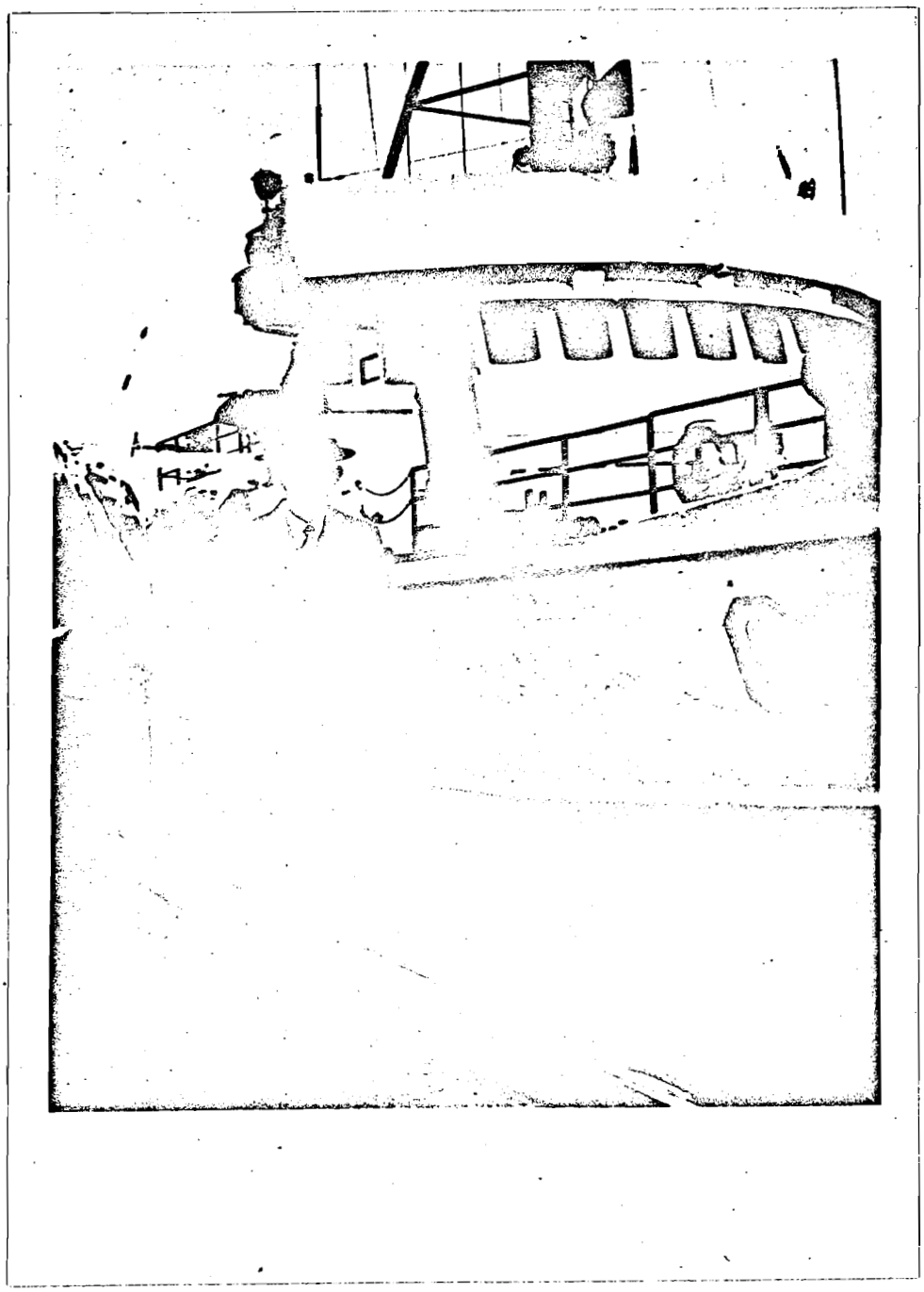
Victor F. Lenzen

PHYSICS AND PHILOSOPHY

An Interview Conducted by
Edna Tartaul Daniel

Berkeley
1965



Victor F. Lenzen

Commissioning of the United States
Coast and Geodetic Survey Ship "Peirce"
6 May 1963, Mobile, Alabama.

PREFACE

Professor Victor F. Lenzen's career at the University of California has spanned more than half a century, from 1909 when he entered as a student to the present when as a professor emeritus he is continuing his studies and committee work. His work has been in both physics and philosophy and he has contributed writings in both fields. It was this unusual combination plus his work on various committees of the Academic Senate and his role in the development of the Department of Physics that brought him to the attention of the Regional Cultural History Project which was collecting first-hand accounts of the history of the University of California and its prominent faculty members.

The Regional Cultural History Project was established to tape-record autobiographical interviews with persons who have been prominent in the recent history of Northern California. One series of interviews, directed by Professor Walton E. Bean of the Department of History, deals with the development of the University of California and the contribution of the University and its faculty to the region

and to the fields of research of the interviewees. The following manuscript is a part of this series; related interviews have been completed with professors Raymond Birge, Ralph Chaney, and Joel Hildebrand as well as with faculty members outside of the sciences. The Project is under the administrative supervision of Assistant Librarian Julian Michel and is advised in its work by the Bancroft Library Subcommittee of the Library Committee of the Academic Senate.

Willa K. Baum
Head

May 6, 1965
Regional Cultural History Project
Room 486 The General Library
University of California
Berkeley, California

INTRODUCTION

Professor Victor F. Lenzen, Professor of Physics, Emeritus, tape recorded his comments about a long and significant academic career at the University of California at Berkeley, once each week, from October 10, to November 30, 1960. Although his teaching and academic duties radiated from the Department of Physics, his interest and publications ranged deeply into philosophical as well as scientific concepts. The unusual range of mind and performance which Professor Lenzen brought to his career is best summarized in a biographical sketch he wrote December, 1958, revised April 25, 1965, and which follows.

Professor Lenzen's manner was easy, cordial, and accomodating to the interview procedure which was carried on in a small office of the General Library of the University. As he spoke slowly and firmly, in a voice of uniform tenor register, his intense interest and close

attention to the ideas and information he presented, entirely suffused his surroundings. Through a quiet manner and frugality of expression there glowed great zest for the analytical process he directed to subjects which claimed his attention. He smiled easily and relished wit and irony when they appeared in what he had to say.

As he walked about the campus, his physical appearance was somewhat stern. His spare frame and angular profile were surmounted by a grey felt hat placed squarely upon his head. He wore somewhat dark and simply cut suits and a topcoat and proceeded at a steady though cautious pace, and usually with a cane.

Following recording, the tapes were transcribed and the manuscript edited by the interviewer for clarity and continuity. Before final typing, Professor Lenzen made corrections and contributed several photographs for illustration of the manuscript.

Edna Tartaul Daniel
Interviewer

May 1, 1965
Regional Cultural History Project
Room 486 The General Library
University of California
Berkeley, California

BIOGRAPHICAL SKETCH

Victor F. Lenzen was born on December 14, 1890, in San Jose, California, where his paternal grandfather had settled in 1862, the son of Theodore W. and Kate A. (Schnoor) Lenzen. He attended school in San Jose, Oakland and San Francisco, and in 1909 graduated from the California School of Mechanical Arts. He manifested interest in pure science while preparing for college, but entered the University of California in the College of Mechanical and Electrical Engineering. His interest in pure theory, however, caused him to turn to philosophy and he graduated from the University in 1913 with a major in philosophy. He was awarded the Scholarship of the Harvard Club of San Francisco and thus pursued graduate study at Harvard University from which he received the degree of doctor of philosophy in philosophy in 1916. During his first year at Harvard he came under the influence of Bertrand Russell whose work in logic and philosophy of science revived Lenzen's earlier interest in physics. As a member and secretary

of Josiah Royce's famous seminar in scientific methodology, Lenzen became even more inclined to abandon philosophy for mathematics and physics. During the Christmas vacation in 1914, Lenzen was sent by the Harvard philosophers to the home, near Milford, Pennsylvania, of the philosopher Charles S. Peirce, whose death had occurred in April, to pack and ship the manuscripts and books of Peirce which his widow wished to give to Harvard University. Upon completion of his studies for the doctorate, he was awarded a Sheldon Fellowship for study in Cambridge, England, and Paris, France, under the trying conditions of wartime. In Cambridge he attended lectures by J.J. Thomson, Joseph Larmor and also traveled regularly to London to study mathematical logic with a small group under Bertrand Russell. Upon return to the United States he was Assistant in Philosophy at Harvard during the year 1917-1918.

He returned to his home in San Francisco in June, 1918, and then resumed his membership in the University of California, Berkeley, first as Assistant in Physics

and then as graduate student and teaching assistant. He became Instructor in Physics in 1921, Assistant Professor in 1925, Associate Professor in 1930 and Professor of Physics in 1939. For more than twenty years he was in charge of the Lower Division Office of the Physics Department. He retired as Professor Emeritus in 1958.

From the beginning of his instructorship, Professor Lenzen gave lectures in Physics 1A-1B, later 4A, and thus introduced many students of engineering, chemistry and physics to the field of mechanics. His special interest in this field was expressed by his graduate course in Advanced Dynamics which he taught with the logical rigor for which his training in mathematical logic had fitted him. His systematic interest was further exemplified by his major work The Nature of Physical Theory (1931).

Although his entire academic career was in the Department of Physics, a request to contribute a paper "The Philosophical Aspects of the Theory of Relativity" to the Philosophical Union in 1923 led to the publication of sixteen papers on Philosophy of Science in the University of California Publications in Philosophy. He contributed a

paper "Philosophy of Science in America" to the volume Philosophic Thought in France and the United States (1950). He also wrote several reports on Philosophy and Physics in the United States for L'International Institut de Philosophie. He was frequently consulted by the National Science Foundation for judgments on projects in the Philosophy of Science. His broad interests also included the history of his subject and for many years he served as Associate Editor of Isis and as a member of the local History of Science Dinner Club. He was a frequent contributor to the United States Quarterly Book Review of the Library of Congress.

Professor Lenzen was Guggenheim Fellow 1927-1928 and during his residence in Goettingen, Germany, pursued the study of theoretical physics. In the summers of 1932-33-34 he attended conferences on theoretical physics at Ann Arbor, Michigan. He married Esther V. Hayden, a graduate of the University, on July 13, 1935. In the summer of 1936 they traveled to Europe where Professor Lenzen read a paper to the International Congress for the Unity of

Science in Copenhagen, and was a delegate to the International Mathematical Congress in Oslo, Norway. In 1938 Professor and Mrs. Lenzen made a trip around the world and en route he presented a paper to the International Congress for the Unity of Science in Cambridge, England. Professor Lenzen's association with the Unity of Science movement led to his contribution "Procedures of Empirical Science" to the Encyclopedia of Unified Science. He was especially interested in the work of Einstein and contributed a paper "Einstein's Theory of Knowledge" to the volume Einstein-Philosopher-Scientist (1949). He also contributed by invitation to the volume of testimonials presented to Einstein on the occasion of the latter's seventy-fifth birthday (1954) and his comparison of Einstein and Spinoza was quoted by the New York Times on the occasion of Einstein's death the following year.

Professor Lenzen served on the Executive Committee of the College of Letters and Science and on various committees of the Academic Senate, including terms as Chairman of the Editorial Committee, of the Committee on

Privilege and Tenure, and of the Committee on Memorial Resolutions. While serving on the Editorial Committee a controversial paper on archaeology led him to investigate the field with the result that he published an internationally recognized paper "The Figure of Dionysos on the Siphnian Frieze" (1946). After retirement, Professor Lenzen reviewed the controversy in archaeology and published a second paper "The Figure of Dionysos on Textiles of Late Antique Egypt." These later years, however, were occupied mainly with studies of the scientific work of Peirce, in whom his interest had been revived by a letter from Professor Max H. Fisch, University of Illinois, biographer of Peirce, inquiring about the mission to Milford in 1914. Professor Lenzen undertook researches in the National Archives, located long neglected instruments used by Peirce, and published papers on Peirce's contributions to the fields of gravity, astronomy and metrology.

Professor Lenzen was a member of the American Physical Society, the American Mathematical Society, the American Philosophical Association (President, Pacific Division 1944),

the History of Science Society, the Archaeological Institute of America, the American Association of University Professors. He was a member of Phi Beta Kappa (President, Alpha of California 1935-36), of Sigma Xi, of Pi Mu Epsilon, the Faculty, the Kosmos Club, and the Harvard Club of San Francisco.

(revised April 25, 1965. V.F.L.)

VICTOR F. LENZEN

from Who's Who

Professor of physics; born San Jose, California, Dec. 14, 1890; son of Theodore W. and Kate A. (Schnoor) Lenzen; B.S.,University of California, 1913; Ph.D., Harvard, 1916; Harvard Sheldon fellow, Cambridge, England, and Paris, 1916-17; Guggenheim fellow, Goettingen, Germany, 1927-28; married Esther V. Hayden, July 13, 1935.

Assistant in philosophy, Harvard, 1917-18; asst. in physics, University of California, 1918-21, instructor, 1921-25, assistant professor, 1925-30, associate professor, 1930-39, professor of physics, 1939-58, emeritus 1958-.

Fellow American Physical Society; member American Mathematical Society, American Philosophical Association (president, Pacific division, 1944), History of Science Society, American Association of Physics Teachers, American Association of University Professors, Archaeological Institute of America, Phi Beta Kappa, Sigma Psi, Pi Mu Epsilon.

Clubs: Faculty (Berkeley); Harvard (San Francisco).

Author: The Nature of Physical Theory, 1931; Procedures of Empirical Science, 1938; The Figures of Dionysos on the Siphnian Frieze, 1946; Helmholtz's Theory of Knowledge, in Essays in Honor of George Sarton, 1947; The Triumph of Dionysos on Textiles of Late Antique Egypt, 1960.

Contributor to Twentieth Century Philosophy, 1943; Albert Einstein: Philosopher Scientist, 1949; Philosophic Thought in France and the United States, 1950; Causality in Natural Science, 1954; and also articles to University of California Publications in Philosophy since 1923.

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EARLY INFLUENCES

Grandparents

Lenzen: My paternal grandfather is the first person I have any information of. He came to San Jose in 1862. Essentially he was a builder and contractor, and my father as a young man worked for him as a carpenter, learning the business from the ground up. I was told that my father's mother insisted that my father be paid wages.

Then at some time my father came to San Francisco from San Jose and took certain lessons in drafting, just where I don't know, but I think he was in San Francisco for quite a period. Then he made a trip to Europe for sketching purposes, and he spent six months in Italy. His program was to make sketches of outstanding buildings which would provide novelty for American building.

Daniel: When was this?

Lenzen: Father was born in November 1864, and he was abroad I

Lenzen: would say in 1886. No doubt if one looked at the back files of the San Jose Mercury one would find letters that he wrote describing his experiences, though it hadn't occurred to me until this moment that one might do that. My brother was more closely associated with our father later on than I was because my brother learned architecture and also building and became a builder himself. So he might have more information than I have.

My father spent the year in Europe after he was 21 --at least that's my impression--and when he came back he entered into partnership with his father under the title of Jacob Lenzen and Son. They had offices at 75-80 East Santa Clara Street, which is between First and Second. Santa Clara and First was the center of town, corresponding with Third and Market Street in San Francisco. The impression I have is that my father did the designing and his father was the public relations man, though they didn't have that term at the time.

Daniel: Do you remember anything interesting about that office in San Jose?

Lenzen: I was often in the office but it didn't mean anything to me. I was born in 1890, and I think my family left

Lenzen: San Jose in 1900. I was young and could hardly be expected to be interested in architecture.

Subsequently when my father was engaged in architecture in San Francisco I servedas his office boy a considerable time. I also typed specifications. He would take a set of specifications of the same general form required for a particular building and would make the changes in pencil, and then I would type them on the typewriter. But I never did any drafting for him; I was never interested in architecture and I was never particularly interested in his experiences.

He had various sketchbooks which have disappeared by now. He had two large books in which photographs were pasted--these were called European views--and they were pictures of the principal buildings in Europe. Also in our home we had large reproductions, I suppose they were enlargements of photographs. I never took an interest in how they were made. There was one of the Colosseum and one of the Rialto and one of the Bridge of Sighs in Venice. So of course at a very early age I became accustomed to seeing pictures of these places. They were there and I saw them every day but I didn't really investigate them.

Lenzen: My attention to them was purely casual.

Daniel: Have you more comments on your grandparents?

Lenzen: I might say this. My father's mother died shortly after he was married, I think. I don't know exactly when it happened but it certainly was in a year or two. Then he remarried rather promptly and that second wife died inside of a year. Then he remarried again and difficulties developed that I heard of from my mother. I don't know what they were in detail, but this mother-in-law who apparently created the problems was a stepmother-in-law. I judge that if my father's mother had lived there would not have been these family difficulties.

Daniel: What about your maternal grandparents?

Lenzen: My maternal grandmother was born in 1840 in Flensburg when it was still a part of Denmark, before that territory was acquired by the Prussians. Her father was a Danish sea captain. She was a member of a large family of ten or eleven or something of that sort. Her husband, and my mother's father, was named Schnoor. Just how this marriage came about I don't know, but he died at an early age. In 1867 my mother was born in Schleswig, which is also on the Jutland Peninsula.

Lenzen: My maternal grandmother had a brother who had settled in Antioch where he farmed, almonds I think. When the transcontinental railroad was finished, my grandmother brought my mother, who was then about five years old, to Antioch to the home of this brother. And then my grandmother married a man by the name of Mathias Berlingen, a pioneer farmer up in the Delta area. He had 160 acres two miles from Byron on the road to Brentwood. I recall this farm very well with a pleasant memory. We used to go there during the summertime, and I think possibly maybe also Christmastime. But in about 1896 my stepgrandfather decided to retire from the farm and purchased a house at 44th and West Street in Oakland.

It was in 1888 that my stepgrandfather and my maternal grandmother went to Europe for a year. Of course my mother then was just 21. I would judge that they divided their time between Wiesbaden and Baden-Baden, which were the two best watering places, resorts. These were the most elite places for the cure and so forth. And it seems that this stepgrandfather came from the same part of Germany as my paternal grandfather did, and that's the way the marriage was arranged between my father and my mother.

Lenzen: In 1900 my father had already started a business in San Francisco, and for a year or so he commuted between San Francisco and San Jose. But in the year 1900 my stepgrandfather and grandmother took another trip to Europe, to Wiesbaden, I think, and spent the whole time there. My father's family occupied the house in Oakland, so I was in Oakland for a year. We went to San Francisco when the grandparents came back.

Daniel: It must have been rather interesting and exciting to have grandparents who went abroad.

Lenzen: Oh yes, that was a very conspicuous event.

Daniel: They went by train to New York, I imagine.

Lenzen: Oh yes, of course, there wasn't any Panama Canal then. They traveled by train to New York and then by boat and back.

Daniel: This is the kind of thing that as a young child you might have bragged about to the other children.

Lenzen: I never mentioned it to anybody else, just took it for granted. Why was there anything to be said about it?

Daniel: It depends upon what the status symbols were.

Lenzen: Well, it seems to me at that time there wasn't so much consciousness about status symbols. It was just taken

Lenzen: for granted that the grandparents would go to Europe. They didn't say very much about their experiences. My father had been to Europe, my mother had been to Europe with her stepgrandfather and stepfather and mother in 1888, the grandmother and the stepgrandfather went to Europe in 1900. I didn't think anything of it at all; it was just the thing to do, perfectly natural and obvious. There were no special remarks made about it.

I was with my maternal grandmother a good deal. In 1906 were the earthquake and fire. My mother and sister were in Oakland with the grandparents. My father and brother and I were at our home in the Sunset District in San Francisco. On the morning of the earthquake and with the fire going we started out very early and got to the Ferry Building by about 10 o'clock, and I think got almost the last boat to Oakland. In fact, it was a creek boat. In those days they had a boat that ran up the estuary and docked at the foot of Broadway. It cost five cents.

We stayed with the grandparents for about ten days. Then my father and mother and brother and sister went to San Francisco, but I stayed with my grandparents until school started again in August.

Daniel: What was the age spread among the children in your immediate family?

Lenzen: My brother was born in August 1893 so that he was not quite three years younger than I. My sister is nine years younger than myself. She was born in 1900.

Daniel: You remained with your grandmother because you were not too much trouble to take care of?

Lenzen: I don't know just why I stayed with her.

Daniel: Would you just as soon have stayed there as go with your family to the city?

Lenzen: Yes.

Daniel: Why?

Lenzen: I don't know. I really couldn't offer any expanation.

Daniel: Did you have more freedom?

Lenzen: No, I wouldn't say that. I got along very well and she liked me.

Daniel: What about your stepgrandfather?

Lenzen: He was very kind to my mother and also to me, so that the step relationship was negligible, it wasn't thought of at all, because he and my grandmother had no children.

Daniel: What sort of place was your grandmother's home in Oakland?

Lenzen: It was a one-story house with a high basement. It's still

Lenzen: standing and occupied.

Daniel: What did you do when you were there?

Lenzen: Well, at that time that area was not built up, and there were vast fields. I remember playing ball with some others from the neighborhood.

Then there was one period--and I don't remember just when it was--that I had a job in Oakland in a brass foundry getting 50¢ a day grinding castings. You see, after a brass casting was taken out of the mold therough edges would have to be ground off. That's what I did.

Daniel: How did you happen to do this?

Lenzen: I don't know. I don't really understand why I happened to get this job. This must have been after the earthquake when I was staying with my grandparents from April to August before going back to school. I must have been 15. But then in those days when a boy graduated from grammar school he was supposed to go to work as an apprentice.

Daniel: But you weren't there as an apprentice.

Lenzen: No, just a sort of a helper.

Parents and Early Schooling

Lenzen: My first school was Lincoln Grammar School in San Jose. The school is still standing, at least the last time I was down there, but it's used as an administration building for the school department.

Daniel: Did you look forward to going to school?

Lenzen: I don't recall whether I did or not.

Daniel: Did you learn how to read at school?

Lenzen: I don't know. I suppose I must have learned at school.

Daniel: Elementary school doesn't bring forth any special impressions?

Lenzen: No, I had no special impressions. I think I was somewhat not as well adjusted as the average child because, for one thing, my mother kept very close watch on me. She would never allow me to play in anyone else's yard. Other children could come in our yard if she approved of them.

Daniel: Was this simply because you were the first child?

Lenzen: Well, it may have been partly because I was the first one. My brother was more difficult to handle later, I understand.

Daniel: She wasn't being snobbish--

Lenzen: She just wanted to make sure that I didn't have bad associates.

Daniel: Then she was selecting--

Lenzen: Oh, she was highly selective. Now, here's one instance, and I always think of it in connection with the United Crusade and the Community Chest and so forth. When I was in high school I wanted to join the Y.M.C.A. in San Francisco to learn how to swim. My mother consulted a grammar school teacher that I had had and had become a friend of the family, and the grammar school teacher advised against it on the grounds of bad associations.

Daniel: Bad associations in what way? Was this a matter of prejudice with respect to racial--

Lenzen: No.

Daniel: On a moral basis?

