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OPHTHALMOLOGY



ORAL HISTORY SERIES



A Link With Our Past

An Interview with

Phillips Thygeson, MD



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A Link With Our Past

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Phillips Thygeson, MD
1960

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Phillips Thygeson, MD

External Eye Disease and the Proctor Foundation

An Interview Conducted by
Sally Smith Hughes, PhD
1987

With Introductions by
Crowell Beard, MD
Chandler R. Dawson, MD
Daniel G. Vaughan, MD

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Introductions by Crowell Beard, MD, Chandler R. Dawson, MD, and
Daniel G. Vaughan, MD

Interviewed 1986-87 by Sally Smith Hughes, PhD

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PREFACE

Ophthalmology Oral History Series

American ophthalmology has undergone striking changes since World War II, not only in terms of basic science, diagnosis, and therapy, but also in terms of its internal organization and relationship with the rest of medicine and with the federal and state governments. Aware of the need to document these changes, the Foundation of the American Academy of Ophthalmology sought a means to preserve the memories, experiences, and insights of individuals who had lived through them.

The result was the inauguration in 1986 of the Ophthalmology Oral History Series, an ongoing series of in-depth interviews with senior ophthalmologists and others who have made significant contributions to the specialty. Aside from providing enjoyment and inspiration, the series' intent is to preserve a fund of historical information which might otherwise be lost and to give ophthalmologists a sense of their discipline's heritage.

In January 1986, an Oral Histories Committee, consisting of William H. Spencer, MD, (chairman), Stanley M. Truhlsen, MD, Susan E. Cronenwett, Patricia I. Meagher, and David J. Noonan, was formed to facilitate collection of the oral histories. A selection subcommittee, with an anonymous membership of three senior ophthalmologists, was appointed to select individuals to be interviewed from nominations by the Foundation Board of Trustees and the Academy Board of Directors.

In selecting individuals to be interviewed, the subcommittee considers the individual's age, prominence in and contributions to ophthalmology, and ability and motivation to participate in the project. As the series expands, an effort will be made to select interviewees from different areas of the country and with different subspecialty interests. Regional subcommittees provide information concerning the local ophthalmologists to be interviewed and assist in fund raising for the oral history series.

Production of the oral histories is carried out by the Regional Oral History Office of the University of California at Berkeley. Sally Smith Hughes, PhD, a medical historian with the Regional Oral History Office, conducts the research, interviewing, and editing, and collaborates with Foundation personnel in final production of the oral history volumes. Willa K. Baum, director of the Regional Oral History Office, serves as consultant. For over thirty years the Regional Oral History Office has conducted interviews with West Coast leaders in all walks of life and is pleased to have the opportunity to expand nationally to document the history of American ophthalmology.

An oral history memoir is a recorded and transcribed series of interviews designed to preserve the recollections, knowledge, and reactions of a person who has played a significant role in or observed important events. It represents an important way to preserve information and opinions that the narrator alone is able to provide. The transcriptions are edited, reviewed by the narrator, retyped, indexed, and bound with photographs and illustrative material, and placed in appropriate research libraries.

The finished product is both a record of a conversation and a primary research source. It should not be regarded as having the polish and finality of a published book. It is not intended to present the final, verified, and complete account of events. Rather, it reflects the narrator's view, sometimes recounted with partisanship and passion, sometimes with impartiality and objectivity, but always vivid, immediate, and irreplaceable.

Oral history in one sense is an informal art, one that relies on the give and take between two individuals holding a directed conversation. Thus the reader should not expect a studied, impersonal, and invariably exhaustive discourse in the pages that follow. Instead, good oral history offers a view of the narrator and his opinions up close, expressed with the immediacy, appeal, and occasional errors of everyday speech.

The interviews, which are entirely supported by private contributions, are meant for a wide audience. Although the focus is ophthalmology, the goal is to produce documents of broad historical interest through full, referenced, multidimensional biographies.

Indexed and bound transcripts of the interviews are available to readers at the Foundation of the American Academy of Ophthalmology, the Bancroft Library, the National Library of Medicine, and other medical and manuscript libraries. The interview tapes and supplementary material relevant to each interview are on deposit at the Foundation. Oral history volumes may be ordered from the Foundation.

Sally Smith Hughes, PhD
Interviewer-Editor
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William H. Spencer, MD
Chairman
Oral Histories Committee
The Foundation of the
American Academy of
Ophthalmology

March 1988

INTRODUCTION

Crowell Beard, MD

I started my training in ophthalmology in 1938. At that time Dr. Phillips Thygeson's name was already well known in the field of external eye diseases. He had graduated from Stanford Medical School and had taken his internship and graduate training at the University of Colorado. He had been on the faculty at the University of Iowa Medical School and had spent many months studying infectious eye diseases in North Africa and on American Indian reservations, much of it with Dr. Francis I. Proctor of Boston. He had then accepted a professorship at the Columbia University Medical School in New York City. There his research had been continuous and profound, even to the point where he inoculated his own conjunctiva with the inclusion blennorrhoea virus to prove its pathogenicity. Even at that time his bibliography was extensive.

Dr. Thygeson had entered the U.S. Army early in World War II as a major in the Medical Corps. He had been assigned to the Army Air Corps and had served in Clearwater, Florida, until his transfer back to the Army Medical Corps as chief of ophthalmology at Valley Forge General Hospital. He had been promoted to the rank of lieutenant colonel.

It was with great pleasure that I learned in 1945 that Dr. Thygeson was being transferred as chief of ophthalmology to Dibble General Hospital in Menlo Park, California, where I had been on duty as an ophthalmologist. This hospital specialized in ophthalmological and plastic surgery cases.

All of us in the ophthalmology department at Dibble were pleased that we would be headed by a medical officer of Dr. Thygeson's caliber, and we knew we would benefit from an association with such a legend in his field. Until Dr. Thygeson's separation from the service in June, 1946, we held group seminars frequently. These meetings, which really amounted to graduate training, were an added bonus to the experience many of us were receiving while assigned to specialty hospitals in the service. Dr. Thygeson's reputation was well established in general ophthalmology and especially in external diseases. Although he was not particularly fond of eye surgery, he demonstrated his surgical capabilities in his work on eye injury cases, which were more prevalent in our wards than infections.

Dr. Proctor had passed away leaving a rather large estate in Boston, a wealth that had made it possible for him during his lifetime to travel, to study, and to become an expert in the field of external diseases. His widow had moved to Santa Fe, New Mexico, and had

decided to endow a foundation for research in ophthalmology. She put an adequate sum of money for this project at the disposal of Dr. Proctor's friend and research associate, Dr. Phillips Thygeson.

Much thought and effort went into the planning of the Francis I. Proctor Foundation during the months before and after Dr. Thygeson's separation from the military service (as a colonel). Dr. Thygeson and Mrs. Proctor had decided to establish the Proctor Foundation in San Francisco. He was planning to open a private office in Monterey, some 120 miles to the south, and to spend about half his time in each locale. During the army time that we spent together, Dr. Thygeson and I had become well acquainted. I had decided that upon my separation from the service I would open an office in San Jose rather than return to San Francisco. It did not take much effort to convince Phil that it would be more efficient for him to join forces with me in San Jose for our private practices, a decision I believe he has not regretted.

Dr. Thygeson's international reputation merited him appointment to the American Board of Ophthalmology—one of American ophthalmology's greatest honors. It took much time, which he gladly gave.

Our practice thrived. We took Dr. Robert Cook and Dr. Daniel Vaughan into the practice within the first five years. We were forced to move to larger, and even larger, quarters. During the early years, because of his fondness for children, Dr. Thygeson consented to do the strabismus surgery, while I, because of my fondness for surgery in general, consented to do everything else. We assisted each other at all ophthalmology operations, an arrangement which continued even after I left the group in 1957 and until Dr. Thygeson left San Jose for San Francisco in 1959.

Our friendship has continued throughout the ensuing years and continues to do so in spite of our diverging pathways. Our continued association at the university, socially, and recreationally at the Thygeson Lake Tahoe home has many happy memories.

August, 1988

INTRODUCTION

Chandler R. Dawson, MD

Dr. Phillips Thygeson has been one of the leading figures in ophthalmology since the 1930s. Dr. Thygeson's laboratory and clinical research studies on trachoma, other ocular infections, and inflammatory diseases of the eye laid the groundwork for much of the current thinking in these fields today.

Dr. Thygeson was one of the outstanding clinicians of his generation who combined a deep concern for the well-being of his patients with superb diagnostic abilities. He was also a great clinical teacher and conveyed his knowledge through his lectures and articles to more than 100 postdoctoral fellows at the Proctor Foundation and to a wider range of ophthalmologists. His conservative point of view about the over-enthusiastic application of new treatments was often unappreciated at the time but, as in the case of steroids, it came to be greatly respected throughout ophthalmology.

Dr. Thygeson's most enduring accomplishment was the establishment of the Francis I. Proctor Foundation for Research in Ophthalmology at the University of California, San Francisco. Dr. Thygeson came to know Dr. and Mrs. Proctor when he was invited by them to visit Santa Fe, New Mexico, to do research on the trachoma then endemic in American Indians. Following Dr. Proctor's death in 1936, Dr. Thygeson was named a committee member to distribute grants-in-aid for ophthalmic research from a trust fund established in Dr. Proctor's will. In 1947, with Dr. Thygeson's guidance, the Proctor Foundation was established at the University of California, San Francisco, under an agreement between Mrs. Proctor and the Regents of the University of California. Dr. Thygeson persuaded Dr. Michael Hogan to become the first foundation director in 1947, and in 1959, Dr. Thygeson himself took over the reins of the directorship. Since his retirement in 1970, Dr. Thygeson has continued to play an active role in the affairs of the Foundation, and still actively participates as a member of the of the Foundation's Board of Governors. The Proctor Foundation today follows the course that Dr. Thygeson set for it, continuing to carry out research in ocular infections and inflammatory eye diseases, and it is still a major center for research on trachoma.

One of Dr. Thygeson's key accomplishments at the Proctor Foundation was the identification with Dr. Ernest Jawetz of type 8 adenovirus as the causative agent of epidemic keratitis. Drs. Thygeson and Jawetz went on to conduct a series of landmark studies on the pathogenesis, clinical course, and treatment of trachoma and other ocular

chlamydial infections. With Dr. Michael Hogan and Dr. Samuel Kimura, Dr. Thygeson also established the Uveitis Clinic which continues to be a major center for training and research in uveitis at the Proctor Foundation.

Phillips and Ruth Lee Thygeson are very much a team. Mrs. Thygeson edited all of Dr. Thygeson's papers and guided many of the fellows and young faculty at the Proctor Foundation in their first publications. The Thygesons frequently entertained prominent ophthalmologists and others at their home in Los Altos, which provided an informal setting for the Proctor fellows to meet with these visiting dignitaries.

Dr. Thygeson has been a leading figure in the development of ophthalmology in the twentieth century. Through the research and training activities of the Proctor Foundation, his ideas will continue to spread into the twenty-first century as well.

August, 1988

INTRODUCTION

Daniel G. Vaughan, MD

In 1946 I was a barely qualified Navy doctor suddenly appointed ophthalmologist to the First Marine Division in Tienstin, North China. It wasn't long before I went looking for help, which I found in the person of Dr. Marie Feng, a luminous eastern star of our profession and a graduate of the famed Peking Union Medical College, established by the Rockefeller Foundation in 1921. The most interesting thing about me in Dr. Feng's estimation, it turned out, was that I was from the United States, where "...you must know Dr. Thygeson!" Of course I had never heard of Dr. Thygeson or anyone else in the field of ophthalmology, though I don't remember saying so at the time.

As it happened, when I started my training in ophthalmology at UCSF in 1948, Phillips Thygeson was one of the first staff members I met. We have been student and teacher, professional colleagues, and good friends since that time.

There are many good doctors who take care of their patients and do some teaching and writing along the way, and then retire. The difference between those physicians and a man of Phillips Thygeson's stature is more than one of degree: He not only knew more and did more; he extended his influence throughout the world over a vast acquaintanceship among notable international ophthalmologists specializing in external eye diseases. He taught in the UCSF eye clinic two days a week, and his ministrations were often interrupted by transoceanic calls from professors around the world needing consultation services pertaining to complex diagnostic or therapeutic problems in this difficult specialty.

I was present when a staff member asked Dr. Thygeson how he was progressing with his long-awaited book on external eye disease. With obvious satisfaction he replied that his publisher had given him an extension until June of 1949. Of course that book has never been written, but since that time Dr. Thygeson has published more than 200 papers and numerous chapters and monographs, as well as conducted a long series of weekly seminars for the Proctor Fellows. Like many medical authors, he tends to avert his gaze from manuscripts in progress that have avoidable deadlines. Luckily for the record of his years, however, he married one of America's finest medical editors. Ruth Lee Thygeson deserves our thanks for converting Phil's rough drafts into models of medical exposition for us to enjoy.

In 1951, Phil Thygeson, Crowell Beard, and Bob Cook invited me to join them in private practice in San Jose. I was expecting a call to the colors in Korea, but the call never came, and I spent the fifties learning my trade from these fine doctors and surgeons.

Dr. Thygeson retired from private practice in 1959, but we continued to pursue our interests in teaching and research. I have never known a man with such a compelling need to share what he knows and what he can give. His major interest continues to be the Proctor Foundation—a name he has made at least as well known today in Tianjin as the name Thygeson was known in 1946 in Tientsin.

August, 1988

INTERVIEW HISTORY

Sally S. Hughes, PhD

Phillips Thygeson is one of the two ophthalmologists selected to inaugurate an ongoing series of oral history interviews with prominent individuals in American ophthalmology. The Foundation of the American Academy of Ophthalmology, sponsor of the series, chose Dr. Thygeson because of his contributions to the field of external eye disease and his central role in the organization and direction of the Francis I. Proctor Foundation for Research in Ophthalmology at the University of California, San Francisco (UCSF).

The interviews begin with Dr. Thygeson's account of his roots in the Norwegian immigrant community of the American Midwest. He tells of a comfortable upbringing in St. Paul, Minnesota, where his father practiced law and his mother was an ardent suffragette and advocate of Margaret Sanger's birth control movement.

Dr. Thygeson describes the shift in family fortune in 1917 when his father's ill health brought the family to Palo Alto, California, where Mr. Thygeson died within a year, leaving his widow with the care of four high school and college age children. All four went to Stanford, where as an undergraduate young Phil made two crucial decisions: to apply to Stanford Medical School and to marry his blond, blue-eyed classmate, Ruth Lee Spilman. Their marriage in 1925 marked the beginning of a personal and professional partnership lasting to this day.

Describing his years at medical school, Dr. Thygeson tells of his early infatuation with microbiology and his first brush with ophthalmology as a patient of Hans Barkan. But it was not until his internship at the University of Colorado that the ophthalmologist William Finnoff convinced him that ophthalmology was a fruitful field in which to pursue his interest in microbiology. Edward Jackson, professor emeritus at Colorado and a leading light of American ophthalmology, taught Dr. Thygeson perimetry and served as unofficial consultant throughout the young man's preceptorship in Dr. Finnoff's private practice.

In Colorado Dr. Thygeson began what was to become a career of research on trachoma, a disease of unknown etiology which was rampant among the American Indians in the days before the sulfa drugs. One of his first papers, published with Dr. Finnoff in 1931, disputed Hideyo Noguchi's claim of a bacterial etiology for trachoma and prompted Dr. Thygeson's acquaintance with Dr. Francis Proctor, a retired eye surgeon studying trachoma in the Indians of the Southwest. The relationship flourished, and in 1929 Dr. Proctor invited Dr. Thygeson to accompany him and his wife to the Giza Memorial Ophthalmic Institute in Egypt to continue laboratory work on trachoma.

From Egypt the interviews shift to the Institute Pasteur in Tunis to which Dr. Thygeson was attracted by the work of its director, the Nobel laureate, Charles Nicolle, who had reported the filterability of the agent of trachoma. "That was where," Dr. Thygeson commented in the interviews, "I became convinced that the inclusion body was the cause of trachoma."

Upon returning to the United States in 1931, Dr. Thygeson accepted C.S. O'Brien's offer of an assistant professorship at Iowa. In 1936 he was invited to Columbia where within three years he advanced to the rank of full professor and co-director with Jack Dunnington of the Institute of Ophthalmology at Columbia Presbyterian Hospital.

Soon after Pearl Harbor Dr. Thygeson applied for a commission in the Army Air Corps, and for the duration of the war he gained invaluable clinical experience in military hospitals in Florida, Pennsylvania, and California. Deciding at the conclusion of the war to remain in California, he established a private practice in San Jose with Dr. Crowell Beard, an ophthalmic surgeon. He also began negotiations to establish a research institute for external eye diseases based on a bequest by Dr. Proctor, who had died in 1936. On September 15, 1947, the Francis I. Proctor Foundation for Research in Ophthalmology was established at UCSF by joint action of Mrs. Proctor and the Regents of the University of California.

Based on his intimate association with the Proctor Foundation, first as a founder and staff member, then as its second director (1959-1970), finally as a trustee, Dr. Thygeson describes the Proctor's institutional growth, its research and training programs, its triumphs and tribulations. He speaks with particular pleasure of the microbiologist, Mas Okumoto, "the backbone of the Proctor," and the Proctor fellows, now numbering about 175.

One of many strengths of the oral history is the detailed discussion of specific research projects. Perhaps most notable is the account of the dramatic experiment in 1936 with a human volunteer which definitively established the filterable nature of the trachoma agent, only later learned to be a chlamydia. Dr. Thygeson remarked laconically: "We got a medal as a result of that. That's why I began to rise from an associate to a full professor." The medal in question was the Research Medal of the AMA's Section on Ophthalmology, at that time a major force in American ophthalmology.

Oral History Process

Seven interviews of varying length were conducted between November 1986 and February 1987 at the Thygesons' hilltop home in Los Altos, California. The interviews were usually conducted on the patio overlooking the five acres of flower garden and orchard, sometimes in the living room, one arm of which contains the Thygesons' face-to-face desks and part of an extensive medical history library.

Before each interview Dr. Thygeson received an outline of the topics suggested for discussion. He spoke expansively and with remarkable recall, answering questions fully and frankly. Although retired for many years, the fact that he remains closely associated

with the Proctor Foundation and its research programs may partially account for his ready and complete answers. Eighty-three when the interviews began, he was still commuting once a week to San Francisco to conduct a seminar for Proctor staff and fellows. Although he has since had to give up the seminar, he has once more leapt into the fray to counter the current attempt at UCSF to appropriate the endowed space of his beloved Proctor Foundation.

Fittingly for one who has always shared in practically every aspect of her husband's life, Mrs. Thygeson was invariably present during the interviews. Sometimes she participated actively, more often she worked quietly at her nearby desk. Over lunch, which she provided on every occasion, she contributed vivaciously to the easy flow of conversation about individuals she and Dr. Thygeson have known over a professional lifetime in ophthalmology.

In the interviews, Dr. Thygeson relates with pride that Mrs. Thygeson edited every medical paper he ever published—to date more than over 200—and continues to serve as editor for the Proctor Foundation. He put it simply: "Without her cooperation I would have been in trouble."

The interviews are at times revealing of a man known for his strong loyalties, firm beliefs, and frank statements. Relaxed in his home of many decades and enjoying the review of his life, the tenor of Dr. Thygeson's discussion is perhaps more tempered than his opponents might themselves have experienced. It is in regard to personalities associated with the Proctor Foundation and the indiscriminate use of steroids in ophthalmology that the interviews are most revealing of his forceful opinions.

The interviews were lightly edited and then sent to Dr. Thygeson, who edited conscientiously, refusing to soften any of his remarks. Seldom one to brook compromise, he wished his views to stand as originally expressed.

Thus the oral history represents one man's frank and personal account of a career in ophthalmology now in its fifty-eighth year. The history of the Proctor Foundation is told with the partisanship of its most devoted supporter. Yet behind the partisan viewpoint—how could it be otherwise?—the reader glimpses a man of towering integrity and force, whose contributions, through research, teaching, and the research foundation which he took such a vital hand in creating and directing, have left an indelible mark on ophthalmology.

July, 1988

Regional Oral History Office
Room 486 The Bancroft Library

University of California
Berkeley, California 94720

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Phillips Thygeson
 Date of birth MARCH 28, 1903 Birthplace ST. PAUL, MINNESOTA
 Father's full name Nels Marcus Thygeson
 Occupation Attorney Birthplace Wisconsin
 Mother's full name Sylvie Grace Thompson Thygeson
 Occupation Housewife ^{Legal secretary} Birthplace Elgin, Illinois
 Your spouse Ruth Lee Spilman Thygeson
 Your children Fritjof (son) Kristin (daughter)

Where did you grow up? MINNESOTA TO AGE 13 THEN CALIFORNIA
 Present community Los Altos, CALIFORNIA
 Education STANFORD A.B. (1925) MD 1928
M.S. Colorado (1933)
 Occupation(s) Physician, ophthalmologist, professor

Areas of expertise ophthalmology - microbiology

Other interests or activities MUSIC, PHOTOGRAPHY,
MOUNTAINS, GARDENS

Organizations in which you are active Retired - volunteer
Proctor Fdn, UCSF



Dr. Thygeson being interviewed by
Dr. Hughes in his home,
Los Altos, California, 1987

I. FAMILY BACKGROUND AND EDUCATION

[Interview 1: November 21, 1986] ##

Grandparents and Parents

Thygeson: My grandfather, Elling Thygeson, on my father's side was born in Norway and came over with his brother in the 1840s, I believe. He was from Bergen, Norway. [brief interruption]

His brother went to North Dakota, and we lost track of him. Our grandfather settled in Wisconsin on a farm near a small Norwegian community where no English was spoken. It was all Norwegian.

My father, Nels Marcus Thygeson, did not know any English until he was ten years old. Nobody spoke any English during his childhood. Then he went to school in River Falls, Wisconsin, and later went on to the University of Wisconsin. He graduated in geology first and joined the U.S. Geodetic Survey and then decided he didn't like the nomadic life and came back and went through law school at the University of Wisconsin. He then became an attorney, in St. Paul first, where he was district attorney for Ramsay County, and then he went into practice in Minneapolis.

Hughes: *Did he come from well-educated stock?*

Thygeson: My grandfather was of farm stock, peasant stock, in Norway, so he was not well educated. My grandmother, Mary Nelson Thygeson was. It was her influence that made my father go through the university.

Hughes: *Interesting that a woman would be well educated in those days.*

This symbol indicates that a tape or a segment of a tape has begun or ended. For a guide to the tapes see page 296.

Thygeson: She was a Nelson, and there's a very large group of Nelsons and Leutholds in Minnesota and Wisconsin, all her relatives. But grandfather's relatives we've completely lost except for his daughter and two sons. We don't have any contact. But we do have with my grandmother's relatives. I never knew her, but I have a picture of her when I was about three years old, taken out at Lake Minnetonka, and so I know what she looked like. But she died while I was still young so I never got to see her.

Grandfather always came up for the Minnesota State Fair. He loved the Minnesota State Fair, so every year he came up from Wisconsin, and he brought each of us a five-dollar gold piece. But we couldn't talk to him because he knew no English.

Mrs.

Thygeson: Do you mind? I'm just going to sit here.

Thygeson: My father became a specialist in railroad taxation and a consultant for a number of the railroads, including the Chicago, Minneapolis, and St. Paul Railroad Company. So he ended up his life as a corporation lawyer.

Hughes: *Do you know how he happened to choose that line of law?*

Thygeson: No. I know that he didn't like geology as a way of life.

Mrs.

Thygeson: The reason he didn't like geology, according to his wife, whom I knew very well and who was Phil's mother, was that it took him away from home. He was a very, very, very child-oriented, family-oriented man. I never knew him. He died before I came along. But I want to tell you one thing, too, just one little thing. When I first knew Phil, he wished he was all Norwegian. He told me that more than once.

Thygeson: Well, I was Norwegian oriented—

Mrs.

Thygeson: Terrifically, just terrifically. He just wished his mother, Sylvie Grace Thompson, were Norwegian, too.

Hughes: *She was of old colonial stock, was she not?*

Mrs.

Thygeson: Yes, and proud of that. But he wanted to be Norwegian to the core.

Hughes: *Well, tell me a bit about her.*

Thygeson: Well, she was a very healthy woman. She never got sick at any time.

Mrs.

Thygeson: Lived to be a hundred and seven. Never ill one day.

Thygeson: She was very supportive of us as far as education was concerned, but she was difficult to get along with. She was rough on the kids, in contrast to my father who was tenderhearted with children.

Hughes: She was quite a disciplinarian?

Thygeson: Yes, and if we didn't do what she wanted us to do, she really let us know about it.

Hughes: She said in the interviews that she had somebody in the house while she was off on her suffragette and birth control activities.*

Thygeson: Relatively speaking, we were quite affluent, and we had at least two servants all the time.

Mrs.

Thygeson: Those were the days, dear, don't forget.

Thygeson: So we had this wonderful nurse, a registered nurse, Miss Anna Larsen, who stayed with the family, and we knew her a lot better than we knew our mother.

Hughes: Your mother was out of the house quite a bit?

Thygeson: Either that or on the telephone.

She was president of the Women's Club of St. Paul. She was into all the church activities, the Unitarian Church.

Mrs.

Thygeson: She knew Margaret Sanger, and trotted her around in the car when she came to talk. She knew all these people.

Hughes: How did she develop these progressive viewpoints?

Thygeson: Well, the whole Thompson family was on the left side of center and very active. My uncle, Elmer Thompson, ran for mayor of New York City on the socialist ticket, so on her side of the family we were very active politically, but not on my father's side.

Hughes: What did your father think of his wife's activities?

Thygeson: Well, he tolerated them. He didn't object.

Mrs.

Thygeson: He was just a wonderful family man as far as I can make out.

Thygeson: He was very wonderful with the children; he was just the ideal father.

* Sylvie Grace Thompson Thygeson. "In the Parlor." *The Suffragists: From Tea-Parties to Prison*, an oral history conducted in 1972. Suffragists Oral History Project, Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1975.

Hughes: Were you aware as a child that your mother was involved with the birth control movement?

Thygeson: Oh, yes, because we had all these visitors in the house, and we heard the telephone calls and saw the literature. So we knew all about that.

Hughes: That was never a source of embarrassment to you?

Thygeson: No, it wasn't a source of embarrassment. It simply meant that we were not as close to her as ordinarily children are to their mother. There was a little wall in between because we didn't see too much of her. But she was very supportive for anything in the educational line, and she liked our farm out on Lake Minnetonka and all of those things it had, and all the boating. So she had her good points, but she was difficult.

Mrs.

Thygeson: When she had very much less money after her husband died and things were pretty tight, she didn't hesitate for one minute to borrow from Stanford University for tuition for these kids. She hated debt more than anything else in the world. I gave her a party when we cleared up all our mortgages. She loved that.

Thygeson: She read very widely. We had a very good library.

Mrs.

Thygeson: Usually about six books at a time.

Hughes: What was her education?

Mrs.

Thygeson: She taught school at fourteen and sixteen.

Thygeson: She was a law stenographer, at the time that my father married her, for her uncle, who was an attorney and judge in St. Louis. She did briefs and political things. But she didn't like to have me go out with stenographers!

Hughes: Why was that?

Thygeson: She had a prejudice against stenographers.

Mrs.

Thygeson: She referred to me as a stenographer. We've only lasted sixty- two years, that's all.

Hughes: Was that an initial barrier?

Mrs.

Thygeson: No.

Hughes: She liked you from the start?

Mrs.

Thygeson: Oh, I think when she actually met me.

Thygeson: But as a class, she didn't like stenographers. She thought they were pretty fast women.

Hughes: What was your relationship in California after your father died?

Thygeson: Well, there was a complete change in economic status. We were four children with an income of about \$200 a month. So everybody had to get out and work. Previously the children in the family didn't work. We all worked weekends and after school, so it was a complete change.

Hughes: Were you helping to support her as well as your education?

Ham Radio

Thygeson: No. All I did was to earn enough money to buy clothes and books. I had a hobby, which was ham radio. I had to feed that hobby a little bit, so I had to earn money.

Hughes: When did you become interested in radio?

Thygeson: I got my first license in 1916 in Minnesota, for radio station 9JY. My interest in radio was very much supported by my father.

Mrs.

Thygeson: You were only thirteen years old.

Thygeson: But then we had to tear down our set when the war came along. Then I reactivated it out here in Palo Alto in 1918, and 6BU was my call letter. That was a big interest, and I had to earn money to keep it going because it was fairly expensive to run. You had to buy vacuum tubes and equipment.

Hughes: Why did you have to tear it down during the war?

Thygeson: For military reasons. All amateur radio was stopped. They thought spies would use it, which they didn't, of course.

Hughes: Do you think that your early interest in radio had anything to do with your decision to go into engineering?

Thygeson: Oh, yes, because I was going to be an electrical engineer. I worked summers for power companies, first San Joaquin Light and Power Company and later PG&E. So I did all the preliminary work for engineering and surveying. In the first year at Stanford I planned to be an electrical engineer. All my high school work was oriented towards electricity and engineering.

High School

Hughes: How were you doing in high school, academically and socially?

Thygeson: Well, I had five years in high school. The first year was at a private school in St. Paul, the St. Paul Academy. We came to California in April, 1917, and so I didn't finish the year. Palo Alto High School wouldn't accept that incomplete year's work, and so I had to start over again as a freshman. I really had five years of high school.

They were wonderful years because Palo Alto High School was the ideal high school. The students were children of the faculty at Stanford. Everybody was interesting and working. Everybody had a program. It was just the opposite of the St. Paul private school filled with rich kids who cared nothing for studies. It was a complete change. Those four years of high school here in Palo Alto were wonderful.

The Move to California, 1917

Hughes: Let's backtrack to the decision to come to California, which was in 1917. Was that strictly because of your father's health?

Thygeson: Yes. He had a cancer of the pancreas which was operated on at the Mayo Clinic. Then we came to California for his recuperation. He survived the first operation for a year, during which he was able to work. Then he had a recurrence which was inoperable. He decided to come to California, and particularly to Stanford, because he knew David Starr Jordan, who apparently was an old friend. David Starr Jordan was the chancellor of the university.

We rented Professor Burlingame's house, a duplex. Professor Burlingame was a professor of botany. The other half of the duplex was occupied by the Hoover family. Herbert Hoover, Sr., was in Belgium with the Belgian Relief Commission. We had an interesting summer.

Then my father died that summer, and my mother decided to stay here in California. She went back to St. Paul and sold the house and contents and came here and bought a very lovely house in Palo Alto for, I think, \$4,000.

Hughes: How do you suppose your father knew David Starr Jordan?

Thygeson: There was some tie-in at the University of Wisconsin.

Hughes: He was looking for a place to settle his family for the summer and called upon Dr. Jordan?

Thygeson: Well, we thought there would still be a convalescence. We didn't know that he was going to die. I guess he knew, but we didn't know.

Brother and Sisters

Hughes: Tell me about your brother and sisters.

Thygeson: I had an older brother [Elling Thygeson] and an older sister [Ruth A. Thygeson] and a younger sister [Mary Thygeson Shepardson]. My older sister was a real student—Phi Beta Kappa— and she went into medicine, graduated in medicine. My older brother was a spoiled—you might call him a rich kid because he lived in the affluent time of our family life. He was a freshman at the University of Minnesota at a time when the family had plenty of money. He wasn't interested in studies. He was more interested in poker and sports.

Hughes: What did your mother think of that?

Thygeson: Well, she didn't like it, and father didn't like it. But they couldn't do anything about it. So it was quite a shock to my brother to come out to California and have this drop in income. He had to get out and work, which was a little hard on him. It wasn't hard on me because I hadn't been spoiled. So he started in medicine at Stanford, but his poker and fraternity life interfered with his medical school training. He dropped out, and later he came back and went into engineering. He ended up as a petroleum engineer down in southern California.

Mrs.

Thygeson: Very capable, too, but difficult. Sometimes a very unpleasant character.

Thygeson: He wasn't a student, but my elder sister and my younger sister were real Phi Beta Kappas.

Hughes: Where do you suppose this medical leaning came from? That's three siblings.

Thygeson: We had a brother-in-law who was in medicine, Dwight E. Shepardson. Maybe we got it from him.

Mrs.

Thygeson: Elling flunked out.

Thygeson: I changed over to medicine for the reasons that I liked zoology so much, and I liked Professor Heath. I think it was through Professor Harold Heath that I decided engineering was not my line.

Mrs.

Thygeson: There was a little something about mathematics?

Thygeson: Well, I skipped the sixth grade, and I never caught up in mathematics again.

Mrs.

Thygeson: That's been his excuse all his life. I tried to tell him that sixth grade could have been covered in a month.

Thygeson: Skipping the sixth grade was all right except for mathematics. Mathematics became my most difficult subject.

Hughes: Had you taken zoology just in the course of things at Stanford?

Thygeson: Yes. It was a customary premedical course anyway.

Mrs.

Thygeson: It was a lower division requirement.

Hughes: Yes, because you weren't initially premed.

Thygeson: Anyway, there was this wonderful Professor Heath. He was just a superb teacher. He wasn't interested in medicine particularly, but anyway, he pushed me towards biology.

Family Life

Hughes: Was there anybody outside the family that was particularly influential?

Thygeson: I can't think of anyone except Lala. She was very influential in everything we did.

Mrs.

Thygeson: She was a graduate nurse; she trained at the best hospital in St. Paul.

Thygeson: She was unusual because she was half Norwegian, half Swedish, and so she could talk to my father, where my mother couldn't. He wouldn't let us learn either Norwegian or Swedish because he wanted us to be one hundred percent American. He didn't want any hyphenated Americans in his family.

He had that ten years without any English, and he had to learn English starting at age ten, so he didn't want a second language to interfere with English.

Hughes: Is there anything more you would like to say about your childhood?

Thygeson: I think that a wonderful thing about my childhood was the Indian lore and the boy scouts. We started our own boy scout troop in St. Paul, and we recruited our own scout master and entered into all the scout activities. All the camping and outdoor activities appealed to me very much, so I had a wonderful time scouting and continued scout activities out here in California for a year or so. Then ham radio took over. But I had a wonderful time with scouting, and especially since we had this lovely farm on Lake Minnetonka in Minnesota which was ideal for all scout activities, with the woods and the lake and all the plants and wildlife. Lake Minnetonka was important in Indian history, Sioux and Chippewa.

Hughes: Was that your grandfather's farm?

Thygeson: No, that was my father and my mother's.

Hughes: What did they farm?

Thygeson: It was a summer home really, but it had been a farm. We farmed everything, but we didn't live off the farm. We had all the known apples and berries, plus the usual vegetables.

Mrs.

Thygeson: It took practically all day to get out there. Now it's part of Minneapolis, isn't it?

Thygeson: Yes, a suburb of Minneapolis now. But we had all the vegetables, and the corn, and the potatoes, and all varieties of berries.

Mrs.

Thygeson: Berries, and berries, and berries.

Thygeson: And all the flowers.

Mrs.

Thygeson: You were right on the lake.

Thygeson: We had a mile of shoreline on the lake. It was a beautiful lake with many islands.

Mrs.

Thygeson: A hill going down to the lake.

Hughes: And that's where the Indians used to come?

Mrs.

Thygeson: Yes, right along the shores of that lake.

Thygeson: There was a Sioux Indian family from the Shakopee Reservation on the Minnesota River that came every summer and camped on our land. They didn't ask permission, but their ancestors had always camped there. So we got very well acquainted with the Indian children. We liked the Indians. They loved our raspberries. So we would give them raspberries, and they would give us these little artifacts that they made—birchbark canoes, and little tomahawks, and all those things. So we had a great time with the Indians. And, of course, I've carried through with my interest in the Indians every since.

Mrs.

Thygeson: Did you show Sally your museum?*

Thygeson: Yes.

* A room in the Thygesons' home in Los Altos contains American Indian artifacts and books about the Indians.

Stanford University

Undergraduate, 1921-1925

Hughes: Did you ever consider going anywhere other than Stanford?

Thygeson: No. Stanford was right next door, and financial reasons prevented us from going to any expensive place. Tuition was forty dollars a quarter at that time.

Mrs.

Thygeson: The Stanfords wanted it to be a poor man's school. The reason they limited the women was that Stanford had been planned as a poor boys' school.

Thygeson: At the time I went, there were two thousand men and five hundred women.

Mrs.

Thygeson: We didn't mind at all.

Thygeson: Another thing, the admission requirements for medicine were practically nonexistent. I had good grades in high school, but I didn't have all "A"s. It never occurred to me that I would have any trouble getting into medical school; I just signed up for medical school.

