days a year) and they survived it very well. As it turned out my services for Carrier set the stage for the control of noise in air conditioning systems. It really hadn't been dealt with very much before that.

MINK: They probably saw the handwriting on the wall.

KNUDSEN: I think they did. They realized then that because MGM had to shut down when they were making motion pictures, they just couldn't afford to bear the stigma of having their equipment shut down more than half the time, just to keep the room cool enough to operate in it.

As you know, travel at that time was almost entirely by train, so I had three days and four nights between Los Angeles and New York (headquarters for Carrier) to work out feasible schemes for reducing the noise in their air conditioning systems, most of which was transmitted through the ducts. When I arrived in New York, I had worked out in detail three schemes that I knew would meet the requirements for reducing the noise that was transmitted through the ducts into the sound stages. The first of these consisted simply of lining (inside) great lengths of the ducts with sound absorptive material. We had no measurements, but I knew that if you lined the ducts with an absorptive material, noise could be reduced. We didn't have many fiber glass materials at that time or mineral wool products, but we did have hair felt. At that time the available absorptive materials were mostly not fire-proof, but usually materials like hair felt or flaxlinum and fiberboards. But they were sufficiently sound absorptive that you knew that if you lined the ducts with such absorptive materials you would suppress the noise very much.

The second system, and one which was used for Metro-Goldwyn-Mayer at that time, and has been used in many other places since then, was to divide the duct into small channels, made of sound absorptive material. The duct has to be made oversize so that you can divide it into a honeycomb structure, using a suitable absorptive material, and resulting in many little absorptive parallel ducts instead of the one big one. The cross section of each little duct was something like 6" x 8" instead of something like 3' x 6', as the big duct was. You see, you divide the big duct into little parallel cells all made with absorptive materials, and by that means you don't have to utilize a great length of the duct. That is, you can obtain a high degree of noise attenuation (at least 15 dB), with a length of about 15 or 20 feet with this honeycomb structure, built up of a sound absorptive fiberboard. It must be rigid enough, of course, so that you can fabricate it. But there were many materials even then that were suitable, and so this method was used for the MGM job.

The third method consisted of what was then known as the use of acoustical filters. A professor of physics, George W. Stewart at the State University of Iowa, had worked a great deal on this subject, and I knew that there were acoustical filters that could be used. They probably would be more expensive than the other two schemes, but they could do the job. So these three schemes were discussed fully with Carrier in this two- or three-day conference that I had there in 1929. This led to a very pleasant relationship that I had with Carrier. Although the contract was for two or three years, they called me in on other problems, and students of mine who had specialized in acoustics here, two or three of them I know, were later employed by Carrier to carry on this work.

Well, the Carrier Company had quite a number of other installations that were giving them noise trouble at the same time, including those in the U. S. Senate and the House of Representatives, and also one in the White House. Carrier was at that time the number one organization in air conditioning, much more in control of the whole field at that time than it now is. The word "airconditioning" was devised by Mr. Willis Carrier, who was the originator of air conditioning. And so they really had a virtual corner on the market at that time. They installed many other noise attenuating systems similar to the one I had devised for sound stages A and B at Metro-Goldwyn-Mayer.

During those early years I often checked their proposals for installing noise attenuators. The honeycomb structure was the one which usually was most effective, although in other instances the entire duct, if it was long enough, was lined, and that turned out to be more satisfactory because it didn't reduce the air flow so much.

MINK: May I ask a question here? You speak of the honeycomb structure which you had presented in a schematic drawing.

KNUDSEN: Yes.

MINK: Was this something that Mr. Carrier considered before, or was this something that you brought to him and his company benefitted by it?

KNUDSEN: Well, I brought it to Carrier. It was novel with me and they patented it.

MINK: They patented it?

KNUDSEN: Yes.

MINK: Now, was that because they were paying you the retainer?

KNUDSEN: Yes.

MINK: You couldn't patent it?

KNUDSEN: I didn't think of patenting it myself at the time. This wouldn't be a fair thing to do, anyway, because they were retaining me. I had worked at the Bell Telephone Laboratories, you know. As is customary, I assigned all my patent rights to BTL for one dollar at the time I was employed there.

MINK: I remember you telling me that.

KNUDSEN: And this has been a more or less standard practice, and it didn't occur to me that I should try to patent that. If I had patented it on the side, I probably would have benefitted much more than I did by this retainer.

MINK: But that's what you meant when you said as it worked out Carrier "benefitted."

KNUDSEN: Yes. Indeed they did.

MINK: That's one point I wanted to get clear.

KNUDSEN: Well, there was one other incident since we are talking about fees and so on. I had charged Metro-Goldwyn-Mayer $12.50 an hour, which figured out at $100 a day. It was considered that this was a fair amount for a top consultant in those days. I considered it very good pay for a young assistant professor. But after I had completed my work on stages A and B, Mr. Mannix called me in just for a personal conversation, and he said, "Knudsen, I want to give you some personal advice." He said, "Your services on the stages A and B were worth so much more than you charged us that I think hereafter you should make your charge on the basis of a fixed fee for the services you are going to perform." And he further said, "We want you to help us with the design of four more sound stages. I think $2,500 would be a more reasonable fee." Well, there wasn't twenty-five days of labor involved. I would estimate that ten days at the most would take care of that total operation.

I learned more about this later from a friend of mine who was at Metro-Goldwyn-Mayer and had done quite a lot of work for them in public relations. I think it's better to leave his name out of here because he would be recognized at once, and it wouldn't be kind to him to bring this up. He went to Mannix and complained, "This is ridiculous that you bring in a consultant of this sort and pay him only $100 a day. Everybody will be asking us to reduce our fees." And so I suspect it was largely the result of this advice that Mannix had received from one of their other highly paid men that he made this proposal to me.

Well, my work at MGM, of course, came to the attention of the other motion picture companies that were entering this new field of adding sound to motion pictures, and as a result I was retained by Paramount, by Fox--it wasn't Twentieth Century-Fox then, it was Fox Studios--by Universal Studios, by Warner Brothers, and by several lesser-known names that have probably gone out of existence since then. But for the major motion picture companies in Southern California, I helped design their first sound stages. These designs really pioneered techniques that were used in New York, Paris, London and elsewhere. Probably without my work they would have arrived at approximately the same solutions. But the similarity between especially the second group of four stages that we designed for Metro-Goldwyn-Mayer became the approximate technique for developing these new sound stages.

MINK: Did it seem to you that there was a conflict of interest here, by working for these various studios?

KNUDSEN: No.

MINK: Or didn't MGM care once they were the first out with sound?

KNUDSEN: I don't think there was any conflict; it was never called to my attention, at any rate. And they knew I was an independent consultant and was free to sell my services anywhere.

MINK: Did you derive any patents from the work that you did?

KNUDSEN: No. I didn't attempt to.

MINK: Was the work you did later patented by somebody else?

KNUDSEN: I'm not sure that the designs of the studios involved patentable material. It just didn't occur to me. I wasn't extremely patent-minded at that time, and it never occurred to me that I should seek a patent for the designs of these sound stages.

MINK: When you had your first conference with Louis B. Mayer and you came back to the university, what did you do? I mean, how did you go about this? Did you set up a model?

KNUDSEN: No. We didn't build models in those days, but I had repeated conferences with this Mr. Pelten, who was their engineer. He was a graduate of MIT, and he was an intelligent and competent person.

MINK: You could communicate with him.

KNUDSEN: Oh, very definitely. As a matter of fact, he also had twins the same age within a matter of a month or so of ours, and years later they were in the university nursery school together. So it was only a couple of years or so until the Peltens and the Knudsens were very good friends because of our twins and their twins, and this friendship has continued. Fred Pelten died long ago, but their mother is still living, and we see her and her twins often. As a matter of fact, one of the twins is the wife of a professor here at the university.

So far as my consulting work is concerned, Pelten and I worked closely together. That is, I had regular sessions with him, maybe two or three times a week. In the late afternoon, I'd go over there and spend two or three hours with him. Often, technical problems arose that I'd take home for further study overnight or weekends.

My consulting work did not interfere at any time with my teaching and research. The record would indicate that my teaching and my research were just as prolific at that time as they were at any other time. This is one thing I always insisted on when I was retained, that this must not interfere with my teaching and my research at the university.

MINK: Now I think you said that you saw part of the Jazz Singer made?

KNUDSEN: Yes.

MINK: Did you watch it?

KNUDSEN: Yes. I saw one or two sequences there. But I was so absorbed in my own work that this didn't attract me very much. But I remember at one time John Barrymore and Lionel Barrymore were performing together on a film while I was making some measurements of the noise from the ventilating ducts, and someone said, "My God, that's John Barrymore and Lionel Barrymore over there. Can't you take time off?" I said, "I'm not interested in them. I'm interested in finding out what's wrong here." I was not present when Al Jolson was there. But I was present when some of the sound sequences or choral sequences for [the picture were worked on].

MINK: For the Jazz Singer?

KNUDSEN: Yes. Douglas Shearer, who is the brother of

Norma Shearer, was their sound engineer at that time. He began at about that time, not during the early design of the stages, but by the time these stages were in use. He often said he got his private instruction on acoustics from me. We had two or three sessions together. He came to me about his problems, and this was not a retainer affair but in appreciation to Metro-Goldwyn-Mayer for what they were doing for me in the way of consulting work.

MINK: Doug Shearer's job was to see that the recording equipment was working properly?

KNUDSEN: Yes, but also to see that the sets were properly designed for sound recording, and so I had several sessions with him on the design of sets for these various productions. You see, it's very much like building a set out in the open, because the walls and ceiling of the sound stage are so highly absorptive. You therefore build into the set the acoustical requirements that you need for the best acoustical performance. In some of the stages that later were used for symphonic music, the stage had to be designed otherwise. One of the first of these I think was at Universal Studios; and for recording symphonic and choral music, you design very much as you would for a concert hall, but with slightly less reverberation. Whereas, in the general utility sound stage, you make it as dead as you possibly can--that is, we were using something like 6 inches of mineral wool material on the walls and four inches in the ceiling. Although such a sound stage is not as dead as our anechoic chamber downstairs, it's a very nonreverberant room. It's almost like out-of-doors.

MINK: And that makes the sound of the voices more audible or makes them come through clearer.

KNUDSEN: Well, you control that by the acoustics of the set. If you want to have living room acoustics, you simulate the conditions you'd have in a living room; and so you have furniture, but it's almost an enclosed set then, you see. It's open for the camera and lighting and things of that sort, but it's almost a complete enclosure then. You try to simulate the reverberant conditions that you would have in a living room. Or you simulate a bedroom, or other typical rooms, or even you simulate the out-of-doors, then you leave the set as open (or nonreflective) as possible.

A second noise problem developed at Metro-Goldwyn-Mayer had to do with the isolation of fans and motors. In particular, in these early days, they recorded on wax disks, and the disk recording machine was very sensitive to vibration.

MINK: They hadn't yet put the sound strip on the film.

KNUDSEN: That's right. In those days it was still recording on a wax master, you see. They first cut the grooves in the master wax disk, which was about 1 inch thick and 14 to 16 inches in diameter. Well, on some of these earliest disk recordings at Metro-Goldwyn-Mayer, there was a 60 cycle background noise. It was 60 or 50, I forget which now. We found out what it was. The difficulty arose from a nearby motor generator that was mounted on what the responsible contractor said was the standard way of isolating such electrically induced vibration so there wouldn't be solid-borne sound transmitted from it. The noise from this motor generator was undoubtedly transmitted through the base, through the concrete floor into the wax shaving machine, where they were preparing them for the next recording. And so after the record was supposedly cut clean and smooth there was still a 50 or 60 cycle hum that was present there. Well, it was quite obvious that this was a solid-borne sound, and we traced it over to this motor generator set that was in a nearby location. We actually measured the amplitudes of vibration along the path from the motor generator to the wax shaving machine. The motor generator on a 4 inch concrete slab was mounted ("isolated") on a 2 inch layer of cork.

Well, when we measured the amplitude of vibration on the slab above the cork and on the concrete floor below the 2 inches of "isolation" cork, the amplitude was greater below than above. In other words, the combination of the slab and its motor generator was of such a mass, together with the compliance (flexibility) of the cork, that the vibration was actually amplified. I'm sorry I can't make drawings for you here, but if the mass on the cork and the compliance of the cork are of such a nature that the mass supported by the compliant cork has a resonance frequency coincident, or nearly coincident, with the frequency of the alternation of the electric power supply, then you would actually get amplification instead of reduction of the amplitude of vibration.

When we calculated this resonance frequency and found that it was actually near the AC frequency it was obvious that the cork in combination with the slab was amplifying instead of attenuating the vibration set up by this motor generator. I thought, "Well, there's a very simple solution to this." It consisted of cutting away 95 percent of the cork, leaving only little pads, uniformly spaced, to support the concrete slab and its motor generator. We got some workmen there to just augur away 95 percent, leaving only 5 percent of the cork. Now this reduces, you see, twentyfold the area of cork supporting the slab. This reduction in the area of the supporting cork increases the compliance twentyfold. And this lowers the resonance frequency by the square root of 20, or more than fourfold. This isn't too technical, is it?

MINK: No.

KNUDSEN: You see, the square root of 20 is about 4.5, so we had avoided the resonance condition that amplified the vibration. And when we measured the amplitudes of vibration on the slab on which the motor generator was mounted and on the concrete floor below the slab, we found a very marked reduction, because we'd shifted this resonance frequency way, way below the 60 cycles. It was down, you see, to less than 15 cycles.

MINK: That's hardly audible, isn't it?

KNUDSEN: Well, that's not audible, but the chief reason is that it is far below the frequency of the disturbing 50 or 60 cycle source. Well, this corrected it completely. And this experience had a bearing on future isolation supports of motors, fans, generators, pumps and similar vibrating or rotating equipment. But this simple method was devised there and the remedy was accomplished without delay, you see, and without expense except for the labor of the workmen who cut away 95 percent of the supporting cork slab. And vibration isolators now are usually small compliant pads or steel coil springs, so that you get the required compliance to make the resonance frequency of the system low compared to the frequencies you want to isolate.

That's what we have, for example, for our reverberation room and for our anechoic chamber here. These two rooms are highly isolated against vibration and solid-borne sound. We can support these large rooms on inflated rubber tires. They are smaller than most tires and are of doughnut shape, and they lie flat. They can be inflated by a motorized air pump sufficient to raise the 12 inch thick concrete walled structure of the anechoic chamber or of the reverberation room from its concrete piers. The dimensions of the reverberation room are 24' x 19' x 30' x 24' high. And the anechoic chamber is about the same size. Well, when you recognize that the walls are 12 inches thick, and that concrete floors and slab ceilings are about 8 inches, you realize what a mass we have there, and the rubber tires are very compliant, which can be controlled by the air pressure. So the cut-off frequency there is below 2 cycles per second, and therefore can be thoroughly isolated against all solid-borne audible, and practically all sub-audible vibrations.

MINK: You were talking about the anechoic chamber here in Knudsen Hall.

KNUDSEN: That is right. It and the reverberation room are in our newest acoustical laboratories here, and I think they are better isolated than any others--that is, we have utterly no solid-borne vibration coming into these rooms. That is, persons outside or in other parts of Knudsen Hall can be doing all sorts of operations, including air-hammer alterations, but we get utterly no vibration as a result of these activities. Well, I have always used these examples in teaching elementary acoustics here and elsewhere.

These examples were very useful, but I had one other that was especially interesting. It occurred during a trans-Atlantic crossing with my family in 1938. We were located in a stateroom which was not far above the motor that was driving the propeller. It was on the American Banker, which was an 8,500 ton ship. It was an eleven-day crossing on the Atlantic, New York to London's Thames River dock. That ship and all her sister ships--the American Farmer, the American Exporter, the American Importer--every one of them was sunk in World War II by the Nazi U-boats; that's something I learned later, but we'll discuss that when we take up my subsurface warfare research. Because our stateroom was located above the engine that was driving the propellers, we could hear and feel the propeller beat. My berth especially, I recognized, was tuned very closely to the beat frequency of the propeller. When I was lying in the bed, I could feel myself jiggling up and down this way. I said, "I'd be much better off if I'd pull this mattress out and lie on it on the floor," and I did.

So I made vibration measurements there and I often refer to them in my lectures. My son Robert was old enough to help me make the measurements. You could actually measure the amplitude of my vibration with a ruler, this way, which I shall demonstrate. Robert would sight across my stomach onto the ruler which had one-eighth markings, as I was oscillating up and down. He noted that the amplitude was of the order of an eighth of an inch when I was lying down in my berth. And then we repeated the measurements when I was lying on the mattress on the floor. The amplitude was only about one-third as large as it was in the berth. So I suspected that the resonance frequency of my weight on the springs was nearly coincident with the blade-tip frequency of this propeller, which is the number of times the propeller rotates per second times the number of blades. "Well," I said to Robert, "let's go down to the engine room and find out just what the blade-tip frequency is." And so we went down, and sure enough, my mass on the mattress with the spring system that they used in the berth was just right to make my resonance frequency in the berth the blade-tip frequency of the propeller that was driving the ship. And so this experience furnished another good example of how to isolate against vibration. Designers of ships and many others should benefit from this experience.

MINK: Did you do anything with the people on the ship? Did you talk to them about it?

KNUDSEN: Oh, I told the captain about it. He was much interested in it and he said he'd report it to the owner.

MINK: Did they do anything about it?

KNUDSEN: I don't know. Presumably they didn't do anything about it. But this blunder could happen, you know, on many other ships, and many other situations where people or things are bothered by vibration.

SECTION XX

KNUDSEN: The early work that I did here is referred to in an article that I don't believe I have discussed with you. I think we should probably at least get it in the record, because it deals with the early research and consulting work I was doing, and I think we might refer to it; the article is in the California Monthly.

MINK: A UC alumni publication.

KNUDSEN: Yes. The entire UC Alumni Association at that time, and the article to which I refer, is in the November, 1927 issue. It begins on page 171 and then it continues on 198 and 199. It discusses first of all the work I did on hearing with the sense of touch. I think we discussed that somewhat. And then it discusses the work on architectural acoustics and how it was spreading out. It was the beginning of the work I did with Metro-Goldwyn-Mayer. But this certainly had a bearing upon my reaching out into other fields of acoustical designing. And I was soon helping to design acoustically a large variety of buildings, and we might mention a few of these important ones at this stage.

For example, besides the motion picture studios, I was brought in on the design of a number of radio broadcasting studios, the chief one of which was the one in Hollywood here, the KNX-CBS Hollywood studios, and that project introduced a number of special acoustical features, one of which was the use of walls that inclined inward. And I know when visitors are taken to KNX they refer to the unusually fine acoustics that were attained there. One of the new features that was used in the studio design was the use of these inclined walls and various devices to give a uniform decay of sound in the horizontal directions as well as the vertical directions so that the various resonances would be properly controlled as well as the reverberation. Of course, in the design of these studios the matter of sound insulation is very important, and so the use of a room inside of a room is very often used when high sound insulation is required, and that expedient was used in the KNX studios at Hollywood.

Among music buildings, I think I've referred already to the University of Washington Music Building in which we had an unusual room for variable control of reverberation. Two that are rather close at hand here to the University of California are our own Schoenberg Hall and the Hertz Memorial Hall of Music, at Berkeley. The Schoenberg Hall probably was one of the early designs in which we made a study by optical models. This made use of a device that had been developed by the late professor Robert Leonard, who died very recently (1967). The shape of Schoenberg Hall, particularly the ceiling--if you'll recall its shape--was investigated by using an optical model. Light, which consists of very short wave lengths, can be used to reveal how high-frequency sound waves will be reflected by the boundaries of an enclosure, which is important for concert halls and speech rooms. And I think Schoenberg was one of the first halls to be examined very carefully in advance of construction by the optical model technique developed by Dr. Leonard, and which we use routinely in our acoustical designing of theaters, music halls, and multipurpose auditoriums.

Leonard and Delsasso and I worked together on Schoenberg, and later Dr. Delsasso made many measurements in Schoenberg Hall, for which the outcome in acoustics was highly satisfactory. One of the outstanding characteristics acoustically of Schoenberg is the uniformity of sound level and reverberation in all parts of the hall. For example, I know when we measured the loudness of sound, the intensity of sound, from the front row to the rear of the hall, there was only one decibel drop from the front to the rear. And the people I know who have been in Schoenberg Hall say it doesn't matter where you sit, you hear equally well in all places.

MINK: It has very little reverberation.

KNUDSEN: It's on the low side for reverberation for music. You see, it was intended at the time as a multipurpose hall, and it has been used for drama and lectures; and as you well know it serves all sorts of purposes besides music. If it had been designed for music alone we would have made it somewhat more reverberant than its present 1.2 seconds, but not more than 1.5 seconds. The one thing Schoenberg Hall needs, and which we have proposed, and for which we have an acoustical model, is an acoustical shell for the stage. The model is one twenty-fourth of full size The acoustical conditions in Schoenberg Hall would be improved for music if we had a shell of the type we have in our collection of acoustical models. It incorporates a number of features that we are utilizing in the design of acoustical shells for multipurpose auditoriums. A new feature existed which had not yet been used. We wanted to try it out on Schoenberg Hall before we advocated it for other places. The feature that has not been tried out anyplace, but which we have in our model, consists of series of convex ceiling splays that have their low point directly over the front central part of the stage--that is, where the soloist would be located or where the first chair people would be located in a symphony or any ensemble of that sort on stage. This means that the sound which is reflected from the solo instrument (or voice) or the first chair people will be reflected to the audience such that it arrives there ahead of the sound that originates any other place on the stage.

Now the ears are of such a nature that they give an undue prominence to the sound which arrives first. And therefore this feature would enhance the quality of the sound coming from the solo instruments or from the first chair instruments. And it's an acoustical feature of the music shell that we hope yet to have installed in Schoenberg Hall. I've assured the music people that it would be advantageous. Drs. Delsasso and Leonard and I have given considerable time and thought to the design of this shell, and we believe it has acoustical merit. The music department has indicated that they hope to get an appropriation that will make it possible to have this shell added to Schoenberg Hall.

MINK: Did you not tell me that this was one thing that was done for the Lincoln Center Philharmonic Hall, at least the stage was enclosed at the Lincoln Center, in a shell? Did you say a shell?

KNUDSEN: Yes, essentially a shell, but not a movable one.

MINK: Was that somewhat along the same lines as here?

KNUDSEN: Well, yes. I frankly had little to do with the final design of the stage enclosure. Lincoln Center initially did have a number of large sound reflectors of odd shape on the sides and on the back of the stage. But these reflectors were by no means a complete enclosure or shell. They have since designed a shell, a permanent enclosure, which was designed largely by a German by the name of Keilholtz, so I don't claim credit for the design of that shell, but it does incorporate certain features--certainly not this ceiling feature that I have spoken about here. But it uses a large number of diffusive and reflective surfaces for the walls of the enclosure, and that I am sure has contributed to the better quality of sound we now have in the Philharmonic Hall in New York.

MINK: As a matter of course, you just are consulted on the acoustics of all buildings that are built here. How was it, for example, in Schoenberg Hall? How did that work? When the building was planned and budgeted?

KNUDSEN: Yes. Our work here was not with the architect directly--with Welton Becket who was also the supervising architect--but largely it was with our campus Office of Architects and Engineers.

MINK: They came to you, or you went to them?

KNUDSEN: Well, it was all on campus here, so the conferences were all here, and Drs. Delsasso and Leonard had this optical model work going on in our laboratory here.

MINK: No, I mean did you go to them and say let us do this, or did they come to you?

KNUDSEN: They came to us.

MINK: And said, "Would you do this?"

KNUDSEN: Yes.

MINK: And then this was presented to the architect as a fait accompli, and he had to build the hall around that?

KNUDSEN: Well, the dealings between the architect, Welton Becket, and the university was through our Office of Architects and Engineers. I don't remember that we had any conferences with Welton Becket's office on this project. But toward the end we had a problem that involved, I know, someone from Welton Becket's office who was out here, because the air conditioning system was noisier than we had specified it should be. We had indicated very clearly that the contractor must provide a certain control of noise especially from the air conditioning system. And the noise measurements that we made after completion of the building indicated that this condition had not been met.

We worked with Becket's representative and others four or five months, repeatedly making measurements of the noise in Schoenberg Hall. They made adjustments, some of which we recommended, and finally the noise level was brought down to what it is now, which is entirely satisfactory.

MINK: Now you spoke earlier about your work in controlling noise in ventilating systems. Is this still followed, I mean this same technique?

KNUDSEN: Yes, very much. The main difference is that there has been more use of what we call acoustical filters, usually, or "attenuators" they're often called--"sound attenuators" that are units that can be inserted in the duct. The duct is built oversize, and this unit is a somewhat more convenient way of handling the problem than lining the ducts. Often they--and lining of certain ducts--are used in combination. In some projects I worked on, important music buildings, for example, concert halls or multipurpose auditoriums that serve for symphonic purposes, that's the most critical problem of all. You must have for music the background noise so low that when, for example, a violin, say, is bowed down to inaudibility, you want to hear it to inaudibility, and not until it is masked by some adventitious noise in the auditorium; and frequently that does come from the air conditioning system.

One of the things we always do now in the completion of an auditorium is to test for any possible noise and make specific recommendations for such changes as may be necessary in the air conditioning system. Sometimes, it's at the fan and the motor, and the appliances in the air conditioning room itself, pumps and things of that sort, you have to be isolated against solid-borne sound as well as air-borne sound, and then you have to take care of the sound that's transmitted through the ducts. That's the most usual offender, and this can be controlled by the methods I mentioned last time that we used at Metro-Goldwyn-Mayer sound studios. They have become standard practice now.

The handbooks for heating and ventilating engineers have very definite directions on how to calculate just what's needed in various duct systems. You know how much noise there is at the origin. The manufacturers of fans, motors, and so on, have given sound ratings, noise ratings, to their facilities; and knowing what the noise level is at the source and what you can tolerate in the auditorium, you have to provide sound attenuation or reduction of noise to meet that condition, and this is something that has to be done especially for low frequencies, but to a lesser extent (usually) for medium frequency and high frequency sounds. It has become fairly standard practice; and we specify very high standards in the matter of noise reduction, especially where music is concerned.

MINK: Well now, at the dedication of Schoenberg Hall, what was the opinion of the auditorium?

KNUDSEN: Well, we had very favorable reports. I was invited to be the main speaker on the occasion of the dedication of Schoenberg Hall. I think maybe we should dictate into the record a letter I have from Gertrud Schoenberg, the widow of Arnold Schoenberg, on the occasion of the dedication. The reviews were very favorable. I mentioned how uniform the hearing conditions are in various parts of the auditorium, and the university people were so pleased with it that the Berkeley people wanted us to help them with the design of Hertz Hall. But let me refer to this letter.

I think I'd like to dictate into the record here a letter I received from Gertrud Schoenberg, the widow, dated May 20, 1956, which followed immediately the dedication of Arnold Schoenberg Hall, and the letter reads as follows:

> Dear Dr. Knudsen, I hesitated so long to tell you how wonderful you spoke about Arnold Schoenberg, because I did not want to use

commonplace superlatives which are so meaningless because [they are] used simultaneously for a great man and Kellogg's breakfast foods. But I want you to know how grateful I am to you, to a man who can give honor because he himself deserves the same. It was not easy for me to go to this concert, but you and your speech made it what I wished for, a sincere recognition from a man who knew Schoenberg's work before he knew the man, and later became a friend. May I thank you in a manner I feel Arnold Schoenberg would have liked to do it, giving you one of his self-portraits. With kindest regards and best wishes to you and the family. Sincerely yours, Gertrud Schoenberg.

And this self-portrait of Arnold Schoenberg was painted when he was thirty-nine years old. And some of my artist friends said, "Well, you know, someday, Dr. Knudsen, this painting may be as famous as his music." And it is a striking portrait, I think, as you will recognize, as you view it here on my office wall.*

MINK: Was this likely to have been a statement of perhaps not so much approval of the music? There has

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*Note added 10/22/70. A year ago my portrait by Ferdinand Earle was stolen from the lobby of Knudsen Hall. Accordingly, I have moved Schoenberg's self-portrait for safekeeping.

been a great deal of debate about Schoenberg's works.

KNUDSEN: Yes. Yes, of course. Of course. That's unfortunate, I think, that we have not had more of his music here on this campus. We did have his Four String Quartets which were performed here in Royce Hall and which I admired very much; I especially enjoyed his Fourth, the most modern and complicated, because I heard enough at the Kolisch Quartet rehearsals to know what it meant. Also, I'd heard Schoenberg describe in my class in acoustics what his techniques were in composition. And when you know how well Schoenberg knows harmony--that is, he wrote this book, you know, Harmonielehre, which became the standard work really throughout the world on the nature of harmony--you have a high respect for him. The "harmony" in his Fourth Quartet may sound dissonant to most ears of our generation, but it has the ordered intimacy of a beautiful cathedral.