Lenzen: Yes, morals. You see, in those days women of the middle class were very much concerned that their children should play only with children of nice families. I think there is still that preoccupation. I've thought about this in recent times; I don't think it can be applied so much because of mass education in the public schools, because if the child is going to the public high school he's going to mingle with the children of the lower classes.

Daniel: But you went to public school. How did your mother guard against bad associates there?

Lenzen: I was inclined to be a very lonely sort of individual. I was an introvert. I remember over twenty years ago I sat next to Professor Kroeber at the Faculty Club. I had got into conversation with him and some question arose about introverts and extroverts, and he characterized me as a typical introvert.

Daniel: What about your brother and sister?

Lenzen: My brother was a very social fellow, naturally, and he was very well liked. Apparently he was born with a sociable personality and with the ability to get along with people. My sister also was very sociable. My mother used to be worried at times for fear that my brother would get into bad company because he was companionable. I think that mothers of that era were very frequently like that. I don't think it was a rare characteristic. My wife's mother had the same attitude; she wouldn't allow her daughter out of her sight.

Daniel: You probably were quite free in your relationship with the children of other families in the comparable social level?

Lenzen: No, I had relatively few contacts with other children,

Lenzen: almost none. There wasn't much social communication, except at school.

Daniel: What about the social communications of your parents and your grandparents?

Lenzen: My father was an active member of the Native Sons of the Golden West and he was a charter member of the Observatory Parlor down in San Jose. When the Native Sons had their annual Admission Day celebration--in those days, 50 and 60 years ago, a great deal was made of Admission Day, it isn't now anymore, as a matter of fact you see the stores are all open--

Daniel: The Native Sons at that time were important.

Lenzen: Yes. I remember on one occasion in San Jose my father was a marshal in a parade. He took a very active part. He was also a Mason, and he was an Odd Fellow and a Maccabees. He belonged to all those lodges. When we were living in San Francisco my mother joined the Rebeccas, which is a section of the Order of the Eastern Star, connected with the I.O.O.F. She was fairly active in the Eastern Star.

Daniel: Your father's affiliations would be partly as a matter of business.

Lenzen: Yes. I guess he went to every lodge in San Jose, but in San Francisco he didn't because he just spent all his time in his office.

Daniel: Did your mother have any charitable affiliations?

Lenzen: No. If there'd been a P.T.A. at that time she would have been a very active member. There were no Boy Scouts. In fact, if there'd been Boy Scouts she wouldn't have allowed me to join because I would have gotten into bad associations probably.

Daniel: What about your religious background?

Lenzen: The Lenzens were Catholics. My paternal grandfather, Jacob Lenzen, sang in the Catholic church. I understand that he was an architect for the Catholic church. But my grandmother was a Protestant. And I never saw the slightest trace of any religion in my father. My stepgrandfather was a Catholic but my maternal grandmother was a Protestant coming from a Protestant part of Europe, and they had no formal religious life. I can recall that a few times I attended some Sunday School in San Jose, but that's all.

Daniel: What Protestant?

Lenzen: I think it was Lutheran.

After she graduated from a little white or red schoolhouse half a mile from where my stepgrandfather's farm was,

Lenzen: my mother was sent down to the College of Holy Names in Oakland. It was a Catholic school on Lake Merritt. She said they were very nice to her, and she always had great affection for them, but she never was converted to Catholicism.

Daniel: It didn't bother the family to send her to a Catholic boarding school?

Lenzen: That was one of the few ways in which a girl from the country could get an education, and housing and boarding, and so on. There was no high school up in the Byron-Brentwood area, nothing near her, so she went to this school. But there was no religion except what you'd find in the general environment.

Daniel: It's interesting that the College of Holy Names didn't mind taking in a Protestant.

Lenzen: It may be because her stepfather was a Catholic, although there was never any evidence that he practiced it.

Daniel: Returning to your grammar school days, what more would you say about them?

Lenzen: I still have my first report card.

Daniel: Could you describe it?

Lenzen: I haven't looked at those report cards in some time. They

Lenzen: were rather general, I think, attendance and deportment, and whether one's scholarship was satisfactory or not.

Daniel: Did you go to school in Oakland?

Lenzen: Yes, when I was ten or eleven years old. Then we moved to San Francisco. First I went to the Moulder Primary School. At the sixth grade I went to Fremont Grammar School, and there I graduated from the eighth grade. At just about that time I began to be an outstanding student. I would be number one, two, or three in the class. I have the report cards of the sixth, seventh and eighth grades. These were in great detail, with grades in arithmetic, spelling, writing, and so forth, given in percentages, even to decimals. It must have been a lot of work for the teachers to make these reports.

Daniel: What size were the classes?

Lenzen: I would judge about thirty or forty to a class.

Daniel: What about your teachers?

Lenzen: There was one teacher, a Mrs. Ivy Douglas Ostrom, who took a great interest in me and in the family. This was at Fremont Grammar School. She was a southern woman. She was very active in the Daughters of the Confederacy. And I have one rather interesting footnote to this: My spouse

Lenzen: belongs to the Mayflower Society. Her ancestors on both sides go way back to Massachusetts. One relative worked out the ancestry and we got it. So I had her join it. They asked her to give a paper on Christmas in colonial times, so I came to the library and looked up a number of things and wrote a very nice paper for her. It was an enormous success, and they're still talking about it. The reason that I bring this up is that among the sources from the library that I used was a little booklet on how George Washington spent Christmas. This book was presented to the library by the Albert D. Johnson chapter of the Daughters of the Confederacy in honor of this Ivy Douglas Ostrom, my schoolteacher.

Mrs. Ostrom was one of the school people who were instrumental in having the state legislature pass a tenure law for teachers early in the 1920s. She was a very able person, a widow, and she took quite an interest in me. My mother had a tendency to become acquainted with the teachers, so she became a friend of the teacher.

High School and Developing Interests

Lenzen: In the eighth grade I had a manual training teacher who asked me to do a particular job. At that time I was interested in marine engineering and naval architecture. This was in grammar school.

Daniel: How did you develop this interest?

Lenzen: I don't know. I just developed it.

Daniel: Where did you get the books that you used?

Lenzen: I got them from a bookstore and out of the public library. I spent a lot of time at the public library reading books on marine engineering and naval architecture. Reed's Handbook on Marine Engineering, especially, a book with diagrams and so forth that was used by apprentices who wanted to become chief engineers on boats. Anyway, I had ambition at that time to become a marine engineer, which would have been utterly foolish because I really was not suited, was not mechanically inclined. I was interested in the theory.

Then at one time I wanted to go to the Naval Academy and become a naval constructor, design battleships and so forth. The reason I bring this up is that I had some

Lenzen: periodical which had an expanded page showing a cross section of an American battleship, and this manual training teacher asked me to make a reproduction of it. I did that. I made a copy of this cross section of this battleship.

Daniel: You didn't trace it?

Lenzen: No, it wasn't traced.

Daniel: You had enough competence in drawing to be able to construct the drawing?

Lenzen: Oh, yes. My father helped me a little, but for the most part I did it.

Daniel: Were you an office boy for your father when you were still in grammar school?

Lenzen: Yes, during vacation.

Daniel: How many people were in your father's office?

Lenzen: There were times when he had a number of people working for him, but he liked to do most of the work himself. He was very meticulous and if he had much work to do he'd always arrange to work nights and Sundays. He was preoccupied with his work; he worked all the time; he didn't do anything around the house.

Daniel: Were you much help to him?

Lenzen: I didn't have to take any interest in the affairs of the

Lenzen: office. I used office time mainly for my own study. My main function was to be there in case somebody came in when he was out and wanted to know when he would be in. Mostly I was very much interested in these battleships.

Daniel: There had been the Spanish-American War. Was this responsible for your interest?

Lenzen: I don't know. I do remember the Spanish-American War. Right after that it was fashionable to wear a cape, a sort of a naval officer's cape, and I had a little suit with a cape that was modeled after a naval officer's. We had a picture showing me and my brother--I was around eight years old then, since my birthday is in December 1890 and Dewey sank the fleet in May 1898 wasn't it? But I remember the Spanish-American War.

I also remember the Russo-Japanese War, and I took a great deal of interest in that. In fact, on the occasion of the graduation exercises when I graduated from grammar school in 1905 I gave a talk on the battle of Tsushima. At that time the American public was strongly pro-Japanese. In fact, the Japanese had the Anglo-Japanese alliance, and Americans were very partial to the Japs. Of course, later on when the Japanese began flooding into California and

Lenzen: there was this law restricting their ownership of land--

Daniel: That was different. Now, let's go back to this speech. You gave this on the occasion of your graduation, and you were selected on the basis of your scholarship?

Lenzen: I was the number one student, you see.

Daniel: And what about the choice of that subject?

Lenzen: I was interested in that war. I followed it in the greatest detail and used to report to the class how many Russians and how many Japanese were killed the day before they appeared in the newspaper.

Daniel: You had an active interest in the history of the time. In this respect you were not introverted.

Lenzen: No, I don't think so. I don't think I was really natively unsocial, but you see, my mother rather restricted my interests. She didn't want me to get into bad company, say, down with the sort of children that would live in the Mission District and that were the children of industrial workers and so forth, the laboring class. But plenty of women, mothers of those days, had ideas like that.

Another thing. About that time I read the life of

Lenzen: Lord Nelson, and I think I read Admiral Mahan's book on the influence of sea power upon history. I was quite interested in all of that.

Then about that time the automobile came in and I was very much interested in theoretical aspects of the automobile, the whole construction, the whole business of it, all of the mechanical parts. The differential was one of the things that interested me.

At that time I used to buy or had subscriptions to magazines like Motor Age and Cycle and Automobile Trade Journal and Marine Engineering and Scientific American. Marine Engineering was almost my favorite periodical because it had pictures and drawings of battleships and I was very much interested in them. I knew all about the details, the exact size of the cylinders of the engines on the various battleships, and their horsepower.

Daniel: You were also interested in the strategy of battle?

Lenzen: I don't think I was interested in the strategy. I was interested in the event and what happened and how it happened.

Daniel: Were you a protagonist of one side or the other?

Lenzen: Of course, since the general sympathies of Americans were

Lenzen: pro-Japanese I was pro-Japanese. You see, at that time, 1905, there hadn't been sufficient influx of Japanese to create antagonisms. But later on there were great antagonisms.

Daniel: What about your brother and sister? Did they have any of these same interests? Or did you have any relationship with them on a thinking level?

Lenzen: I don't think I had any relationship with them on a thinking level.

I studied these magazines and books on marine engineering a great deal and I used to go to the library a lot. My favorite recreation was to walk by myself all over San Francisco. I'd start out early in the morning.

Daniel: Where did you live?

Lenzen: We lived at various places--on Geary Street, and Stanyan Street, and finally on Irving Street, which is in the Sunset District, not very far from the medical school, between 17th and 18th avenues.

Daniel: Where did you walk?

Lenzen: I walked to the other end of San Francisco, just wandered by myself. I used to walk in the direction of the Union Iron Works where they built battleships. It was around

Lenzen: Hunter's Point. I would spend a whole day, leaving early in the morning on a Saturday or during vacation. Then at noontime I would buy a bag of cookies. That would be my lunch. And I'd get home in time for dinner.

Daniel: It didn't worry your mother that you were gone all day?

Lenzen: No, she didn't worry because she knew I wouldn't get into bad associations.

Daniel: What about the beach?

Lenzen: That didn't interest me.

Daniel: What about the railroad yards?

Lenzen: No, I wasn't interested in railroad yards. There was a period when I was interested in collecting transfers, and wandering around the city I would pick up these transfers. I wasn't the only one. I think many schoolboys had an interest in collecting transfers.

Daniel: You went by yourself.

Lenzen: I didn't go with anyone else. I walked all by myself.

Daniel: And this was perfectly natural.

Lenzen: It seemed to me perfectly natural.

Daniel: What about the selection of the California School of Mechanical Arts as your high school. Were there several high schools to which you might have gone?

Lenzen: There was Lowell High School, which was academic; then there was Polytechnic. Then there was the Lick School and the Mission. Mission, although Sproul went to that, did not--

Daniel: What kind of school was Mission High School at that time?

Lenzen: We never thought of it as existing because it didn't have a football team.

Daniel: Football was important?

Lenzen: Oh yes. The three leading schools were Lick, Lowell, and Polytechnic for football. There was great rivalry between Lick and Lowell. And track was very important. But Mission High just didn't count. In these track meets there was the San Francisco Athletic League, then there was the Bay City, and there was the Academic. There were three track meets. Lick and Lowell would always enter full teams. Maybe there would be one or two runners from Mission but never more than that.

Daniel: These were amateur athletics?

Lenzen: Oh yes, just for high school students. The various schools would have teams. Oakland High always had a good team, and Berkeley High usually had a good team. Healdsburg usually won the Academic Athletic League, they had good teams. The

Lenzen: leading schools were Lick and Lowell.

Daniel: Did you ever participate in athletics?

Lenzen: Yes, I tried. When I was a freshman in high school I went out for track. I was never very good, but I trained very faithfully. I tried to run the half mile. As soon as the season started I would be out on the track right after school. In those days there wasn't the coaching and supervision that there is now.

Daniel: You did this on your own?

Lenzen: Of course the older boys may have made some remarks, but I was really on my own.

Daniel: Did any of the teachers devote time and interest to athletics?

Lenzen: We had a teacher of chemistry, Sydney Tibbetts, who was a graduate of the University of California and had won the two-mile against Stanford. He was known to be a track man, but I don't think he really did anything for us.

Daniel: He didn't come out with you when you practiced?

Lenzen: No, he didn't.

Daniel: Who was on hand?

Lenzen: There was hardly anybody, the students had to do it on their own. There wasn't the supervision and organization

Lenzen: that there is now. In my school there was no physical education teacher, no physical education. The students would try out, of course. We'd just run against one another and the best ones would be selected. I did finally make the team and did participate. I ran the half mile, but I didn't do so very well.

Daniel: What about your equipment? You bought your own shoes?

Lenzen: Oh yes, everything, jockey straps and shorts and sweater and track shoes.

Daniel: How were you drawn to this activity?

Lenzen: After school started there was a rally and the captains of the various teams urged the students to come out for the team, so I went out for the track team and did what I could. I took great interest in the meets and always attended. They were held at the California Oval in Berkeley. The track was then just about at the west end of the Life Sciences Building, five laps to the mile. All the high school meets were held there.

Daniel: There wasn't a place in the city for them?

Lenzen: Our school, the Lick School, was right across the street from the Wilmerding School, and the Wilmerding School had a track. So there was a place to run.

Daniel: Getting back to the choice of Lick as your school, what determined it?

Lenzen: I think the family thought that this was a school where the students were taught practical things. It had a fine reputation.

Daniel: There was training in practical skills.

Lenzen: Yes, machine shop and so on.

Daniel: Did you develop your ideas and interests by yourself, or did you talk to your father about this sort of thing?

Lenzen: No, I was mainly on my own.

Daniel: To what extent did the family plan your future?

Lenzen: There wasn't any general discussion.

Daniel: Was your mother interested in what you might be?

Lenzen: The only thing that I can remember that she once said to me was that her stepfather had said that I should learn a trade. In those days you learned how to be a carpenter by being an apprentice for five years, or a machinist by being an apprentice. Well, I wasn't at all adapted for that sort of thing.

Daniel: What did you want to do?

Lenzen: I had that idea that I would take the examinations for admittance to the Naval Academy. At that time battleships

Lenzen: were designed by Naval Academy graduates who were given special additional instruction at, I think, M.I.T.

Daniel: This was really what you were aiming at?

Lenzen: Oh yes. I had scrapbooks in which I had pasted pictures of the battleships and cruisers of the various navies of the world, all navies. One book I had was Peake's Naval Architecture. This was mainly about wooden ships so it didn't interest me, I didn't make much use of it. I was interested in warships, but once I started in high school I lost that interest and became more interested in pure science and pure theory.

Daniel: Did your teachers lead you on in this direction?

Lenzen: No, if anything they threw cold water on my inclinations. You see, in my senior year one of the things that somewhat interfered with my development, in my opinion, was that people thought that I wasn't sufficiently well-adjusted socially.

Daniel: What people?

Lenzen: Teachers in school.

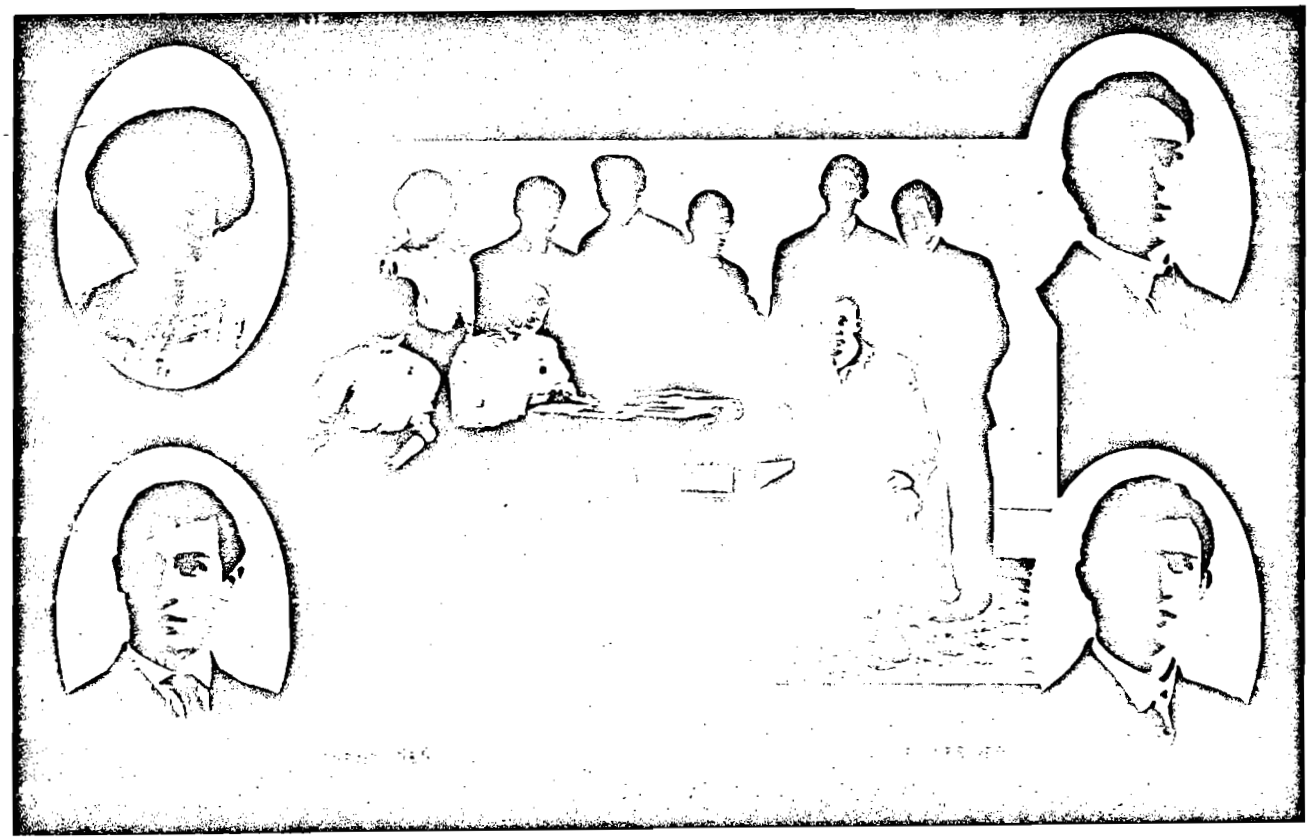
Daniel: How did they think you manifested it?

Lenzen: I don't know. I don't think it was justifiable because I attended every rally and all the games and yelled, and

Lenzen: so forth and so on. What more could you want?

I remember one incident. This was a small school; there were only about 75 in my class, and comparatively few girls. Some of the boys were rather socially inclined. There was to be a school play, a senior play. The teacher who was arranging this wanted me to take a very minor part in the play, and I didn't want to do it. The feeling I had is that she wanted to suppress the activities of these more prominently social people. I didn't cooperate, you see. It was just an insignificant part.

In my junior year I got involved in debating as a result of various tryouts, and in my senior year I was a debater and a very successful one on the school debating team. We won our debates and I got some medals. But I always felt that I spent too much time on those debates and it interfered somewhat with my school work. By that time I was primarily interested in achieving scholastic excellence. I worked very hard, though, at the debating. There were three men on a team and I wrote the speeches for the other two and myself. [Laughter] I spent a lot of time at the library. One of the members of my debating team is one of the most prominent attorneys of the East



DEBATING TEAMS, 1908-1909.

Lick High School Debating Team, 1908-09. President, Willard Beatty; vice-president, Victor F. Lenzen; treasurer, Helmut Strouss; secretary, Florence Manheim. At table, seated: Lillian Capp (Mrs. J. Deter McNary), Florence Manheim, Rolla Bishop Watt. Standing: Irene Frank, John Beuttler, Freeman Mills Smith, Helmut Strouss, Victor F. Lenzen, Thomas M. Carlson.

Lenzen: Bay, Thomas Mervyn Carlson in Richmond, quite a famous attorney. I was in the class of 1909 at the Lick School and he was 1911. I used to have to write his speeches; he had wonderful delivery but he had to have somebody to do the work for him.

Daniel: Social pressure determined your non-academic activities; what influences controlled your studies?

Lenzen: Just about at the beginning of my senior year (1908) I learned something about radioactivity. I became interested in so-called modern physics, the structure of the atom and so forth. The basis for that interest was in what I discovered in a branch public library in San Francisco on 16th Street. My family lived on Irving Street in the Sunset District and the school I attended was at 16th and Utah in the Potrero District. This was quite a distance from my home and it required riding on three different streetcars to get from home to the school. But I used to like to walk a great deal, and I think, to a considerable extent, I walked from home to school. On 16th Street near Church Street there was a branch library there where there were books on shelves, and I discovered

Lenzen: a book by Robert Kennedy Duncan, The New Knowledge. This told about the discoveries in x-rays and radioactivity. Radioactivity was discovered in 1896 by Becquerel in Paris. Then around 1900, shortly after that, principal contributions were made by Rutherford in McGill University at Montreal. In fact, he sort of analysed the nature of the radiations better than anyone else. This information was set forth in this book by Duncan, and it is interesting to me to have read an account of Dr. Edward Condon--he's been a well-known figure in physics--in which he said he was stimulated by this book.

But there was another source of information. In this library, on the shelves, there were reports of the secretary of the Smithsonian Institution, and appended to these reports were scientific articles of a popular nature, some of which were translated from the French and German. And I read those articles. About the end of my junior year it was my hope to get hold of some radioactive material and do some experimenting. But then debating interfered.

When I applied for entrance to college I signed up for the college of chemistry because I thought subjects

Lenzen: like radioactivity and atomic structure belonged in chemistry. Then just shortly before college opened I went to the school and talked to the chemistry teacher. I indicated that I wanted to become a scientist and he threw cold water on the idea. At the very last minute I became timid and requested my college changed to mechanical engineering. You see, this school prepared many different students. It had apprentices and turned out machinists and cabinetmakers and molders and forge workers. But about a third of the class took the college preparatory course which prepared more for engineering. There were a dozen of us who came from that school and entered the University of California in 1909. I think one or two were in chemistry, and the others were in engineering, and one in architecture, but all technical.