Hughes: You made that decision when you became interested in zoology?

Thygeson: Yes.

Hughes: When was that?

Thygeson: My sophomore year.

Hughes: What was the academic reputation of Stanford in those days?

Thygeson: Well, we thought it was very good, and we used to look down on the University of California.

Mrs.

Thygeson: We usually called Harvard the Stanford of the East.

Thygeson: Stanford had a real complex at that time and still has. But it was very strong in those days. Stanford really looked down on Berkeley.

Hughes: You mentioned Professor Heath. Was there anybody else on the faculty that you had a particular association with as an undergraduate?

Medical School, 1925-1928

Thygeson: Well, in premedicine it was really all Professor Heath, but in the early medicine, which was also at Stanford, a year and a half before we went to the city [San Francisco], we had a marvelous faculty.*

The best one was Edwin Schultz, who was professor of bacteriology. He was the one who influenced me to go into infectious disease. He was really a very poor lecturer but an absolutely marvelous seminar man. He stimulated me.

Hughes: *Did you do any laboratory work in microbiology?*

Thygeson: Well, we had to write a thesis, so what I chose was the public health status of Palo Alto. I made a survey of the city and checked all the dairies and restaurants, and I wrote a thesis, which I guess is on file at the Stanford library.

Hughes: *Was that a requirement for every medical student?*

Thygeson: Yes. You had to write two theses, one preclinical and one clinical. So when I got to the city I wrote another one on allergy—hay fever and asthma.

Mrs.

Thygeson: Allergy was a new word.

Thygeson: Allergy was just starting then as a science.

Hughes: *How did you become interested?*

Thygeson: On account of Ruth Lee who had hay fever.

Hughes: *Was that laboratory research?*

Thygeson: It was clinical research. What I did was to examine and treat a series of asthma and hay fever patients with new procedures at that time. One was pollen desensitization. It had just come in. You could moderate the hay fever by an injection of pollen if you knew what the pollen sensitivity was.

Hughes: *Who developed that technique?*

Thygeson: Well, there were several pioneers, but they were not in the San Francisco area. There was one in Philadelphia and a couple in New York, so that the science really started in the East and then spread out West here.

Hughes: *And you learned of these procedures through reading?*

Thygeson: Reading. There was one clinical allergist in San Francisco, and I've forgotten his name. He was on the medical staff at Stanford. I worked a bit with him. He provided some patients for me to work on. So it was clinical research for

* Before Stanford Medical School moved from San Francisco to Palo Alto in 1959, the preclinical courses were taught in Palo Alto and the clinical in San Francisco.

the professor. The science of allergy was not well thought of at that time, and when I had to defend my thesis, the new professor of medicine gave me a bad time. He thought allergy was verging on charlatanism.

Hughes: Who was that?

Thygeson: Professor Arthur Bloomfield. He let me by all right, but he thought I had a terrible subject.

Hughes: Did you ever consider specializing in allergy?

Thygeson: No. But I did consider internal medicine as a specialty.

Hughes: You were still setting your sights on microbiology?

Thygeson: No, I really came into microbiology a little bit later.

Hughes: Aside from Schultz, was there anybody on the medical faculty that either had an influence on you or was of particular note?

Thygeson: Well, there was a Professor McFarland at the university who was a histology professor and a very good teacher and very stimulating. There was also a professor of physiology who specialized in ocular physiology. I lost his name, but he was internationally known. Then there was also a professor of physics, who was internationally known, named David Webster.

##

Mrs.

Thygeson: I didn't like his teaching, but I liked him personally.

Thygeson: Well, I liked him personally, but as a teacher he was terrible.

Mrs.

Thygeson: Oh, he was a ghastly teacher. He would give lecture after lecture after lecture, and I never understood a word, not a word.

Thygeson: He was a world authority. He had two teaching assistants, and you would never guess who they were. They were the two Varian boys. They were superb.

Mrs.

Thygeson: I remember that. We used to say, "Thank God for those assistants," because we would never know what Webster would be saying.

Thygeson: They really told me what Webster had been trying to tell me.

Mrs.

Thygeson: I think they understood it just by osmosis.

Thygeson: Sigurd and Russell Varian started the Varian Company. They were really born teachers.

Mrs.

Thygeson: But not born managers. They were terrible managers, while Hewlett-Packard doing the very same thing, practically, just skyrocketed.

Thygeson: The boys died young, so the company had to carry on afterward without them. Anyway, they were superb teachers. They were influential. We had to have physics for medicine, a required course.

Ruth Lee Spilman Thygeson

Hughes: *What we left out is your marriage, which was in your senior year of undergraduate school.*

Thygeson: 1925.

Mrs.

Thygeson: We were twenty and twenty-one.

Thygeson: In 1920, as a freshman, I went to this freshman football rally for the Big Game [between Stanford and the University of California]. I saw this blue-eyed blond girl. She was in the balcony of this amphitheater. She was yelling her heart out for dear old Stanford. I was very much attracted. Then in the lower division courses we were all alphabetically seated, and her name was Spilman so she was always about two rows ahead of me. So I could see the back of her neck, but I never got to meet her.

Mrs.

Thygeson: He was shy.

Thygeson: I never got to meet her, and one reason was that my social life at Stanford really revolved around a fraternity, Kappa Sigma. Her sorority, Theta, would have nothing to do with the Kappa Sigs. Now they are great friends.

Hughes: *Why was that?*

Thygeson: Well, in those days the Thetas thought the Kappa Sigs were pretty rough characters.

Mrs.

Thygeson: Yes, pretty questionable to go out with.

Thygeson: Kappa Sig was a pretty macho fraternity with a lot of drinking and rough house.

Hughes: *Was it a medical fraternity?*

Thygeson: No.

Mrs.

Thygeson: Phil's brother was ahead of him as a Kappa Sig.

Thygeson: I really didn't want to join a fraternity. He insisted on my joining.

Mrs.

Thygeson: Phil really wasn't interested. He got caught up in it because they try to get the freshmen to do this and this and this.

Thygeson: I was not a good fraternity man because I wasn't an athlete, and Kappa Sig specialized in athletics. They were always into football, basketball, everything.

Hughes: Was that why you weren't interested in joining a fraternity?

Thygeson: I was not interested in joining the fraternity, but my brother insisted. So I was kind of a lag-behind on the usual fraternity activities, social and athletic.

Mrs.

Thygeson: He had his radio until he went into medicine. He was still working that radio night and day.

More on Ham Radio

Thygeson: I was tied up with amateur radio.

Mrs.

Thygeson: His mother had fixed him up with a shed, specially, all his world, and he and a buddy of his, Phil Scofield, had a wonderful time with the radio. The whole thing was magical in those early days.

Thygeson: Amateur radio was new at that time, and it was really fascinating because it was extending its range all the time. Why when I got from St. Paul to Milwaukee or Cleveland, I was really getting somewhere.

Mrs.

Thygeson: It was really exciting. Then the first time they got around the world, ahhhh!

Thygeson: We got to South Africa the long way around the world. Really exciting.

Hughes: This must have been quite a shared experience when you eventually met the Ralph Heintzes, with all their history of radio?

Thygeson: Oh, yes.

Mrs.

Thygeson: It was very recently that we met them. [Daniel] Vaughan, Phil's dearest friend, brought him. The two of them sat here, and we waited on them and sat here, too. It was incredible: "Did you by any chance every know so-and-so?"

"Oh, good heavens, he was my first assistant," says Dr. Heintz.

Thygeson: Mr. Heintz.

Mrs.

Thygeson: He was older. How many years older?

Thygeson: Ten or eleven. He was in amateur radio at the very beginning, which was about ten years before I went in, which was 1916. Ralph was in earlier than that.

Mrs.

Thygeson: He built all these radios for the [Richard Evelyn] Byrd expedition [to the Antarctic]. Everything that went on that Byrd expedition in the way of communication by radio was Ralph's doing. He made the instruments.

Thygeson: I brought his oral history.*

Hughes: Well, getting back to medical school, you got your B.A. in 1925.

Thygeson: I was actually the class of '24, but I dropped out for about six months. I was in commercial radio. I had a commercial license. So that put me behind, so I didn't graduate until '25.

Hughes: Were you planning to go into medicine eventually?

Thygeson: No, at that time I was still registered in engineering.

Hughes: You received your M.D. in 1928. Isn't that pretty quick? That's three years.

Thygeson: Well, see I was really still the class of '24, but I just had to delay to get the degree. I went to summer school to make up for that time out.

Mrs.

Thygeson: We were both '24.

Hughes: You mentioned taking physics classes. What was your major?

Mrs.

Thygeson: Oh, I changed my major several times. Certainly it was not physics. There's a hole in my head. I've never had anything there, never, not to this day.

Hughes: You ended up in chemistry?

Mrs.

Thygeson: Well, I took a lot of chemistry. I really couldn't make up my mind. I had a bad time. Then I had to quit because my mother needed money desperately, and there wasn't anyone to get it; my father was gone. So I quit school after three and one quarter years. I was always going to go back just to tidy it up, but I never did.

Hughes: What were you considering?

Mrs.

Thygeson: I considered history. I was a little half-hearted about what I was going to major in.

* Ralph M. Heintz. *Technological Innovations and Business in the Bay Area*. An oral history interview conducted in 1976 by the History of Science and Technology Program, The Bancroft Library, University of California, Berkeley, 1982.

Courtship and Marriage

Hughes: You left the story about your courtship dangling.

Mrs.

Thygeson: Tell the rest of the story, darling.

Thygeson: Well, when I was a junior, I was playing tennis with Jack Field, who was also a classmate in Palo Alto. I saw her picture on his bureau, so I made an arrangement for a blind date.

Mrs.

Thygeson: That was not an amorous affair at all. In fact, Jack Field and I were always at each other's throats, but we played tennis together. We were about even enough so I enjoyed playing tennis with him. He was a nice boy.

Thygeson: He was a Ph.D. candidate, and I met him in physiology. He became associate dean of the U.C.L.A. Medical School. He's dead now. He was a good research physiologist.

Hughes: What happened after the blind date?

Thygeson: We went out to Pacific City, which was a resort just about on the bay near San Mateo, and we danced all evening.

Mrs.

Thygeson: Jack was madly in love at that time.

Thygeson: With the girl next door to me. So after that it was just one date after another.

Mrs.

Thygeson: Jack put Phil to dancing with me all evening because he didn't want Emmaline danced with at all except by him. Kid stuff. So that's how it happened. We had a wonderful time, didn't we dear?

Thygeson: Yes. Anyway, that was the beginning.

Mrs.

Thygeson: We started going for hikes on the weekends.

Thygeson: Yes, we had a great time.

Hughes: How soon after that were you married?

Thygeson: About a year.

Mrs.

Thygeson: We had to wait because we were awfully young, and no money. My mother still needed support. When we were first married I made the large sum of \$125 a month and sent \$80 of it to my mother.

Hughes: What were you doing?

Mrs.

Thygeson: Stenography.

Thygeson: I had eighty-five dollars from my mother for the school year, and I had to work in the summer. Anyway, we got married in 1925, and I went through medical school.

Hughes: *Where were you living?*

Thygeson: Palo Alto.

Mrs.

Thygeson: We were married in All Saints' Church, which was an adorable little church there, and has been struck now, and a horror has been put up in its place.

Thygeson: Anyway, it didn't cost too much in those days. You could get a wonderful Italian dinner for fifty cents.

Mrs.

Thygeson: I kept a book, and I wrote down, "Steak, thirteen cents."
[interruption]

Medical School (continued)

Hughes: *Dr. Thygeson, is there anything more to say about medical school before we move on?*

Thygeson: Well, I think Stanford Medical School was really exceptional in those days because it was research oriented and the staff was mainly from Baltimore, from Johns Hopkins. Everybody was research oriented. This was a time when there was practically no research orientation at Berkeley. So Stanford Medical School was completely different from it.

There was one really wonderful thing about the medical school. There was a real research man by the name of Thomas Addis, who was an authority on the kidney and endocrinology. He had an elective course on experimental medicine, which was superb, and any research man coming into the Bay Area he would corral and get to talk to us. So we got to hear wonderful lecturers like Herbert Evans, K. F. Meyer, Walter Lilly, and many others. It was very thrilling. It fitted in with the research orientation of the medical school and stimulated everybody to consider a research future. As a result, of thirty in our class, twenty-seven became university affiliated, either as teachers or researchers.

Hughes: *Isn't that remarkable!*

Thygeson: It is, because there was nothing like that at the University of California.

Hughes: *Or many other places, I might add. Most medical schools were oriented towards practice in those days.*

So that exposure at Stanford gave you the idea that you might be interested in research?

Thygeson: Yes.

And then there was a professor [A.W.] Hewlett who was the father of Bill Hewlett of Hewlett-Packard. He was professor of medicine and really a marvelous writer. He wrote a wonderful book on pathophysiology in medicine. I just got in about one year before he up and died. Then Dr. Bloomfield took over.

Hughes: What caused you to consider specializing in internal medicine?

Thygeson: Well, one thing was I had a brother-in-law who was in internal medicine. He offered to share offices, a big partnership. So that was a push.

I had a special interest in infectious disease, and then I became interested in ophthalmology.

Hughes: That was because of—

Thygeson: Hans Barkan. He was a fine teacher and taught us clinical ophthalmology. He was a superb lecturer.

Mrs.

Thygeson: You also had a tear sac problem.

Thygeson: Yes. They say everybody who goes into medicine, if they have anything wrong with them, they go into that specialty.

Mrs.

Thygeson: It certainly applied in your case, didn't it?

Thygeson: If you're crazy you go into psychiatry.

I had accepted an appointment as instructor in bacteriology at the University of Colorado before I went into ophthalmology. I was going to go into microbiology.

Hughes: But you hadn't done your internship yet?

Thygeson: It was during my internship at the University of Colorado that Professor [Ivan C.] Hall, who was chief of microbiology, offered me the appointment as instructor, and I accepted. But I had to change later into ophthalmology.

Dacryocystitis

Hughes: I understand that your dacryocystitis influenced your decision to change to ophthalmology.

Thygeson: Well, what happened was I'd had a sinus difficulty, so I had a Caldwell-Luc operation. Everybody operated on the sinuses in those days. They don't do that anymore. Anyway, at the time of surgery I was all blocked up, and I got a back-up into the tear sac, so I got a tear sac infection, and the tear sac had to be removed. That was done by Hans Barkan, but it was done incompletely, so I had to have it done again at Colorado, and that was by Dr. [William C.] Finnoff. He was the one who persuaded me to drop out of microbiology and go into ophthalmology.

Hughes: *So ophthalmology wasn't a consideration when you were in contact with Dr. Barkan?*

Thygeson: Well, he had tried to influence me and told me he would have a place for me in his office. That was back at Stanford.

Hughes: *Why do you suppose he singled you out?*

Thygeson: Well, I was interested.

Hughes: *You mentioned somebody earlier that was interested in ocular physiology. Was that Barkan?*

Thygeson: No, that was a Ph.D. physiologist at premedical. He was a real authority on ocular physiology, which interested me, too. He was a good teacher.

The Barkans and Pischels*

Hughes: *Adolph Barkan was the first ophthalmologist in the Barkan family.*

Thygeson: Adolph was the father of Otto and Hans Barkan. He was the first real, honest-to-God ophthalmologist in San Francisco. I think Dohrmann Pischel's father, Kaspar, came in just slightly afterwards. Barkan and Pischel were the two leading ophthalmologists and, in fact, the only real ophthalmologists in San Francisco.

Adolph Barkan was dead when I came in, but Kaspar always came down to Dohrmann's office every day for a few hours, even when he was well into his eighties. Adolph Barkan and Kaspar Pischel were the two pioneer ophthalmologists in San Francisco.

Hughes: *Was Otto Barkan also at Stanford?*

Thygeson: Yes, but he didn't do so well because he was very feisty, and they kicked him out as a teacher. But he was a good research man, and his brother, Hans, was not. Otto was a real innovator. He did a lot of work from his private office.

* For more on the Barkans and Pischels, see: Dohrmann Kaspar Pischel, MD. *American Links with Germanic Ophthalmology; Retinal Detachment Surgery; San Francisco*. An oral history interview conducted in 1986-1987 by Sally Smith Hughes. The Foundation of the American Academy of Ophthalmology, San Francisco, California, and the Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1988.

Hughes: *Did he have a particular specialty?*

Thygeson: Yes, glaucoma. Otto was a great fellow, but he didn't have the personality to get along with his colleagues at Stanford.

Mrs.

Thygeson: Especially his brother.

Thygeson: Well, the wives didn't like each other, so they developed a fight there. The husbands took over their wives' fighting. So the brothers, who were once very close, separated.

Hughes: *What did Otto do when he lost the appointment at Stanford?*

Thygeson: He just did his private practice, but right in his office he carried on a real research program, and I think he had a new laboratory in his home. He did office innovation. He did remarkably well. He developed several operations and instruments. He was an innovator.

Hughes: *Would you like to say something about the Pischels, beginning with Kaspar?*

Thygeson: Well, I met Kaspar. I didn't know him well. What I knew was that he came down almost every day. Dohrmann will tell you about that. But I knew Dohrmann quite early when I had just gone into ophthalmology. I met him when he was very young. He was a University of California graduate, and then he went to Stanford Medical School.

Hughes: *He's a bit older than you, so you wouldn't have gone through medical school together, would you?*

Thygeson: No, he was older. I met him through my brother-in-law, Shep, who was older than I am. He was a friend of Dohrmann's.

Hughes: *What was Shep's specialty?*

Mrs.

Thygeson: Diabetes, endocrinology, internal medicine.

Thygeson: He was trained by Thomas Addis. But Shep did not have a research bent. He was a real practitioner.

Mrs.

Thygeson: When medical people came to town and had a convention or something, he would leave town. [laughs]

Thygeson: What he didn't like were conventions.

Dacryocystitis (continued)

Hughes: *Well, getting back to your dacryocystitis, was Barkan's operation a failure?*

Thygeson: Well, it was incomplete.

Hughes: Was that intentional?

Thygeson: No. Finnoff finished it in Denver.

Hughes: You had some trouble, I understand, en route to Denver.

Mrs.

Thygeson: Where did you learn about that?

Hughes: Dr. O'Connor's introduction. It's very good.*

Mrs.

Thygeson: It was a terrifying experience for me.

Thygeson: I had a postoperative hemorrhage.

Mrs.

Thygeson: In the middle of the night he began to hemorrhage while camping in the High Sierra. I felt comforted, presently, when told it was stopping, and it was all right, but I was really quite frantic. I didn't know just what to do, and there was no hospital at Tahoe then or anything like that. Anyway, we weren't at Tahoe. We were on the way to Denver.

Thygeson: It was eventually easy to control by pressure.

Hughes: So you drove the rest of the way?

Mrs.

Thygeson: It took us nine days at that time to get to Colorado.

Thygeson: It didn't scare me because I knew that I could control it by pressure.

Mrs.

Thygeson: It scared me to death.

* GR O'Connor. Biography of Phillips Thygeson. In: MR Allansmith, GR O'Connor, eds. *Phillips Thygeson and the Proctor Fellows*. 1972; 1-7.

II. PREWAR POSITIONS AND RESEARCH

Internship, University of Colorado, 1927-1933

William C. Finnoff and Edward Jackson

Thygeson: Then I went to see Dr. Finnoff right away, and he took care of removing the remaining part of the sac.

Hughes: *He was the logical person to go to?*

Thygeson: He was professor of ophthalmology, and he was considered to be the best ophthalmologist in the Rocky Mountain area.



Interns, University of Colorado, c. 1927-33.
Dr. Thygeson 3rd from left, Dean Rees in center.

Hughes: *Was he chairman of the department?*

Thygeson: No, Melville Black was chairman, but Melville was not a research ophthalmologist, and he was not a very outstanding clinician, but he was the chairman. Finnoff was the real backbone of the department at that time.

Mrs.

Thygeson: But what about Edward Jackson?

Thygeson: He was emeritus. He was still teaching, but he was not on the university staff. I got to know Jackson very well.

Mrs.

Thygeson: Wasn't he in Dr. Finnoff's office often?

Thygeson: Eventually. We had private offices right across from each other in an old medical building downtown. That's really where I got to know Dr. Jackson, because whenever Finnoff was away and I was faced with a rough case, I went to Dr. Jackson for advice. Jackson was never too busy to help me out.

Hughes: *There was a technique mentioned in connection with his name.*

Thygeson: There was the cross-cylinder technique for refraction. He was a real innovator in physiological optics. I had been interested in this because of my Stanford days in physiology.

Hughes: *Was that the first time you had come in contact with the technique?*

Thygeson: Yes, that was the first time I came into contact with it.

Hughes: *He taught you?*

Thygeson: He taught me cross-cylinder, which was a wonderful technique at that time, but hardly anybody knew it except Jackson.

Hughes: *Would you describe it?*

Thygeson: Do you know what an optical cylinder is?

Hughes: *No.*

Thygeson: It's a lens which has power only in one direction. The ordinary spherical one has the same power in all directions, but the cylinder is only powerful in one. But if you have two cross cylinders at a ninety-degree angle, you can change the testing of your refraction very easily, and you can work out the axis of the astigmatism very easily. Its main purpose was in detecting the axis and amount of astigmatism. This was very difficult to do in those days without this technique.

##

Hughes: *Was Jackson responsible for building up the department at Colorado?*

Thygeson: Yes, he made Colorado kind of a Vienna of ophthalmology, you could say, because he attracted ophthalmologically important people, and he developed the first graduate course in ophthalmology in the U.S., and also the first degree in ophthalmology, which is a doctor of ophthalmology.

Hughes: *Which was his idea as well?*

Thygeson: Which was his idea. He got it from Oxford, where they had the degree. I got that degree.

Hughes: *Why did you choose the internship at Colorado?*

Thygeson: I had been East for some reason or other, and I came back and stopped off in Colorado Springs in March. The weather and the mountains and everything were so ideal that I thought I would have to intern in Colorado and then go back to San Francisco, but I was going to spend a year in Colorado.

Hughes: *When you say San Francisco, you mean Stanford?*

Thygeson: No, I was meaning clinical work in San Francisco. So in a way I was coming back to Stanford Medical School. I was toying with the idea of going into practice in internal medicine with my brother-in-law at that time.

Hughes: *Would you say something about Dr. Finnoff's background and also what he was like as an individual?*

Thygeson: Finnoff was a little fellow with a little man's complex. You know what a little man's complex is?

Hughes: *Fiesty.*

Thygeson: Yes, and he always had a great big car, and he drove it at high speed just the way all the little fellows do. He wouldn't accept much in the way of conflicts. He knew he was the best ophthalmologist in the Rocky Mountain area, and he didn't mind telling his patients that he was the best. And he was the best. He had the first slit lamp microscope in the whole western United States. He did the first intracapsular cataract extraction in the western United States. He was a real innovator.

Hughes: *Where had he been trained?*

Thygeson: He was a local Denver boy, the University of Colorado, and then Jackson had trained him in a preceptorship. Then he went to Vienna where he learned ocular pathology from Ernst Fuchs, the great professor there.

Mrs.

Thygeson: They all went to Vienna.

Thygeson: Yes, everybody had to go to Vienna to cap off their eye training.

Mrs.

Thygeson: To be any good at all.

Thygeson: Anyway, he was a student and protégé of Jackson. He carried on the Jackson spirit about courses and advanced training.

Hughes: *Why wasn't he made chairman?*

Thygeson: Well, he was so fiesty that he ruffled feathers.

Mrs.

Thygeson: Dr. Jackson was just the opposite—gentle.

Thygeson: In giving consultation, Finnoff would offend the doctor who had referred the case because he would find that the patient had been mistreated and would tell the patient so. That would get back to the referring doctor, and that didn't do so well, whereas Jackson was just the opposite. He knew how to please both the patient and the referring doctor.

Mrs.

Thygeson: He was just naturally that way. It wasn't something he learned in the business or from somebody else. He was just naturally a gentle, sweet guy.

Thygeson: He was also an innovator, too, because he developed retinoscopy, which is another optical technique.

Mrs.

Thygeson: He had the blessing of a wonderful personality.

Thygeson: Everybody liked him.

Mrs.

Thygeson: Couldn't help liking him. I just loved him.

Thygeson: He could also be very firm. For example, there was a rebate system, which was very widespread, in which a rather small charge would be made for refraction, but it would be made up by a kickback from the optical company that made the glasses. This infuriated Jackson, and he knocked out rebating in all of the Rocky Mountain area, personally. This was a one-man crusade.

Mrs.

Thygeson: He was a strong man. But he was gentle.

Thygeson: He was very ethical. There was a fee-splitting program between optometrists and surgeons at that time in which an optometrist would refer a cataract case to an ophthalmologist and then expect a kickback. Jackson knocked all that out.

Mrs.

Thygeson: Then guess who got rid of it in San Jose?

Thygeson: There was a fee-splitting problem in San Jose when we came there, and Dr. [Crowell] Beard and I knocked that out.

Hughes: *Dr. Finnoff was renowned for his work in tuberculosis.*

Thygeson: And in ocular pathology. He was a real student of all eye diseases.

Hughes: Did he give a course in ocular pathology?

Thygeson: Yes, he had many students. We had this yearly course at Denver, but he also had students who came just to him.

Hughes: You were getting more and more interested?

Thygeson: Well, see I was an intern. He had worked on my eye, and I was interested. So in my spare time I worked in his little laboratory on eye pathology. Then I helped him in surgery. He had lost an assistant, Dr. Donald O'Rourke, and so I helped him with all his operations.

Mrs.

Thygeson: We used to go to his house in the evenings sometimes and work over slides.

Thygeson: Yes, we worked evenings at his house. We did the first studies on trachoma in the area there, first of all on the pathology of trachoma and then on the cause of the disease.

Hughes: Had he been interested in trachoma for a long time?

Thygeson: No, but he was Vienna-trained, and trachoma was very important in Europe at the time. He had seen the early work on the chlamydia in Vienna.

Mrs.

Thygeson: He didn't know they were chlamydia, did he?

Thygeson: No, he didn't know they were chlamydia, but that's what they were. So he had all that background from Vienna, which I didn't have. I had a microbiology background, and he didn't have that. He had pathology. We worked together very well.

Hughes: Did he single you out? Presumably there were other interns.

Thygeson: Nobody else was interested in the eyes or in infectious disease. The interns of those days were not interested in research at all, especially at Colorado. Colorado was just like Berkeley. There was no interest in research at that time. Colorado was not a research school. It was a practitioners' school.

Hughes: Was there anybody else of note on the faculty?

Tuberculosis

Thygeson: Yes. There was a Dr. James Waring who was a Hopkins-trained man, a professor of medicine. He was a superb teacher. He had tuberculosis himself, and that's why he came to Colorado. But he liked the teaching, and he liked the interns. So I had quite a good contact with James Waring. He was an innovator in the treatment of tuberculosis.

Hughes: How did he treat it?

Thygeson: Well, for example, he knew that tuberculosis did better at high altitudes and with sunlight, but nobody knew why. I think it was he that worked out the reason. There was no doubt about it that tuberculosis did better in Colorado and Arizona and New Mexico— sunlight.

The main reason was that there's an increase in white blood cells at high altitude, and so it increased immunity and benefited tuberculosis by that way.

Colorado was a tuberculosis area. They had sanitariums all over the state.

Mrs.

Thygeson: Patients came from all over the country.

Thygeson: So we had no problem finding cases of ocular tuberculosis, and that was what Finnoff was interested in. I saw an awful lot of ocular tuberculosis in my early days. Now you don't see any at all.

Hughes: How was ocular tuberculosis treated in those days?

Thygeson: Well, by a method which did not work out well, which was desensitization with tuberculin. It had no future, but it was widely used.

Hughes: It wasn't until chemotherapy came along that there was any really effective treatment for tuberculosis?

Thygeson: Well, the sulfanilamides did not work in tuberculosis. It was INH that was the first.

Hughes: What is INH?

Thygeson: It's isoniazid. It was the first effective treatment. It revolutionized tuberculosis treatment.

Mrs.

Thygeson: It reached the Eskimos, and it helped them.

Hughes: Post World War II?

Mrs.

Thygeson: Yes.

Thygeson: It was in my day in New York, because the Squibb Institute was very active. Squibb developed isoniazid. Squibb Institute in Rahway, New Jersey was very active, and I was working down there. So that's how I got tied in with that.

Mrs.

Thygeson: Not all the time.

Thygeson: No, but I was a consultant. I was working on chlamydia. They were working on chlamydia down there, too.

Isoniazid was a real victory because it just changed the whole picture of tuberculosis. Other things came in later, but isoniazid persists.

Mrs.

Thygeson: Do you remember what year that got loose in Alaska and really helped?

Thygeson: Well, it was after we were there, and we were there in the early fifties.

Mrs.

Thygeson: We were there in '49 and '50.

Thygeson: Anyway, it was about the mid-fifties that isoniazid came in.

Mrs.

Thygeson: That's what I thought. It was after we had been there.

Thygeson: Tuberculosis was a terrible problem in Alaska when we were there.

Mrs.

Thygeson: Oh, just heartbreaking.

Hughes: Are you talking about the lungs or the eye as well?

Thygeson: Well, the eye, but mostly it was bone and joint tuberculosis—humpback, Pott's disease.

Mrs.

Thygeson: There was that adorable little nurse, and after we left she got TB and died.

Thygeson: See, the Eskimos' way of life is that the whole family lives in a one-room place, and so tuberculosis spread like wildfire in the Eskimos. Isoniazid changed the whole picture.

For both the Indians and the Eskimos, tuberculosis was number one as a killer. The Indians were just as badly off in a tepee in the winter as the Eskimos were in an igloo. Tuberculosis was a family disease. We had ninety percent plus tuberculin reactors. That means they were infected. Now it's down to less than three percent.

Hughes: Was the Indian Service helping out with the treatment of tuberculosis?

Thygeson: Well, they had a study of a vaccine in connection with the University of Pennsylvania. It was a tuberculosis-like organism that immunized against, but didn't cause, tuberculosis—Bacille Calmette-Guerin. The Indian Service was very much interested in pushing this immunization program, so all young children they were treating with BCG.*

* The discussions of trachoma and the Rockefeller Institute, which follow on the tape, were moved for better chronology to the end of the transcript of the first interview.



Mrs. Elizabeth Proctor
and Phillips Thygeson,
Egypt, November, 1930.

The Giza Memorial Research Institute, Egypt, Winter 1929-1930

Hughes: You mentioned in passing that you received the degree of doctor in ophthalmology from the University of Colorado in 1930. Did you have to do a research project?

Thygeson: I had to do a thesis for that, and I did it on the basis of the trip to Egypt and what I found there.

Hughes: Could you tell me how that came about?

Thygeson: It was Dr. Proctor who insisted that we should go to Egypt, which was the home of trachoma. That was where almost everybody had trachoma. There was more blindness in Egypt than in any other country in the world. He made all the arrangements to work with a Dr. [Roland P.] Wilson at the Giza Memorial Ophthalmic Laboratory. It was a very fine, small research unit in Cairo.

At that time we took over cultures of *Bacterium granulosis*, and we made inoculations over there, both in monkeys and in humans. It didn't work out. So I studied also the bacterial flora of trachoma, what organisms were found

with trachoma, because trachoma brings in a lot of other organisms, particularly *Hemophilus egyptius* and the gonococcus, another organism. That was my thesis, the bacterial flora of trachoma in Egypt. *

Hughes: *How well was that laboratory equipped?*

Thygeson: It was quite well equipped for the time. It was supported by the funds generated for a memorial to the camel drivers of Egypt in World War I. [Edmund Henry Hynman] Allenby, his army, which went into Palestine and all, was dependent on the Egyptian camel drivers. All the supplies of Allenby's army were carried on camels. A lot of the drivers were killed in that war. So the English government started a fund for a memorial for the camel drivers who had died. It was called the Giza Memorial Ophthalmic Laboratory. It was dedicated to the camel drivers of World War I. It had a staff of four full-time investigators, which was very unusual for that time because full-time investigators were rare.

Hughes: *Was the staff Egyptian?*

Thygeson: No. There were two English and two Egyptians.

Hughes: *They were microbiologists?*

Thygeson: Or pathologists.

Hughes: *What about Wilson himself?*

Thygeson: Well, trachoma being the number one disease, Wilson worked on it but not exclusively. At first he was interested in *Bacterium granulosis*, and then as we found out it wasn't any good, he went on to study the inclusions, and so did we. But his associate there, Dr. F.H. Stewart, decided that the inclusion bodies were not specific in any way, that they were intracellular bacteria that had undergone digestion and were artifacts. So I had a run in and a fight with Stewart over the specificity of the inclusion. I knew Stewart had the wrong idea.

Hughes: *He thought that they were the causal bacteria?*

Thygeson: No. He thought they had nothing to do with trachoma, that the cause was still unknown. A virus probably. Anyway, these studies were the basis for my thesis for the degree of doctor of ophthalmology.

Hughes: *I understand that you had quite an eventful trip over to Egypt.*

Thygeson: Yes. We had an interesting time because we travelled with Dr. and Mrs. Proctor. Mrs. Proctor was a real Maryland belle. She was a real beauty and used to all the best things in life. She wouldn't accept anything other than first class. So, of course, we went over deluxe on the S.S. *Augustus*.

Hughes: *Were they paying your way?*

* P. Thygeson. Bacterial flora in Egyptian trachoma. *Am J Ophthalmol* 1931; 14:1104-07.

Thygeson: Yes. This was all on Proctor's money.

Hughes: *And your wife went as well?*

Thygeson: No, she didn't go on this trip.

We landed in Naples. Dr. Proctor was a Harvard man who knew everybody. He was a real man of the world, socialite, cataract surgeon, yachtsman, and world traveler. He knew everybody. His classmate, [Henry Lewis] Stimson, was the Secretary of State. Stimson notified the authorities in Naples that we were coming, and so we were met by the chief of police of Naples and were taken right through customs to the Hotel Excelsior and given the big treatment. Then an appointment was made with Mussolini, an audience with Mussolini, which Dr. Proctor turned down.

I've forgotten whether it was the Egyptian or Italian government that provided passage from Naples to Alexandria. The interesting thing on that voyage, which took two days, was that our two rhesus monkeys, that we had brought with us and that supposedly had *Bacterium granulosis* conjunctivitis, got loose and went all over the boat. They scared all the passengers.

We arrived in Alexandria and went up by train to the Giza Laboratory at Cairo where we started work.



Dr. and Mrs. Francis Proctor studying bacteriology at a Tucson hospital in the early 1930s. Unidentified bacteriologist in center.

Hughes: *How were you accommodated?*

Thygeson: We were at a hotel first, and then we were in a boarding house that was run by a very high-class English lady. She ran this boarding house where everybody dressed for dinner at eight o'clock sharp. She wouldn't let any Egyptian friends come. She really had race prejudice.

Hughes: *Were the Proctors staying there, too?*

Thygeson: Yes. They worked with me for a little while in the laboratory, and then they went on up to upper Egypt by Nile boat.

Hughes: *Mrs. Proctor worked in the lab, too?*

Thygeson: Yes.

Hughes: *What was she doing in the lab?*

Thygeson: She had a winter with some bacteriology training at a hospital in Tucson, so she could make cultures and could read cultures. She knew how to use the microscope very well.

Hughes: *So they both were good research people?*

Thygeson: Well, in a slightly amateurish way because their background was one winter of training, so they weren't trained as professionals. But then they decided that their usefulness had come to an end, so they went on up to upper Egypt. Then they came back and went down to Sicily for the rest of the winter. They paid all my expenses.

Hughes: *Did you join them before you went back?*

Thygeson: Yes.

The Pasteur Institute, Tunis, 1930-1931

Hughes: *It was on that trip, I believe, that you stopped in Paris to see Victor Morax?*

Thygeson: No, that was the next trip to the Pasteur Institute of Tunis.

Hughes: *You returned to the United States in 1930 and immediately applied for a NRC [National Research Council] Fellowship, which you got, and went to Tunisia.*

Thygeson: The reason I went to Tunisia was because of Charles Nicolle, the Nobel Prize winner, who had reported the filtration of the trachoma agent in Berkfeldt B filters. He was one of the principal trachoma research workers of the early days. His several reports on the filtration of the trachoma agent interested me. They were the first filtrations and were made on Algerian mangeby monkeys.

So Dr. Proctor thought that the place to go next was Tunisia to work with Nicolle, so that's when I got the National Research Fellowship, which was \$200 a month and \$150 for transportation. Dr. Proctor gave me \$1000, and then he had classmates who were in the American Export Line, a freighter service, so they gave us free transportation on the freighter, *S.S. Exermont*. We were the only passengers on this ship. It took us twenty-six days to get to Tunis.