MINK: When did you first meet him?

KNUDSEN: When he came to this campus. But I knew of him before then. This is a rather interesting incident, if we can digress a moment. There was a vacancy in the Music Department and we were looking for a man in composition. I was chairman of the faculty committee that was engaged in a search, and I had interviewed some of the leading possible composers in the country, [Roger] Sessions, [David] Diamond, and others were coming up at that time. I was in New York interviewing some of these people at a time when I received a letter from my wife Florence. She had just returned from a gathering in Los Angeles that was largely a Schoenberg club. They were a group of devotees of his. Maurice Zam, the pianist, was talking with Mrs. Knudsen that night and was asking where I was. And she said, "Well, he's out in New York just now interviewing some possible candidates for appointment as professor of music in our department here." And he said, "My God! Why does he go to New York? Arnold Schoenberg is right here in Los Angeles over at USC." And I received this letter which listed the qualifications of Schoenberg, and I took up the matter with Ernest Carroll Moore at once. So Ernest Carroll Moore began negotiations with Arnold Schoenberg and USC--you know, you have to deal at the top when you steal a man from a neighboring institution that way. Well, the result was that Schoenberg came to UCLA at that time. I became acquainted with him quite soon because Otto Klemperer was in Los Angeles at that time, and he was a great admirer of Schoenberg, and I knew Klemperer both professionally and socially.

MINK: He was the conductor of the Los Angeles Philharmonic.

KNUDSEN: He was the conductor of the Los Angeles Philharmonic, and he lived out here near the university. And there was a very close relationship between Klemperer and the university--that is, he was much interested in the university and our Royce Hall. Among other things he rehearsed the chorus and the Los Angeles Philharmonic Orchestra in Royce Hall for a magnificent performance of Beethoven's Ninth Symphony. Richard Lert had strictly trained the chorus, but Klemperer finished the training, and this brought him close to Royce to perform the Ninth Symphony and also to me because I worked with Klemperer to provide a suitable acoustical environment for the large chorus and orchestra, which required an extension of the stage floor over the pit. This was at about the time Schoenberg came here. Schoenberg had been commissioned by Mrs. Elizabeth Sprague Coolidge to compose the Four String Quartets. And the distinguished Kolisch Quartet--Kolisch, you know, is a brother of Mrs. Gertrud Schoenberg--had been engaged to give the world premiere of Schoenberg's Fourth in a series of four quartet concerts in Royce--at each of which there were performed a Beethoven and a Schoenberg quartet. Kolisch and his partners were trying to learn the Four String Quartets of Schoenberg, and they were having a desperate time, insisting that it was impossibly difficult to play. Klemperer and Schoenberg both had to encourage the Kolisch Quartet to go on and on in their rehearsals to actually master this thing, and it was a heroic thing. In the process, I attended many of the rehearsals because I was then helping Klemperer provide the suitable stage extension and enclosure for the Beethoven Ninth. It was the first time anything that big had been done here.

MINK: This was in Royce?

KNUDSEN: Yes. And the rehearsals were taking place there, and this afforded an opportunity for me to become acquainted also with Otto Klemperer. This was really an extraordinary experience for me because, you know, today Otto Klemperer is regarded as one of the greatest of living conductors. He had an illness here, a brain tumor, that practically paralyzed him, and nearly everybody here thought that he was finished. But even with his infirmity, which confines him in his wheel chair, he's one of the most sought-after conductors in all Europe today. After Herbert Von Karajan you couldn't name more than one or two other conductors that would be rated the equal of Klemperer. Well, he was and is a great conductor. He insisted upon hearing the music from all over the auditorium. And so when Klemperer was conducting, part of the time he was walking around in the auditorium, and I had occasion to walk around with him during that time. We went upstairs. We listened everywhere. Usually he wanted to listen everywhere. Sometimes he would conduct, when they could see him there. They'd sometimes have a light on him so that he could be better seen from the stage; sometimes an associate would conduct for him. But he wanted to hear the music from all locations, which is important, because the sound isn't the same standing at the podium as it is out in the auditorium, and in many auditoriums the sound is very dependent on location.

MINK: What did he think of Royce Hall?

KNUDSEN: He liked it. It is good for music.

MINK: Did he compare it to the (old Los Angeles) Philharmonic Auditorium?

KNUDSEN: I don't remember that he did. It was somewhat smaller than the Philharmonic Auditorium, and therefore more nearly the size of the European halls that he was accustomed to. I think most European conductors feel that most of our halls are too large; so Royce was pleasing to him, one, because it was somewhat smaller, and two, it had a high ceiling, and this had a number of the features of European halls. Furthermore, the big coffers in Royce Hall are very good for introducing diffusion of sound from the ceiling, and that is advantageous.

The principal acoustical difficulty in Royce Hall is a reflection that comes from the rear wall. I think I've mentioned that already. It's concave toward the stage, and that gives a convergent reflection, and so you get an echo in the front seats and on the stage. This could be corrected, and we have indicated what could be done to correct it. We've had to do that in quite a number of auditoriums. And the sound system in Royce needs renovation. It's a fairly old sound system, and they should have a more modernized sound system, especially for drama. And it's largely in drama that you have difficulty. You don't have difficulty in hearing lectures. If they speak into a microphone from a distance of a foot or so, the intelligibility of the speech is very good for all seats. But when it's set up for drama, conditions can get pretty bad. This is a very exacting requirement on the sound system, and it needs to be improved in Royce.

MINK: Well, was Schoenberg relatively well received here?

KNUDSEN: Yes, I think certainly by the musical community, but not by all his colleagues. He had his problems with the department because every European professor is <u>the</u> professor, and he couldn't understand the multiplicity of professors in one department. He felt, as was customary in most European universities, that because he was professor he had direct access, of course, to the head of the university, and some of the problems that developed here occurred largely because he was irritated that he had to go through the department and dean, for example, for many things. I being dean of the graduate division, he often felt that I could solve these problems, and I did often go directly to Dykstra--Dykstra was here during much of that time--or to others to help smooth the way for Schoenberg.

MINK: How long was he here?

KNUDSEN: I believe seven years. He died, I believe in 1951--could it be that long ago? Yes, I am quite sure, 1951.

MINK: And do you think as a result of his having been here that the quality of the music department was increased and that he added to the stature of the music department?

KNUDSEN: I couldn't say "yes" to all of that, because he was a controversial figure. There is an undue amount of jealousy within departments, and during much of his career, it was greater in the music department than in almost any other department here, and it was not easy for Schoenberg, as later it was not easy for Jascha Heifetz to feel comfortable in the department.

This I think is understandable, especially in a man like Schoenberg, because he was an exceptionally strong-willed person, and he was determined to have his way, and his way was not always the way of an American university. That made it especially difficult for him and also difficult for the department. And, of course, he was a giant in the world of modern music--often named as the "father" of modern music. I am convinced that it will be to the eternal glory of UCLA that he was here for seven years.

MINK: During the time he was here did you see him socially?

KNUDSEN: Yes. During the time he was here there was a close relationship, as I mentioned, involving socially the Schoenbergs, the Klemperers, and a professor in the German department, Rolf Hoffmann, who, with his wife, often hosted Sunday evening parties, at several of which we met distinguished visiting musicians and other artistic and literary persons. I don't know whether Hoffmann is known to you, but he knew the artistic people over here, and also the Gustave Arlts. Dr. Arlt was chairman of the Germanic languages department--later, he succeeded me as dean of the Graduate Division. These names come to mind. There were the Richard Lerts from Pasadena and Maurice Zams--I think he had a wife at that time--and a few other music people. But when important musicians or even other artists came to Los Angeles, there was usually a gathering at the Schoenberg home or at the Hoffmann home, entertaining these guests. We often heard very fine music at gatherings of this sort, more at the Schoenberg home than at any other place. So the Schoenberg home was really a gathering place for fine artists besides musicians that came to Los Angeles. And this was at the time Klemperer was here, and we'd often meet Klemperer and Schoenberg together with acquaintances of theirs from other parts of the world, and we'd have an unusually joyous and cultural evening.

MINK: Now you were saying that as a result of the success of Schoenberg Hall you were asked to design Hertz Hall.

KNUDSEN: Well, the acoustics of Hertz Hall.

MINK: Who approached you?

KNUDSEN: This again was the Office of Architects and Engineers, but this time at Berkeley; also I did meet the architect, Gardner A. Dailey. He was a very famous architect in the Bay area--one of the very best. Here again Delsasso and I worked together very closely on the design of Hertz Hall, and one of the principal differences between Hertz Hall and Schoenberg Hall is that Hertz does not have a proscenium. It's a true concert hall--that is, you have the platform more in the European tradition, and it has a pipe organ which forms really the back wall of the stage. So initially there was planned and actually constructed a wood screen that could close off the organ when you wanted it, but the organ is so beautifully and so well integrated with the architectural design of Hertz Hall that they almost never close this screen.

MINK: This organ was designed by Walter Holtcamp.

KNUDSEN: That's right. It's a Walter Holtcamp organ. You know the organ? You know the hall?

MINK: The university organist. And I wanted to ask if you knew him, Lawrence Moe?

KNUDSEN: Yes, I have met him.

MINK: Moe came from MIT to become the university organist at Berkeley. Did you meet him and talk to him?

KNUDSEN: Yes. I met him. I don't know him well. I think I met him first, I believe, in connection with Hertz Hall, and later Zellerbach Hall. I mentioned the essential difference between Schoenberg and Hertz. Hertz Hall is more a concert hall and is not intended as a multipurpose hall, and consequently we have more reverberation, whereas in Schoenberg Hall with a capacity audience the reverberation time is about 1.2 or 1.3 seconds. In Hertz Hall it's more like 1.6 seconds. And this makes it a little more live for music, and it's definitely a better music hall. Furthermore, when you have a proscenium you have a divided hall somewhat. The sound originates in one space and it is transmitted to the other, whereas, in a concert hall such as Hertz you have a one-space hall, and this is an advantage. There's a little closer bond between the listeners and the performers in this type of hall.

We had the same optical facilities for investigating the shape of Hertz Hall. But the one thing that we wanted there was proper control of reverberation as a function of frequency, and this was attained by the use of vertical wood strips for the walls, backed with either reflective or absorptive panels. If you have seen the interior--the side walls of Hertz Hall--what you see is largely wood strips. They are pretty bold wood strips. I think they're two inches by three inches that have been trimmed to that size, two inches wide and three inches in depth, but tapered a little so that they're narrower in the front than they are at the rear, with spaces between these wood strips that had been determined carefully by experiments on panels of the material that were conducted in our reverberation room. It was then of course the old reverberation room in Kinsey Hall.

But we conducted tests of panels that were made first of all of a two-inch layer of mineral wool, which is very absorptive material. But when that is covered with wood strips, the absorption characteristics--that is, how much sound is absorbed for low-pitch sound, for middle-pitch sound, and for high-pitch sound, is a function of the depth and the spacing of these wood strips and the size of the strips themselves. So we made a series of tests with different spacings. The size of the strips was desirable, for visual reasons; the architect wanted that size. But we had freedom in the spacing. The narrower the openings between the separate strips the less sound absorption you have at high frequencies. That is, you've covered up most of the absorptive materials for high frequencies, but you have not lost appreciable absorption for low-pitch sounds.

So this provided a means of getting proper control of reverberation throughout the entire audible range of frequencies that make up musical composition. This spacing was on the average, I think, of the order of three-quarters of an inch, but it's not uniform. People may not have noticed that, but the spacings are not uniform. If they're uniform, the strips become very much like an optical diffraction grating, and you get certain frequencies reflected into a diffraction pattern. It's a picket fence effect that would tend to reinforce certain frequencies and cancel other frequencies. But by having spacings vary, say, from one-half inch, the narrowest, up to one inch, with the average at three-quarters of an inch--that's approximately the spacing that we have in Hertz Hall up there--you get rid of this diffraction effect, and you get a more uniform absorption characteristic of these panels. That I would say was the principal innovation in the acoustical design of Hertz Hall. And designing for music more than for speech it enabled us to get a somewhat better musical hall than we had for anything I think I had worked on up to that date.

MINK: And were you there at the dedication of Hertz Hall?

KNUDSEN: I was not there at the dedication. I was there later. I have been there on a number of occasions. But a very high tribute was paid to the acoustics of the hall at the dedication, and I know Clark Kerr later sent me a very complimentary letter. I also had a very complimentary letter or oral communication from the chairman of the Board of Regents at that time, Donald McLaughlin, and so I did have these. McLaughlin said, "I don't know why you weren't there at the dedication." But I don't believe I was invited. I don't consider that a slight. It was just one of the inevitable things that sometimes happen.

That is, after all, I wasn't on the Berkeley campus. The architect undoubtedly was there and represented the designers; there were many other things besides acoustics in this Hertz Hall.

MINK: Was there a dichotomy between Los Angeles and Berkeley?

KNUDSEN: [laughter] I don't think so. I have always had very pleasant relations with Berkeley. When we come to the graduate division program here I think we'll have more to say about that, because we owe a great deal to Berkeley. And I'm among those who feel that we've gained much more than we've lost from our association with Berkeley. We're very fortunate to have had this association with her. Initially, it was a mother-daughter relationship. It's grown to one of sisterhood, and that's as it should be. But throughout the years this growing up of UCLA owes much to what we got from Berkeley.

MINK: Well, what other auditoriums did you wish to speak about?

KNUDSEN: Well, I might mention a few buildings that indicate the kind of things we deal with. Other music buildings I have helped design are at the University of Washington, Arizona State University, the University of Montana, at Orange Coast--the one at Orange Coast was with Richard J. Neutra. He was the architect for that.

MINK: Was this your first encounter with Mr. Neutra?

KNUDSEN: This was my first consulting work with Richard Neutra. The others for which I have consulted with Neutra were not as important as the one at Orange Coast. One at the University of New Mexico, but largely the library there, and this was largely a control of reverberation for the library and a few classrooms there, and some music practice rooms. But Orange Coast was an interesting project largely because the stage was designed in such a way that it could be used for exterior use as well as interior use, and so we had to have an acoustical shell that could be oriented in such a way that it could accommodate an audience outside as well as one inside the auditorium. I know this Orange Coast project was featured in a volume I have. It is not here. I think it was published in Rome. You see, they don't use the word "auditorium." The root word is "specular." It's a place for seeing. We say "auditorium," and they say the "speculum" or "specula." You could supply the exact word that they use. And the Orange Coast school auditorium is one of not many American examples included in this international <u>Speculum</u> book. There are probably a dozen altogether, and this one by Richard Neutra was the one singled out that he had designed here in the United States.

MINK: How was it you got involved in this with Neutra?

KNUDSEN: Neutra came to me.

MINK: He did?

KNUDSEN: Yes. Well, this was just after World War II, so this was not one of the earliest auditoriums I had worked on. I think I had just returned from war research, and Neutra sought my help on this project. We had several conferences. I made my usual acoustical studies of his plans, and then made specific recommendations. The shape was modified somewhat to make it a little longer and the ceiling a little higher than initially had been planned. But it was not intended to be the last word in a concert hall. We didn't have that much money. It was a multipurpose auditorium. But the reverberation was properly controlled, the noise was properly reduced, and the shape was free from faults and quite satisfactory. It would have been desirable to have had a still higher ceiling and a larger volume for music, but after all it was to be used for all activities that you would normally have on a campus and its neighboring community. But architecturally it is regarded as one of the good works of Richard Neutra.

MINK: Was this your first encounter with Richard Neutra?

KNUDSEN: It was the first consulting work I had done with him. I had met Richard Neutra on a number of occasions. He has lectured on campus. I'm not sure it was before or after that that I had occasion to introduce him when he spoke on his architecture here at the UCLA campus. But we had met the Neutras socially at their home and at our home. Not frequently, but I came to know him quite well, and in 1960, after my retirement, I worked with him even more closely on a project that was discontinued before it was completed; it was a multipurpose auditorium for Reno, Nevada, and we had devised some novel acoustical features.

MINK: What happened?

KNUDSEN: Well, I was told this was a conflict between those who wanted casinos and those who wanted a really multipurpose auditorium that would serve the usual cultural arts, and especially it was to be suitable as a music hall. This project was probably about the first one I was free to work on after I retired as chancellor. I remember that Neutra and two or three of his associates came out here several times during our discussions; also, once, someone came from Reno. We worked on the design a great deal. We were planning to use a transondent screen for the ceiling, a screen that also would look good. That is, it was to be opaque optically but transparent to sound, so that we could do anything we needed in the space above this ceiling. This gave us an opportunity to have variable control of reverberation without changing the appearance of the hall.

This feature was planned for that auditorium, and it's now planned for at least three projects that I'm working on now at the present time. One is for San Jose--a multipurpose auditorium that will serve as the home for their symphony orchestra, and another is for the University of Akron. Both of these would utilize this feature for the walls rather than for the ceiling. But you'd have, back of the transondent screens, facilities for varying absorptive and reflective materials, largely by a system that operates much like roller blinds on a window; so that the absorptive material rolls up or down. When it's rolled up, you have a hard reflective surface, hard plaster, concrete or something of that sort. When it's down, it's a very thick blanket of cotton material that we have tested for absorption in our laboratory here. And if you have an adequate air space behind these absorptive curtains, something like eighteen inches, you can get a good control of the absorption as a function of frequency. And so this offers a means of controlling the reverberation, and these are on rollers, say, four, six, or even nine feet wide, and you can have any number of them down, which would then furnish vertical panels of absorptive material with reflective materials between them. And besides furnishing different amounts and kinds of absorption, they also offer a means of getting diffusion.

The screen then is a wire mesh--that is, the mesh is close enough that you have a uniform appearance. As I say, it looks almost like a continuous solid material, a nonperforated material, and yet it's not as fine as fly screen; but it's a little finer than hardware cloth which has a quarter-inch or eighth-inch mesh. It's intermediate between these, and it's a decorative material. There are quite a few of these on the market now that can be used for acoustical purposes where you want to change things behind what you see.

Well, those are among the music buildings that I worked on, and there are several on the boards at the present time that I am working on in Florida, in Kansas--there are two in Kansas, in Wichita and Topeka, the Kennedy Center for the Performing Arts. I'm not consultant to the architect for the Kennedy Center, but I'm consultant to the General Services Administration there. Then there are several projects for the Taliesin and Associated Architects, the successors to Frank Lloyd Wright. I was brought in on some very interesting and important multipurpose auditoriums because of the success we had with the Grady Gammage Auditorium at Arizona State that I will speak about a little later.

I'd like to mention a few of some other completed projects to give the scope of the kind of things we work on. Among multipurpose concert halls and municipal auditoriums, there's a municipal auditorium at Portersville; there's the convention cultural centers at Las Vegas, at Honolulu, at Fresno, at Bakersfield, at Anaheim, and at the Seattle Opera House, which is a multipurpose hall; and at the Dorothy Chandler Music Pavilion in Los Angeles downtown. There are three multipurpose auditoriums nearing completion in California, at Glendale, Oxnard, and at Downey. These are smaller auditoriums. Two of them are to be used as concert halls, which, among other uses, will house the Oxnard Symphony and the Glendale Symphony. These two are low-cost auditoriums, but good examples that numerous communities throughout our country could well emulate. The one in Downey started out as a children's theater, but it is now called the Downey Theater. And these are some of the more interesting of recent auditoriums for which I have served as acoustical consultant. Since 1922 I have consulted on more than 500 building projects, including a wide range of auditoriums; music and school buildings; commercial and public buildings; churches, hotels and apartments; radio, T.V. and sound-recording studios.

Only this morning I was called from Houston, Texas, to find out if I would be willing to help them with the design of a multipurpose auditorium for the University of Akron, and I've agreed to help them. That's one of the newest projects, which promises to be very interesting. There are some other recently initiated projects: at Arizona State University, a new music building; and similar projects at the University of Arizona, and at Northern Arizona University. There are large coliseums; we did some work on the Pauley Pavilion here; I think we've referred to that. A similar one (with some 13,000 seats) is a sports arena--a large coliseum-type, multipurpose enclosure, but for sports more than anything else, at Phoenix. It seats something like 13,000. It's about the same size as Pauley, but its shape is more nearly oval (rounded at its two ends), and chiefly for this reason not as good acoustically as Pauley.

And at Phoenix also there's an unusual structure that's similar to a coliseum. It's a race course for night performances; it is used year round, even in the wintertime, and therefore had to be enclosed and air-conditioned. And so there's a glass enclosure facing the track, and this presented some new problems. There are some restaurants and other fancy places in it for the high-priced tickets. So this presented some new problems. There was the Cow Palace at San Francisco. This will give you some notion of the range of multipurpose auditoriums and other projects that have occupied much of my spare time.

Among residences, there are two I have worked on that we have not mentioned. One was the Isaac H. Jones residence. I have talked at some length about our research. He explained to me that he wanted a jewel box for his jewels, and his jewels consisted of a famous old piano, and a new pipe organ. The piano was a Beckstein, as I recall, and it was a family possession that had been handed down to him from his father and grandfather. It had been in this country a long, long time. It was a beautiful instrument, acoustically as well as visually. And then he had this pipe organ on order that he planned to house in a new home. And so I helped Isaac Jones with this interesting problem, and he was greatly pleased with the outcome. This was the time we were working together; we made many visits together to his home, during both the planning and construction of this splended home.
It was referred to as the Stone House; it's up near Griffith Park. It is very near the Cecil B. DeMille home and not far from Griffith Park.

And another home I helped design, but which I recently learned was not completed, consisted primarily of an interesting music room. The building was to occupy a beautiful hillside lot facing the ocean. The owner I don't remember now, but he has two very famous harpsichords that are among the finest in the world. They were made specially in Germany, and he has these and a very fine piano. The living room was to be used as a recording studio as well. The owner, who has lectured at UCLA, was interested in high quality recording, especially for his two harpsichords. And so we devised variable control of reverberation in this room, and also other facilities for making it acoustically ideal, as a living room, as a music room, and as a recording studio for different kinds of music.

Then there have been some hotels and casinos that I have worked on that involved acoustical problems, nightclubs at Las Vegas, especially the Tropicana, the Riviera, the Flamingo, and the big hotel nightclub at Tahoe--the name I don't remember--the Ahwahanee Hotel at Yosemite, and some parts (guest rooms and the main ballroom) of the Beverly Hilton Hotel in Beverly Hills. These examples will further indicate the range of buildings for which I have consulted.

Court buildings involve some interesting and important acoustical problems. I was acoustical consultant for a large court building at Bakersfield; and I am currently helping Adrian Wilson Associates, architects, on the Los Angeles County Criminal Court Building. There is one at Tulare County, where I was called in after it was completed. They were in trouble because there had been a misuse of acoustical plaster, and so it was largely a matter of getting the right kind of reverberation in these court rooms. A court building at Eugene, Oregon, had some interesting innovations and changes somewhat in the arrangement of where the jury, the attorneys, the judge, and the public seating would be located.

There are court buildings under construction at the present time. One is the Marin County Courthouse, which is part of the Frank Lloyd Wright design there--that is, before Frank Lloyd Wright died, he and his organization had planned a large municipal center for Marin County, including also a Veterans Memorial Auditorium, fair grounds, and the courthouse. But the courthouse is an extraordinary building, which may well become another great monument to the genius of Frank Lloyd Wright. It is a large project that will serve most functions of Marin County. I am currently working on the project with the Taliesin Associated Architects and the associate architect Aaron Green, who was a Frank Lloyd Wright-trained architect, and was architects. He was in the class of Richard Neutra and almost Frank Lloyd Wright. He was one of the most prominent architects in Vienna and Berlin before coming to the United States. He was the architect for a hospital that is in the news today because of the Israeli-Arab war that broke out yesterday [5 June 1967]. Mendelsohn designed a hospital that was in the Jordan side of Jerusalem. And it was never finished because of the partition. It just stands there. It's a monumental building--the creation of a master builder--but it's still just a monument, not a useful and much needed hospital. I don't know whether that will sometime change or not, but it had not changed the last time I was in Jerusalem, which was--oh, I guess twelve years ago now.

Then Mendelsohn designed these synagogues at Cleveland, at Minneapolis-St. Paul, and at Philadelphia. And I helped him with the acoustical design of these buildings. He's been on this campus several times. He was a very interesting, unforgettable person..

I remember the first time he came out here from the airport by taxi. I was taking him to lunch that day, and I was in the Administration Building at that time. We came from my parked automobile there and drove toward the entrance on Hilgard. When he saw the facade of the old original Economics Administration

Building, he looked at it this way, and his jaw dropped as he said, "Oh my God!" This was his reaction to the architectural design of it. He didn't like it at all. Dr. Clarence Dykstra didn't either. Dykstra always referred to it as the building that had all the gingerbread and bathroom tile decorating its facade.

MINK: Now this is the last building that was built in conformance with the original structural scheme on the campus--that is, the romanesque.

KNUDSEN: That is right. And it had quite a tower, useless, you know; you almost had to go up a ladder to get into it. But Mendelsohn didn't like that at all, and I reminded him that he had at least one other person of considerable authority around here who shared his dislike of the building, that Dykstra always referred to it as the building that has its bathroom tile on the outside.

Among prominent churches other places are the Spokane cathedral in Washington and the First Methodist Church at LaVerne. Ladd and Kelsey are the architects over there, and this has received favorable mention architecturally, not especially for its acoustics, but Ladd and Kelsey said it also was superb for its acoustics. I have never been in it since it was completed, but they say it's outstanding. There's the Fifteenth Church of Christian Scientists in Seattle, St. Luke's Church at Wichita, Texas, and oh--others scattered around the country. I'm sure there must have been twenty-five or thirty churches altogether.

Shopping centers: I've helped Victor Gruen Associates with the design of several shopping centers. These became significant acoustically, because some of them are glass enclosed, you see. A very large and pioneer one was at Southdale, which serves Minneapolis and St. Paul. It is completely enclosed. And so year round they have symphony programs there inside of the glass enclosure, but outside there are other buildings and also sidewalk cafes. In January, with temperatures below zero, people can come in there and sit and enjoy the atmosphere and acoustics of a sidewalk cafe or outdoor concert, things of that sort. These enclosed shopping centers did involve some special acoustical problems especially controlling reverberation. I have assisted Victor Gruen with other centers that are enclosed--at Midtown, which is out of Chicago, and at Rochester, New York.

Then, well, there's quite a number of apartment houses. I don't think we will go into any except the Wilshire Terrace here at Wilshire Boulevard and Beverly Glen Boulevard and the Fox Plaza at San Francisco, of which the first twelve stories, I think, are for offices and the next up to twenty-six or so are for apartments. This has been more than enough to give you some idea of the extensive range of my consulting work. For much of it I have had the beneficial services of Dr. Delsasso.

MINK: I don't want to leave this subject without asking you to say something about the music center and your work on it.

KNUDSEN: You mean in Los Angeles?

MINK: Yes.

KNUDSEN: My introduction to the Los Angeles Music Center happened at the time I was attending a meeting of the regents in San Francisco, at which I was named the successor to Raymond B. Allen as chancellor for the campus. I recall receiving a phone call, from Welton Becket, architect, asking me if I would be willing to work with him on the acoustical design of this music center for Los Angeles. You can imagine how I had mixed feelings about whether I could take on the task of that. So I said I couldn't possibly take it on alone. If someone else could do most of the detailed work I would be willing to look over his shoulders and guide the design as much as I could in that respect. And he said, "Well, do you know anyone in New York?" He said, "Part of this work will be done from our New York office." And apparently that was the plan initially. I don't believe it panned out, because all the work was really done here. And I said, "Yes, a former student of mine, the co-author of Acoustical Designing in Architecture, Cyril Harris, is at Columbia University." And he said, "Well, that's fine then. We'll work out arrangements whereby you and Cyril Harris will do it." This former student of mine and I had planned to work together on it.

Before we really made any headway, Cyril Harris accepted a Fulbright appointment that took him to Japan for a year, and he felt under the circumstances he should not work on this project, because during that year much of the shape and early planning of the center would take place. So Becket called me in and said, "Well, they're not designing much of this project at New York anyway. Is there someone in Los Angeles?" And I mentioned two people that I thought could work with me on this project very well. One of them was Paul Veneklasen, whose name I've mentioned before, who also was a former student here. He didn't quite get his PhD degree; the war intervened and he never returned. But certainly he has done as much and as good work as any PhD has in this particular field, and he's one of the top four or five consultants on architectural acoustics and noise in the country. He was a member of the team that was called in to advise the Lincoln Center people on Philharmonic Hall. I think I've named that team already--a Bell Laboratory group of four headed by Manfred Schroeder, Veneklasen, Keilholtz from Hamburg, and myself.