Daniel: What did your family think about your going to the University?

Lenzen: I think that the family thought I couldn't earn a living, because of my nature, unless I had an education. They were quite fearful of me. They thought that I was peculiar, you see, and studious, and interested in these theoretical matters. They decided that I probably wouldn't be able to make a living unless I got an education. My brother was

Lenzen: considered a more practical and sociable person than I, and they didn't worry about him, but they were quite worried about me.

Daniel: Did he come to the University?

Lenzen: No, he didn't.

Daniel: Where did he go to high school?

Lenzen: He went to the same high school.

Daniel: Then what about your sister?

Lenzen: She went to Girls High School and Polytechnic High School in San Francisco.

Daniel: Did she come to the University?

Lenzen: No, my sister didn't care for it.

Daniel: They finished their schooling at the secondary level?

Lenzen: Yes.

UNIVERSITY YEARS

Study at Berkeley, 1909-1913

Classical and Modern Physics

Daniel: When you entered the University in 1909 you wanted to be in chemistry but you weren't. Why?

Lenzen: The man who discouraged my enrollment in chemistry was a very wonderful teacher. I'm not blaming anybody. If I have made mistakes, they have been due to a certain lack of resolution or judgment on my part.

Although I had changed my college to mechanical and electrical engineering, during my first year I continued studying modern science. One book I especially made use of was J.J. Thomson's The Corpuscular Theory of Matter. About that time J.J. Thomson of Cambridge University, England, who later became master of Trinity College, had the most highly regarded theory of the structure of the atom. That theory is just a matter of history now because

Lenzen: of later experiment. I remember studying that book.

Daniel: Was it part of the reading for a course you were taking?

Lenzen: No, I discovered it for myself.

Daniel: You were enrolled in what?

Lenzen: Mathematics, physics, chemistry, civil engineering, hygiene, and physical education.

Daniel: But the physics that you were learning wasn't including the ideas--

Lenzen: It was just classical physics.

Daniel: Not concerned about the internal structure of matter.

Lenzen: No.

Daniel: What did it cover?

Lenzen: Mechanics, the motions of large-scale bodies. In fact, the first semester, Physics 1A, we read of and dealt with the sort of ideas that were developed in the 17th century. There was almost no so-called modern physics in the elementary physics courses at that time. Of course, I didn't take the elementary course. I took the two-year course that was required of engineers, Physics 1A-B, and 2C-D, it was called.

Daniel: I think it would be useful for you to characterize classical and modern physics.

Lenzen: I would say that classical physics is the physics that was developed in modern times up to the year 1895 when x-rays were discovered. Modern physics began with the discovery of x-rays by Roentgen. And it was Becquerel experimenting with vacuum tubes from which x-rays were generated who then accidentally discovered radioactivity. Also about the same time J.J. Thomson demonstrated the existence of the electron. That was about 1897.

So the idea came to be accepted that electrons or corpuscles of negative electricity were universal constituents of all matter. This led to the problem of constructing a theory of the atom. And since Thomson insisted that matter is ordinarily neutral electrically, an atom would have to be built up of positive and negative electricity. J.J. Thomson atoms consisted of a sphere of positive electricity with electrons imbedded in it.

About 1911 Rutherford reported an experiment in which he set forth the theory that the positive part of the atom is concentrated in a very small nucleus, and that was the beginning of the nuclear atom. The electrons were then supposed to revolve in orbit about this nucleus.

Lenzen: In 1913 Niels Bohr applied the quantum theory The orbits of the electrons were quantized. This provided the great impetus to the development of the quantum theory. Of course the quantum theory was first enunciated in 1900 by Max Planck. Then a very important addition was made by Einstein in 1905 and also 1907. But the thing that really brought it into the stream of physics was the development of the Bohr atom of 1913. (Of course, having the background in the conversations with Professor Birge, you ought to know something about that.*)

Now, one might raise the question--I was highly regarded by my instructors in physics--why I didn't decide to become a physicist at the time. I was too reflective, I think; I had read that Galileo at the age of 18 discovered from observing the swinging lamp that the period of oscillation is constant, and it was my view that to become a scientist it was necessary to make a discovery by the age of 18, since Galileo had done so, and I had not made a discovery. I had hoped that in my senior year

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*Raymond T. Birge, Raymond Thayer Birge, Physicist, a Regional Cultural History Project interview by E.T. Daniel, Berkeley 1960.

Lenzen: in high school by studying radioactivity I could do something that would justify my going into the field.

Philosophy

Lenzen: I also remember that during my freshman year I found a book in the library (the library was in Bacon Hall and the stacks were open to all students) entitled The Art of Scientific Discovery, by Gore. I read that book and I questioned whether I had the qualifications.

About the middle of my sophomore year I picked up an old-fashioned book in a second-hand store in San Francisco on what might be called mental and moral philosophy. I don't remember the name of the book, but in the rear of it there was a brief history of philosophy. I read about the early Greek philosophers, Thales, Anaximander, and Anaximenes. They were interested in the nature of matter and I got the idea that philosophy was the study of the nature of ultimate reality, and this was what I was interested in.

Daniel: This was your first knowledge of philosophy?

Lenzen: Yes, absolutely my first contact. I had the ill fortune never to talk to anybody who knew anything about the

Lenzen: subject. It was this book that I picked up in a second-hand store, whose title I don't remember, which started me studying philosophy, and I came upon the fact that the philosopher Kant published his Critique of Pure Reason at the age of 57. This encouraged me. Foolish--

Then furthermore, I decided that I had certain philosophical ideas, and a certain capacity for original thinking.

Daniel: You could do this outside of the laboratory.

Lenzen: I wasn't against the laboratory.

Daniel: You hopped over what had been your chief interest, science, to another subject.

Lenzen: This was a very erroneous judgment on my part because the concern of these early Greek Socratic philosophers was really the concern of the present-day physicists, only in those days they didn't have the laboratory experiments and they didn't have the mathematics with which to develop their theories.

So, I decided that philosophy was my field. I asked people for advice. I spoke to my mother and father about it, and a former grammar school teacher of mine who took an interest in the family. But my family didn't know

Lenzen: anything about philosophy.

Daniel: What about the people on the campus?

Lenzen: Well, I didn't consult with them.

Daniel: Why?

Lenzen: Oh, I don't know. I didn't know any of the philosophers on the campus. So, I made a decision to major in philosophy without having had a course in the philosophy department or without having talked to a philosopher. I made the transition in the second half of my sophomore year. I left the College of Mechanical and Electrical Engineering and I went into the College of Natural Sciences. At that time there were three colleges, the College of Natural Sciences, the College of Social Sciences, and the College of Letters. I needed Latin and beginning Latin was not taught in the University at that time. I hadn't had it because my high school trained people to be engineers or mechanics. But there was a course in Latin given by a student who was preparing to teach, so I took that. During the summer I studied Caesar and other Latin authors and took the entrance examination. The University gave entrance examinations then. And I got credit for more Latin on the basis of the examination and self-study.

Lenzen: Then, although I didn't have to do it, I took the examination in Greek history and got credit for that. Although the California School of Mechanical Arts prepared for engineering, the academic program for the college preparatory student would be considered ultra-liberal in the present day because there were four years of history with one whole year devoted to the history of Rome. You can't imagine a high school nowadays doing something like that. Also, in freshman English in high school we had studied the Odyssey in George Herbert Palmer's English translation. Then another thing I did in high school, I wrote a paper illustrated with pictures which I was able to purchase from outside sources, of Greek and Roman architecture. With my father's experiences in Europe, our house decorated with views of the Colosseum and the Bridge of Sighs and the Rialto, and with his big desk with his sketchbooks and his two big books of European views, it was very easy for me to take an interest in classical antiquity.

In my junior year I began Greek, but about three weeks after the beginning of school I was stricken with typhoid fever, so I was out that semester, and I never resumed that. Then I began taking courses in philosophy. About

Lenzen: that time there was beginning to be an interest in symbolic logic or mathematical logic. Professor B.A. Bernstein of the mathematics department was one of the pioneers in teaching the subject, and in the beginning of my junior year I decided to take his course in the elements of logic.

Daniel: How did the faculty regard your attack on the nature of reality?

Lenzen: A new man came to the University from Harvard, Professor C.I. Lewis. He's quite a well-known person now. He took quite an interest in me. Another person who took quite an interest in me was Arthur Upham Pope. Pope was interested in ethics and aesthetics, and during the summer he used to go back to Providence, Rhode Island, which was his home, and sell Oriental rugs. Pope later became famous as an expert on Persian or Iranian art. There are about five or six huge volumes, Survey of Persian Art, edited by Pope. He founded an institute for Iranian studies. I don't remember the exact title of it now. He has done an enormous amount of work in fostering interest in the art of Persia. Well, he took quite an interest in me. So did G.P. Adams, to a certain extent, but mainly C.I. Lewis

Lenzen: and Pope.

During my senior year Professor C.I. Lewis told me that Bertrand Russell was going to be at Harvard in the spring of 1914. I was graduating in May 1913 and he said it was the opportunity of a lifetime to study with this great logician. At that time Russell was at the height of his fame as a logician and as a philosopher. In the present day he is noted primarily as an opponent of nuclear tests, and he has ideas about marriage and morals which people have criticized. But all of these things were unknown at that time. Russell's Principles of Mathematics had appeared in 1903 and was one of the fundamental bases of a philosophical movement in this country called the new realism. Professor C.I. Lewis had published some papers in symbolic logic disagreeing with Russell's ideas.

In 1910 the first volume of Principia Mathematica by Alfred North Whitehead and Russell came out. Whitehead was ten years older than Russell. They collaborated in doing the mathematical part of the program that Russell had outlined in his Principles of 1903. C.I. Lewis had been given a copy of Whitehead and Russell's Principia

Lenzen: by Professor Royce, under whom he studied at Harvard, and had written some papers giving a different point of view. So I acquired a copy of the Principia, volume one.

I was rather reluctant to go to Harvard because I was a bit uncertain about continuing in philosophy. I wasn't sure that this was the right thing, and I wanted to remain another year as a graduate student here. The fact that I had been ill didn't make me quite as assiduous a student as I should have been. Then I had a very good friend--I'm not going to blame him--who was interested in student activities. These matters interfered a little with my studies, although I guess I had the highest scholarship record in the class, just as I did in grammar and high school. But I wasn't satisfied. You see, I never measured myself against my fellow students; I always measured myself by some far-off ideal. Professor C.I. Lewis, who was certainly very kind and helpful, got a scholarship from the Harvard Club of San Francisco for me and I got to Harvard in the fall of 1913.

Graduate Work at Harvard

Lenzen: Russell didn't get to Harvard at the beginning of the 1914 spring semester. He was delayed a couple of weeks and didn't get there until March.

Daniel: You had one semester there with no Russell at all.

Lenzen: Yes, in the fall. But I took certain basic courses.

Daniel: By the way, before going on, how did you take Harvard?

Lenzen: I just took it in stride, and as I look back on things now, I made several mistakes. I tried to do too much. The normal load of the student was four courses, but I audited some additional courses.

Then the president of the philosophy club persuaded me to read a paper to the philosophy club at the beginning of the second semester. The president of the philosophy club was T.S. Eliot, the famous poet. He was a fourth-year graduate student, and I think nobody suspected that he would ever become an eminent poet and literary critic. I knew him fairly well. I was, of course, only a first-year graduate student.

Daniel: What was your paper about?

Lenzen: It had to do with realism and idealism. I was somewhat

Lenzen: influenced by C.I. Lewis. He was, I think, neo-Kantian, and had criticized the realistic doctrine.

It was a successful paper. But because I had spent so much time auditing additional courses I had to work too hard during that Christmas vacation. This is an illustration of the fact that I lacked judgment, because when you are being judged by some professors as a graduate student, they will judge your performance in the courses in which you are registered. One course of lectures that I audited was by Professor Robert M. Yerkes, a psychologist especially interested in animal behavior, and I think he performed certain experiments on the chimpanzee or the ape that are historic. But it's a question of whether I should have used up energy in this way. I didn't really take a vacation and about the middle of the second semester I became sort of exhausted and tired.

I might say that T.S. Eliot at the end of the year got a Sheldon fellowship from Harvard University to go to England. He was writing a doctor's dissertation on Bradley's conception of the absolute. At that time a philosophical work which was very much studied was F.H. Bradley's Appearance and Reality. In fact Professor

Lenzen: C.H. Rieber of the University of California conducted a seminar in it.

But Eliot, when he got to England, fell in love with some English girl, married her, started to teach school, and began writing poetry. So he didn't come back. So far as I knew he never got his Ph.D. from Harvard. These items are not published. I read a long account of T.S. Eliot in the Saturday Review some months ago, and the indication is that he was reluctant to tell about his background and past history. The article explained that he was a fairly grounded student of philosophy. I haven't paid much attention to his poetry. What I've seen of it doesn't attract me, doesn't appeal to me, and I haven't read any of his critical articles. But every once in a while I see some article about him. When I was in England I spent an afternoon with him at tea.

Bertrand Russell

Lenzen: Russell finally appeared in March, and he was a very delightful person. His lectures were the wittiest that I have ever heard. They just sparkled. And he was at the

Lenzen: height of his power. You see, he was only about forty then. He had completed his work in mathematical logic and was going on to certain other things in philosophy. He gave some lectures at the Lowell Institute in Boston. He was a most affable and engaging person with respect to students. We just hung on every word. No man ever had more adoring students than Russell did.

Royce, who was a distinguished professor of philosophy at Harvard, and no doubt you've heard of him, said Russell had received more attention than any other logician since Aristotle. He didn't say he was the greatest logician since Aristotle; he said he's received more attention. And that was true. Russell had quarters in an apartment house a few blocks from the Harvard Yard. Once a week he had tea for us students.

Daniel: How large was his class?

Lenzen: There were two classes. He had one course in mathematical logic which was a small group with about twenty in it. Then there was a larger course, the theory of knowledge, which had possibly 50 or 60, and even some of the professors attended that course. I took both courses.

Daniel: He gave a series of lectures clothed with bibliography and

Daniel: suggestions for reading?

Lenzen: I don't know that he suggested any bibliography. He just lectured. Then there might be some discussion. He invited discussion. But he was very delightful and very kindly.

I early became known to him. He invited me to dinner to the Colonial Club, which was the Harvard version of the Faculty Club here. I think I told him that I was interested in the philosophy of science. He said, "Well, I want you to write a paper for me on causality and it must be treated from the standpoint of differential equations." This was a great blow to me because I decided right then and there that I was not properly prepared to write that paper.

Daniel: Anybody would have been "not properly prepared" at that time, I think, don't you?

Lenzen: Well, yes.

Daniel: This was an impossible subject.

Lenzen: No, it wasn't an impossible subject but it required that a person be a mathematical physicist to do it.

Daniel: Were there any mathematical physicists?

Lenzen: There was Benjamin Osgood Peirce at Harvard. And there was a man at Clark University, Arthur Gordon Webster.

Daniel: It sounds like an awful assignment.

Lenzen: Oh yes, very.

Daniel: Why did he do this?

Lenzen: He apparently thought well of me.

In 1954 there was published a little booklet of mine entitled "Causality in Natural Science." I was invited to write it, and I wrote it during the year 1951-1952. I feel that that book was finally the answer to Russell's topic. It took me all that time, you see.

Daniel: How did you handle the assignment in 1914?

Lenzen: I didn't say anything in particular about that assignment. I wrote him papers on other subjects. I might say that he autographed my volume one of Principia Mathematica, and that's now in the Rare Book Room of the University Library. Also I have my copy of his Problems of Philosophy. He autographed that. That's also in the Rare Book Room. I entrusted to Miss Dornin [archivist, University of California Library, Berkeley] the term papers that I wrote for Russell with comments in his own hand. The Library seems to be interested in such things.

But the effect of Russell upon me was that I decided that I was not properly equipped to do philosophy of science.

Daniel: Russell was a provocative influence.

Lenzen: His ideas received an enormous amount of attention. They were the basis for extended discussions in the seminars and among the students. He used the methods of mathematical logic to define his ideas in the field. His lectures were entitled, "Our Knowledge of the External World as a Field for Scientific Method in Philosophy." Russell gave everyone the idea that as a result of the techniques of the new mathematical logic, it would be possible to formulate and follow philosophical problems in a scientific manner.

Daniel: This was a new idea?

Lenzen: Well, I wouldn't say that. In the 17th century there had been an attempt at exactness on the part of Spinoza. He wrote his Ethics in the form of Euclid's geometry--postulates and definitions and so forth. This was assumed to be offered as an exact system of philosophy. Descartes, who in a sense is the founder of modern philosophy, tried to be scientific; he first doubted everything, and then, after having doubted everything, sought to find the one thing that he could be absolutely certain about, and that was his own existence, and therefore he said, "Cogito,

Lenzen: ergo sum"--"I think, therefore I am." From that he derived the various philosophical doctrines, that he had doubted, and so he was trying to be scientific.

Russell was very influential, partly because of the enormous prestige of mathematical logic. All the student had to do in a seminar was to write some symbols on the board and everybody looked on with wonder and awe. It reminds me of the description of the village schoolmaster by Oliver Goldsmith.

But the Principles of Mathematics of Russell, which came out in 1903, furnished a lot of the basis for this new realism. The Principia Mathematica of Whitehead and Russell (1910) of course made a tremendous impression on students of philosophy, most of whom couldn't understand it. Russell, you see, had this brilliant and dominating personality. He had clear ideas that were interesting to work with and they had the prestige of Russell behind them.

Daniel: Did you figure out any loopholes in his thinking?

Lenzen: Only in this respect: the predecessor who had the greatest influence on Russell was a German mathematician named Gottlob Frege who anticipated many of the ideas that Russell got later, but Frege used a very complicated

Lenzen: system of notation which made his work almost impossible to read. He wrote several books on the foundation of arithmetic. He wrote a paper entitled "Ueber Sinn und Bedeutung," "On Meaning and Denotation." In an appendix to his Principles of Mathematics, Russell set forth this distinction of Frege and criticized it. Well, one of the things that I did was to read Frege's article myself and then write an article for Russell criticizing Russell's interpretation. I said that Russell had incorrectly interpreted Frege, and Russell agreed with me, but whether he did so out of politeness or not I don't know. This was rather important so far as my thesis was concerned because the nature of meaning was central to this thesis. This was really the beginning, I might say, of the ideas which led into my thesis.

In the summer of 1914 I stayed at Cambridge instead of coming home to San Francisco, and I spent most of the summer reading Edmund Husserl's Logische Untersuchungen, Logical Investigations, just out.

Daniel: How did you come on this?

Lenzen: I really don't know how I discovered it, but I did discover it, and he discussed problems of meaning. Husserl called

Lenzen: himself a phenomenologist, and you might say that phenomenology is the study of the essence of phenomena. It was really a new term. The question was to determine the structure of phenomena, the essence of phenomena. Husserl considered these problems of meaning and denotation, and I was interested in them because of this Frege-Russell problem, and so I spent the summer at Cambridge studying Husserl and taking notes.

Then there was another philosopher that I was interested in, and this was an Austrian by the name of Alexius Meinong, and he had set forth a point of view called Gegenstandstheorie--theory of objects--and this was somewhat related to phenomenology. My Ph.D. thesis [June 1916] consisted of a system which I worked out for myself involving elements of Russell, elements of Husserl, elements of Meinong, and elements of Charles S. Peirce, because Royce was talking about Charles S. Peirce, an American philosopher. Peirce set forth a theory of signs which I made use of in considering meaning--the question of the relation or the significance of a sign comes in, and that's all part of this general problem of the nature of meaning in its relation to objects.

Daniel: What was the title of your thesis?

Lenzen: "Outlines of the Science of Phenomenology with Especial Reference to Meaning and Truth."

I think I neglected to state that when Russell came to Harvard he spoke about Wittgenstein. Wittgenstein, in the history of philosophy of the 20th century, probably would be looked upon as the most outstanding proponent of logical positivism. Of course, the term logical positivism was invented in this country; it was not invented in Europe. Wittgenstein was an Austrian. I never met him personally. There are all sorts of stories about him. Russell apparently was quite enthusiastic about him and told us about him in his lectures in the spring of 1914. Subsequently Russell translated a work of Wittgenstein's called the Tractatus. It consists of the German in one column and Russell's English in the adjacent column. That has been a very influential work. In 1930, when Moritz Schlick was here for a year, his seminar consisted of a study of the Tractatus, and this has exerted an enormous influence on, you might call it, analytical philosophy.

Of course, there have been philosophers who rejected the whole point of view, because in effect logical posi-

Lenzen: tivism denied the traditional task of philosophy, and the point of view of logical positivism has sometimes been expressed by the statement that philosophy is not a subject in itself but the clarification of meaning. I remember when Schlick gave a public lecture here, which was attended by Herman Weyl, the eminent mathematician who was also interested in philosophy and wrote a number of things that are very good. Weyl at the end of the lecture came up to Schlick and protested. In the course of the lecture Schlick had said that this work (Tractatus) was the most important philosophical treatise in five hundred years, and he looked forward to the time when there would be no department for philosophy, but there would be philosophical activity in the various fields of science.

In any case, I thought I ought to mention the fact that Russell introduced us at least to the name of Wittgenstein.

Daniel: What was your feeling about these ideas?

Lenzen: I don't think I paid very much attention to them. I paid attention to Russell's ideas, and subsequently, after the summer of 1914, to Husserl and to Meinong, and I paid a little attention to Peirce's theory of signs.

Daniel: Have you any way of considering the effect of logical positivism on other students?

Lenzen: I don't think that it had very much effect. Russell told about Wittgenstein, his genius and his original ideas, and the more or less esoteric nature of his doctrine. Wittgenstein was a very peculiar character: he inherited wealth in Austria, but he gave it all away; he tried to live the simple life. Subsequently he became a fellow at Cambridge University, and he lectured to a small group, but one had to have special permission to attend his lectures. He was very difficult to approach, he wasn't accessible to any student, and there is a story about a certain graduate student at the University of California --Daniel Belmont I think was his name--who had a fellowship to go to Cambridge and he was a member of Wittgenstein's group. When he came back to America he abandoned philosophy and became an accountant. That was the effect that apparently he had. Because if philosophy doesn't have any special subject matter and is merely clarification of meaning, then the whole history of the subject has been a mistake, and why would anybody want to pursue it?

One of the things that Russell told was of a meeting

Lenzen: between Wittgenstein and Whitehead. Apparently Wittgenstein made a pronouncement about some logical problem in a very authroitative and esoteric manner, and was considered a genius, but very few could understand him.

A second person Russell told us about, and in fact he had preef sheets, was John Maynard Keynes, the economist. I remember what Russell said about Keynes. Keynes's father was John Neville Keynes, registrar of Cambridge University, and he had written a book on formal logic which was considered about the best in its field--that is, the old-fashioned, traditional, Aristotelian, scholastic, formal logic--and I remember Russell told us one day that the son, John Maynard Keynes, wanted first to study philosophy but decided that he wasn't smart enough, so he went into economics. He wrote a book on the theory of probability which presents a theory of probability different from a lot of others, a concept of subjective probability.