Hughes: Was that the passage where you ran into storms?

Thygeson: Yes.

Hughes: And there was a mutiny?

Thygeson: We had a mutiny at Gibraltar, but it only lasted about half a day.

Hughes: What was the cause?

Thygeson: Well, there was a fight between an oiler and a steward. The steward went down and hit the oiler while he was on duty with a wrench and knocked the oiler out. This was high seas mutiny because it put the engines in jeopardy. The captain went down with the mates and arrested this steward and put him in the brig. Then they left him, I think, in either Souta or one of those towns along the African coast, left him with the American consulate. The other sailors were friends of the oiler, so they wanted to see this steward really put in irons. That's how the mutiny started.

Hughes: Going over you stopped at the Pasteur Institute in Paris. Is that right?

Thygeson: No, it was after I had been there at Tunis for a while. Then I went over to Paris. Dr. Proctor knew Victor Morax, who was an early pioneer in ocular microbiology and who worked at the Pasteur Institute. I went and had a day with Victor Morax.

Hughes: What was he like?

Thygeson: A wonderful fellow. He was a French-Swiss who became a French citizen. He was a leading light in ocular microbiology in France, but he was a jack-of-all-trades. He developed a number of surgical procedures. He was also a pathologist. He could do anything. He was one of those Renaissance men. So I had a wonderful day with him.

Hughes: How had that meeting been set up?

Thygeson: Through Dr. Proctor.

Hughes: But Dr. Proctor was not along?

Thygeson: No. The next year I went back again to visit Victor Morax. He had a course in ocular microbiology which I attended. It was probably the first course ever conceived in ocular microbiology. I have the syllabus from that class.

Hughes: Were you the only student?

Thygeson: No. About a half a dozen.

Hughes: Anybody of note?

Thygeson: Well, there was a Turk, Necdet Sezar, who eventually wrote a book on ocular microbiology, which I have, but nobody otherwise of note. But through this I got to see almost everybody in Paris that was interested in microbiology. I met [L.C.A.] Calmette and [Alphonse F.M.] Guerin, [Emile] Roux—all the people who were working on bacteriology. Morax knew everybody there.

Hughes: Were all these people at the Pasteur Institute?

Thygeson: Yes.

Hughes: The course was a practical course, a laboratory course?

Thygeson: Yes.

Hughes: How long did it last?

Thygeson: It lasted a week.

Hughes: And then you returned to Tunis?

Thygeson: Yes.

Hughes: What was the set-up at the institute?

Thygeson: The Pasteur Institute of Tunis was number two in the so-called jewels of Pasteur Institutes all around the Mediterranean. They were in Algiers, Casablanca, Istanbul, Athens. They had one in Indo-China, too. But Tunis was number two because of Nicolle, who became a Nobel Prize winner in typhus. Nicolle started the institute at Tunis.

Hughes: Why was a Pasteur Institute established at Tunis?

Thygeson: Because Tunisia was an important province. It was like Algeria or Morocco.

Hughes: So it wasn't that it was a focus of a particular disease?

Thygeson: No, but it happened to be a focus of trachoma, as all the Mediterranean area was at that time, so there was plenty of material.

When I got there Nicolle treated me well because of my Norwegian name. He provided me with a laboratory and a technician, which is very unusual for a fellow. He gave me periodic advice. He had a wonderful staff, particularly the associate director, Charles Anderson, who was half English and half French. I had much conversation with Anderson. That was where I became convinced that the inclusion body was the cause of trachoma. Nicolle didn't buy that at all.

Hughes: *Why not?*

Thygeson: He said in trachoma the virus would not be in the epithelium; it would be in the follicle.

##

Thygeson: He was a tall Norman Frenchman who was totally deaf.

Hughes: *Deaf from birth?*

Thygeson: No, later in life, but he was a lip reader and an intellectual giant. He wrote and spoke as a world authority. But he happened to be all wrong on trachoma.

Hughes: *He also was a novelist.*

Thygeson: Oh, yes. I have his novels.

He had a delightful way of recognizing his fellows. There weren't many fellows in those days. I think he'd had about half a dozen. I was one of them. He left Tunis in the summer, vacationed usually in Norway. He would send postcards. So he kept in touch with his half-a-dozen fellows by postcards. He would send out these novels to us. And he did a very nice thing in the library. He had a beautiful library with a big marble wall, and on it he put the names of his fellows in chronological order, the years they were there.

Hughes: *Your name is there?*

Thygeson: Yes, my name is there. It's still there according to Dr. [Chandler] Dawson, who has been in Tunis recently.

Hughes: *You haven't been back?*

Thygeson: I was back once, and the new director recognized my name right away because of this library. So I got a gala visit to the institute.

Hughes: *Were there other fellows at the same time?*

Thygeson: No, I was the only fellow at the time.

Hughes: *Was most of the staff French and trained in France?*

Thygeson: I think almost all French. There were helpers who were Tunisian. The second or third director after Nicolle was a Tunisian pathologist. He was the one I met on my second visit to Tunisia.

Hughes: *Were you fluent in French?*

Thygeson: I wasn't fluent, but I had started French in high school, and then French was my language for medical school, so I knew the literature, and I knew how to read it and write it, but I couldn't speak it worth anything at all.

We lived down in Carthage on the peninsula with a French family. The husband was a teacher, so we hired him. Every evening he gave us lessons in French. I picked up French in a usable fashion in that way. I got along fine with Nicolle because he could read my lips. I never developed any fluency, but Ruth Lee did. She was very good.

Hughes: Was Nicolle active in the laboratory at that point?

Thygeson: Yes. He was in his sixties at that time, but he was still going strong. He had a wonderful system. He had Charles Anderson, who was the associate director, and Anderson did all the leg work, all the scut work, as they call it. He left Nicolle free to do his laboratory studies. Nicolle made the policies, but Anderson settled all the disputes and everything like that, and he liked it. Anderson was a born mediator, so he took care of everything.

Hughes: And a microbiologist?

Thygeson: Yes. It would take him about half a day to do all this institute business, and the other half he would put in on the laboratory, and he was good.

Hughes: You mentioned Carthage.

Thygeson: We lived in Carthage in a French villa with a French family. This family was a husband and wife. The husband was a retired schoolteacher, and the wife was a mail-order bride. She was a very fine woman.

Hughes: Did you explore the ruins?

Thygeson: Oh, yes. Every weekend we went out and I made seventy-five different photographs of ruins that could be identified. Then we obtained many Punic, Roman, and Byzantine coins, lamps, and other little artifacts.

Hughes: Do you still have those?

Thygeson: Yes. I have them in the back room. The most exciting time was on the plage, as they call it, the beach. I was sifting the sand, and I ran across a coin. It turned out to be a silver coin, Louis Philippe, 1830, five-franc piece. I still have that back in the other room. Then we collected many pieces of marble. Carthage was noted in the ancient world for its marbles. All those fragments of marble are still around. I collected most of the different types of Carthaginian marble.

Hughes: Was there any systematic exploration of Carthage at that period?

Thygeson: Yes. It was all done by Les Peres Blancs, which was a religious order. There was one father who was an archeologist. He ran a continual archeology study in excavated tombs. There were a lot of Byzantine tombs around there. Now Carthage is an archeological park. It was taken over by the government and made into an archeological park with real preservation. When I went back, Carthage had changed a great deal because they had restored a number of these ruins in a wonderful way.

Incidentally, Tunisia had a dictator, [Habib] Bourguiba, who was a benevolent dictator. He understood the importance of archeology. [interruption]

Assistant Professor, University of Iowa School of Medicine, 1931-1936

Hughes: *Dr. Thygeson, before you left for Tunis, Dr. C.S. O'Brien, who was chairman of the department of ophthalmology at the University of Iowa, invited you to join the staff. I guess you agreed?*

Thygeson: Yes. We had that appointment at the time we left for Tunis. The department at Iowa was special because it really had the best residency in the country, all on account of O'Brien. O'Brien's whole interest in life was ophthalmology. He made a department, which I didn't know at the time but I knew later, by far the best in the country, much better than Harvard or Columbia or Hopkins or any other. It was by far the best.

Hughes: *He was a teacher as well?*

Thygeson: Yes. He was a teacher and investigator, a very good surgeon, a very good ocular pathologist, and very much interested in research. He ran a very tight ship, which I think was unusual in that day. He worked every weekday and Saturday morning until one o'clock and then on Sundays from nine until noon and then two nights a week for two hours each for special courses.

Hughes: *The whole department was expected to do that?*

Thygeson: Everybody. You started at eight o'clock in the morning sharp, and there were no excuses—staff, everybody. He wanted his trainees to do well, particularly on the American Board of Ophthalmology exams. They were really the best-trained residents of the time, by far.*

Hughes: *Who else was on the staff when you first arrived?*

Thygeson: There was Dr. P.J. Leinfelder, who became a very important neuro-ophthalmologist, and Dr. [Alson E.] Braley, who became chairman of New York University department of ophthalmology and then at Iowa. So there was quite a number of trainees that made marks around the country.

Hughes: *You were an assistant professor?*

Thygeson: Yes.

Hughes: *What were you doing in the way of research?*

Thygeson: Well, I had my own laboratory. I was also in charge of refraction and external disease. Then the other field I had was neuro-ophthalmology. Each staff member was assigned to a ward where we examined every patient who came

* A discussion of the American Board of Ophthalmology, which follows on the tape, was incorporated in the transcription of a later discussion of the same topic.

into the hospital for eye changes. I was assigned to neurology, so every day I examined all new patients to see if there were any eye changes that could be of teaching value. We taught the medical students in a big way there. We trained them about as hard as you would a resident.

Hughes: I understand that you developed an external disease program for residents.

Thygeson: Yes. I think it was the first such program in the United States. There were six residents, two a year. Each new resident was assigned to me first. So I had to teach them both external diseases and refraction. Then they did small research projects. They were assigned to me at my laboratory.

Hughes: How long did you have them?

Thygeson: We had them for six months.

Hughes: Did any of those people go on to make a name for themselves?

Thygeson: Yes. One of the best was James Allen, now at New Orleans, Tulane University. The other one was Alson Braley, who became a distinguished professor at the University of Iowa. There were half a dozen others that went on to university appointments.

Hughes: Was your course a laboratory course?

Thygeson: We didn't have a regular course. We just had a kind of preceptorship. I had this one man, and he did everything with me, all examinations and cultures and projects.

Hughes: Had these people had any background in microbiology up until that point?

Thygeson: Just the ordinary medical school course. Nobody was trained in microbiology.

Hughes: So presumably that was a heavy emphasis in your teaching?

Thygeson: Oh, yes, external disease and microbiology go together.

Hughes: So it was essentially a course in microbiology applied to the eye?

Thygeson: Yes.

We had a great time in Iowa. I learned more in those three and a half or four years than I have ever learned in a similar time later. It was such an intense study time. We had wonderful contacts with other departments, particularly pathology and biochemistry, so we got a lot of basic ophthalmology there that you don't get nowadays because they don't have the close contacts with the other disciplines now.

Hughes: Did the other disciplines actually participate in the preceptorship?

Thygeson: They helped in the consultations. Whenever we got into difficulty on any basic problem, we had a consultant. Pathology particularly was important.

Hughes: *Were you actually presenting cases to the students?*

Thygeson: Well, we had all the medical students, and they had to see all the eye problems in the whole hospital. We provided a very intensive medical school training in ophthalmology. Quite a number of those students became ophthalmologists later on because they were excited by the eye.

Hughes: *Was ophthalmology a specialty that was sought out by young men in the 1930s?*

Thygeson: It was a desirable specialty, but in the university it was not ordinarily performed by full-time faculty at all. It was only taught by volunteers, such as Hans Barkan and Dr. [Frederick C.] Cordes. They never got paid anything, but they had the prestige of being professors.

Hughes: *They supported themselves through their private practice?*

Thygeson: Yes.

Hughes: *When did that begin to change?*

Thygeson: Here in California, UCSF [University of California, San Francisco] was the first. Sam Kimura became the first full-time faculty member in ophthalmology here at UC, San Francisco. Now they have about eight or ten full-time faculty members.

Published Papers

Inclusion Blennorrhoea, 1934*

Hughes: *I read a few of your papers from the Iowa period. The first one was published in 1934 on inclusion blennorrhoea. You maintained that it was a distinct clinical entity, distinct from trachoma and distinct from anything else. You claimed a viral etiology of the disease. How did you establish the cause?*

Thygeson: I did the study on ophthalmia neonatorum. A certain number of babies had conjunctivitis, but no bacteria, ordinary bacteria, but they had the inclusions in the scrapings. So they were clearly tied in with the disease because the inclusions paralleled the course of the disease. When the disease was over, the inclusions were gone. When the disease was established, there were inclusions, and in the incubation period of the disease, you could find inclusions. We did a number of things you can't do nowadays. You would be sued right now.

Hughes: *Using patient volunteers?*

Thygeson: What I did was, since this was a harmless disease—it didn't cause any damage to the eye—I didn't hesitate to transfer a unilateral case into the other eye. That way I could tell when the inclusions started in the epithelium. About

* P Thygeson. The etiology of inclusion blennorrhoea. *Am J Ophthalmol* 1934; 17:1019-35.

five or six days before the clinical disease started, I could find two or three cycles of development of the inclusion bodies all in the incubation period. So it was very clear that this agent was the cause of the disease.

The other thing we did to establish this, we filtered the elementary bodies and used the filtrate to inoculate monkeys, baboons, and also humans. I was a volunteer in one experiment. My disease, after the inoculation, came down in seven days. The other volunteer came down in five days. She had a much more severe disease than I did. She got over hers in three or four months while mine hung on for about seven months.

Hughes: What are the symptoms?

Thygeson: Just an acute conjunctivitis. It's a harmless disease as far as ultimate damage to the eye. No scars.

Hughes: There had been confusion between it and trachoma and other diseases?

Thygeson: Yes. It was originally called genital trachoma because of finding these inclusions in the urinary tract and cervix of the mothers of the babies. The name was genital trachoma or paratrachoma.

It was Professor Karl Lindner of Vienna who showed that the adult cases of inclusion blennorrhoea had clinical courses which resembled trachoma in the early phase, but then spontaneous healing occurred which almost never occurred in trachoma. So he wrote a paper showing that these cases could not be trachoma because they had a benign course and a benign outcome.

Hughes: That finding had just recently come out?

Thygeson: Yes. But there was still talk about trachoma and inclusion blennorrhoea being the same disease. It was mostly from microbiologists who didn't know any clinical ophthalmology. If they knew any clinical medicine, they would realize you've got two distinct diseases.

The Etiology of Trachoma, 1934

Hughes: Another paper published in 1934 suggested the viral etiology of trachoma. Well, now we know it is chlamydial and that chlamydiae are bacteria.*

Thygeson: Yes. We based that suggestion on the filterability and the presence of inclusion bodies and on the intracellular nature of the organisms, which at that time were considered to be viral characteristics.

Then James W. Moulder later showed it couldn't be a virus. It had the life cycle of bacteria, not the cycle of the viruses. There's RNA in the cycle of the chlamydia which is not present in the usual viral molecule. So it became obvious that we were dealing with a completely distinct group of organisms, chlamydia, which resemble rickettsiae on one side, and have some resemblances with the viruses on the other side, but form a distinct chlamydial group. There are many different types of chlamydia.

* P Thygeson. The nature of the elementary and initial bodies of trachoma. *Arch Ophthalmol* 1934; 12:307-18.

Hughes: *Were other people coming to the same conclusion, distinguishing the chlamydia from viruses and rickettsiae?*

Thygeson: Morax was the principal one and maybe Lindner. I was the only one in the United States that felt that way.

Hughes: *Most people were still assuming that they were viruses?*

Thygeson: Yes. Well, you see, we had Elford filters set up. We were able to make special filters that wouldn't absorb the agent. Ordinary Berkfeldt filters absorb a lot of the active material, so you have to have a lot of active material before any goes through, just as a matter of adsorption, not filtration.

Hughes: *Is that true of any virus, or was it because these were particularly large?*

Thygeson: Any virus suspension is largely adsorbed by the filter before any viral particles get through.

Hughes: *How did you get around that?*

Thygeson: We used the Elford collodion filters, which were thin, wafer like filters in which you can measure the pore size. It was all worked out by [William J.] Elford in London. You can make these filters of any pore size you want. We used a pore size of about three to six millimicrons.

Hughes: *You mean you bought the filters without the pores?*

Thygeson: We made the filters. We poured the collodion by a special technique. I had to learn all this, and we had to make our own filter holders and everything. It was quite an elaborate procedure.

Hughes: *Did you learn how to do this by reading the literature?*

Thygeson: Yes.

Hughes: *You never met Elford?*

Thygeson: No, I've never met Elford, but I got a little hint from several other people—I've forgotten who they were—as to just how to measure the pore size. It was a tricky thing to measure the pore size.

Hughes: *How did you do that?*

Thygeson: This was by Poiseuille's law about the way fluids pass through a filter. It's a law of physics by which you could calculate the average pore size of the filter. We made these filters with a pore size which stopped all the bacteria that we knew of, small bacteria like *Homophilus influenzae*, but would let viruses through. We used vaccina virus as a test. We had done studies on monkeys

with the trachoma agent, and we had shown that elementary bodies were in the filtrate and that a bacteria-free filtrate would produce infection in the monkeys. So we were ready for humans.*

Acute Follicular Conjunctivitis, Beal's Type, 1935**

Hughes: *A paper published in 1935 is entitled, "Acute Follicular Conjunctivitis, Beal's type."*

Thygeson: This was an interesting paper because it was the first American study of a form of conjunctivitis that was later shown to be caused by adenovirus type 3. The adenoviruses weren't discovered until quite late. The clinical features of their diseases were well known, but the etiology wasn't known. This type of conjunctivitis, which is an adenovirus 3 conjunctivitis, was called Beal's conjunctivitis because [R.] Beal, who was a resident in Morax's service at Lavoisier Hospital in Paris, first wrote a paper on this type of follicular conjunctivitis, which is always associated with a pharyngitis and fever. So the disease is called pharyngoconjunctival fever.

Then we didn't know the etiology, but we had the clinical picture. I think this was the first description of Beal's type of follicular conjunctivitis in the United States, long before we knew what the etiology was.

Hughes: *Morax and Beal had done their work sometime previous to that?*

Thygeson: Well, four or five years.

Hughes: *Was that a subject of discussion when you visited Morax?*

Thygeson: Yes, Morax mentioned it. In 1933 he wrote a treatise on follicular conjunctivitis in which he describes this pharyngoconjunctival fever entity.***

Inclusion Conjunctivitis, 1936

Hughes: *A paper published in 1936 is called, "The virus of inclusion conjunctivitis."**** Please tell me about that work.*

Thygeson: What we did was to investigate the genital origin of inclusion blennorrhoea. We studied the epithelium of the mothers of the babies with inclusion blennorrhoea.

Hughes: *This is inclusion conjunctivitis. Is that the same thing?*

* A further discussion of trachoma, which follows on the tape, was combined with the earlier discussion and moved for better chronology to the end of the transcript of the first interview.

** P Thygeson. *Arch Ophthalmol* 1935; 13:853-4.

*** V Morax. *Les Conjunctivites Follicular*. Paris: Masson et Cie, 1933.
A discussion of Fort Apache, which follows on the tape, was moved to the end of the transcript of the first interview.

****P Thygeson, WF Mengert. *Arch Ophthalmol* 1936; 15:377-410.

Thygeson: Same as inclusion blennorrhoea. Conjunctivitis of the adult or conjunctivitis of the newborn, inclusion blennorrhoea was the name given to them by Professor Lindner in Vienna because it was more than just an ordinary conjunctivitis. It was a pretty hot conjunctivitis which was termed blennorrhoea instead of just conjunctivitis. But in the adult it's a much milder disease than in the newborn.

Hughes: *What does blennorrhoea mean?*

Thygeson: I think it means the flow of pus.

Hughes: *That was one of the symptoms?*

Thygeson: It's a purulent conjunctivitis. The chlamydia stimulate pus cells, as opposed to viruses, which stimulate monocytes. The chlamydia stimulate polymorphonuclear leukocytes, the ordinary white blood cells. There are a couple of interesting features about the work with Mengert. We found inclusions in the cervix of women who did not have babies, who had gynecological problems. One of the younger gynecologists, Dr. Randall, was doing a dilation and curettage on a young woman, and a spurter developed. The spurter hit him in the right eye. So he immediately washed out the eye and put in argyrol. Five days later he came down with a terrific inclusion conjunctivitis in that eye. That woman was still in the hospital, so we went back and found good inclusions in her cervical epithelium.

This case stimulated our work on the genital side of chlamydia. We made several studies on epidemiology and the chlamydia relationship to swimming pool conjunctivitis. While sulfanilamide does not have any effect on the elementary bodies, chlorine has a remarkable effect. So chlorinating a swimming pool knocks out chlamydia. The old-fashioned swimming pool conjunctivitis of chlamydia origin (adult inclusion conjunctivitis) disappeared. I haven't seen a case in twenty years, all because of chlorination.

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Thygeson: Swimming pool conjunctivitis was very common in Europe in the public swimming baths, as they called them, public swimming pools where they had no chlorination. So there was plenty of chance for genital chlamydia to come into the water by urination and other ways. The chlamydia survived either as free elementary bodies or as inclusion bodies in the desquamated cells. Then chlorination destroyed them all. But then the swimming pool conjunctivitis recurred again but in another form that turned out to be due to adenovirus type 3. Chlorine has no effect on adenovirus 3. So we still have swimming pool conjunctivitis, but it's an adenovirus type.

Hughes: *Not really the same disease?*

Thygeson: No, different symptoms, everything.

Hughes: *Do you care to say anything else about the Iowa period?*

Thygeson: Well, we were always interested in the relationship of gonorrhea and chlamydia because they went together, both venereal diseases. We found combined cases where there were two together or each one separately.

One interesting case which occurred was the illegitimate child of a very young woman where the grandfather was the father of this baby. She had gonorrhea, which we knew. We were prepared for the baby so we could give her silver nitrate prophylaxis which knocks out the gonorrhea. But the baby came down with inclusion blennorrhoea. So we had gonorrhea cervicitis in the mother, and we had inclusion conjunctivitis in the baby, a girl baby.

But about three weeks after birth—we were able to hold both mother and the baby in the hospital—the baby came down with gonorrheal vaginitis (later spreading to the eye) on top of the inclusion blennorrhoea. This was of interest because of the cell types. Inclusion blennorrhoeal chlamydia affect only one type of epithelium. If there's any keratin in the epithelium, it won't support chlamydia, nor would it support the gonococcus. So what happened in the baby was a change over of epithelium, an estrogenic change in the three week period, which made the baby susceptible to the gonococci. We then had a gonococcus conjunctivitis and an inclusion blennorrhoea together.

We were interested in just the type of epithelium that would support chlamydia, and it was only found in one part of the cervix, the external os of the cervix, not in the canal. It wasn't found in the uterus at all. We thought this was a very specialized organism that would only attack certain types of epithelium like the eye and the cervix. It turned out later that some chlamydia will infect the uterus and the fallopian tubes, but not very often, so it was mainly the cervix.

Hughes: *Did you publish that work?*

Thygeson: Yes, we published that. Part of that's in that study with William Mengert.

Hughes: *He was a gynecologist?*

Thygeson: Yes. Then we made a couple of experimental inoculations in the cervix of volunteers. We felt it wasn't dangerous. So with the help of Dr. Everett Plass, who was head of the gynecology department, we obtained volunteers. We made a number of inoculations. We made an experimental infection of the cervix, which didn't tell us much except that they had an induced venereal disease which would have been dangerous to the baby if there had been a baby. But it's self-limited. The length of time was about three to five months at the most for the spontaneous resolution of the disease. We couldn't do that type of experiment on volunteers any more! That's all completely off.

Later on in New York we did all the epidemiologic studies on the parents of the babies with inclusion blennorrhoea.

There was an accidental experiment. We had a gynecologist who was making studies of the bacterial flora of the cervix. He developed a glass rod and a cotton obturator in it attached to a string. He would put this in the cervix, and then he would pull on the string and that would suck in some secretions

that he could use for the study. One day he pulled too hard on the string and the obturator came back and hit him in the eye. He came down with a lovely inclusion conjunctivitis.

Then at Iowa we had a resident who spanked a baby to get him to breathe. He spanked the baby a little too hard or something and got material in his eye. He came down with a good inclusion conjunctivitis. So we had an occupational disease of gynecologists and obstetricians. That stimulated us to do the genitourinary studies that we carried on in New York City.

Hughes: *They hadn't been pursued in ophthalmology before then?*

Thygeson: They had been pursued back in 1908 when they found inclusions in the genital tract of the fathers and mothers of affected babies. So this was an old observation, but nobody had done any work on it in the interim. Old-timers like Karl Lindner knew about the genital aspect, but there were not any studies in the current literature.

Hughes: *Do you remember who did that early work?*

Thygeson: I remember a man named Stargardt Heymann. There were about three people who found these inclusions.

Hughes: *Europeans?*

Thygeson: Europeans. It was because of these findings that microbiologists decided to throw out the inclusions as having any significance [in the etiology of trachoma] since you could find them in diseases that weren't trachoma. So they couldn't possibly be the cause of trachoma!

Hughes: *Well, anything more about Iowa?*

Superficial Punctate Keratitis, 1950 and 1961

Thygeson: There was an interesting disease that we studied in Iowa, which was the superficial punctate keratitis that I had first seen in Colorado. I wrote a paper or two on it.*

Hughes: *The disease was named after you.*

Thygeson: Named after me, unfortunately. It was a new disease.

Hughes: *New in what sense?*

Thygeson: Hadn't been in the literature. It had probably been seen but overlooked. So we wrote it up. I continued to study it later in New York.

Hughes: *What are the symptoms?*

* P Thygeson. Superficial punctate keratitis. *JAMA* 1950; 144:1544-48. P Thygeson. Further observations on superficial punctate keratitis. *Arch Ophthalmol* 1961; 66:158-62.

Thygeson: An irritation of the eyes and a drop in vision. It affects the pupillary area of the cornea and cuts down on the vision temporarily, but it doesn't damage the eye unless it's overtreated.

Hughes: *What is the treatment?*

Thygeson: There isn't any treatment. You've heard of the steroids?

Hughes: *Yes.*

Thygeson: A heaven and hell drug. Well, the steroids have the remarkable effect that they can abolish the signs of this disease but don't cure it, and then it results in a chronic disease that lasts a long time. So steroid treatment in the end is worse than not treating it at all.

Hughes: *Why did you object to the disease being named after you?*

Thygeson: I don't like the use of personal names attached to diseases.

Hughes: *Who was responsible for doing that?*

Thygeson: I think it was Barry Jones who inserted it into the textbook of ophthalmology of [Stewart] Duke-Elder, which everybody read at that time.* That's why everybody refers to it with my name.

Chronic Follicular Conjunctivitis of Axenfeld

Thygeson: There was one other disease that interested us in Iowa at one time, which was the chronic follicular conjunctivitis of Axenfeld. This was a follicular disease that was sometimes confused with trachoma but wasn't trachoma. The etiology is still unknown. It occurs in epidemic form, particularly in orphan asylums. It's a disease of children, and it spreads through a whole orphanage and lasts a year or two but never causes any damage to the eye. We think it should be a viral disease, and we suspect papilloma virus, but we haven't got the proof.

Anyway I studied this at an orphan asylum in Cedar Rapids, where we had an epidemic. We had later epidemics in San Francisco and Arizona.

Hughes: *When there was an epidemic like that, did somebody contact the eye department and somebody was sent out, such as you, to deal with it?*

Thygeson: Yes. We heard about it and so we went.

The disease has an interesting history because it was named after [Karl T.P.P.] Axenfeld, who was a very great pioneer ocular microbiologist. His colleagues said that these cases, then known as orphans' conjunctivitis, were actually

* S Duke-Elder, AG Leigh. *Diseases of the Outer Eye*. In: S Duke-Elder, ed. *System of Ophthalmology*. (15 vols) Vol 8. London: Henry Kimpton, 1965: 743-6.

mild trachoma. Axenfeld disputed this. He inoculated his own eyes with follicles from this disease, and he developed the same conjunctivitis which lasted two years but didn't produce any scars. It wasn't trachoma.

What we did was to show that chlamydia had nothing to do with the disease. Then we inoculated monkeys, and we couldn't transfer the disease to monkeys at all. Then at the same time there was a Jewish ophthalmologist in Palestine who was interested in this disease and who inoculated himself, his wife, and his office girl with scrapings from this disease. They all three of them came down with typical disease which lasted about a year and a half and got well without any trouble. So there was pretty good evidence that we were dealing with an unknown viral disease which can be confused with, but isn't, trachoma. It's open for grabs if somebody can work out the etiology. I'm pretty sure that it ought to be papilloma virus.

Hughes: What makes you think that?

Thygeson: Because of the duration. The wart has about a two-year duration and involves the epithelium primarily. This Axenfeld conjunctivitis is really an epithelial disease where the follicles are secondary to the epithelium. So it's open for grabs.

Hughes: Do you mention the disease in your seminars?

Thygeson: We have been, yes. I have good pictures.

Mrs.

Thygeson: Did you talk to Sally about your work on staphylococcus?

Hughes: No. [Discussed below]

Mrs.

Thygeson: I heard somebody say that was the most valuable thing you've ever done. You probably don't feel that way.

Thygeson: That was all done at Columbia.

Research with Monkeys

Hughes: You mentioned monkeys. Was there a monkey colony at Iowa or did you have to order monkeys?

Thygeson: We had to order them, but we used mostly sphinx baboons because they were the easiest to obtain, and they were more susceptible to chlamydia infection than the rhesus or other monkeys. They cost about twenty-five dollars apiece. Now they cost several hundred dollars apiece.

Mrs.

Thygeson: You can't use them at all now, they're so expensive.

Thygeson: The baboon was much more susceptible than the ordinary monkey, just as the Algerian magot in Tunisia was more susceptible than the rhesus monkey. The big difference is susceptibility. The apes are much more susceptible than monkeys to all the chlamydia.

Hughes: *Do you have an explanation for that?*

Thygeson: No, I don't. I guess they're more like humans.

I told you about my experiments with the sooty mangabee monkey?

Hughes: *Yes, but not with the machine on.*

Thygeson: We had been using baboons and sometimes cytomologous monkeys. We got a new sooty mangabee of a type that I had never worked with before. I didn't know that these monkeys had needle teeth instead of flat teeth like the baboons. So I had my leather gloves, and I went to grab this monkey, but the monkey grabbed my index finger and took off my nail. The bite went right through the leather. I had a bad time with paronychia infection.

The interesting thing was the week before Albert Sabin had reported the death of a young investigator who had been bitten by a monkey who had come down with *Herpes simiae* encephalitis and died of encephalitis just a week before the monkey got me. So after this I was wondering when I was going to come down with my encephalitis. But I never did, and I found out that the type of monkey I was working on did not have *Herpes simiae*. Not all the monkeys have this herpes virus. So I really was safe, but I didn't know it.

Trachoma

Hughes: *On to another disease which has played a significant role in your life, trachoma. What was known when you came into the field in the late twenties?*

Thygeson: That was about '28. When I came into it as an intern, what stimulated me was that there was so much trachoma in Colorado. It was a follicular disease—bumps on the conjunctiva. We would take ring forceps, anesthetize the conjunctiva, and we would squeeze out the follicular material. It was thought that the follicles were the center of the disease at that time.

Hughes: *You mean an infection of the follicles?*

Thygeson: Yes. We thought the organism would be in the follicles because the follicles were so obvious, and that's where the scars occurred—right in the follicles. The treatment was expression of follicles, and it was remarkably effective. Just why, I don't know. Probably because it brought blood into contact with the infected epithelium. Anyway, it worked.

Hughes: *The white blood cells you mean?*

Thygeson: Yes, everything in contact with the epithelium where the organism was. Chlamydia is a surface organism in trachoma.

Hughes: I see. So that would result in cures?

Thygeson: Well, it had a very remarkable effect in putting them into remission rather than cure. I realized that trachoma was a terrible eye problem. Number one.

Mrs.

Thygeson: Number one in the world.

Hughes: Now?

Thygeson: It was then. It's still important in the world, but it was number one then.

Hughes: Was it mainly the Indians that were infected?

Thygeson: Well, in the United States about fifty percent of the Indians had active trachoma. Like the Egyptians, everybody got trachoma. But sometimes the disease was self-limiting and produced very little damage. Sometimes it produced terrible damage.

Anyway, the other area of trachoma in the United States was the Daniel Boone Trail.

Hughes: In the Appalachians?

Thygeson: Yes. In my early days, about seventy percent of all trachoma in the United States was in the Scotch-Irish, those mountain people along the Daniel Boone Trail.

Hughes: Was that because they were living in an isolated, closed community?

Thygeson: Well, poverty was a factor. They got it from the colonists in West Virginia and Virginia. As they came across the Appalachians, they carried the trachoma with them.

Hughes: What did these people, both the Indians and the Scotch-Irish, do about it?

Thygeson: The Public Health Service moved in, and they did two things. One, they excluded all immigrants with trachoma at Ellis Island. Anybody with trachoma they would send back.

The other thing they did, they had six public health hospitals for the treatment of trachoma. The treatment was mainly correction of lid deformities. With trachoma there are scars and the lids get turned in and the lashes scratch the cornea. So mainly it was correcting the deformities produced by trachoma. There were six hospitals all through Appalachia. The last one to close was in Raleigh, Missouri, in the Ozark Mountains.

Hughes: When did the Indian Service get into the act?

Thygeson: The Indian Service was always interested because trachoma was the number one eye problem, but they didn't have any means of control. They had general practitioners; they didn't have specialists.

Hughes: *That was because the pay was poor?*

Thygeson: Yes. There wasn't any money, and it was under the Department of the Interior, not the Public Health Service. It wasn't until the Public Health Service came in that any progress was made.

Hughes: *Why the change to the Public Health Service?*

Thygeson: Well, they realized that trachoma and tuberculosis were the two big diseases and they could not be taken care of adequately by a general practitioner. So the Public Health Service with more specialized people came in.

Hughes: *The Public Health Service had ophthalmologists?*

Thygeson: No, but they had practitioners who were interested and had received special training. They didn't have any honest-to-God ophthalmologists.

For instance, they had one who was in charge of the trachoma program, Dr. Polk Richards, who could do cataract extraction; he could do all the surgical procedures on the lid, and he could do almost everything. He was a jack-of-all-trades. He could remove gall bladders or remove teeth. He could do anything. He was just a natural, all-purpose doctor. He was the first man I ran into in the Indian Service. He had trachoma himself.

Hughes: *Because of contact with the Indians?*

Thygeson: Yes. He had an accidental infection. He was doing the operation of squeezing follicles. Some of the material went into his eyes. Five days later he came down with a violent trachoma.

Hughes: *Tell me what was known about the etiology of the disease in the late twenties.*

Thygeson: Actually, the etiology should have been known because the inclusion body had been discovered, but nobody accepted it.

Hughes: *That discovery was by [Stanislas J.M. von] Prowazek.*

Thygeson: Yes, that was 1907 when the inclusions were first seen. But they were not accepted as the cause because they were found in nontrachomatous diseases, as in the genital tract and in the eyes of babies that were not trachomatous. This eye disease turned out to be inclusion conjunctivitis. So that threw everybody off.

The microbiologists, who were never any good on morphology... That's a funny thing about the professional microbiologist: he's very good on cultures, but he's no good on the microscope. They didn't realize the importance of the inclusions, so it was in innocuous dissuetude at the time I started work.

Then [Hideyo] Noguchi, the great microbiologist from the Rockefeller Institute, was persuaded by Dr. [Francis I.] Proctor to go out to New Mexico and Arizona and find the cause of trachoma, which was then considered to be unknown. So Noguchi came out and found a small gram negative bacterium in trachomatous eyes. He inoculated monkeys and produced what he thought was experimental trachoma. But it wasn't. It was an activation of a natural follicular disease of the monkey. It didn't have anything to do with trachoma.

Hughes: Do you remember when that was?

Thygeson: He published his gold medal study on trachoma in 1928. He received the gold medal of the AMA for finding this organism, which wasn't the cause of trachoma.

Hughes: He didn't have a very good record, did he?

Thygeson: Poor old Noguchi died of yellow fever.

Mrs.

Thygeson: He had a rough time with his experimentation.