Becket was friendly to the choice of Veneklasen, with the understanding that Veneklasen would do most of the detailed work but he would work in consultation with me and with Dr. Robert Leonard. Bob Leonard, my colleague here, did much of the early work again with the optical model in determining matters that had to do with shape, especially for the Pavilion and the Forum. And so this was a team of three of us, and these three UCLA physicists are always named as the consultants on acoustics for the Music Center.

There has been very little fanfare about the two recently opened buildings, the Ahmanson Theater which has something like 2,400 seats, and the Forum, a thrust stage theater, which has 850 seats. I have been at the Ahmanson Theater, but have not been at the Forum. The Ahmanson Theater did not turn out satisfactorily acoustically, principally because of its sound system. But the Music Pavilion has received nothing but praise. And if you read the Los Angeles Times (June 6, 1967) this morning, you may have seen Goldberg's review of the Eugene Ormandy Philadelphia Orchestra which performed two concerts there Sunday. The final paragraph of this review by Goldberg said that on both occasions, the afternoon matinee and the evening performance, Ormandy, the conductor, stepped forward and thanked the audience for their attention and so on, and then said, "I cannot close without paying tribute to the fine acoustics of this hall."

We've had similar acclaim from really all the important people who use that hall. The first words spoken at the dedicatory (symphonic) program of the Dorothy Chandler Pavilion were by Zubin Mehta, without the use of a microphone. Zubin Mehta came out and faced the audience and said, "Ladies and gentlemen: we like the acoustics." These words were of course music to my ears. I was surrounded by a number of my friends and colleagues there, who wanted me to stand up. Henry Dreyfus was among the first who said to me, "This is really one of the great ones." And music lovers from Vienna who have attended concerts here have said, "This Pavilion has acoustical qualities that would compare favorably with the Musikvereinssaal in Vienna which we normally regard as the best on the continent." So we've had unusually fine reviews about this. Isaac Stern after trying out the hall said it has what a good hall has; it makes music sound better than life.

MINK: Did Dorothy Chandler come up and thank you?

KNUDSEN: Yes, she did. She's been profuse about her appreciation of the outcome in acoustics.

MINK: Did you have any discussions with her about the hall?

KNUDSEN: Oh, many. I served in a rather dual capacity. I was not only a member of this consulting team of the three that I have mentioned, but I was also a member of the Citizen's Advisory Committee for the Music Center. Professor Fred Wight of our art department was on this committee. Henry Dreyfus was its chairman. Bill Pereira was on it, and of course Welton Becket, as were representatives from the Hollywood Bowl, the Philharmonic Orchestra, the Los Angeles Light Opera, and others. Mehta, even after he had been selected as conductor, met with the committee one or two times. So there were many conferences with the architects, county supervisors, and others. This advisory committee would hear reports on acoustics, on lighting, on seating, on color design. It really was a very effective group in guiding the Becket organization in the design, especially of the Music Pavilion, which received my special interest. An extremely low noise level; a hall of good shape, free from echoes and favored by early reflections; a specially designed acoustical shell; and an adjustable ceiling canopy in front of the stage; and the use of wood for much of the side walls: these are features that contribute to its splendid acoustics.

SECTION XXI

KNUDSEN: Following these reports on some of the buildings I have worked on, there is one auditorium I helped design that should be described in some detail, because I consider it the most important research project I've worked on in connection with the acoustical design of a multipurpose auditorium. This auditorium is of great interest for many reasons, chiefly because it's one of the last works of the great architect Frank Lloyd Wright. And the finished auditorium, which was completed in 1964, is Arizona's monument to Frank Lloyd Wright. His widow, Mrs. Olgivanna Wright, participated actively in its design and was responsible really for the color problems, both interior and exterior. Mr. Wright died in 1959, and was succeeded by William Wesley Peters, Taliesin Associated Architects, who completed Wright's monumental work. The auditorium also has received the highest acclaim acoustically of any auditorium I have ever helped design, and it's of special interest acoustically because the original sketches prepared by Frank Lloyd Wright had to do entirely with the exterior, and my work had to do with the interior. The exterior design was a modification of one that

Mr. Wright had previously worked out for Baghdad, and therefore, architecturally, it retained some influences of Middle East or Iraqui architecture; and I think anyone who looks at it will recognize the Near East influence on the exterior.

Well, acoustically this is interesting because the exterior shape is almost an impossible acoustical shape. The design worked out by Frank Lloyd Wright consisted geometrically of really two intersecting cylinders, a large one for the audience space and a smaller one for the stage. Well, any acoustician worth his salt would know that this is an impossible shape. If you were standing inside of a very large rain barrel, you would understand the kind of problems you would have. The concave surfaces are, of course, concave acoustical mirrors, and they reflect sound to a focus, just as a concave mirror will bring light to a focus. And so you'd have all sorts of foci all over the hall. You'd have echoes from the rear of the hall, and you'd have echoes from the ceiling, if it were domed. It would have been really a horrible shape to handle acoustically.

About all you could do in such a job is what they had to do in the Arnold Beckman Auditorium over at Cal Tech. That also is a circular design, and they had to treat the walls and the ceiling with about two or three inches of mineral wool in order to get rid of the objectionable reflections from such a shaped auditorium. The result is that it sounds like a morgue for live music. It's all right for speaking into the microphone. You would have difficulties of that sort in this Grady Gammage Auditorium if the interior shape conformed with the exterior shape.

This project was of interest to me for another reason, namely, that I was retained not by the architect to help him with the design but rather by the owner, Arizona State University. The auditorium to which I refer is named the Grady Gammage Memorial Auditorium because Grady Gammage was for many years, prior to his death in 1959, president of Arizona State University, and also closely associated with Frank Lloyd Wright--that is, they talked together a great deal about an auditorium for the Arizona State campus. Gammage died almost the same time that Frank Lloyd Wright died. Both of them died, I think, in 1959. It was within a matter of months of each other; there had been some preliminary discussions between Frank Lloyd Wright and Grady Gammage on the design of this auditorium. Therefore, following the death of Gammage and Wright, the Arizona State people decided that it would be appropriate to make this auditorium a memorial to both of these people--that is, they would name it the Grady Gammage Auditorium.

As I say, I was retained by the university, and just at the time of my retirement, in June of 1960, I had my first conference with the trustees of the university at Tempe, Arizona. And this was a rather interesting session. The successor to Frank Lloyd Wright, Taliesin Associated Architects, was represented by their chief architect, by William Wesley Peters, who had been identified with the Frank Lloyd Wright Foundation and with Taliesin Associated Architects for a long time. In that first conference I was advised by the trustees that my assignment would not be an easy one because the Frank Lloyd Wright people had fairly fixed ideas about the design of this auditorium.

Well, that advice was very soon dissipated because I at once became attracted to William Wesley Peters who was present at this same meeting. I made some radical recommendations at this meeting, that the shape really could not at all follow the exterior shape, that it would have to be grossly modified from that shape. And as a matter of fact, the interior design that was finally approved was one that essentially turned the shape outside-in--that is, instead of having concave surfaces for the entire interior we used very large convex splays. As a matter of fact, in plan there are only seven convex surfaces encompassing the entire seating area, two for each side wall and three for the rear wall. The two on each side wall are approximately 60 feet in length--that is, the chord lengths of these convex splays are of the order of 60 feet; they are, I believe, bigger convex surfaces than have ever been used in an auditorium before.

This became the guiding principle in the design, and this was worked out in detail by use of acoustical mirror models that we use in the laboratory here. These large surfaces were found to give not only freedom from any echoes or sound focusing effects, but also to provide unusually good diffusion of sound. Now, in the design of a music hall, especially, you want good diffusion of sound. The listener wants to feel that he's immersed in the sound field. The sound comes to him from not only the stage but from the side walls, from the rear wall, from the ceiling--from all directions. For the reverberant sound, especially, you want to feel--well, "I'm surrounded by sound." This is a fundamental criterion for good acoustics of concert halls. Furthermore, the sound that is reflected to the audience must not be delayed too long. If it's delayed as much as 60 feet--that is, if the path length that the reflected sound travels is 60 feet or longer than the direct sound from the source to the listener, it is heard as an echo. If it's even delayed as much as 50 feet it causes some confusion, and if it's delayed only as little as say 30 feet or something like that, it gives enhancement; it reinforces the direct sound, and it introduces a property which is referred to as "acoustical intimacy," and it's a characteristic of the famous concert halls of Europe. Normally, these concert halls of Europe don't seat more than, at the most, 1,600 or 1,800 people, sometimes only 1,200 or 1,400 people, whereas the Grady Gammage Auditorium had to seat 3,050 people. In that respect, you see, it is nearly as large as the Dorothy Chandler Music Pavilion that seats 3,250.

Well, I might review a few of the characteristics that differentiate a music hall from a lecture hall or from an auditorium which is to be used primarily for speech, for drama, for conventions, for, well, commencement exercises and all other kinds of university assemblies. This was to be a multipurpose auditorium, but like so many auditoriums that are under design at the present time, it is to serve also as the home for the local symphony. Nearly all of the projects that I am working on at the present time--and there are some twenty-one around the country--have a primary auditorium that's called a theater, concert hall, or something of that sort, but it's always a multipurpose auditorium, which means that it has a proscenium and stage and can be used for theatricals and can be used for all sorts of assemblies. But it is also used for all kinds of music programs including symphony programs and choral symphony programs. In the case of the Grady Gammage Auditorium, it was to be the home for the Phoenix Symphony Orchestra. The orchestra had been performing in a high school auditorium which was far from a proper hall for a distinguished symphony; and it is a rather good symphony.

The requirements, then, for speech rooms and music rooms, I think I should probably review briefly. [tape off]

This is an appropriate place to refer to the different requirements for a music room and a speech room. For example, a large music room such as we were planning to design for Grady Gammage must not have certain impairments in general. It must not be impaired with echoes or interfering reflections, and I indicated that the circular design would be especially bad. It should not be afflicted by disturbing noise, and this is characteristic also for a speech room. It must not be disturbed by focusing effects and malfunctioning concave surfaces. These were some of the things that we had to deal with specifically there and that we deal with in the design of all auditoriums.

And speaking of some of the advantages it must have, the room must, or at least it should have, these following characteristics: the optimal dimensions in shape to provide the most favorable distribution of sound to performers and all listeners and to provide both early and suitably delayed reflections of sound from all wall and ceiling surfaces. This is especially an important requirement for a music room. It must have the optimal reverberation characteristics at all audible frequencies. It must not be too alive and must not be too dead. It must have proper diffusion of reflected sound. These and other desirable acoustical characteristics can be attained within certain limits by judicious designing, which usually includes certain adjustments in the completed room.

The optimal shape of the music room is a requirement of the highest priority. This is not so important for a speech room. Many architects believe that faulty shapes can be corrected by covering the offending surfaces with highly absorptive materials, as I referred to in the Arnold Beckman Auditorium here at Pasadena. Many recent innovations using concave shells of concrete are acoustical perversions which can only be partially ameliorated by the treatment of the offending surfaces with absorptive materials and often major surgery. Certainly, a bad shape is a permanent liability.

Frank Lloyd Wright's representative in San Francisco. He is still listed in the telephone directory and in his office as a Frank Lloyd Wright associate. Frank Lloyd Wright had so much confidence in him that he made Aaron G. Green his associate in the San Francisco area.

The biggest one of them all is the Los Angeles County Court building. Dr. Leonard and I worked together on this project until his recent death. I regret very much Dr. Leonard's passing on, but we had completed practically everything connected with the acoustical design. Dr. Leonard handled the noise problems, and I handled the problems connected with the design of these court rooms. The foregoing examples, I think, will represent improvements in the acoustical designing of court rooms. In many of these rooms it will not be necessary to use sound amplification. The rooms, generally, are a little smaller than conventional court rooms, and they're shaped so as to obtain the maximum benefit of reflecting surfaces, for the jury, the judge, the attorneys, and even for the spectators or audience.

Among synagogues, there's the Wilshire Boulevard Temple synagogue here in Los Angeles. There were several--three at least--designed by Eric Mendelsohn. Eric Mendelsohn was one of our most distinguished

In the choice of reverberation characteristics of a music hall, we must also remember that we deal with such diverse kinds of music. We deal with musical tastes on the part of the public and of the performers and a wide range of musical compositions. They may be simple or complex; they may be slow or fast, oriental or occidental. They may be classical or modern. They may be symphonic, or they may be musical comedy of a light nature. They may be choral or operatic, for one, or few, or many instruments. You have all of these combinations, from a single solo up to a large choral symphony.

These present many subjective problems, and you see what a diversity of things you have to deal with, which makes the problem of designing an ideal music room an extremely difficult one, the complete solution of which, although possible, must await even further knowledge. We don't know the last word about the design of music rooms yet. That is, as I indicated earlier in our discussions, that for a speech room you have definite criteria: how well do you hear the speech? That's the important thing, and the percentage of speech articulation and how that is affected by these various factors of reverberation time, of noise background, of shape, and of loudness--these are the four factors, and they can all be assessed quantitatively, objectively, and you can predict in advance just how well you will be able to hear speech in any part of the auditorium.

You can't do that for music. Music is a much more elusive thing, and you don't have simple criteria for measuring it as you do for speech. And therefore it's a work of art as well as technology, and you have to try to blend these things in the best possible way. This calls for the use of a great deal of empiricism.

What have been successes in the acoustical design of music halls? The concert halls of Europe and the opera houses of Europe and this country that have been most famous would be models that you more or less should follow. We have such places as the Boston Symphony here and the Academy of Music in Philadelphia that are ordinarily cited as the primary ones. And you have halls in Europe, like the Leipzig Gewandhaus which was destroyed by bombing in World War II, and the Musikvereinssaal in Vienna. These were models that had a lot to do with guiding the criteria, especially of reverberation and early reflections in the design of the Grady Gammage Auditorium.

Well, this in brief outline indicates the chief differences between a music hall and a speech hall. In the design of the Grady Gammage Auditorium we had to meet both requirements, but the highest priority, I believe I said, was to be given to the symphonic orchestra requirement because it was to be the home of the symphony orchestra. Furthermore, the opening of an auditorium, such as this one, is always judged more by the first performance by the symphony program in the hall, just as our Music Pavilion in Los Angeles was judged on the basis of the first symphony performance.

So in the design of this hall we gave more preference to the requirements for music than we did for speech. Specifically, if we had been designing it for speech only we would have designed it for a reverberation time of not more than 1.4 or 1.5 seconds. Inasmuch as we were giving high priority to music, we designed it to have a reverberation time, when fully occupied, of between 1.8 and 1.9 seconds. Fortunately, the outcome in reverberation was very close to this predesigned criterion. Part of the reason we were fortunate is because, again, of these large convex splays which mix up the sound--low pitch, medium pitch, high pitch--thoroughly. And you have therefore unusually the same quality of reverberation and the same quality of intimacy and the same quality of diffusion (and immersion in the sound field) in all parts of the auditorium.

The objective measurements that we have made in the auditorium support this conclusion. They are described in a paper which Dr. Delsasso and I presented at the Fifth International Congress on Acoustics, which was held in Liege, Belgium, September 7-14, in 1965. And the title of the paper which we presented there is "Acoustics of the Grady Gammage Auditorium at Arizona State University." It is not generally available because it is published in the proceedings of the Fifth International Congress on Acoustics. And inasmuch as it has not been adequately described elsewhere it might be well to review it in more detail that I would otherwise do.

I'm sorry it is not possible to include here some drawings, but anyone interested can find the drawings on and even a model on, a portion of, the auditorium in our acoustics laboratory downstairs. It's of interest to examine the design of this architectural triumph, because it has received a great deal of favorable attention. As a matter of fact, when the governor spoke at the opening program, he said in the future, visitors coming to Arizona will be interested primarily in two things, the Grand Canyon and Grady Gammage Auditorium. That is a politician's statement and probably requires some modification, but it does indicate the enthusiasm which the governor had for the design at that time. Certainly it stands out as a monument on the Arizona State University campus; it's in a central location, and it will be surrounded by other buildings that are for the fine arts.

A separate music building is under design at the present time, also by the Taliesin Associated Architects, and also based upon some original sketches that Frank Lloyd Wright had made, so there will be at Arizona State University a very fine center for the performing arts that will compliment the beauty of the architecture of the Grady Gammage Auditorium.

I might cite a few things from this report that was read at the Fifth International Congress. As I've indicated, the chief architect was William Wesley Peters of the Taliesin Associated Architects of the Frank Lloyd Wright Foundation. This organization has been perpetuated very effectively following the death of Frank Lloyd Wright. Mrs. Wright participates actively in the guiding of the Foundation. She is president of the Foundation, and Wesley Peters is the chief architect for the Taliesin Associated Architects.

Part of the success of this auditorium I'm sure is attributable to the splendid cooperation of all concerned. I don't know of any project on which I worked in which there was such a splendid, cooperative attitude on the part of everybody concerned with it. The architectural staff--George Izenour of Yale

University, who was the theater design consultant, and particularly worked out the design for the motorized stage enclosure (acoustical shell), and the equipment for lighting the stage. The contractor, Robert E. McKee, who has built several buildings on the UCLA campus, and the Arizona State University personnel gave the undertaking their very best. I'd say I know of no instance in which the cooperation was as cordial and complete as it was throughout the entire designing, construction, and testing of the auditorium. Although this auditorium, as I said, was to be multipurpose and serve all the functions, the highest priority was given to the requirements for musical programs, and more specifically it was to be the concert hall for the Phoenix Symphony Orchestra.

The diverse requirements for this multipurpose auditorium were greatly facilitated by the design of the shell, which is a most unusual one. It's built of steel that's been doped, like they dope the-- well, the metal body of an automobile. If it wasn't doped it would ring and resonate, and it would be impossible, but by properly doping the rear surface of 18 gauge steel, pressed into the shape that we wanted, it sounds like wood. That is, you hit it with your hand and the average person would say, "That isn't steel; it's wood." And as a matter of fact, some of the conductors have said that it's a good orchestra shell because it's of wood. It is possible to simulate the acoustical properties of wood and get the characteristics that you would want, which is an important matter in the design of a good auditorium for music. As a matter of fact, one reason I think the Music Pavilion in Los Angeles has received such high acclaim is that the shell itself is entirely of plywood, heavy enough to meet the requirements.

This "heavy enough" is an important requirement, because if the material isn't sufficiently dense, the low frequencies are not adequately reflected. If we had used a thin material for the shell in the Music Pavilion or in Grady Gammage, we would have had a bass deficiency. The low frequencies would have been muffled somewhat, and they'd say, "Well, you can't hear the double basses; you can't hear the cellos." And it's necessary, therefore, that the shell material be massive enough (not less than four pounds per square foot) to reflect low frequencies about the same as it will reflect high frequencies.

This was carefully incorporated into the shell for the Grady Gammage Auditorium. Furthermore, the side walls and the ceiling are modulated in shape so that they reflect some sounds backward and they give good diffusion within the shell and good mixture of the various sections of the orchestra so that a large orchestra really sounds very much like a single instrument. Furthermore, this shell is fully automated by push-button control. The side walls and the ceiling, in a truncated arrangement can be moved on tracks back to the rear wall of the stage where they are stored when not in use. By pushing two or three buttons that control motors that move the whole truncated shell on tracks, it can be put into concert hall or storage position in less than twenty minutes. When the auditorium is used as a symphony or concert hall the shell is always in position. As you look at the auditorium then, it looks like a symphony hall--that is, architecturally, the shape of the shell blends with the convex splays that we have on the side walls and the ceiling of the auditorium itself; and thus there is attained a continuation, architecturally as well as acoustically, of the design of the seating area uniting harmoniously with the design of the shell. And when you look at it from the audience position you are really not aware that there is a proscenium and a theatrical stage. It looks like a concert hall, and it definitely has the acoustical characteristics that would be necessary for a good concert hall.

As a matter of fact, when we measured the delay of the reflected sound behind the direct sound, we found that the earliest reflected sound arrives at something like 29 milliseconds. That's approximately thirty-one feet behind the direct sound, for most of the seating area. This gives good reinforcement, and also it gives good intimacy and definition. Furthermore, when we measured these reflections and the directions from which they come--Dr. Delsasso has spent a great deal of time on the Grady Gammage Auditorium making such measurements--we find that the reflections do indeed come from all directions wherever you are. And this is true even of the last row under the balcony (called the "Grand Tier" there) but it's the first balcony.

A unique feature of this balcony is that it is suspended not from the rear wall, but from the side walls; thus, the entire balcony is forward of the rear wall, so that sound can circulate entirely around the balcony. Listening to a concert from the last row of the balcony was a new experience to me; I felt like I was sitting out in the middle of the auditorium. I knew that the sound was coming to me from overhead, from behind, from the sides, and from all directions, and I felt immersed in the reverberant sound field the same way as I did when sitting out in the middle of the auditorium.

Well, the extensive measurements that we have made in the auditorium support the findings of critics and others that this is one of the finest auditoriums in the country. Personally, I think that for music it's one of the finest large auditoriums in the world. The measurements we have made support the views of music critics that Grady Gammage Auditorium is an outstanding success.

To get some idea of the size of this place, the exterior dimensions of the building are 300' x 250'. That's the original Frank Lloyd Wright design, and it's an imposing building.

You see these two intersecting cylinders that project up into the air, and the color blends in ideally with the desert colors. It's a desert red or a desert orange. The entire exterior of both the stage and this large seating area are faced with brick, the color of which was carefully selected by Mrs. Wright and Wesley Peters. And it blends with the stucco portions that also reflect the colors of the desert. So as you come into Phoenix--by Western Airlines particularly--you fly directly over the campus of the Arizona State University, and this building is an imposing sight from the air. I think you would have to admit that it's an impressive work of art.

The exterior, as I say, is 300' x 250', and it stands some 80 feet high. The volume of the auditorium is about 700,000 cubic feet, which was an acoustical requirement for good concert hall acoustics. We like to have that much volume in order to get the kind of reverberation we want. It seats 3,050 persons, using continental seating, and every seat has a perfect view of the entire stage enclosure when it's set for symphony or concert programs. You can see every performer from every seat in the hall, which is unusual for most auditoriums. Furthermore, the most remote seats are only about 110 feet from the front of the proscenium, so that even though it's a fairly large auditorium, like the Los Angeles Music Pavilion, the seats are brought near the stage because it's a fan shape. The side walls diverge outward toward the rear. Also, there are two complete "balconies," the Grand Tier and the Balcony.

The reverberation time at midfrequencies is 1.8 seconds with a capacity audience, and this rises to about 2.5 seconds at low frequencies, which is desirable. In a music hall this is important; in a speech hall it is not. But in a music hall you want a longer reverberation time at the low frequencies than you do at the high ones because this condition accords with the acoustical characteristics of our sense of hearing. As a result, you'd like to have about 20 percent longer reverberation time at low-pitch sounds than at medium-pitch sound, and you'd like it to stay uniform at higher frequencies. You can't quite accomplish this latter condition at high frequencies because the absorption in the air itself, which was first fully explored and evaluated in our laboratory here, is a limiting factor. No matter what you do to the air, it absorbs high-frequency sounds much more than it does low-frequency sounds. Even if you had no absorption at all in the walls you couldn't get more than about 1.4 or 1.5 seconds at very high frequencies, because for frequencies above about 4000 cps the air absorbs much more than the walls would absorb under any circumstances that you would use in the design of the hall. [tape off]

MINK: I'll ask you about the balconies and whether or not this was your idea.

KNUDSEN: There is a unique feature about the design here, namely, the suspension of the first balcony, which is called the "Grand Tier," from the side walls instead of the conventional attachment to the rear wall. As a matter of fact, it's forward of the rear wall as much as--oh, in some places as much as 12 or 15 feet. It's because the rear wall is convex and the seats move in the conventional arc toward the stage. This distance from the rear wall varies as you go across the rear of the hall, but it would average about 10 feet and in places it is as much as 15 feet. And also it's a very interesting view that you get looking up from the rear of the main floor. Among artists who have been inside the hall, they say this view that you get from the rear of the main floor as you look up and see this huge convex surface of the rear wall of this Grand Tier, which is forward of the main rear wall, looks like a massive sculptured work of modern art. This has been commented upon by many people. "What an interesting view these people have at the rear of the auditorium." You see also the second balcony above, and you can see through even as you look up this way and see much of the main ceiling of the auditorium, something that you do not have with conventional balconies. And you feel visually as well as acoustically that you are really in the main part of the auditorium.

This design of the "flying balcony" was really made possible by Frank Lloyd Wright's original intention of having a ramp that would deliver the concert goers at the sides of the auditorium and at a level approximately comparable to that of the Grand Tier. It was not possible to maintain that feature in Frank Lloyd Wright's design because it was too costly. But William Wesley Peters, the architect, said to me, "Well, this looks like an attractive idea. Can't we actually suspend the balcony itself without having these exterior ramps constructed so as to support the loads of automobiles delivering people who come to the concert hall?" And I said to him, "Oh, that would be wonderful acoustically." I seized on the idea at once, and I said, "Well, I don't know of any auditorium that's ever had that." And he said, "Well, no auditorium that I know of has ever had it anyway." But he said, "Do you think it would be good acoustically?" I said, "Oh, this has great merit acoustically." I at once endorsed the idea, and it was decided therefore that we would have it. Of course, it involved the design of a very massive truss, you see, to support a balcony that extends across--oh, it must extend about 150 feet across the hall from wall-to-wall, and there is no other support except at the side walls. It's entirely supported by a massive steel truss that extends across the entire wide part of the auditorium.

MINK: Well, had you ever in your experimental designs with auditoriums considered this independently? Had you ever thought of this as being something that might enhance acoustics within an auditorium?

KNUDSEN: No, frankly, I had not--that is, we just took for granted that a balcony suspended from the rear wall is one of the fixed requirements in balcony design.

MINK: Structurally it would be impossible to do this?

KNUDSEN: As a matter of fact, the idea had not occurred to me, because it seemed absurd that it would be feasible to have widely separated side walls support such an enormous load. I'm not a structural engineer, or I probably would have thought of it. But I had not really thought of such a departure, and it probably was first in the mind of Frank Lloyd Wright, and so Mr. Peters asked me about it. I jumped at the idea like a trout jumps at an attractive lure.

MINK: Then it would seem to me that here is an incident in which the architect and the engineer and the expert on acoustics have wedded together an idea that has improved the building of auditoriums.

KNUDSEN: I'm sure it has.

MINK: And you now say that you're going on to do this in other auditoriums even more than it was done here.

KNUDSEN: Yes. In one of the designs we're working on now--I probably shouldn't name it at this early stage because sometimes these things are planned and then when costs come along they're canceled--but an auditorium that's almost as large as the one at Grady Gammage has two balconies, both of which are to be suspended. Incidentally, the design for that auditorium in plan is almost a Chinese copy of the design of the Grady Gammage Auditorium; so it's design is influencing later designs, especially by the Taliesin Architects. I am presently working with Taliesin on three other major multipurpose auditoriums to which the highest priority is symphony performance. But they are multipurpose auditoriums and will have many of the characteristics of Grady Gammage. The outcome of Grady Gammage has had a very salutary effect upon the design principles now that are guiding these auditoriums that are the major responsibility of the Taliesin Associated Architects. There are presently under design or construction other auditoriums for which I am consultant that incorporate acoustical features that originated with the Gammage design.

MINK: Now again, in these new auditoriums where you are going even a step further and suspending the second balcony, is this something that you are advocating or is this something that they wish to do?

KNUDSEN: Oh, this was arrived at by joint discussion. The outcome of the Grady Gammage Auditorium indicated--well, why not do this also for the second balcony? It would help particularly the people who are in the grand tier, you see, or the first balcony.

MINK: How would it help them?

KNUDSEN: Well, because then they get sound that circulates around them, so that the only people who do not get this immersion are really the ones in the top balcony.

MINK: Would this be something that would have been impossible to do 50 years ago or 100 years ago?

KNUDSEN: Probably. The advancement in design of trusses that have to carry heavy loads, I'm sure, has evolved so much during the past 50 years that this would have been considered, oh, just a pipe dream then, an impractical pipe dream.

MINK: As we see at Houston, for example?

KNUDSEN: There, that's variable volume as well; the entire ceiling can move up and down in the (Jesse Jones) Houston job. And it has many features that are desirable. I had nothing to do with the acoustical design of that hall, but I understand it is quite good. I'm doing some work for an architectural firm headquartered in Houston, so I had an opportunity to inspect the Jesse Jones Auditorium there. It has attracted a great deal of attention, primarily because it has variable volume. This feature was the idea of the theater consultant, George Izenour, who worked with the architects and me on the design of the Grady Gammage Auditorium. He is probably the most imaginative theater designer in the country at the present time. George Izenour of Yale University is on all of the projects that I'm working on with the Taliesin Associated Architects. The chief architect for Taliesin, Mr. Peters, Professor Izenour and I get along together very well as a congenial and effective team. I always regard it as a pleasant mission when I go to confer with these men on the design of projects by Taliesin Associated Architects.