He did not accept the view that probability was defined in terms of a statistical ratio, as for example Professor Jerzy Neyman here would, or Richard von Mises, or Reichenbach or most people in physics. There is quite a divergence between people as to the foundations of the

Lenzen: theory of probability.

Well, Russell had the proof-sheets of this book by Keynes, and he read us some of it. Of course, we hadn't heard of J.M. Keynes (this was the spring of 1914). And Russell said, "J.N. Keynes wrote an old-fashioned logic book, but his son is a much abler man." That was characteristic of Russell.

He was looked upon as--it's impossible to appreciate the enormous prestige that Russell occupied at that particular time. This was before any difficulties that he got into with his government over the war, or writing on marriage and morals, or writing letters on nuclear tests and debating with Teller, and so forth. He was to us the very embodiment of logic and rationality. Even now, when you see him on television at 86 or 87, he's still a very vigorous man, so you can imagine what he was like at the age of 40.

Ralph Barton Perry, Josiah Royce, and Other Figures

Daniel: We have been a bit carried away by Russell in your Harvard experience. Could we go back to other teachers and subjects?

Lenzen: I took a course in ethics under Ralph Barton Perry. He was a relatively young man, one of the six new realists. About 1910, I think it was, six American philosophers got together and formed a platform for a philosophy which they called "the new realism." This realism asserted that the objects of perception exist independently of the mind, whereas the idealistic doctrine, which was very influential in the United States and Great Britain in the second part of the 19th century, maintained that reality depends upon mind. And, of course, this could hardly support the point of view that the object of perception exists independently of the finite mind; therefore, they had to introduce an absolute mind, and this was absolute idealism, which was represented at Harvard by Josiah Royce and very much opposed by William James, the pragmatist. James died a number of years before I got to Harvard, so I never knew him personally. But his books on pragmatism and radical empiricism were very important items in the history of American philosophical thought. Ralph Barton Perry subsequently published a large book entitled <u>The Theory of Value.</u> He was a man who tried to be as scientific as

Lenzen: possible in his philosophical thinking, a method which I think is very difficult.

Daniel: How can one be scientific about philosophy?

Lenzen: Well, it was a matter of using scientific method, and philosophers have on repeated occasions attempted to be as accurate or as precise as scientists, but the difficulty is, the subject matter is too complex and also involves problems that really are not susceptible of scientific treatment in the ordinary sense of the term.

Daniel: What did you think of this idea at the time? Can you remember considering the inconsistences involved?

Lenzen: No, I didn't think of that, especially. When I attended this course in ethics by Professor Perry I wasn't especially interested in the subject, but I worked hard and did satisfactorily.

I also attended some lectures in Greek philosophy by Professor James H. Woods. He subsequently became chairman of the department. He isn't well-known as a philosopher. His special interest was Indian philosophy. He published the text for a translation of certain Indian philosophies after my time, so I haven't read it. But he was a very good historical scholar, and lectured on

Lenzen: Plato and the Greek philosophers, and he was very kind to me.

Daniel: What do you mean, kind?

Lenzen: Well, subsequently, looking after my welfare, taking an interest in me as a personality. As I have indicated, I was in a very disturbed state of mind, especially in the last year, because of my conflicts and doubts as to what I was doing.

Then a course to which I gave a good deal of attention was a course in logic by Dr. Harry T. Costello. He was a recent Ph.D. and instructor. Subsequently he became professor of philosophy at Trinity College, Hartford, and although he hasn't published much he was well known and regarded very highly, for the few things that he said were very worthwhile reading. He has retired now, of course. This course in logic was quite a comprehensive survey and a very good course.

Then I took Professor Royce's course in metaphysics, which followed more or less the line of his work, The World and the Individual. These were the Gifford lectures that he had given in Edinburgh. The term paper which I wrote, with his comments, and also my notes, I had deposited in

Lenzen: the Library Archives because Miss Dornin is making a collection of Royce material. Maybe this should be at Harvard, but Royce graduated from this University and he taught here for a number of years, and any biographer would have to come here anyhow. As a matter of fact, some months ago I had a telephone call from a Jesuit from St. Louis University who was writing a doctor's dissertation on Royce. He asked to confer with me and I spoke to him for about two hours. It was rather difficult, however, because I haven't thought of some of these matters for over forty years. However, I did my best.

Daniel: What most vividly do you remember about Royce?

Lenzen: I was very close to him, especially during my last year. In his course in metaphysics I remember waiting eagerly to find out what his theory of reality was. In brief, it is that ultimate reality is the fulfillment of purpose. I didn't think that this was sufficiently detailed. I would say that the most important thing that I learned from him was that the interest of the philosopher in the nature of reality was different from the interests that

Lenzen: I had in reality.

Daniel: In what way?

Lenzen: Professor Royce during the course of the term discussed the various types of philosophy--mysticism and critical rationalism and realism, and then finally, what I was waiting eagerly to hear, his own point of view. He had four different theories. I remember the exposition of his point of view very vividly. It was that reality is the fulfillment of purpose, and that an external object is the external meaning of an idea, and then your thought of the object is the internal meaning. He made the distinction between external and internal meaning. This, to me, was a disappointment. I had jumped into philosophy without sufficient thought, in my opinion, on the assumption that philosophers dealt with the nature of reality; and they do, they are concerned about the nature of reality, but in such a general way that I was disappointed. Now, whether reality is independent of mind or dependent upon mind is of course a very important problem from the standpoint of the philosopher, but it really wasn't what I was interested in. I was really interested in the sort of

Lenzen: information that was being obtained by the physicists.

Daniel: And reality was secondary?

Lenzen: The question of the ontological status, that was a secondary matter. Now here I'm using certain philosphical lingo in order to express myself, you see.

I got an erroneous opinion of philosophy from reading the early Greek philosophers. They were called cosmologists. They said that the fundamental substance was water. Heraclitus said it was fire. Then Empedocles said there were four elements, earth, air, fire, and water. Democritus said the world consists of atoms moving in the void, and so forth. Well, these were ideas about the nature of ultimate reality that I was interested in, and in the Greek period there was no separation of science, in the present-day sense, and philosophy. There was just one field of knowledge, called philosophy. A philosopher was a lover of wisdom.

In modern times we began to have special fields being lopped off of the general field of philosophy. In fact, it can be said that as soon as a particular field of knowledge becomes rather positively formulated and definite in

Lenzen: its concepts, it ceases to be part of philosophy and becomes a special science. At one time economics was part of philosophy. Mathematical logic, symbolic logic, was developed by philosophers initially, but now it's become part of mathematics.

Daniel: Are there more avenues of thought from Harvard courses and personalities?

Lenzen: We were supposed to have four courses, and I think the fourth one was Münsterberg's seminar in psychology. Hugo Münsterberg was professor of psychology; he was a very dominant figure about that time. He died suddenly in 1915 or 1916, just toppled over in class. He had been brought over from Germany by William James as a very promising young experimentalist. Münsterberg was also interested in philosophy, in voluntaristic philosophy. Voluntarism is the doctrine that the will is fundamental.

Royce's philosophy was voluntaristic because he defined reality as the fulfillment of purpose, and the absolute was that in which all purposes ultimately are fulfilled. Royce described categories of thought as forms of the rational will. This type of philosophy goes back--Royce was especially interested in and influenced by Schopenhauer--to

Lenzen: Schopenhauer's great work entitled The World as Will and Idea. The fundamental reality is will, and this was the basis of Schopenhauer's pessimism, because one is continually striving, that's a manifestation of will, for something, and this causes pain, but then when the striving accomplishes its purpose, one falls into a state of boredom, so that according to Schopenhauer there is oscillation between the pain of desire and the boredom of satisfaction. Royce was very much influenced by Schopenhauer.

I mention this because Royce is supposed to be in the Hegelian tradition. Idealism as it was understood in the 19th century derived from the Germans. First there was Kant, and then there were the post-Kantian idealists --Fichte, Schelling, and Hegel. Hegel's doctrine was absolute idealism. It received a great deal of attention in Great Britain: The Secret of Hegel by James H. Stirling; and J.B. Baillie translated the Phenomenology.

The Hegelian point of view was represented also by Bosanquet, a philosopher of fifty years ago who was very much studied. Bosanquet was represented at Harvard by Professor R.F. Alfred Hoernlé. I think Hoernlé's father

Lenzen: was British, and his mother was German, and he was born in India, I believe, but he studied at Oxford. He also taught in South Africa, and his wife was a distinguished anthropologist, an authority on certain tribes in South Africa. He was brought to Harvard the same year that I started, in 1913. I was in his seminar on Bosanquet. Hoernlé called himself an objective idealist, and he lectured on the British realist Samuel Alexander. Just about that time Samuel Alexander was publishing a series of articles on realism. Subsequently he wrote Space, Time, and Deity, and Hoernlé was setting forth his point of view. And the year that I was in Hoernlé's seminar, the work we studied was Bosanquet's Principle of Individuality and Value. Subsequently I spent a great deal of time on Bosanquet's logic. He had a two-volume work. It was an idealistic type of logic based on a so-called "coherent" theory of truth.

One of the difficulties about philosophy as a subject is that no two philosophers can agree. They can't agree upon the definition of terms, and of course that leads to discussion, and controversy, and to some people that's

Lenzen: very interesting. I mentioned Ralph Barton Perry, a new realist, one of six American philosophers who attempted to be scientific in coming to an agreement about certain fundamentals on which they could build. The other five were E.B. Holt, Edward Gleason Spaulding, F.S. Marvin, Durant Drake, and the man who said that "life begins at forty," Walter Pitkin.

They were very much criticized by Arthur O. Lovejoy, who was a professor of philosophy at Johns Hopkins and a California graduate, by the way, and no doubt the most learned man in the field at that time in the sense of knowing Greek and Latin and being erudite. He didn't have a constructive view but he was a very good critic, and he just went after the new realists, especially Perry. He was most unmerciful in his criticism.

You have to know some technicalities about the theory of knowledge to understand new realism. The realism which was most prevalent and which was the point of view of Descartes and Locke, and others, was called a dualistic realism. That is, the external object exerts an influence upon the mind and then the idea in the mind of the object or the perception is to be distinguished from the object.

Lenzen: This is a dualistic point of view, which philosophers will always try to overcome.

The aim of the new realist was to do away with this dualism, and to have what they called an "epistemological monism," in which the object is just as it is perceived. Now, this creates enormous difficulties because suppose you see a star. According to our science you see only the light that has left the star some thousands of years ago; then how can you be seeing a star as it actually is? There is the problem of illusions that is very difficult to explain on epistemological monism. These people tried to do that. The movement disintegrated inside ten years. When the critics got to work they could show the absurdities. The most effective of these critics was Lovejoy. His book, The Revolt Against Dualism, is an account of these attempts to establish a realism on the basis of an epistemological monism, and his criticisms.

Beginning the second year at Harvard I was in Royce's seminar in logic, scientific methodology, for two years; in fact, I was in it the last two years that he gave it. Beginning the second semester of the first year I was

Lenzen: appointed secretary and wrote minutes on the papers and discussions. I had two large volumes of notes and minutes, which I deposited in the archives. Of course in this seminar scientific problems were discussed.

Daniel: The ideas developed in the seminar were more satisfying than those in Royce's lectures you first heard?

Lenzen: Yes, I could say that. It wasn't on metaphysics, it was on scientific method. There were distinguished people in this seminar. There was Lawrence J. Henderson, a Harvard biochemist, who lectured. He was interested in the history of science and scientific methodology, and he wrote a number of books: The Order of Nature and Fitness of the Environment. His outstanding biochemical work was entitled Blood. He was brought to the University of California about 1930 as Howison lecturer. At that time the thing that interested him was Pareto, the Italian sociologist who wrote the General Treatise of Sociology. Subsequently this work was translated under the title The Mind and Society. I've always wanted to read it but never had time.

The central point, the essential distinction that Pareto made and that Henderson was very much interested in, was the distinction between facts and emotions, or values.

Lenzen: You have to make a sharp distinction between a fact and a judgment of value. That's the basis of the whole system. This was a distinction that is not always recognized.

Henderson also lectured on the history of science. Then there was Dr. E.E. Southard--a biography has been written of him by Frederick Gay, entitled The Open Mind. He died at the age of 44, quite suddenly. I was interested to learn in looking up that biography that just before he died he had arranged to be the editor of the Peirce manuscripts.

Daniel: I gather that this seminar was important because of the minds of the people who were participating.

Lenzen: These were outstanding people, but of course they were attracted to Royce because he was an outstanding person. Only an outstanding man could get other outstanding people to come.

Daniel: Do you remember Southard and what he contributed?

Lenzen: He contributed a paper on the logic of pathology, which I wrote up in considerable detail in my notes. He was studying logic in order to make psychiatry and pathology scientific.

Another member of the seminar was Frederick A. Woods,

Lenzen: a professor of biology at M.I.T., who wrote a number of books: Mental and Moral Heredity in Royalty was one of them. He studied the hereditary characteristics in certain royal families of Europe.

Then there was a Professor George Howard Parker of zoology, and Professor Herbert Vincent Neal, professor of zoology in Tufts College. For one year Y.R. Chao was a member. Norbert Wiener was a member after he came back from Europe. Tenney L. Davis, who was a graduate student in chemistry at M.I.T. and who later became a professor of chemistry and became a special student of Chinese alchemy. (Tenney Davis never published his history of chemistry--a case of procrastination, so many people procrastinate.) There were two girls from Radcliffe College, their names are mentioned in my notes, but they didn't have anything to contribute.

One member of the seminar was Professor Raphael Demos. He was a Greek who came from Constantinople in the same year that I did, got his Ph.D. He was a very good debater. Subsequently he became professor of philosophy at Harvard. His most important work is a book on the philosophy of

Lenzen: Plato. He was a member of the committee that drew up that Harvard Report on General Education of some ten or more years ago, written maybe some fifteen years ago; a committee studied general education in a free society, and Demos was a member of this committee.

I should mention Professor E.V. Huntington, one of the pioneers in this country among mathematicians, who worked on the foundations of mathematics, beginning around 1900. He published papers on postulates for algebra, and possibly his most notable work in that field was entitled The Continuum and Other Types of Serial Order. When I was an undergraduate student at the University of California Professor C.I. Lewis called my attention to this monograph and I studied it. During my graduate years at Harvard I took Professor Huntington's course, Mathematics 27, on the foundations of mathematics, entitled "Fundamental Concepts of Algebra and Geometry." I have a complete set of notes on that course still in my office.

Travel and Decisions

Daniel: What broad effects did Harvard have on you?

Lenzen: At Harvard I met other graduate students because we dined together and lived in the same dormitory. There was a student who was aiming to be a theoretical physicist. I decided that these other graduate students who were studying physics were no smarter than I was, and why couldn't I have done the same thing? I decided that I had made a mistake.

The result of my first year at Harvard disturbed me very much. I was really interested in physics and felt that if they were qualified to be students I was also qualified. Number two, I had felt that the philosophical way of treating reality was not what I was interested in. Then I had decided that you can't do philosophy of science and write a paper on causality without having a thorough knowledge of science. Then, by the middle of the second semester I was getting kind of tired because I hadn't taken any rest.

So I became very disturbed and wanted to abandon

Lenzen: philosophy. I wrote Professor Lewis, and he said, well, I'd just been working too hard; and I wrote home and of course my family looked upon me as the problem child. They were as tolerant and sympathetic as possible but what could they do? I was such an erratic person. The advice that I got was that I should at least finish up and get my degree. So that's what I did. During the summer of 1914 I did, as I mentioned, the foolish thing of staying at Cambridge, living in the dormitory, studying Husserl, instead of coming home to San Francisco.

I ought to go back a little. When I got the scholarship of the Harvard Club of San Francisco to go to Harvard it was for only $150, which was then the tuition. But my maternal grandmother persuaded her husband, my stepgrandfather, to give me $500, which I carried in gold in a little bag on my way to Cambridge until I deposited the money in the bank. That gave me a basis for the first year, in addition to the scholarship. But for the second and third year I got a scholarship from Harvard. In the third year I had their best scholarship, James Walker Fellow. It paid $525 a year which was very good in those

Lenzen: days. Yet I was very unhappy about everything, very uncertain.

I might mention this: in December of 1914 I went to Milford, Pennsylvania, on a mission for the department. Charles S. Peirce died in April 1914 and his widow wished to give his manuscripts and library to the university, although she raised certain difficulties later. I was selected by Professor Woods and by Professor Royce--who was the one who was mainly interested--to go to Milford during Christmas vacation to pack up these manuscripts and books and ship them to Harvard University. I did this, and this particular incident is the basis of what I'm working on now. In 1931 Harvard University began to publish these collected papers in eight volumes. The last two, volumes seven and eight, were published a number of years ago. Harvard invited a Professor Max Fisch of the University of Illinois, who has especially studied Peirce and American philosophy, to write the definitive biography. Professor Fisch, having discovered that I had gone to Milford and therefore knew Mrs. Peirce, wrote to me to get my impressions of her. This started a correspondence. A year ago, last summer, I made a

Lenzen: trip East and went to Milford again and I discovered some interesting things.

Last summer I went East again and spent a week with Professor Fisch, who was at Harvard working on the papers. I'd been looking up and doing some study of Peirce's scientific work, and I read a paper on October the fourth (1960) to Dr. Herbert Evans's History of Science Dinner Club about Peirce.

Daniel: What did you know about Peirce in 1914 when you went to Milford?

Lenzen: Professor Fisch, the biographer of Peirce, has raised that question. I told him that Peirce didn't have very much impact on the students. There were three reasons for that: one was the relative inaccessibility of the Peirce writings; secondly the fact that students of philosophy didn't know much about the history of science; and thirdly, the dominance of Russell. I was interested in Russell, but Peirce had a theory of signs. Costello talked about him in that course in logic and Royce talked about him a great deal—his theory of interpretation.

When Professor Fisch wrote to me about a year ago and said he'd like to know more about Royce's seminar, I had

Lenzen: certain portions of my notes microfilmed--the notes in which Peirce was mentioned. Southard, in his talk on the logic of pathology, began by giving an account of Peirce's ideas of scientific method.

In the beginning of the spring semester of the year 1915-16 Professor James H. Woods, chairman of the philosophy department, came to me and said they wanted me to take a Sheldon Fellowship to go to Europe. I didn't want to do that. I said, no, I didn't want a fellowship, I didn't want to continue in philosophy, I wanted to go into mathematics and physics. I thought this finished me with the philosophy department, but Professor Woods came back and said, "Oh, you can take the fellowship and study mathematics and physics." I was glad to accept it, and I said, "All right."

My idea was to go to the University of Chicago. I figured that with this fellowship after a year I'd be able to give it up and go on with my own. But the dean of the graduate division, Charles Homer Haskins, a famous medieval historian, said that I ought to go to Europe. He said that I was too provincial. It was during the

Lenzen: First World War, and I didn't care about going to Europe, and I thought it would spoil my chances of making a start in this country. You see, it was really a sort of unprecedented thing to be considered a brilliant student in philosophy, getting a Ph.D., and then wanting to throw the whole thing to one side. Maybe I didn't have good judgment, but, they didn't care what I did. It didn't offend them at all. Of course, there's a tradition of extraordinary liberalism towards talent at Harvard.

I received my Ph.D. in June 1916. And so I sailed for England, and the first thing I did when I got to London was to look up Russell.

Daniel: How long had he been at Harvard?

Lenzen: Only the spring semester of 1914. Then he went back to England. When the First World War broke out Bertrand Russell opposed Great Britain's entrance into the war, and he wrote a book called Justice in Wartime consisting of letters he wrote to the Times, I think. So the British government wouldn't allow him to go to certain restricted areas where he might talk to workmen and give pacifistic speeches. He was living in London at the house of his brother who was then the Earl Russell, 57 Gordon Square,

Lenzen: which is not very far from Russell Square, which is right alongside the British Museum.

I spent one evening with Russell. Then I went to Cambridge.

Daniel: You told him about your change of interest?

Lenzen: I don't remember just what I told him. But Russell gave me T.S. Eliot's address, so I spent an afternoon at tea with T.S. Eliot.

Then I went to Cambridge but Russell conducted a class in mathematical logic once a week for four of us. Myself and a woman named Dorothy Wrinch who is quite well known as a mathematician. Then there was a Frenchman by the name of Jean Nicod who wrote a book on induction. He died at a very early age, he was a very brilliant fellow. Then there was an Englishman by the name of Armstrong who had been injured in the war, and I don't really know what happened to him. So I used to go from Cambridge to London once a week to be with Russell and his group studying mathematical logic. This was informal.

During the Christmas vacation Russell and our group were invited to the manor house of a pacifist member of

Lenzen: Parliament, Philip Morrel. He was opposed to the war and in with Russell. This was near Garsington which was near Oxford. During Christmas vacation I was there for about a week. As one of my friends, a professor of English, indicated to me, this was a rather rare privilege, to get into the home, the manor house, country estate. Although he was a pacifist member of Parliament, he was distinguished, and his wife belonged to the nobility.

Daniel: Did this impress you?

Lenzen: No, not particularly. I remember taking walks in the gardens, in the country, with Russell and the others. I knew Russell very well at that time.

In March I crossed the Channel to France and was in Paris until August. I was very unhappy and didn't do much in Paris. I attended some lectures.

Daniel: You knew French well enough to get by?

Lenzen: Yes. Conditions were rather unsatisfactory. In August of 1917 I came back. I traveled steerage on the steamship Rochambeau of the French Line sailing from Bordeaux. It was pretty awful.

Then I got to New York, and Cambridge. Well, there wasn't anything for me to do except be an assistant in

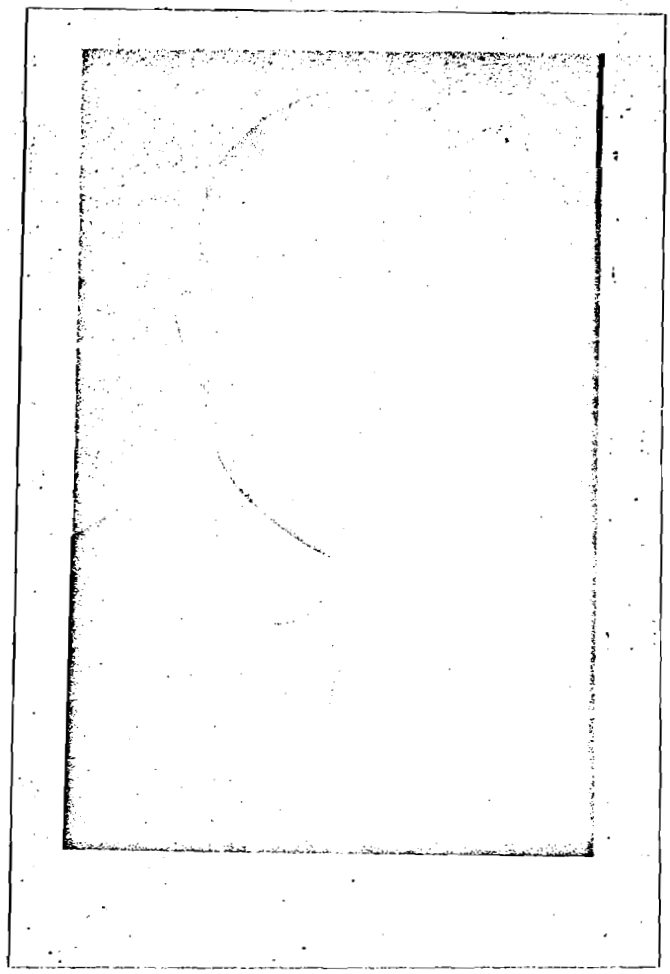
Lenzen: philosophy for that year. I was paid $550. That was my stipend. I was an assistant.