Thygeson: So we started in on *Bacterium granulosis*. We found *Bacterium granulosis*. We inoculated monkeys, and we found the same follicular disease that Noguchi had found, because all the monkeys had this spontaneous disease that would be activated if you did any manipulations on the eyes. Then we, Dr. Finnoff mainly, arranged through a blind organization in Denver to get blind volunteers. So we inoculated a number of blind volunteers and paid them a certain amount of money. We inoculated them with *Bacterium granulosis*, and we didn't get any trachoma. So it became pretty evident that *Bacterium granulosis* was not the cause. So then we started working on the inclusion body deal.

Hughes: Had Prowazek thought that that was the cause of trachoma?

Thygeson: Yes, [Ludwig] Halberstaedter and Prowazek. Halberstaedter became a radiologist. He was not a high-powered microbiologist. Did you hear the story of how the inclusion bodies were found?

Hughes: No.

Thygeson: It was interesting. Have you heard of [Max] Neisser, who discovered the cause of gonorrhoea?

Hughes: Yes.

Thygeson: Well, Neisser was working down in Java on gonorrhoea, and he had with him two assistants, Halberstaedter and Prowazek. They became interested in trachoma, but Neisser wouldn't let them work on the disease. It would interfere with the gonorrhoea study. So on the side they surreptitiously started studying trachoma. What they did was to inoculate orangutans with trachoma scrapings. They got an experimental disease that was a real

trachoma. In these orangutans they found intracellular bodies in the epithelium which they thought were like those of malaria, which is also [characterized by] intracellular bodies in blood cells.

They thought that these bodies were the cause of the disease trachoma. They named them chlamydozoa. They named them chlamydozoa because they resembled mantled bodies. They were little red dots surrounded by blue protoplasm and were a little bit like the malaria parasite.

Hughes: That was the staining, of course?

Thygeson: Yes, that was the giemsa stain. They thought that these were the cause of trachoma. This stimulated quite a bit of interest. Other people said they found these inclusions in vernal catarrh and hog cholera, which they didn't. It was a mistake. But they were found in the genital tract of the mothers and fathers of babies with ophthalmia neonatorum, which was nongonorrheal. That was inclusion blennorrhoea. But then all the microbiologists said, if you find these bodies in nontrachomatous conditions, they can't be the cause of trachoma. That was the situation when we went in. We knew about the inclusions, but they were not given any credence as the cause.

Hughes: People wouldn't think that they were viruses because you could see them in the microscope?

Thygeson: Well, actually Prowazek thought they were viruses in the form of little elementary bodies, small bodies which were part of the inclusion, and were about a quarter of a micron in size, about the same size as the elementary bodies of smallpox and vaccinia. Prowazek was correct in recognizing that these little bodies were the active agents. The blue stuff was just a developmental stage. So he was correct, but he thought they were really viruses like smallpox, but they really weren't. They turned out to be bacteria, small bacteria.

Hughes: But parasites, are they not? They have to develop within the cell?

Thygeson: They have to be in the cell, so they're strict parasites, and they only attack epithelium, so they're very selective.

Hughes: Are they completely distinct from the rickettsiae?

Thygeson: They bear a certain relationship. I don't know what their relation is in classification. They are a completely distinct group of agents. Like rickettsiae, the chlamydia are distinct. They form a new group of micro-organisms.

Hughes: Can you put a year to the recognition that it really was a distinct organism?

Thygeson: Well, in the 1930s we recognized that they were distinct. We did filtration studies, and they would pass filters, like viruses.

Hughes: Did you initially think they were viruses?

Thygeson: Yes. We thought they were viruses because they passed filters and because they had inclusion bodies, which are characteristic of many viruses.

Hughes: *And they also had to develop within the cell.*

Thygeson: Yes, just like viruses. But it turned out that they were more like rickettsiae, which are bacteria. They're intracellular, but the rickettsiae don't form inclusion bodies. They're very small. They don't pass filters because they're not cocci; they're rods, and so they don't pass the filters like the chlamydia do.

Hughes: *Did they go through at the right angle?*

Thygeson: No, it just didn't work. They just were nonfilterable, but chlamydia were. I think it's just the size, the shape—that the sphere passed the filters that a rod would not.

Hughes: *They were difficult to culture, were they not?*

Thygeson: Terribly difficult to culture, and here we failed. We had spent a whole year trying to cultivate them. We had a laboratory out at Fort Apache [Arizona]. I spent a whole winter working on tissue cultures getting the technique of cultivating conjunctival and corneal epithelium.

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Hughes: *In those years at the University of Colorado, you were going to several Indian reservations. Were they right there in the vicinity of Denver?*

Fort Apache, Arizona

Thygeson: Well, we had trachoma in Denver, but it gradually kind of dropped out, so we went down first to the Navajo area and then to the Apache area—New Mexico, Arizona. We did most of our work with the Apaches at Fort Apache.

Hughes: *Had Fort Apache been set up by the Indian Service?*

Thygeson: It was set up actually by the Public Health Service at Dr. Proctor's request. So I had teamed up with Dr. Proctor and Dr. Richards right away.

Hughes: *Maybe you should bring in Dr. Proctor and how that came about.*

Thygeson: Well, Dr. Finnoff and I wrote an article describing the isolation of *Bacterium granulosis*.^{*} Since Dr. Proctor had worked with Noguchi, he immediately was interested, and so he came up to Denver. That's where my tie-in with Dr. Proctor came from. That's where we made those inoculations with *Bacterium granulosis*. We later went to Egypt, and we inoculated cases there.

Dr. Proctor and I were convinced that *Bacterium granulosis* was not the cause of the disease. That's when we started going after the inclusion bodies.

* WC Finnoff, P Thygeson. The finding of *Bacterium granulosis* (Noguchi) in trachoma. *Am J Ophthalmol* 1929; 12:651-2.

Hughes: How were you finding time to do this?

Thygeson: I had a remarkably good chief, Dr. Finnoff. He was so interested in research that he gave me time off to do all these things. An ordinary doctor wouldn't do that, but Finnoff was interested.

Hughes: Describe, if you would, the setup at Fort Apache.

Thygeson: Well, originally Fort Apache was the headquarters, I think, of the Fifth Calvary. It was a wonderful frontier post. It was the principal area from which the Apache wars were fought. It was the headquarters there of all the military action against the Apaches. They had a wonderful fort and all these fort buildings there. What Dr. Proctor did was to have this declared a trachoma reservation. The old barracks and school were used to house trachoma children. They were treated by the known methods then, mainly copper sulfate.

Hughes: The children actually lived there?

Thygeson: Yes. So we had all this material. Then we set up the hospital as a laboratory. The Indian Service, after some delay, provided us with wonderful laboratory equipment—incubators—and one of the Rockefeller Institute technicians to help us on tissue culture.

The Rockefeller Institute, Winter 1937

Thygeson: That's where I learned tissue culture techniques myself.

Hughes: Do you remember the winter?

Thygeson: Well, it was probably '37. I had worked down there at Fort Apache every year.

Hughes: At that stage you were at Columbia?

Thygeson: I was at Columbia. I got permission to spend a winter with [Alexis] Carrel, which I did. (I had a wonderful job at Columbia, incidentally. I was a real free lance.)

Hughes: Alexis Carrel was quite a controversial figure.

Thygeson: He was a real character. He was really pro-Nazi. He thought the Germans were the coming race.

Hughes: He thought so when you knew him?

Thygeson: Oh, yes, and he spouted it off every noon at lunch, much to the disgust of a lot of people. He was controversial but was a real pioneer in tissue culture. He did much of the pioneer work.

Hughes: Were you learning from him?

Thygeson: I was really learning from his first assistant, A.H. Ebeling, a very fine Ph.D., a super technician. He was the one who taught me.

Hughes: *What was Carrel doing in those days?*

Thygeson: Well, he was mostly political. He was mostly talking and writing. He seldom came to the laboratory except for the weekly lectures. Well, much more often than that, because we often had lunch with him. I got to know him pretty well, but he wasn't my teacher.

I got so I could grow human epithelium from the eye, which I guess had been done before but never done in quantities the way we did it.

Hughes: *That was useful as a research tool?*

Thygeson: What we thought was that in the trachoma agent we had an epithelial parasite which in vivo would only grow in the epithelium of the conjunctiva or cornea. We thought it was only parasitic for the eye, only for the conjunctiva and the cornea of the eye. So we grew conjunctival and corneal epithelium in tissue cultures, on which we were going to grow the organism. Well, the only trouble was that the organism didn't grow. It would actually grow for one generation, which was about two days, and then die out. So we never could keep the agent going.

Hughes: *Were you able to keep the epithelial cells going?*

Thygeson: Yes, indefinitely.

Hughes: *That was no problem?*

Thygeson: No problem. We had a very good technique.

Hughes: *Were you actually growing the epithelium at Fort Apache?*

Thygeson: Yes, but of course I had done it at Columbia too, and we had our own laboratory there. But it was mainly at Fort Apache. So this whole summer we were inoculating all these tissue cultures but not getting results. We were also using the egg. You know the egg has a beautiful epithelium in the chorioallantoic membrane. We were inoculating the chorioallantoic membrane and occasionally the yolk sac. Well, it turned out that the yolk sac was the proper place. I didn't realize it because I thought this agent was a strict epithelial parasite and wouldn't grow in anything except epithelium. But actually it would grow in the egg in the yolk sac.

Hughes: *Which is endothelium?*

Thygeson: Yes. The other thing I didn't know—nobody told me—was the importance of blind passages, where if you didn't get a culture at first, then you would transfer the material again a second or third time, and then it would come out in about the third or fourth passage. I didn't know that.

Hughes: *Why would it take that long?*

Thygeson: Because it was in small numbers. The numbers increase until you can get a manifest culture. I didn't know that. But when the organism was finally cultured for certain, it was done in China by T'ang and a group.* T'ang was trained at Harvard and in London. He actually made the first bona fide culture in the yolk sac of the trachoma chlamydia. Before that we knew that this was the cause because we had filtered the elementary bodies and then used the elementary bodies to produce trachoma in a volunteer. So we had the answer before the organism was grown.

Hughes: *Did you come into contact with Thomas Rivers while you were at the Rockefeller Institute?*

Thygeson: At noon we all lunched in this big room, so I got to meet Rivers. I wrote a few chapters in his book on virology.**

Hughes: *What sort of a man was he?*

Thygeson: A delightful fellow. A real student. He was interested in the eye because he had found Rift Valley fever virus in the retina of a human eye. He was oriented to the eye so he was interested in me in the sense that I was working on an eye problem.

Hughes: *What was he working on when you were at the Rockefeller?*

Thygeson: He was working on viruses. He had a study on tobacco mosaic virus; he had two or three problems going.

Hughes: *How were these lunches arranged?*

Thygeson: They had a big lunch room, and all the staff met. We had two big tables. Carrel took command of one table, and it was Rivers who really took command of the other table. They brought up all kinds of interesting projects.

Did you ever hear of Peyton Rous?

Hughes: *Yes.*

Thygeson: He had a lot to say about tumor virology. Then there was a young veterinary virologist from Iowa who had done remarkable work on papilloma virus in rabbits.

Hughes: *Do you remember his name?*

Thygeson: Yes, Richard Shope. He was the leading light at the Rockefeller-Princeton unit. Richard Shope found this virus in rabbit papilloma. This was so interesting because a certain number of these papillomas became malignant, so here was a question of viruses and tumors. Shope made quite a name for

* T'ang, HL Chang, YT Huang, KC Wang. Studies on the etiology of trachoma with special reference to isolation of the virus in chick embryo. *Chinese Med J* 1957; 75:429.

** "Trachoma" and "Inclusion Conjunctivitis." In *Viral and Rickettsial Infections of Man*. Edited by TM Rivers. Lippincott, 1948, pp. "Trachoma and Inclusion Conjunctivitis." In *Ibid.*, 2nd ed., 1952, pp.

himself, and then he up and died as a fairly young man. He had a wonderful future in front of him if he hadn't kicked off. He was a University of Iowa boy, and we had something in common because I had had those years at Iowa.

Hughes: Anybody else you can think of at the Rockefeller?

Thygeson: Well, there were half-a-dozen others who were of note who were working on different projects, but those I have mentioned were the ones that I really got acquainted with.

Hughes: Did you find that very exciting to participate in these lunchtime discussions?

Thygeson: Oh, yes. I didn't like Carrel's talk.

Hughes: Did you try not to sit at his table?

Thygeson: Yes, I usually got out of it. But Carrel was a great investigator. His politics were just bad.

Hughes: Did you overlap with [Charles Augustus] Lindbergh?

Thygeson: Lindbergh developed the pump (artificial heart) which made tissue and organ culture possible. I came to the Rockefeller the week after Lindbergh had left. So I inherited Lindbergh's cap and gown. There was a lot of talk about Lindbergh. He made a big splash at the Rockefeller Institute. I never met Lindbergh, but I had his cap and gown.

Trachoma (continued)

Fort Apache (continued)

Hughes: Do you think you've said enough about Fort Apache and the trachoma work that went on there?

Thygeson: I think we learned a few other things about trachoma at Fort Apache when we had these children who were given a daily treatment. Dr. Proctor devised three types of treatment. One was the classical copper sulfate. Copper sulfate was used back in Egyptian times. And we used silver nitrate, which was a favorite eye remedy at the time. And we used chaulmoogra oil, which was once touted as a cure for leprosy, which it never was. It was applied to the conjunctiva and then brushed in with a cotton swab. We tried these methods with three groups of children every day and compared the results. About twenty percent of the children were cured every year by each of the three methods. So it was obvious that there wasn't anything specific.

Hughes: Do you think it was just nature taking its course, or was the medication doing some good?

Thygeson: It was doing some good. I worked on what it did, which I think was interesting. The inclusion bodies, the chlamydia, live in the surface epithelium. They don't infect the bottom layers of cells in the epithelium. The bottom layers seem to be immune to the chlamydia. They don't support the chlamydia. What we were doing was knocking off the surface layers of epithelium and causing the production of resistant cells. So we were curing the disease by improving the epithelium.

Hughes: *Do you have any idea why they wouldn't live in the lower epithelium?*

Thygeson: No. We just know that chlamydia love the surface epithelium, whereas herpes virus loves the deep epithelium. It's just a peculiarity.

Hughes: *Did you publish that as well?*

Thygeson: I've taught that, but I never published it.

Hughes: *I ran across the term "White River Agency" in connection with Fort Apache.*

Thygeson: Fort Apache was a unit in the White River Reservation. They're the White River Apaches. There are two main divisions of Apaches, the desert Apaches and the mountain Apaches, the White River Apaches.

Indian Lore

Hughes: *Were you at this stage already interested in Indian lore? I know you're very interested now.*

Thygeson: Oh, yes, I always have been from childhood. I took every advantage of getting all the oral history I could.

I remember at Fort Apache the army hadn't pulled out very long before. The adjutant had a little cottage. I went in there, and I found all the old regimental records. Among them I found a write-up on the Cibaque Massacre, which was quite a famous Apache massacre. I don't know what ever happened to those books. I should have saved them. Historically they really told the history of this post.

Several of Geronimo's wives were still living on the reservation, and there were quite a number of scouts still surviving who had worked with [George F.] Crook. The Indians loved to scout for the army. Some Indians were especially good, like the Crow, who were [George Armstrong] Custer's scouts. There was something about the army that they liked. These Indian scouts were well trained and famous. There were quite a number of them still living on the reservation. They had all been given medals. They wore their medals every day. Some of them could speak English, and so I would talk to them and get the history of where they had been.

Hughes: *They accepted you readily?*

Thygeson: Oh, yes. The younger Indians nowadays don't, but the older Indians did. The interesting thing is, they're like the Japanese. When they got licked, they accepted it. That was it. They didn't harbor any grudges. It was a part of warfare. If you're licked, you're licked, so you might as well accommodate yourself. So they were very pleasant.

The other interesting thing was that they considered that the Public Health Service, like their schools, belonged to them and to theirs. So the doctors belonged to them.

We were very careful with the medicine men, who were very important in the tribal life. We got along with the medicine men. The medicine men accepted us, too, as fellow doctors. They were psychiatrists. So we made sure always to consult with the medicine men.

Hughes: Was Apache medicine primarily psychiatric medicine? They didn't have an herbal lore?

Thygeson: The Apaches had almost no herbal medicine, but the tribes varied greatly. The Chippewa had a wonderful herbal medicine. There were some of the Indian tribes, like the Chippewa, that had two types of medicine men. There would also be medicine women. They had the psychiatric type, which was all the chants and everything. Then they had the herbal. They had two separate kinds of doctors. The herbal ones were mostly women in the Chippewa.

Hughes: Was there an understanding that once they came into the laboratory that was your terrain?

Thygeson: The Indians were wonderful patients. They were stoic, and they never complained.

Hughes: Even the children?

Thygeson: The children very seldom cried. White children would have gone crazy.

Hughes: How did you do these scrapings?

Thygeson: Well, we anesthetized the cornea, and then we used a platinum spatula. We scraped the surface epithelium.

Hughes: Was it painful afterwards?

Thygeson: No. It was mainly the idea of having your eye scraped.

I never had any unpleasantness with any of the Indians in all the fifty years I spent with them. One time I ran into a southern Cheyenne medicine man up in Montana who had come up to visit his relatives in northern Cheyenne. He took about five minutes to blast the white man and various things the white man had done wrong. This was the first time that I had ever heard such a tirade. He was quite pleasant about it. He just wanted us to know the sins of the white man, and he knew all the sins.

Hughes: When he recounted all the sins, was he then satisfied?

Thygeson: Yes. He wasn't vicious about it. He just wanted us to know.

Hughes: He was right, wasn't he?

Thygeson: Yes.

Hughes: From what you're saying, I gather the patients were mainly children. Was that true at Fort Apache?

Sulfonamide

Thygeson: They were the ones that had the very active trachoma that we used for experiments. The older folk had milder trachoma, cicatricial trachoma, that didn't have the same number of inclusions. We had this sulfonamide program for the older ones, but they weren't much use for the laboratory studies. It was the children who were good subjects.

Hughes: How successful were the sulfas in the treatment of trachoma?

Thygeson: Wonderful. This was a fabulous victory for sulfanilamide.

Hughes: In the early days it was difficult to get penicillin. Was that also true of sulfanilamide?

Thygeson: No, sulfanilamide was easy to make so there wasn't any problem.

Hughes: The drug companies were doing it by then?

Thygeson: They were producing it. I've forgotten where we got our sulfanilamide, but we had no problem getting sulfanilamide.

Fred did this experiment, and then he told us about it. We were at Fort Apache, Dr. Proctor, Dr. Richards, and I. We did the same thing at Fort Apache, and we obtained an eighty-five percent cure rate.

Hughes: Had the dose been worked out at that time?

Thygeson: Pretty well, yes. We had to have blood levels. We had to have about five milligram percent in the blood. That was a therapeutic level.

Hughes: You worked that out by trial and error?

Thygeson: Yes. The standard dose was four grams a day for an adult, reduced on children.

Hughes: For trachoma? That dose wouldn't apply to other diseases, would it?

Thygeson: No, for trachoma. They became the first two experiments with sulfanilamide in this country, but studies were also done in France and in Indonesia. There were about three reports that came out at about the same time. Fred Loe's was the first, and then Dr. Proctor's and ours. These others came in about the same time.

Someone worked out why the sulfanilamide worked. It competes with a substrate for the chlamydia. What it does is to stop the developmental cycle. It doesn't kill the chlamydia at all. It just prevents their multiplication. What you did to prevent the multiplication of chlamydia was in the eye and to wait for the natural desquamation of the conjunctival epithelium containing the chlamydia. When all the agent was desquamated, gotten rid of, you had a cure. It took about three weeks to do this.

Hughes: And the substrate is a chemical substrate?

Thygeson: Well, sulfanilamide competes with a chemical necessary for the reproduction of chlamydia, para-aminobenzoic acid. When the sulfanilamide gets in, the life cycle doesn't work, so there's no reproduction of chlamydia.

Hughes: Who worked that out?

Thygeson: I don't know who did that, but it was a very nice piece of work.

Hughes: Was it about that time?

Thygeson: About that time, yes. One experiment was done by Louis Julianelle in St. Louis, in which he took very active trachoma material and placed it in sulfanilamide and left it there overnight. Then he inoculated a monkey. He got trachoma in the monkey. The sulfanilamide didn't kill the chlamydia at all. It became very evident that sulfanilamide worked by some other method than by killing the agent.

Hughes: Do you think that was the first example of a drug working in that manner?

Thygeson: I really don't know about that, but it soon became evident that's the way sulfanilamide worked.

Transmission to a Human Volunteer

Thygeson: Mr. Brown was a patient of the department at Iowa, who had lost an eye by neglect. This was a sad story. He had come into the department with this tumor on the eye. It turned out to be an epidermoid carcinoma, which is the worst kind of carcinoma, on his bulbar conjunctiva. A biopsy was taken, but he was never notified—it was a mistake—about this result. It was a year that he let the tumor develop. When he came back it had spread into his orbit. Then the eye had to be removed, and the tumor had spread into the olfactory tract with resultant uncinatate fits. He had these fits where he would get all these bad smells.

So we figured he didn't have long to live but that he might live long enough for the experiment. So we gave him the story of what we wanted, and it interested him. He knew he was going to die, and so he volunteered. Dr. Proctor gave him \$1,000. I took him down to Fort Apache where we had the laboratory for the trachoma patients.

I thought he was going to die on the way down there. He scared me all the way. But we got down there. We selected fifteen Indian children with very severe trachoma. We got all this harvest from them. We scraped off the epithelium, and then we ground it all up. We passed it through a course filter to take out the big stuff.

Hughes: You mean the cellular material?

Thygeson: Yes. We let the small stuff go through this collodion filter.

Hughes: Which you hoped were the elementary bodies?

Thygeson: Yes. Then we divided the filtrate into three parts. One part we sedimented in an ultracentrifuge, and we looked at it, and we saw the elementary bodies in the sediment. One part we cultured for bacteria. One part we inoculated Mr. Brown with. We did it all on one afternoon. Then we took off by train to come right back to Iowa City. We got back all right, and then five days after the inoculation Mr. Brown came down with a violent acute trachoma loaded with inclusion bodies.

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Fort Apache Trachoma Research Laboratory, 1935.

l to r: Dr. Thygeson, Clarence Brown, Dr. Proctor, Estelle Peterson, a nurse stationed at Fort Apache, Dr. Polk Richards, a trachoma physician who worked for the Indian Service, and the chief nurse stationed at Fort Apache who was nicknamed "Yazzi" by the Indians. This photo was taken after Mr. Brown was voluntarily inoculated.

Thygeson: Well, in six days I could see the beginning of the pannus. In six weeks the pannus had come down into his pupil.

Hughes: Is that what blinds?

Thygeson: Yes. So we had no problem with his diagnosis. It was confirmed by Drs. O'Brien and Leinfelder. No bacteria got through the filter. He had no bacteria in his disease. But he had a classical acute trachoma. So we started treating him every day with the copper sulfate, which was the only treatment we had at that time. He actually got well in a year of copper sulfate treatment, which was an interesting experiment in itself. Then he was killed in an auto accident.

Hughes: Very soon thereafter?

Thygeson: Yes. He and his wife were killed when he was for all practical purposes cured.

Hughes: And the cancer was in remission as well?

Thygeson: Yes. We expected him to die all that time, but he died of an automobile accident.

Hughes: What was the reception of the ophthalmological community to this work? Did it create a stir?

Thygeson: Well, it created quite a stir, and we got a medal [from the AMA Section on Ophthalmology] as a result of that. That's why I began to rise from an associate professor to a full professor.

The interesting thing to me was, on my next trip to Fort Apache—I was working on monkeys at that time a nurse came in, and said, "You're on the air." I went to her room where the radio was, and "The March of Time" was on the air. They had abstracted the study we had reported in the *American Journal of Ophthalmology*. I got in there in time to hear myself say, "Mr. Brown, the experiment is a success. You are blind." Of course, he wasn't blind.

Then his own mother happened to hear this thing, and she practically had a heart attack. She hadn't known about this experiment. Then all the Sunday supplement people descended on Mr. Brown, but he wouldn't have anything to do with them. The family sponsors wanted to fly him to New York to appear on their radio program, but he wouldn't have anything to do with it.

Hughes: How had the press gotten ahold of this story?

Thygeson: They read this article that Dr. Proctor, Dr. Richards, and I had written in the *AJO*.*

Hughes: It wasn't actually you saying the words?

* P Thygeson, FI Proctor, P Richards. Etiologic significance of the elementary body in trachoma. *Am J Ophthalmol* 1935; 18:811-13.

Thygeson: No, they made up a scenario which didn't have any basis in fact. I expected to get a lot of raspberry from my colleagues, but the program occurred in daytime so none of my friends ever heard it. I never got any flack from it at all. But it was written up in *The Country Gentleman* by Paul de Kruif.

Hughes: *In those days it was not unusual to use human volunteers.*

Thygeson: Oh, no.

Hughes: *Nobody looked much askance at that?*

Thygeson: No. What we used were volunteers and prisoners. It was very effective. If a prisoner would volunteer, we could either pay him or commute his sentence. So there wasn't any trouble about getting human volunteers. We can't do it now. There were many volunteers who were residents or medical students.

Hughes: *Nobody at that time looked askance at the experiments with Mr. Brown because of the volunteer aspect?*

Thygeson: No. You remember how the yellow fever volunteers were treated as national heroes? Well, there was a little bit of that for Mr. Brown. But he didn't survive too long to get any such recognition.

Hughes: *But he survived long enough to know that the experiment was a success?*

Thygeson: Yes. He survived and showed the whole trachoma picture from beginning to end. I have photos in black and white of all the stages of his disease. It was very well documented. This preceded the culture of the organism, but it was a clear-cut demonstration of the etiology without having the culture. This had been done for other diseases, too, such as leprosy, and they never cultivated the lepra organism. They determined the etiology of hepatitis long before they could grow the hepatitis virus. So you don't always have to cultivate the organism to prove etiology.

Hughes: *Was the year of the experiment with Mr. Brown 1935?*

Thygeson: It was roughly in there. It was in the thirties.

Hughes: *I can probably find that in your bibliography.*

Thygeson: You'll see that article by Dr. Proctor and Dr. Richards and me. That created quite a bit of excitement. It was a very short article, but it was the first experiment of its kind with filtration with Elford filters. Nobody else had ever used those filters in ophthalmology.

Hughes: *What did Elford design them for?*

Thygeson: Well, he was a virologist, and he designed them originally to measure the size of viruses. He was very successful because he showed that vaccinia virus was a very large virus visible to the ordinary microscope and that polio virus was a

very tiny virus, only about six to eight millimicrons, whereas vaccinia was two hundred millimicrons. He measured all the viruses and made a table from the largest to the smallest. Elford did a tremendous piece of work.

We took advantage of the Elford filters. We also measured the size of the elementary bodies by the same method, which came right around a hundred and seventy-five to two hundred millimicrons, average size.

Hughes: So they are big?

Thygeson: Yes.

Hughes: They are visible in the light microscope?

Thygeson: Sure.

Hughes: They would have to be—you talked about staining them.

Columbia University, 1936-1942

[Interview 2: December 5, 1986] ##

Hughes: Dr. Thygeson, in 1936 you accepted an appointment in the College of Physicians and Surgeons at Columbia as an assistant professor in the department of ophthalmology. Would you tell me, please, how that came about?

Thygeson: This resulted from a meeting with Dr. John Wheeler at a dinner given by Dr. W.C. Finnoff for visiting speakers at a special course that was given in Denver. Dr. Wheeler was recruiting for his relatively new eye institute in New York. He offered me an appointment as research director, which sounded very good to me at the time, and so I accepted.

Hughes: Was it unusual to have a research position in ophthalmology at that time?

Thygeson: It was, yes. There weren't many available.

Hughes: Was that what attracted you?

Thygeson: Yes. I had been very happy at Iowa, so in a way I hated to leave, but the opportunity in New York seemed to be better.

Hughes: Were the facilities special?

Thygeson: The facilities were better. There was more money available for research, and the future seemed much better.

Hughes: I understand that at that time it was unusual to have full-time positions in ophthalmology.



Dr. Thygeson at Columbia University, c.1936

Thygeson: Yes, there were only a very few in the country. Ophthalmology was mainly a clinical subject practiced by volunteer ophthalmologists. It was part of the tradition in New York that the ophthalmologist would earn his living by practice in the morning, and the afternoon he would devote to a university or to a free clinic. So there was plenty of help available on a volunteer basis.

Hughes: *Does volunteer mean that there was no salary involved?*

Thygeson: No salary. Most of the clinical work was done that way, without any salary. There were a few research appointments in basic science around the country, maybe half a dozen at the most, and they were mainly at Harvard and Johns Hopkins and Columbia and Iowa. Those were about the only universities that had any full-time ophthalmologists. It was recent, too, because basic research was not a feature of ophthalmology in the early days.

Hughes: *So you were unusual on two counts, first that you by then had more or less decided that research was what you were interested in, and secondly you were interested in infectious disease?*

Thygeson: Yes. Dr. Wheeler obtained for me an endowment for my appointment from the founder of the eye institute, Edward Harkness. So I had an endowment for my salary and the salary of a secretary and a technician and then money for research. There was no federal money or state money at that time.

Hughes: What do you suppose it was that called you to Wheeler's attention?

Thygeson: I guess it was microbiology, because he wanted to have his institute cover all the specialties [of ophthalmology]. So external disease and microbiology interested him in my respect.

Hughes: Had he had anybody doing anything in the field of infectious disease prior to that?

Thygeson: He had a woman who was a bacteriologist, but she had no clinical experience, Dr. Deborah Loratcher-Khorazo. She had good microbiology training, but she had no clinical training whatsoever. Dr. Wheeler needed someone with clinical experience.

Hughes: Would you describe the relationship of the department of ophthalmology and the institute?

Thygeson: The department was in Columbia University, the College of Physicians and Surgeons. It had a very small budget. It primarily functioned on a volunteer basis for teaching medical students. But the eye institute was a different set-up under Presbyterian Hospital. It was the Presbyterian Columbia Medical Center. Presbyterian had all the money, incidentally. Columbia did not have very much money at that time. So the endowment for my salary was in Presbyterian Hospital. All the money that I got, any special grants, I had to go through Presbyterian Hospital, not through Columbia.

Hughes: Do you know the story of why Mr. Harkness gave the donation to the eye institute?

Thygeson: He was a patient of Dr. Wheeler's, and he admired Dr. Wheeler very much, and he decided Dr. Wheeler ought to have an eye institute. I've forgotten the amount of money that was necessary, but there was a very beautiful building right on the grounds of Columbia Presbyterian Medical Center. There was also a neurologic institute and an urologic institute and various other things all in a medical center between 165th Street and 168th Street.

Hughes: The institute had just opened, had it not? I think I saw the date 1933 for when it was founded.

Thygeson: Yes. And then it took Dr. Wheeler quite awhile to get organized, so he was just getting started when I got there. He was still recruiting.

Hughes: You happened to have an appointment at both the department and the institute, but that wasn't true of everybody?

Thygeson: Well, those who had clinical experience could have an appointment in Columbia College of Physicians and Surgeons. So I had an appointment as an assistant professor. There were no professorial appointments in Presbyterian Hospital, but they had another title, visiting ophthalmologist.

There were two separate units which were really affiliated, but the finances were all separate. Presbyterian Hospital really had all the money.

Hughes: Were you teaching?

Thygeson: Oh, yes. I taught medical students. But I had what I might call a roving appointment, because I could leave without any red tape for special work such as in Europe or with the Indians. I did a lot of work with the Indians. I could take my technician and secretary with me. So this was a very interesting and unusual appointment.

Hughes: *Was it your idea to set up the appointment that way?*

Thygeson: Well, I told Dr. Wheeler that would appeal to me. He had no objections. He had plenty of people to teach the medical students while I was gone. I was not essential in that. As far as the research director appointment was concerned, I could be absent for several months without difficulty.

Colleagues

Hughes: *Let's talk about some of the people that were in the institute at that time. Do you have anything more to say about John Wheeler?*

Thygeson: Well, Dr. Wheeler was one hundred percent surgical at the time that I knew him, but he started his medical life as a bacteriologist, so he was interested in my field. In fact, the first paper he wrote was on typhoid fever. He was very much interested in all the basic sciences even though he didn't do any of the work himself.

He was completely different from Dr. O'Brien in many, many ways. Dr. O'Brien, for example, visited my laboratory every day and discussed the work we were doing. Dr. Wheeler only came to my laboratory once, and that was to bring Sir John Parsons to show him the laboratories. Otherwise, any contact with Dr. Wheeler was in his office in the institute, not in the laboratory.

Hughes: *Did you make appointments to see him on a regular basis?*

Thygeson: I didn't make appointments, but it turned out that I had to see him frequently for financial reasons. We also had staff rounds every week. I had some experience in plastic surgery, so he wanted me to go along with him on his plastic cases.

Hughes: *Where had you gotten that experience?*

Thygeson: Dr. Finnoff was really deeply interested in plastic surgery of the lens and orbit, so I had helped him on that surgery.

Hughes: *Did you ever operate with Dr. Wheeler?*

Thygeson: No, I never did. I never actually assisted him. But I often examined his cases before and after they were done and offered some advice.

Hughes: *I understand that he had quite a way with patients, that sometimes he would actually operate in the patient's home. Is that true?*

Thygeson: He didn't do that when I was there. The big thing he did was he operated on the King of Siam.

Hughes: How did that come about?

Thygeson: Well, Dr. Wheeler had the reputation of being the best eye surgeon in the United States at that time, so the King of Siam had heard about him. He came to New York for the operation. I remember he paid Dr. Wheeler \$25,000 for the operation, which was about the same as \$250,000 now.

Hughes: What was the operation?

Thygeson: Cataract extraction.

Dr. Wheeler received a lot of newspaper publicity as a result. A man with cataract from Tennessee wrote to Dr. Wheeler saying, "I seen your ad in the paper." Dr. Wheeler was much amused by this letter, one of many that he received.

Hughes: Some other names in the department—George Smelser?

Thygeson: George Smelser was a very fine anatomist who specialized in sense organs. He was a very fine teacher and an exceptional research man. He did a lot of fine work, a very good man.

Hughes: Then there was a Maynard C. Wheeler.

Thygeson: He was a clinician and interested in [eye] muscles. I didn't have much to do with Maynard [professionally] because I was not a muscle man, but I personally knew him very well.

Hughes: Was he, too, a top person?

Thygeson: He was high caliber, yes.

Hughes: Then there was Karl Meyer, a biochemist.

Thygeson: Karl Meyer was a German-born biochemist who was a very difficult person to get along with because he thought everybody was stealing his stuff. He was very paranoid. He guarded his laboratory findings, wouldn't let anybody come anywhere near. He thought Dr. Smelser was stealing his stuff. My laboratory was in between those two, so I wasted many hours trying to settle that little feud that was going on between them.

But Meyer did some very fine work, such as the discovery of hyaluronic acid in the vitreous. He eventually moved to the department of medicine. Arthritis was really a better field for him than ophthalmology, although he did some very fine work on the vitreous.

Hughes: Wasn't that unusual to have a biochemist in an institute of ophthalmology?

Thygeson: Yes. That was only possible because of the money available through Mr. Harkness.

Hughes: *Was that again Wheeler's idea?*

Thygeson: Yes. Dr. Wheeler wanted to have all the basic sciences as a part of the institute. Smelser was in anatomy, and Khorazo and I were in microbiology, and we had a number of other specialists.

Hughes: *I saw the name Ramon Castroviejo.*

Thygeson: Castroviejo was a young Spaniard who started to work on keratoplasty, corneal transplantation. He had a laboratory, and his first work was entirely laboratory, on rabbits. He developed a very good technique for corneal transplantation at a time when this was a very rare operation. He eventually abandoned any laboratory work and did just clinical surgery. He became the top-notch corneal transplant man in the world at that time.

Hughes: *Then there was—I think he didn't come until the late thirties—Ludwig von Sallmann.*

Thygeson: Yes. I was helpful in getting von Sallmann—we called him Uncle Soli—to come. He was a refugee from Hitler in the sense that his wife was Jewish.

Hughes: *He was German or Austrian?*

Thygeson: He was Austrian. He was the number two man in Vienna under Professor Lindner.

Hughes: *In which clinic?*

Thygeson: He was in the Fuchs clinic, which was number two [the Second University Clinic of Ophthalmology]. He left, and he came in with Dr. Arnold Knapp when the [Herman] Knapp Memorial Eye Hospital was at about the end of its history. They closed the Knapp Hospital, and the money was merged with Columbia.