Incidentally, on the Grady Gammage Auditorium Taliesin had a senior architect on the job all the time. He watched and guided everything from the beginning to the end of construction. He lived with the auditorium during the two and a half years it was under construction. Even in preparations for the opening concert, there was a splended esprit de corps in this Taliesin group. You see, most of them are fellows of the Frank Lloyd Wright Foundation. They are studying architecture, and they are, or soon will be, registered architects, and they know how to pitch in. They're accustomed to working with their hands, and we had some changes to make, especially on the rear wall of the acoustical shell for the stage.

The rear wall is not movable, as the side walls and ceiling are, which, as I explained, can move back and telescope against the rear wall. But the rear wall has what we call a facility for tuning the shell. The lower portion of the rear wall structure is made up of panels that are 4' x 4'. Normally they are three-quarter inch plywood that are very well reinforced. They are fastened by screws, and they can be removed and replaced by any number of four by four absorptive panels that are of fiber glass or mineral wool, two inches thick. And very often the sound from the drums, which are usually in the rear of the shell of the stage or platform, are overloud.

One of the changes that we decided to make before the first concert in the Gammage Auditorium was to remove some of the plywood panels and replace them with the absorptive panels, particularly where the brass and the drums were located. We had less than twenty-four hours in which to do this. Well, the whole work was done by all of us working together, and the union workers didn't object--that is, the members of the architectural staff of Taliesin really came to the rescue and they did most of the required work. You know, many of the things that are built by Taliesin Associated Architects are built by the architectural students themselves, the fellows of the Foundation. And so they know how to do things with their hands. It was interesting to see this group of Taliesin architects and fellows--many of them mature, and even members of the permanent staff, some of whom have been there 10 years or more, all pitch in, and this work, including the making of the absorptive panels, was done in short order. We decided one night in conference what should be done. When I came back the next morning it was all done. And it was quite an extensive operation, including especially the building of the absorptive panels.

I wish to describe briefly now the experimental findings that Dr. Delsasso and I have obtained in the completed auditorium. Delsasso was with me as the prime experimental expert on this important investigation. He had his Volkswagen camp wagon loaded with apparatus that we needed for the tests. But whenever we go for tests of this sort the Arizona State people just do everything to make it easy for us to do our work. That is, they have someone to move our equipment around for us, and they have a man on the job all the time to see that we get the electrical facilities we need to operate our equipment. They really manifested an interest in the outcome. Men from the business office and from their office of architects and engineers came over to participate in the work. We spent three days making the necessary tests and measurements, and we worked pretty much night and day. The tests were made to determine just how well steady state sound permeates--for example, if you have a sustained sound on the stage, how does the loudness of that sound vary in different parts of the auditorium? Well, Delsasso had devised an ingenious method of cables, pulleys, and counterpoises and so on, so that there's no sag in the cable over which a motor-driven microphone moves over the seating at head height. I won't go into the details. This is a mechanical piece of wizardry that men like Delsasso can work out ideally. But this would cause the microphone to travel along a typical path at head height in the auditorium.

MINK: And the cable was perfectly taut so that it never would go up or down.

KNUDSEN: The cable was equivalent to that, but you can't get a cable that taut. But you can by use of counterweights. A system of pulleys have the recording microphone move along a cable in a straight path. Sometime Dr. Delsasso will be glad to exhibit this system. We have it in our anechoic chamber downstairs. The microphone moves along a course as though it were moving on a rigid track, and yet it's moving along a cable. You couldn't stretch a cable tight enough to avoid some sagging and the cable system we used at Gammage had no sag at all in it. Well, this was only one feature of our apparatus, but the upshot of it is that we were able to move the microphone at head height over any section of the auditorium we wanted to on the main floor and in both balconies. In general we ran three courses from front to rear of the main floor, one along the center line, another one-third of the way toward one side, and another two-thirds of the way toward one side wall, so that we had these three paths.

For example, I have in this paper that we presented at Liege, Belgium, in the congress, records here that show how the sound level in decibels on the main floor varied from the front row to the rear row along the center line, or longitudinal axis, of the auditorium. And this was for a distance of some 30 to 105 feet from the sound source which was a one-third octave band of white noise coming from a suitable loudspeaker on the central part of the stage. From the front row to the rear row, we did this at four different frequencies--well, a medium frequency up to high frequencies, and these are the ones that are of most interest: third octave bands of white noise centered at 500 cycles per second, 1,000 cycles per second, 2,000 cycles per second, and 4,000 cycles per second. And from the front to the rear at the 500 cycles, it drops not more than 3 decibels. At 1,000 cycles it drops not more than 2 decibels. And at 4,000 cycles it drops about 1.5 decibels from the front to the rear. Now this is a very extraordinary uniformity of loudness of sustained sound in a large auditorium with balconies. These were sustained sounds; the moving microphone picks up the sound and we feed that after amplification into what we call a "sound level recorder," which makes a pen trace (which registers decibels) on a piece of paper that moves at constant speed. We have a trace therefore that shows just how the sound level in decibels varies from the front to the rear.

Now if the balcony had been suspended in the conventional form there would have been a marked drop in the sound level as you go under the balcony toward the rear row of seats. We made many similar measurements in other auditoriums, in which the sound level would drop as much as 5 decibels from just in front of the balcony to the rear row under the balcony. And as these records show, there's no indication of a drop at all in the sound level under the Gammage balcony. As a matter of fact, at the 4,000 cycles there was actually a rise of about 1 decibel. The lowest level is out near the middle part of the auditorium, and there's a slight rise even at 2,000 cycles, and there's a rise at 500 cycles even of about 0.5 of a decibel or so. That rise is attributable to the reflection from the rear wall that reinforces the sound near the wall. The uniformity of the sound level was surprising; with a conventional balcony there'd be only the sound coming through the balcony opening, the great absorption that takes place in this relatively small volume from the upholstered chairs, and often complete carpeting. In Gammage, the sound circulates around the balcony, and an especially designed ceiling splay reflects sound downward to the rear seats under and behind the balcony.

Well, we also made measurements in Gammage of impulsive sounds. And for that we used pistol shots. As a matter of fact, we also used pistol shots for one of the three methods for measuring the reverberation. I told you that we had a reverberation time of about 1.8 seconds when it is fully occupied. The measurements that were made when it was fully occupied actually utilized the choral symphony of the Beethoven Ninth. They did the Beethoven Ninth, and we had them make a tape recording of it, which they sent us for reverberation measurements. We used a particular specimen that comes in the fourth movement just before the initial baritone solo. And if you remember the orchestra builds up to a tremendous triple fortissimo and then suddenly stops, and there's a silence of at least two seconds before the baritone takes over.

So this tape recording, then, furnished us with another means of measuring the reverberation in the hall when the hall was completely occupied, and not only completely occupied but with the stage, you see, fully occupied including about 150 in the chorus. With all instruments performing you get low-pitch sounds, medium-pitch sounds, and high-pitch sounds, and then we play that tape back in our laboratory here through different filters. Normally, we use filters that pass one-third octave bands. And so we make measurements with the tape, playing it back and forth, with each of these filter settings, and from that we can measure the reverberation, just how the sound dies away. It's interesting to hear this reverberant sound. When you hear it, you hear it die away uniformly. The decay curves that we showed at the Liege meeting were ones that we obtained from pistol shots, and so were the ones from live music. They are very uniform.

Now, in many auditoriums that do not have good diffusion, the sound dies away much faster the first part of the decay than it does in the latter part. This was one of the difficulties they encountered in Philharmonic Hall in New York, for example. In rapidly moving passages of music, therefore, the hall sounds much less reverberant than it was calculated to be. That is, you hear only the first 15 or 20 decibels of decay, then another sound comes along and masks any further decay of this first sound; so that in rapidly passing music it's not what is the reverberation time, which is the time required for the sound to die away 60 decibels, but how much does it die away, say, in 0.2 of a second, or something like that. If you're listening to sound that comes at 5 decibels per second, then you're not much interested in what happens after 0.2 of a second in that decay, because it's the initial decay that really determines how reverberant a hall sounds.

So in some halls, the Philharmonic Hall, for example, in its original condition, it sounded like a hall with less than 1 second instead of 2 seconds. In contrast, you can see from these curves that I am showing Mr. Mink here that they die away uniformly in all parts of the auditorium. These curves happen to be for pistol shots, and they reveal two things. They are for 12 different pistol shots with the microphone in 12 different positions, 9 on the main floor representative from front to rear and from center line to side walls, and three in the upper balcony. And we had found out from previous tests in our laboratory that these blank pistol shots--the pistol was a 22-gauge starting pistol, and the caps were a special German make--that is, they were very uniform in their loudness. As a matter of fact, we found that by shooting 100 of these the mean deviation was only 0.6 of a decibel. And therefore this simple pistol becomes a good instrument for producing a standard impulsive sound in an auditorium or for any other purpose.

These curves that I am talking about now indicate first that the peak level of these pistol shots is essentially the same in all parts of the auditorium. That is, it's about the same in the rear, and as a matter of fact, it's a little higher in the upper balcony than it is in the middle of the main floor, because up there they get also the benefit of some of the reflections from the nearby walls and ceiling.

MINK: The shot was always fired from the same position?

KNUDSEN: The shot was always fired from the same position.

MINK: Which was?

KNUDSEN: It was the front, central part of the stage about 8 feet back of the proscenium opening. Secondly, these decay curves show how the reverberation time varies for different locations. The curves are for the empty hall. It was 2.3 seconds for the middle frequencies at the front part of the hall, and it ranges as high as 2.7 seconds and as low as 2.0 seconds in the balcony. And this shows how the reverberation time varies in different parts of the hall. And it also shows that the decay rate is uniform throughout this entire auditorium. This is for a range of nearly 40 decibels, and there's no change in the rate of decay so that the hall will sound equally reverberant for rapid passages of music as it will for slow movements. Or when you listen to a loud chord, for example, as we did in the triple fortissimo chord that just preceded the baritone solo in the fourth movement of the Beethoven Ninth, you hear the thing die away, and it dies away uniformly--not fast at first and slower at the end. It's a uniform, pleasant, reverberant decay. And this has been characteristic of the Grady Gammage Auditorium.

MINK: Now this certainly was one dedication that you attended.

KNUDSEN: Yes.

MINK: Do you want to say something about the dedication of the auditorium?

KNUDSEN: Yes, I do. I not only attended, I was invited to be one of the speakers. As a matter of fact, the speakers, besides the governor of Arizona and the president of the university (Homer Durham, who obtained his PhD at UCLA) were the architect, Mrs. Wright, George Izenour, the theater consultant, and myself. And we were each given about fifteen minutes. And my address was, along with the others, published in Arizona Days and Ways, which is the Sunday supplement to the Republic (Phoenix), dated November 29, 1964. Anyone who would like to know more about the Grady Gammage Auditorium would find in this publication a good description. At the dedication, Mrs. Gammage was present, the widow of Grady Gammage, and she also made a brief thank you at the end of the ceremony.

It may interest you to know that on this occasion I summarized some of these things I've related to you today about the requirements for a speech hall as compared with a music hall, indicating that we favored music very much for the design of the Grady Gammage Auditorium. At the end, I made one remark which I think was worth putting in the record here. I had praised the outcome in the acoustics and the other aspects of the auditorium, because it was a thrill to all of us who were there. And my closing remarks were the following. I think I can quote them verbatim. "There is only one thing wrong with this auditorium." There was a pause. People came to attention. Then I said, "It is not on the UCLA campus." I think that's a fitting close.

SECTION XXII

KNUDSEN: Today, I wish to narrate a discussion of my work during World War II, which took me away from the university for a period of approximately three years. But, to give a little background, we'll go back to London in September, 1938, when Prime Minister Neville Chamberlain was going back and forth between Munich and London to have conferences with Adolph Hitler in the hope that they could stave off war at that time. On the night before Chamberlain returned from his last trip to Munich, things looked very black in London, very dark. As a matter of fact, there was a blackout even that night. Mrs. Knudsen and I were at a theater. I forget the title of it, but it was a war subject involving especially a mathematical physicist. The sound insulation of the theater was so poor that you could hear the news boys outside calling, "Hitler may strike tonight!" This was the headline that we heard while we were listening to this war drama.

We had our children with us on this sabbatical leave, the eldest (Robert) fourteen and the twins (Morris and Margaret) eleven, and we were greatly concerned about the safety of our entire family there that night. On the way from the theater to our apartment we saw queues of Londoners receiving gas masks, and workmen digging trenches in neighboring parks. And we talked things over after the theater that night. War was that near London in September of 1938. And we decided that it was time for us to move, and we therefore made arrangements early the next morning to go to Newcastle-on-Tyne where Mrs. Knudsen has relatives. We hired a car--they always say "hired" over there--to drive to Newcastle-on-Tyne with the expectation that if war did break out it would be a good takeoff point for Oslo, Norway, and from Oslo we hoped we could get ship passage to take us back to New York. We were successful by the following noon in obtaining a car for our purpose, and that day, we drove as far as Stratford-on-Avon, where we stayed overnight.

To our great pleasure, a performance of the Mikado by the D'Oyly Carte Company was scheduled for that night, so we took the children to the Mikado, a play that they had seen with us here in Los Angeles, and it had delighted the children; they'd seen it only a year or so before at the old Philharmonic down on Fifth Street, Los Angeles. At the close of the first act, the Mikado was interrupted; an official came out and announced that Mr. Chamberlain had just returned from Munich and that the headlines in the papers were proclaiming "Peace in Our Time"--hopeful but infamous words that reverberated around the world. This was, you know, the false hope that was engendered on the occasion of this last visit of Chamberlain with Hitler, and nearly everybody sighed relief.

We went on our way the next day toward the Stoke-on-Trent country where we were interested in porcelains and saw some of the porcelains under manufacture there.

Incidentally, the day we were there they were making plates that featured the University of California, Berkeley, especially its campanile. It was a special series they were making for the University of California, and we just happened to be there the day these were in process of manufacture. So we saw not only how they manufactured their fine porcelain but one that was of special interest to us; we have one of the plates as a souvenir that we obtained on that occasion.

Well, we went on our way to Newcastle-on-Tyne and met for the first time relatives of Mrs. Knudsen. Her parents had left England many years before, probably in 1870, and there had been only a casual correspondence with them during the past fifteen or twenty years. But we readily located her relatives, and had a friendly visit with them. My wife and son Robert have since then visited their Newcastle relatives.

and continue to correspond with them.

Peace apparently was temporarily established, and so we went on to Edinburgh, Scotland, and other places, and returned to London.

A few weeks later, we left for Germany. I was on sabbatical leave (especially in London and Berlin) studying acoustical developments. A great deal of architectural and noise research was going on in London where I made good use of the British Museum (the library at the museum) during the three months that we were in London. And we lived the real life of Londoners there. We had a penthouse. It sounds expensive, but it was in [laughter] an old building adjacent to the British Museum and Library. As an indication of the affluence of our penthouse apartment, we didn't have a bathroom. We shared one with the tenant on the floor below us. Just the same, we occupied a penthouse [laughter] apartment there.

Well, this going from London to Berlin was like entering another world. At once you got the impression as you walked on the streets of Berlin that this is a nation at war. I have never seen more men in military uniform on the streets of New York, say, during the period of World War II, which I saw a great deal of in subsequent years. And one of the questions I raised--because most of the uniforms you saw were Luftwaffe ones--and I asked some friendly colleagues over there who were working in acoustics and with whom I had exchanged correspondence and reprints over a period of years--and I asked one of these, (I think it's best to leave his name out of it), "How many officers and pilots have you in the air force?" And he answered back immediately, "Oh, we have 100,000 trained pilots and 300,000 reserves."

Well, later when I returned to the United States I made reference in a public lecture to this comment. The word came to me from the grapevine following the address I had given in reporting the militarism I had seen in Berlin during this visit, that this German professor certainly pulled the wool over Knudsen's eyes. That is, you know that people believed what they wanted to believe at that time; for example, Charles A. Lindbergh, about the same time, was discredited because he had gone to Germany to accept some honor for his initial flight across the Atlantic, and when he quoted estimates that he had been told about the strength of the Luftwaffe over there, most Americans didn't believe him. So, not only Vern Knudsen but even Charles Lindbergh had been disbelieved at that time, and it was thought that our German acquaintances were greatly exaggerating Germany's military strength.

MINK: May I ask where this address was given?

KNUDSEN: Mine was on the UCLA campus.

MINK: Oh, on the campus here.

KNUDSEN: Yes, it was an address here on campus. I forget whether it was at a University Religious Conference meeting or for some other group here on campus. Well, Berlin presented a scene that was certainly revealing a nation preparing to make war, to strike soon. There was no question about that. They wouldn't have that many men armed if they were not going to use them soon. It just seemed to me this was not defense. One of the questions I asked this same professor over there had to do with the Austrian Anschluss [the annexation of Austria] and Hitler was quoted by him as saying, "This is the last of our territorial ambitions. We're through now with our expansion." And I asked this professor, "Do you think Hitler means what he says when he publicly announces that this is the last of his expansions into other lands?" He said, "Of course! If Hitler says that, that's the truth!" This was his remark, and he gave the appearance of believing it. Well, I wasn't at all surprised the following spring when the Sudetenland was taken over, which was the next and the last big grab, of course, before the war broke out on September 1, 1939, when they invaded Poland--of course, without any warning whatsoever.

Well, from Berlin we went to Nuremburg and then to Munich. We happened to be in Munich during the November, 1938 night that probably the worst pogrom against the Jews took place. And that next morning, as we walked out from our hotel, we at once saw signs on the doors of many stores and public buildings, "Juden Kein Eintritt." "No entrance to Jews." We walked down the main streets of Munich, and soon recognized that the front windows of the shops that were Jewish-owned had been not only broken but almost pulverized, and all this had happened at the precise hour of 2 A.M. during our first night in Munich.

MINK: You couldn't help but hear it, could you.

KNUDSEN: We didn't hear it in our hotel. We were far enough away so that there was no molestation near the hotel. But the synagogues had been burned, and the storefronts had been smashed to smithereens. As you walked down the main shopping streets of Munich, you would see people staring in the window. When you saw the emotional expressions on the faces of many people, they had the expression, "My God, could it come to this? Is this actually true?" This was the sort of expression you read on the faces of people who were staring there, and of course there were others who were jubilant about this massive attack on the Jews.

And we decided to get out of Munich as fast as we could. We left Munich later that day for Nuremburg where things were a little quieter. We were on our way to Italy, and we stayed in Nuremburg only one day, long enough to get train reservations on to Florence, Italy, which was our next stop.

I don't think we've ever had such relief in our lives as we did when we got out of Germany on that particular occasion. We had the feeling that this was a nation at war already. The vindictiveness of this _pogrom_ that took place in Munich, you see, was commemorating the event of Hitler's coming to power. Some of his comrades, you know, were killed there at the civic center in Munich, and this became a shrine, you see, to Nazidom. So this _pogrom_ was a commemoration of Hitler's loss of his comrades when he himself, I think, was slightly wounded, or was reputed to have been slightly wounded, in this _putsch_ that took place, I believe, on November 11, if my dates are correct. Well, that is part of the background.

Now, as I said, I was not at all surprised at what happened with the Sudetenland in the spring of 1939, and all of us were glued to our radios much of the time from then on until September 1, 1939 when the war really broke out over Poland. Hitler, on the air almost every night, was screaming louder and more emotionally all the time. [tape off]

Of course, with the experience I had had in Germany in 1938 and with what happened in the spring of 1939, I was not at all surprised at the nature of the attack on Poland, and it was obvious that with such strength as you saw manifested in Germany at that time (November, 1938) that Poland was a pushover, and, of course, Hitler and his men realized that.

There was one other experience (in October, 1938) that I might interject. While I was in Berlin, there was the Annual Engineer's Ball, and this is one of the big social events of the year in Berlin; it's a white tie affair for the men. I had at that time possessed myself of those facilities in London, because they were used quite commonly in social events here in Los Angeles at that time. (I almost never use them now. The outfit is full of moth holes now, and I've used it only once or twice I guess in the last five or six years.)

But this engineer's ball was of special interest to me. A German colleague had tried to indoctrinate me about what a great influence Hitler was and what a wonderful person Goering was, for example. Goering was to be the guest of honor at this annual ball of the engineers, physicists, and so on of Berlin. And this was really a super-duper affair. There were three bands performing dance music in the Tiergarten. The building had three separate ballrooms there that were more or less joined together; they had excellent food, and excellent champagne flowed through the night. And we waited and waited for Goering to appear, but at about 3:30 or 4 A.M. we gave up and went to our hotel. And our host called me the next morning to tell us that he regretted very much that we had left so early because Goering appeared at this party at 5 A.M. And so I didn't get to meet Goering, but I would have met him if I had stayed until 5 A.M. I have no great regrets today that I didn't meet him, and my only regret is that I waited as long as 3:30 or 4 A.M. to meet this old reprobate.

Well, as I say, I was not surprised at the outbreak of the war in September of 1939, and I followed it with very great interest--I think with greater than common interest because of the experiences I had had in London and Berlin and Munich and other places the preceding fall, when Chamberlain was misled to believe that they could trust Hitler. Well, the war blitzes, as you know, continued. During the early phase of the war, you remember, Russia was brought in with Joachim Ribbentrop and the Nazis, and they invaded Finland.

Two or three days after this invasion began and the Finns were putting up a very courageous and brave front, I met Ernest Carroll Moore on the campus. He stopped me and said, "I tell you, Knudsen, those Finns are indomitable. Nothing can defeat them! The Russians will be turned back!" He was always very positive about statements of this sort and in making predictions of this nature. This is one I happen to remember, about the Finns. Well, of course, the Finns were overrun in very short order by this juggernaut that the Russians had, and it was utterly absurd.

Well, the war came, you know; this blitz of the fall of 1939 was followed by the unbelievable quiet of the winter of 1939-1940, and the French felt that their Maginot Line was impenetrable and that the Germans would certainly find something that they couldn't break down when they came to the Maginot Line. You know, the Maginot Line melted like butter and the Nazis marched on. And during commencement of that year, early June, I guess it was, of 1940, I was sitting beside Ernest Carroll Moore at the commencement exercises in Hollywood Bowl, and at that date it just looked like France was ready to give up completely. And I remember Ernest Carroll Moore patting me on the knee and whispering to me, "Weren't those French magnificent?" And really they were magnificent in their tumbling down to get out of the way of this massive attack which the Germans had unleashed upon France.

Well, about that same time, very soon after that you may recall, the attack on Norway and Denmark took place. And this had a profound influence on me. I was coming downstairs to breakfast that morning, when we lived near the campus here, and Morris, my son, who was then probably twelve years old, handed me the morning Times, and remarked to me, "Daddy, Norway is invaded." And goose-pimples just came out all over my body. I remember the precise words I said in possibly one of the most emotional experiences of my life. I said, "My God, they can't do that!"

This was definitely a turning point in my attitude toward the war. I just had this feeling: "They can't do this to my father's land." This is something that arouses a national pride, a national interest, and things of that sort, and I knew I had to get into the war someway. I was forty-seven years old, and it wasn't quite likely that I could do very much as a private in the armed forces, but I did feel then that, well, I could get into the scientific-technological aspects of this war. I thought I could do more good there than in any other way.

I didn't have to wait very long to receive a call from [Frank] B. Jewett, who was then president of the National Academy of Sciences, and who had formerly been president of the Bell Telephone Research Laboratory.

I had met him just casually in that capacity because of my early experience at the Bell Telephone Laboratories and my continued interest in connection with the work they were doing in acoustics. I was a frequent visitor at the Bell Laboratories, and I had become more than just one of the bottom, working rank men at that time. I had been president of the Acoustical Society and had received other awards, so that F. B. Jewett knew who I was at that time. And he said, "I would like you to serve on a committee that the National Academy has been asked to set up to investigate the adequacy or the inadequacy of the U.S. Navy's program of the anti-submarine warfare."

He told me whom they expected to have on this committee. The committee was to be chairmaned by E. H. Colpitts, a name that's well-known to all radio people. There are Colpitts and Hartley vacuum tube oscillators, circuits, named after E. H. Colpitts and R. V. L. Hartley, both of whom I knew during my nineteen months' sojourn at the Bell Telephone Laboratories in 1918-1919. And so this opportunity to serve in the war program pleased me very much. Colpitts had then retired from the Bell Telephone Laboratories; I had very great respect for his competence and I had known of the unusual work he had done in the early development of thermionic vacuum tubes and applications to radio and long-distance telephony and so on, followed by distinguished administrative service at BTL. And the other members of this committee were to be William D. Coolidge, who was then vice-president in charge of research at the General Electric Company, and our own Louis B. Slichter, who was then professor of geology and geophysics at the Massachusetts Institute of Technology, myself, and the secretary was a Mr. Knox.

Knox had been formerly president or vice-president of Electrical Research Projects Incorporated. It was referred to as "ERPI." And ERPI was largely the wing of the Bell Telephone System that handled their interest in sound as it came to the motion picture industry. ERPI was very prominent in the Los Angeles area here, and I had met quite a number of their officials--but not Knox at that time--in connection with their work with the motion picture studios here and the introduction of sound techniques for recording of sound on film and sound on discs and so on, for which ERPI played a very important role during those early years.

This committee was charged with the responsibility of making an investigation of the various facilities of the U.S. Navy and related laboratories that would have anything to do with the adequacy, or the lack of it, of the Navy's program in anti-submarine warfare.

This committee got underway in the late fall, probably October or November of 1940, and we visited such naval agencies as the one at Boston (the Submarine Signal Company) where there's a prominent research staff, and other related facilities there where they manufactured submarines. We made probing visits to New London and to Washington at the Bureau of Ships and the Naval Research Laboratory, and at naval stations in Chesapeake Bay and down to Miami and Ft. Lauderdale and on to Key West.

Our visit at Key West was very interesting, because just before we visited Key West, we had gone to Woods Hole above Boston.

At Woods Hole Oceanographic Institution, two of their men, C. O. D. Izelin and one of his associates whose name eludes me right now, but I believe it was Athelstan Spilhaus, had prepared a very important memorandum on the subject of refraction of sound in the sea. And this was a very significant document to this committee, which we studied with a great deal of interest because it described how sound is refracted or bent, normally downward, in the sea; because especially in waters that are subjected to a lot of solar radiation, the upper layers of the water are much warmer than the lower layers.

The experience we had at Key West furnished an outstanding example of this type of refraction, predicted by Izelin _et al_. because the sun does beat down with a great deal of fury on the waters out of Key West. The upper temperature of the water is often as high as 80 degrees, 78 or 80 degrees Fahrenheit, whereas on the bottom it's Arctic water that's flowed down there. That's heavier, and so it settles to the bottom at near the freezing point. And so you have a change in temperature from something like 80 degrees at the surface down to almost 32 degrees at the bottom. And this is a very great temperature gradient, and such a temperature gradient means that the sound is going to be greatly refracted.

I can explain the refraction very simply. If you think of my hand up in the air here and the tips of my finger being the top of the ocean and the bottom of my hand, say, the bottom of the ocean, then sound travels faster in warm water than it does in cold water. And so a beam of sound coming out this way-- even though it's a shallow beam there--is the gradient that counts, that is, how much does the temperature change per meter or per yard of depth as you go down? It's getting colder as you go down, and therefore the upper part of this beam is going to travel faster than the lower part, and therefore as it advances toward you it bends down in this fashion because of the downward refraction. It's just as simple as that, that you would have a refraction. The paper Izelin and his associate had prepared at Woods Hole developed a formula for calculating the refraction, and for predicting how far a beam of sound would be deflected downward, say, after it's gone 100 yards, 500 yards, 1,000 yards or 2,000 yards. If you know what the temperature gradient is you can calculate just how much this sound will be bent or refracted down into the ocean.

Well, the Navy had made preparations for our visit to Key West and this was certainly a very important part of our survey. They had prepared to have us listen, both on a destroyer and on a submarine, in which they conducted some of their search techniques for locating a submerged submarine. We were briefed somewhat on what they were doing out there, and they described some of the problems they had in detecting a submerged submarine. They said, "Well, you know, in the morning we can detect a submarine at periscope depth at a range of 2,000 yards, until eight or nine o'clock in the morning. At ten o'clock the range begins to diminish somewhat to 1,500 yards; at noon it's down to 800 yards; at one o'clock it's down to 500 yards; at three o'clock it's practically nothing, 100 yards or less." And we asked, "How do you explain that?" He said, "Well, we don't explain it. We refer to it as the 'afternoon effect.'" And this was the advice we got from these naval officers who were conducting training operations for training submarine officers or anti-submarine (destroyer) officers in locating a submarine.