Royce had died in September 1916 while I was in Europe. So I was in a sort of unhappy state. I came back to San Francisco in June 1918.

PHYSICS DEPARTMENT, BERKELEY, 1918-1927

Lenzen: I came back to San Francisco. I was at draft age, but when I got back to Cambridge there was some hitch in my draft registration, and the matter wasn't settled until I got back to San Francisco in June of 1918. My family wanted me to try for the navy, so I tried to enlist in the navy, and was rejected as underweight. Then I was called up by the draft board. In the meantime I got sick with bronchitis, and I was given special examinations, and they didn't think that I was a good risk, so I was not taken for military service. I was not feeling very well then.

But I decided I needed to do something, thought maybe I could get a job teaching mathematics and physics in high school, so I came over to Berkeley to the physics department to get recommendations. They remembered me; I had been an outstanding student, and they offered me a job as assistant in physics. That was exactly what I wanted. I happened to go first to the physics department.



Victor F. Lenzen, 1926

Lenzen: If I had gone first to the mathematics department I might have gotten a job there.

Daniel: Do you regret that in any way?

Lenzen: No. I was interested in both, and this matter of mathematics I'll bring in again later.

I had a double assistantship, they were so short of help. The assistants were paid $500 a year, so I got $1000 a year. Since the previous year at Harvard I had gotten $550 as an assistant, this was a very great advance in economic status.

I was very happy that year. I assisted in the sophomore laboratory, conducted recitation sessions, and I felt that I was very successful and very happy. In many respects that year was one of the happiest in my life.

Shortly after I had accepted this job as assistant, I received a letter from Professor Hoernlé, who was then chairman of the philosophy department at Harvard, telling me that Professor Sheffer, who taught logic, was probably going to Washington on war work, and asking me to come back as an instructor. I still have that letter. He suggested that I start in to prepare for this course in logic, and

Lenzen: one other course which I was supposed to give, but I wrote back and said I had accepted this assistantship and decided not to go back, because this was just what I wanted. I think some of the professors who knew me thought it was very strange that after being a successful student of philosophy that I should just turn my back on the whole subject and take up something else.

I think that I was really interested in the nature of the physical world and its mathematical representation, rather than philosophical considerations of it which I thought were too general. I was interested in the details of the physical world. Also, I felt that I was much more successful as an assistant in physics than I had been as an assistant to Henry Sheffer in philosophy the year before. So, although I had been offered a chance to go back and take his place, I turned it down flatly.

Then at the end of the year as assistant in physics I decided definitely to abandon philosophy as a field, and by this time the stipend of an assistant--the title was changed to teaching assistant--became $750, so then I stepped down to $750 as just normal teaching assistant. In order to supplement what I had previously learned about

Lenzen: physics I registered as a graduate student in several junior courses which I had missed previously. In effect I went almost to the bottom of the ladder; not completely because I knew enough to teach lower division.

I also took graduate courses in mathematics, with Professor M.W. Haskell and Professor John Hector McDonald and Professor T.M. Putnam. During the second year as a graduate student--I guess it must have been in the spring of 1921--Professor Haskell asked me to become an instructor in mathematics. He was chairman of the department and a very learned mathematician who had taken his Ph.D. in Göttingen under Felix Klein, the world's number one mathematician around 1890.

Daniel: You seem never to have been without job and scholarship offers.

Lenzen: My professors apparently thought I had possibilities and they were very kind. I was interested in applied mathematics, or mathematical physics. I wasn't interested in experimentation, and this was a field that was almost neglected in this country. You could name the people in it in this country on the fingers of your hands. Mathematicians weren't interested. They were interested in

Lenzen: pure mathematics.

Daniel: It wasn't so much that they weren't able to cope with this as that they didn't want to?

Lenzen: There's a field in physics in which people develop mainly mathematical theory, and this was hardly recognized in this country, and in fact it hadn't been recognized for very long even in Europe.

Daniel: Where were the most outstanding mathematicians in this country?

Lenzen: The outstanding mathematicians were at the University of Chicago. Prior to 1900, the well-trained American mathematicians got their training in Germany at Göttingen--William Fogg Osgood at Harvard, Haskell at California, C.A. Noble in California, and so on. The University of Chicago was started around 1895 or 1896, and rapidly it built up a first-class department of mathematics. The first Americans who were properly trained in mathematics in this country came out of Chicago, like John Hector McDonald, here, and Professor Putnam, and Tom Buck--he's retired, he's still here. They had a very excellent man at Chicago by the name of E.H. Moore, but they also had a

Lenzen: number of--one or two, I don't know just how many--German mathematicians, and one of them was Oskar Bolza, an expert in the calculus of variations.

John Hector McDonald was professor of mathematics here for many years and in my opinion the most brilliant one, although his standards were so high that he didn't publish as much as he should have, and then there were other difficulties. McDonald used to talk to me quite a lot about Bolza, and I think McDonald--I don't remember now whether it was the year after retirement or whether he took a sabbatical--but he went to Europe and visited Bolza in Freiburg, and they discussed certain problems. This is mentioned in the Memorial Resolution for John Hector McDonald, which I wrote under the tutelage of Mrs. McDonald.

Harvard, however, came up fairly quickly, but the University of Chicago was the best place at that particular time.

Professor Haskell wanted some mathematician who was interested in physics in the mathematics department, and that's why he wanted me. I remember that a friend of mine said that Haskell told him that he'd tried to get a

Lenzen: mathematical physicist on a number of occasions; he had salted their tails but they had gotten away.

Daniel: Professor Haskell was criticized by some people for not developing the department of mathematics at the University in this direction.

Lenzen: He tried. He offered me this job, and of course I was so modest, I suppose, that I supposed at first that he was asking me to be a teaching assistant, but, no, it was an instructorship. So, I went and told the people in the physics department--Professor E.E. Hall--that I had this offer, so then the physics department said they would like to have me stay with them as an instructor. So, you see, I got that offer.

Daniel: Meanwhile, you continued to expand your knowledge in physics.

Lenzen: Yes. I was a graduate student in physics and my aspirations were to be a theoretical physicist or a mathematical physicist; there's a slight difference, I must say, between a theoretical physicist and a mathematical physicist, although the distinction may not always be made.

Daniel: Who were some of the people who advanced your studies?

Lenzen: I think special mention should be made of Professor W.H. Williams. I'll have to tell you about him. Frederick Slate retired as professor of physics and chairman of the department, or head of the department as he was called in that day, in 1918, and Professor E. Percival Lewis became head of the department.

Lewis was really the only person doing research in what could be considered modern physics. He got his Ph.D. at Johns Hopkins under Henry A. Rowland and spent a year or two in Germany, in Berlin. He knew about electrical discharges through gases, which was in the field of modern physics, because x-rays and radioactivity came out of studies of such phenomena. He was better informed and more active in the modern fields than anyone else in the department.

Of course, the state was not as liberal in appropriations at that time as it is now, and the department, the teaching schedules were heavy, and well, throughout the United States the subject of physics didn't receive very much attention. There was some research at Harvard and Chicago and possibly Wisconsin and Johns Hopkins, and E.P. Lewis tried to do a little at the University of

Lenzen: California, but we were so far behind Europe that there was no comparison.

Frederick Slate was an expert in classical mechanics; he was a very good scholar in that field, but the textbooks that he wrote were so difficult to understand that you had to know the subject in order to understand the textbooks. He had a terrible reputation among the students; he gave Physics 5, and engineering students were required to take it in their junior year. It was considered the greatest ordeal an engineering student had to go through. I understand that the final examination was in two parts: a four-hour period on theory and a four-hour period in solving problems. A lot of students flunked. The course had a terrific reputation.

Around 1913, the colleges of engineering started their own course, and the physics department lost those engineering students. This engineering course subsequently became Mechanical Engineering 102A-B, then it became 35, and 102B; the statics part (35) was put in the sophomore year and the dynamic part (102B) then came in the junior year. I don't know just what the number is now. This Physics 5 subsequently was changed to 105A-B, and then,

Lenzen: when I started giving the graduate side of this work, the graduate course became 205A-B.

Professor Slate was a very good scholar in classical physics, but he was an opponent of the theory of relativity, which had been accepted pretty well, and I don't know what his attitude was toward the quantum theory--I guess they didn't pay much attention to it here.

After Slate retired, the department needed somebody to teach theoretical physics, and they then brought in William H. Williams. He taught vector analysis. He did very well, and he taught the dynamics course one semester, and electron theory, and subsequently thermodynamics, and so forth. Well, he did more than that. He was able to read and understand the new theoretical physics that was just coming out, and so he would lecture to the department on that. He was the only person competent to read those papers.

You see, in 1913, Bohr had set forth the Bohr theory of the atom, and in 1915, during the war, A.J.W. Sommerfeld had published an extension of the Bohr theory, and greatly refined it. For a while it was called the Bohr-Sommerfeld theory, and the Germans were accused of trying to steal

Lenzen: some of Bohr's priority. But I heard Professor Birge say that he thought the term Bohr-Sommerfeld theory was correct because of the great extension that Sommerfeld had applied with the theory of relativity.

Daniel: Was the chemistry department in on any of this thinking about the structure of atoms?

Lenzen: G.N. Lewis had his own theory.

In 1918 two papers appeared: a big monograph in the Danish Academy Proceedings by Bohr, on the foundations of the quantum theory, in which Bohr set forth his correspondence principle, which subsequently was influential in guiding the remainder of the movement; and a student of Bohr, H.A. Kramers, who had gotten his Ph.D. at Leyden, and who became Bohr's assistant in Copenhagen, applied Bohr's extension of the quantum theory--beyond Sommerfeld's, in turn--to the intensities of spectral lines, and had worked out the quantitative side of it. Owing to the war, people in this country didn't hear about such developments; Kramers subsequently became professor of theoretical physics at Leyden. He died a couple of years ago, after surgery, but he's one of the important people in the

Lenzen: history of the subject during the 20th century.

About this time, there were two rival theories of the atom: there was the static atom--the view that the atom consisted of a positive nucleus with electrons at stationary positions around it--and there was the dynamical atom, in which the electrons moved in orbits around the nucleus. The Bohr theory was the dynamical atom, and involved the quantum theory.

But in 1916, or maybe earlier, G.N. Lewis set forth the theory of the cubical atom, and this was developed so as to meet the requirements of chemistry, the periodic table. This cubical atom of G. N. Lewis had the electrons arranged at the corners of a cube, and successive cubes, you see. It think it was 1916 that G.N. Lewis published this, and almost simultaneously it was published in Germany by Kossel. But the Lewis cubical atom was especially developed by Irving Langmuir of the General Electric Company. He made a very extensive development of this cubical atom, and so around 1920, about the time when we were beginning to learn about the new developments of the Bohr theory by Sommerfeld, and the further work of Bohr and Kramers, there was this clash between the static atom

Lenzen: and the dynamical atom. G.N. Lewis, who was the most powerful scientific man in the University, was for the static atom, and Birge stood up for the dynamic atom, which was based upon spectroscopy. Of course the chemists didn't know anything about spectroscopy. The Lewis atom was based upon the requirements that explained the valence properties of atoms as manifested in chemical combinations.

G.N. Lewis was a very dominating figure in chemistry in the United States, a very outstanding person. Langmuir developed that G.N. Lewis atom while he was with the General Electric Company, so therefore, on both the Atlantic Coast and the Pacific Coast you had sponsors of the cubical atom.

Daniel: You have introduced Professor Birge. How had his ideas developed?

Lenzen: Professor Birge finished his work at the University of Wisconsin in 1913, I believe. Then he went to Syracuse where he was for five years as an assistant professor. But the prospects were not good at Syracuse. Birge was a spectroscopist; he knew especially the theoretical side ot it, and he was trying to correlate, to systematize,

Lenzen: the material, and he very quickly took an interest in and accepted the Bohr theory of the atom. So since E.P. Lewis was interested in spectroscopy, and after he became chairman of the department, Birge was brought here in 1918.

He stepped down from assistant professor to instructor —you couldn't imagine a thing like that happening at the present day. He brought the Bohr atom to the University of California, and during the period from 1919 to 1920, when I was attending Professor Lewis's lectures on Physics 111, he expressed a certain scepticism about this course. When Professor Birge came as an instructor, the physics department began having seminars in which the new ideas were discussed and he was very active.

Daniel: How did the physics department cope with the atom of G.N. Lewis?

Lenzen: The physics department didn't pay much attention to the static atom, but because of Professor Birge's sponsorship of the Bohr atom, the dynamical atom, you see, this was a topic of discussion. Spectroscopy was a field in physics, and E.P. Lewis was a spectroscopist who gave a laboratory course in spectroscopy and had students working for him in

Lenzen: spectroscopy and research was done in spectroscopy--such as there was--and Birge was trying to find formulae to arrange spectral lines in series. In the physics department the whole interest was in spectroscopy, and therefore in the Bohr atom.

Daniel: The physics department wasn't dampened by G.N. Lewis's ideas?

Lenzen: There wasn't any real communication; there were two rival schools. The fact that G.N. Lewis believed in the static atom, cubical atom, didn't really affect the activities in the physics department.

Then when Slate retired in 1919 there wasn't anybody qualified to teach theoretical physics on a really high level, and Williams was able to do that. He's never been given adequate credit for it, but I wrote the Memorial Resolution for him and I put those things in that Memorial Resolution. The rest of the department was like a bunch of schoolboys--well, maybe not as extreme as that.

As Mrs. Williams explained it to me, Williams was born in a suburb of Buffalo, New York, and his father was a professor in some local academy, but he died while young Williams was a child, and so the mother had a hard

Lenzen: time. The story is that Williams was very good in mathematics, and there was a high school teacher who encouraged him to go on, and he took the examinations for West Point with a view to getting a free education. There are different versions of this story. The version that I first heard was that his mother insisted that he go to West Point, but Mrs. Williams said that that wasn't the case, that he took the examinations on his own initiative and passed. There is a conflict of testimony there; I've discussed it with Professor Leonard Loeb and he has a different view of it from what I got from Mrs. Williams.

In any event, he graduated from West Point. (The West Point graduates have a periodical called The Assembly and in it appear obituaries with pictures of the various deceased members. Mrs. Williams gave me the copy of The Assembly with Williams' obituary in it, and I gave it to Miss Dornin for the archives.) Williams was recognized as a brilliant student at West Point, but he was rather independent and he once told me that every student at West Point had a certain peculiarity for which he was famous, and Williams told me that his particular trait

Lenzen: was that he was never to be caught studying. He graduated from West Point in 1902 and was assigned to the Philippines.

I think it was in 1906 that he resigned from the army and for a time he had a job as an accountant in Sacramento, and then he went into high school teaching. He was principal, I think, of a high school in Nevada City. Then he came to Oakland and was head of the science department of, I think it was, Fremont High School.

At that time he began coming out to the University to attend seminars by G.N. Lewis. He was interested in chemistry. G.N. Lewis was one of the first persons in this country to take up the theory of relativity, and he and Richard Tolman wrote a paper on relativity which was an important contribution to the field. Williams got interested in relativity through his association with G.N. Lewis, and began taking some work with Slate in mathematical physics, and he did very well. When the United States entered the First World War in 1917, Williams went into the army and was in charge of an artillery training school. He came out with the rank of colonel.

It was at this point that the department needed somebody to teach theoretical physics, and there were

Lenzen: hardly any theoretical physicists in the country. Now there are dozens of them. So Williams was brought here in 1919 to teach theoretical physics, and I remember I took his first course in vector analysis. He taught very well; he was a very good scholar and a profound thinker on matters of physics. He was forty years of age when he got this appointment, and my observation is that if a man is going to do any writing, he's got to start at an early age and acquire the habit. It's hard for him to get started, but once he gets started--the first job may be hard, the next job will be easier, and finally he gets into the habit and can write things out at a pretty good rate. Williams got into the subject rather late and he was highly self-critical, and also he tried to solve problems that were practically insoluble. There's such a thing as being too ambitious; if one is too ambitious one may not do anything at all.

When Williams came into the department, we were just beginning to get the literature from Denmark and from Germany about the new atomic theory. Sommerfeld's Atomic Structure and Spectral Lines [Atombau und Die Spektrallinien] was the Bible of the subject for at least

Lenzen: five years. I recall that I got a copy of it from the Sather Gate Book Shop through Mr Sommer. I must have given it to Professor E.P. Lewis. I have the second, third, and fourth editions but I haven't got the first. The editions followed rather rapidly and when Sommerfeld was here in February 1923 he autographed my third edition.

In any case, these were the papers which Professor Williams studied and presented in departmental talks.

Daniel: Did you have to have Professor Williams' interpretation or could you take these things apart yourself and make sense of them?

Lenzen: I learned from Williams. However, I don't remember now just when it was that I gave a talk to the physics department--we had departmental meetings every week--on the quantum theory. I read the literature, looked up and read papers that Professor Williams had read, and presented the department with a sort of perspective of the subject, and it impressed them tremendously. I think this was one of the reasons why they asked me to be an instructor. I was developing a little behind him--he had a little head start on me. Well, as I said, in the spring of 1921 Professor

Lenzen: Haskell asked me to be instructor in mathematics, and the physics department decided that they wanted me to remain in the department as instructor, so after being an assistant from 1918 to 1921, I became an instructor.

After I became an instructor, I decided just what field I should concentrate on: statistical mechanics. And in the summer of 1922 I worked on a manuscript which had nothing original, but it put together various things in the field. It was about four or five hundred pages long. This was the first draft of a treatise. I gave a lecture to the department on the subject in the fall of 1922, and then shortly after that I was asked by Professor G.P. Adams of the philosophy department to contribute a paper on relativity to the Philosophical Union. The Philosophical Union had been started by Howison many years before, and had been quiescent, but they were going to revive it, and they were going to have a series of lectures: "Issues and Tendencies in Contemporary Philosophy." A.O. Lovejoy of Johns Hopkins was in residence and gave the first paper, entitled "The Anomaly of Knowledge." He suggested that they have a paper on

Lenzen: the philosophical aspects of the theory of relativity so Professor Adams came to me and asked me to do it. I was reluctant, but I went to Professor E.P. Lewis and asked his opinion. He advised me to do it; he said it would help to create good will between departments. So, with some misgivings, I wrote the paper and read it to the Philosophical Union.

That paper represented primarily my own views and my own analysis. I read it in Room 11 Wheeler before a large audience, and it was a tremendous success. This was at the height of interest in Einstein and relativity. It was an enormous success, and I have this to say, that if you do something for which there is a demand and for which you get praise and recognition, it's pretty hard to drop it. I had intended to cut myself off from philosophy entirely, but I was drawn into this matter, and it was a great success, and after that I began to give the Philosophical Union a paper every year on some aspect of the philosophy of science. They were very successful.

Daniel: Although you were drawn into this thing, and you did

Daniel: it as an assignment, what was there about it that did interest you?

Lenzen: I would never have taken an interest in philosophy if I hadn't been interested in fundamental questions, and when I began thinking about relativity it raised all sorts of fundamental questions in my mind, which of course I felt impelled to go on and think about. If I hadn't been invited to write and read this paper, though, I doubt very much if I would have gone into it. I doubt it very much.

I published my first paper in the Monist, and it occurs to me now that in the student period at California, when I became interested in philosophy, I came upon the writings of Paul Carus, who was the editor of the Monist, and I was influenced by him. After his death the family established a lectureship--the Carus Lectures--and one of the first lecturers was Arthur O. Lovejoy.

Daniel: What did Professor E.P. Lewis teach?

Lenzen: He gave a course on electric discharges through gases, in which the new ideas were set forth, but one must remember that at that time there was no generally accepted theory of the atom, and the quantum theory was just barely

Lenzen: known, and relativity had not been generally accepted, so that the course was mainly on the phenomena in vacuum tubes, x-rays. Of course, the electron had been discovered by then, and its properties were pretty well established--particle or corpuscular properties.

Spectroscopy seemed to be the most active part of physics, but it was in a rather chaotic state because of the big mass of data--photographs of spectral lines and measurements of spectral lines, and there were relatively few correlations between them.

When Professor Lewis was in Berlin doing post-doctoral work he discovered a phenomenon in nitrogen which he called the after-glow in nitrogen. Subsequently Strutt, who was the son of the great Lord Rayleigh and subsequently became Lord Rayleigh himself--I want to make a distinction between the great Lord Rayleigh and his son, who was a competent physicist but not great--Strutt called it active nitrogen, and Professor Lewis was very unhappy because it appeared to him that Strutt was trying to obtain the credit for this discovery which Lewis had made in Berlin.* On the occasion of the Exposition which was held on Treasure Island in 1939 there was

*Robert John Strutt, son of the great Lord Rayleigh, the 3rd Baron Rayleigh, himself became Lord Rayleigh, the 4th Baron Rayleigh. VFL.

Lenzen: set up an experiment demonstrating this active glow in nitrogen. This was done by Professor Francis Jenkins. This was possibly Professor Lewis's most important contribution, so far as acceptance from the outside is concerned, and he guided certain students.

He was a very idealistic man and a very unselfish man, very devoted to the welfare of the department. When he became chairman he set about trying to build it up. There had been practically no promotions in the department during the preceding ten years, and at the end of the first year Ralph Minor was made a full professor, and E.E. Hall was made a full professor; they had been associate professors for quite a long time. Then Lewis brought Birge from Syracuse.

Professor William Raymond was related to Professor Birge in some way. Professor Birge explained in his report that Professor Raymond met him at the station. Raymond was a graduate of this University about 1888, I think in electrical engineering, and he taught a course, Physics 7--I think later it was called 107AB-- which was electrical measurements, and had a very good laboratory.

Lenzen: The laboratory assistant was usually an outstanding senior student who was majoring in electrical engineering. A very good course, the first semester having one experiment a week and the second semester having two experiments a week, as I recall. These experiments took a great deal of time to write up. I know because I took the course myself. Professor Raymond also taught mechanics. Professor Slate had been especially interested in analytic mechanics, and this course had been taught to engineers—was required of engineers, and they feared it very much because many of them flunked.

Raymond also taught this course. He was a tall, bearded, solemn-faced, dignified gentleman; I suppose that was supposed to be the proper attitude for professors at that time. I have heard comments about J. Willard Gibbs at Yale as being rather reserved and dignified and aloof and unemotional, but I've seen a statement by one of Gibbs' students, E.B. Wilson, to the effect that that was just a pattern of the professor of the old school, and E.B. Wilson defended Gibbs. He just behaved as a professor was expected to behave in those days.