Hughes: *Why did that happen?*

Thygeson: Herman Knapp, the father, died, and Arnold was in his seventies. There wasn't anybody in the family willing to carry on this quite expensive hospital.

Hughes: *Arnold is the son?*

Thygeson: Yes. I had come to know Arnold very well in Iowa, because he visited us in Iowa a lot in his role as editor of the *Archives of Ophthalmology*. He was interested in my work. Anyway, he, you might say, merged with Columbia, not with the Presbyterian Hospital, because he didn't like Dr. Wheeler at all.

Hughes: *Was that a personality clash, or was that professional rivalry?*

Thygeson: Well, it was a mixture. It was a surgical difference of opinion. Dr. Wheeler touted the extracapsular operation for cataract, and Dr. Knapp brought in the intracapsular operation. They could not see each other's operation at all. Dr. Knapp would not have anything to do with Dr. Wheeler. At Dr. Wheeler's death, then he came back to Columbia where he had once been professor of ophthalmology. He waited until Dr. Wheeler was dead. He knew me, so he set up this Knapp Foundation, which in a way we modeled the Proctor Foundation after.

The interesting feature of it was, it was run by a three-man committee (like our three-man board of governors at Proctor), consisting of the dean, me, and Dr. Arnold Knapp. We had this committee, which was not self-perpetuating.

Then, unfortunately, this foundation came to a bad end. The reason was that Dr. [Willard C.] Rappleeye, the dean, died, and then Dr. Knapp died [1956], and I went in the army air force. The committee evaporated, and so the finances were taken over later on by the eye institute. It just became a research fund, but it doesn't have any entity as a foundation at all now, which is too bad, because it had the same amount of money or more than we had later at the Proctor Foundation.

Hughes: *So it could have been an institute of its own?*

Thygeson: If Knapp had lived or if I had stayed, I think we could have kept it going.

Hughes: *Tell me a little bit about the Knapps. Herman Knapp was Viennese. Am I right?*

Thygeson: Yes. He was a pioneer New York ophthalmologist who had a tremendous effect in New York. He was really outstanding, and he was really the top man of ophthalmology in the area—just like Dr. Jackson in Denver. One of his ideas was that the ophthalmologist should make his living in the morning and devote his afternoon to charitable work.

Hughes: *That's the way it was in Vienna?*

Thygeson: Yes, apparently.

Hughes: *Was he authoritarian as so many of the Germanic school were?*

Thygeson: He was dead when I got there, so I don't know.

His son was also pretty German in that sense. He was very rough with his residents at the hospital, but a wonderful teacher. But he was a dictator. I got along with him very well, however.

Hughes: *Did father and son have particular specialties within ophthalmology?*

Thygeson: Well, the son was a real surgeon and a competitor of John Wheeler's. A different type of operation. The funny thing was that Knapp's type of operation became predominant after awhile. Everybody did the intracapsular operation. But now it has gone back to the extracapsular.

Hughes: Did the Viennese school prize research?

Thygeson: Yes, and that was because of Ernst Fuchs, who was in the same category as Edward Jackson in Denver and Herman Knapp in New York. Fuchs in Vienna just dominated the whole Viennese picture, and his students were top flight. He particularly prized pathologic research. He specialized in pathology. A very good clinician, but every day he did an hour or two of laboratory work, which was unusual in those days. He had a profound influence on ophthalmology all over the world and especially in the United States.

Hughes: He had a lot of students as well?

Thygeson: Yes. They came from all over the world.

Hughes: Von Sallmann was one of his students.

Thygeson: Yes, and he came to the Knapp Hospital, and when the Knapp Hospital closed he came up as the one member of the new foundation, the Knapp Foundation. He took over the laboratory released by Castroviejo, who no longer needed a laboratory because he had become clinical. Then when I left to go in the army, von Sallmann took over my job as research director.

Hughes: What sort of a person was he?

Thygeson: He was an extremely intellectual man. You know how some of the Viennese are—students of music and the arts. He was a mountaineer. A typical Viennese.

Hughes: What was his specialty in ophthalmology?

Thygeson: Radiation cataract and radiation damage. He worked for quite a bit with the Lawrence Berkeley Laboratory people on radiation. He was here in our building many, many times because he was interested in radiation cataracts. [interruption]

This is the Uncle Soli tree. [points to oak] He gave us this tree. He gave us a lot of things here because he stayed with us while he was working at the Lawrence Laboratory. He was our best friend for all those years. I have his gold watch. So he played a big part in our lives.

He became a world authority on radiation effects on the eye and also the effect of heavy metals on the eye, such as silver, and lead, and mercury, and other toxic effects on the eye. He later delved a little bit into infectious disease because he became interested in iontophoresis, which is an electrical means of getting ionizable medications into the eye. The sulfonamides are ionizing. By means of iontophoresis he could get a heavy concentration of sulfonamides into the eye. He worked on that for quite awhile.

Hughes: Was the reason that he was at Lawrence Berkeley Lab because of the radiation-induced cataracts?

Thygeson: Yes. That was what he was working on.

Hughes: *So he was looking at radiation from the standpoint of the harm that it did?*

Thygeson: Yes.

Hughes: *I thought he might have been interested in radiation for treatment of cancer.*

Thygeson: No, he wasn't interested in treatment, although he was pathologically trained. He was a student of Fuchs, and so he knew his pathology very well. He was interested in all the pathological changes caused by radiation, all the different types of radiation. He was also interested in pigmented tumors from the pathologic side. So he had a tremendous bibliography. He was a real research man.

Hughes: *Do you know if he had any part in the studies that followed the dropping of the bombs on Hiroshima and Nagasaki?*

Thygeson: He was in on that in some way.

Hughes: *Do you think he was a part of the Atomic Bomb Casualty Commission?*

Thygeson: I know he went over there for studies, but he was not as active in that as David Cogan from Harvard. David Cogan was very active in the Nagasaki and Hiroshima studies.

Hughes: *Cogan was an ophthalmologist?*

Thygeson: Yes. He was the director of the Howe Laboratory at Harvard. He was very much interested in radiation cataracts, too.

Hughes: *Was there any treatment for that?*

Thygeson: Remove the cataract surgically. You can prevent it by proper screening.

Hughes: *In the early days, they didn't know much about the necessity of screening.*

Thygeson: No, and, of course, all the atomic physicists were at risk for cataract. Quite a few of them developed cataract. I saw some. I didn't operate on them, but I saw them.

Hughes: *As I remember, Lawrence Berkeley Lab has a pretty good record in terms of cataract; other laboratories had much more problem with cataract.*

Thygeson: Yes, I think that's true.

Hughes: *Where did your patients come from?*

Thygeson: Well, they were workers over there in Berkeley. There were other cataracts, too. You remember the klystron tube?

Hughes: Yes.

Thygeson: Anyway, cataracts are produced by the intense radiation from the vacuum tubes used in radar. So I saw a number of those.

Hughes: *Another name that I came across in connection with the institute at Columbia was Algernon Beverly Reese.*

Thygeson: He was in charge of pathology. He was a very fine pathologist and clinical ophthalmologist. He was the first man recruited by Wheeler for the new institute.

I remember a funny story about him. He had a rival at the New York Eye and Ear Infirmary, Dr. Bernard Samuels.

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Thygeson: Dr. Samuels said that Dr. Wheeler was surrounding himself with inferior people so that he would stand out above them. That infuriated Dr. Reese.

Hughes: *And probably Dr. Wheeler as well.*

Thygeson: Anyway, he was a very superior man. I'm sorry to say he died of Alzheimer's disease. We knew him very well, and Ruth Lee edited his first book on pathology of the eye.*

Hughes: I understand that he, too, was interested in radiation, but in radiation therapy in this case.

Thygeson: Yes. He was intensely interested in tumors and radiation treatment of the tumors. He worked with the big tumor group in New York, Sloan-Kettering. He was a consultant. He worked with a number of the staff, particularly on retinoblastoma. Well, that was a terrible disease, but by means of radiation a number of eyes were saved. Al Reese was right in on that.

Hughes: *It had been used before he came along?*

Thygeson: I suppose it had, but I didn't know it. As far as I knew it was all Al Reese.

Hughes: *When you say radiation, you mean x-rays?*

Thygeson: Yes.

Hughes: *Then Manuel Uribe Troncoso?*

Thygeson: Yes. His real name was Uribe, the Mexican way, putting the mother's name at the last. Troncoso was his mother's name. He was a volunteer worker. He wasn't on a salary. He was a glaucoma man. He was interested in the angle of the anterior chamber, which is the main area of glaucoma significance. He published a number of very fine papers and an atlas. He was a pioneer in gonioscopy. Dr. Wheeler had some really good, outstanding volunteer men.

* AG Reese. *Tumors of the Eye*. Paul B. Hoeber, 195

Hughes: Unusual, was it not, to have a Mexican with such fine training?

Thygeson: Well, Troncoso, I think, had been professor of ophthalmology down in Mexico City. He came to the United States in his later years. He was really good. I think he became an American citizen.

Hughes: Was his early training in Mexico, or did he go to Europe?

Thygeson: Probably in Europe. I really don't know for sure, but he was well trained. He was deaf, so he couldn't lecture or participate in the teaching, which hurt his feelings, of course. But he would work half a day in the laboratory on the angle of the anterior chamber in animals, and he did comparative research exclusively in his later years. He did the angles of all the different animals. So he was a good comparative ophthalmologist.

Hughes: Then the last one that I know of at Columbia was John Hughes Dunnington.

Thygeson: Dr. Dunnington was a superb clinician and surgeon, but he wasn't interested in the basic sciences or in laboratory research at all, which was a little disappointment, because the department of ophthalmology suffered when he became the full director. I was his co-director, but when I left, the basic sciences suffered. He just wasn't interested. He recognized, however, the importance of von Sallmann.

Hughes: His disinterest in the basic sciences defeated the original purpose of the institute, didn't it?

Thygeson: Yes. It was too bad. But von Sallmann, who took my place as the research director at the Institute of Ophthalmology, kept the basic sciences going in a modest way. So they weren't lost, but he really got insufficient support from Dr. Dunnington, who was a very fine fellow, and an exceptional clinician.

Hughes: Did that mean that von Sallmann had to go outside the university and the institute for his money?

Thygeson: No, he was paid by the Knapp Foundation.

Hughes: And that was enough for research as well?

Thygeson: Yes. Before any NIH funds became available, the Knapp Foundation paid for Dr. von Sallmann's salary and his technicians and his research. Later on he got NIH grants for the work with the Lawrence Laboratory, and all that was a part of the early NIH program. Initially he was completely dependent on private funds. The Knapp funds carried him very well in those early days.

Hughes: Have we left anybody out of those early days at the institute, the 1930s?

Thygeson: There was a younger man, Eduardo Gallardo. He was a Spaniard and a classmate of Dr. Castroviejo's. They had been close friends, but then when the Spanish Civil War started, they parted ways.

Hughes: Opposite camps?

Thygeson: Yes. They became really bitter enemies. Gallardo was trained in bacteriology. He worked with me on herpes research. He did some nice work. He later went into the Veterans' Hospital laboratory work, and I've lost track of him. He was good in his field, but he was not a clinician.

There were other fine staff members, including Gordon Bruce.

Hughes: *What about scientists and physicians at Columbia who were outside the institute. For example, Alphonse Dochez?*

Thygeson: He became professor of bacteriology. He had a very illustrious history at the Rockefeller Institute. He was quite elderly when I knew him, and he had Marie Strumple's arthritis—stiff back and all that. I came in contact with him first because he did some of the early work on sulfonamides. I'll tell you how a mistaken lecture by Dochez started work on trachoma and sulfonamides.

Well, I had kept my contact with the Indian Service as a consultant. I was working periodically at the laboratory at Fort Apache. One time I brought Dr. Proctor and Dr. Polk Richards, who was in charge of the clinical trachoma program, and another Indian Service ophthalmologist, Fred Loe. We had all three of them stay at our house in New Jersey. We went to a lecture at Columbia University Physicians and Surgeons by Alphonse Dochez on sulfanilamide, in which Dr. Dochez reported that he had cured a case of distemper in dogs by means of the new drug, sulfanilamide. It was known at that time that distemper was a viral disease, so it looked as though sulfonamides were going to work on viruses.

We got all excited about this, and Fred Loe, who was in charge of the trachoma work at Pine Ridge Reservation, the Sioux, in South Dakota, immediately said, "Well, now, trachoma is a viral disease. This works on dog distemper, why not on trachoma?"

He went back to his reservation, and he started sulfonamide therapy. He published the first effective treatment of trachoma by sulfonamides, he treated trachoma cases in two ways, by mouth or by putting the powder in the eye every four hours, day and night. He got about a ninety percent cure rate of trachoma on this regime. This was on mistaken grounds. The first mistake was Dochez's dogs didn't have distemper; they had a bacterial disease. The second thing was that trachoma is not a viral disease; it is a bacterial disease. So sulfanilamide worked. But this was the first report in the United States on sulfanilamide and the treatment of trachoma. Shortly thereafter Dr. Proctor and Dr. Richards and I did the same thing in Fort Apache, and in our first series of Indian children we had about an eighty-five percent cure rate, so we confirmed the results of this first paper by Fred Loe.

The Indians called him Hostine. That's the Indian name for doctor. Fred Loe was the first man in the United States to publish on the sulfanilamide treatment.

Hughes: *Your paper came out in 1939,* and I guess Fred Loe's had just come out?*

* P Richards, WG Forster, P Thygeson, Treatment of trachoma with sulfanilamide. *Arch Ophthalmol* 1939; 21: 577-80.

Thygeson: There are six months or something between the publications. We confirmed Fred Loe's results.

Hughes: Does that mean that you were still thinking that trachoma was a viral disease?

Thygeson: Yes. And I accepted Dochez's diagnosis of distemper. I didn't find out until later that it wasn't distemper at all. That shows how mistaken ideas can lead to something good sometimes.

Chlamydia

Hughes: When was it that the agent of trachoma was identified as a chlamydia?

Thygeson: It was in the forties. It was known to be chlamydial, but it was thought that chlamydia were viruses.

Hughes: So they were actually using the term chlamydia?

Thygeson: They were using the term virus for the trachoma-psittacosis-lymphogranuloma group of agents. It had to be thrown out because they weren't viruses.

Hughes: Who was responsible for naming them chlamydia?

Thygeson: That goes way back to Prowazek. He called them chlamydozoa, and chlamydia came out of the term chlamydozoa.* He thought that the trachoma inclusions were mantle bodies, that they had a little elementary body surrounded by a mantle. That was partially right, in the sense that the elementary body was the infectious agent, and that the so-called mantle was actually an early developmental stage of the elementary body.

Hughes: Did Prowazek think that they were viruses?

Thygeson: Yes, he thought they were viruses.

Hughes: Was there any critical work in which the chlamydia were definitely shown to be different from the viruses?

Thygeson: Well, there was a man at the University of Chicago, J.W. Moulder, who published the first paper on that subject. He showed that the chlamydia had the characteristics of bacteria, that they had the enzyme systems of bacteria. He wasn't the only one that came to that conclusion, but he was the one who really defined them as a bacteria.

Hughes: Do you remember when that was?

Thygeson: I can't remember at the moment, but it was considerably later.

Hughes: In the forties?

* The following four sentences were moved from their original location later in the transcript.

Thygeson: In the forties, I'm sure. We used the term "trachoma virus" for quite a while, in the early forties anyway.

Moulder published a little book which I have. It's called *The Psittacosis Group as Bacteria*.*

Anyway, it soon became evident that there was a similarity to rickettsiae, which are small bacteria, of course.

Hughes: *I understand that you have objected, and perhaps still object, to the fact that there are only two species of chlamydia?*

Thygeson: Yes. That wouldn't have happened except for the death of Dr. Geoffrey Rake. He was a real chlamydiologist. His prime interest was lymphogranuloma venereum. He defined three species of chlamydia active in trachoma, inclusion conjunctivitis, and lymphogranuloma venereum. He called them *Chlamydia trachomatis*, *Chlamydia oculogenitalis*, and *Chlamydia lymphogranulomatis*. The classification was much better than it is now. All it is, you see, is groups A, B, and C are the serologic groups for trachoma, and D, E, and F are the venereal chlamydia, and L1, L2, L3 for the lymphogranuloma venereums.

I think the system currently in use is very confusing. In one reputable eye journal, the story came out—you probably saw it in Dan's book**—trachoma is now the most common venereal disease in the United States, which, of course, is a confusion based on the term *Chlamydia trachomatis*.

Hughes: *Was there a group that was responsible for the present system of classification?*

Thygeson: There were a group of taxonomists, with the chairman based in Iowa, who really fouled up the taxonomy of chlamydia. They were not clinicians. Any clinical difference between trachoma and inclusion conjunctivitis and lymphogranuloma didn't mean anything to them. It was the biologic characteristics of chlamydia that they were interested in. There are remarkable similarities biologically between all the chlamydia. Very few differences in the chlamydia group.

Hughes: *You're talking about anatomic differences or functional differences?*

Thygeson: I'm talking about biologic differences. For example, the trachoma and LGV chlamydia produce an inclusion body with an iodine-staining matrix, and the psittacosis inclusions don't have that. That's a big difference, but that's a very minor difference when you come down to it.

Paper on Matrix Staining of the Trachoma Inclusion Body, 1938

Hughes: *You wrote a paper on the fact that the matrix stains with iodine?****

* New York: John Wiley and Sons, 1964.

** D Vaughan, T Asbury. *General Ophthalmology*. Los Altos, California: Lange Medical Publications, 11th ed., 1986, 84.

*** P Thygeson. The matrix of the epithelial cell inclusion of trachoma. *Am J Pathol* 1938; 14:455.

Thygeson: Yes. I was the second man to do that. The first one was C. E. Rice, a Public Health Service officer. He wasn't an ophthalmologist. He was working at Rolla, Missouri, and just happened to stain these inclusions with lugol solution, an iodine solution. He noted that these inclusions stained brown with lugol's. Then he worked it out so that he could digest this material with saliva. It had the properties of glycogen. He wrote the first paper on that phenomenon, and I wrote the second paper.

Hughes: *Had you seen his paper before you published?*

Thygeson: No, I hadn't seen his paper, but I had visited him. Rice showed me the phenomenon, so I knew about this. I extended his work to take in inclusion blennorrhoea. He had only been working on trachoma. He was a very interesting general practitioner with the Public Health Service. He had wide interests, so he dabbled in everything.

The College of Physicians and Surgeons

Hughes: *Let's get back to Columbia.*

Thygeson: Well, Columbia was, in a sense, very fortunate, because the College of Physicians and Surgeons had a wonderful staff, so you could get consultations on every subject. It had a wonderful library with good librarians, and it had this marvelous outpatient clinic.

Hughes: *Is that the Vanderbilt Clinic?*

Thygeson: Yes, the Vanderbilt Clinic, where you could see anything you wanted in this outpatient clinic. So there was a wonderful chance for seeing a lot of things.

Hughes: *The Vanderbilt Clinic was funded by the Vanderbilts?*

Thygeson: I imagine. It was a part of the College of Physicians and Surgeons, not a part of Presbyterian Hospital. There was a big difference between Presbyterian and Columbia. Columbia would have been sadly off if it hadn't been for Presbyterian, because it furnished the hospital. And a lot of the money for research came out of Presbyterian.

Hughes: *Did you have any call to consult basic scientists on the staff at Columbia?*

Thygeson: Oh, yes, because Columbia had the best mycologist of the time, a woman, Rhoda Williams Benham. She was by far the best mycologist in the country. She had a course every year, so I took her course. It lasted all winter, several hours a day.

Hughes: *Was that your first introduction to mycology?*

Thygeson: Well, all that I had had was in the usual microbiology course in medical school. You always had a smattering of it. But this was the first time I really got down to real nitty gritty on the fungi, so it did me a lot of good.

Hughes: Did she have any particular interest in fungal infections of the eye?

Thygeson: No, but she was interested in Pityrosporum, which is a yeastlike organism. It's found in the skin. I took this up because we have a lot of it in the eyelids. I decided that it was the probable cause of seborrheic dermatitis. I wrote a couple of papers on this. Then it became pretty evident that it wasn't the cause. It's just been revived again in the last year, and it looks now as if Pityrosporum is actually the cause of seborrheic blepharitis and dermatitis. So my early work wasn't entirely in vain.*

Hughes: Were you aware of the work of Harold Urey, the physical chemist, whose claim to fame is the discovery of deuterium and stable isotopes?

Thygeson: I knew about it, but I never met him. I did go down to Columbia itself, the college, which is farther down the island [of Manhattan]; I had to go there for the Ph.D. examinations. I would often be on those committees. So I got to meet quite a few of the Columbia faculty.

The most interesting man I met was a professor of engineering, Edwin Howard Armstrong, who developed the FM, frequency modulation. He revolutionized radio by frequency modulation. He died and his wife was involved in terrible litigations with Radio Corporation of America on patents about this FM thing. She finally won out.

Hughes: Any other names connected with the Columbia days?

Geoffrey Rake

*Thygeson: Well, I think one of the most interesting people whom I came across was Geoffrey Rake, who was then at Squibb Institute in Rahway, New Jersey. Squibb was a private institute, but it was top notch in every way. Geoffrey Rake was an English microbiologist who did wonderful work on lymphogranuloma venereum. So I collaborated with him on this, and we showed, I think for the first time, that there was a common antigen among trachoma, inclusion conjunctivitis, and lymphogranuloma venereum. We published this paper. I was the third on the list of authors.***

Hughes: Was that because of age?

Thygeson: No, it was because I furnished the clinical material, and they did the work in the laboratory. I was the end of the string.

Virologists at the Rockefeller Institute

Hughes: Some of the best virologists in the world were at the Rockefeller Institute in the 1930s.

* JS Gots, P Thygeson, M Waisman. Observations on *Pityrosporum ovale* in seborrheic blepharitis and conjunctivitis. *Am J Ophthalmol* 1947; 30:1485-94.
Thygeson, DG Vaughan, Jr. Seborrheic blepharitis. *Trans Am Ophthalmol Soc* 1954; 52:173-188.

** G Rake, MF Shaffer, P Thygeson. Relationship of agents of trachoma and inclusion conjunctivitis to those of lymphogranuloma-psittacosis group. *Proc Exper Bio Med* 1942; 49:545-7.

Thygeson: Yes.

Hughes: Did you have any personal ties with any of those people?

Thygeson: Of course, I knew Rivers quite well, and then I had this winter on tissue culture. I was trying to grow the trachoma "virus," as we called it at that time. I was developing a method to grow ocular tissue so we could isolate the trachoma agent. As I told you, I spent the winter with a pioneer tissue culture man, Alexis Carrel. So every noon we would have lunch in a large dining room, so I got to meet all the Rockefeller people eventually.

Hughes: Did you know Peter Olitsky?

Thygeson: Oh, yes, I knew him very well. We had a lot of contact because he was interested in trachoma. Jerome Syverton was also interested in trachoma, and Peyton Rous in tumor virology, and Richard Shope at the Rockefeller Institute at Princeton, New Jersey.

Hughes: Did you have time to really follow the work in basic virology?

Thygeson: No, I didn't have time to follow, but I heard about it and had contact, mostly at lunch time. But I had more contact with Shope because I had known him quite well. He visited me in Iowa a number of times, so I was on good terms with him. But the others I just heard talk of, except Rivers I knew quite well. He was interested in Rift Valley fever, which is a viral disease of the Rift Valley in Africa. They get a retinitis with that disease. He asked me quite a bit about the retina. I offered advice about that. Rivers was a very fine fellow.

Hughes: He was a very outspoken individual, was he not?

Thygeson: Yes, and he was very active politically in the Society of American Bacteriology as they called it at that time; it is now the American Society of Microbiology. He was president.

Hughes: Was he head of the Rockefeller Hospital at that stage, or did that come later?

Thygeson: Well, he worked there, because he did clinical work, too, besides the laboratory.

*Hughes: I think it might have been a bit later that he became head of the hospital.**

Thygeson: But anyway, he was very active in the hospital, and he wrote several very good books before he put out the real book on viruses.

Hughes: Well, just a few other names: Albert Sabin, Joseph Smadel, and Harold Cox.

* Dr. Rivers became director of the Rockefeller Institute Hospital on July 1, 1937. (Tom Rivers: *Reflections on a Life in Medicine and Science*. An oral history memoir prepared by Saul Benison. Cambridge, Mass.: MIT Press, 1967; 194.)

Thygeson: Albert Sabin I met because he worked with Olitsky on toxoplasmosis. This disease was very interesting to me because I had been with Nicolle in Tunisia, who had discovered the *Toxoplasma* organism in the gondi, a rodent in Tunisia. He didn't have any idea that it had any human involvement. Olitsky and Sabin were interested in it because it was an intracellular parasite and quite easy to see. It was big. It had an affinity for nervous tissue. So it interested me because the eye is nervous tissue.

Olitsky and Sabin produced a wonderful exhibit at the New York session of the American Society of Bacteriology on a new intracellular organism, in which they showed all the intracellular life cycle of *Toxoplasma*. I went through that very carefully on account of Nicolle. So I knew Olitsky and Sabin. I've seen Sabin quite a number of times since. He was interested in herpes, a big interest of mine.

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Thygeson: I was very fortunate to see the first human case. I acted as a consultant on infectious disease having anything to do with the eye.

Hughes: *At the Rockefeller?*

Thygeson: No, at Columbia. Well, at the Babies' Hospital. They had a baby who had an infectious disease that involved the retina of both eyes. I was called to see it. I had no idea what it was, but then the baby died, and the autopsy showed toxoplasma in the retina of both eyes. I have the specimen.

Hughes: *How had the baby gotten it?*

Thygeson: From the mother. Toxoplasmosis. If the mother gets the disease during pregnancy, it goes right through the placenta, and the baby gets it. If she's already had the disease, then the baby doesn't get it. So it's a matter of protecting her against exposure to toxoplasma. That's why cats and raw meat and all that are so important to stay away from in pregnancy.

Hughes: *Did you have any particular connection with Joseph Smadel?*

Thygeson: No, but I met him. I knew about him.

Hughes: *And Harold Cox?*

Thygeson: I knew about him. I had met him, but that's all.

Debate Over the Nature of the Virus

Hughes: *Wendell Stanley crystallized the tobacco mosaic virus in 1935,* which, of course, was before you had even arrived at Columbia, but do you remember that being a momentous achievement?*

* WM Stanley. Isolation of a crystalline protein possessing the properties of tobacco-mosaic virus. *Science* 1935; 81:644-5.

Thygeson: Oh, yes. There was a big argument as to whether viruses were living or dead.

Hughes: *Did you take sides on that issue?*

Thygeson: Oh, yes, I took sides on this. Crystallization is a matter of shape and not of living or dead. That was my thesis. I knew Stanley very well, because I was on a trans-Atlantic steamer with him.

Hughes: *The meeting of the International Congress of Microbiology in London?*

Thygeson: Yes. Stanley was on the same boat on the way back, so I got very well acquainted with him. What was 1936. I had very many pleasant conferences with him.

Hughes: *He was a pleasant individual?*

Thygeson: Very nice, a very attractive fellow. I liked him.

Hughes: *It was the following year that two Britishers, F.C. Bawden and N.W. Pirie, came out with a paper claiming that the tobacco mosaic virus was not strictly a protein, as Stanley had claimed; that it was really a nucleoprotein.* Do you remember that discussion?*

Thygeson: I don't remember that too well. I just remember that I took the side that all viruses were living entities.

Hughes: *Do you think that was the usual approach of the clinician, the physician?*

Thygeson: Yes, I think so. The biochemists had a different approach. [interruption]

Hughes: *I'm interested in this period when people were debating about the nature of the virus. Would it be true to say that you and other physicians were more interested in what the viruses did pathologically than in their anatomic structure?*

Thygeson: Well, we were interested in morphology because we could see the large viruses under the ordinary microscope, like vaccinia, smallpox, but we couldn't see tobacco mosaic virus because it is very small. Then while I was there at Columbia, the Elford filters came in, and Elford was able to measure the size of viruses by differential filtration. You could do the same thing by ultracentrifugation, the way the viruses sedimented. Then you could get the whole spectrum of size. I was very much interested in that.

The Ultracentrifuge, Electrophoresis, and Electron Microscope

Hughes: *The ultracentrifuge was a new introduction, wasn't it?*

Thygeson: Yes, that was new.

Hughes: *Did you have an ultracentrifuge?*

* FC Bawden, NW Pirie. The isolation and some properties of liquid crystalline substances from solanaceous plants infected with three strains of tobacco mosaic virus. *Proc R Soc* 1937; 128, B:274-320.

Thygeson: Oh, yes, we bought one.

Hughes: *Was it made by the Beckman Corporation?*

Thygeson: I've forgotten who did that, but it was the same one that was used in all those early studies on size of viruses.

Hughes: *It was easy enough to get an ultracentrifuge in those days?*

Thygeson: Yes. All we had to have was money.

Hughes: *What about other techniques? Electrophoresis, for example. Did you ever have any call to use it?*

Thygeson: I had practically no experience with electrophoresis, which was an immunologic procedure at that time.

Hughes: *Nobody in the institute was using that?*

Thygeson: Well, I'm sure in P and S [Physicians and Surgeons] they were, but not in the institute. Say, there was a doctor who did some work on it. He later became professor of bacteriology at Colorado. His name was Richard Thompson. He was immunologically interested and he did some work on--

Hughes: *He was an ophthalmologist?*

Thygeson: No, he was an M.D., but he was a microbiologist.

Hughes: *He wasn't in the institute then?*

Thygeson: Well, he was attached to the institute. He was paid partially by the institute. Dr. Wheeler got certain members of the basic sciences and gave them a partial stipend to act as consultants for the institute. Richard Thompson's appointment was in bacteriology, but he had one of these consultation stipends from the institute.

Hughes: *Was electron microscopy making any inroads at this point? Was it too experimental?*

Thygeson: That was very experimental at that time. That didn't come into the picture at Columbia at all. But it did, of course, as soon as we got out here to California. [Vladimir Kosma] Zworykin was with the Radio Corporation of America, and he did pioneer work on the electron microscope. But I had nothing to do with that. Zworykin was a Russian. He was one of the early pioneers in electron microscopy.

Hughes: *Was virology one of the first applications of the electron microscope?*

Thygeson: Yes, but it had a very wide application for bacteria, too, for yeast, all those things. It turned out to be a very wonderful tool.

Hughes: *Any other tools that were particularly important in the thirties, the prewar years?*

Elford Filters

- Thygeson: Well, I looked on the Elford filters, graded filtration, as being very important, at least for my work. We were able to separate the elementary bodies of trachoma from ordinary bacteria by means of graded filtration. If we tried to do it with the old-fashioned filters, the virus got lost by adsorption by the Berkefeld filters. But we had very little adsorption in the Elford filters. So they were a tremendous advance for my work.
- Hughes: *Later, when electron microscopy was being applied to the study of viruses, there was some discrepancy between the size determined by the electron microscope and by Elford filters?*
- Thygeson: Yes, but it wasn't serious. See, it was the shape. For instance, the vaccinia virus was kind of brick-shaped. There wasn't any discrepancy on the spherical viruses, but when you got a different shape, then the filter would hold back these non-spherical viruses. That's where the discrepancy came. But otherwise I think it was very good. The elementary body being spherical, there wasn't any problem because that tied right in with the electron microscope and the Elford filters.
- Hughes: *We've talked about associations with the Rockefeller Institute. Did you have any associations with any other institutions in New York?*
- Thygeson: Well, yes, because Columbia had a lot of affiliated institutions—Manhattan Eye and Ear, New York Eye and Ear Infirmary, and Roosevelt Hospital. If they had an infectious disease involving the eye, why I usually got called in as a consultant. So I saw a lot of infectious disease that I wouldn't have seen if it hadn't been for those affiliations.

The Basic Science Course in Ophthalmology

- Thygeson: The other thing is we had a basic science course that we developed at Columbia, which was available to all the residents in New York City. St. Luke's and other institutions sent up residents, so I got to meet all the young residents. Therefore, I got to hear about all the interesting cases from their hospitals.
- Hughes: *What did the basic science course entail?*
- Thygeson: It took in all the basic specialties, like physiology and anatomy and everything, as related to the eye.
- Hughes: *Was that unusual?*
- Thygeson: Yes. As far as I can think of, it was the first one. That was mandated by the dean, Dr. Rappleye. He decided that we had to have a basic science course, and so I was delegated to start it. I had a little trouble because the residents at that time weren't interested in basic science; they were interested in

cataract extraction. So we had a little difficulty, but it came out all right. Then after I left, Dr. von Sallmann kept it going. It became a very important part of Columbia-Presbyterian.

Hughes: Did everybody in the institute teach an aspect of the course?

Thygeson: No. Only those who had some basic skill. Smelser was very active in it. Meyer was active. We had a terrible time with Meyer because he had a guttural German accent, and the residents couldn't understand him. So his teachings were a total loss.

Hughes: What were you teaching?

Thygeson: I was teaching microbiology. It was an interesting experiment, I think, bringing basic sciences to the ophthalmologists, which they hadn't had before.

Trachoma (continued)

Hughes: Well, we've talked about some of the research that was going on in these years at Columbia. Have we said enough about trachoma?

Thygeson: Well, I think that the important things at Columbia in regard to trachoma were the relations between lymphogranuloma venereum and inclusion conjunctivitis in the chlamydia group. That was investigated at Columbia, particularly with Geoffrey Rake. So that was important. Then the sulfonamide treatment was started at Columbia, as was the tissue culture work on trachoma.

We had done all this work on tissue culture with the idea of developing an epithelial tissue that we could grow the virus in. This was a mistaken idea. In the eye, trachoma involves only epithelial tissue. I thought that would apply to tissue culture in the laboratory, too, which it didn't. We developed all these tissue media: epithelia from the cornea and conjunctiva, and then the epithelium of the developing egg—the chorioallantoic membrane. We thought we had at least three different epithelial tissues to try out, and in passing I did a few yolk sac inoculations, but I never did this in series. The ultimate cultivation was done in 1957 in China by serial passage in the yolk sac. They had to do two or three passages before they could get anything out of it. Well, I didn't catch on to that.

What I got was one cycle in the development. In the tissue culture I could get one growth cycle, but then the agent died. I couldn't get any further cycles for the chlamydia agent.

Dr. Proctor was interested at that time in corneal transplantation, so he came up, and he wanted Dr. Castroviejo to come in on our trachoma team because the blindness in Indians was due to scars in the cornea. He wanted to correct them by corneal transplantation.

Well, it didn't work. Corneal transplants were failures in those days. The techniques had to be improved further. The corneal transplant worked all right on the nonvascular cornea—a scar that didn't have any blood vessels—but if you had vessels, as in the pannus of trachoma, it didn't work.

Hughes: Why is that?

Thygeson: Well, because if you bring in the blood vessels, you get graft rejection. If the graft is separated from the blood supply, then you don't get a graft rejection.

Hughes: How was that gotten around later?

Thygeson: By topical immunosuppression, for one thing, topical steroids.

Hughes: Which you just didn't have in those days?

Thygeson: No. Then later on it was by a doughnut-type of transplant. One team made a double transplant, the first transplant knocking out the blood vessels, and then after they were knocked out, then they did the regular transplant.

Hughes: When did that technique come in?

Thygeson: That was quite a bit later. I don't think Castroviejo came in on that technique. Anyway, everything we did on trachoma at that time didn't work, so we gave it up.

Hughes: Was there a lot of blindness due to trachoma?

Thygeson: Yes. At that time trachoma was still the most important cause of blindness in the world. In the United States it was still the most important cause of blindness in Missouri, Tennessee, Oklahoma, and Arkansas. But then gradually trachoma died out spontaneously or was treated, and it lost its importance as a cause of blindness in the United States, but for the world it still is a very important cause of blindness.

Hughes: It died out here because of the sulfonamides?

Thygeson: That was the first real treatment of trachoma. But there occurred a rather disturbing development. It was found that trachoma would die out spontaneously if you did two things: if you improved the water supply or if you improved the nutrition. So just simple hygienic measures plus better nutrition really knocked out trachoma.

Hughes: Did you have to have both those things working together in order to get improvement?

Thygeson: That was the best. For example, the Public Health Service had six trachoma hospitals in the trachoma belt: the Daniel Boone Trail area, Missouri, Arkansas, Ozarks, all through there. They had six hospitals working on trachoma, but really all they did was surgical correction of deformities. They didn't really affect the disease at all. But then as the hygiene and nutrition of these mountaineers improved, trachoma just died out spontaneously. So you can't credit sulfanilamide for all the cures of trachoma. They were often spontaneous.