Well, I remember we were observers on either the submarine or destroyer. There were five of us on the committee making the survey. On one occasion, all five of us were on the destroyer when its officers lost contact with the submarine. Yet, we could hear the submarine without any detecting gear at all when it was passing under us. [laughter] This submarine was actually passing under the destroyer and the echo ranging had lost contact with it. Because of refraction, the beam of echo ranging sound passed under the target submarine, even when the destroyer was at close range. This event occurred in the afternoon when the temperature gradient in the upper layers of the water was very large, so that you just couldn't detect the submarine at any distance at all. I remember one of the officers on the destroyer at this time remarked, "Well, they're directly under us now. Should we give them the bomb now or should we let them survive a while?" They detected it not at all by their gear; but it was perfectly obvious from the commotion that was taking place there that they were passing over the submarine. They were using an echo ranging method, you see. They were shooting out a beam which was echoed back again.

You asked if this had been explored somewhat. Well, quite a lot of exploration had been done toward the end of World War I and even after, particularly in the North Sea. The Germans and the French had both done some work here. Langevin, a French physicist, had developed a suitable "sandwich," as it was called. It was a piezoelectric crystal that generated a beam of sound, high-frequency sound, ultrasonic sound, that would send sound out in a narrow beam and, if directed at the submarine, they would get the echo from the submarine. This was a technique that had been proposed toward the end of World War I, but I don't believe it was used at all as early as that.

It had subsequently been worked on by the Germans, and especially by the British; the British version was called "Asdic." The letters "A" and "S" stand for anti-submarine, "D" for detection, something. But it was referred to always as the "asdic." It was an asdic type of equipment that this destroyer used for detecting submarines, and they were using them for training our submarine officers and crews for detecting submarines, using the British asdic, which was an echo-ranging device, a sonar device as we say today.

Well, this, of course, in the light of what we had learned from the report at Woods Hole, which had indicated how the temperature gradient actually does affect the refraction of sound in the ocean, was a high spot in our survey. It referred to this as an "afternoon effect" instead of actually calculating how much the sound rays would be refracted, and therefore how much they would be deflected. This became really a very important step in the development of a real anti-submarine warfare program. And this had a relation to research work that was later done at San Diego out of what is now known as the U.S. Navy Electronics Laboratory where Dr. Delsasso and I were working together during the early stages of World War II, and where Delsasso served as a senior naval officer until after the war.

MINK: I think I had asked you the question about a lack of communication between the actual research that was going on at such facilities as the Naval Research Laboratory and the practice in the field. This relates to this question.

KNUDSEN: Well, the total problem of lack of communications between the people really who understood these problems and the naval officers and others who were practicing the art dates back really to what happened immediately after World War I. Although there were by no means the numbers of civilians involved in advising the military about technological problems in World War I, there were quite a number of civilian people involved. R. A. Millikan, for example, was in charge of much of the military research and development at that time--and others.

DELSASSO: Louis Slichter?

KNUDSEN: Yes, Louis Slichter was with Max Mason on anti-submarine warfare programs at that time in developing detectors. W. D. Coolidge, too, whom I referred to as a member of this committee, had worked with this group, and they had developed what is known as a "Coolidge tube." There are two Coolidge tubes. One is the X-ray tube and is really the basis of most of the X-ray machines that are used for diagnostic and treatment purposes in medical hospitals. The other is a submarine listening tube--that is, this device was a means of spreading your ears out. The submarine chaser was equipped with a Coolidge listening tube which consisted of two large tubes in a shaft that went down through the hull of the ship and then the tubes were spread out about six feet, and each tube was a listening tube. By means of the shaft and its handles you could rotate the pair of listening tubes back and forth until you could hear most distinctly the sound of the propellers of the sought after submarine. And so with this listening gear they'd spot the direction of the submarine and then they'd try to estimate how far it was away. Max Mason and Louis Slichter worked with this kind of detector and other kinds of detectors in World War I.

Well, as soon as World War I ended, most of these scientists and technologists who had been assisting the military forces rushed back to their laboratories, their universities, their industries, and so on, and so there was a lack of scientific and technological interest in carrying on military developments, such as those I've referred to here as taking place in the North Sea, just toward the end of World War I, and which continued somewhat. They were described, you will recall, Dr. Delsasso, in Franz Aigner's book, Unterwasserschalltechnik, a book which we used here in the training of some of our students in connection with underwater sound problems. There was obviously a lack of communication, or these naval officers at Key West certainly would have known about the effects of refraction of sound in the ocean where you have a temperature gradient, and this temperature gradient can be very large in waters that are subject to a lot of solar radiation.

The Colpitts committee then wrote its report. We had a number of sessions in which we brought together the information we had obtained. Without going into details, the committee made two principal recommendations. One, that there certainly was an inadequacy of developments to meet the threat of the German U-boat. It was obvious that the Germans had not been asleep at the switch as we had been over here. Intelligence indicated that they had been developing very good listening gear, at any rate, that they were using in their submarine program and anti-submarine program. And our committee recommended that first, there should be research in fundamental ocean physics, and especially ocean acoustics.

Sound is about the only form of energy that you can transmit for any distance in sea water. Any good conductor of electricity, as sea water is, will not transmit radio waves at all. They'll go a fraction of a wave length and that's about all. You just can't use radio waves. You can't use any kind of electromagnetic waves. And light can't go very far because the water is usually pretty nontransparent, and you can't see very far. You are restricted, therefore, to sound. And so underwater sound became a very important phase of physics in making this fundamental study of the acoustics of the ocean. Our recommendation was that there should be a real intensive effort to understand how sound does behave in the ocean, not only refraction but all other aspects of sound.

The other recommendation was that we must undertake a massive program in the development of submarine detectors and means for destroying enemy submarines. We were dependent entirely upon British developments at that time, such as their asdic gear for determining the location (the direction and distance) of enemy submarines. The British had done much more than we had. As a result of our recommendations--the National Academy of Sciences and the Navy decided that there should be set up two laboratories, one on the West Coast and one on the East Coast. And the one on the West Coast was to deal especially with the fundamental acoustics of the ocean. That was to have the highest priority in the laboratory that would be established at San Diego.

MINK: Was this established at San Diego because of the close proximity to Scripps?

KNUDSEN: This certainly had a very significant bearing because Scripps had some very first-rate scientists. Harald Sverdrup was the director of the laboratory at that time. He was a distinguished person in oceanographic problems and would readily understand these problems, and many members of his staff likewise were experts in this field: Richard Fleming, Roger Revelle, and likely others.

DELSASSO: Martin Johnson.

KNUDSEN: Martin Johnson, and also Scripps had a very efficient staff and splendid facilities for oceanic research. There were two outstanding oceanographic staffs, one at Woods Hole, Massachusetts, and the other at San Diego. And it was quite feasible, therefore, to establish the one that dealt with instrumentation, the devices for detecting submarines and so forth, on the East Coast; and it was decided that that should be established at New London.

Well, subsequently, F. B. Jewett, who was really carrying the ball for the National Academy of Sciences and also coordinating his efforts with the Navy officers, called me and said, "Would you be willing to take over the research directorship of the laboratory at San Diego, at the U.S. Navy Radio and Sound Laboratory?" That was its name then. It has subsequently been named the Navy Electronics Laboratory, and only in the Los Angeles Times yesterday there was announced a change in title again. It's divided into two parts, and this interested Delsasso and me very much because it says, "This week NEL changes its name." This is in the Los Angeles Times, Monday, July 10.

MINK: Page and column?

KNUDSEN: It's on page 1 of part II. And it said here, "NEL is known as the Naval Undersea Warfare Center as well as the Naval Command Control Communications

Laboratory." So instead of the Navy Electronics Laboratory, or the initials NEL, which we have used for many years now since. . .

DELSASSO: Since the end of the war.

KNUDSEN: Since the end of the war. They changed the name to the Navy Electronics Laboratory. Now it has these two names. It's been divided into two new commands, one of which again is "Naval Undersea Warfare Center" and the other is "Naval Command Control Communications Laboratory." And that looks to me as though there will be more control by the Navy than probably they've had in the past.

DELSASSO: Well, I think there's one unit (the Undersea Warfare Center), at any rate, that will be largely civilian.

KNUDSEN: Yes. Dr. Delsasso, I think, should enter the discussion at this point. [tape off] Dr. Delsasso will describe how he was called to duty at this time; but he and I had worked together on acoustical problems for a long time, so it was a rather propitious arrangement that developed in early 1941. And Delsasso, you might tell about your going down to the Navy laboratory, and then I'll tell about my going there.

DELSASSO: Well, early in 1941 we were working here at the university on high-intensity sounds, and it was during that work that I was asked by the Navy if I would go down to the laboratory. They don't usually ask you, but in this case the war hadn't started, and they asked me if I would go down there to work on underwater sound, because they were branching out and developing this laboratory. This was before war broke out. And after talking to all the wise people around here, it looked as though things were going to happen, and so I went down in January, 1941. The laboratory then consisted of just a single small building and was intended only to service radio, and a bit of it was underwater sound. There was very little available at the time; and at that time there was no radar at all. And so I reported there in 1941.

MINK: You were called to active duty?

DELSASSO: Active duty.

MINK: You were put into the Navy in other words?

DELSASSO: Yes.

MINK: Had you been in the Navy before?

DELSASSO: I was in World War I, and I was in the reserve in the intervening time and had spent a lot of my time on practice cruises on the underwater sound and worked on some devices for measuring the ocean depths. I made, I think, some of the first measurements that were made down to a depth of 5,000 fathoms, about 30,000 feet, so that this was not quite new to me. I knew pretty much what the Navy was doing at the time, and

I was given the job of working on this program.

KNUDSEN: I think it would be worthwhile for you to describe your depth sounder just a little, don't you?

MINK: Yes.

KNUDSEN: Because it was one of the early instruments that was used for navigation.

DELSASSO: Well, this was made possible by a grant from the Bureau of Ships of $500 to work on this depth finder.

MINK: When did you receive this grant?

DELSASSO: In 1924. And by 1925 I had finished making it and used it with the existing equipment on the battleship Maryland, to go to Australia on a goodwill cruise and make practice runs with it.

KNUDSEN: I think it's interesting to interrupt to say that Leo Delsasso was an upper-division student in college at the Southern Branch on the Vermont Avenue campus at this time. He was also an assistant. He taught, but he was also an undergraduate student and had this dual capacity. But I think it is of interest that while still an undergraduate student, he was inventing a device here for measuring the depth of the ocean and making a continuous record of it.

MINK: Dr. Delsasso, will you describe the device?

DELSASSO: Yes. First, I'll have to say a word about what equipment existed on these battleships at the time. It was a modification or a refinement of what

Dr. Knudsen has already pointed out they used in World War I. Instead of just rubber bubbles on the end of a pipe and an ordinary stethoscope, entirely an acoustical device, this one had hydrophones located along the bow of the battleship and electrical connections to a so-called compensator to vary the time of arrival of the sound, and it had electrical headphones to listen with. This was used also for making rough measurements of ocean depths by timing the interval between the production of a very intense sound in the bottom of the ship and the reflection from the ocean floor. The velocity is about 4,800 feet a second and from that and the elapsed time for the echo to return, you could measure the depth. Well, what I did was to just utilize this equipment and extend it to provide a continuous record, which was done by amplifying the returning sound to the extent that it would produce a spark between two electrodes, a rotating needle point and a knife edge, a circular knife edge, and between these two electrodes a dark piece of paper passed and the spark perforated the paper at the positions where the echo came back. The sound was sent out at one particular point at each revolution, and when the echo came back, the pointer would be in a different position and it would mark the beginning of the reflections. We took records off of Guam and off of

Samoa and New Zealand and Australia, and I think this was the first time that continuous graphic records of ocean depths were obtained. Other methods of measuring this time had been developed previous to this. [tape off]

KNUDSEN: I had just related that I had been invited to become the, I guess, research director. It's a question about title that I think will be clarified by a little later discussion on this subject, because the naval officer in command of the station at San Diego was a captain and had the title "director of the laboratory." This was a naval establishment, and the intention therefore was to perform a marriage--possibly an illegitimate marriage at that time--between a civilian group, of which I was to be the director, and the naval group, of which Captain Ruble, who was Delsasso's boss and was laboratory director. So there was the beginning of a conflict of interest there between two persons who were more or less directors at the same institution, and you can imagine that there might have developed some frictions there. That's a mild word, I think, to use in connection with this experiment, really, the working together of a naval officer group and civilians.

Well, Dr. Jewett explained to me that they were negotiating with the University of California to be the sponsor, you see, for this work. And so the organization that I worked with was still a part of the University of California. It was called the University of California Division of War Research. And this was a part of the National Defense Research Committee's program, which was guided by a committee in Washington, made up of civilians again. Vannevar Bush of MIT was the head of it, and James B. Conant of Harvard was active in the organization, and other men of that distinction were at the head of the National Defense Research Committee, NDRC as it was called at that time, and later was called the Office of Scientific Research and Development, OSRD. Those were the titles that we were associated with, and so I was a part of the National Defense Research Committee as the administrative officer for this program for which the University of California gave overall guidance. That is, it was a contract very much like the contract for the Livermore Laboratory or the Berkeley Radiation Laboratory.

MINK: Los Alamos?

KNUDSEN: This included Los Alamos later and other contractural programs that are continued activities of the University of California under contract with the federal government. So my contract was really with the University of California, because the University of California was strictly my boss there, and so I didn't change employers; but I changed employment a great deal in going down to this environment. [laughter] It was quite different from the sheltered life we had had here at the university. And my first job was to recruit a staff; and I think this probably was the most important thing I accomplished while I was there. And so I was out on the road a great deal finding suitable men to bring into the laboratory.

In this connection, we received a great deal of valuable help from Ernest O. Lawrence who was then at his laboratory, of course, at Berkeley. This was before the Manhattan Project for the development of the atomic bomb had got underway. So Ernest Lawrence really came to our laboratory on several occasions during the first few months we were there and gave very good advice about men we could recruit, and particularly some from the Berkeley campus, where we got Professor Jenkins, who was one of his colleagues there, and two other younger men who came with him.

DELSASSO: Byron Wright.

KNUDSEN: Byron Wright, who is here now. [laughter]

DELSASSO: Chairman of our department.

KNUDSEN: Yes, he's chairman of our department, acting chairman of the department here during the past year, and one other--Tilles?

DELSASSO: Tilles, yes.

KNUDSEN: I believe it was Israel Tilles. I regretfully recall that he and a young astrophysicist (also from Berkeley)--I believe his name was White--lost their lives in a helicopter experiment in 1942. And a few others came really from Berkeley. Ernest Lawrence actually participated in our planning of the program in those first days. And he also was trying to establish peace between the naval director of the laboratory (Captain Ruble) and the University of California's director of the civilian group at the laboratory.

MINK: Well this interests me, and maybe you don't want to talk about that too much, but it is a case in point, and I suppose it exists in other places, too.

DELSASSO: Oh, yes.

MINK: What specifically were the problems that you had to face?

KNUDSEN: Well, let me first discuss this recruitment problem, then we'll come to that, because there were no frictions in the matter of recruitment, really. Captain Ruble was to provide space for these people and was doing a good job, really, of the housekeeping and providing suitable space for such members as I mentioned: Ernest Lawrence and Professor Jenkins and Professor Wright and many others of that type. Early, we recruited L. J. Sivian, from the Bell Telephone

Laboratories, who was one of their most competent men in the fields of acoustics and electro-acoustics. He had done some extraordinary work in--well, even in the attenuation of sound in the air and of propagation of sound in liquids and in development of electro-acoustical devices there. Harvey Fletcher, who was his boss, said, "Well, do you believe, as I do, that Sivian is the best man in his field at Bell Laboratories?" And Delsasso and I agree that he was. He was like the rest of us--eager to get into this acoustical research to contribute toward the winning of the war. This was certainly a very strong motive in the minds and hearts of all of us who participated in this early program.

Another early recruit was a former colleage of mine at the University of Chicago. We were graduate students together and had become very good friends. He was Karl S. Van Dyke, who was at Middletown, Connecticut, Professor of Physics at Wesleyan University. He had worked closely with Professor Walter G. Cady there with quartz crystals and the application of crystals to many, many problems. A quartz crystal was the heart of the Asdic equipment that the British were using, and so Karl Van Dyke was a natural to work into this particular type of work. Sivian and Van Dyke and I were closely associated. They were made associate directors of the civilian group at the laboratory. We must make that distinction all along here. Well, the program got underway with Sivian and Van Dyke and with Ernest Lawrence and others who were consulting with us at that time.

DELSASSO: Carl Eyring.

KNUDSEN: We must mention Carl Eyring who was one of the most distinguished of our early workers here, a grand person, outstanding in acoustics, from Brigham Young University. It was natural for me, of course, to want to have Carl Eyring here. He had done some very distinguished work at the Bell Telephone Laboratories over a period of two years in the field of architectural acoustics, and so he was trained in that field. But probably the most elite of the members we recruited in the first few months was Professor Carl Eckart (outstanding in both theoretical and experimental physics) of the University of Chicago. He stayed on at the University of California after the project was terminated at Point Loma near San Diego, and has served in various capacities at Scripps Institution and the University of California and now is an important part of the University of California at San Diego, as vice-chancellor of academic affairs. So he's had much to do, as much perhaps as anyone, along with Roger Revelle, in the recruitment of a high caliber staff for the University of California at San Diego. And as most people know, this part of the university really began at the top. They developed a graduate division before anything else. And Carl Eckart played an important part with Roger Revelle and others, in developing this part of the university's program at San Diego. And he was at that time and has continued to be, I think, one of our ranking physicists in the country. He's thoroughly competent, both in theoretical and in experimental physics, and his services were especially valuable at that time. In consulting capacities, though, we drew on such men as Paul Epstein and Harry Bateman at Cal Tech.

DELSASSO: Jacobsen.

KNUDSEN: Dr. [Lydik S.] Jacobsen came from Stanford. He was one of their most distinguished men in dealing with earthquake vibration problems. And we had this kind of staff working together there in a matter of a few months, and I think it was a very, very distinguished group of physicists--mostly physicists, mathematicians, and then a few outstanding engineers among them, one of whom was Louis Statham, president of Statham Instruments. And with this group there, we more or less formulated an early program. Some of the things we outlined are of course an extension of this work on measurement of temperature gradients in the ocean.

Among the other programs was the propagation of sound in the ocean. How does sound propagate itself? How is it affected by the bottom conditions of the ocean? And what happens to sound as it reverberates in the ocean? Why does it reverberate? It's very much like a lightning flash in the atmosphere. You have scattering from the inhomogeneities in the air and you get thunder as a result as it rolls on, reverberates on. The ocean is full of scatterers of the same type, so this reverberation interfered very much with the performance of Asdics, of sonar gear, and so on. We decided we had to know more about the scattering of sound in the ocean, about the way sound is reflected by these inhomogeneities, what reverberation actually consists of, and how it depends on the various parameters of the sea.

A program of that nature was laid out, and some of the early discoveries that were made I think are worth mentioning at this point. One especially important discovery is called the "deep layer scattering." Professor Eyring, to whom I have alluded, Dr. Russell W. Raitt, recruited from Cal Tech, and Dr. Ralph Christiansen was another recruited through the help of Ernest Lawrence. He had been trained as a PhD in physics at Berkeley and was then an assistant professor at San Mateo Junior College.

DELSASSO: And he is now the chief scientist at the laboratory.

KNUDSEN: And he is now the chief scientist there. He stayed on at this laboratory throughout the years and has risen in the ranks until now he is the chief scientist. That's the title they use now. It's the equivalent of the director of the civilian group. This deep layer scattering has turned out to be a very fundamental discovery in marine biology.

SECTION XXIII

KNUDSEN: The discovery was made when this distinguished team were investigating reverberation in the ocean. In the process they noticed that they were getting rather peculiar reflections from a deep layer. It came back as a prominent but peculiar echo. They could determine how deep this layer was, and it moved up and down diurnally, day after day. Do you remember, Leo, when it was at its most shallow depth?

DELSASSO: No. I don't, but I have the picture here.

KNUDSEN: It varies daily, and it varies seasonally. And we will supply a little further information as Dr. Delsasso looks this up, about how this layer moves up and down during a twenty-four-hour period; but it's a function again of temperature. And this phenomenon has received a great deal of scientific attention throughout the world since this discovery was made by these three physicists; it's always referred to as deep layer scattering. And it's been found to result from organic life in the ocean, made up of little organisms and medium-sized organisms and of little fish and bigger fish. They're all together there. The bigger ones are feeding on the little ones, and they move up and down together diurnally.

A lot of work has been done to really ascertain that there were these small, medium, and large objects in the scattering layer, and they have been identified as actually living organisms. This has received a great deal of study by biological oceanographers throughout the world. I know a Los Angeles man whose name is Giles Mead, professor of marine biology at Harvard University, and the last time I talked to him he had just returned from the Indian Ocean where he headed a group studying this deep layer scattering. There has been much in the literature of oceanography that has dealt with this, and they took a long time to really decide what it was. At first, all they knew was that there was something down there that was moving up and down vertically and diurnally, and that it was reflecting a lot of sound. It was reflecting more sound than they were getting from horizontal distances and other sources. And yet this was not from the bottom. It was just down 100 meters or 100 fathoms, then sometimes it would rise above that.

DELSASSO: This is in the next book.

MINK: Did your team ascertain only that this scattering came from a layer?

KNUDSEN: They didn't know what it was at this time. They weren't sure. They suspected that it was living organisms, but no one was quite sure. It took several years of research, many years, and it is still being researched, because the kind of scattering that you get is a function of the size of the scatterers. This is not an easy problem, but it's been worked on a great deal, and I think the composite opinion now is that these are made up, as I mentioned, of organisms of many kinds and many sizes.

DELSASSO: Including some very small fish.

KNUDSEN: Including some small fish and some large fish.

DELSASSO: As a matter of fact, one PhD was given for just this, putting the sound source down, an electric spark down in the deep water in this layer, and measuring the sound that was reflected from some of these small fish in the vicinity.

MINK: This was a PhD at La Jolla?

DELSASSO: No, here. It was done here because at that time they weren't giving PhDs down there. But the work was done there.

KNUDSEN: Well, another important contribution to marine biology resulted from the investigation of the sounds produced by marine animals in the sea. Martin W. Johnson of the Scripps Institution of Oceanography was in charge of that program and did a lot of magnetic tape recordings of these various sounds. We were interested in them because they interfere with sonar. There was especially a type of sound produced by snapping shrimp that are found in waters that are warm enough. The waters around San Diego are warm enough so that there were large numbers of these snapping shrimp; there were even more of them in the warmer waters of Key West.

These snapping shrimp, it was found, produced very high frequency sounds, much of them in the ultrasonic range --that is, frequencies of 10, 20, 30, 40, even 50,000 cycles a second, whereas 20,000 is about the upper limit of audibility. And these high frequencies were right in the very range of the Asdic and similar sonar gear using echo ranging techniques. The sounds of snapping shrimp were a serious masking sound that would limit very much the distance at which you could get distinct echoes from a submarine.

This was of extreme practical importance in respect to how far you could detect a submarine in waters that are infested--maybe that's not the right word--with snapping shrimp. They're not edible, at any rate, but they're certainly a noise-making type. They make the noise with their claw, snapping this way. It's very much like the sound you make by snapping your fingernails together this way. Or it resembles static on a radio, or the sound produced by frying fat in a hot skillet. They were crackling, hissing sounds, and when billions of these are contributing their little bits of crackling sounds, this noise can become very, very intense. I'll speak about that a little later because we made some measurements of it.

Martin Johnson's name is very prominently identified with the investigation of the sounds from snapping shrimp and from other marine animals. The second most important one, so far as the anti-submarine warfare program is concerned, is the croaker. There are croakers on the West Coast. They are most numerous, however, in the Chesapeake Bay. And the name is derived from the croaking sound that they produce. When you catch croakers at sea, when you first land them, you hear them croak even in the air. But when millions or hundreds of millions of these are in the Chesapeake Bay the chorus that they produce is almost deafening. We likened it at the time, I remember, to the noise on Forty-Second and Broadway in New York, the traffic noise there. This was a terrific noise. As a matter of fact, it was so great that a German submarine could have entered Chesapeake Bay and never have been detected by listening gear, because the noise of the croakers would have completely masked this sound.

I remember that the croaker sound has a diurnal effect, and this is something that every fisherman likes to refer to, namely, that the croakers produce a maximum noise a half hour before sunrise and a half hour after sunset. And this is the time they're out feeding, and they make the most noise when they're out feeding. There also is a seasonal variation. This croaker noise is a maximum in May and June, which is the mating season. That is, it's the season when they do the equivalent of mating, at any rate. And they're out thinking about propagating themselves, and they make plenty of noise about it. Now when you superpose this noise of May and June in the mating season on the diurnal variation at morning and night, you have some noises that do actually resemble, in the underwater, the sounds that you might have from the worst traffic conditions in New York or Los Angeles, places like that.

Well, the principal type of research which Eyring, Christiansen, and Raitt pioneered was the investigation of reverberation. The deep layer scattering was just one aspect they discovered as a result of their investigation of the reverberation of sound in the sea. They made important contributions to the basic nature of reverberation of sound in the oceans and its similarity, say, to the kind of reverberation that you might get in a forest or in the open air, which is quite different from the kind of reverberation that you get in a room where the boundaries of the room and the volume of the room (and the humidity and temperature of the air) determine how that sound reverberates.

Well, other basic investigations and important discoveries were made that deal with the attenuation of sound in sea water. This, you see, is important in determining how far you can detect a submarine or detect sounds in the sea. If you don't have the limitation of refraction that we talked about, then at these high frequencies the attenuation itself, the conversion of the energy into heat, can be a significant factor, and important discoveries were made there. Later, discoveries were made by Leonard N. Liebermann, who was part of the group. When did Liebermann join the group? Was he there during the war, or was it just after the war?

DELSASSO: Just at the end of the war.

KNUDSEN: He came toward the end of the war and was associated with the Scripps Institution and the Navy Electronics Laboratory.

DELSASSO: And the Marine Physical Laboratory.

KNUDSEN: Yes. It was called the "Marine Physical Laboratory," which was also operated by the University of California at this time. But this was in peace time that Liebermann did his work on the attenuation of sound in sea water. It was becoming generally known by the measurements that were made not only at San Diego but all over the Western world, including measurements which the Japanese had made earlier, that the attenuation in sea water is greater than the attenuation in fresh water. And Liebermann's work at San Diego and the work of Professor Robert W. Leonard of our laboratory, who died here a short time ago, explained why this was. It was a result of a relaxation effect--that is, the conversion of energy, a transition of energy from one of the salts that's in the water reacting with the water itself. The salt responsible for the extra attenuation was magnesium sulfate; that's one of the salts, a prominent salt in sea water. The process of this molecular attenuation is similar to the process we described for the attenuation of sound in air. This is a discovery that came from the University of California laboratory that Liebermann worked in down there, and from experiments that Professor Leonard conducted here in our acoustics laboratory. Dr. Leonard devised a large spherical bulb filled with water in which he set up various sound vibrations, and then obtained a visual record of how this sound reverberated. Thus, he could actually measure the attenuation in the water with different amounts of magnesium sulfate added to it.

Well, these are some of the principal discoveries that came from the underwater research at San Diego. Later, more developments in connection with sonar and training devices were developed at the laboratory, but I should mention at this point that I didn't remain at the laboratory very long. Soon after I arrived there conflicts developed between the civilian group, on the one hand, and Captain Ruble on the other, over such trivial things--for example, as how we were going to mount the equipment that we were going to experiment with.

The physicists and the laboratory people that we had recruited from universities were accustomed to using what they called "breadboard techniques." That makes use of a big piece of board, a good, nice, smooth piece of board on which you mount your equipment. You've got everything under observation, and you can readily move things around; it isn't "shipshape." And this was the beginning of a conflict with Ruble. He objected very much to what he regarded as a sloppy, haphazard method of conducting research, and he felt that the Navy's insistence upon following very definitely shipshape rules was a procedure that was going to be followed in his laboratory. "By God," he said, "they weren't going to get away with these slipshod methods in his laboratory." And so this was one of the beginnings of conflicts.

Then Professor Eyring established a series of seminars. He hoped very much that this would bring together our civilian scientists and Captain Ruble and some of his officers. We didn't worry too much about Delsasso; he was on our side all the time. But Ruble certainly was not, and Eyring, who was a very tactful person, invited Ruble to give the first lecture at these seminars. And Ruble's lecture dealt with the importance of "higher mathematics" in the kind of research we were going to do. His conception of higher mathematics was what he'd learned as a naval midshipman, I guess, when he was at Annapolis. His higher mathematics was spherical trigonometry, so he was lecturing these professors of physics and mathematics and engineering on the importance of higher mathematics. He used trigonometry as an example of how important it was that we use these techniques in the kind of research we were expected to do. Well, Delsasso and I have often commented that it would be delightful if we had a tape recording of this captain lecturing these distinguished scientists, physicists, mathematicians, and engineers on the importance of higher mathematics in the kind of research they were to do for the Navy.