Daniel: And Raymond was in this tradition?

Lenzen: Yes. There was some experimental work done by him, I think on the acceleration of gravity but I don't recall. He had a great interest in conchology--that's the study of shells. He was a special student of shells and wrote papers on the subject. The reason I happen to know about this is that after the death of Professor Raymond I was chairman of the committee to write the Memorial Resolution, so I found this out. But he was a very conscientious, industrious person. In those days the teaching schedules were very heavy and the department had only small facilities.

When E.P. Lewis became chairman, the department was in South Hall, and there wasn't very much space. One of the first things that Professor Lewis did after he became chairman was to undertake a campaign to get a new building, so LeConte Hall was obtained, and I remember at the dedicatory exercises in 1923 that Professor Lewis especially thanked Sproul, who was then the University representative who went to Sacramento. He gave credit to Sproul for having persuaded the legislature to vote the money--it was $500,000. Acquiring a new building of course gave a great boost and provided for a research base.

About this time, there were attempts to get certain

Lenzen: distinguished people, Arthur Compton, for example, but without success; he went to the University of Chicago.

Professor Birge was youthful, enthusiastic, and I suppose one could say aggressive. In 1921 he had published an article entitled "The Quantum Theory of Line Spectra and the Balmer Series of Hydrogen," in which he had compared experimental results with the Bohr theory as it was then extended by the work of Sommerfeld and Bohr and Kramers. Sommerfeld, in his third edition, made use of Professor Birge's results, and referred to his work as being very careful. When we got this third edition, with Professor Birge's citation, Professor Lewis was very pleased because it meant recognition. We were social climbers, or well, scientific climbers, I suppose you'd say. Professor Lewis was terribly pleased. He felt that the department was on its way up.

In 1923 Professor Lewis brought Professor Loeb, and he greatly added to the enthusiasm of the department.

Then there was John J. Hopfield, who was a student at Syracuse and came to California as a teaching assistant with Birge, and Birge's idea was that Hopfield had great

Lenzen: talent for experimental work. He was a very hard worker, he'd work all night, and he made a very favorable impression upon Professor Lewis. He got his degree under Professor Lewis. Somehow or other, I have the impression that it was stated that Hopfield got his degree under Birge—that isn't true. Professor Birge was not an experimentalist, although he did experimental work for his thesis. He was primarily interested in the applications of theory to experiments, and in computing.

Spectroscopy was a field requiring very extensive computing, and this was before the days of the mechanical computing machines. I can remember when Professor Birge was originally using one of these 20-inch slide rules for his work. Subsequently, the computing machine came in and Professor Birge was very expert with computing machines. Spectroscopy was a field in which a great deal of computation was involved, and Professor Birge was enormously industrious. I remember, I think it was beginning one semester, he had been working all summer on a particular paper. This was the Birge-Shea paper, I believe. Professor Lewis in introducing him spoke with

Lenzen: great admiration of his industry, remarking that he was working six to eight hours a day six days a week. Of course people don't do that any more. In those days, professors didn't have automobiles, and they couldn't go on week-end trips and do the various things that are done now. There weren't parties, you see, as they have now. Professor Birge was extraordinarily industrious, and he was also a very fast worker.

He soon started working on band spectra, and he made important contributions to that, and the interest was so great that when he got some new result in band spectra he would cable a letter to Nature--that was the first medium of publication. Now the Physical Review has letters. If you had to wait until something was published in normal channels it would take a long time.

The National Research Council had a committee on molecular spectra, and Professor E.C. Kemble of Harvard was chairman of the committee. Kemble was one of my fellow students when I was a graduate student at Harvard. He became professor of theoretical physics. And Professor Birge was on this committee, too, and I think their

Lenzen: report was published about 1927. This report was quite a thick volume, and I think Professor Birge did most of the work. He was really quite remarkable for his alertness and industry.

About Hopfield: Professor Birge's idea was that Hopfield would do experimental work and Birge would do the theory, and I think they collaborated to a certain extent. I think some bands are called the Birge-Hopfield bands. After he got his degree, Hopfield became an instructor in 1921, and continued his research. He was very industrious. He had a son. Hopfield I understand died some years ago, but a year ago we had a Dr. Hopfield speak to the department and there was some discussion about having him added to the department. I don't know how far along that is, but this is the son, who's a good physicist.

I think it was in 1922 I was wandering on the campus one Sunday morning near the Ellsworth Street entrance, and I saw a gentleman in the garb of an Indian of India. As I encountered him, he looked inquiringly in my direction and I responded. I learned that this was Professor C.V. Raman, a very famous physicist. In fact, he subse-

Lenzen: quently won the Nobel Prize. He hadn't discovered the Raman Effect at that time. He had just been elected fellow in the Royal Society. I suspect that he was probably on his way home after having been in London to participate in the ceremonies of having been made a member--I'm not sure of that. Well, I quickly found out that this was Professor C.V. Raman, and so I called Professor Lewis on the telephone. He was very much interested and told me to bring Professor Raman up to the house; Professor Lewis lived on 25 Panoramic Way. I took Professor Raman up there, and we had lunch, and Professor Lewis and Professor Raman got along very well. Raman knew about Lewis's work and of course Raman himself was a famous person, otherwise he wouldn't have been made a fellow in the Royal Society.

One incident that I especially remember about that luncheon--Mrs. Lewis had some tomato soup with some little specks of beef in it, and Professor Raman said he couldn't have that because it had meat in it. This indicates that he was a Hindu. Mrs. Lewis was a lady of great poise and self-possession, and she remarked that if she just hadn't put that meat in there everything would have been all

Lenzen: right. That was the only disturbing incident and it wasn't too serious.

One of the reasons I remember this is that after lunch the subject of Professor Birge came up and I remember that Mrs. Lewis said, "Oh, Dr. Birge, he's been making good for some time," indicating that Professor Lewis had said some very favorable things. The reason I'm bringing this in is that it is a further indication of Lewis's satisfaction that the department was progressing. He believed very greatly in scientific research and the advancement of science, and especially in the advancement of the department.

In 1922 we brought in Frederick S. Brackett from Johns Hopkins. He had extended the series of hydrogen and found a series called the Brackett Series. There was the Lyman Series, the Balmer Series, and the Paschen Series and the Brackett Series, depending on whether the spectral lines are emitted when the electron jumps to the innermost orbit, or the second, or the third or the fourth. The Lyman Series was discovered by Theodore Lyman of Harvard. According to the Bohr theory of the atom, this series was produced by electrons jumping into

Lenzen: the innermost orbit. In the Balmer Series it jumped to the second, and in the Paschen Series to the third. That was discovered during wartime, partly as a development of the Bohr theory of the atom. The Brackett Series is in the infrared where the final stage is the 4th Bohr orbit. Brackett was a very brilliant young fellow but he left the University. I think he went to Washington and I don't know very much about his activities.

The most active members of the department, along with Professor Lewis, were Professors Birge and Loeb. They, I would say, were mainly responsible in building up the department. They would take turns going to physical society meetings in Washington every year in April. In 1926 Samuel K. Allison came from Chicago. Loeb was responsible for that. Professor E.P. Lewis died in 1926 and the question arose as to who should be the next chairman of the department. By that time, the head of the department had acquired the title of chairman. They tried to get W.F.G. Swann, at Yale, but in a recent discussion, Professor Loeb said that Swann didn't understand that President Campbell had made him a

Lenzen: definite offer, and therefore it didn't come through.

I might say that after the arrival of Loeb, and after we got into LeConte Hall, we began having meetings with the Stanford physics department, in San Francisco. About this time David L. Webster was brought to Stanford to be head. He was an outstanding man in the x-ray field, in the theoretical field, and so under the leadership of Lewis and Birge and Loeb on our side about once a month we would meet with the Stanford group, headed by Webster; Stanford was trying to build up also. We met for dinner at a French restaurant in North Beach, La Favorite. You got a big meal for fifty cents in those days. Then there would be a discussion about physics. I don't remember how many years this went on, but it was quite an important affair.

Daniel: Do you think this was a one-way arrangement in which the Stanford people were stimulated by the University of California people?

Lenzen: Webster was a very good man. He stimulated the Berkeley people just as much as the Berkeley people stimulated him. This was a two-way arrangement, all right.

Lenzen: E.O. Lawrence came in 1928. I think it was in the spring of 1926 I was coming home with Professor Loeb on the Alcatraz car, which connected with the Key Route at Alcatraz Avenue. Professor Loeb spoke about Lawrence; he said that Lawrence had done an experiment with J.W. Beams, who later went to Virginia. This experiment was done at Yale and it was for measuring very short time intervals. Loeb said to me this was the best work that had been done recently. He had seen Lawrence at the physical society meeting and had said to Lawrence, "When E.P. Lewis retires in a few years would you like to come out to Berkeley?" Loeb told me that Lawrence said that he would be delighted.

There's been some discussion as to who was responsible for bringing Lawrence to Berkeley. My own view is that Loeb first discovered Lawrence, but Professor Birge claims that he was the one who finally persuaded him to come. Lewis died in 1926 and Lawrence didn't come until 1928. There were certain negotiations that were necessary. Professor Birge was, I know, positively instrumental. He visited Lawrence at Yale, and I remember that Lawrence

Lenzen: said that one of the things that persuaded him to come out here was the intense dedication that Professor Birge showed to science. He was most intent on his research. This impressed Lawrence very much.

Daniel: I think it would be helpful to have some comment about Professor Loeb. He is mentioned somewhat briefly by Professor Birge. Professor Loeb was the son of the distinguished--

Lenzen: Yes, Jacques Loeb was the most distinguished physiologist of his time in the world. When President Wheeler was trying to build up the University, Jacques Loeb was one of the people he brought out here. The son, Leonard, started his academic career at the University of California. His father went to the Rockefeller Institute around 1912 or 1913. When I was a freshman student there was a course on general science given at the University, in which various scientific men would give a certain number of lectures. Professor E.P. Lewis gave some lectures, and Professor Lawson, and there was (I remember hearing) a lecture by Professor Jacques Loeb.

When I spoke to Professor Loeb about this project

Lenzen: a number of weeks ago, he said that he had all the correspondence dealing with the bringing of Lawrence to this University. He had been consulted by a biographer of Lawrence, so if you're interested in this aspect of the history I think it would be possible to get copies of Professor Loeb's material. I do know that he was very enthusiastic and active in trying to build up the department. I think that Professor Loeb said to me that Leuschner was a very important factor in this matter of building up the department. I think he said to Leuschner, "Instead of giving us $10,000 for a famous professor, why not let me get two assistant professors who will build up and develop?" The upshot of the matter was that we got Robert Brode in 1927.

Daniel: Who had first noticed Professor Brode's particular abilities?

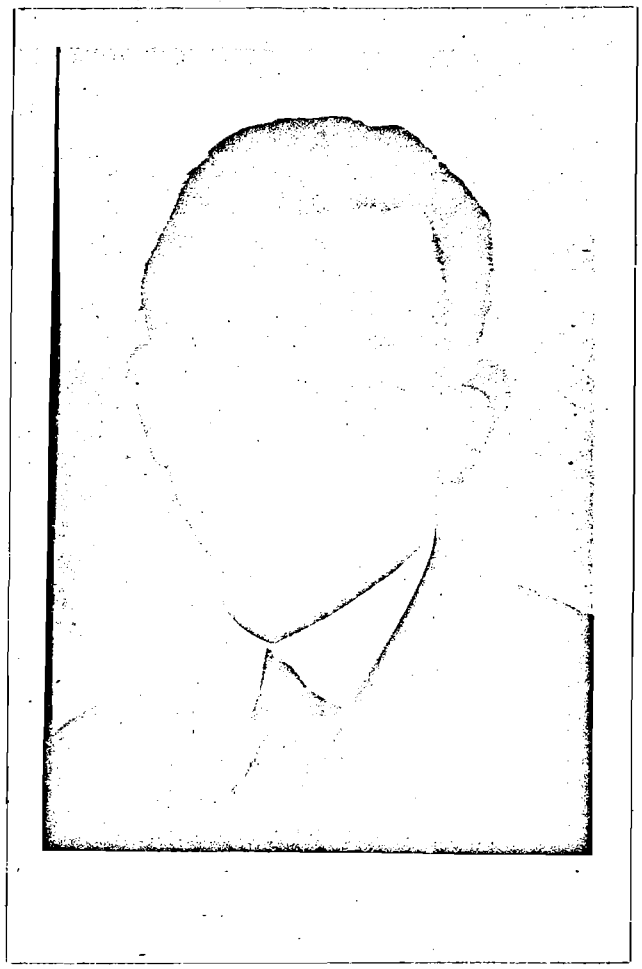
Lenzen: In 1927 Professor Williams went to Göttingen, which was then the center of theoretical physics because in 1925 Heisenberg, who was just the assistant to Max Born, had written the first paper on the new quantum mechanics. Then Born took it up. Having more mathematical background than Heisenberg, he was able to push it along

Lenzen: further. The next paper was by Born, Heisenberg, and Jordan. Göttingen became the center of theoretical physics at just about that time. Williams had a sabbatical for a year, and in Göttingen he met Brode and J. Robert Oppenheimer.

Oppenheimer was recognized as the young genius. He had gotten his degree at Harvard and then he went to Europe, and in a few weeks he wrote a Ph.D. thesis under Max Born and got his Ph.D. at Göttingen. Just about this time the Guggenheim Foundation was started, and in 1927 I received a Guggenheim Fellowship to go to Göttingen, but on my way to New York I stopped at Ann Arbor to visit with Professor Williams. The University of Michigan had started some special conferences on theoretical physics in the summertime. I don't think they are held any longer. Anyway, he was there, and he told me about his experiences in Göttingen, about the various individuals, and then he mentioned Brode and he mentioned Oppenheimer.

By this time Professor Hall had been made chairman of the department. I haven't said very much about him. He was a very scholarly and kindly gentleman, very much

Lenzen: devoted to the department and a very important factor in building it up. But in the early days, teaching schedules were heavy and there wasn't the money for research, and there weren't facilities and so forth. He was very objective and fair. I suppose the general public inclines to think that a university community should be a harmonious community of scholars who are always on fine, friendly terms with each other, but the fact of the matter is quite different, you're likely to have departments split up into factions, and if a chairman has to be appointed it is frequently very difficult to find somebody who will be looked upon as fair to all elements. Hall took the advice of Birge and Loeb, and I think, from what I've heard Professor Birge say, that they knew about Brode independently of Williams. I've heard Professor Birge say that Brode and Lawrence were considered the two most promising young men, around 1927 or so, and the department got both of them. Lawrence came in 1928, and I think Oppenheimer came in 1929, and he was considered the most gifted of the younger theoretical physicists in this country who knew about the quantum theory, and he attracted students. He trained some very good ones.



Victor F. Lenzen, 1927

EUROPE AND LOGICAL POSITIVISM

Guggenheim Fellow, 1927-1928

Daniel: As the department of physics was growing and developing, so apparently, were you. How did you happen to become a Guggenheim Fellow?

Lenzen: I think that it was suggested by members of the faculty that I apply. I think the Guggenheim Fellowships were started in 1926, that's my recollection, and I was a Guggenheim Fellow 1927-28. I spent most of my time in Göttingen, Germany, which was the center of theoretical physics of the world. The new quantum mechanics had been started there by Heisenberg and expanded with the help of Born and Jordan.

There were two institutes of experimental physics. The director of the first institute was R.W. Pohl, who was quite famous for his very excellent lectures and experimental demonstrations. He lectured at 7 o'clock in the morning, a most unsatisfactory hour from the

Lenzen: standpoint of Europeans, so he'd have the rest of the day for his work. He was such an excellent lecturer that he got students to come. He also wrote up his lectures in the form of books, which are very highly regarded. Pohl's own research was in crystal structure, and that was not really in the forefront of interest in physics at that time.

The second institute was headed by James Franck. Franck later came to this country and was in Chicago. We had him here for a visitor at one time. He was a very nice man, and made some of his experimental studies with Gustav Hertz in 1912, which were very important for the development of the quantum theory.

Among the people at Göttingen at that time was John von Neumann who later came to this country and was at the Institute for Advanced Study at Princeton and was a member of the Atomic Energy Commission. He did a great deal in developing the electronic computers, and also the theory of games in economic behavior--I think one might claim that he was the outstanding mathematician of his generation. He was first educated in Budapest, and then he was

Lenzen: in Berlin and then came to Göttingen. He wrote probably the most rigorous book on quantum mechanics of anybody, a book which is very difficult even for theoretical physicists to understand.

Pascual Jordan was a privat dozent. In Germany it took a long time for a man to become a professor. There weren't the series of gradations that we have. Recently I read an article about the present situation in universities in Germany. The older professors are opposed to any modification of the system, and the younger people are pressing for some hierarchical structure so that they can get on the ladder and go up, as in this country.

While I was in Göttingen I attended a big scientific meeting held in Germany in Bad Kissingen, which is a very nice resort. Another person who was in Göttingen with me was Professor Franklin of the department of mathematics at M.I.T. Mrs. Franklin--Constance Franklin--was the sister of Norbert Wiener, so I saw a great deal of the Franklins, since I had known Wiener when I was at Harvard for one year after he came back from England where he had been with Russell. By this time Wiener had already

Lenzen: obtained his position at M.I.T. and was winning recognition as an outstanding mathematician. Professor of Philosophy Moritz Geiger was very kind to us and had us to dinner at his house one evening.

The hospitality was quite wonderful. Every semester they had what they called a professorium, and this consisted of a dinner, a very magnificent dinner, and there would be a symphony orchestra that played during the dinner. After the dinner there was dancing until early in the morning. I was a guest at two of these affairs. I didn't realize it at the time but they did me great honor—I guess I was somewhat thoughtless: the gentlemen were all asked to take a certain lady into dinner, and the lady that I was asked to escort--and I still have her card--was the wife of Professor David Hilbert, who at that time was considered perhaps the world's leading mathematician. He was retired. From the European point of view it was really a very high honor to escort Frau Hilbert. He developed the field of mathematics which was very useful in the development of quantum mechanics, and this was one of the reasons why Göttingen was such

Lenzen: an important center.

There was a professor of mathematics by the name of Richard Courant, who is in New York now. Courant wrote a two-volume work on (in the English translation) Methods of Mathematical Physics. Courant wrote the book, but since it represented so much of Hilbert's work it's called Courant-Hilbert's.

On another occasion I went to Berlin, and there I looked up Hans Reichenbach, who had written some very good things on the philosophy of relativity. Later on he was at Los Angeles. Reichenbach, after the National Socialists came to power, left Germany because one grandfather was Jewish, I think. He had an appointment in Ankara or Istanbul. Professor C.W. Morris of the University of Chicago, who was active in the logical positivistic movement and the unity of science movement in the United States, wrote to me and said that Reichenbach was completing a five-year contract and would like to come to the United States. I happened to know at the time that there was need for a professor down at U.C.L.A. so I wrote to Morris and told him of this opening, and then I wrote

Lenzen: to President Sproul and recommended Reichenbach, and he was brought to Los Angeles and made a very outstanding contribution to the philosophy department at U.C.L.A. I think that my part in this was important. I recall that at the conclusion of an Academic Senate meeting President Sproul told me that Reichenbach was being appointed. He had a good record, and it was just a matter of Morris and Reichenbach knowing about possibilities.

In Berlin, I attended one lecture by Max Planck, but I made no attempt to meet him personally. I think he was retired at that time, but he was still lecturing. It was a lecture on mechanics on the undergraduate level. I heard one lecture by Einstein, but I made no attempt to meet him personally, either. Of course, Einstein was a fabulous figure at that time. Einstein had a very pleasing, quiet, soft manner, I would say, when he lectured, rather quietly and without any flamboyance. I think Einstein would arouse one's sympathy a little more than Planck would. There was more of a link between Einstein's audience and himself than between Planck's and himself. Planck was a very serious man, one of these very serious,

Lenzen: hard-working, industrious, humorless sorts, a careful, methodical lecturer--there's not much possibility of histrionics in lecturing on a subject like classical mechanics, of course. He had done a great thing in starting the quantum theory, you see.

On another occasion I went to Munich, where I visited Sommerfeld, and heard some lectures by him. He was most agreeable. What impressed me about Sommerfeld's lecture was that when he came into the lecture hall in which his class assembled, all the students stood up at attention as he walked in and took his place behind the lecture desk.

Daniel: This was not just a matter of etiquette?

Lenzen: I don't know. I think that custom prevailed certainly in some European universities, but I remember quite vividly seeing everybody stand up respectfully as Sommerfeld came in.

Logical Positivists: The Vienna Circle

Lenzen: I also went to Vienna and there I looked up Moritz Schlick, one of the key figures in the logical positivistic movement.

Lenzen: I suppose the fountainhead of it was Wittgenstein, who was in England.

Daniel: How were the logical positivists characterized, or were they characterized?

Lenzen: I don't think they were characterized in any particular way. I'll tell you later how this phrase "logical positivism" came into being, who invented it.

Schlick was a professor of philosophy at the University of Vienna. He occupied the chair that had been occupied by Ernst Mach, and after the death of Mach, as I understand, the mathematicians and scientific people insisted that the appointee to this chair be somebody who knew something about science. Schlick had got his Ph.D. in Berlin under Planck; then he went into philosophy. He was at Kiel first, and he wrote one of the first things that came out on relativity, Space, Time and Contemporary Physics, a very good thing, almost the first thing that came out. It was translated almost immediately. It's a well-known work in the bibliography, and very good. Then he wrote Allgemeine Erkenntnislehre; about 1918 he was called to Vienna, to the chair that had been occupied by Mach.

Lenzen: In Europe status or rank or title counts for a great deal, and there are relatively few professors. A university may have only one professor, ordinarily. He may have assistants, but the tradition has been for there to be one professor with subordinates. He would certainly have at least one or two assistants, and those were very desirable jobs for young men who wanted an academic career. Unlike California, where with a growing population and growing wealth once a person is appointed instructor he goes right up and may become a full professor regardless of how many professors there are, under the European system it's very difficult to break in. Usually a man has to wait until somebody retires or dies.

I mention this to indicate that although Schlick was not the most creative member of the logical positivistic group, he was a very important figure because his seminar in Vienna provided a meeting place. Rudolf Carnap was the most creative member, after Wittgenstein, of this logical positivistic group. But he was only an assistant or something of that sort. Later on he became professor of philosophy at the German university in Prague.

Daniel: What he had to say was automatically less important because he was lower on the academic scale?

Lenzen: No, what he had to say was important, of course, and was recognized as important, but Schlick was the focal point in Vienna because he was a professor.

Daniel: When you met these people, you had a plan of what you wanted to discuss with them, or did you just have an interest in meeting them?

Lenzen: I suppose it was primarily an interest in meeting them. Then I was also in Prague and visited Philipp Frank, professor of theoretical physics at the German university. He was looked upon as an active member of this group in Vienna. It was called the Wiener Kreis, the Vienna Circle, and its aim was to develop philosophy on a scientific foundation, and to investigate the foundations of mathematics and of physics. There was a mathematician by the name of Hans Hahn who was an active person in that group. I never met him.