Hughes: When you say purification of the water, are you meaning in terms of chlamydia?

Thygeson: In terms of hygiene.

Hughes: *In general?*

Thygeson: Just general hygiene. Water and hygiene go together.

Hughes: *So what was happening was that the human host was being worn down by infection and by poor nutrition and was a better target for the trachoma agent?*

Thygeson: Yes. Trachoma is probably fundamentally a self-limited disease, but in some cases it lasts a lifetime, but it's really tied in with poor nutrition and debilitation and poor hygiene and so on. And repeated infections.

Hughes: *Was it found in this belt because those were poor areas?*

Thygeson: Yes, poor mountaineers. All they had to eat was corn and salt pork. They had all the nutritional diseases, too. They had very poor hygiene. There wasn't any personal hygiene at all. There wasn't any running water. They were marginal. They were just about like Egypt. And our Indians the same way. As soon as you improved the hygiene of Indians, why spontaneous cures occurred, and probably relapses, secondary infections, stopped. Trachoma is often a series of multiple infections.

We had the grandmother syndrome, where the grandmother has a flareup of an old trachoma, just as tuberculosis flares up in the aged. Well, trachoma flares up in the aged, too. The grandmothers would then infect the young children. The grandmothers usually took care of the young children. They would wipe the eyes with their tunics and so on. No running water to wash their hands. The same water used for everybody in the family. The same towels used for everybody in the family. So hygiene and trachoma don't go together very well. I didn't know that. It took me a little while to realize how important spontaneous healing was in trachoma. I knew in inclusion blennorrhoea that it was a self-limiting disease, three or four months, but I didn't know about spontaneous healing in trachoma.

Lymphogranuloma Venereum

Hughes: *Have we said enough about lymphogranuloma venereum?*

Thygeson: There's one interesting case that might be mentioned which was a bit of a shock to me. I was working in Vanderbilt Clinic and I had the external disease clinic almost every day. Dr. [Algernon B.] Reese, who was at that time in charge of the clinic, brought this case to me. It was a New York tailor who had a terrible right eye. He couldn't see in this eye, and the lids were all puffed out. The eye was discharging. This had been going on for three years. He had been to all the clinics; everybody had seen him. So Dr. Reese turned him over to me. Because of the involvement of the skin, the lids and everything, I took him into the skin department where there was a husband and wife team, W. and H. O. Curth, young dermatologists from Vienna. The woman made an immediate diagnosis by inspection. She said, "lymphogranuloma venereum."

Hughes: How could she have done that so readily?

Thygeson: Because she had worked on esthiomene. Have you heard of the term, "esthiomene"?

Hughes: No.

Thygeson: Well, that's the female lymphogranuloma venereum. It causes a marked swelling of the labia. Dr. Curth immediately recognized the similarity in the lids, and she immediately made the diagnosis.

We then did cultures. The chlamydia of lymphogranuloma is much easier to culture than that of a trachoma or inclusion conjunctivitis. The way you do it is you inject it into a monkey or a mouse brain. Both monkey and mouse are susceptible. So you get the agent, and you make a smear from the meninges in the monkey or the mouse brain. You see all kinds of inclusion bodies and elementary bodies. So it wasn't any problem proving the diagnosis. We also had a positive Frei test, which at that time was thought to be specific for LGV.

Hughes: What is the Frei test?

Thygeson: The Frei test is a skin test taken from sterilized material from a bubo. These lymph glands get pussy in LGV. They heat those to kill the agent, and then they inject the material into the skin. There's a big reaction, inflammatory reaction; it's like the tuberculin reaction. That was called the Frei test after an Austrian dermatologist, [Wilhelm S.] Frei.

This patient had a good positive Frei test. So we had all the information we needed on this case. But it had been going around for three years. The ophthalmologist didn't recognize it. I didn't recognize it. But Dr. Curth did right away. That was the value of consultation. She did all the workup, and then treated this man with sulfanilamide for three weeks. He made a complete recovery as far as the inflammation, but he had lost the sight of his eye. She wrote the paper up.

Hughes: Was your name on that?

Thygeson: She mentioned me as a consultant on that. It was actually my case, but she did the main work. That was very important because we then recognized that there was a syndrome of primary lymphogranuloma venereum of the eye. If the eye is involved secondary to a genital infection, you get a different picture, an endogenous uveitis. But a primary infection of the eye produces this esthiomene-type external reaction. So I learned a lot just from this one case.

Paper on Sulfonilamide Treatment of Inclusion Conjunctivitis, 1939

- Hughes:* We talked about inclusion conjunctivitis, but we didn't talk about a paper that you published in 1939 on the treatment of the disease with sulfanilamide.* Was that the first time the sulfas had been used to treat inclusion conjunctivitis?
- Thygeson:* Yes, that was the first time. I remember I wrote a paper for the AOS [American Ophthalmological Society] where I think I had thirty cases, and I got a hundred percent cure rate with sulfanilamide. I wrote two or three papers on that.** The most important on inclusion blennorrhoea was with Bill Stone. Did you read that?
- Hughes:* Yes. I have that here.
- Thygeson:* That was really a good paper because it showed the epidemiology involving the mother and the babies.
- Hughes:* And it was not known before that it was possible to pass the infection from mother to baby?
- Thygeson:* Well, it had been hypothesized by early workers in Vienna, because they had found inclusions in the genital tract of mothers of the babies. So it was hypothesized, but it had never been worked out as a real study. Because of Bill Stone, a real go-getter, we were able to get all these case histories and everything. It was a very clear-cut picture of a venereal disease, of which the eye was only a rather minor part.
- Hughes:* Had the eye portion of the disease been confused with trachoma previous to this?
- Thygeson:* It had in connection with swimming pool conjunctivitis because it looked like early trachoma.
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- Thygeson:* Professor Lindner studied swimming pool conjunctivitis and recognized the inclusion bodies. He recognized that swimming pool conjunctivitis was of chlamydial origin. He didn't use the word chlamydia at that time. But he recognized it was not trachoma because it was benign and had no scars or pannus.
- Hughes:* Is that the way you differentiated it in those days?

* P Thygeson. Sulfonilamide therapy of inclusion conjunctivitis. *Am J Ophthalmol* 1939; 22: 179-80.

** P Thygeson. The treatment of inclusion conjunctivitis with sulfanilamide. *Trans Am Ophthalmol Soc* 1940; *Arch Ophthalmol* 1941; 25:217-27.
 P Thygeson, W Stone, Jr. The treatment of inclusion conjunctivitis with sulfathiazole ointment. *JAMA* 1942; 119:407-8.
 P Thygeson, W Stone, Jr. Epidemiology of inclusion conjunctivitis. *Arch Ophthalmol* 1942; 27:91-119.

Thygeson: Well, there was another way which was developed by Victor Morax in Paris. It was to transfer infectious scrapings to a monkey or ape. There's quite a difference between the two experimental diseases. With inclusion conjunctivitis you get a very good experimental model, a subacute or acute disease with a selflimited duration. It always gets well in three or four months. Then you can use the animal over again. It doesn't have any immunity.

But trachoma doesn't work that way. In trachoma it's very hard to get an experimental model, and the incubation period is longer, and the disease is low grade, but it lasts many months or even years.

Hughes: *Why the difference do you think?*

Thygeson: Well, it has to do with the biology of the chlamydia, but that's still under study right now. The tremendous difference is in the clinical manifestations of the serologic types, the trachoma types A, B, C, versus the D, E, F, venereal types. In trachoma the disease transfers eye to eye, but in the D, E, F venereal types the transfer to the eye is accidental. Not every mother who has the chlamydia infection infects the baby. It's just a rather small percentage.

Staphylococcal Eye Diseases

Hughes: *During this period at Columbia you also were interested in staphylococcal eye diseases. One paper that I looked at is entitled, "Mannitol fermentation as an indicator of conjunctival pathogenicity of staphylococci."**

Thygeson: Yes. I think we had shown that the conjunctiva and cornea were involved but not by direct growth of the organism on the conjunctiva or the cornea but by toxin produced by the organism. It was the toxin that caused the conjunctivitis. The organism lived on the lid margins. It spilled over onto the conjunctiva. But no invasion of the conjunctiva or cornea occurred. We wanted to be able to tell which staphylococci were toxin producers.

We found out that those staphylococci that fermented mannitol generally produced toxin. There was only one type of toxin that produced conjunctivitis. There are half-a-dozen types of toxins produced by staph, however. It was the dermonecrotic toxin that produced the eye problem. We could reproduce the eye lesions by putting toxin in the eyes of rabbits and human volunteers without infecting the eyes at all.

Hughes: *How did you identify the type of toxin? How did you know that it was produced along with mannitol fermentation?*

Thygeson: It's called a dermonecrotic toxin, and you inject it in a guinea pig's skin, and it produces a necrosis in the guinea pig. It was the dermonecrotic toxin that produced the eye disease. Of course, there are new toxins that we didn't know anything about. All we knew about in those days was the enterotoxin

* P Thygeson. *Arch Ophthalmol* 1938; 20:274-5.

that produced food poisoning and the dermonecrotic. I only knew two types of toxins. Now they're about six. They're even labeled A, B, and C, and all that.

Hughes: You had to know some biochemistry to do that kind of work, didn't you?

Thygeson: Well, either that or I had to get consultation.

Hughes: Do you remember what you did in that case?

Thygeson: I had good consultation whenever I wanted it.

Hughes: Would that have been Karl Meyer?

Thygeson: Karl Meyer and Richard Thompson would have helped me on that. Anyway, we spent a lot of time on the staphylococcus. We worked on developing immunity to the toxin by means of a toxoid.

Hughes: Were you making the toxoid?

Thygeson: Yes, we made it. We grew the staph on broth and then filtered it. We could test the toxin strength by several different ways, including its ability to produce keratitis in rabbits. Then we could develop an antitoxin to it. We could measure the antitoxin in international units. I was correlating in the patient how much antitoxin he had in the blood with how he reacted to the staph in the eye. We had a lot of fun with that thing.

We came out with several syndromes. One of them, of course, was staphylococcal blepharitis, which was quite different from seborrheic blepharitis or *Moraxella* blepharitis. Then we came out with a syndrome of staphylococcal phlyctenulosis, which is different from the ordinary phlyctenulosis, which is tuberculous. So we had a lot of fun with staph. We had a lot of fun with toxoid.

We had some rather startling results with the toxoid. For example, I had one medical student that I couldn't get rid of the staph from his lid margins. Everything we did didn't work. He was going to drop out of school because he couldn't stand the photophobia and irritation of his infection. So I immunized him with toxoid, and his symptoms disappeared even though he still had the organism on the lid margin. He graduated.

Then I had an opera star who couldn't stand the klieg lights because of her photophobia. We immunized her, and she was able to carry on with her career. Our benefactor's [Edward S. Harkness] chauffeur had this problem, so he was sent to me. We took care of him with complete success. We had about twenty-seven of those cases that were relieved with toxoid. You don't need the toxoid any more because now you can usually get rid of the staph on the lid margins by antibiotics.

Hughes: Before the antibiotics came in, did other people adopt the toxoid treatment method?

Thygeson: Well, there was a Dr. C.E. Dolman, who was at Connaught Laboratories in Toronto, who was a pioneer in the toxoid use. I collaborated with him by mail. He measured the antitoxin content of the blood that I sent him in international units.

Hughes: *What is the international unit?*

Thygeson: Well, it goes up from one to twenty. The average person runs around three. Everybody is exposed to staph in their life, so they all develop a little antitoxin. But some do very well, up to twenty units. They're really hyperimmunized. Toxoid was very valuable. It isn't any more because you can handle staph without toxoid.

Hughes: *So for some years Connaught Laboratories manufactured toxoid?*

Thygeson: But not for us. We made our own toxoid. They just simply measured the antitoxin.

Hughes: *Did other people come to you for the toxoid?*

Thygeson: They would send patients to me. I wouldn't send out any manufactured toxoid because I was a little afraid of toxoid. You see, we made a broth culture of staph and then filtered it, so we got a bacteria-free filtrate. But the filtrate had all the broth proteins in it, so I was afraid of adverse reactions from the proteins in the broth.

We never ran into any difficulty, but I still kind of shudder when I think of all the antigens we were putting into the patient. But as far as I know, I never got into trouble.

Hughes: *How many patients do you suppose you treated with the toxoid?*

Thygeson: Well, it wasn't very large, fifty, seventy-five, a hundred, or something like that, and only those cases in which I couldn't get rid of the staph by ordinary medication. We used yellow oxide of mercury and silver nitrate on the lid margins to get rid of staph, and it often worked. But it often did not work; then I used the toxoid.

Hughes: *The yellow oxide and the silver nitrate are old treatments, aren't they?*

Thygeson: Yes.

Hughes: *How far do they go back?*

Thygeson: As far as you can remember. They're in all the old textbooks.

Hughes: *Were they used across the board for any infection?*

Thygeson: For blepharitis. Silver nitrate was used for other staph infections like impetigo in babies. The blisters of impetigo would be rubbed with silver nitrate.

Hughes: When did the silver nitrate treatment for infants come into regular practice?

Thygeson: That was by [Karl S.F.] Credé who was, I think, at Leipzig in Germany. I've forgotten the date of that, but that was a wonderful advance because about ten percent of the babies in his area were losing their sight from gonorrhoeal ophthalmia (ophthalmia neonatorum). So he was able to prevent gonorrhoeal ophthalmia by instillation of weak silver nitrate [solutions]. The Credé procedure was one of the real victories for preventive ophthalmology.

Hughes: You have always been interested in viral ocular diseases. Do you have anything specific to say about work that you were doing in those Columbia days?

Epidemic Keratoconjunctivitis

Thygeson: Well, in the Columbia days we were faced with what was later called epidemic keratoconjunctivitis due to adenovirus. There came by way of Hawaii in 1941, which was the year before I left Columbia, an epidemic viral disease which was called epidemic keratoconjunctivitis. We didn't know the cause, but it was epidemic and tied in with dispensaries and doctors' offices.

My first contact with it was through an industrial ophthalmologist down the island [Manhattan] who had eighty cases that followed removal of foreign bodies in his office. So it became obvious that it was due to contamination in the office. We didn't know whether it was his instruments, solutions, or his fingers, or what it was. But it was very obvious that it spread under medical conditions.

General Electric's factory in Schnectady had a major epidemic, and it looked like it might interfere with the war effort. So a commission was set up in Washington to study this disease, and Dr. Murray Sanders, who was on the staff at Columbia, was on this commission. He called me in to do co-work on this, so I helped on that.

Hughes: Why were they particularly concerned that it affect the military?

Thygeson: Well, because it knocked out these workers from the factories.

Hughes: They thought in any sort of group situation there might be an epidemic?

Thygeson: Yes. General Electric was very important for the war effort. They thought if it got into the troops, it could knock out the whole military, the way trachoma did with Napoleon's troops in Egypt. The eye problem really killed Napoleon's project. It looked as though that might happen, but it didn't.

Sanders thought he had a virus isolated. He wanted to prove it. This virus had no effect in monkeys or other animals. Sanders had me inoculate his eye with it, which I did. But we didn't get anything. But it was called Sanders' virus.

Hughes: Why do you suppose it didn't take?

Thygeson: Well, he didn't have the real virus.

Anyway, we all went into the army at that time, so Sanders' virus was put in deep freeze. Then after the war, when they took it out to renew the studies, it turned out to be St. Louis encephalitis virus. I don't know whether I inoculated Sanders with the St. Louis. [interruption]

This epidemic keratoconjunctivitis thing was interesting because of the mix-up on its history. The disease actually was first described in Austria. It was called superficial punctate keratitis by Fuchs. Then when it actually became epidemic in Australia, Hawaii, and California, Dr. [Michael] Hogan, who wrote, I think, the first American paper on it, called it epidemic keratoconjunctivitis and that's the name that stuck. It's now known as epidemic keratoconjunctivitis, or adenovirus conjunctivitis. So Sanders' virus just wasn't the cause. It turned out that it was a fake virus due to a seeding by a technician.

The technician, Mrs. Alexander, salted the cultures. She really didn't get a virus out of that. She wanted to please Sanders, so she salted this thing. I don't know what virus she used, except it possibly was St. Louis encephalitis. It had nothing to do with the disease itself.

Hughes: But Dr. Sanders did not come down with encephalitis?

Thygeson: No. So we were lucky on that.

Hughes: There were some later epidemics of keratoconjunctivitis in California that I'm aware of.

Thygeson: Yes. At California [UCSF] our group isolated the prototype of adenovirus 8, which was the actual cause of epidemic keratoconjunctivitis.*

Hughes: That was done at the Proctor Foundation?

Thygeson: Yes, and the department of microbiology—Dr. [Ernest] Jawetz and Miss [Lavelle] Hanna. We had a team between the Proctor Foundation and the department of microbiology.

Thygeson: Actually, Miss Hanna was the one that really did the important work on it, with our help. She's retired. She's living in San Francisco, and she was a very important woman in all that early work.

Hughes: She was a microbiologist?

Thygeson: She was a super technician. She never got a Ph.D., but she became really a super. She isolated the first trachoma agent in the United States and the first inclusion conjunctivitis agent in the United States, as a part of our team.**

* E Jawetz, SJ Kimura, L Hanna, VR Coleman, P Thygeson, A Nicholas. Studies on the etiology of epidemic keratoconjunctivitis. *Am J Ophthalmol* 1955; 40:200-9.

** E Jawetz, L Hanna, SJ Kimura, P Thygeson. A new type of APC virus from follicular conjunctivitis. *Am J Ophthalmol* 1955; 41:231-4. L Hanna, P Thygeson, E Jawetz, C. Dawson. Elementary-body virus isolated from clinical trachoma in America. *Science* 1959; 130:1339-40.

For about ten years we had a team going. It was headed by Dr. Jawetz, and I collaborated at Proctor. We had four or five in the team. Miss Hanna did most of the actual culture work.

Hughes: Did her name appear on the papers?

Thygeson: Oh, sure. Her name really should have been first, but Dr. Jawetz was first on them. He was the brains of the outfit, but he didn't do the actual work. That makes a difficulty—whose name should come first on the paper, the brains or the labor?

Hughes: I guess that's why a lot of people get around it by alphabetizing the authors' names.

Thygeson: Yes, that's one way to handle it. The way we worked it, we put first who did the most important work.

Hughes: Whether the conceptual or the laboratory.

Thygeson: Yes.

Hughes: Was that usually easy to decide?

Thygeson: It wasn't too difficult. Miss Hanna was extremely valuable. She deserves more credit than she actually got.

Hughes: Where had she been trained?

Thygeson: She had been in pathology at California [UCSF]. We stole her from the pathology department. We recognized that she was good, so we offered her more money than she was getting in pathology.

Hughes: That must have made you popular with the pathologists.

Thygeson: Yes, we weren't too popular after that.

Hughes: Are there other viral diseases associated with this Columbia period?

A New Form of Follicular Conjunctivitis

Thygeson: Yes. There was a very interesting disease still unworked out that occurred during the Columbia period while I was at Fort Apache. Dr. Proctor, Dr. Richards, and I were working at the laboratory, and we got a call from a doctor in San Diego. You've heard of Dr. Michael Hogan?

Hughes: Yes.

Thygeson: Well, his future father-in-law, Dr. Horace Merrill, called us—I had known him for some time—and told us that he had a new disease in San Diego. So Dr. Richards and I got in our little car and travelled out to San Diego. Dr. Merrill had about thirty cases of this follicular conjunctivitis, which was new. It wasn't trachoma and it wasn't inclusion conjunctivitis. We didn't know

what it was, but it was different from anything we had seen before. It was epidemic in San Diego. We made all the usual cultures, but we never could isolate anything. We couldn't transfer it to monkeys or other animals, and then it disappeared spontaneously. It didn't do any damage to the eyes.

Then it occurred again later on, about three or four times I ran into it. Each time I couldn't get anything out of it. So it's an unknown type of follicular conjunctivitis.

Hughes: Nobody has ever identified the agent?

Thygeson: I'm sure it's of viral origin, but we didn't get anything out of it.

Hughes: Has it always recurred in the San Diego area?

Thygeson: No. We had it in Arizona, and we had it in San Francisco, and I saw it in San Jose. Periodically it pops up, but we still don't know what it is. We haven't even got a good name for it. We called it after this referring doctor, Dr. Horace Merrill.

Then I worked on superficial punctate keratitis in New York, which I still think is a viral disease, but we haven't been able to go get anything specific out of it.

Primary Herpetic Conjunctivitis

Thygeson: Then I became interested in primary herpetic conjunctivitis, which was rather new to me for some reason or other. I wasn't familiar with the primary eye involvement in herpes. It's quite different from the recurrent type of ocular herpes.

Hughes: Had work been done on primary herpes infections of the eye?

Thygeson: I'm sure it had been, but it hadn't been written up properly. Most everything has been done before but not written up properly.

Hughes: Does the primary infection look different from a secondary infection?

Thygeson: Oh, yes, quite different. It's either a follicular conjunctivitis or a pseudomembranous conjunctivitis. In the recurrent type you get neither. It's completely different.

Hughes: So people prior to this paper were not even associating the two diseases?

Thygeson: They didn't associate them. This primary infection occurs in young children, one to five years of age. The pediatricians weren't recognizing its herpetic nature. But we had the culture methods, so we were called in because of the eye involvement. We were able to grow the herpes virus without any trouble. The pediatricians didn't recognize that it was herpes because it wasn't characteristic of the herpes that they usually saw. It wasn't the fever blisters of herpes. So we did a little teaching of pediatricians about how to recognize primary herpes involving the eye.

Hughes: How was it treated?

Thygeson: You didn't treat it because like most viral diseases it spontaneously disappears. It's not curable. Once you get herpes you have it the rest of your life because it goes in the ganglia, but the actual manifestations are self-limited. It lasts about two weeks or so and then heals spontaneously. It's better not treated. It's better to allow the natural course of the disease to occur.

Hughes: Why do you say better?

Thygeson: Because the treatment is often worse than the disease. That's something I learned back in Colorado days, that the less you treat herpes the better off you are. Some terrible treatments were advised for herpes in the early days.

Hughes: What sorts of things?

Thygeson: Well, carbolic acid. Carbolic acid was applied to the cornea for dendritic keratitis, which is entirely wrong. It was Dr. Finnoff who set me straight on it. He said, "Treat your ocular herpes the way you would treat a fever blister." Well, you don't use carbolic acid on a fever blister because the fever blister gets well spontaneously. The eye gets well spontaneously. The less you treat the eye the better.

Hughes: When you treat the eye with carbolic acid, you can't limit it to just the lesion, can you?

Thygeson: Yes. You use a little pinpoint applicator.

Then, they used a terrible treatment, which was seven percent tincture of iodine to the cornea. You wouldn't do that on the lip to a fever blister. I learned early in life to treat your herpes as you would a fever blister.

Hughes: Do you think it was your connection with Finnoff that made you somewhat leery of therapy? I've heard of your ideas about the use of steroids.

Thygeson: He was very knowledgeable about the side effects of medication. The overtreatment of herpes bothered him terribly. He crusaded, as I have done, to avoid overtreatment. They didn't have any steroids in those days.

**Chairman of the Department of Ophthalmology and
Co-Director of the Institute of Ophthalmology,
Presbyterian Hospital, 1939-1942**

[Interview 3: December 12, 1986]##

Thygeson: The last years at Columbia were interesting because the Knapp Foundation came in with Dr. von Sallmann. Then, because of Dr. Wheeler's death, I was appointed chairman of the eye department and co-director of the Institute of Ophthalmology. This was a disaster for my laboratory work because of so many visitors.

Our unit at Columbia was divided into three parts. We had the eye institute, and we had the laboratories over in the College of Physicians and Surgeons, and then we had the clinic at the Vanderbilt Clinic. So I had three ways to travel. In winter this was bad because we had to go outside to get to the eye institute. So when a visitor came to the eye institute, and I was working in the laboratory, it would take me five or ten minutes to get over to the eye institute and then waste all the time on the visitor. Most of the visitors were on their way to Europe, and they thought they ought to see the new eye institute.

Hughes: They were Americans?

Thygeson: Yes. Well, we had a lot of visitors, Europeans, too. Eventually the Presbyterian Hospital set up a visitor unit, and we had a multilingual woman in charge who would meet the visitors first and then start them out, decide which were worthwhile and which were casual. That saved a lot of time.

Hughes: What did she do with the ones who were disposable, so to speak?

Thygeson: Well, she took them on a tour herself, and then those that she thought were special, why she would refer to some staff member.

Hughes: Do you remember any particularly notable people who came through in those years?

Thygeson: Well, we had all of them. We had Dr. [Hermenegildo] Arruga, who was in retinal detachment, and we had Sir John [Herbert] Parsons. We had all the European notables—[Adolph] Franceschetti—because they all funneled through New York on their way to some other city.

Hughes: Did your research really come to a standstill?

Thygeson: Well, it was badly damaged.

Hughes: Had you realized that when you accepted the position?

Thygeson: No, I hadn't realized that I would be stuck with it because I thought my co-director, Dr. Dunnington, would do most of it, but he kept his office downtown, so all the local stuff I had to handle, including the problems of the hired help and disputes and everything. So it was really very annoying. I couldn't have lasted. So the war came along at a good time. I was on the essential list, so I could have stayed out, but I wanted to get in so I volunteered. I got in in September of 1942.

Hughes: Before we get that far, I wonder why the decision was made to have co-chairmen. Wasn't that unusual to have two people running an institute?

Thygeson: Well, Dr. Dunnington was the logical choice. He had been the associate director. But he didn't want to be full time, and he wanted to keep his office downtown, so they compromised by making co-directors. He was strictly clinical, and I was more laboratory than clinical. It would have worked out all right except for the visitors. Anyway, it ruined my last year of laboratory work.

Hughes: You said off tape that probably Iowa was one of the best times because there wasn't any administrative work.

Thygeson: That's right. I always said it was off the beaten travel, so only the visitors came who really wanted to come and were interested. So we didn't have all the unnecessary visits. Anybody going through New York felt that they ought to go and see the new eye institute. So we had this rush of visitors.

Hughes: I read intimations of disagreements, I believe, with the dean of the medical school and certain members of the faculty?

Thygeson: Since I was the chairman of the eye department and co-director of the institute, and I was also one of the three members on the board of trustees of the Knapp Foundation, I had to see a lot of Dean Rappleye. Dean Rappleye was a real autocrat of the old school and a very difficult character. I found that I could work very much better with his associate dean, Vernon Lippard, who later became dean of Yale Medical School. So I channelled most of my contacts through the associate dean, and it was very satisfactory.

Rappleye was opinionated and very difficult to get along with. For example, he would only talk to the heads of departments. He wouldn't have anything to do with the underlings in any department. Now, that isn't the way it should be. It isn't the way it was at Iowa, where everybody knew the dean. They were all good friends. It was a complete shift from Iowa.

Hughes: Was Rappleye a generation before you?

Thygeson: Oh, yes, he was older. He died shortly after the war started. He was not, in my opinion, a desirable dean. He failed to communicate with the younger people, and he was autocratic. He couldn't have lasted at all now, because the trend is now really to have the dean as the secretary of the faculty. But in this day, the faculty, of which I was a member, was a group of senior staff members who advised the dean. They didn't tell the dean what to do; they advised him only, and he did what he wanted irrespective of what the feelings of the faculty were.

Hughes: Were there specific problems in reference to the department and the institute, things that weren't happening or you weren't getting?

The Basic Science Course in Ophthalmology (continued)

Thygeson: No. The real problem that came up was that Rappleye demanded a basic science course be set up. He delegated me to set it up. We weren't prepared to do this. We didn't have money to finance it, so it was a rather difficult situation, but we managed it, and we started it.

Hughes: The course was intended for whom?

Thygeson: For all the eye residents from all over the city. It was a big affair. I had to round up the physiologists and the biochemists and everything, but I didn't have money to pay them honoraria or anything. So this was a little difficult. Dean Rappleye didn't raise any money for me to help on this.

Hughes: *So they were doing it on a strictly gratuitous basis?*

Thygeson: Yes.

Hughes: *Did they come from within Columbia Presbyterian?*

Thygeson: Mostly from within, but some from without. It worked out eventually all right, but it was a little difficult. Then Rappleye wanted to set up for a degree of doctor of medical sciences for a three- or four-year resident. Well, we didn't have any money for that.

Hughes: *A doctor of medical science was supposed to go into research?*

Thygeson: Yes. If a resident showed talent in research, why he would be a candidate for this degree, which he had to pay a tuition for, and then he had to take an examination, and he had to write a thesis.

Hughes: *This was four years beyond the M.D. degree?*

Thygeson: Yes. And quite a number of the residents chose to do this. [tape interruption]

In my point of view it worked out very well except the difficulty in raising money for honoraria. See, when Dr. Wheeler died, our income dropped, because he contributed \$80,000 a year from his private practice to support the research unit. When he died, that was gone, so we had to get other sources for the research program. Presbyterian Hospital came through very nobly, but not in the amount that we needed. Then Dean Rappleye either couldn't or wouldn't raise money, so we got no money from him. When the Knapp Foundation was set up, then a little money trickled in through that. We got Dr. von Sallmann's services, and he was very good in this basic science course, and we didn't have to pay him any honorarium.

Hughes: *Von Sallmann had been with the Knapp Institute?*

Thygeson: He had been with the Knapp Hospital, but then he came up when the Knapp Foundation was established at Columbia—not in the Presbyterian Hospital, incidentally, where the institute was. Then I had to find laboratory space for him. We didn't have any laboratory space, so I manipulated Dr. Castroviejo out of his laboratory. He originally did quite a lot of laboratory work—rabbits and everything—but then when his clinic took over, he used his laboratory just to show visitors. A bit of swank, you know, if he showed visitors that he had a laboratory. He didn't do any work there, though, so I manipulated him out of that laboratory and gave it to Dr. von Sallmann. It worked out very well. Dr. von Sallmann was a great asset to the whole thing.

Anyway, we did develop a good basic science course, and we did have a good doctor of medical science set up.

Hughes: *Was that a unique degree?*

Thygeson: Yes.

Hughes: *No other institution offered it?*

Thygeson: No, not at least in the eastern area, that I know of. It stimulated some of the residents to do real good research work. We had about half a dozen very good theses out of that program.

Hughes: *At a time when most physicians were not oriented towards research.*

Thygeson: That's right. I liked it. It was very good. I had wished that they had started it here in California, but there was no support for it here at all.

Hughes: *The University of California at San Francisco, until recently, has not been a very research-oriented medical school. Isn't research orientation in medical schools largely a post-World War II in phenomenon?*

Thygeson: Yes, that's right. It was a service institution, not a research institution. But the University of California at San Francisco now is a very major research institution.

Hughes: *It was during this period at Columbia, I believe, that you built a house.*

Thygeson: Yes. We built a house in Tenafly, New Jersey. I remember it was about a twenty-minute commute from the eye institute on a bus from Tenafly. The only bad thing about it was in winter. See, everybody smoked except me, and they had their windows closed because of the cold air, so I suffocated morning and night from this tobacco smoke, which I didn't like at all. They don't do that anymore. It was terrible.

Hughes: *Was it a productive time? Were you riding with some of your colleagues?*

Thygeson: Sometimes. Sometimes with Dr. [A.E.] Braley. He also lived in New Jersey, so we often took the same bus.

Hughes: *He was an ophthalmologist?*

Thygeson: Yes, he was on our staff. Dr. Braley worked with me often down in Fort Apache. He worked on trachoma. He was an investigator, and he came on our staff at Columbia for several years. Then he became chairman at New York University.

Hughes: *And his specialty was trachoma?*

Thygeson: His specialty was pathology with reference to external disease, so trachoma was one of his interests. He was a good investigator because he had had good training in Iowa. He was an Iowa graduate.

Mrs. Thygeson's Editorial Work

Hughes: *Mrs. Thygeson, you had two little children at that stage, I believe?*

Mrs.
Thygeson: Yes.

Hughes: *Were you already involved with your husband's work? Were you doing editing at that stage?*

Mrs.

Thygeson: For him. And then I don't know how you got started with those colleagues sending manuscripts home to me, but they sure as heck did. They advertised me.

Thygeson: They liked the way you worked on mine, so they wanted to get a part of it, I guess.

Mrs.

Thygeson: You must have told them.

Thygeson: Anyway, four or five all got help from her.

Mrs.

Thygeson: I had a lovely clientele from the eye institute for whom I did editing.

Thygeson: Dr. von Sallmann was one of them, and Dr. Reese.

Mrs.

Thygeson: Dr. Reese was the best one.

Thygeson: You edited his book on pathology.*

Mrs.

Thygeson: I finally did his book. He referred to himself as that illiterate southern boy from Georgia. [laughs] He was very generous in his praise. He sent me red roses.

Thygeson: Doctors ordinarily are very poor writers. Their handwriting is bad, too.

Hughes: *Is editing a natural talent of yours?*

Mrs.

Thygeson: In a way, but I must give my father credit for this. He was in love with the English language. There was hardly a dinner at our house while I was growing up that the unabridged dictionary didn't land in the middle of the table because we were looking up something to set me straight. So I got a great feeling for words and use of words and for rhythm in writing, a kind of rhythm, if you know what I mean.

Hughes: *I know very much.*

Mrs.

Thygeson: But I wouldn't say I had ever had any formal training. I did have one marvelous course at Stanford University with Mrs. Russell in the English department. I think it was a required course, but she was excellent. When

* *Tumors of the Eye*. New York: Hoeber, 1951.

she edited something that I had written, she let me know it. It wasn't just glossed over; it was *corrected*. It entertained me to do it, and I really did the first that I ever did for you [Dr. Thygeson].

Thygeson: The first paper I wrote, you edited.*

Mrs.

Thygeson: And right on through.

Thygeson: All the way through, yes.

Mrs.

Thygeson: He was very generous in accepting my suggestions.

He took me and—some won't.

Thygeson: What I learned was in high school taking composition. Miss Carheart, the teacher, told me, "Your sister can write a better composition than you can."

Mrs.

Thygeson: She was three years younger. [phone rings]

Hughes: Does the fact that you built a house mean that you thought you were going to be staying in New York for a long time?

Thygeson: Yes. Actually, the war coming along changed our minds. We didn't decide on going to California until maybe our third year in the war.

* WC Finnoff, P Thygeson. The finding of *Bacterium granulosis* (Noguchi) in trachoma. *Am J Ophthalmol* 1929; 12:651-2.

III OPHTHALMOLOGIST IN THE MEDICAL CORPS, U.S. ARMY AIR CORPS, 1942-1946

Recruitment

Hughes: You've already said that soon after war was declared, you volunteered. Why the army air corps?

Thygeson: There was a very attractive recruiter—I've forgotten his name now—who was making a kind of fetish of recruiting young university faculty for the air force. He wanted to get the university group in there, not just the general practitioners. So he made this big effort. He was quite successful in getting his staff mostly from the universities rather than from general practice. What he wanted was the best for the air force. He thought the university people were the best. Somehow I got caught up in it.

Mrs.

Thygeson: Wasn't there another reason?

Thygeson: I don't remember another reason.

Mrs.

Thygeson: What I remember was the reason he wanted a commission, he wanted to fight those Nazis. But he thought that they would send him overseas if he were in the air corps.

Thygeson: Well, what this particular recruiter stressed to me was that the air corps would have hospitals in North Africa.

Hughes: Were you thinking of trachoma?

Mrs.

Thygeson: That's one place he could really do a service.

Thygeson: I knew the French doctors over there, and so I was under the impression that I would be sent to North Africa. But then it so happened that the army prevented the army air corps from having hospitals outside of the United States.

Hughes: What was the basis for that decision?

Thygeson: I don't know the reason. Jealousy, I guess. They didn't want to have two kinds of hospitals overseas. So after much palaver, they decided the army would do the overseas and the air corps would do the domestic.

Hughes: Exclusively?

Thygeson: No, the army also had some hospitals in the United States, but not to the degree that they would have had if the air corps hadn't had hospitals. But the air corps was very large, so there were a lot of air force hospitals scattered all over the United States.

Hughes: Was there any basic difference between the army and the army air corps hospitals?

Thygeson: Well, there were many more university people in the air corps, and there was a little tendency to be a little bit snooty about looking down on the army. There was a little truth in it. Better medicine was practiced by the air corps than by the army.

Hughes: Obviously from the number of papers you published during the war, you had opportunity to do research and write up your research.

Thygeson: Yes. The air corps encouraged that. The army didn't encourage it, but they didn't prevent it. But the navy actually prevented it. The navy did not want research done.

Hughes: Why?