Unfortunately the conflicts became more and more acute. Ernest Lawrence, who devoted much of his time with us during the summer and fall of 1941, tried to resolve the differences at one time by telling Captain Ruble how lucky he was to have these distinguished people around there. And he said, "You're the rank of captain. The position that Dr. Knudsen occupies at the University of California would be the equivalent, anyway, of a vice-admiral." [laughter] And the sparks flew between Ruble and Ernest O. Lawrence on that occasion. [laughter] I think Ruble hated Ernest Lawrence's appearance at the laboratory every time after Ernest Lawrence had given him this dressing-down.

Well, things got worse, especially at the time of Pearl Harbor. Leo may have something to say further about that. But I was in Los Angeles on Sunday, December 7, 1941, and early in the morning we got this first news, that Pearl Harbor had been attacked, that the Arizona and other capital ships had been sunk, that the loss of life was terrifying, and that the airfields had been bombed so thoroughly that we lost nearly all our airplanes on Hickam Field. Mrs. Knudsen and I had accepted an invitation to attend a party in the San Fernando Valley that afternoon. Of course, the party was very much concerned with only one thing, listening to the latest dispatches on the radio. I returned to San Diego on a late train that night, and I think you, Leo, had been detailed to keep the watch that night.

DELSASSO: I was here, incidentally, throwing boomerangs with Dr. Leonard when the first reports of the bombing were announced.

KNUDSEN: Oh, yes. On that Sunday.

DELSASSO: On that Sunday, and I heard the news, so I took Dr. Adams with me, and we went back early in the afternoon. I had the watch that night.

KNUDSEN: Yes. Among the distinguished people we recruited for the laboratory, I neglected to mention Dr. John Mead Adams. He was one of our recruits down there and helped especially in keeping a sort of historical record of what was going on. And besides, he was in on the discussions of our program, and he was a very helpful adviser.

MINK: He had retired at this point?

KNUDSEN: No. He was on leave like the rest of us, from our active duties here. Delsasso and Adams and I, three of us from our Department of Physics, were together there. We shared an apartment together. It was about the only apartment that was for rent, I guess, in all of Point Loma at that time.

Well, Pearl Harbor, I think, brought on other problems. I remember a comment that Captain Ruble had made about how little we needed to be concerned about the Japanese. Before Pearl Harbor, even, there had been rumors about the development of the Japanese Navy, and that they were really becoming a threat in the Asiatic waters over there. Ruble pooh-poohed this idea very much. He said, "Why, they'd have to drop a bomb down the <u>funnel</u> of one of our capital ships before it could do any damage at all." And two or three days after Pearl Harbor we got the reports of the loss of several of the capital ships of Great Britain, some of their finest ships in those waters. They were wiped out by the Japanese aircraft bombing. And we received reports at the laboratory that a part of the Japanese fleet was moving up and down our California coast here. And they actually fired a projectile of some sort I believe somewhere up by Ventura.

DELSASSO: Goleta.

KNUDSEN: By Goleta, yes. By Goleta near Santa Barbara. That was one.

DELSASSO: From a submarine.

KNUDSEN: One Japanese submarine paid that token [laughter] respect to the United States after Pearl Harbor. And this happened within a week or two after Pearl Harbor. They were over here. Well, the lack of preparedness that existed along our coastline here, I think, was demonstrated by the fact that we had a blackout at San Diego that next night. And I think you had the watch maybe the next night. You had the watch one night, I know, very early there at the laboratory. And we were told--I think this is true, and you can correct me, Del, if I'm wrong--that there was only one .50 caliber anti-aircraft machine gun available on Point Loma at that time.

DELSASSO: It was certainly very, very limited and no radar at all, at that time.

KNUDSEN: And there was real anxiety that the Japanese could effect a successful landing near Point Loma or San Diego or someplace there where we were utterly unprepared to withstand such a landing. I know we got rumors about the Japanese ships back and forth, and I know Ruble was very much worried at this time about the possibility of something like this happening. This, of course, was an excuse to enforce more strictly naval regulations, and Ruble became much more insistent upon observance of all sorts of rules and regulations. For example, some of our staff members were required to go to Honolulu to try to use an optical method that R. W. Wood and Jenkins and Wright had worked on. Who, besides Jenkins and Wright, went on that mission? Those two, I remember, Maybe there was a third man--Tilles?

DELSASSO: Tilles was on it, I think.

KNUDSEN: The three men had come to us from Berkeley, and all of them were experts in optics. R. W. Wood, who was among the top men in the country in optics, had spent some time with us, and particularly with Jenkins and his two assistants in exploring the possibility of detecting submarines by seeing them through the water. How transparent is the water? How far can you see a submerged submarine? Well, of course, it depends a great deal upon the transparency of the water. If the water is free from turgidity, why then, you can see a hundred feet, at any rate, or 200 or 300 feet under favorable conditions. But if the water is roilled, why, you can't see very far.

Well, Ruble was requested to send a team of our scientists to go over and see if they could explore the damage which had been done to some of our capital ships that had been sunk at Pearl Harbor. This again was a source of the tightening of regulations that we had, and you may recall, Del, some of the things that happened in connection with the departure of these three men for that trip.

DELSASSO: Well, I was ordered to get them some money and get them aboard ship, and this I did. I remember you offering to help in any way you could and being firmly told by Ruble that there wasn't anything you could do.

KNUDSEN: Yes. Ruble took over. This was an order of the Navy, you see, to go over there. This, I am sure, affected his attitude toward the group, that by God he was going to take charge of this group.

I think there's one incident here that should be in the record. He was often referred to by people who knew him as "Red Ruble." He had red hair. And his face would always turn very, very deep red whenever his emotions were aroused. And he had the reputation, one, of being a very successful taskmaster in training some of the radio crews that had been at sea, and he had been cited for excellence as an officer in getting discipline and things of that sort, with performance, out of his officers and men on one of these ships in the Pacific. But he had the reputation also of being a very, very hard man to get along with. And a young officer--what was he, a junior lieutenant--had been assigned to the laboratory down there under Ruble's command. As is customary, he made his courtesy call on Captain Ruble, and Captain Ruble can turn on the charm and really be a very delightful person. We've entertained him in our home here on several occasions, and socially he was a very delightful person. And my son Morris, who was living with us at that time, I think, brought luncheon meals from someplace in San Diego before we had . . .

DELSASSO: Box lunches.

KNUDSEN: Box lunches. He brought them from someplace in San Diego and distributed them at cost to our staff members in the earliest days there. An attachment developed between Morris and Captain Ruble. Morris, I think, was his caddie when Captain Ruble went out to play golf. Well, this young junior lieutenant had called on Ruble, and Ruble turned on his usual charm. And the young man had heard also that Ruble was a very difficult man to get along with. In leaving, the young man made the mistake of saying, "Well, Captain Ruble, I think you and I are going to get along very well." And Ruble sternly came to attention and his face grew red and he said, "You're goddamned right we're going to get along very well, and you're going to do the getting along." [laughter]. Well, excuse me, but I thought that story probably should be included.

These differences, I think, brought on certain anxieties between naval officers who were superior to Ruble and the OSRD officers in Washington who were superior to me. And they felt, well, possibly we should make a change in the directorship of the laboratory there, and it was decided that I should go on a mission to Great Britain, which was a rather important mission, and it was an honorable way, I guess, of making a change in command there of this civilian group. At any rate, I accepted the suggestion of my two superior officers, of Professor John Tate, professor of physics at the University of Minnesota, who had come to the OSRD office in New York, and Dr. E. H. Colpitts, who were in charge of what was called "Division 6.1," that had charge of the entire civilian program in helping the Navy in the matter of anti-submarine warfare. And it was decided, therefore, that Gaylord Harnwell, who was then professor of physics at the University of Pennsylvania and is now (1967) president of the University of Pennsylvania, should take over the directorship of the civilian group. Harnwell relieved me possibly around May 1, 1941.

DELSASSO: In 1942.

KNUDSEN: Excuse me, May of 1942. Thank you. Of course, I had been there nearly a year at the time I left and went to New York for briefings and other places to get information. I was to exchange information through OSRD with the British. Such exchanges were customary and important. Dr. Tate had been on such a mission as this before, and Louis Slichter had also been on such a mission. I was a logical candidate because I had this experience with the San Diego laboratory and a lifetime of work in acoustics, so I was considered a suitable person to go over there and perform this exchange mission; and I was pleased with the choice of Dr. Harnwell as my successor at

San Diego.

I think the manner of getting to Great Britain is something worth mentioning at this point. After having been briefed by several of the naval officers at our own laboratory at San Diego, New London, Key West and other places, I made detailed notes on these various briefings that would facilitate useful exchanges with the British anti-submarine research establishments. They had their principal establishment then at Fairlie, which is about 40 miles south of Glasgow. And, of course, there were other establishments also around London, including our own OSRD office in London. One of my missions was to examine an important submarine that had been captured by aircraft off Halifax, Canada--the U-540 it's called. This German submarine was in dock at one of the harbors on the west coast of England. It was one of the reasons for my going over there, because this submarine had sonic gear on it that our people wanted to know about. What was on this U-540 that had been captured by the British off Halifax? It had been taken over to England for careful study of all of its battle equipment and its detecting gear and so on. The detecting gear was of special interest, of course, to our program in the United States.

I had prepared myself for this mission--my two or three weeks of careful investigation of what was going on at our submarine stations and various laboratories--and was ready to leave, weather permitting, for Great Britain as early as May 20, 1942. It was probably May 31 or June 1, after waiting for favorable weather, before I actually got aboard a Boeing amphibian plane. There were two ways of going over. One was by surface ship, which was very hazardous at that time. We were told that the U-boats were getting 25 percent of ships that left our Atlantic seaboard bound for Europe, and so that wasn't considered a very safe mode of travel at that time. The Boeing amphibian plane was considered both faster and much safer. Well, I waited nearly ten days before weather conditions were good enough for the plane to take off from Long Island Sound. I went out to the airport four or five times but the flights were canceled because of poor weather conditions at New York or at necessary fueling stations en route. Mrs. Knudsen had been with me in New York during most of this long delay. About twenty-four hours before I actually got on a successful flight, I put her on the train to return to Los Angeles.

One night we finally made it, we were taken out and put on board the Boeing amphibian plane, at about 1 A.M. We taxied about a dozen times and then came to a stop. Conditions weren't favorable for takeoff. It was 4:30 in the morning before we actually were airborne. And the first stop was Halifax, and it was very smooth landing there. And we had our first very good meal made of lobsters that had been caught near there, and I remember how good they were. And the next stop was in Iceland. There was a good harbor there. You see, these landings had to be where the water was quiet enough to "land" or take off safely. And there were no incidents there. We didn't have to wait at either place. We were off again and in the air for the final landing of the amphibian plane on the River Shannon.

Of course, the rest of that night--that is, from 4:30 in the morning of June 1, I believe it was, or May 31--and continuing the next day and all of the next night we were in flight. We landed in the River Shannon, which is, of course, a very safe place for a landing on water. It's wide enough there so that it's very easy and it's calm. I had my first view of Ireland. What a view it was to see Ireland's emerald green as we came down. We made a safe landing there. There had been some incidents in which the Luftwaffe had attacked even civilian planes like this one that were carrying personnel across from the United States to Great Britain. As a matter of fact, most of the passengers on our ship were officers in the armed forces who were going over on various missions.

We were taken by bus from Shannon to Limerick and that was our first stop. We had breakfast at Limerick, I remember, and we composed a few limericks. I don't remember them now, [laughter] but this was one of our diversions there. Then we were put on a land-based plane at Dublin that took us over to Brighton in England, where we were met by a private special train, which took us to London on this luxurious first class train. We had good snacks and drinks on the way to London. We were properly met at the station in London, and that night we were admitted to temporary membership in the Athenaeum Club, which is the club that George Bernard Shaw and men of his type belonged to. You see, we were treated royally. Great Britain didn't think of us then as the members of the American Rebellion of which George Washington was the [laughter] principal personage on our side. And they had not been very favorable about the Americans over the years, but I assure you we were treated very, very kindly by the British on this occasion.

Interview of July 13, 1967

KNUDSEN: Before continuing with my description of the mission to Great Britain and the anti-submarine warfare program, I want to introduce a reference to a paper that was called to my attention by Professor Louis B. Slichter. We mentioned his name as a member of the committee that investigated the inadequacy of the U.S. Navy's anti-submarine warfare program. Slichter and I were talking about this at luncheon the other day, and he said, "Well, you know, I wrote a paper about the propagation of sound in the ocean as early as September, 1922." I said, "You did? I've never heard or seen reference to it." He said, "Well, it wasn't published, but it was a memorandum that I wrote for the Submarine Signal Company." He said, "If you'd like it, I'll send you a copy." And I have a copy here, and I would like to refer to one or two things because I think it established an earlier priority than the paper I referred to when we discussed the committee of which Slichter was a member.

This Slichter memorandum is entitled, "The Economic Limits of Submarine Sound Propagation." In this memo he calculates first the theoretical range for which sound might be propagated. This theoretical limit is determined by two things. One, if it's out in an infinitely deep ocean, then sound spreads out spherically, and you have the inverse square law. There's a diminution with the inverse square of the distance. And in addition to that there's an attenuation constant which had been worked out by Stokes. And so Slichter simply put these two things together. Franz Aigner, whose book Slichter had at the time, wrote this memorandum--is quoted as having calculated the incredibly great distances through which one might propagate sound in the ocean.

You see, the viscosity of the water is very low, and the speed of sound is very great, so the range at which sound would propagate is extraordinary. And one of the systems that's used now, is called "Sofar"--and that's an appropriate name. They have what they call channels in which the sound is propagated up and down. It is possible to propagate very low pitched sounds halfway across the Pacific.

But Aigner had made a calculation that Slichter refers to on page 2 of this memorandum that he sent me. This showed that if you're in water, say, 20 meters deep, and you had a source 10 meters deep that generated 100 watts of acoustical energy, then Aigner calculated that if you have good reflection from the bottom--and you always have good reflection from the surface when the ocean is calm--you might propagate sound very great distances. The theoretical limit would be about 146,000 miles. That, of course, is not possible in any kind of water; but even in the extreme case of spherical propagation you would be able to propagate it some 746 miles. This was calculated.

Dr. Slichter then calculated some of the ranges--the limited ranges--because of a number of things: lack of homogeneity, variations of density, but especially temperature differences. And this was the thing which we had talked about that was so important and which the officers and crew at Key West really didn't seem to know anything about. And yet this had been published in 1922, and other things had been published subsequently. Slichter says on page 5 here, "By far the most important variations in sound velocity are produced by temperature differences which are especially deleterious in the summertime when the surface layers are warmer."

Slichter actually determined the temperature gradient near the lighthouse at Boston, and he has a curve here that shows how the temperature varied there. The surface temperature near this Boston lighthouse in the summertime, September 15, 1922, when he made these measurements, was 60 degrees F. It progressed linearly down--that is, the temperature diminished as he went down. Down to a depth of 30 feet it had been reduced to 53 degrees--that is, the temperature changed

7 degrees in going down that 53 feet. Well, I think almost any diver who has gone down 10 or 15 feet in the ocean recognizes that there is a marked difference.

But Louis Slichter used this temperature gradient to calculate what the range for detecting a submarine would be in that kind of water. And he shows that the range depends somewhat on how deep you and the submarine are and the angle at which you direct the sound. Among other things here he shows that the range could be less than 250 meters, with the kind of temperature gradient you had at Boston in September of 1922. So I wanted to get that much in the record about the early work that had been done, especially inasmuch as it was done by one of our colleagues here, who is one of my most admired colleagues, Louis Slichter.

MINK: Did he publish this paper?

KNUDSEN: No. This was a memorandum for the Submarine Signal Company by whom he was employed at that time.

MINK: Would this have been available to the Navy?

KNUDSEN: Yes! Yes, I'm sure it was. As a matter of fact, it was addressed to Dr. H. B. Hayes who was then with the Submarine Signal Company but later became the director of the Naval Research Laboratory, and was director of the Naval Research Laboratory at the time we were making our survey.

MINK: This is strange, isn't it?

KNUDSEN: Very strange, yes. It's a lack of communication. It just didn't reach the officers. As I mentioned I think last time, at the end of World War I the scientists and the technologists and many others left their offices with the armed forces and rushed back to the universities and their laboratories and their industries and so on, and things were at a standstill essentially between the end of World War I and the beginning of World War II. [tape off]

MINK: You are now going to continue talking about your work in England.

KNUDSEN: Yes. One of the highest priority missions I was supposed to perform in Great Britain was to get information about the sound detecting gear on the German U-boat, U-540, which had been captured. I believe I mentioned that.

MINK: I believe you did, yes.

KNUDSEN: This submarine had been taken to Plymouth, England, and was available there for study by the British and the American forces, and I had been detailed particularly to examine its sound detecting equipment that they used for locating surface ships. It could be used also for locating another submarine, but it was used very effectively at that time in destroying surface ships. We all know that the U-boat at this particular time was a great threat to our surface shipping. I believe I mentioned that one-fourth of the ships that left New York and Atlantic seaboards were sunk before they got over to European ports.

Well, the mission to visit at Plymouth was very interesting. This was my first opportunity to meet British naval officers, and I found them most cordial. We had a very pleasant lunch together, and among other things I know they had their ration of gin which was a very generous ration. [laughter] I couldn't handle my part of it by any means. I believe there were four of us altogether who were supposed to finish off approximately a fifth of gin at lunchtime. Well, I cut off long before these naval officers did; but they were very friendly and very helpful in explaining to me and taking me to see the gear on this ship.

The gear was certainly a great improvement over those detecting gears that had been developed toward the end of World War I. It was an elaborate listening gear. In the primitive device that had been used by our own navy toward the end of World War I, it was simply sort of a binaural listening device in which there was a primitive hydrophone (or diaphragm) on the end of each of two tubes, one at each end of a horizontal beam about 6 feet long, and one tube led to the right ear of the listener and the other to the left ear. And a rotatable shaft containing the tubes went through the bottom of the hull of the submarine. You simply manually turned this shaft with its two separated hydrophones until you heard the propellor sound of the suspected submarine most distinctly, and further when the sound is equally loud in the two ears, then the listening gear is pointing toward the propeller source of the submarine. And they always had to stop the anti-submarine chaser during the listening, otherwise they'd only hear their own propeller. They became a sitting duck really [laughter] while they were making this listening test to determine the direction from which the propeller noise of the submarine came.

The gear on the German's U-540 was really a thing of beauty to examine. There were 24 hydrophones on each side of the bow of the hull of the submarine. And the 48 hydrophones were all connected to compensators. By compensator we mean electrical circuitry by which you can introduce electrically a delay in the electrical response of each hydrophone corresponding to the actual delay in the time of travel from one hydrophone to the other. The controls for operating the steering of the submarine and adjusting the compensator enabled one to determine the direction of an enemy ship with an uncertainty of only a few minutes of arc, much less than one degree. By introducing the proper compensation you get the maximum response, which occurs when the responses of all 48 hydrophones are in phase. The sound that is picked up by each of the 48 hydrophones is amplified and the combined response actuates an indicating meter, and when that meter reading is at a maximum, you have determined the direction of the submarine--that is, the direction of the propellers of the submarine or the surface ship that you're detecting--and the direction is thus determined to a very, very high degree of accuracy.

One of the sections in my report on the U-540's listening apparatus was that the precision of workmanship that was required for making and installing the gear, and the manual hours that were required in the process, would be of such a nature and magnitude that it would probably be a handicap for the Germans, and that we would be at an advantage by having less precise sonar apparatus, but made in mass production, so that we could overwhelm the relatively small number of U-boats that could be thus equipped, although the Germans were indeed producing a lot of submarines at that particular time. I remember that some of the confidential reports we received at that time that indicated the Germans were actually equipping and turning out as many as twenty-five submarines in a month--of the U-540 type for their submarine warfare program, and of course they made later improvements in their sonar gear. Their submarine program was given very high priority, and it was exceedingly successful until we won in submarine warfare with our acoustical torpedo.

I think the next significant event in connection with my mission to Great Britain was a visit to the anti-submarine experimental station which had been moved to Fairlie, Scotland, about 40 miles south of Glasgow. It had been moved from its former location on the southeast coast of Great Britain because the Germans had very early destroyed most of the anti-submarine stations that the British had there; Fairlie was considered a much safer place. As a matter of fact, there had been no bombing of the Fairlie Station when I arrived there, although it had been there almost a year at that time.

This was a most interesting part of Scotland. It's on really the western coast of Scotland, and I was there at the most beautiful time of the year. It was early in June, and the rhododendron trees were in full bloom. It's really one of the beauty spots I visited during my British mission. And my arrival was at an unusually interesting time, because only two days before my arrival at Fairlie, Rudolph Hess had made his forced landing in a field only a matter of a hundred yards or so from where we were billeted during my stay at Fairlie. I was there for ten days or two weeks, because this was the principal anti-submarine experimental station that the British were maintaining at that time. They had a great deal of research going on that our people wanted to know about. The U.S. Navy and the laboratories at San Diego, New London, and the other laboratories where we were investigating these problems wanted relevant information. There was a very free exchange of information between the British and us. I was treated--so far as displaying what they were doing--the same as if I had been a Britisher or a Canadian.

The following incident is rather interesting. A week or two before I was there, two Russian naval officers had been there; this was rather unusual because such exchanges weren't continued much longer. A significant story was related to me about these two Russian naval officers who were there. Among other things they were brought up to date on the British Asdic, which was coming into use in the U.S. Navy at the time. I was to learn everything I could about the British Asdic, and also about their fundamental investigations of the acoustics of the ocean, which was a primary responsibility of our San Diego laboratory and a matter that I was especially interested in--how does sound behave in the ocean in respect of applications to our underwater warfare program?

Well, the Hess incident, along with the Russian incident, were in combination. Soon after arriving at Fairlie I was reading in the newspapers about Hess, then suddenly there was nothing in the newspapers. And when I asked questions of officers and others about the Hess incident, they said, "We have received orders to not discuss the Hess incident." This was always a mystery to me because suddenly the newspapers of Great Britain didn't include anything anymore about the Hess mission. Well, I won't go into that any further because historians, I think, have pretty well established what the probable reason for Hess's going over there was. But as the record now indicates, his mind, or at least part of it, was perhaps slightly unbalanced at that time, and it's hard to say just where the truth is completely because there have been differences in stories about the Hess incident. He's still alive, in prison, and historians are still trying to solve completely the reasons why he defected or went at the behest of Hitler (or in spite of Hitler's wishes) to perform this mission in Great Britain.

MINK: Have the stories really ascertained, though, why the silence was ordered?

KNUDSEN: I have not been [told].

MINK: I don't know.

KNUDSEN: I think it would be interesting for historians to pursue the subject somewhat further. I have always felt that the full story of Hess has not been adequately told and I have done quite a lot of reading on the subject.* But I don't think this is the time to go into that.

There is this other incident, from which I have digressed. These two Russian naval officers I mentioned had been allowed to attend one of the instructional classes that had been set up for training officers and operators of the Asdic equipment. And I was told by one of the British instructors of this class, "You know, these Russian officers were very well trained in their mathematics and their physics and so on." He said, "You know, after the first lecture that we gave on the Asdic, the two officers came to me and they said, 'You don't need to go into all the details about explaining these things. We've had higher mathematics in our naval training.'" And they mentioned the advanced math courses that they had taken during their officer's training. They'd had vector analysis, and they'd had quaternians and advanced

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*Note added 11/13/1970. Albert Speer's Memoirs give the most reasonable account I have read.

calculus and differential equations. This was somewhat of an eye-opener to me, to have this first-hand report about the higher mathematics that had gone into the program of naval officer training in Russia at that time. Well, I think it's generally known that the Russians, the Eastern European nations and also the Western European nations have paid more attention to the mathematical aspects of the training of military officers, as well as civilians, than we have in this country. Certainly they wouldn't regard solid trigonometry as higher mathematics.

One other incident that I think is relevant to this discussion was the activity of the Norwegians in anti-submarine warfare. It was well known that many Norwegians were able to escape the Nazi net that they kept around Norway; and this was true also of the Danes. But very early in my visit at Fairlie I became acquainted with two Norwegian officers. One was a full commander and the other was a lieutenant commander. And the commander has since risen in the ranks. I met him later again possibly in 1948. We first met at Fairlie in 1942. Six years later I met him in Oslo, and he was still interested very much in anti-submarine warfare problems and had continued advising the Norwegian Navy in problems of that sort.

But the way he escaped from the Nazis was rather interesting. Because of his naval background the Nazis were going to use him as much as possible. After he became trusted he was sent on a mission to Stockholm by the Nazis to perform certain tasks, and while he was in Stockholm he very conveniently "contracted a cold." At least he claimed he had contracted a very severe cold which prevented his returning to Oslo on the day he was requested to return. He simply said, "I'll have to postpone my coming because I'm under doctor's orders to stay in bed." And instead of that he managed to get in a dingy and make his way over to Scotland.

This was a usual way. They would acquire a small rowboat of some sort and really make their way across the straits there from the southern part of Norway (or Denmark or Sweden) over to the northern part of Scotland. And so many were coming that the Scottish people had a regular welcoming committee to welcome these Norwegian naval officers and others who were coming to assist in the war program. And the pleasure of my stay at Fairlie was very much enhanced as a result of my acquaintance with these two Norwegian officers who were naturally interesting to me because of my Norwegian background. They also were well-trained scientists and engineers, and they were contributing significantly toward the improvement of techniques for anti-submarine warfare.

SECTION XXIV

KNUDSEN: One other incident during my British mission is significant. Dr. Harald Sverdrup at that time was director of our Scripps Institute of Oceanography. He comes from a very prominent Norwegian family. The family has coal mine holdings in Spitsbergen. One of Harald's brothers had moved from Spitsbergen to Oslo and then had defected to England along with many other Scandinavians that had made their way over to England. As you know, the Norwegians had a government in England that was carrying on for the war effort. Sverdrup's brother was a competent engineer, and he was active in the Norwegian underground. It was very difficult for Harald Sverdrup to get letters to his brother under the circumstances. He couldn't reveal his identity or address in London, where Sverdrup's brother had an apartment. Harald had given me a letter that I was to transmit personally to his brother. When I called at the apartment to deliver the letter to him, there was no one there, and I could not get a trace of him anywhere.

When I returned from my British mission and chatted with Harald Sverdrup by telephone and told him that I failed to deliver his letter for the reasons I mentioned, Sverdrup said, "Well, you could not have delivered it because he was killed in his underground activity." He was making trips to Norway, you know, in line with the underground activities there against the Nazis, and Sverdrup's brother didn't get this letter because he had been killed as a commando in the Norwegian underground.

Well, now to go on with just a few of the technical aspects of my work at Fairlie and these other stations. I kept a complete journal record of the things we investigated. Among other things, I was taken to sea on one of their--oh, it's the equivalent of a Corvette. It was the roughest ride, I think, I've ever had on sea in my life, and I was delighted that I didn't get seasick. I think my vestibular mechanism was beginning to deteriorate somewhat. I had been seasick earlier in life on fishing trips at sea, but I was able to withstand the rough sea that we had in the North Sea, which was very rough even in June when we were out at sea from Fairlie.

We had an opportunity to test some of these Asdic observations that they were making on a target. They had a submerged submarine that they were using as a target and detecting this, and in the northern waters there, the range was very much longer than it was at Key West. I think I have told you that at Key West in the afternoon [laughter] the range was zero. And here we were getting nice signals from their Asdic at ranges of 1,500 to 2,000 yards. That's one of the good things I was able to report. In that kind of water the Asdic was working very well, even in rough water. (The stirring of the rough sea prevented the large temperature gradients we had experienced at Key West.)

They were conducting other experiments on the attenuation of sound in sea water and especially why sound might be attenuated in sea water by the action of bubbles in the water, by the action of salt in the water, by other possible variables of that sort, on all of which I kept complete notes. And this classified information was sent to our appropriate agencies in the United States. I'm sure I wrote a dozen reports during my six weeks there, some of them of considerable length; these reports were classified then, but all of them would be available certainly as unclassified material now. I do not have copies of them, so I can't, even if I wanted to, give the full details, because my memory is rather dim about many of those things that I reported at that time.

But I do remember that the British were doing work of a very high quality, and that it was a worthwhile mission to exchange information with them. They were much interested in the things I could report on our findings, and certainly we benefitted very much from the things I was able to learn by seeing at first hand the actual experiments and the gear that they were developing and the testing.