Daniel: How did you regard these people and their ideas?

Lenzen: I thought of their work as really an extension of the work of Russell. I would say that Whitehead and Russell really

Lenzen: founded the application of symbolic or mathematical logic or the algebra of logic--there are various terms --to these problems of science. By this time, Russell wasn't quite as active in that particular field as he had been. He was writing books on education and on morals and marriage, and on politics, and he was, by the late 1920s, no longer the leader that he had been some fifteen years earlier.

Daniel: Did the Vienna Circle see itself historically attached to Russell?

Lenzen: I think the Vienna Circle probably thought that they had greater associations to Ernst Mach than to anybody else. Carnap wrote a little book entitled Scheinprobleme in der Philosophie, in which he set forth the doctrine that the fundamental issues which philosophers had been debating were meaningless, senseless, since you couldn't make any empirical distinctions between them.

The whole trend of the logical positivists was anti-metaphysical. Russell had had that phase when he gave his Lowell Lectures in 1914 on "Our Knowledge of the External World as a Field for Scientific Method and

Lenzen: Philosophy." Russell had changed his position so frequently, and oscillated from one extreme to the other, that you can't specify that he has held any specific position.

Another man that I looked up in Vienna was an Edgar Zilsel. He had written something that interested me. He didn't have a university position. (One of the things that greatly impressed me in Europe was to find outstanding people in various subordinate positions.) I think he probably taught in some school. He wasn't directly affiliated with the Vienna Circle. Subsequently, he came to this country and wrote some very good articles on the history of science, especially on the sociological and technological bases of the development of science. Then he became professor of physics at Mills College. He was rather unhappy there because he had to do so much work, setting up apparatus and so on, didn't have any assistants, and for some reason or other he committed suicide. Very tragic affair, very unhappy.

Another person in Vienna was Herbert Feigl. He's now at the University of Minnesota, as director of a center for the study of the philosophy of science. Feigl knew

Lenzen: mathematics and physics and took his degree under Schlick. I didn't meet Feigl in Vienna, I met him only later when he came to this country and came to Berkeley.

Late in the 1920s a young man by the name of A.E. Blumberg, American, went to Vienna and got his degree under Schlick, I believe, and he and Feigl wrote an article for the Journal of Philosophy, and in this article they introduced the term "logical positivism." Schlick called the point of view "consistent empiricism," and Charles W. Morris of Chicago, who was the American most active in this unity of science movement--I think he was a pragmatist--invented the term "logical empiricism." In this way he tried to unify the logical bias of the Vienna Circle and the empiricistic bias of the American pragmatic philosophy under the title "logical empiricism."

Another person I visited was in Munich, a man named Hugo Dingler. He was a very prolific writer. He had a rather individualistic view about matters pertaining to the philosophy of science. His view was that you could deduce from theoretical considerations a set of categories which you could impose upon any scientific material. He

Lenzen: showed great independence of thought, but he was entirely out of the current of development. I wrote reviews of a number of his books, for the Philosophical Review, so I was known to him. I had tea with him in the Englische Garten. Dingler wasn't a regular professor; he was lecturing on trigonometry for forestry students when I went to his class. I had some correspondence with him, and I think I treated him with more respect than most people did. He was opposed to the theory of relativity and to new theories. When I reviewed his books, I did so as sympathetically as possible. Whenever I write a review I don't like to take the dogmatic attitude that I know the subject and the author doesn't. Some reviewers do, you know.

With regard to logical positivism, the Berlin group is not to be considered as directly affiliated with the Vienna Circle. The dominant personality in Berlin was Hans Reichenbach, who wrote expertly on relativity, space and time, probabilities, and subsequently, when he came to Los Angeles, on quantum mechanics. The reason why I would not put him directly in the Vienna Circle group is that the Vienna Circle people maintained--at least that was the

Lenzen: view of Carnap and Schlick--that certain basic issues in philosophy were without meaning, since you couldn't decide one way or the other. They were not verifiable. I have in mind the issue between, say, realism and idealism, which was certainly a central issue in American philosophy around 1910. Also, it was central in British philosophy. The Carnap point of view was that since you couldn't decide empirically which view was correct, the problem--the scheinproblem--was not a real problem.

Reichenbach took very definitely the realistic position, and therefore you could say that he did not deny metaphysics in the sense in which the Vienna people did. Reichenbach considered himself a scientific philosopher and gave his presidential address to the Pacific division of the American Philosophical Association on the rise of scientific philosophy. Since he took this view, you couldn't say that he had thrown out all metaphysics, and once you've admitted this problem, then you're back on the philosophical level again. As a matter of fact, my observation is that the Vienna Circle has pretty well disintegrated. Otto Neurath, who was a prime mover in it,

Lenzen: is deceased. Moritz Schlick is deceased. Wittgenstein, who really provided the basis of the movement, is deceased. Carnap as I understand has withdrawn from his extreme position. Philipp Frank is in this country and he represents the original position to a certain extent.

Unity of Science Movement, 1934-1940

Lenzen: A very important person in this unity of science movement was the sociologist in Vienna, Otto Neurath. He was a very bulky, fat, good-natured gentleman. He was very skillful at getting money out of foundations to conduct international congresses on the unity of science. I think the first congress was in 1934, in Paris, though I didn't attend. I don't think there was a congress in 1935. I attended the one in 1936 which was held in Copenhagen, at Carlsberg. Niels Bohr was the host.

Carlsberg was the estate of Carl Jacobson who founded the principal brewery in Denmark and then built a mansion in a beautiful park and left his property,

Lenzen: the grounds and the house, so that the income was to be devoted to supporting scientific research, physical and chemical. As I understand, the most distinguished Danish scholar was to occupy Carlsberg, and according to my information the first occupant was Harold Höffding, a professor of philosophy. He wrote very well on the history of philosophy. Niels Bohr was the second occupant of Carlsberg. This 1936 meeting was held in Bohr's house, and there were opening sessions at which various people spoke. ~~The Count de Neuilly~~ Le Comte du Noüy spoke; he worked many years in this country for the Rockefeller Institute, and about that period he was writing philosophical books on biological science. Then he wrote a book called Human Destiny. A very delighful gentleman, spoke English perfectly, but he said he could express himself better in French. (Well, I think this was just a matter of national pride. I have observed at international meetings that scholars who could speak English perfectly would use their own language and give their paper, but in discussion they could speak English as well as anybody else.) Another person who spoke was a man from Poland, Kotarbinsky, and then I

Lenzen: spoke for the United States.

Daniel: What was your subject?

Lenzen: Work in logic and such matters in the United States.

The sessions for papers were given in the Institute for Theoretical Physics at the university, Bohr's institute. I presented a paper, and Edward Tolman had submitted a paper, but he didn't come and my recollection is that I read his paper. This was a very delighful occasion; there was a very fine dinner and outing at Marienbad, some distance north of Copenhagen, right on the seashore in a fine hotel. I think the whole thing was financed by the Rockefeller Foundation.

After the meeting in Copenhagen, I went to Oslo and was a delegate of the University to the International Mathematical Congress, which was in Oslo in 1936. This was a very nice occasion. I met Norbert Wiener again, after having not seen him for about twenty years. I haven't very much to say about that meeting. They had fine dinners and excursions on the Oslo fiord, under the patronage of the Crown Prince, and so forth.

The next congress for the unity of science was in

Lenzen: 1938. Oh, yes, at this meeting in Copenhagen, Neurath was planning an encyclopedia of unified science and he asked me to write a monograph on procedures of empirical science. I did that mainly in the summer of 1937, and the fall of 1937, and I think this came out about 1938 or 1939, I don't remember exactly. It took a long time, however, for him to get all the papers. Egon Brunswik, for example, wrote one on psychology which took about ten years. They finally all came out, however. Only two volumes of this encyclopedia came out before Neurath died. He was a refugee during the war, and when he died a good deal of the motive power disappeared. However, the University of Chicago Press took over these two volumes, and for a year or so these volumes were offered free if you joined, I think, a science club—one of these book-of-the-month clubs. These volumes got a good deal of circulation.

Daniel: What did you think of them?

Lenzen: Well, I have this to say about the monographs. I was given to understand they they would be understandable to the general intellectual and scientific reader, and so the

Lenzen: standards which I used were adapted to that, but I think a number of the monographs are of very highly technical nature. I have sometimes wondered if I shouldn't have made mine more technical. However, I do know that my particular monograph has been used a great deal for instructional purposes. In fact, a year or so ago, I had a request from Argentina to have it translated into Spanish and included in a volume of things. There is a relatively young man by the name of Mario Bunge (a lot of Germans went down to Argentina so that he had German ancestry), a professor of theoretical physics, who became interested in philosophy, and in the last year or so he's published two books, one on causality and the second one entitled Metascientific Queries. He was at a Stanford meeting, the Congress for Logic and Scientific Methodology. This year he's in this country teaching at the University of Pennsylvania. I met him for the first time at Stanford. He's very attractive personally, and Harvard University Press asked me to give an opinion on his manuscript on causality, and I reported favorably on it and they published it. He was the one who translated one chapter of

Lenzen: my monograph into Spanish, to make it available to his students and also for a combined volume of papers on these general matters.

The next congress for the unity of science (1938) was at the University of Cambridge, England. It was held in Girton College; we were housed in Girton College, which is about a mile outside of the city of Cambridge proper. This was a very delightful affair. I think there was a congress held in this country in 1940, but these congresses for the unity of science seem to me to have ended.

You see, Neurath was the motive power behind them, and now we have instead these congresses for logic and scientific methodology, which in some sense are a continuation, but by a different group of people--Tarski, for example, is one of the principal persons back of it, and the people interested in axiomatics and logic and so forth. The question of the unity of science doesn't come up. It's mainly the foundations of mathematics and physics that they discuss--logical problems and problems of methodology. This last summer, 1960, it was possible

Lenzen: to get funds to bring people from Europe and there was another congress for logic and methodology. The first one was in 1957, I think.

Daniel: Would you say that logic and methodology is the developmental continuation of the energy that went into the unity of science?

Lenzen: Yes. You see, the unity of science movement was anti-metaphysical, and its aim was to find a common language for all the scientists. That's the reason for the term "unity of science."

Daniel: Could you explain why the anti-metaphysical attitude was so strong?

Lenzen: Within the confines of the Vienna Circle, before this last war, there was a special attempt to eliminate unsolvable traditional philosophical problems, idealism vs. realism, immortality, things of that sort, and the basis of this rejection was a principle that meaning depends upon a principle of verifiability. If you could not verify a certain proposition, or disprove it, then the proposition was meaningless in their sense. In an

Lenzen: early work of Carnap's entitled Scheinprobleme in der Philosophie he discussed the issue of idealism vs. realism; where there's no empirical criterion kept of whether the one is true or false, it's a scheinproblem --an apparent problem.

I remember one of these congresses--I guess it was the one in Cambridge--one of the people who attended was a mathematician from Holland named Van Dantzig, and there was a confrontation between him and Neurath. Neurath said, in effect, that there was too much prejudice or emotional consideration in metaphysics, and Van Dantzig said to him that he was quite irrational in his opposition to metaphysics. The reason I brought that up is that you raised a question: Why should they be so a-metaphysical?

Wittgenstein, whose work was a sort of foundation for this unity of science, maintained that philosophy had no subject matter but was an activity. Schlick had taken up that point of view, and the others of the Vienna Circle. This in fact was the repudiation of the whole history of philosophy. That's really what it amounted to: philosophy didn't have subject matter. It was just

Lenzen: an activity which clarified the meaning of conceptions and they were especially interested in the concepts of mathematics and physics, exact sciences. Neurath, of course, was trying to introduce exact science into sociology.

I think I told you on one occasion that philosophical attitudes sort of oscillate from one extreme to the other, even in a single individual in his lifetime, and that's especially illustrated in the case of Bertrand Russell. In later years Carnap, who next to Wittgenstein was the most constructive thinker in the movement, modified his previous views very much. The present group is especially interested in developing series of systems and methods of mathematical logic, and this is an ultra abstract type of subject matter.

Daniel: Why has this become a pressing interest?

Lenzen: I think it's a result of the fact that logic has become a part of mathematics. They're not particularly interested, so far as I can see, in any general philosophical applications. It seems to have become a subject of its own, and only the very initiated can even know what they are talking about.

CHANGES IN PHILOSOPHY AND SCIENCE

Lenzen: A number of years ago Herbert Feigl published in the Philosophy of Science, I believe it was, a very long article in which he reviewed all the possible theories regarding the relation between perception and the external world, and he came out with a realistic position. This was characteristic of the way things developed in philosophy; there was an oscillation from one extreme to the other. You have an extreme idealistic position in one generation and the succeeding generation goes over to the opposite extreme, and then they find difficulties in that, and they come back to the original. This is, I think, one of the reasons why I was very dissatisfied with philosophy as a profession, because once I had decided that you could debate these issues interminably, I didn't see any further use in continuing the argument.

Daniel: However, you came back always to physics and philosophy, didn't you?

Lenzen: Well, I came back to it for a number of reasons. The paper on the theory of relativity before the Philosophical Union was a great success, and once you produce something for which there is an audience it's very difficult to abandon it.

The second point is that there really are problems which have arisen in physics since 1900 which require a conceptual analysis, which an ordinary practicing physicist would not engage in. There really is a problem there. So it's very difficult for one who has been thoroughly grounded in the traditions of philosophical analysis, once he discovers these problems, to refrain from doing what he can.

Daniel: In teaching, has your subject matter always been physics?

Lenzen: Well, at one time in the 1920s I gave a course in the history of physics for a number of years.

Daniel: Does the philosophy of physics in this university find itself in the department of philosophy?

Lenzen: Oh yes, completely.

Daniel: Have you given any courses in that department?

Lenzen: Yes, I did give a course in the department of philosophy

Lenzen: when I was writing my book, The Nature of Physical Theory. This was the spring of 1930, but that was the only time.

Daniel: Was there any inclination to continue the course?

Lenzen: I think the department of philosophy would have been glad to have me continue it, but the chairman of the department of physics, who was very favorably inclined to me, and who was trying to find more funds for his department, made the suggestion that the department of philosophy contribute partly to my salary, and I decided it was a very bad scheme. I decided that being divided between two departments was a very unwise provision from my point of view.

Daniel: Your first paper for the Philosophy Union was on the theory of relativity. Was your next paper the one on the philosophy of nature in the light of contemporary physics?

Lenzen: Yes. After that paper was published I sent a separate copy to Professor C.I. Lewis at Harvard, and he wrote to me and said he found it very enlightening, and suggested that this be expanded to a book. I was really so concerned about fundamental questions, and there were other

Lenzen: reasons why I never did that. Of course, this book that I later published, The Nature of Physical Theory, in a sense has a historical cast to it.

Daniel: What problems in physics lead to philosophical consideration?

Lenzen: Well, of course, in the theory of relativity, as a result of the relativistic theory of space and time, the question arises, to what extent does this theory apply to the space and time of the external world, and to what extent is it merely a hypothetical construction for purposes of calculation? This issue was debated very vigorously in the 1920s. There were philosophers who said that we know by intuition that space is Euclidean, and all that the relativistic theory does is give a framework for calculation; therefore, there is no conflict between the traditional idea of Euclidean space and the non-Euclidean geometry which relativity brought forth. You see, as soon as you raise that question you raise the whole question of just what kind of knowledge about the world do you get from physics? That's a highly debatable question, and when people become aware of it, they can take opposite points of view.

Lenzen: Now, the working physicist doesn't think about these matters. He probably thinks in terms of a distinction between primary and secondary qualities, which goes back to Democritus in Greek times, and Descartes and Locke and Galileo in the early modern period. That is, he would look upon the spatial and numerical qualities of things as being independently real, and such things as color and taste and sound as existing merely in sensations. But philosophers have found such a dichotomy between qualities very difficult to comprehend.

Earlier I referred to the 1910 platform for the new realism, in which six American philosophers said you could believe that what you see is independently real, and the criticism to which it was subjected. About ten years later, another group came forth with what they called critical realism, which was in effect re-establishment of the doctrine of Locke and Descartes. The new realistic doctrine disintegrated completely. This neo-realistic movement was a revolt against dualism, but it seems to have collapsed and it's been my experience with philosophers that when you really pin them down on a question as to whether the external world is real or not and what

Lenzen: the relation of the external world is to what you perceive, that they're very vague and hazy.

The development of quantum mechanics created further problems because especially in the formulation in which Bohr and Heisenberg were mainly responsible, there is this principle of indeterminacy which arises from disturbance of objects during the process of measurement. The question is, just what is the nature of our knowledge which is given us by these equations of quantum mechanics? I spent a great deal of my time in the last thirty years thinking about this problem, and I have written a number of things about it. I don't think that the average philosopher or the average physicist, even, has a real understanding of what the problem is.

Daniel: You mean because the physicist hasn't the discipline of the philosopher, and the philosopher hasn't the discipline of the physicist?

Lenzen: One of the difficulties is that it requires an understanding of the fundamental mathematical theory which only relatively few people have. I think the reason why I have a certain confidence in my own views in the matter

Lenzen: is that I had very good opportunities to hear Niels Bohr expound his views in the matter, and he was by common consent the most profound thinker in the matter, but the nature of his thinking was such that it's somewhat apart from the work that the average physicist does in the laboratory or in making calculations. It's possible to make calculations without really understanding what you are doing. My preoccupation with this problem over many years has possibly interfered with productivity. If I could have solved this problem satisfactorily I might have been able to produce more.

Of course, the physicists are now concerned about matters of politics and morals, a good many of them. The destructiveness of these atomic and hydrogen bombs has caused physicists to be interested in questions of political aims. You know, of course, of the Bulletin of the Federation of Atomic Scientists, in which you find all sorts of discussions.

My own concern has not been in that direction at all. Using philosophical language, one would say that my interest has been epistemological, the question of the nature of

Lenzen: knowledge, rather than a question of morals. My own tendency is to say that a scientific man cannot really control the use of scientific discoveries. When Einstein's theory of relativity was set forth in 1905, and Einstein subsequently developed his formula for the relation between mass and energy, $E = mc^2$, nobody could have foreseen the destructive possibilities arising out of that formula. Nobody could have foreseen it. Should Einstein have refused to publish this formula? He couldn't foresee the possibilities. Probably there was no man more horror-struck at the use made of physics with atomic bombs than Einstein. Any particular scientific fact or technological advance can be used either for the benefit of society or for the destruction of society, and you can't tell in advance just what's going to be done.

You see, I really like to be an ivory tower thinker, although I do take a great deal of interest in the affairs of the outside world. But I don't like to debate with people, and I find that I know more about political matters and historical matters than most of my colleagues.

Daniel: Where do you consider that knowledge is going to help

Daniel: expand knowledge in philosophy?

Lenzen: I think historically it is a fact that philosophical doctrines have been based to a considerable extent upon scientific discoveries and developments. The doctrine of the Pythagoreans, in ancient Greek philosophy, is based upon mathematics, and Plato's theory of ideas was really an interpretation of the existence of mathematical objects. In history, one finds that for the most part leading philosophers were also leading scientific men--Descartes, for example, and Leibnitz, and Kant. After there are certain developments in a scientific field, there is a modification of philosophical ideas. You could say that the philosophy of Kant was simply based upon a philosophical justification of Newton's theory of mechanics, and perhaps ultimately we'll have a new type of philosophy which is based upon the physics of the 20th century.

One difficulty is that there is no unified system of physics, and it's very difficult to build a philosophy upon a science that is not completely unified. Of course, philosophers will attempt it, and attempts have been made,

Lenzen: but the prospects for success are somewhat dubious. There is a tendency on the part of philosophical thinkers to extrapolate and try to generalize beyond what is scientifically known. Schlick's point of view was that philosophy was not a subject, did not have subject matter of its own, but was an activity, and his view was that all scientific fields could be treated philosophically through the clarification of meaning. From that point of view, the great systems of the past are merely historical islands, and you wouldn't expect to duplicate them. But I think eventually somebody will try to do that.

It has been pointed out that as a field of philosophy becomes definite and positive it is lopped off from the whole field of philosophy. At one time such subjects as economics were a part of philosophy, and what is called political science. And logic, as the result of the development of logic or symbolic or mathematical logic, has now been taken over my mathematicians. However, we are still in a transition period because philosophy departments, at least a number of them, will have symbolic logicians

Lenzen: on their staffs, and so, especially in this university, a student could study mathematical logic either in the philosophy department or in the mathematics department. Fifty years ago, logic was only in the philosophy department. It may be that in another fifty years logic will be only in the mathematics department.

At one time, we know, psychology was really part of philosophy. When I was a graduate student at Harvard University psychology was in the same building with philosophy, and psychology in effect was almost looked upon as part of philosophy. Now you find psychology in the life science buildings. Psychologists associate with physiologists, statisticians, and sociologists, and so forth. Psychology has changed enormously in my lifetime. There has been the development of behaviorism, and there's even a special science now called behavioral science, which has special publications. I sometimes wonder just what kind of a science psychology is; sometimes it's classified as a social science and sometimes as a natural science. If the psychologist is mainly using statistical methods to study social phenomea, it's really a social science

Lenzen: rather than a natural science.

There has been a great deal of interest in the last few years in the application of these methods of logic to the problems of economics and psychology, and so forth, and also to problems of physics--to constructing a set of axioms. Very possibly the best and strongest school in this country is at Stanford University.

Daniel: Who were the people associated with this?

Lenzen: Well, there was a man by the name of J.C. McKinsey, who was the leader in the group of logicians, but for some reason he committed suicide some years ago. He had a younger associate who has certainly taken over the leadership and is doing very well, but the name of Patrick Suppes. McKinsey and Suppes and their students have produced sets of axioms for fields in classical mechanics and they are also working on matters of interest to economists, concepts of utility, for example, This is a very limited type of activity, so far as philosophy is concerned, and the persistent traditional problems of philosophy are just ignored.

Lenzen: Some months ago I wrote a short review of a book consisting of a symposium of papers given in 1957 on the subject of measurement. Measurement was considered in a general way by various speakers, in postulates and measurement, and problems were considered for physics, and then for economics and psychology. There is a great eagerness on the part of people in the non-quantitative disciplines to imitate as much as possible the methods of physics.

Daniel: Do you think it's a matter of imitation or a feeling of the need for exact quantification?

Lenzen: They want to be scientific, because there is a lot of prestige associated with science in the present age.

Daniel: This may be true. At the same time, it is helpful in any area of knowledge to be able to quantify.

Lenzen: However, when you quantify you have to simplify, and when you simplify you may falsify. You have to construct a model of a real situation.

The uninitiated, who doesn't know very much about mathematics or physics, may have an entirely erroneous idea of what a mathematician or a physicist can do. The

Lenzen: layman thinks that the physicist can answer all sorts of questions, out of his head on pure theory, questions that really are so complicated and for which we don't have enough empirical material. In the effort to become scientific, as I know about it, in the so-called social sciences, they make a great deal of use of statistical methods, just counting, you see, compiling statistics, and they feel that they're scientific. I suppose it is scientific to a certain extent, but--

From what little I know, American sociologists in their great efforts to be scientific, have been concerned with piling up statistical data, and that's no substitute for an understanding of certain basic relationships and processes. The subject is not as controllable and is not subject to the same simplification required for analysis as material in physics or chemistry or astronomy.