Thygeson: The navy felt that if you had an M.D. degree, you were an all-purpose person. They didn't want any specialties in the navy. A doctor was a doctor, old-fashioned. He took care of everything. Well, the air force valued the specialties, and the army did so to a lesser degree. The air force was very good in that respect. They really encouraged research, and since they were university people, they just carried on with what they had been doing at home. So did I.

*Hughes: I have yet to get a copy of ophthalmology in World War II.**

Thygeson: I've seen a copy of that. It was done by the regular army, not by the air force, and so, of course, it was slanted to the regular army, and the air force wasn't very well treated. I remember that story.

* JB Coates, et al., eds. *Surgery in World War II: Ophthalmology and Otolaryngology*. Medical Department, United States Army. Office of the Surgeon General, Department of the Army, Washington, D.C., 1957. (Dr. Crowell Beard graciously lent his copy).

Hughes: You were sent to Drew Field near Tampa, Florida. Is there a story behind that?

Thygeson: I was first sent to Morrison Field, which is at West Palm Beach [Florida]. We had what was called an officers' training course. It was a three-month course where you got the rudiments of how to be an officer. In three months.

Mrs.

Thygeson: They called them the ninety-day wonders.

Thygeson: Well, we were green; we didn't know anything about the army. We had to drill, and we had to do parachute jumping and use a rifle and a revolver, and we had lectures on tropical diseases.

Mrs.

Thygeson: And decorum generally.

Thygeson: It was a very intensive course in three months.

Hughes: Were you all physicians?

Thygeson: Yes.

Mrs.

Thygeson: I had a volume called *The Officer's Wife*. It scared me to death.

Thygeson: This course was run by a young surgeon from the University of Rochester whose name was Hrolfe Ziegler. He was a marvelous commandante for those three months. He was just perfect. He didn't do so well later on, but for that course he was just perfect. He had been a reserve officer, so he knew the military. But he ran it in a very effective way. We had drill, and we had hiking, and we had all the rudiments of how to be a soldier.

Hughes: Was the course in infectious disease well taught?

Thygeson: Yes.

Hughes: Did Ziegler teach that?

Thygeson: No, he brought in visiting professors. It was very well done, very intensive. I liked it.

Drew Field, Florida

Hughes: Were you destined to specialize in ophthalmology in the war?

Thygeson: Yes.

Hughes: They weren't expecting you to be a general practitioner?

Thygeson: No, but they expected us to have some general knowledge. For example, we had to be officer of the day on rotation, and during that time you did everything.

Mrs.

Thygeson: It was a twenty-four hour duty.

Thygeson: About once every ten days or so, I had to be officer of the day. Then you took care of all emergency injuries and all the illnesses of the personnel, and the—

Mrs.

Thygeson: Families.

Thygeson: Yes, the families, and the soldiers, and so on.

Hughes: Did you like that?

Thygeson: It was very good training, and we had good consultation. If you got into difficulty, you could get consultation right away—all the burns and broken legs and everything. Aviation at that time was very risky, and we had all kinds of accidents, aircraft falling out in Tampa Bay. We had a fast boat on Tampa Bay to get out to the crash.

Hughes: You weren't getting men from overseas yet?

Thygeson: Not yet, no. We had a very busy schedule at that time. All these soldiers in training, we had to examine their eyes.

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Thygeson: Originally we had both fighters and bombers, and then we kind of graduated to the B-17, and they built special fields for the fighter planes. The signal corps, radar, became very important. So radar was all channeled through Drew Field, I think for the whole air force. So we had as many as fifty thousand men on the field at one time, and the majority was signal corps. Our air corps was five or ten thousand, but then we had fifty thousand all together.

Hughes: Did radar come in at the beginning of the war?

Thygeson: Yes. So training radar units was very important. These signal corps men were in a way kind of the rejects from combat soldiers. They had some disability, and the eyes were often part of that disability. So we had a lot of problems. The air corps was too busy to have much in the way of sickness, but the signal corps had a lot of time on their hands, so they developed all these eye complaints and all the other complaints. Looking in this radar unit eight hours a day, why then their eyes would begin to hurt, and then they would come into the eye clinic.

Remember when we had that one soldier who came in complaining about the spots before his eyes, especially on the sands, the white sands of Florida? His name was Seymour Dotz. So we thought that was very appropriate. [laughter] We also had a signal corps man named Luke Warm. We liked Luke's name.

Hughes: You mentioned Ziegler and chuckled about what happened to him later. Is it worthwhile picking up on this thread?

Thygeson: Ziegler became the chief of surgery at Drew Field about a year or so after this officers' training program. He didn't do well as chief of surgery because he was so autocratic and he ruffled so many feathers. He was a good surgeon and all that, but he developed a very autocratic way of thinking.

Mrs.

Thygeson: Executive complex.

Thygeson: Yes. So he wasn't as good as he should have been, but he was very friendly with us. Ophthalmology was under surgery officially. It was a subdivision of surgery.

Hughes: That was true at certain universities as well, was it not?

Thygeson: Oh, yes, that was a rule at most universities. But it was definitely true in the army and air force. Ziegler was the boss, so everything had to be funneled through him.

Hughes: Yet dealing with pilots I would think that ophthalmology would be given quite a bit of weight.

Thygeson: Well, it was. We had to examine, of course, all the pilots, and take care of all their troubles.

Hughes: Did they have any particular problems?

Thygeson: Well, one problem that was very annoying was pterygium. A lot of the pilots were from the South. For some reason or other the Texans and Southerners were attracted to the air force. The chief of the air force was Texan. The Texans liked to fight, and they loved the air force. There was a lot of pterygium, which is a little growth on the eye, which is very common in the South in people exposed to a lot of sun.

Hughes: It's benign?

Thygeson: It can be. There are many borderline cases, and the question was whether to operate on them or to let them go. This was a source of trouble. Then we had muscle imbalance. See, if a person has a muscle imbalance, their depth perception may be off. For a pilot it is very important that he can judge depth. When he lands, he has to know where he is.

Hughes: Were there good tests for that?

Thygeson: Well, one of the research projects was to work on depth perception and to see who was safe and who wasn't.

Hughes: *Were you involved in that research?*

Thygeson: I wasn't involved in that research, no. I was in the testing, but not in the research.

I think the funniest thing that happened was, we were examining Pan American pilots who were being inducted into the air force. They were for trans-Atlantic service, ferrying the planes across. There was one fellow whom I examined in the dark, and for some reason or other I took his [ocular] tension on my fingers. The old folks used fingers to check tension. His eye was hard as a rock. So I found he was wearing contact lenses. Contact lenses were prohibited—they had just come in—in the air force. Without the contact lenses, he had about a tenth normal vision. But he had been ferrying planes across for Pan American for a couple of years. He was perfectly safe, but I had to let him go just because of regulations. There were a lot of things like that.

Hughes: *What about infectious disease? Were you seeing anything of note?*

Thygeson: Yes.

Mrs.

Thygeson: Tell her about your trachoma uncovering.

Thygeson: Well, see, since I was interested in trachoma I examined everybody carefully, and I found about twenty-five cases of trachoma.

Mrs.

Thygeson: That had never been diagnosed.

Thygeson: I think I wrote a paper on that.*

Hughes: *Could you trace those men back to the trachoma belt?*

Thygeson: Often, yes.

Then another condition we ran into was meningitis. We had an epidemic of meningococcic meningitis. The eye was involved in a small proportion of that epidemic, so we had quite a number of eye infections with that epidemic.

Hughes: *How was that treated?*

Thygeson: Well, we didn't have anything to treat it with at that time, so it usually ran its course. But as soon as penicillin came in, it was very effective. Then sulfas worked to a certain degree.

* P Thygeson. The diagnosis and treatment of trachoma. A report of 19 cases in military personnel. *Military Surgeon* 1945; 97:355-61.

- Hughes: Do you know anything about how penicillin was distributed in those years? It wasn't just something that you went down and bought, was it?*
- Thygeson: No, it had a priority basis, but the air force had a high priority, so we had no problem. We could get anything we wanted on the basis of priority.
- Hughes: There never was a problem with shortage?*
- Thygeson: Not in the air force, but there was in the general population.
- Hughes: Was it being made by drug companies at that point?*
- Thygeson: Yes. You see, it was a rush order, and it was a remarkable development of mass production of penicillin by American drug houses. See, the original work was done in England. Then when the United States came in, there were about four drug houses that came in, in a big way. Pfizer was one of the important ones and did a marvelous job of building great big vats to grow the mold.
- Hughes: That must have been a war-priority industry.*
- Thygeson: It was. There was a recent television program that was devoted to this. It started with [Alexander] Fleming and went through the English and Australian contributors. [Howard W.] Florey was an Australian working in England. He was mainly responsible for the English development. Then when the Americans came in, the quantity got up in no time. By the end of the war there was plenty of penicillin.
- Hughes: It must have had an impact on how the war turned.*
- Thygeson: Well, it did, because the war wounds, which were such a problem, became much less so because we had sulfa and penicillin. Resistance had not been built up to either one. Now, the [effectiveness of the] sulfas has been very badly damaged because so many organisms are resistant. The same way with penicillin. But at that time there wasn't any resistance, so they had a remarkable effect. Penicillin was a marvelous thing for eye infections.
- Hughes: Had the dosages been worked out scientifically by then?*
- Thygeson: Pretty well, yes. There was a mass program, and it was a little bit like AIDS. You know how much has been accomplished on AIDS in two years? Well, the same thing happened with penicillin. The massive effort paid off.
- Hughes: Was the penicillin relatively pure?*
- Thygeson: It became pure. Contaminants were not a problem, as I remember. It was used very freely. There weren't any problems, and we didn't run into the complications that developed later. Every new drug that comes has a kind of honeymoon period before the complications develop.
- Hughes: Why is that?*

Thygeson: I don't know why it is, but they say if you want to use a new drug, use it right away before the complications start.

Penicillin was a tremendous victory. It saved a lot of lives in the war. It saved a lot of eyes that would have been lost from sympathetic ophthalmia, which was a terrible curse in World War I.

Hughes: You were seeing that in World War II?

Thygeson: I didn't see any sympathetic ophthalmia at all in World War II, but I had seen the residue of sympathetic ophthalmia from World War I. When I came into ophthalmology, we were still seeing the soldiers from World War I.

Hughes: What are the symptoms?

Thygeson: Well, it's a granulomatous uveitis that can destroy the eye. It's an autoimmune disease.

Hughes: Why wasn't it seen in World War II?

Thygeson: Well, I think the wounds were cleaned up fairly rapidly, weren't neglected, and the infection was controlled with penicillin and so forth.

Hughes: So it just never was allowed to take hold?

Thygeson: Yes. Penicillin changed the whole picture.

Hughes: Were there other eye diseases that were feared might have an adverse effect on the troops?

Thygeson: Well, we had the problem of epidemic keratoconjunctivitis that we discussed. It turned out to be caused by adenovirus 8. That was feared. In the first part of the war it did do some damage, not in the troops but in the factories. It looked as though it would spread to the troops, only it never did.

Hughes: And there was nothing else that reached epidemic proportions?

Thygeson: Have you heard of solar retinitis? If you look at the sun you get a burn of the macula, the burning glass effect. Well, we had a rash of that in the gunners, because the Japanese fighters always came out of the sun, so the gunners were looking into the sun, you see.

Hughes: The Japanese did it deliberately?

Thygeson: Yes. So the gunners looking into the sun got burns of the retina, and we saw a lot of that.

Hughes: What could you do for that?

Thygeson: Protective glasses were developed.

Hughes: But once the burn was there—

Thygeson: No, you couldn't do anything about the burn. But then there was a similar-appearing macular lesion, central serous retinopathy, that came on in people that were not exposed to the sun, particularly air force and submarine people. It was supposed to be related to stress, the stress of combat and so on. A lot of research was done on that. It was never, to my mind, ever worked out properly. A number of the doctors got this maculopathy, supposedly on a stress basis, not by looking at the sun.

Hughes: *Is it still a recognized disease?*

Thygeson: Yes, it's still around, but not in epidemic form.

Hughes: *And it's still thought to be due to stress?*

Thygeson: That's one theory. It really hasn't been worked out properly. Every time we have an eclipse, we have solar burns. That was a big research problem in the air force at that time.

Hughes: *Where was this research being performed? At the different military hospitals?*

Thygeson: Yes. So at Drew Field, which was the headquarters of the Third Air Corps, we had a lot of research projects. Every specialty had research projects.

Hughes: *Were there adequate laboratory facilities?*

Thygeson: Yes. They built very good laboratories and instruments. All I had to do was call the adjutant in Washington, D.C., and he would fly down the instruments from Washington, so it was very satisfactory. Anything we wanted we could get.

Hughes: *I understand you arranged to have your friend, [Alexander] Rodman Irvine assigned to Drew Field.*

Thygeson: Yes, I just got a letter from him last week.

Hughes: *How had you known him?*

Thygeson: Well, at meetings where I would have an exhibit, why Rod would come around the exhibit. He was very curious, a real student.

He wanted to learn everything about everything. So I got very well acquainted with him at meetings. I've also met his father, [Alexander] Ray Irvine, who was an ophthalmologist, not a curious ophthalmologist, but a very fine fellow. Rod got in the air force the same way I did, through this recruiter. Rod was at the University of Southern California. So Rod got put into a small hospital up in Marin.

Mrs.

Thygeson: Hamilton Air Force Base.

Thygeson: This was beneath his talents, you might say, too small an institution.

Mrs.

Thygeson: Besides, you needed him.

Thygeson: Yes, I needed him, so he and I together manipulated the Surgeon General's Office in Washington and got him transferred.

Hughes: Did you need him for anything specific?

Thygeson: Well, he was real good on muscles, which I was not, and muscle problems in the air force were very important.

Mrs.

Thygeson: He wasn't a full professor at that time, but he was a typical professor, if you know what I mean.

Thygeson: He was actually assistant clinical professor at USC [University of Southern California]. His father was chairman of the department at USC.

Hughes: There's a third generation of Irvine ophthalmologists?

Thygeson: That one [Alexander Rodman Irvine] is here at UCSF.

Anyway, I got Rod out, and Rod was very valuable because he could do things that I couldn't do. For instance, he was a good surgeon, and surgery was a minor thing to me. There were a lot of problems, particularly with the families of the soldiers on the base, that Rod could take care of that I couldn't or didn't want to.

Hughes: Was it just the two of you ophthalmologists at Drew Field?

Thygeson: No. We had as many as fifteen ophthalmologists at one time. But, see, we were the central base. The new man being recruited would come to us, and then he would be sent out to the various hospitals. So we always had a pool. We called it a pool. They would stay there for anywhere from a week to a month prior to their being sent out to some other area.

Hughes: But you always stayed put?

Thygeson: Rod and I would stay. We were permanent. We were not subject to being moved. We had conferences and everything.

Rod and I were rivals in a little way because he had been trained at Harvard in refraction by [Walter B.] Lancaster, and I had been trained at Colorado by Edward Jackson. Jackson and Lancaster started the American Board of Ophthalmology [1916] and they were rivals on physiologic optics. They had two different methods of refraction. So I had the Jackson method, and Rod had the Lancaster method. We would compare notes.

Hughes: Can you tell me briefly what the difference between the two methods is?

Thygeson: Well, it was a matter of testing for astigmatism, which is an important part of refraction. Lancaster had a dial on which he mapped out the astigmatism. Jackson had a cross cylinder, the Jackson cross cylinder. I could work out the

axis and strength of the astigmatism in about half the time that Rod could with the Lancaster dial. So we had a lot of fun. We would check each other's refractions.

Hughes: Did you convince him that your way was better?

Thygeson: Oh, yes, he changed over to the cross cylinder; it was so much better than the Lancaster dial.

Hughes: Was the rest of the world convinced of that?

Thygeson: Yes, everybody uses the cross cylinder now. I don't think the dial is used at all any more. Jackson's cross cylinder was far superior, but it took quite awhile to get it used routinely.

Hughes: You and Dr. Irvine apparently spent a lot of time talking ophthalmology?

Thygeson: Yes. We did a lot of that.

I think one interesting thing that we did was we had a young man in the signal corps. Each unit of the signal corps had a doctor attached to it. We had a Philadelphia boy by the name of Joe Hallett who was attached to one signal corps unit in training. He hung around the eye clinic as much as he could because he wanted to be an ophthalmologist. So we got Joe transferred to us from the signal corps. Rod and I took him on, and we had him for about two years, I guess. Every night Joe worked on his books, and we gave him all the training we could. Then later on when he took the American board [exam in ophthalmology in 1946], he was number-two man.

Mrs.

Thygeson: Without any other training except what he got at Drew Field.

Thygeson: He was really good. He went on the staff at Wills Eye Hospital in Philadelphia.

Mrs.

Thygeson: He was very serious. He really wanted to do this.

Thygeson: So Joe Hallett was mostly preceptor-trained, but he actually did better than a residency because he had constant supervision.

Mrs.

Thygeson: Constant talk, training. It was wonderful. He was a wonderful fellow. A lovely wife.

Thygeson: See, Rod was a born professor, because he liked to teach. So he really did his best.

Mrs.

Thygeson: He looked like a professor. [laughs] He always looked a little worried. A wonderful guy.

Hughes: Your houses were next to each other on the beach in Florida?

Thygeson: Next to each other on the beach. So we were in the same car pool all the time.

Hughes: *Was it a considerable ride?*

Thygeson: Thirty miles from Clearwater Beach to Tampa. So we had this eight- or nine-man car pool. Each one had a separate day off during the week. It worked out we would have seven in the car pool at one time—Hrolfe Ziegler, Van Robinson, S.R. Irvine, Lynn Cooper, and me.

Mrs.

Thygeson: Rod's wife was a biochemist and had these three little boys like little stepping stones. At that time they were little boys.

Hughes: *Was she a practicing biochemist?*

Mrs.

Thygeson: No, she wasn't really doing anything specifically.

Thygeson: She was very interested. And you know the three boys did remarkably well. One's a physicist; the other one is an orthopedic surgeon, and then the ophthalmologist. All three have really done remarkably well.

Hughes: *Was there anybody else of note in that seven-man pool?*

Thygeson: Well, we always had a good dermatologist, a good psychiatrist, always a good surgeon. For a while we had an orthopedic surgeon. We had the first chief of medicine, Lynn Cooper, and the second chief, Van Robinson. We had two chiefs of medicine there in the car pool.

The car pool was very valuable because we could discuss the cases of the day. It wasn't the usual boy talk. It was all medicine.

Hughes: *Day after day.*

Thygeson: Every day, and so it was good. Then we had a good library there at headquarters. So I had a lot of chances for reading. It was a rather remarkable unit because it was young university people, without exception.

Mrs.

Thygeson: Didn't you have a journal club?

Thygeson: Yes, we had a journal club. We had a weekly seminar.

Mrs.

Thygeson: These were extracurricular things.

Hughes: *A journal club for all the specialties or just for ophthalmology?*

Thygeson: Just for ophthalmology. Then we had, I think, one interesting feature that impressed me—our Saturday night parties. [laughs]

Mrs.

Thygeson: Those were purely social, but they were awfully funny because Saturday night we were out of school. The way we had never been in our lives and never would be again.

Thygeson: We had the best house on the beach, so everybody came to our house. We could go to bed, and then they would come in and wake us up.

Mrs.

Thygeson: Tell us it was Saturday night. Sometimes we would turn out all the lights and scuttle into bed early, but on the whole we enjoyed it immensely.

Thygeson: There were two or three old fogies (retreads!) on the unit. They had been in World War I. There was Clyde Cooper, the chief of medicine, and then the urologist, Shultz, and a surgeon. Otherwise they were all young people. I became forty while in service there, so I was an old fogey. I was the first one promoted. I was promoted from major to lieutenant colonel. There was a big celebration because everybody took this seriously. So there was a party to end all parties for my promotion.

Hughes: *At your house?*

Thygeson: Yes. There was a lot of fun connected with the party.

Mrs.

Thygeson: The one and only time in his entire life—I can testify to that since I've really been in on most of it—he was pie-eyed. [laughs] He was gloriously drunk. He never has been before or since.

Thygeson: The promotions came through pretty regularly after that. The one feature I was impressed by was the young pilots who were promoted early and rapidly. You would see baby-faced pilots as majors; they were promoted much faster than they would have been in the regular army.

Hughes: *Was that because of the hazards?*

Thygeson: I suppose so. Anyway, they had a hazardous life. There were a lot of injuries and a high mortality rate. The unit at Morrison Field in West Palm Beach ferried the planes over to Africa and to Egypt and so on. There was a lot of loss of life.

##

Thygeson: The surprising thing to me was the number of Texans. Texans loved the air force. Of course, they did a good job.

Hughes: *Did you ever consider making a career in the military?*

Thygeson: No, I never did. I had planned to go back to Columbia until the last year of the war. But I liked the military service. I really enjoyed it because I didn't have the financial worries that most of the people in practice had. We never

had a big salary. We spent five thousand dollars over our income, but we made it up on our sabbatical, because Columbia gave me a half salary, \$5,000, for a sabbatical, and it just evened it out, so I ended the war even.

I enjoyed the military because I liked everything about it; it was a good experience, and I learned a lot of general medicine which served me later.

Mrs.

Thygeson: We had a sailboat. Our sonny boy [Fritjof Thygeson] dug it up out of the mud and resurrected it. We all resurrected it; we all worked on it over an entire winter, even Krissie, who was a little eight-year-old. She scrubbed the sails. It was a racing cat, and it had been a beautiful boat. We got it for twenty-five dollars. Fritjof came home and said, "I found a boat; I found a boat! Can I have it?" We said, "You can have it if you can find the owner and ask him if you can have it." So sure enough, he found somebody, and he got hold of the owner, and the owner said, "Aw, you can have it for twenty-five dollars." It never would have been resurrected; it would have been just sunk. I was procurement. Every time I went to the village I had a list of things to get for the boat. [tape interruption]

Hughes: *Dr. Thygeson, the next step is the return to Columbia, unless you've got more to say about Drew Field.*

Thygeson: One of the last things at Drew Field that was interesting was when we got all the Eighth Air Force returnees from England. After they had done their fifty flights, they'd come back to Drew Field, usually to be mustered out. Many of them had psychological problems, and they'd have a lot of eye problems, too. So that was an interesting time. We'd finished practically all the training.

One feature that I'd like to mention is the importance of the old army master sergeant. Did I mention before how he saved our lives?

Hughes: *No.*

Thygeson: Anyway, in the training period we were told by our commanding officer and our supply sergeant to service the navy, which had units at Dunedin, Florida. They had water buffalo training for the marines at Dunedin, and then they had a regular naval service at St. Petersburg on the ocean. So we had to service both the marines and the navy. We were told to give them the same care that we gave our men; they were allies of ours. And so we did. We were able to give glasses without charge to the people who needed them—not the families but just the service men. So we did that for the marines and for the navy.

Hughes: *Were you providing total eye care for them, not just eyeglasses?*

Thygeson: Yes, everything for them. Towards the end of my time there at Drew Field we suddenly got a bill from the fiscal office in St. Louis, to the amount of some thousands of dollars, for glasses that we had prescribed to the navy without authorization. I forget how many thousands it was.

Mrs.

Thygeson: It was five, anyway, or seven.

Thygeson: Something like that. It was to be divided between me and the commanding officer and the supply sergeant who had authorized our service.

Mrs.

Thygeson: We didn't have an extra dime. We were just living from hand to mouth and taking care of these two children.

Thygeson: So the master sergeant—he had become a major at that time—said, "Let me handle it." He started a letter to St. Louis, with reply by endorsement, and had these letters going back and forth. He got after the navy in Washington, and after some months the navy agreed to pay for this without charging us. So this master sergeant got us off the hook.

Mrs.

Thygeson: It showed the magic of this marvelous business of answer by endorsement. That meant you had to answer this letter; it was a command.

Thygeson: You couldn't hedge on that; you had to answer. He knew just how to do it, and he knew how to handle the navy. He knew all the ropes. That's a lesson I never forgot, because I used it myself later on handling the [U.S.] Indian [Health] Service bureaucracy. The Indian Service bureaucracy was pretty rough.

Hughes: You mean the letter by endorsement?

Thygeson: Yes, all that stuff.

Valley Forge General Hospital, Phoenixville, Pennsylvania

Thygeson: And then I was [transferred into the regular army and] moved to Valley Forge General Hospital, which was the big army plastic center.

Hughes: Why that move?

Thygeson: Because the war on the European side was over. There really was a big drop-off on air force activity that really took Drew Field down to the size of the original headquarters for the third air force.

Hughes: When was this?

Thygeson: It was the last year of the war, after the European war closed down. I was transferred up to [the eye center at] Valley Forge because the ophthalmologist, Elliot Randolph, there wanted to get out of the army, so he manipulated to have me go up there.

Hughes: Did you know him?

Thygeson: Oh, yes, I knew him. I was there for I don't know how many months. Anyway, this was a big plastic service and tremendously valuable for anybody interested in plastic surgery, which I was not.

Hughes: Had you done some in Colorado?

Thygeson: Oh, yes. I had assisted in a tremendous amount of plastic surgery and done a little on my own—not much. I wasn't interested in plastic surgery, but it was good training for me. We had a tremendous number of patients (870) in the hospital; it was a tremendously big hospital.

Hughes: These were mainly boys who had been in combat?

Thygeson: Yes, and anything of urgent nature got funnelled into Valley Forge, so we had all kinds of other things.

Mrs.

Thygeson: It was interesting being at Valley Forge, too. Historically fascinating.

Hughes: Had plastic surgery made tremendous leaps during the war?

Thygeson: Yes, it had. There was the eye plastic service, and then there was the general plastic service. It was a marvelous hospital. There was plenty of money and plenty of help for secretarial work. Record keeping was always a problem, and we always had good secretaries for record keeping.

Hughes: Was it just eye plastic surgery?

Thygeson: It was mainly eye, and then the other part was general plastic surgery, but it was really facial and eye plastic surgery.

Hughes: Did you notice changes in technique in that period since Colorado?

Thygeson: Yes. There was a lot of development in plastic surgery at that time, and a lot of it was right in the army. The army did much more than the air force did at that time in plastic surgery.

Mrs.

Thygeson: The children and I came home here [California], incidentally. I never went to Valley Forge.

Hughes: Why?

Thygeson: Because we didn't expect it would be very long. The war was almost over.

Dibble General Hospital, Menlo Park, California

Thygeson: We were just waiting for the Japanese to fold up. Valley Forge gradually slowed down. So I wrangled a transfer to Dibble, which is in Menlo Park [California], so I ended the war at Dibble General Hospital.

Hughes: That was because you wanted to end up in California?

Thygeson: Yes, I wanted to end up here, so I manipulated that.

Mrs.

Thygeson: He came back to his family.

Thygeson: It was also a plastic center; it was comparable to Valley Forge except it served the Pacific. Valley Forge was also a blind center, rehabilitation of blinded soldiers. That was a big deal. And the same thing at Dibble. That was where I met Dr. [Crowell] Beard; he was a captain on my service.

Hughes: *He was a plastic man, was he not?*

Thygeson: He developed plastic surgery during the war. He was not trained in plastic surgery before. He became a very good surgeon. He had a very interesting history in the sense that he had his eye training at the Mayo Clinic where they didn't do surgery—in the fellowship they didn't have any surgery training; it was all medical.

Hughes: *Why was surgery left out?*

Thygeson: Rochester [Minnesota where the Mayo Clinic is located] didn't have the outpatient facility and everything for it, and then the patients at the Mayo Clinic all had to be operated on by the staff, you see, so the fellows didn't get any surgery at all.

Hughes: *Wasn't it then rather unusual to assign Dr. Beard to Dibble?*

Thygeson: Yes. Beard was a natural surgeon, so he just picked it up, just the way the old surgeons, the Mayo brothers, did. They never had any training; they just picked it up. Dr. Beard was a natural. He made his own curtains; he could sew; he was just a natural. He liked surgery, and so he just fell right into it. He, of course, became the plastic surgeon of the [San Francisco Bay] area. He's by far the most important plastic man in this whole area.* He's trained a couple dozen students during his career. He was always in the army during the war, never in the air force.

Hughes: *So that changed his career, didn't it?*

Thygeson: Yes.

Decision to Remain in California

Hughes: *Were you champing at the bit to get back to external disease?*

Thygeson: Yes, in a way, but we had made a decision to go back to California.

Hughes: *When did you go back to Columbia on a visit?*

* In May, 1987, Dr. Beard received the Charlotte Baer Memorial Clinical Faculty Award, which is given annually to a member of the clinical faculty at the University of California, San Francisco, in recognition of outstanding service to the School of Medicine. He is considered by many to be singlehandedly responsible for the success of oculoplastic surgery. (*UCSF Newsbreak*, vol. 2, no. 10, May 9-22, 1987, 2.)

Thygeson: I went back at the end of the days at Drew Field, and I took some soldiers up to the New York area to go into the VA hospital. Whenever you sent a group of soldiers that was discharged for mental reasons or anything, they had to be taken up by a doctor. This wasn't a very nice chore, but it was passed around. So when my turn came I had to take them up to this VA hospital on Long Island. Then I stopped in New York at the eye institute—this was in March [1945]. Everything was very sad. The school had deteriorated; many of the staff had left, and it was running very poorly. The weather was terrible. In Florida it was orange blossom time, and March in New York is about as a bad a time there is. Then I went around to see the dean.

Hughes: *Who was still Rappleye?*

Thygeson: Yes. I had some plans for the development of the eye unit there, and Rappleye was totally uninterested.

Hughes: *What were your plans?*

Thygeson: Developing the basic science unit and the degree deal—doing it in a big way.

Hughes: *You were asking for more money?*

Thygeson: Yes, and for support. He was totally uninterested.

Hughes: *Didn't you tell me that it was originally his idea?*

Thygeson: Yes. But I had a little more grandiose ideas than his original ones. I was going to make this basic science thing not just for the New York area but for general ophthalmology in our country.

Hughes: *Why do you suppose he wasn't interested?*

Thygeson: Well, probably he had other things on his mind. [laughs]

Hughes: *Was he a research-oriented person?*

Thygeson: He had been. I probably caught him at the wrong time. Anyway, I didn't have a very happy meeting with him. I was all full of enthusiasm because I had been up to the Lancaster course.*

I had taught in the first Lancaster course, and this course had proved a remarkable success.

I liked the idea of having a basic science course that would attract people from around the country; von Sallmann would help me. So that fell flat.

Hughes: *Was it never done at Columbia Presbyterian?*

* In 1946, Walter B. Lancaster established a basic sciences course in ophthalmology for medical officers returning from the army. The course continues today during the summer. (G. Gorin. *History of Ophthalmology*. Wilmington, Delaware: Publish or Perish, Inc., 1982; 248.

Thygeson: It was done in a very partial way by Smelser, who attracted some South American ophthalmologists for a basic unit in Puerto Rico. But it wasn't done the way I wanted.

So I arrived back in Florida not very happy about the New York deal. But that wasn't the [only] reason. One of the reasons we wanted to get back to California was that Ruth Lee had bronchial asthma from ragweed hay fever, and you don't have ragweed in California. It was a combination of reasons. So we decided to take a chance and go west.

Hughes: *Did you have any specific ideas at that point?*

Thygeson: No, [but] we had the Proctor Foundation under study at that time.

Published Papers

Blepharitis, 1946

Hughes: *Before we get into that, there are a few papers from this period that I'd like to touch on. One of them is entitled, "The etiology and treatment of blepharitis: A study in military personnel," which was published in 1946.* I believe this won a prize. Would you tell me about it?*

Thygeson: Yes, it won second prize, and I've forgotten the name of the prize. But anyway, first prize was \$500, and second prize was life membership in *Military Surgeons*. [laughter] We wanted the \$500.

Mrs.

Thygeson: Boy, did we want the \$500. We needed it.

Thygeson: But anyway, blepharitis was quite a problem for some reason in the Air Force, and we had plenty of cases. Then we had a very good microbiology laboratory at Drew Field, and it was run by a young staff member from the University of Pennsylvania, Joe Gots. He was very good in providing facilities for me to work in. So whenever I had a chance, I would study the blepharitis cases.

Blepharitis had not been properly worked out by dermatologists or ophthalmologists. It was called everything. It was considered to be seborrheic. There were actually three main types of blepharitis, and the seborrheic was one type, staphylococcal was another, and moraxella (diplobacillary) was another. There were three clinically distinct types, and it was important to differentiate them because the treatment for each was different. So our study consisted in defining these three types and the treatment thereof.

Hughes: *Do you remember how you treated those three different types?*

* P Thygeson. *Military Surgeon* 1946; 98:191 and 279.

Thygeson: Well, the seborrheic type we treated with silver nitrate scrubs and mercurial ointments. For the staphylococccic type we used penicillin, which we can't do now because the staphylococci generally have become resistant to penicillin. The moraxella (diplobacillary) type was easy to treat with topical sulfonamides.

Hughes: This had all been worked out previously?

Thygeson: No, it hadn't.

Hughes: You did that as well?

Thygeson: Yes. There had been dabbling on this subject but nothing definitive until we came along and did this work.

Hughes: Was blepharitis common enough to be a real cause of concern in the service as far as the efficiency of the soldiers was concerned?

Thygeson: Yes, particularly the staphylococccic type because it produced so much eye irritation in pilots; it was a serious disability. So it was important to define and treat the staphylococccic type.

Hughes: When a pilot developed a problem such as that, was he immediately sent back to home base and treated?

Thygeson: No, he was sent to me first. We could handle it, so we never had to send a pilot any place.

Hughes: I was thinking of pilots on a European or African flight; as soon as the problem developed, would they immediately—

Thygeson: The pilot himself would seek treatment because of the eye irritation.

Mrs.

Thygeson: Oh, it's a miserable thing. If you've ever known anyone who had a severe blepharitis—I always think of G.B. (Bietti).

Thygeson: It's a disabling condition if severe.

Mrs.

Thygeson: Red eyelids—it's awful looking.

Thygeson: It's important, and even now it's not properly taught in medical schools. I noticed that Proctor fellows coming in don't know the differentiation properly.

Hughes: Everything's lumped under blepharitis with no discrimination?

Thygeson: Yes. I think it's very important to differentiate between the types, so one of my jobs in teaching Proctor fellows is to define blepharitis.

Hughes: Are the three types still treated in different fashions?

Thygeson: Yes.

Hughes: *It seems illogical to lump them all together.*

Thygeson: The dermatologists consider that lesion as seborrheic, as part of seborrheic dermatitis. The dermatologists are really at fault on this because they recognize staphylococcic impetigo but they don't recognize staphylococcic blepharitis.

Mrs.

Thygeson: How do you treat that now that you can't use penicillin?

Thygeson: Erythromycin is now the major remedy for the staphylococcic type, but it is the staphylococcic type that is very important. It's important for many reasons. One of the reasons is that if you have staphylococcic blepharitis and do a cataract extraction, you're subject to infection, so your chances of infection [after the extraction] are very high. The ophthalmologist should be able to recognize staph blepharitis and treat it.

External disease and refraction are low on the totem pole in the training of an ophthalmologist. Cataract extraction gets first priority, and at the bottom is refraction, and the next is external disease. So it's still very poorly taught.

Hughes: *Why is external disease so low on the totem pole?*

Thygeson: Well, I hate to say this, but it doesn't pay very well. Cataract extraction pays well. The treatment of blepharitis is not economically very good for a doctor.

Mrs.

Thygeson: Except that it takes forever for a stubborn case. I remember your talking to G.B. about getting busy with his blepharitis, and he never did.

Thygeson: The seborrheic type doesn't cause any irritation but is cosmetically bad, but it doesn't disable. So it's the most poorly treated of all the three types. The staphylococcic gets treatment because it's disabling. External disease is not well taught.

Hughes: *Do you know what the breakdown would be in terms of percentages of patients with infectious disease or cataract problems in a general practice of ophthalmology?*

Thygeson: In general practice, refraction takes in about seventy-five percent, and the remaining twenty-five percent is divided between surgery and medicine. A very good surgeon won't have anything to do with a minor thing like blepharitis. So it's neglected.

Mrs.

Thygeson: It may be driving the patient out of his skin.

Thygeson: Then what they often prescribe is an antibiotic-steroid combination, which in my opinion is very bad.

The Cytology of Conjunctival Exudates, 1946.

Hughes: *There's another paper of this period, which was also published in 1946. It is entitled "The cytology of conjunctival exudates."**

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Hughes: *The paper is based on cases seen at the Vanderbilt Clinic in New York and Drew Field, Florida.*

Thygeson: Yes, but we really didn't start writing about that until we got to California. This became very important in external disease, because by means of cytology one could get a very good guess as to etiology.