MINK: Would you say at this point that they were just about equal with us in their development, or had they excelled us?

KNUDSEN: At this time they were definitely ahead of us. We were not using the Asdic nearly as well as they were, and we owe a great deal to the British in the successful outcome of the battle of the Atlantic. The U-boat was ultimately destroyed, as you know. The Allies won the Battle of the Atlantic, and the Americans did their part. But we must recognize that the British really were out front. We were catching up, and in the end we had an acoustic torpedo before they did. I'll refer to that a little later here, which was very effective, especially against the Japanese, in really causing the Japanese surface ships--the civilian ships as well as the naval ships really--to be almost completely destroyed during the latter stages of the war. I'll refer to that later.

[tape off]

The U.S. Navy had an important mission that was headquartered at London. After I left Fairlie, I stopped also at Edinburgh, Scotland, where there were some experiments going on conducted by the British. And as you know, the Nazis had done great mischief to many of the British ships in the straits up north near Edinburgh, so there was an experimental station operating there mostly investigating the fundamental characteristics of the ocean, and so I spent a day or two there. Then I returned to London and most of the rest of my mission was spent at the United States Embassy building where our Office of Scientific Research and Development and also our naval attaché were located. We had the finest cooperation from these people there. Most of my time there was devoted to reading and writing reports. There were, I guess, a half a dozen other American scientists who were stationed in Great Britain at that time on other aspects of the war program. Aside from the blackouts and war research in London, there were dinners at the Athenaeum Club, occasional luncheons at the Connaught Hotel, splendid theaters, and sundry officers' pre-requisites that were shared with us war-connected civilians.

I had one other mission besides the anti-submarine warfare program. I had been serving with another section of the Office of Scientific Research and Development that had to do with gun ranging. This was accomplished by acoustical methods so it was very much in my bailiwick. And I spent some time with the Army officers that were concerned with the artillery problems of the Army and particularly with the location of enemy guns. On one occasion, I was taken by a Sir William Bragg, who had been in charge of gun-ranging research for a long time. He was a civilian. But there were two Braggs, who both got the Nobel Prize together in 1915, working largely on X-ray spectra. It was classical physics, and they had received very high acclaim for this particular work. But Sir William was my escort to go out to see their gun-ranging experiments that were in operation at an artillery station, located a two-hour train ride from London. What I saw and learned on this trip convinced me that the British were no more advanced than we were. In the anti-submarine warfare program they were, as I said, out front, but they were using [old] techniques [for gun ranging].

This was a pleasant visit primarily because it gave me an opportunity to meet Professor Bragg. I asked him if he could recommend a book that I could take home with me that would represent not what the British were doing in the war, but rather would portray the spirit of the British at war--what they were doing, for example, when they resisted the air attacks to which they had been subjected. He said, "Oh, there's an excellent book. It's called The Oaken Heart." And it's by Margery Allingham. She's a fairly well known novelist. I have the book at home.

This book is a story of the heroism, really, of the British in dealing with the wounded and taking care of the destitute victims in air shelters during the worst bombing that took place in 1940. The Oaken Heart symbolizes the sturdiness that they had in their emotions and their concern with each other in carrying out the wishes and commands, really, of Churchill in combating this Nazi aerial warfare. They were greatly outnumbered, of course, by the aircraft of the Nazis at that time, and yet the heroism of the British people remained steadfast in these worst days of the bombing which almost destroyed London, but not their hearts. Parts of London were completely wiped out. The book deals with the way they utilized their school buildings and their churches, and the way they organized their civilians to resist on the home front. This Oaken Heart is really a very fine tribute to the sturdiness of the British people. I lent this book to many of my friends after I returned to this country; it's worth reading even today to get a true impression of the British people fighting at home for their survival. It helps one to agree that there will always be an England.

I don't believe I'll go into the details of my work on gunnery ranges.

After my return from the British mission--I was there about six weeks until mid-July of 1942--I was asked to take charge of a project that involved bringing together the available data and information from all parts of the world dealing with the sounds of surface ships, the sounds of submarines, and the sounds (noise) of the sea. The two colleagues that especially worked with me on this project were trained statisticians from the Bell Telephone Research Laboratories. One of these persons was R. S. Alford, and the other is J. W. Emling. Together, we wrote three comprehensive reports. The one dealing with the sounds of surface ships was the first; the sounds of submarines was the second; and the one on ambient sounds in the oceans was the third. Each of them, with extensive charts and tables, was more than 100 pages. They brought together and evaluated all the data that were available from the British, from the Canadians, as well as from the United States. There were many laboratories in the United States, at New London, San Diego, Key West, Miami and Boston. I had accumulated extensive data from the British that they had obtained on the sounds of surface ships and submarines, and on the ambient noise in the sea.

The preparation of these three documents was a major effort during my remaining term of duty in the civilian program of the Office of Scientific Research and Development.

One of these, I think, is of special interest and has persisted to this day as a useful document to the Navy, namely, "The Sounds in the Sea." The curves that we devised, giving the most probable values of noise level versus frequency, were based upon a careful statistical analysis and compilation of all available data, treating them analytically, applying critical techniques to correct errors and to get the best possible average values.

Well, this report led later to a paper that I was invited to contribute to a volume that commemorated the sixtieth birthday anniversary of Harald Sverdrup, to whom I referred, who was director of the Scripps Institution of Oceanography. This volume was published in the Journal of Marine Research of the Sears Foundation, under the date of November 15, 1948. And it summarizes much of the work that we did. We use the title here, "Underwater Ambient Noise," by Knudsen, Alford and Emling. Inasmuch as the results published in this paper have wide application I might refer to one or two statements in the paper. The introductory statement here was written, you see, three years after the war had ended. This document had then been declassified, as have the other two of the three that I have referred to.

In introducing this subject I said here:

> Underwater sound continues to be a problem of high priority in naval warfare. Since atomic bombing may render obsolete most types of surface ships, the relative if not absolute importance of the submarine certainly has increased. Thus the U.S. Navy logistics chief recently told a congressional committee, "By far the most important and difficult problem which confronts the Navy insofar as ship characteristics and fleet operating techniques are concerned, is the problem of undersea warfare."

Well, for this record I think it is more important to speak of the purely scientific aspects of this investigation because it does deal with a problem that will be with us and doesn't change very much with the years, and it has a very definite bearing on, not only applications to undersea warfare, but it has I think a general interest, particularly the one that deals with these ambient sounds in the sea.

There are three kinds of ambient noise. The first is water motion. This really is very much like--well, the model we used here is, how much noise do you have when you drop a steel ball on a marble plate from various heights? The water noise in the sea is largely a matter of waves and the breaking of waves, and when the waves break at or near the crest, the water falls down and hits at or near the trough. It's this impact of water hitting against water--you know, like this New York play that has a title, slightly modified, I Cannot Hear You Because the Water Is Flowing in the Bathtub. And the resulting noise is largely from impact of water against the water in the basin of the tub or whatnot.

Similarly, whenever there is motion of the water on the surface of the sea, the waves produce noise in a quite similar manner, and this was the key that we used to predict the amount of noise that will result from water motion at sea. Now it's well known that there are various states of the sea, based on the heights of the waves, and sailors generally judge sea states by this means. They look out and say, "Well, it's a calm sea," and "a calm sea" means the waves--if you measure the distance from the crest to the trough--are at or near zero. You just don't have any wave noise. And if it's judged "smooth," the wave height is less than a foot. Then there are "slight," "moderate" and "rough" states. If it's "rough," the wave heights are 5 to 8 feet from crest to trough. Then it goes on to "very rough." Very rough is 8 to 12 feet from crest to trough. And "high" is 12 to 20 feet. "Very high" is 20 to 40 feet. And if it's greater than 40 feet from crest to trough, they say it's "precipitous." And so hopefully you can judge the state of the sea.

When you know these distances from crest to trough you know how far the water has to drop. This is very much like calculating how much noise you get when you drop a marble on a steel plate from various heights. The higher it is--from your elementary physics you know that the velocity of impact is proportional to the square root of the height, and the energy is proportional to the square of the velocity, so the energy of impact, and the energy of the resulting noise, is just proportional to the height of the wave. The distance from the crest to the trough of ocean waves is a very simple measurement of how much noise results from the motion of the sea.

In deep oceanic water this is the principal source of noise. And it's quite appreciable. As an indication here, the overall levels vary from sea-state one of only 63 decibels. That's for the overall level. Whereas, for sea-state six, which is high--

that's when the waves are 12 to 20 feet--the level is 83 decibels. This noise has increased 20 decibels in going from sea-state one to sea-state six.

MINK: Where is the noise measured from, how far under the water?

KNUDSEN: For oceanic water out in deep water, it's almost independent of the depth. Normally, we'd measure depth at 100 feet or even down further, but in deep water it's quite independent of the depth. It spreads out and it's reflected from the bottom and so on, and this is a feature which makes these noise data useful in naval warfare. If you're out in deep water the state of the sea determines what the noise level in the ocean will be. These noise levels are given by the curves shown in figure 4 on page 417, the legend for which reads, "Ambient Noise from Water Motion." The curves give the spectral noise level, that is, what the noise is in each 1-cycle frequency band. That would be 1-cycle wide all the way from 100 cycles up to 25 kilocycles or 25,000 cycles. And this covers the entire audible range, and it also covers the high frequency range for the use of the Asdic and similar echo-ranging (sonar) gear.

I was told by a nephew of mine only a few weeks ago that they still refer to these curves as the Knudsen curves, which are used for determining how much noise they have in the ocean for different states of the sea. Well, you see, this information is very important for determining how far you can detect a submarine. For listening tests, for example, the difference between 63 decibels and 83 decibels of background noise, which are the noise levels for sea-state one and sea-state six, is a very big difference--20 decibels. In energy terms, a 20 decibel change is a change of one hundredfold. And so this will affect very much the range at which you can detect if you're using listening gear. You can't hear the propeller noise of a ship very far away if the sea state is as high as six. You can hear the noise only about 0.25 or 0.2 of the range that you could when you are in sea-state one, a rough sea, so this would be a limiting factor.

Furthermore, the Asdic or sonar gear is similarly affected at the high frequency end of the noise spectra. The range limitation is determined by the background noise. If your echo is masked by the ambient noise in the sea, you can't hear or detect it. Well, this is the most important (practical) aspect of ambient water noise in the sea, but there are two other sources of ambient noise in the sea that are really interesting. These noise sources are from two especially interesting species of marine life. We have noise spectra for these two soniferous species of marine animals--snapping shrimp and croakers. On page 422, we show the sound pressure spectra of the ambient noise in the presence of snapping shrimp and croakers. Those are the two principal marine animals that give rise to great noises in the sea. You'll see in the curves the way these sounds vary with the seasons. You will note that it is late May and early June when the croaker noise is greatest. I believe I told you last time that croaker noise is greatest in the mating season, and also that it's greatest a half-hour before sunrise and a half-hour after sunset.

So we have this information about the way the croaker noise is distributed throughout the audible frequency range and also about the high frequency range for snapping shrimp. And they are the two principal sources of marine life noise. A third source of noise, of course, is ship and man-made sources which are found in busy harbors and in connecting waters. Well, this reprint has considerable general interest about these various sources of noise in the oceans, these three kinds of noise, from water noise, from marine animals, and from man-made and ship noise. These three comprehensive reports, one of which I've referred to here, give a rather complete record of what was known before about 1944 concerning the magnitudes and frequency characteristics of sounds in the sea.

MINK: These measurements that you've compiled and the curves that you drew were done from measurements that had been done by various other people. They were not made from measurements that you yourself made.

KNUDSEN: No, I have participated in only a very few of the measurements. The measurements were made by many agencies concerned with noise levels in the waters of Boston Harbor, Long Island Sound, off Brock Island, New York Harbor, lower Chesapeake Bay, off Bulfork, North Carolina, Puget Sound, San Francisco, San Diego, off Ohau, in Midway Islands, off Portsmouth, England, Loch Goil in Scotland, and the Hebrides Islands. Of course, the ones off Portsmouth and Loch Goil and so on were obtained by the British. The Canadians were getting some data, and before this project was completed we had also Canadian data that had been obtained near Halifax.

After this survey work was completed, a related investigation by Dr. Liebermann at San Diego, and by Professor Leonard here in our laboratory, discovered an important characteristic about the attenuation of sound in the sea. It was well known from numerous measurements that sound is attenuated more in sea water than it is [in] fresh water. Liebermann and Leonard separately found that the excess attenuation was attributable to magnesium sulfate molecules exchanging energy with the water molecules in much the way oxygen molecules reacted when colliding with water vapor molecules, as in the absorption of sound in air which I discussed in a previous session here. And this work that Professor Leonard did here, and Liebermann did at San Diego, has done a great deal to evaluate the attenuation of sound in sea water.

I told you in the beginning here today that Stokes and Kirchhoff, distinguished physicists, had presumably worked out the correct attenuation on the basis of viscosity and heat conductivity of water, but they did not take into account the effect that the salt in solution has. And this later work which was conducted at San Diego and here at UCLA cleared up the problem that had troubled us a great deal, namely, why the attenuation of sound in sea water should be greater than it is in fresh water. Their discovery also contributed to a better understanding of the fundamental processes involved in the propagation of sound in other liquids, which are concerned with the exchanges of energy (including time delays in such exchanges) between colliding molecules.

SECTION XXV

MINK: This morning we are going to begin to talk about your work in the Graduate Division and about the development of graduate work in the Graduate Division.

KNUDSEN: Graduate work at UCLA probably can be best introduced by recalling some of the early history I have recorded already in respect to the situation I found at the Vermont Avenue campus in 1922. We recall that it was as late as 1919 when the University of California Southern Branch came into existence, which represented really a union of the old [Los Angeles State] Normal School, over which Ernest Carroll Moore was president, and the first two years of college--junior college--in letters and science. And we recall that the facilities I found in 1922, when I arrived, were not very encouraging with respect to the establishment of graduate work. We recall also that my dean, when I announced that I was coming to UCSB, exclaimed that, "My God, Knudsen; it's only a junior college!"

This was indeed the situation I found. I think I recorded that I had a three-in-one office--that is, two other instructors besides myself, with the help of a shoehorn, were really fitted into this little cubby-hole intended for a janitor's closet under the stairway.

And I think I characterized the research condition I found there by referring to my own facilities. If we represent the research space by RS and the equipment by E then the mathematical representation of this unfavorable condition is RS = 0 and E = 0. And this was a true representation. The year or two before, John Mead Adams had taught freshman physics with the help of a meter stick and a brick. And this was certainly not an auspicious prospect for graduate work.

And yet, we find rather rapid developments taking place from that period on. Ernest Carroll Moore himself was dedicated to making this a real university, and this was quite apparent from the things which happened in sequence. In 1923, the third year of letters and science was added; in 1924, the fourth year was added; and we were soon a very respectable senior college as well as junior college. And I often liken these things that occurred at the Southern Branch [of the University of California] to those that occurred at Riverside, say, when it changed from a citrus experiment station to a first-rate college, that is, when the University of California established upper-division college work there. And we all know that this was cited as one of the shining examples in the country for high-grade, undergraduate instruction. And this was true also following the establishment of the third and fourth years of letters and science here at UCLA. We were most fortunate, I think, in the person of the first dean that came down here from Berkeley, Charles Henry Rieber, who was really a first-rate philosopher-scholar and he had the vision of graduate work as well as Ernest Carroll Moore did at that time.

The recruitment of faculty, I think, that was taking place during this early period [was excellent] at the Vermont Avenue campus (where we remained until 1929 when we moved to this campus). To name just a few, the one distinguished scholar who was there, that I recall, was Loye Holmes Miller, who was a great zoologist who's still living--well up in the nineties--today at the Davis campus. And he still writes and does research. He was the "grand padre," really, of the campus in those days in more ways than one. That is, he was a first-rate scholar, but he was also a friend of all new faculty members and an inspirer of the value of research, and it was most appropriate that he was named the first faculty research lecturer at the University of California, Southern Branch. Loye Holmes Miller initiated that series on the Vermont Avenue campus.

Miller was largely responsible for bringing two other first-rate scholars into the Department of Zoology--Bennet M. Allen and Albert Bellamy--both of whom were outstanding research scholars and were cut out very definitely for graduate work as their primary teaching and research responsibility. Others that came at that time--you yourself, Mr. Mink, will recall the additions in history--for example, were such scholars as Westergaard and Klingberg and Parish. And the other man who went to Florida some years later. (His name eludes me now--he died soon after he left here.) There's a fourth one in history. This quartet were really first-rate historians who would grace any graduate department of history.

Lily Campbell was brought at that time to English, and I may have recorded that whenever I referred to our English department at UCLA in conversations with English scholars in Europe as well as at American universities, they'd say, "Well, that's where Lily Campbell is." And she was an illustrious example of research attitude and graduate work. And Frederic Blanchard, also in English, was of that same caliber.

Then, three others that I should name at this time: Shepherd Ivory Franz who was brought here was a truly great psychologist, and the university has fittingly named the psychology building over here Franz Hall; Charles Grove Haines, a highly distinguished political scientist whose name fittingly graces our Haines Hall; and Gordon Watkins, who was a tower of strength in economics, and later transferred that strength to the scholarly development of UCR. All of these people were thoroughly qualified. I could name many others but these (and also Earle Hedrick, mathematician, who later became our provost) come to mind now. They were not only thoroughly qualified to participate in graduate work at a first-rate university, they were thoroughly dedicated to the establishment of graduate work here on this campus.

And so Ernest Carroll Moore and Charles Henry Rieber were soon joined by these other scholars. Perhaps my own name might be added to this list, because I was really among them, at least in my dedication to graduate work at that time, and I was starting a research career that at least in this personal discussion I shouldn't leave out, and selfishly I relish the belief that I was considered worthy of working with them for the establishing of our graduate studies.

With the move to the Westwood campus in 1929, this was definitely an indication of big things to come. To cite again a single example: the facilities that had been provided for research in the new physics building on this campus is evidence in brick and concrete that scholarly work, research and training of students, was to be expected. President

Sproul often referred to the facilities we had here as an indication that we were going to train scholars here, and this certainly was a turning point in my career. That is, instead of having laboratory facilities across the city at high school auditoriums, and a private reverberation room that had been built over on Central Avenue, one of the very finest facilities for acoustical research that existed anywhere in the United States was provided in the new Physics-Biology Building (now Kinsey Hall). And other facilities were made for other departments here on this UCLA at Westwood Hills campus.

At about that time there was real discussion throughout the state as to whether there should or should not be graduate work at the new campus here. I've mentioned the people here who were determined that there should be, and there are of course many others who were equally dedicated to that early development of graduate work here: for example, in physics there were S. J. Barnett, John M. Adams, Lee Kinsey, Joseph Kaplan, Joseph Ellis, and of course L. P. Delsasso (my lifelong colleague and collaborator), all distinguished physicists competent to teach and train graduate students.

But, of course, the ugly problem of cost was continually thrown up in our faces at that time. The

Taxpayers Association was opposed to the development of graduate work here. The private universities of the state were not altogether cordial about the development of graduate work here.

I think this was symbolized very much by a Charter Day address that Professor R. A. Millikan, who was then the president of Cal Tech, delivered here on campus in Kerckhoff Hall which was our social gathering place at that time. (This must have been in 1931 or 1932, it was a year or two before graduate work began.) The message of this Charter Day Address was that "the key arch in our higher educational system is the interplay between privately endowed and publicly endowed universities." And Millikan documented this by referring to the concern that the University of Illinois had when the University of Chicago was established in 1893 and called attention to the benefits that extended to both the University of Illinois and the University of Chicago. The University of Illinois had great anxieties that this was going to throw it into eclipse; but the establishment of the University of Chicago was really a benefit to the University of Illinois.

Privately, around the campus here, at any rate, Millikan's address was interpreted to mean that the University of California at Los Angeles here would teach through the college level. This would continue on to be a college, and we would be an undergraduate institution therefore, and that graduate instruction would be at such institutions as the California Institute of Technology. As a matter of fact, Millikan and many others from private institutions had supported the view of the California Taxpayers Association that it was not appropriate to establish graduate work on the Los Angeles campus of the University of California.

MINK: First of all, I think that we might speak for a minute about the personnel of the committee appointed by Dr. Sproul to study the problem of offering graduate work at UCLA. This committee met for the first time on October 27, 1930. One of those present was Dr. Leuschner. Could you comment on him?

KNUDSEN: Dr. Armin O. Leuschner had been dean of the Graduate Division at Berkeley before Charles Lipman, who was dean at the time graduate work was established here. But Leuschner, an astronomer, was considered one of the top scholars at Berkeley. Also he was then chairman of the all-university research committee that had the responsibility, really, of recommending to the president just who would get research grants and how much they would get. Leuschner would come down here and meet with a small local committee for arriving at the rewards of research grants to various scholars on the Los Angeles campus.

I first met Leuschner in that capacity sitting with a local committee here that reviewed the applications for research grants very much the way they're reviewed now by our own faculty committee on research, the Academic Senate Committee on Research. Leuschner certainly added a very high tone to the quality of research that should be supported on campus and brought to this campus in his visits here, perhaps twice a year, beginning about 1929 or 1930, when we moved to this campus, the precedence of the high standards of scholarship that had been developed at the Berkeley campus. And we benefited very much, certainly, from his presence here. He was hard of hearing and therefore he did nearly all of the talking at these meetings. I'm sure Leuschner did about 60 to 75 percent of the talking because this was a very useful way of concealing his impairment of hearing.

MINK: Did you ever test his hearing?

KNUDSEN: No. I did not. But I had opportunities to make subjective tests--that is, conversational tests. We knew we had to raise our voices very much in order for him to hear us, and it was apparent to me that he had severe loss of hearing. At that time, I had been working very closely with Isaac H. Jones, and possibly his hearing impairment was more apparent to me in a quantitative sense as well as a qualitative sense than anyone else because it affected even slightly his speech. You could hear in his speech some of the characteristics of impaired hearing. You didn't hear the differnces say, between his "f" and "th" and "s." And these unvoiced consonants tend to coalesce in the speech of the person with a gross impairment of hearing. I noticed some of these symptoms in the speech of Leuschner--and these symptoms are characteristic of nerve or cochlear deafness. He certainly was a good example for exhibiting these symptoms because he spoke so much at these meetings.

MINK: Was he well liked?

KNUDSEN: Yes, I think so. He was probably a bit imperious. Maybe it was more than his impaired hearing that led him to do most of the talking at these meetings. And I think we felt that we were in a sense second-class professors as compared with Leuschner, who came down here more or less as a god to get us started on the right track in the matters of research. At any rate, the end result was good.

MINK: The next one is Dr. Lipman who you referred to just a minute ago in speaking of Leuschner, I believe.

KNUDSEN: Lipman succeeded Leuschner as dean of the Graduate Division at Berkeley. I think his deanship included Riverside and Davis where there was some graduate work. He was the dean of the Graduate Division at the University of California, period! That included Mt. Hamilton and Davis and Riverside as well as Berkeley where research at the graduate level was conducted; and some training of graduate students took place at these other three campuses besides the one at Berkeley. Lipman probably had more to do with the character of the development of graduate work on the Los Angeles campus than anyone else at Berkeley, primarily because he was the dean of the Graduate Division, Northern Division, and partly because I was named dean of the Graduate Division, Southern Section. This brought a close relationship between Lipman and me.

Certainly, we derived very much from him about how to establish a graduate division here, and we agreed with his advice in nearly all things. We differed appreciably only in one respect--I'm not sure that I've referred to that. Lipman didn't believe we should ever have a PhD degree, for example, in horticultural science. And some of us down here firmly believed that it was possible to do scholarly work worthy of the PhD in an applied field like horticultural science just as well as in "pure" fields. And I think this was a good example of a PhD program in which we were not mistaken because the horticultural program here at the Los Angeles campus was thoroughly distinguished. That is, we had men like Professor William Chandler who was a member of the National Academy of Sciences, who had a world-wide reputation, and we had other world famous horticulturists; these scholars attracted graduate students from the semitropical and tropical climates from many parts of the world. And this program certainly became one of our early distinguished graduate programs at UCLA.

But quite apart from that one minor difference, really, in which Lipman and I particularly disagreed, I have the highest respect for Dean Lipman, who was himself a distinguished scholar in plant physiology. And he was probably the first to bring forth evidence--it was questioned very much at the time--of the million-year age of some bacteria that had been found in mines throughout different parts of the world. This was pooh-poohed by a lot of the conventional botanists and plant physiologists at that time. But I think time has supported some of these early views of Lipman about the age of some of these living organisms that persisted, embedded in coal and other media of that sort, that had certainly been laid down a million years ago or more.

Dean Lipman was really a statesman in American graduate education. I had the pleasure of attending meetings of the Association of American Universities which in those days consisted of the presidents and the graduate deans of some thirty-four of the top-hat universities of the country. It was a very exclusive club, I think as most people recognize now. More and more it came to be a place where graduate deans met, and there was just a sprinkling of presidents present, and a separation of these two groups later took place between the presidents on the one hand and the graduate deans on the other. But it continues to this day to be really a high-hat organization.

Its duties at the beginning were primarily the accrediting of colleges throughout the United States and to train students that would be accepted for graduate work as the institutional members of the Association of American Universities. And the association developed from that initial purpose, to set the pattern really for graduate education in the United States. And I know of no other graduate deans among the thirty-four deans who would attend these meetings of the Association of American Universities who exerted a more powerful and beneficial influence than Charles Lipman in determining graduate education policies. He was a thorough statesman and an excellent speaker on his feet and a very persuasive person in the discussions that took place in these meetings in the 1930s, and I counted myself fortunate to have him as a mentor in the development of graduate work at the UCLA campus, and later at other campuses of the Southern Section--at Scripps, Riverside, and Santa Barbara. A great debt is due to Charles Lipman for the part he played in the establishment of graduate work here.

I might say that it was also through Dean Lipman's negotiations, or really they were personal negotiations, between Dean Lipman on his part and on my part in the first year or two that I was graduate dean here, beginning in 1934, that initiated graduate work leading to the PhD degree in physics on this campus. This is an interesting story regarding UCLA's transition from an undergraduate college to one of the nation's great universities. I cite this primarily because in this personal history I'm describing my own activities in the Graduate Division.

There were three physics students in graduate work at the University of California at Berkeley who wanted to do work in acoustics at UCLA. They wanted to get their PhD degrees in acoustics--more specifically, very close to either architectural acoustics or some physiological acoustics dealing with hearing. The first of these was Norman Watson, who is now professor of physics in our own department. The second was Richard Bolt, who now is chairman of Bolt, Beranek, and Newman, which is one of the largest groups of consulting physicists and engineers in the country, not only in acoustics, but also in computer technology and other fields. Only yesterday I received a brochure outlining the courses in higher education that they are giving at such campuses as New York, Cambridge, Boston, Washington, D.C., Dayton, Ohio, Minneapolis-St. Paul, Los Angeles and half a dozen other places throughout the country.

Dick Bolt is chairman of the board of BBN which has its stock listed over the counter. BBN has been outstanding in many fields besides acoustics--data processing, and now lasers and computers and solid-state physics and technologies of this sort. And Dick Bolt is the top man in that organization and he was one of these first three persons to do their doctoral dissertations in the Department of Physics here at Los Angeles, but having their degrees issued from Berkeley, because we were not at that time authorized to grant a PhD degree here. But Lipman was liberal enough to make private arrangements for these three students to come down here to work in acoustics with me at that time. And they are among the best graduate students in acoustics who received their training at UCLA.

MINK: Who was the third graduate student?

KNUDSEN: Robert Leonard, who also was professor of physics here. Leonard and Bolt came together, possibly one or two years after Norman Watson. Norman Watson was the first, and Norman Watson is often listed with Kenneth Bailey as the two who were the first recipients of PhD degrees here, although Norman Watson's was issued from Berkeley and Kenneth Bailey is therefore normally rated the first person to receive a PhD degree on the UCLA campus.

MINK: In history?

KNUDSEN: Yes. Well, that's a sort of a sketch of Lipman and some of the things that resulted from our friendly and fruitful collaboration. We'll have occasion to refer to him again and again, I'm sure.

MINK: Lipman was very statesmanlike. Did he get a chance to give any statesmanlike remarks in spite of Leuschner's constant talking?

KNUDSEN: He was not nearly so monopolizing of the meeting as Leuschner. Definitely there was a modesty there that characterized Lipman. And there was a personal charm about him that attracted him to me very, very much.

MINK: Now I see that the next person, of course, is Dr. Moore. I think we can skip over him for the time being. We'll go on to Charles Grove Haines from the Political Science Department, who certainly should have been on that committee.