Daniel: The term "social science" seems to arouse the scorn of some scientists.

Lenzen: I think it's correct. I think that the people engaged in social studies have tried to rise to prestige on the coat-tails of the natural sciences by calling their disciplines

Lenzen: "social science." And they've gotten away with it.

Daniel: What about considering social phenomena according to a scientific method--I mean scientific method as a term which is apparently accepted among scientists: selection of fact, evaluation and verification of facts.

Lenzen: Well, there may be a certain justification for applying the term science to social science. The Germans have the word wissenschaft [science, knowledge], which is broad, and naturwissenschaften and geisteswissenschaften. If you use this broad concept of science, which makes it practically identical with knowledge, you could say the field of social study (if sociologists or psychiatrists interview some call-girls, for example, and write a book --there is such a best-seller out in a paperback) is social science.

The fact of the matter is that there is enormous activity on the part of psychologists and sociologists and economists, and even people in business administration, to make use of the methods of logic and the quantitative methods of physics (because physics has provided the model of quantitative science) so as to become more scientific.

Lenzen: There is at the present time a great interest in what is called operations research in business administration, in which they are using these techniques. This could be considered a type of science. What I would question is this: If a professor of political science gives his opinion of national issues and thinks that a Democrat should be elected over a Republican, I don't consider that scientific.

Daniel: I think most people would agree with you. What I'm trying to find is some definition of science and scientific. You have written on the nature of scientific truth. I think it would be helpful for you to comment on the meaning of these words.

Lenzen: When I wrote that paper on the nature of scientific truth, I didn't know about these comparatively recent developments--which are really recent, and did not exist to the same extent that they do now--these developments in so-called operations analysis and creation of models and so on. I think that if I were called upon to frame a definition of science so as to include everybody who wanted to call himself a scientist, I'd have to modify what I wrote.

Daniel: Your concept of science and scientific is different today than it was.

Lenzen: I would say that the use of the term is different. You have this theory of games, for example, in economic behavior, and this operations analysis, and this great use of statistics by the sociologists. I once asked a professor in the history department, "Is history a social science or not?" He said that the department was divided, half and half. Half thought it was a social science and the other half thought it was a humanity. I would very much question whether the writing of history is to be considered science. The word "science" has acquired a great deal of prestige, so everybody else wants to share in the technical reputation.

I was reading a book that was first published about fourteen years ago by John Mills, of the Western Bell Telephone System, The Engineer in Society. He discusses the concept of research and indicates a very wide use. Now, if educators make a statistical survey of the opinions of high school administrators, as to whether certain subjects should be in the curriculum or not, that is considered

Lenzen: research, and Mr. Mills questions the propriety of calling that research really fact-finding. The question as to what you call research is very much like the question as to what is the meaning of science. If the sociologist should be considered a scientific man, why isn't an archeologist? Ordinarily we don't think of archeology as a science; it's a part of the humanities, but there's a lot of science required in archeological work.

Daniel: Yes, and now that there's a way of determining age, this relationship is quite apparent.

Lenzen: You mean the carbon 14 business. Well, it's the use of physical method.

I think what has happened in the history of science is that certain quantitative methods have been developed, certain mathematical methods, especially by physicists, and then they've been extended to other sciences, borrowed by other sciences, to as great an extent as possible. Chemistry really has taken over from physics in fundamental principles, and so we have physical chemistry, and now we have nuclear chemistry, and the tools of the nuclear chemists, at least the big machines, have been provided

Lenzen: by physicists. But of course the chemists know more about quantitative analysis than the physicists do, and so when you get a set of products produced by bombardment from particles of the cyclotron, you have to have chemical methods to help you identify those products.

Daniel: Do you know if the 1958 supplement of the International Institute of Philosophy titled Philosophy and Physics in the United States of America has been published?

Lenzen: Oh, yes. You see, there is an International Institute of Philosophy, which headquarters in Paris, which I think is sponsored by UNESCO, and every few years they publish a chronique of philosophical writings. Right after the war, I received a request from the secretary to prepare a memorandum on the principle works on physics and philosophy in the United States. I was given a certain period, I think from 1939 to 1945-46. Later I was asked for another one up to 1949, and then I was asked for still another. I have the volumes of the first two summaries that I wrote, but I didn't get the third volume which came out some months ago in a book edited by Klibansky,

Lenzen: Philosophy in the Mid-Century. He's in England. This report of mine was included in that.

The first two volumes that I got were in paperback, and had the Institut International de Philosophie, under the sponsorship of UNESCO, on the front cover, but this volume by Klibansky was got out in a somewhat different form, and the connection with the Institute isn't apparent, as I recall. I have looked at the volume, and from the title you'd judge that it was just a typical philosophical book. The reason I looked at it is that I read a review of it in the British Journal of the Philosophy of Science, and the reviewer mentioned by contribution. Of course, it's characteristic--an egotistic author, when he finds his name is mentioned, wants to see what they say about him.

They sent me a copy of the very first one; the second one I found out about, and the way I found out about it was rather curious. There is a professor of philosophy in the State University of Iowa, by the name of Gustav Bergmann, who writes some philosophy of science; he got his Ph.D. in mathematics. He wrote me a note and said he

Lenzen: had heard that I had mentioned him in this account. He had written something about quantum mechanics which I'd mentioned. I wrote back and said that I had no recollection; I'd forgotten about it; I'd had some serious cholecystectomony surgery in the interim and had forgotten. Shortly after, he wrote to me and gave me the reference, and then, through Stechert-Hafner, I got the volume. Otherwise, I would have forgotten entirely.

COMMENTS ON EDUCATION

Lick-Wilmerding School, 1948

Daniel: In 1948 you became chairman of a faculty committee named to study Lick-Wilmerding School. What is the background of this assignment?

Lenzen: You see, I graduated from the Lick School in 1909. There were two schools, the California School of Mechanical Arts, called the Lick School, founded by James Lick, from which I graduated, and the Wilmerding School, right across the street. The Regents of the University of California were the trustees of Wilmerding.

From 1915-1920 the two schools were consolidated, so it became known as the Lick-Wilmerding School, and Mr. George A. Merrill was the first director of the school, and he also became director of the Wilmerding School within a few months after it started because the first director, Mr. Schwartz, died. He ran the two schools

Lenzen: and the joint school from about 1895 to 1938 when he retired. He was an absolute autocrat. As far as the Regents were concerned he did anything he wanted, and one of the members of the Lick Board, Mr. R.J. Taussig, was elected president of the Mechanics Institute and ex officio he became a member of the Board of Regents. The situation was such that the members of the Lick Board controlled Wilmerding and many of the Regents.

After Mr. Merrill retired in 1938 the joint school was in trouble, so in 1948 I was asked to be chairman of a committee to make a report to President Sproul. I began to look into it and I was horrified to discover the poor state of their finances.

Daniel: This was a school which was not tax-supported?

Lenzen: No. They were running the school on about $60,000 a year. This wouldn't even run the administrative offices of a public high school. The salaries of the teachers were low, they had no retirement system, and buildings were old, out-moded. A joint administrative board, consisting of three members appointed from the Regents and three members from the Lick Board, were supposed to be in charge

Lenzen: of the schools, appointing the director and passing the budget, and so forth.

The Lick people really ran affairs, and had great difficulty in getting decisions out of President Sproul. As the president of the Lick Board said to me, "It takes three weeks to get a telephone conversation." Two members of the Lick Board met with President Sproul and said, "Isn't there some way in which we could expedite the consideration of the Regents on common matters?" So the president said he appointed me as her personal representative. I didn't want to be that, but you can't very well turn it down, and I began meeting with the joint board.

Professor Hiram W. Edwards, director of relations with schools, was one of the Regents' representatives. The representatives of the Regents usually included one of the Regents. During the period that I was involved, William Merchant, who was a graduate of the Wilmerding School and who became president of the Mechanics Institute, was a Regents' representative. He was an architect.

It was a ticklish thing, though of course everything that I did is recorded in the files in the Regents' Office, with the letters that I wrote.

Lenzen: Some years before, the Lick Board had conducted a drive among the alumni to raise some money. They had a plan for modernizing the school. The administrative board considered various plans, discussing what should be done with the school. A graduate of the school by the name of Edwin Rich, who was the headmaster of the Town School, a private school for boys in the Pacific Heights district, had been asked to submit a report. My committee was asked to analyze this report and to comment on it. I listened to these discussions, and it occurred to me to ask certain questions, and so I called up Professor Frank Hart of education--Hart and Lars Peterson used to go around investigating schools and school systems--and asked him some questions. He offered to go over there and grade the school; they have a regular grading system on the basis of 1,000, and a practically perfect school will get around 900, and an average good school will get 700 or 800, and if they fall below, say, 500, they are pretty poor. I took Hart and Peterson over to the Lick-Wilmerding School, and the grading which they gave was below 300!

Daniel: This is on the basis of staff--

Lenzen: No, mainly on the school buildings and grounds, the physical plant. One of the buildings was a brick building which had been built by Wilmerding apprentices, and this was the property of the Regents. There was an old wooden building and this brick building, built around 1910 or so. Hart and Peterson raised the question as to the safety of the brick building with respect to earthquakes. In 1933 the state legislature had passed the Field Act, after the earthquake down in Long Beach when a lot of schools were destroyed. Hart and Peterson indicated in their report that these buildings should be investigated by a structural engineer.

So I called up Harmer Davis, a structural engineer at the University, and he offered to inspect it, and he did another thing which he probably shouldn't have done because the University doesn't like to have outside state agencies involved; he called up the supervising structural engineer of the state division of architecture, who was the leading earthquake expert in the state. A few weeks after the Hart-Peterson report, I went to the school and I had in tow Professor Davis, a man from Sacramento who

Lenzen: happened to be in this area, and the San Francisco representative of the structural engineers of the state division of architecture. I had these three structural engineers.

Davis wrote me a report which was quite devastating. Of course, the buildings did not meet the requirements of the Field Act, and it was my contention that if the Regents were running a school they ought to meet the requirements of state law, just as well as the public schools. In fact, since it was tax exempt, if anybody were injured I think the Regents would have been in a very bad position. I submitted this report, and this caused the attorneys of the Regents and the secretary of the Regents, Robert Underhill, to be very nervous and so forth.

Then the Lick Board hired a structural engineer to investigate the matter, and the structural engineer made tests on the strength of the cement and so forth, and confirmed these reports. This scared the Lick Board, and the result of the matter was a meeting of the Lick Board and some of the Regents--Admiral Nimitz was one, and Jesse Steinhart and Bill Merchant. The Lick building was condemned, but the school was allowed to go on in the wooden building temporarily, and the Lick Board was told that

Lenzen: it had to bring new plans. The Lick Board worked awfully hard, and they deserve a lot of credit; they acquired a nice property right across the street from the City College of San Francisco, and raised some money and built a new plant, so the old buildings were abandoned, and the Regents sold the property to the Teamsters Union, I think. About a year and a half ago, the wooden building caught fire and there was a big blaze that lit up the whole Bay Area; the city fire department, however, was very efficient, and it didn't burn to the ground but was pretty well gutted. Several months ago, when I was riding on the bus down to Palo Alto, I passed the school and saw that the buildings were being razed, and so it's all gone.

Then I was appointed to the administrative board, and I found that things were being done without the knowledge of the administrative board that shouldn't be done. I found out, for example, that the director of the school had a direct pipeline to the University Regents' budget, and that the budgets for the Wilmerding School were being passed by the Regents before the administrative board, which had budgetary authority, had any knowledge of their

Lenzen: content. I discovered another thing: back in 1933, Director Merrill had written a letter to the secretary of the Regents suggesting that insofar as any of the teachers were on the Wilmerding fund they should be notified that they were being paid so much per month, so that at the end of any month they could be dismissed. This was still standing--just think of running a school on that basis in this day and age when the public school teachers have at least a yearly contract and tenure.

Daniel: Why would anybody teach there?

Lenzen: Of course there were a lot of old teachers. I have copies of the letters that went out. For example: "Mr. So-and-So, You are hereby appointed as instructor in plumbing in the Wilmerding School at $100 a month." And I think there was also the provision that they were not to be paid for any holidays or vacation periods. Mr. Merrill was a real tyrant from all I heard from the teachers. It's very bad for a man to have absolute power over so many years; in his letter to the secretary he said that this was the depression, so he must protect the Wilmerding fund, but the Wilmerding fund already had a surplus of $50,000, and it

Lenzen: seemed to me that it was pretty small on the part of the Regents not to give a man a contract for at least a year. In the letter that I wrote to the president I said that these were 19th century practices that wouldn't be tolerated for one minute by a public school board. President Sproul said he didn't know anything about it; it was all done by Merrill and Bob Underhill.

Daniel: How long did you continue your tour of duty on the administrative board?

Lenzen: I finally, I think about 1954, got out of it.

They made another arrangement; the Lick Board made a proposal that Regents' representatives be authorized to meet with their board, and there was a discussion about procedure. The final outcome was that the Regents just handed over to the Lick Board the income from the Wilmerding fund and the Lick Board is supposed to report to the Regents twice a year--that's as I understood it several years ago.

Daniel: Then the Lick Board runs the school?

Lenzen: Yes, the Regents merely give them the money. This was, in my opinion, a good arrangement, because the Regents weren't interested in the school, and nobody from the

Lenzen: University was particularly interested, and the Lick people were really interested, they really worked at it.

Another thing I found out: in 1900, the Lick people had a constitutional amendment, a provision put in the state constitution, exempting their school from taxation. Two schools, Wilmerding and Lick, exempt from taxation. However, this constitutional amendment provided that every year a report would have to be made to the governor as to the expenditures. It occurred to me to see what sort of reports were made, so I got the documents division of the Library at the University, and Miss Jackson was very cooperative, and said they had a representative up in Sacramento who would look into the matter, but after a couple of months the reply came to me that they could not find any reports from the Lick Board to the governor, although it is required constitutionally. I was curious to see what the nature of the reports were, but I could not find any evidence and I didn't pursue the matter any further, I just let it go. It had caused me too much grief as it was.

Mathematics Teaching

Daniel: Has the growing urge to measure that you spoke of a while back stimulated the teaching of mathematics?

Lenzen: The fact of the matter is that the mathematics department had to introduce qualifying examinations for admission to mathematics 3A, which was the basic course taken by engineers, and chemists, and aspiring physicists, because it was found that the students who came from the high schools, a good many of them, were not properly instructed. The entrance requirements of the University are so high in comparison to other state institutions that we get only the best 10 or 15 per cent of students. Well, if among the 10 or 15 per cent best in the state, you get, say, a third who've had the high school courses who are not sufficiently grounded in mathematics to go on in the University, this looks like a very serious situation in public education.

Daniel: Has this always been so, do you know, or is this something that has developed recently?

Lenzen: I think it's developed in the last twenty or thirty years, as a result of the decline of standards, not in all the high schools but in quite a number of high schools, and I think this is the result of mass education. Mass education means diluted education, and although Professor Joel Hildebrand is continually denouncing the educationists, he was a member of the Citizens' Advisory Committee that reported to the Governor of the State of California, and I've heard him expound very frequently on the subject.

I don't blame the educators too much. I think, for the most part, they are uneducated people, that is in the sense of sixty years ago, and they have pretty poor material to work with, and it's inevitable, it seems to me, that there should be this decline. At the present time there's a great deal of comparison between our system of education and the Russian--well, the Russian system is merely mass education, or at least it's merely an application of the European system, and our high school education sixty years ago was comparable to what one would get in Europe. But as a result of the extension of these laws

Lenzen: requiring attendance of children in school up to the age of sixteen or eighteen, with the masses going to high school and so forth and wanting to go on to college, I think the decline in standards has been inevitable.

Professor Hildebrand told of one instance that I thought was very interesting. There was a certain woman in the Citizens' Advisory Committee, I think she lived in Roseville, and she was very much interested in these matters. The fact that she was appointed to the committee indicates that she had been active. At one meeting of this committee she brought arithmetic texts, the text for the fourth grade in arithmetic used by her own children in school, and the text that had been used by her and her husband from, I guess, twenty or thirty years ago. In the older text, multiplication and division involved numbers with several digits, and in the present-day text, only numbers with single digits were involved, showing the enormous decline in standards.

Present-day educators insist that students are now making higher scores than previous generations. The material has been diluted, so there is really no basis

Lenzen: for comparison. In the old days a child who didn't do the work wasn't promoted.

When I was working on the Lick-Wilmerding School report in 1948 I read Director Merrill's report for that year. (Since the Wilmerding School was under the trusteeship of the Regents, the director every year wrote a report to the president of the University.) He was quite a student of education in general, and he pointed out that a large number of students dropped out about the sixth grade, fifty or sixty years ago, and went to work. This was a line of demarcation, according to him, and he used this as the basis for argument for the junior high schools. The junior high school begins at the 7th grade.

My idea is: it was just about in the 6th grade that arithmetic began to get difficult, and if you didn't do your arithmetic in those days you weren't passed, so a large number of students never graduated from grammar school. Most of those that graduated from grammar school went to work as apprentices or something of that sort. Only a small fraction of the grammar school graduates went on to high school, and a still smaller fraction of

Lenzen: high school graduates went on to the University. But with the requirement that everybody be in school until the age of eighteen in California, you can't apply such a system.

Mrs. Lenzen was a school teacher, taught biology, and when she came to Berkeley she didn't do anything for quite a while, but then she went to Richmond, where she works in the Richmond schools not as a classroom teacher but in special education--speech correction and audiometry, lip-reading and so forth. She knows what goes on in the schools, so I get a lot of first-hand information. The way it is now, the child is supposed to remain with his age group, and so youmay have children in the 5th or 6th grade still reading first grade primers. Some months ago I saw a statement of a survey in San Francisco. There were students in the junior college, I think, who had only 5th grade reading abilities, something of that sort. They vary all the way from 5th grade reading abilities to the ability of a senior in the University. There are children in these schools who can't read.

The National Science Foundation has been sponsoring

Lenzen: institutes for science teachers. One summer I happened to be in the departmental office, and a woman came in to turn in some work. Professor Helmholz was participating in one of these institutes, and this woman--I would say around forty-five or so--was attending this institute, and I found out that she was a biology teacher in McClymonds High School in Oakland. She said she had a class in biology in which the average I.Q. was 55. She said she had students in sophomore biology who couldn't read. Now, sixty years ago a child who couldn't read wouldn't have gotten to high school, but now they not only get to high school but they get graduated. So, a high school certificate just means that the student has been on the rolls for four years.

I think the San Francisco high schools are probably a little better than the average. George Washington is very good, and Lowell has held the line--the teachers have fought having vocational and shop work put in at Lowell, so it's a good academic high school.

Daniel: Do most students in physics courses come from Berkeley and San Francisco?

Lenzen: We get them from all over the state. I think the distribution is pretty much according to population. Of course, with the establishment of UCLA, not as many students come up from the south as formerly, but still we got them from the south. And there were certain high schools with good physics teachers that produced good students; for example, one high school in San Diego, Herbert Hoover High School, and Mrs. Sorkness was highly praised as a teacher of physics and chemistry. For a number of years the top students in the class came from Santa Cruz, and there was a certain very good teacher by the name of Green down there.

Professor Hildebrand had a system of finding out from what schools the students came, then determining the rating of schools as to whether the students got A's, B's, C's, and so forth, and he would publicize this information. But there's one objection to this method. In Berkeley High they make it a point of flunk students and prevent their admission to the University so as not to lower the standard of Berkeley High. In fact, I once heard Professor Latimer of chemistry denounce that. He

Lenzen: said they were preventing a lot of students who could make a success in the University from coming to the University, and my colleague Professor Harvey White, a famed television personality now, told me--he knows the high school people because of his work in the science fairs--that these teachers say they want to be represented only by good students in the University. So the weak sisters are just thrown out, and therefore the school can make a fine showing.

It seems to me there is a certain disadvantage in having a too high standard. Cal Tech prides itself on the fact that they have very severe grading standards; it's almost impossible for a student to get an equivalent of an A, but the effect of that was that in competition for teaching assistantships, their students lost out. This matter of grading and standards is one that has a good many aspects to it that you wouldn't think of at first.

When the Oakland Junior College was started, one of our students who got an M.A. with us--and I was chairman

Lenzen: of the examining committee--Mr. Theodore R. Gentry, who had been teaching in the Oakland high schools, took over the work of physics in the Oakland Junior College. He came to see me to get advice, and we gave him a lot of help. I talked to him once about the question of teaching of mathematics in the high schools, and he was head of the department of mathematics, I think; he taught mathematics in high school, but he had an M.A. in physics so he taught physics in junior college. He said to me that the teaching of elementary algebra was very poor, but that there was good teaching in the higher algebra and trigonometry. These were highly selected students, you see, and in a school of 2,000 they would have only enough students for, say, two classes in higher algebra or trigonometry. These were the students who were planning to come to the University to go into engineering. I said to him, "Are there any students in the school who could do mathematics but don't take any?" "Oh," he said, "Yes." "Why don't they take it?" "Well," he said, "They can go out and earn more money collecting garbage, for example, than if they went on and got a higher education

Lenzen: for mathematics and so forth.* That's the way he expressed it.

Until very recently the high school administrators haven't liked to have teaching of mathematics and foreign languages in high school. When Professor Washburn of the art department died, I wrote the Memorial Resolution for him, and so I looked up his history and found that in 1940 he had written a paper for the California Journal of Secondary School Education, in which he defended the University's entrance requirements in mathematics and foreign languages. The school administrators were trying to get rid of those requirements because it restricted the number of students who could come to the University. Professor Washburn, who had been chairman of the committee on schools, was in favor of these requirements and he argued in favor of them. Well, the journal had an editorial comment indicating that they didn't agree with him. It's only because the Regents have their powers directly from the constitution and independent of the state legislature that this University has been able to maintain its high entrance requirements, but now, as a result of Sputnik and all this concern about Southeast

Lenzen: Asia and Indonesia and the Congo and so forth, they're now saying it's important to know foreign languages; and because of the importance of physics and mathematics, they're saying that mathematics is important, and the whole point of view has changed. There was a long period of time when, as I understand it, high school administrators as a group maintained that mathematics was just something for a person who wanted to study engineering, and a high school shouldn't be burdened with teaching that. The function of a high school was to prepare for life citizenship, and for daily living there was no need to do algebra and foreign languages.

That's democracy, and that's the point of view of the common man. Professor Hildebrand the other night was saying that teachers at Stanford University had made a study of the backgrounds of county school superintendents. Now, I can't be exactly sure of the figures, but of 129 county school superintendents, 79 had been athletic coaches with no teaching experience. Well, if a man is an athletic coach and then becomes a principal,

Lenzen: as for example the principal of Berkeley High and the principal of El Cerrito High, you can't expect him to have the same concern for the traditional academic subjects as was held, say, sixty years ago.

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