KNUDSEN: There's no doubt of that, and it was almost an oversight on my part when I just hurriedly called attention to this group of scholars who were here. Haines and probably Shepherd Ivory Franz were in a class almost by themselves and certainly were two of the foremost scholars on this campus who worked and worked effectively for the establishment of graduate work here. Charles Grove Haines was probably one of the leading authorities in the country on constitutional law, and this particular field set him apart. I know a number of my colleagues sometimes said, "Well, Charles Grove Haines will probably someday find himself on the Supreme Court of the United States." This was the attitude of people here who knew him; so this is, I think, an index of the esteem in which he was held here. He was an extremely modest man, modest even by a comparison with Charles Lipman, about whom I've spoken, who was much more modest than Leuschner. Haines was modest almost to a fault. He was very retiring and yet he was very forceful in his insistence upon high standards for graduate work. We were very fortunate to have a man of his high scholarly attainment here at the time graduate work was established, because he was recognized as a scholar everywhere throughout the world in this field of constitutional law.

MINK: I see. Now next is, of course, Dr. Rieber, who you said had come down from Berkeley.

KNUDSEN: He certainly raised the tone here for upper-division work. Rieber was not the type of scholar you would normally select for teaching at the graduate level. He was not interested as much in publication as he was in setting the tone for high scholarly work, and his lectures were more an index of his greatness than anything he published. I don't know much of what he published, but he truly was a leader among scholars here on the campus. Some of the lectures he delivered, beginning on the old campus in Berkeley Hall at the Vermont Avenue campus and ones that he delivered later here in Royce Hall and in other halls on this campus, are truly great. There was one I know that he delivered in Royce Auditorium that always stands out among the things I remember around here and which influenced me. The title of the lecture was "In Default of Demonstration." And Rieber was truly a religious person, not in the orthodox sense but in the ethical religion of Jesus, rather than the gospel of Christ sense.

MINK: Was it a Unitarian concept, perhaps?

KNUDSEN: Yes, quite definitely a Unitarian concept, but a recognition that there were things that you could not demonstrate by science that were worthwhile. This is what he meant: when science defaults or is not able to explain these things, there are within the spirit of man, the spirit of God, things that can be validated and can be made useful and enriching parts of life. And they are somewhat beyond the pale of scientific demonstration. This is a lecture which I think should be included somewhere in this oral history. I'm afraid it was not orally recorded at that time. I'm sure this was delivered before we were making any tape recordings of any speeches. But his lectures were models of scholarship and beauty, and he had a great admiration for the scholarly life.

He influenced, I think, higher education here very much through his large classes. One class dealt with the history of religion, and I think another dealt with various aspects of philosophy. But they were very much down-to-earth. He was an expert collector of the pertinent sayings of others. And he himself made many tape recordings of these sayings; it would be worthwhile exploring this through the Oral History Program. It's possible that grandchildren of his would have the recordings.

Charles Henry Rieber had one son by the name of Frank. Frank also died a few years ago--maybe ten years ago. He was a fabulous person in geophysical prospecting, and he made his living pretty much in that field. He had a very checkered career. He'd rise up to heights of affluence with the money he made in geophysical prospecting, then he would lose it all again in some related venture. He was a peculiar person in that respect, but a genius. He inherited much of his genius from his father.

His mother, Mrs. Winifred Rieber, was no less distinguished in her right. She is the painter of this portrait of Albert Einstein, a photograph of which (inscribed to me by Einstein in 1935) hangs on the wall here in my office. She also did portraits of David Starr Jordan and of several distinguished philosophers at Harvard University. So it was these influences in the Rieber family that contributed much to their culture. It was a place of culture to go to the Rieber home, say, for a turkey dinner which had been cooked by Charles Henry Rieber. Also, he was an expert at cooking Indian curry dishes. And I have many pleasant memories of early invitations to the Rieber home where we enjoyed this kind of hospitality.

He also was a great admirer of the Salt Lake Mormon Tabernacle Choir. It came on the radio every Sunday morning. And on several occasions Mrs. Knudsen and I, sometimes others, were invited to breakfast at the Rieber home, but invited in time to hear the Salt Lake Tabernacle Choir and organ program at 8:30 in the morning. Rieber would call us to attention very much like one would open a religious service and say, "Now during the next half hour we're going to have our religious service and nobody talks during our religious service." And so we sat at attention during the thirty minutes of the music which preceeded our breakfast. He knew that my wife and I were of Mormon ancestry, and that we would especially appreciate these programs, but he appreciated them very much, too.

He was also a great admirer of the included little sermons--he objected to my calling them sermonettes--by Richard Evans, who normally gave these sermons on Sunday morning and continues to give them to this day; they are primarily nondenominational. And if you listen to some of them, you'll realize that they're mostly ethical and moral. In a very broad sense they could be Christian or Mohammedan or Jewish; they are very much of that character.

Well, I bring up this subject because Dean Rieber often invited his guests to dictate into his tape recorder favorite gems that he was collecting. And I know he had me dictate two of my favorite ones, and I think I can remember one. Have I told you about this?

MINK: No, I don't believe you have.

KNUDSEN: I think one of them went something like this. It was a time when the theory of indeterminacy in physics was receiving a great deal of attention. You see, Niels Bohr, Peter Debye, Werner Heisenberg, and others were developing this principal of indeterminacy. That is, when you get to very, very fine structured things like electrons and so on, you can't quite discriminate between one electron and another. There is indeterminacy there as you get to the more and more minute divisions of matter and structure and energy and so on. And this saying Dean Rieber wanted me to record was one I'd picked up on one of my sabbatical leaves in England, and it went something like this: "God, to you it must seem very odd, that a tree as a tree, simply ceases to be, when there's no one about in the quad." And the sequel to that is: "Young man, your astonishment's odd, for I'm always about in the quad, and that's why a tree continues to be, as observed by yours truly, God." And Rieber liked to collect little things like that, and there are one or two others I know of mine that I recorded that time; but this one I happened to think of at the moment now.

Well, I think that's enough about Rieber to indicate that he exercised an influence quite different from that of the research scholars who were here; but certainly he was a wonderful person to have in the university. If you're going to build a great university you need more teachers like Charles Henry Rieber, who was one of the best teachers we ever had here, and who set the tone for high scholarship and effective teaching on this campus, I think, as much as any other one person.

MINK: I just would like to pick up on one point, and that is, you said he would not have been the type that you would have selected for graduate work. Were you talking about then, or are you talking in terms of now?

KNUDSEN: Well, with respect to your question, <u>then</u> and <u>now</u> are not very different. That is, he wouldn't have a record of publication. Any committee who would look at the publications of Charles Henry Rieber then or now would say this record doesn't meet the minimum requirements for a professor who is to train graduate students. And I think Charles Henry Rieber might agree; he was content to teach undergraduates; but he taught them in a very scholarly manner, and teachers of graduate students would do well to utilize his teaching ability.

MINK: I see. The next person is from education, Dr. Marvin Darsie.

KNUDSEN: Yes. Well, Darsie is a person for whom I had great affection. We discussed more problems, I guess, swimming on our backs in the pool over there.

MINK: In the gym?

KNUDSEN: In the gym. We made daily swims over there, and we extended our swim distance so that ultimately we would do a quarter of a mile each day. We had measured the lengths of the pool. We knew how many round trips we had to do to make a quarter of a mile. We could do this--oh, in a little over a half an hour or something like that. There was no hurry, and as I say, much of the swimming was on our backs because this made it possible to carry on our conversations. The conversations were not of the scholarly nature. They were just good friendly conversations between two colleagues that found joy in swimming together over there.

But he was a person of high intellectual standards, and I think we were fortunate in having a person like him as the dean of the Teachers College, 1922-1939, and of the School of Education, 1939 until his death, 1940. (Incidentally, he was also our first football coach at Vermont Avenue--and of course without salary.) He was here at the time the transition took place from a normal school to a school of education. And as such I think he will always be remembered as the first dean. Again, I'm not sure that his publications would meet the minimal requirements that were considered essential then, if you were going to recruit a man to become, say, even dean of the School of Education where graduate work was to be the primary responsibility. He was more concerned with the training of good teachers at all levels than with the training at the graduate level. But as the senior member of the School of Education, or the Department of Education, which was by all means the largest department we had on campus at that time he was an important person in planning for the future of graduate work.

[The following pages will supply information about others who participated in the initiation of graduate work at UCLA. Dictated November 18, 1970. V.O.K.]

Gordon S. Watkins was born the son of a Wales miner and rose to distinguished economist and dean and university administrator. He came to the Southern Branch as full professor of economics in 1925, and since then has contributed greatly to the development of economics, and in administrative positions as dean of the Summer Session, dean of the College of Letters and Science, and then the first provost and first chancellor of the University of California at Riverside. His scholarly contributions are primarily connected with labor problems, personnel administration, economic reform and especially labor problems during World War II. He has served with distinction such diverse activities as the Danish Committee on Public Monopolies, as a personnel counselor to the Title Insurance and Trust Company of Los Angeles, as a member of the Alien Enemy Hearing Board, and as a trustee of the John Randolph and Dora Haynes Foundation. He was no less distinguished and revered by his colleagues and students as one of our most distinguished teachers as well as scholars. His counsel as a member of the first Administrative Committee on Graduate Study contributed significantly to the establishment of policies that guided the development of our Graduate Division.

César Barja was a leading scholar in our Department of Spanish, and was eminently qualified to deal with the problems of graduate study, especially in the fields of languages and literature. His standards of scholarship were high, and his counsel in the early years of graduate work at UCLA helped to establish the high reputation that UCLA early acquired as a graduate school.

Sigurd B. Hustvedt joined UCLA at the Southern Branch as assistant professor of English in 1921, and after becoming professor of English in 1930, contributed significantly to the scholarly prestige of that department, and throughout the development of UCLA he served on many important campus and university committees. His field of specialization was folklore and folk music, especially of northern European and more especially Scandinavian countries. He contributed greatly in developing the resources of the campus library as a useful source of material for graduate work. He was a no-nonsense type of professor and exacted high scholarly performance from his students. He also contributed to setting a high standard of graduate work in other fields and departments as a stalwart member of the first Administrative Committee on Graduate Study.

Junius L. Meriam joined the faculty as professor of education in 1925. He was especially devoted to the pursuit of research and experimentation in curriculum and methods of instruction in elementary schools, and especially among disadvantaged minority pupils. He was fastidious and meticulous in guiding the thesis work of graduate students in the Department of Education. His graduate students were pursuing programs leading only to the master's degree, and in judging the merits of master's degree theses, both in education and for other departments in which he served as a member of a thesis committee, he differed from the other members of these committees, and during the early years of graduate work it became my responsibility, or even the responsibility of the Graduate Council, to adjudicate disputes as to whether the thesis in question met our standards for acceptance. Although he was often argumentative in such decisions, he was a genial colleague, and both he and his wife were generous entertainers in their home of both students and faculty members.

I believe I have already said some kind things about Loye Holmes Miller. He has the distinction of having joined the faculty of UCLA and its forerunner, the Los Angeles State Normal School, earlier than any other living faculty member. He was appointed as assistant professor of zoology as early as 1903. He was the first head of the Department of Science at the Normal School and began his career at the Southern Branch in biology as assistant professor in 1919. Named by his colleagues as the first Faculty Research Lecturer in 1925, he was recognized as our most eminent research scholar at that time. He was responsible for bringing to the campus such distinguished zoologists as Bennet M. Allen and Albert Bellamy. All three of these scholars contributed greatly not only to the Department of Zoology but in the setting of high standards for both teaching and research at UCLA. Dr. Miller was an outstanding scholar in the field of paleontology of bird fossils, and he also was a learned authority on living birds. Interested also in music, he was able to imitate the songs of birds and there are a number of phonograph recordings he has made of bird calls and bird songs. For many years he also was our number one peacemaker when quarrels or differences developed between various faculty members. When I complimented him once about his valued service in these matters, he said, "Oh, I simply carry around with me an oilcan and an alamyte gun." He served well his students and often led them as well as other faculty members on social and learned expeditions in the desert, to the Santa Barbara Islands. He has continued active in research and writing, mostly since his retirement in 1943.

Bennet M. Allen has been identified with research, superior teaching, wise administration since he first came to UCLA (Southern Branch) in 1922. His research dealt primarily with the metamorphoses of tadpoles into frogs. He developed microscopic surgery in removing or modifying the thyroid and other glands of internal secretion, and by these experiments made important contributions to the physiology of the glands of internal secretion. He served as acting dean of the Graduate Division during my leaves of absence and on my prolonged leave during World War II. Like Loye Holmes Miller he was greatly admired by colleagues and students. After retirement in 1947, he continued his research in the Medical School's Atomic Energy Project. He served on numerous faculty and administrative committees, and organized the first Faculty Research Committee at UCLA.

John C. Parish was professor of history specializing in American history during the initiation of graduate work at UCLA. He was a member of the first Administrative Committee on Graduate Study. His leadership in the field of scholarly research is evidenced by his having been named one of the early Faculty Research Lecturers.

SECTION XXVI

MINK: The next person is Dr. Frank J. Klingberg from the Department of History.

KNUDSEN: I didn't know Klingberg very well in these early years. I came to know him much better as graduate work progressed here. I always remember a remark he made that I think was typical of Klingberg throughout the years. Not only during his active status here, but as an emeritus, he continued using the facilities here a great deal, and he was always grateful to me. I think he gave me more credit than was due me because partly through my efforts he was assigned one of these little rooms in the library. And he always said, "You know, the way to do research in history is to get a pair of leather pants, then try to wear them out in the library." You'd wear them out in the seat while sitting in the library doing your work, and this was a symbol of the way he did research.

He certainly was a scholarly man in the way of the number of things that came from his pen. He was really prolific throughout all the years that I knew him. But he made a strong point of calling to the attention of those who could help him the things he published. I believe I obtained from Klingberg copies of probably everything that he published during the years I was dean of the Graduate Division. Even after he retired he would bring over to me personally an autographed copy of these works of his. So I have had an opportunity to learn about Klingberg through many personal encounters, but principally after graduate work was established. I knew very little of him at the time this committee met, except that he was one of the recognized scholars in the Department of History.

MINK: The next man is from your own department, and that's Dr. Barnett.

KNUDSEN: Yes. Dr. Barnett was brought here really in the same category as these other scholars that I mentioned. He certainly had an illustrious record of research. He and his wife worked together, and many of the earlier publications from Barnett were by Mrs. Barnett as well as Samuel Barnett. I think this was more a gentlemanly gesture on his part rather than a real partnership in the work, because no one that I knew, including myself, encountered much research work on the part of Mrs. Barnett after she came here. But Barnett had already established a superior reputation in the narrow field in which he did research. He had begun this work at the Ohio State University where he was professor of physics for a time.

I'm sure the first paper I ever heard Dr. Barnett give was one when I was a graduate student at the University of Chicago. He presented one of these papers at a meeting of the American Physical Society. It was customary to hold a November meeting once a year at the University of Chicago. I know I heard Barnett present a paper at one of these sessions that I attended, either in November of 1921 or 1920. And this dealt with the subject he worked on his entire life, namely, two effects in the field of magnetism. One is the inverse effect on the other: rotation by magnetization, and magnetization by rotation. I think most physicists, at any rate, recognize that these two effects have something to do with the spin of electrons inside of the atom, but it had much to do also with the nature of magnetism.

He could demonstrate just how rotation could be produced by a magnetic field or how magnetization could be produced by rotation. It was largely the rotation of the electrons which gave rise to these magnetic effects. And this had a bearing on some of the precise evaluation of the constants of physics--like the charge on electrons and the ratio of the electrical charge to its mass, and other things that are concerned with the development of electron physics that was in the ascendancy at that time. Barnett had correspondence with Albert Einstein about the work he was doing and with other distinguished European physicists who were familiar with the work that Barnett was doing in this field.

After leaving Ohio State University he was, I believe, at least most of the time thereafter, at Cal Tech, where he was a research associate rather than a professor on the staff. He worked with the Carnegie Foundation. It was full-time research, as I remember now. He was recruited from there to join our staff here, probably in 1927. It could have been 1928, but I think it was 1927, because he was here in time to play an important part in the recruitment of two other scholarly physicists who were brought here, Lee Kinsey and Joe Kaplan, who came, I think, a year after Barnett was here. And Barnett had the help of people at Berkeley in recruiting Kaplan and Kinsey, in about the same way that John M. Adams had in recruiting me. Physicists at Berkeley particularly aided in the appointment of Kaplan and Kinsey. Barnett was favorably influenced a great deal by Professor Leonard Loeb, who was a distinguished electron physicist at Berkeley.

Barnett brought to UCLA a high standard of research. He was in many respects what one would call one of the last survivors of the classical physics era. That is, he was definitely a classical physicist, and most of his work was eliminating systematic errors that occur in experimental processes. He was refining experiments that he had initiated or that others had performed. He was refining them and eliminating all possibility of any kind of error so that the values that he obtained were very trustworthy values and highly respected by the physics profession.

MINK: Dr. Earle Hedrick from the Mathematics Department was another member of this first committee.

KNUDSEN: Yes. Dr. Hedrick really had many facets in his scholarly background. He was an author of some well-recognized and highly respected textbooks on higher mathematics at the time he came here, and he continued writing others after he came here in the fields of differential equations and higher calculus and texts of that sort. Even to this day many of his texts are used in the teaching of higher mathematics. I think he'd been president of the American Mathematical Association, which was largely concerned with the training of mathematics teachers, rather than pure research. But Hedrick would qualify both in research and in the training of teachers.

He was a character in many senses of the word. Whenever he entered the company of a social gathering, you knew that Hedrick was there. He often was the centerpiece of conversation and he always had a jovial manner after he became provost here. It was always a pleasant experience for the faculty to be invited to his home, and he did a great deal in promoting good relations among the faculty. He wasn't provost very long, but during this time people got to meet each other. For example, maybe faculty from the Physics Department, the Chemistry Department, or the Geology Department, and two other departments; typically, two from social science and humanities and two from the physical sciences would be invited to the Hedrick home. Maybe as many as forty or fifty at a time came. This would be an opportunity, before we had a faculty center of any sort around here, to meet other members of our faculty and to participate in the hospitality of the Hedrick family.

This furnished an informal gathering in the provost's home. He didn't occupy the chancellor's home on campus here. I believe that had been occupied by the Robert Gordon Sprouls during the time Hedrick was provost here. This was probably the president's home at that time. But the Hedricks did a lot, I think, to promote good feelings around the faculty, and between the faculty and the administration.

He was a jovial person. I remember a luncheon he gave for certain faculty members in one of the private dining rooms here that was not much bigger than this office, in which we could have private lunches. You could probably seat as many as sixteen or eighteen around the table, and Hedrick would always begin by saying, "Now you have two choices at this luncheon which I'm giving here today. You can either take it or leave it." [laughter] There were no changes in the menu, which he meant.

There's an anecdote that I always remember about Hedrick. Soon after he came here he made a visit to the Berkeley campus and he related that he was very much impressed with the large number of young men and women walking back and forth along the sidewalks. And he spotted one that he thought looked a little brighter than the others and he stopped him and he said, "How many students are there on this campus?" The young man looked up at him and in a drolling voice he said, "About one in a hundred." So Hedrick always used this as more or less calling attention to the difference between Berkeley and Los Angeles, and he insisted that the score was considerably higher than one in a hundred on the UCLA campus. He was a good jokester in that sense.

It was very difficult to understand Hedrick in administration. Mathematics probably isn't the best training for administration. Often I would go to his office to have a problem clarified and I would leave more confused than when I entered. But you always left with a good feeling for Hedrick. He was a charming person, and he would more or less infer that what he had explained had made the subject perfectly clear. He'd usually end the conversation by saying, "That's perfectly clear, hm? hm?" That was the end of the conversation. It wasn't clear at all. [laughter] But he was a good mathematician, and he served very well during this early era in establishing graduate work which attracted graduate students in mathematics to him and to others in the Department of Mathematics.

MINK: Well, the last member of the committee which Dr. Sproul appointed was Dr. Shepherd Ivory Franz. I think you have commented on Dr. Franz.

KNUDSEN: Yes. Probably not as much as I should. Franz certainly brought here a very distinguished background in his study of the brain. He was named Faculty Research Lecturer, and I believe he was number two on the list of Faculty Research Lecturers here. I think he may have been the first on this campus. I'm sure that the first one by [Loye Holmes] Miller was on the old Vermont Avenue campus. Franz's Faculty Research Lecture was "How the Brain Works," and it's a classic. I reviewed this lecture on the occasion of the dedication of Franz Hall, when the Franz family--his wife had died, but the daughters were present--came for the dedication. This was a good example of the type of research he had performed at an early date, and in a sense it was the beginning of our brain research activity on the campus, which now is probably the most renowned and generously supported research activity going on at the UCLA campus. I believe their present budget (1969) is something like $4 million a year. That should be verified, but that's the figure at least I have heard.

Franz, even more than his own research, influenced other people. He brought to the campus the first psychiatry we had, and he was the psychiatric counselor to faculty members and students who were troubled with emotional and psychological problems. Franz's office was always open to these people in distress, and he straightened out many faculty members and students before we had anything resembling a student health program. And he was a many-faceted person and very vocal in expressing his views. Among his many talents and accomplishments, he brought strength, distinction, and leadership to the campus, and especially to his department, of which he was truly head as well as chairman.

Also, he and Mrs. Franz performed an unusually commendable service in entertaining faculty members at their home. The Sunday afternoon gatherings at the Franz home were truly cultural gatherings in which Shepherd Ivory Franz and his wife (and even the daughters as they grew up) performed a very splendid service in making the faculty acquainted with each other at a time before we had a faculty center of any sort.

MINK: Did these gatherings include music?

KNUDSEN: No, mostly conversation. And sometimes there were paintings and other art objects that Mrs. Franz was interested in at the time. But they were suitable Sunday afternoon gatherings.

MINK: I have two other questions. First of all, when the announcement of this committee came out, people probably talked. Did they think that the people that were appointed were qualified, or did they think that others should have been appointed?

KNUDSEN: Frankly, I have no recollections of anything but goodwill towards this committee. It was representative of the top scholars here on campus, and I don't believe anyone could really complain about the membership of that committee. Certainly it came to grips at once with the real problems of developing graduate work. And I'm sure the faculty and administration alike recognized that the kind of service this committee was performing was really necessary to prepare us for graduate work. For example, they encouraged very much the research activities of other members of the faculty, particularly junior members. I'm sure their dealing with this problem had salutary effect upon the transition from a teaching institution to a teaching-research institution, which the Graduate Division was destined to become.

MINK: Do you have any recollections as to whether the members of this committee actively sought the opinions of others who were not members or junior members of departments? Were these things discussed, for example, at luncheons?

KNUDSEN: I can't answer that for departments other than physics.

MINK: All right. What about Dr. Barnett? Did he actively seek the advice of the other members of the department?

KNUDSEN: We had regular departmental meetings, but I think Dr. Barnett acted more privately in this committee. It was a confidential committee, and we really did not see reports on the actions of the committee until a few years later. I don't remember seeing the committee's confidential reports until we were discussing the history of twenty-five years of graduate work leading to the PhD degree here. I learned many things by documents which were made public for the first time then; so there wasn't much opportunity to even discuss the deliberations and decisions that had been reached by this confidential committee. They met with Sproul or with Charles Henry Rieber and Ernest Carroll Moore, and I think these administrators were the ones who were familiar with what took place in these committee meetings. But the rank and file, in general, were not kept informed of the decisions that had been made there, or of the recommendations that they would pass on to Sproul, which in turn would be passed on to the regents, where ultimately decisions regarding this important matter were made.

MINK: But did Dr. Barnett seek the opinions of the other members of the department about the direction of graduate work that he could pass on as a member of this committee?

KNUDSEN: Probably so. I frankly can't remember specific examples of what transpired at that time that would bear on your question. It should be recalled that Barnett was having difficulties in administration of the department which tended to break down communications. Specifically, he had expressed his opinion that three members of the physics faculty should not be reappointed. And this caused frictions and exacerbations which I think we have discussed.

MINK: We have discussed this, yes. And probably this could have had some bearing on the fact that he did not communicate.

KNUDSEN: I'm sure it had a very definite influence. But as early as 1932 Barnett was no longer chairman.

MINK: The committee was in 1930.

KNUDSEN: Yes. It had been a year or so, and he'd been active with that group, but I don't believe he continued with this committee after about 1932.

MINK: This committee was appointed in 1930, the first year of President Sproul's administration. Here is a brand-new president. I wonder what the faculty might have thought about his appointments on the committee, he being new and so on, although, of course, he was comptroller and vice-president.

KNUDSEN: Yes. Undoubtedly Sproul sought very earnestly and necessarily the advice of his intellectual peers at the Berkeley campus, and he recognized them in such men as Armin O. Leuschner and Charles B. Lipman and people like that, and I presume Sproul had relatively little to do with the selection of the membership. Ernest Carroll Moore and Rieber and Lipman and Leuschner would be the four people I would name who probably determined the membership of this committee.

MINK: The meeting of September 27, 1933, was the first faculty meeting called by Dr. Moore to discuss graduate work at the University of California at Los Angeles.

KNUDSEN: During this three-year interval [1930-1933], I knew that there were discussions going on about the establishment of graduate work here and that there was a committee working, but, so far as I can recall now, I did not read any reports of the committee during that period. What we did learn was more or less through the grapevine. Also, this was the most active period in my research career at UCLA. My afternoons were completely devoted to research, and I was very jealous of anything that would interfere with the carrying on of my research, afternoons and evenings, and therefore I knew little about what was going on in that committee until in the fall of 1933 when Dr. Moore asked me if I would serve on an administrative committee, really to serve as the dean in lieu of a dean of the Graduate Division. This committee, with Dr. Moore as chairman, was to be the administrative head of the first year of graduate work. The regents, in their meeting in August of 1933, to which Dr. Moore referred, had limited enrollment to 125 students. We were then authorized to start graduate work leading to the master's degree in those departments that were mentioned in this document to which we referred here. In the first year, 170 students actually enrolled. I recall these numbers from the documents I have read.

MINK: How could 170 enroll if it was limited to 125?

KNUDSEN: Well, I have no idea. [laughter] I have no responsibility for how many were admitted. I was not concerned with the admissions that year, and I know it was after the year was well underway that

Dr. Moore first called together this committee that served as administrative head of the Graduate Division. But the documents around here, the registrar's records, indicate that the number was not actually 125 but 170, which I presume should be considered in round numbers. The other condition that the regents had stipulated very definitely was not only this number of 125, but the offering of graduate work must in no way interfere with or prejudice the offering of undergraduate work. This was interpreted to mean: you can't increase the faculty; you can't increase the costs of operation; you'll do graduate work as a sort of a volunteer gift to the university. UCLA will continue to be primarily an undergraduate institution, but you will gratuitously serve to do such graduate work as from time to time may be authorized. And the administrative committee, appointed by Dr. Moore in the fall of 1933, did not meet often. There must be a record somewhere of how many meetings were held. But my recollection is that there were not many held during that year.

Dr. Moore was becoming increasingly concerned about the growth of communism, and this became really a crucial issue with him and had a lot to do with the falling apart, really, of the administration here at the UCLA campus the following fall--the fall of 1934. It was then that Dr. Moore dismissed these four students.

Dr. Sproul had to step in as the administrative officer of UCLA, and Dr. Moore had very little to do with the administration of the Los Angeles campus after that inauspicious event.

The first recollection I have of my participation in the development of graduate work here was my appointment to a committee that was to make recommendations to President Sproul for the first dean of Graduate Study. It was called Graduate Study then. The decision to appoint this committee probably had been reached by President Sproul as early as late fall, or as late as December of 1933, but I know we had several meetings without Sproul's presence. He then met with this committee at a crucial meeting on the selection of a graduate dean. We met at the Bel-Air Restaurant, which is now the site of the Bel-Air Hotel. I well remember this Saturday luncheon, but I do not remember the names of all the persons present. I know Moore and Barnett were there. Sigurd Hustvedt in English and Bennet M. Allen of zoology, as members of the committee, were there. Do you have a record of the other members? [tape off] I believe Charles Grove Haines and Gordon S. Watkins were there. I doubt that Franz was there. This meeting was the first thing I remember about my participation in the offering of graduate work here. I had been appointed to this committee to recommend to President Sproul. I recall that a number of names were considered for the deanship at this committee. Two members of the committee were considered and each was asked to withdraw while his qualifications were discussed.

MINK: Were you discussed?

KNUDSEN: The two were Hustvedt and myself. Hustvedt was out for a time when his qualifications were discussed, and I was out when my qualifications were discussed. Others from other campuses had been mentioned at that time, but I do not remember their names. I suspect that Haines and Franz, who were preeminently qualified, were favorably considered, but their age at that time may have disqualified them. However, I do not recall that their names were mentioned as possible candidates. I do remember that Hustvedt and I were asked to withdraw while our qualifications were discussed. He was in English and had been working a great deal on ballads and related things, largely in the north countries of Europe.