Hughes: *Apparently there were different types of cells.*

Thygeson: Yes, and, of course, the inclusion bodies were seen in epithelial cells, and tumor cells could be recognized at an early stage of tumors. So later on cytology became very important and became a part of all good external disease examination. Right now cytology is very important on our Proctor Foundation teaching list.

Hughes: *Were people paying attention to it in those days?*

Thygeson: No. The only cytologic examination that was used was the test for eosinophilia and allergy. There was a Lieutenant Colonel [Herbert] Herbert in the Indian army who noted that in what was called rose fever—that is, hay fever—and vernal catarrh there was a special cell which was regularly present, and that was the eosinophil. That was the first cytological observation in ophthalmology. Then later on was the observation that inclusion bodies could be found in cells. Otherwise there was no attention paid to cytology, so that's where we started working on defining the problem.

Hughes: *Were your publications making a ripple?*

Thygeson: Yes, I think so. It soon became evident that for part of the external examination you had to do cytology, too.

Eye Laboratories

Hughes: *Were most ophthalmologists set up for that?*

Thygeson: No, but the eye laboratories came into existence. Eye laboratories developed in universities around the country.

Hughes: *When was that?*

Thygeson: After the war.

* P Thygeson. *Am J Ophthalmol* 1946; 29:1499-1512.

Hughes: What would an ophthalmologist in private practice before the war have done with a case of external disease that he couldn't diagnose?

Thygeson: Refer it or treat it nonspecifically.

Hughes: Would he refer it to somebody like you at Columbia Presbyterian?

Thygeson: That's what happened in New York.

Hughes: But you weren't really set up to be a referral service in those days, were you?

Thygeson: Well, yes, in a way I was, because I got a lot of referrals.

Hughes: But didn't that just happen because you were an expert and people needed you?

Thygeson: Well, I encouraged it because that gave me material for my special studies. In New York I got to see all the major infectious problems of the eye.

Hughes: What would somebody on the West Coast do?

Thygeson: Once in a while I'd get a slide from somebody. But in New York I would see the patient, or from the East Coast I'd get referrals, from Boston or Baltimore or some place like that. So I had quite a referral practice that fed my laboratory.

Hughes: Was anybody else in the prewar period doing that kind of referral work?

Thygeson: Well, Dr. [Frederick H.] Theodore in New York developed an interest in it. He spent quite a bit of time with me in my laboratory and then had his own laboratory.

Hughes: Where was that?

Thygeson: He was at Manhattan Eye and Ear Infirmary. He's still very active.

Hughes: Is there anywhere else where people could go for consultation in those days?

Thygeson: At the moment I can't think of any place else.

Hughes: Was the development of those referral services just a matter of filling two needs—the researcher's need for material and the clinician's need for consultation—or did the ophthalmologists say, "We've got to set up these units across the country for referral purpose"? How deliberate was it?

Thygeson: It was a matter of training. For instance, starting in Iowa I had control of quite a number of the young residents, so many of them started their own little units. The same way at Columbia. We had six residents, two new ones a year. So I would provide training in external disease, and as a part of that we taught them the laboratory methods that are useful in differential diagnosis, not in the elaborate way we do now at the Proctor Foundation, but in a simple way. It filled a need. And then as consultant there really wasn't

anybody else in New York who was trained in infectious diseases of the eye, so as a result I got to see all the major infections—intraocular infections particularly.

Hughes: Again, I'll go back to the man practicing on the West Coast, and of course I'm talking about the period before the Proctor Foundation was set up [1947]. If a patient came in—

Thygeson: There wasn't anything out here at all, so a doctor just treated nonspecifically, without any specific knowledge; guessed by the appearance of the eye and everything what it was.

Hughes: Most ophthalmologists in those days probably had no training in infectious disease as part of their residency, had they?

Thygeson: Well, they were bound to see some cases, but not in any specialized way. The West Coast was a number of years behind the East Coast at that time. There were no full-time ophthalmologists here at all on the West Coast. There was no specialized eye laboratory or anything out here at all. The first full-time ophthalmologist was appointed at UC, San Francisco.

Hughes: At the time you were starting the Proctor Foundation?

Thygeson: Yes.

Hughes: Was that Dr. [Frederick C.] Cordes?

Thygeson: No, it was Dr. [Samuel J.] Kimura. Dr. Cordes was a volunteer; he never received a salary. The West Coast was five or ten years behind the East Coast in all the basic sciences at that time. The West Coast has caught up, so it's doing just about as well as the East Coast now.

Hughes: Do you remember where the first full-time positions in ophthalmology were in the East?

Thygeson: I think the first one was at Harvard, Frederick Verhoeff, and the second one, I guess, was at Johns Hopkins, and that was Alan Woods. As far as I know those were the only two full-time ophthalmologists in the country. Then, at Iowa, O'Brien was full-time. Then he set up two full-time appointments, one of which I had, and another one which Dr. Placidus Leinfelter had. So at Iowa there were three full-time appointments. That was prewar, of course. But nothing out here on the West Coast at all.

IV. THE FRANCIS I. PROCTOR FOUNDATION FOR RESEARCH IN OPHTHALMOLOGY

Formation

Hughes: Today I'd like to talk about how the Proctor Foundation got established. Perhaps we should start with Dr. Proctor's death in 1936.

Thygeson: I'd like to start before he died, with his interest in establishing a laboratory or institution for the study of Indian eye disease. He wanted to start a minor Rockefeller Institute at Santa Fe, New Mexico, just for the study of Indian ophthalmology, particularly trachoma. He went back to the Rockefeller Institute to get advice, and they told him back there that it would be too expensive and that the only hope would be to tie it in with a university. Santa Fe didn't have a university or medical school or anything there, so he gave it up. But it was his wish that there be an institution primarily devoted to infectious disease. That was the basis for my recommending to Mrs. Proctor that we set up a special memorial eye laboratory in a western university. That was while I was in the army.

The reason it came up was that at Dr. Proctor's death he left a sum of money, which at that time was about \$300,000, for the study of trachoma and other eye disease—that's the way it was labelled in his will. This money was in the Old Colony Trust in Boston, and Massachusetts law stated that if trust money was not used for so many years it reverted to the state. Dr. Proctor's nephew, Harrison Proctor, decided something had to be done about that, so he got ahold of Mrs. Proctor, and Mrs. Proctor got ahold of me. Initially, we set up a grants-in-aid committee for distribution of money, but that didn't work out well. So I advanced the idea of a special laboratory in a western university under Dr. Proctor's name.

Hughes: Were grants-in-aid actually issued?

Thygeson: Yes, but only a couple of them.

Hughes Do you know whom the money went to?

Thygeson: [pause] No, I've forgotten. It went to two universities.

Hughes: *In the West?*

Thygeson: Iowa I think was one of them. But those were the only two applications that we had. There wasn't any interest in grants-in-aid at that time because nobody was doing anything because of the war.

Mrs. Proctor telephoned me in Florida, and I took advantage of the training flights. The air corps had B-17s, and to train the crew they'd fly to Denver or to Butte, Montana for a weekend, and then come back. It was part of their training. Or they'd fly to Havana—that was a favorite place. I took advantage of one of these flights to Denver, and then from Denver I took another flight down to Santa Fe and met with Mrs. Proctor and advanced the idea of a special memorial laboratory. She was interested because Dr. Proctor had been interested in this idea. So she commissioned me to survey the western situation.

Hughes: *Had the five-person committee been set up at that stage?*

Thygeson: Yes.

Hughes: *Who was on that?*

Thygeson: Well, I was the chairman, with Dr. [Polk] Richards of Albuquerque and Dr. [Arnold] Knapp and Dr. John Dunnington of New York. I think that was the whole committee. *

Hughes: *You chose those people?*

Thygeson: Yes.

Hughes: *Any particular reason?*

Thygeson: I just knew them.

Hughes: *They didn't have any particular interest in trachoma?*

Thygeson: Well, Dr. Richards had.

Hughes: *I have that John Wheeler was on the committee as well. Is that correct?*

Thygeson: He was dead at that time.**

Hughes: *So he was never on the committee?*

* According to a history of the Proctor Foundation, Harrison Proctor and John Wheeler were also committee members. The Foundation: History. In: *Francis I. Proctor Foundation for Research in Ophthalmology. Five-Year Progress Report, 1975-1980, 2.*

** Dr. Wheeler died in 1938.

Thygeson: No, I'm pretty sure he couldn't have been.

Hughes: *Maybe Wheeler was appointed and never served.*

Thygeson: Might have been, but I don't remember that. Anyway, the committee was disbanded. It had been set up with a nominal amount of money—I think it was \$5,000 or \$10,000—and that was returned to the trust. The trust under Dr. Proctor's will was under the direction of Mrs. Proctor and the Old Colony Trust of Boston, so she really had control of it. I got a leave of absence from the air force and went out to San Francisco. I looked at the University of Southern California, Stanford, California, and Oregon. It was obvious that Stanford seemed to be the best bet at the time.

Hughes: *You were thinking of [Hans] Barkan and [Dohrmann] Pischel?*

Thygeson: Yes. I knew Dohrmann pretty well. The idea was approved by Pischel and Barkan and the dean—there was a very nice dean at Stanford.

Hughes: *Was that [Loren] "Yank" Chandler?*

Thygeson: Yes. Very popular dean. He approved it. But we had to have the approval of the president, Don Tresidder.

In the meantime, Mrs. Proctor gave twenty-five thousand dollars to Stanford preliminary to setting up that laboratory, which was established under the direction of Jerry Bettman. Jerry Bettman wrote the first paper for the Proctor Foundation.

Hughes: *Do you remember what the topic was?*

Thygeson: No, I don't remember, but I have a reprint.

I went down to see Dr. Tresidder, and he said that he was very happy to have the money, but he didn't want to favor any special department or unit because that would make the other units jealous. That didn't fit with my ideas, so we turned it down.

Hughes: *Was the laboratory in the division of ophthalmology?*

Thygeson: They started the preliminary laboratory at Stanford.

Hughes: *In San Francisco?*

Thygeson: Yes. It wasn't official, but we thought it was going to go through because the dean had approved it. It never occurred to me that Tresidder would turn it down, but he did.

So then I went to Dr. Cordes at California, who immediately got me in touch with Dean [Francis Scott] Smyth, who was wonderful, just what a dean ought to be. He was a pediatrician, and he recognized immediately the need for the unit. Then we got in touch with Mr. [John V.] Calkins, who was the regents'

attorney at California, and he immediately recognized the importance of this. We got a lawyer for Mrs. Proctor, who was the most prominent lawyer in San Francisco at that time, Edwin S. Pillsbury.

Mr. Pillsbury and I got together and drew up a tentative plan for the foundation. That's when I went around to see Karl Meyer to learn about his experience with the [George W.] Hooper Foundation. I learned a few things. I learned that a trustee system, or some group, is essential for a foundation, that you can't rely just on a director, because he comes and goes. To keep the thing going you have to have some kind of a board of trustees.

Hughes: Which the Hooper had?

Thygeson: It had, but Meyer had gotten rid of them. Meyer didn't like anybody to tell him what to do, so he managed to get rid of them. And then the university set up a faculty committee which was much worse for Dr. Meyer than the Hooper trustees. Meyer made a terrible mistake at the Hooper, and I recognized it at the time, because he got rid of the Hooper family, which was a source of money for the Hooper Foundation. It was terrible, I thought. I learned that from talking to him, and I recognized right away that he had made a mistake.

Hughes: Did he have money problems as a result?

Thygeson: Yes. He had terrible money problems with the Hooper Foundation because its corpus was tied up in lumber in northern California, and the university sold off most of the lumber land at a terrible sacrifice. It was a financial disaster. I think a good trustee group could have prevented that.

Anyway, I recognized that you had to have a good trustee unit, so we used the three-man group—the same we had for the Knapp Foundation in New York, which in New York consisted of the dean, Dr. Knapp, and me as chairman of the eye department. So we used the same technique for the Proctor Foundation board, with the dean, later the chancellor, and a representative of Mrs. Proctor, and the chairman of the eye department. That's what we still have.

Hughes: That's worked out very well?

Thygeson: I think it's worked out reasonably well. It hasn't worked out as well as I thought, because I thought the board could help raise us some money, and in all this time it has never raised a nickel. But it was quite a safety thing, because otherwise the Proctor Foundation would have been taken over by the eye department.

Hughes: Was it under your advice that Mrs. Proctor advised separation of the Proctor Foundation from the university and the eye department?

Thygeson: Yes. I thought that for the survival of it as a memorial laboratory you'd have to keep it separate from the eye department, otherwise, it would just be absorbed in the general purposes of the eye department. That has happened elsewhere. It happened with the [Lucien] Howe Laboratory [of Ophthalmology] in Harvard.

Hughes And it happened to a degree to the Knapp Foundation as well.

Thygeson: The Knapp Foundation was totally absorbed. I knew you had to have a unit [board of directors] aside from the director [of the foundation]. That I learned the hard way through the Knapp Foundation.

Hughes: There were some other conditions which Mrs. Proctor placed on the institution, which I suppose came from you.

Thygeson: They came from me. That had to do with space. There was a very sad story at Harvard, where the Howe Laboratory had space which was not owned by the Howe Lab, but loaned by the Massachusetts Eye and Ear Infirmary. They never had any chance for expansion or anything, and the space situation was terrible. I insisted we at least had some space that could rightfully be called Proctor space.

Hughes: You apparently insisted on at least three thousand square feet.

Thygeson: Yes, because three thousand was the amount of laboratory space we had at Columbia, which was a small workable unit. Of course, we didn't have any NIH money or anything, so expansion wasn't possible. I thought we could work on three thousand as a base, so we got that.

Hughes: Were you planning at this early stage to operate on just the Proctor money?

Thygeson: Well, there wasn't any NIH at that point. Of course, there was always a chance for gifts and so on, but the base thing was Proctor money.

Hughes: The university agreed to both of these qualifications?

Thygeson: Yes. The way it was handled, Dr. Cordes got one of his patients to donate the money for three thousand square feet in the new Medical Sciences Building. That was a gift from the [Berthold] Guggenhome family (who were patients of Dr. Cordes) assigned to the Proctor Foundation. That's the central base of the Proctor Foundation, in the Medical Sciences Building.

Expansion

Thygeson: Then we expanded, so we now have about seventeen thousand square feet of laboratory space. But that was only possible because of NIH money.

Hughes: In 1953 Mrs. Proctor gave another substantial sum.

Thygeson: Yes, twice. The Proctor Foundation office building at 95 Kirkham was a gift from her of \$60,000, matched by the regents. And then the extra space in the Medical Sciences Building was a gift from her matched by the NIH. Then the

extra space that we built at 95 Kirkham [1965] she contributed to, but not in a very large amount [\$25,000].* We had to raise \$260,000 to build an addition to 95 Kirkham. We got it from every possible source that we could.

Hughes: Who was actually raising the money?

Thygeson: Dr. [G. Richard] O'Connor and I. He was associate director, and he and I raised the money. I did most of the money raising. I went to Harry Hind, and I went to old patients of mine. Harry Hind gave the [Harry Hind Research] library [in the Proctor Foundation office building].

Hughes: Do you remember when that was?

Thygeson: No, I don't remember, but it was at the time when we were adding the addition, while I was still director, so it must have been in the sixties.

Hughes: Talking about donations, I came across the name, Forrest Davidson.

Thygeson: Forrest Davidson had the optical house, Jenkel-Davidson, and he gave, I think, ten thousand dollars** to develop the basement into an electron microscope unit.

Hughes: Electron microscopy became a very prominent area of Proctor Foundation research.

Thygeson: A very important part, yes.

Then an old patient of mine, who was former president of FMC Corporation, Mr. John Crummy, gave ten thousand dollars. May I tell an interesting story about Mr. Crummy?

Hughes: Sure.

Thygeson: I was trying to raise money, so I called up Mr. Crummy and told him I wanted to talk to him about money. He said he was taking twenty of his relatives to Europe that summer—he was ninety years old at that time—and he said, "When I come back, I'll come up and see you."

I put in the call, and he called me back while I was seeing an old patient who looked like a poverty-stricken old lady, whom I carried without any charge. She heard this turn-down, and she said, "He shouldn't have done that; he should have written you out a check right then. I'll have my secretary write you a check in the morning." The next day a check for a thousand dollars came from Mrs. Klopstick.

##

Thygeson: The name of the board of trustees was changed to board of governors, and is now known as that.

* The Foundation: History. In: *Francis I. Proctor Foundation for Research in Ophthalmology. Five-Year Progress Report, 1975-1980*, 5.

** \$5,000 (*Ibid.*, 4).

Hughes: Is there any significance to the name change?

Thygeson: Yes, they couldn't use the word trustee because it interferes with the university trustees, so they changed it to board of governors.

Hughes: Did any of their responsibilities change at the same time?

Thygeson: No, just the change of name.



Phillips Thygeson and Michael Hogan, early 1950s,
2nd and 1st Directors of the Proctor Foundation.

Michael J. Hogan

Hughes: Dr. Hogan was the first director of the Proctor Foundation [1947-1959]. Who was responsible for appointing him?

Thygeson: It was at that time the committee [the board of trustees]—same as the board of governors.

Hughes: Did you consider anybody else?

Thygeson: No, because Dr. Hogan was Dr. Cordes's right-hand man. Dr. Hogan started a little pathology laboratory right after the war, and Dr. Cordes was very anxious to have Dr. Hogan become the first director. Dr. Hogan was not the proper choice because he was not really interested in external disease. He had done a very interesting study on adenovirus conjunctivitis, but he really wasn't interested in external disease. So as a director he would not have

carried through Mrs. Proctor's wishes in her husband's interests [regarding research on infectious diseases of the eye]. But I was able to keep it going. Dr. Hogan did very valuable work in electron microscopy and uveitis—not on the subject of Mrs. Proctor's interests, but he kept the foundation going very well.

Hughes: What sort of a man was he?

Thygeson: He was an Irishman who was very down on the English. [laughs] He was bitter about our going into the war on the side of the English. I remember that caused quite a lot of unhappiness. He went in the navy against his wishes and wasn't very well treated in the navy.

Hughes: Was he an Irishman born and bred?

Thygeson: No, but in all other respects he was. He looked like an Irishman, and he talked like an Irishman. He was actually born in Montana of parents who were from Ireland.

Relationship with the Department of Ophthalmology

Hughes: Did it work out that the Proctor Foundation and the eye department got off to a good collaborative start?

Thygeson: Well, the collaboration had its ups and downs. For example, Dr. Cordes did not approve of having a separate unit. He wanted the Proctor Foundation to be a part of the eye department. He went along with my ideas because he really couldn't do anything else, but he didn't approve of it at all. He made one major effort to combine it with the department.

Cordes did it down at the Bohemian Club with Dr. [John B. de C.M.] Saunders. They cooked up a deal where by administrative edict the Proctor would become a part of the department. Of course, that's contrary to the founding papers.*

I heard about it by way of Dr. Cordes's secretary, so I immediately got Mrs. Proctor to write a letter to the president of the university, Clark Kerr, and he, of course, looked up the papers, and he really gave Dr. Saunders a bad time. That was the end of that. Dr. Saunders wouldn't speak to me for about a year, and then he became one of my best friends.

The president gave the dean a bad time over this, because the president thought that the wishes of the donor should be followed to the letter. He thought that the idea of going against Mrs. Proctor's wishes would have a bad influence on the donor system in the university. So he let the dean have it. Saunders became our best ally later on.

Hughes: Was there a lot of collaborative research between the Proctor Foundation and the eye department?

* This and the following paragraph were moved for the sake of better continuity from the transcript of interview four.

Thygeson: Yes, it worked out very well, because Proctor gave space to the eye pathology unit of the department.

Hughes: *Did eye pathology arise with the Proctor Foundation?*

Thygeson: It had a very small beginning before the Proctor Foundation, and then its space in the old library building was extinguished when they tore down the old library. The eye department was without any space [for the pathology lab], so we gave the space in Proctor.

Hughes: *I understand that pathology was a particular interest of Dr. Cordes.*

Thygeson: Yes, that's right. Pathology was very well treated in the early days. It's not done so well now; it's a rather minor part of the department. It was, at the time of Dr. Hogan, number one in the department.

Hughes: *Is that just because of changing interests of the chairmen?*

Thygeson: Yes, the new chairman was not interested in pathology, and then our best pathologist at that time—other than Dr. Hogan—went over to Presbyterian [Pacific Presbyterian Medical Center, San Francisco]. That was [William] Spencer, who is head of the eye pathology lab over there. He expected to be the chairman of the eye department at UCSF, but the search committee chose another man, Steven Kramer. Kramer became chairman, and the reason he was chosen was the fact that he was M.D./Ph.D. The chairman of the search committee had a Ph.D., so, of course, he picked Kramer. Kramer's worked out all right. It looked, when he started, that the choice wasn't going to be good. Many on the staff didn't like him too well. He developed a very good department and turned out to be a real money-raiser.

Hughes: *Is he a supporter of the Proctor Foundation?*

Thygeson: Philosophically, yes, but I can't say that he's been either a help or a hindrance to the Proctor Foundation.

Hughes: *Would you say that the relationship between the eye department and the Proctor Foundation has been relatively benign over the years?*

Thygeson: Yes, it's been good. The Proctor Foundation has contributed much more to the eye department than the eye department has contributed to Proctor. Let's put it that way. It's a one-way street.

Hughes: *Why weren't you considered for the Proctor Foundation directorship in the very beginning?*

Thygeson: I was, because Mrs. Proctor demanded that I be, but I thought it best for me not to be the director.

Hughes: *Why did you think that?*

Thygeson: I thought I'd do better from the outside as a staff member, so I turned it down.

Hughes: Were you also perhaps thinking back to the New York days when administration took over your research?

*Thygeson: That's probably one reason. Anyway, I didn't want to get caught in the university administration. But I later became director [1959-1970], as you know, so I changed my mind.**

Mrs. Proctor wanted and expected me to be the first director, but I'd been so fed up with administration in New York that I preferred not to, and then Dr. Cordes was very high on Dr. Hogan, whom he had trained. So it seemed desirable to have Dr. Hogan be administrator, and I had a laboratory I could work in.

Hughes: Even though Dr. Hogan was not an external disease man?

Thygeson: At the time I really thought he was, because he did one of the first pieces of work on epidemic keratoconjunctivitis. He was the first one to recognize it in the United States, and he gave it the name of epidemic keratoconjunctivitis in 1941. It looked as though he was going to be important in infectious disease. He had worked in New York for about six months, half with Dr. Reese in pathology and half with me in external disease. So I thought he believed in the germ theory, but it didn't take. [laughter]

I had to keep the thing going in infectious disease. It worked out all right though because he made quite a good contribution on uveitis and pathology and so on. He was not the ideal director in Mrs. Proctor's sense.

Hughes: How about in the other sense?

Thygeson: He was a very fine ophthalmologist.

Hughes: Was he skilled in fund raising?

Thygeson: No, he wasn't much good in fund raising. [laughs]

Hughes: Did that then fall on your shoulders?

Thygeson: Well, in a way. I have one story I thought was funny-sad about Dr. Hogan and fund raising. I was at the opening of the Jules Stein Eye Institute, and I was sitting on the dais next to Ken Swan of Oregon, and Ken Swan was smirking at me. I knew there was something wrong. He told me that he had pulled a fast one on Uncle Mike [Hogan]. What had happened was that a glaucoma patient of Dr. Hogan's from Berkeley had gotten tired of waiting every visit in Dr. Hogan's office for an hour and she'd moved up to Portland where her daughter lived. The daughter's ophthalmologist was Ken Swan and Ken Swan became her ophthalmologist. She gave Ken Swan half a million dollars—that Dr. Hogan could have had. She just got tired of waiting in the office. He always kept patients waiting a half an hour or an hour or more.

* Dr. Thygeson expanded on this topic in interview four. The transcription was moved and follows here for reasons of continuity.

Hughes: *Why?*

Thygeson: Well, he was always late in getting to the office, and usually started about an hour late. So the patients didn't like that very well. He just wasn't much good at fund raising.

Pressures to Change the Original Mission*

Hughes: *Could you summarize the original purpose of the Proctor Foundation, and is it still the same today?*

Thygeson: It's still the same. We kept it that way with some difficulty, because there was a lot of pressure to change the purpose. The purpose was a memorial unit to follow the interests of Dr. Proctor, who was primarily interested in infectious eye disease.

Hughes: *What have been the pressures to change the original purpose?*

Thygeson: There was pressure from one of the deans, John Saunders, who was on the board of governors, to change it to optics. He thought infectious disease had been taken care of by antibiotics and therefore had no future and that we should change to optics, which had a future. Then, as I mentioned, there was pressure to make it the eye laboratory of the ophthalmology department, which would have completely changed the Proctor purpose. Another individual, with the same idea that we'd exhausted research on infectious disease, suggested that we change to physiology. So I had to defeat that, of course.

Hughes: *Why did they pick optics and physiology?*

Thygeson: Well, one of them was a physiologist, and the other one was particularly interested in optics, I guess. Of course, they didn't know anything about Dr. and Mrs. Proctor or about the mission. All they did was to come to a meeting of the Proctor's board of governors once a year, so they really didn't know what the Proctor was all about. I had to take the burden on that.

Hughes: *In each case did you go back to the original mandate from the Proctors as your defense?*

Thygeson: Yes. I had to do it; there wasn't anybody else to defend it. Now, I think, we're pretty well established. After forty years it would be very hard to change the purpose.

Hughes *A second objective, I understand from reading some of the literature, was to contribute to teaching programs of the department of ophthalmology and of the medical center in general.*

* This section incorporates material from a later interview.

Thygeson: Yes, and another sub-purpose was preventive ophthalmology, to prevent eye disease, infectious or otherwise. But infectious disease was the basis for it; everything else was subsidiary.

Hughes: *The fellows program was a later development?*

Thygeson: Yes, that was kind of intertwined there, the teaching program and the research mixed. Fellows do both; they are taught and they do research.

Hughes: *Is there anything further that you want to say now about the early stages?*

Collaboration with the U.S. Indian Health Service

Thygeson: The early stages of the Proctor Foundation were still oriented to the American Indian. It was the trachoma of the American Indian that was all important to me in the early times of the Proctor Foundation. We collaborated with the [U.S.] Indian [Health] Service, Division of Indian Health, which was separate from the Department of the Interior; it was under the Public Health Service. Medicine and the Indians is now under Public Health, not Department of the Interior.

Hughes: *Did that make a practical difference?*

Thygeson: Sure, because we collaborated with the Public Health Service, and even set up a laboratory down in Phoenix.

Hughes: *So when it was under the Department of the Interior you didn't have the manpower already in place to collaborate with?*

Thygeson: No, we didn't, but they were very good to us, and we had a laboratory set up in Fort Apache, and they financed it.

Hughes: *But they didn't have the personnel to immediately feed into it, did they?*

Thygeson: No.

Hughes: *What was the major advantage of working with the Public Health Service?*

Thygeson: With the Public Health Service we were able to get much better local physicians. We got ophthalmologists rather than general practitioners. So it made quite a difference. The Public Health Service was much better than the medicine under the Department of the Interior.

The Department of the Interior runs the medicine in American Samoa, and they've got a big unit out there, the LBJ [Lyndon Baines Johnson] Medical Center. But it's staffed in a very haphazard way by general practitioners and New Zealanders, and in no way is comparable to how it would have been staffed by the Public Health Service. The Department of the Interior is not set up to handle medicine, so it was a wonderful hospital unit at Fort Apache,

but a poor staff. The Indian Service was set up originally under the Department of the Interior with good nurses but a poor physician staff. [tape interruption]

The American Board of Ophthalmology*

Hughes: I understand that you served as a member of the board of ophthalmology from 1946 until 1952. Could you tell me how members are chosen?

Thygeson: The American board was established by Walter Lancaster and Edward Jackson. They originated it, and it was a good idea to improve the caliber of the teaching of ophthalmologists and to certify those who had good training. It was set up so that each of the major societies would nominate candidates for the board. I think I was nominated by the American Ophthalmological Society as one of their two candidates. I was made a junior member of the board—although there really wasn't any junior or senior member. It had a big prestige factor at that time. If you were a member of the board, you really amounted to something.

We had about three meetings a year, at which we gave the written examination and then the oral examination at separate times of the year. Candidates did the written first, and if they passed the written, then they were eligible for the oral. Originally, when I took the boards [1930], it was all in one day. We had the oral in the morning and the written in the afternoon. When I did it, it cost me twenty-five dollars, and in the evening I was told [by the chairman of the American board] at a cocktail party that I had passed.

After that it became very complex, and so it would take the most part of a week to do this examination. It became much more complex than was needed, so it became a chore on the part of the examiners to spend that time away, and I don't think it was worth it. It should have been more than one day, but it shouldn't have been most of a week.

Hughes: Do you remember who was on your examining board?

Thygeson: Yes, I remember going up for ophthalmoscopy, and they gave me one patient to examine. I examined her and gave her the correct diagnosis, and then the examiner said, "Who trained you in ophthalmoscopy?" and I said, "Finnoff." He said, "Goodbye," and that's all there was to it. Then I went up for refraction, and they gave me the cross cylinder to define, which I did correctly. And they said, "Who trained you in refraction?" I said, "Jackson," and they said, "Goodbye."

I remember that the hard one was Dr. [L.C.] Peter of Philadelphia, who was in perimetry, visual fields. He gave me a couple of hard cases to do, and then he gave me a real quiz and kept me for over half an hour. I thought I was going to be in trouble, but he let me through. I wasn't as well prepared in that. I remember pathology was easy because I had done pathology with Dr. Finnoff.

* This section includes information from an earlier discussion.

Hughes: Who was the examiner there?

Thygeson: The man who ran the Lancaster course for so long, Parker Heath. He was the second chief for the Lancaster course, a Detroit ophthalmologist who specialized in pathology and anatomy. I remember taking the anatomy course questions, and we had an argument over the attachment of the vitreous. It turned out I was right, and he was wrong. [laughter] Then he invited me to play tennis with him in the afternoon.

Hughes: In how many fields were you examined?

Thygeson: There were about ten, I think.

Hughes: Which represented each of the subspecialties?

Thygeson: Yes.

Hughes: Now there must be thirteen.

Mrs.

Thygeson: The interesting thing, I think, is that he didn't prepare for this examination at all. He went to Detroit for another reason, to give his first paper.

Thygeson: I didn't prepare. I went by train from Denver, and I took Fuchs' textbook on ophthalmology, which was the prime authority, so I read it through twice on the train.

Mrs.

Thygeson: I didn't think you even planned to take the exam.

Thygeson: Dr. Finnoff [who was secretary of the American board at the time] told me sometime before that I was to take it, and I had paid my twenty-five dollars.

Mrs.

Thygeson: Anyway, Phil never mentioned it to me. When he came back I said, "What went on in Detroit? Tell me all, a play-by-play account, please." He said, "Well, and then I took the boards." I said, "What are they?" I had no idea what the boards were.

Hughes: Nowadays people spend months, if not years, preparing.

Thygeson: The sad thing was that I miscalculated the time. I thought Detroit was on central time, so I arrived for my examination an hour late. They accepted me anyway. I was surprised to find everybody going through the motions, but I didn't catch onto that until I arrived an hour late for the writtens. But I was about the first one out; I went through the writtens awful fast. [laughing] That was when I realized I was on Eastern Standard Time.

Hughes: How necessary were the boards in those days?

Thygeson: Not as necessary as they are now. You could have a hospital appointment without the boards. Now you can't have a hospital appointment without the boards. A young man coming out who wants an appointment in some unit or doctors' group or something like that really has to have the boards to even be eligible for consideration. So the boards are very important. What they're doing right now is having a reexamination every so often, beginning next year.

Hughes: *Obligatory?*

Thygeson: Yes. Which is something new. For a number of years they tried to separate medical and surgical ophthalmology in two separate boards. They even tried to use the American College of Surgeons to certify the surgical side, but that fell through. So there's still only one board. But reexamination will become necessary beginning next year. I've forgotten how many years you can go before you have to have reexamination.

Hughes: *In the early days of the boards, what provision was made for practitioners who had been in the field for some time? Did they still have to go through the whole procedure?*

Thygeson: Somebody who had been on the university faculty, teaching for some years, did not have to take the boards. It was called the grandfather clause. Same way, for instance, if I were going into practice right now, I wouldn't have to take the reexamination because I would come under a grandfather clause as somebody who had been teaching.

Hughes: *When you were on the board, were you responsible for setting the actual exam?*

Thygeson: Yes, because each member usually contributed for one specialty. They wouldn't let you examine [orally] in your specialty, because they figured you'd be too tough on the candidate, but for the written you could. For instance, I used to write the microbiology and sometimes pathology questions for the written. But when it came to the orals, I never examined in external disease; I always examined in pathology or refraction or ophthalmoscopy or something.

Hughes: *Was that your choice or were you told?*

Thygeson: Told. This was set up by the chairman. The chairman actually appointed the [sub]specialties and who was going to examine. In addition to the board examiners, they also would have certain invitees, who were either previous members of the board or who were certified. At one time it was quite an honor to be asked to examine in the American board.

Hughes: *A candidate might face more than one examiner at a time?*

Thygeson: Yes, there might be two people on each [sub]specialty, for example. I examined with Fred Verhoeff at his last examination at the board. I'll tell you what happened there.

Ordinarily, they wouldn't let Verhoeff examine in pathology, or they tried to prevent him, because he was so tough. But he wouldn't examine in anything else, so they let him do it.

Anyway, he and I were on this board up in San Francisco. The university was underequipped, and the microscopes provided were the old-fashioned monocular microscopes. We had this one candidate from southern California who had driven up all night and was in a terrible emotional state. Verhoeff apparently put in one of his prize slides—which you should never do. The candidate took the oil immersion and ran the oil immersion lens right through the slide. Verhoeff attacked him. [laughter] We pulled him off the candidate, and he never examined again.

Mrs.

Thygeson: Mrs. Lea Stelzer; she was the secretary of the board. I used to help her at the time of the exams when I finally began to go with Phil, when my kids were big enough. These candidates would come along, and I'd check them out and get their names and everything about them. Tell them how long they were probably going to have to wait, and that sort of thing.

They'd say, "I've got Thygeson coming up. Is he terrible?" I'd say, "I hear he's terrible. [laughter] Oh, he couldn't be worse. He's just really terrible." They were all so scared they didn't know what they were saying.

Thygeson: I got into trouble several times by what I asked. One of them was a Canadian girl. I asked about onchocerciasis, which is a filarial disease found in Africa and Central America, the cause of river blindness. I thought this was a legitimate question; it was a world blind problem, but she took this as a terrible insult to ask a question that had no relation to Canada—they don't have onchocerciasis in Canada. I thought she ought to know something about world blindness. I never heard the last of that.

The other one I got in trouble for was for a candidate from the University of Pennsylvania. I had him check corneal sensation in herpes, because the cornea loses sensation in the eye. So he took a wisp of cotton and put it in the dendrite, the lesion of the herpes—

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Thygeson: —infected eye and then put it in the non-infected eye. I decided that he shouldn't pass. He was from a very prestigious university, and I heard an awful lot about that.

Hughes: *But you were right.*

Thygeson: Yes, of course. Then the funny part of that was when I came back here I saw Dr. Hogan do exactly the same thing.

Mrs.

Thygeson: Well, he didn't believe in the germ theory. [laughter] None of this new-fangled stuff.

Hughes: *Why didn't he practice antisepsis?*

Thygeson: He just paid no attention to—

Mrs.

Thygeson: —trivial things like that.

Thygeson: —like infection, sterility, or anything.

Hughes: *Did he get into trouble?*

Thygeson: Oh, yes, he got into trouble. We had more postcataract infections up there [UCSF] than we began to have in San Jose.

Mrs.

Thygeson: Danny Vaughan, who in his long years of private practice has done many, many, many cataracts, has never had an infection of any kind, or even a scare.

Hughes: *How could Hogan keep using poor sterile technique?*

Thygeson: He was so good in other things, so I guess they forgave him for that.

Mrs.

Thygeson: He had a reputation.

Thygeson: He was totally unstressed by anything like that. If an infection occurred, why, it was an act of God; it wasn't his fault.

When we practiced in San Jose, we had a good infections committee, and we monitored everything about the surgery, and we just didn't have the infections they had up at the university.

The modern thing now is infection control in every hospital, and every hospital has an infections committee, and they monitor everything about the operating room and sterility practices and all that. But they didn't used to.

Hughes: *When did that control come in?*

Thygeson: It came after the war. We had it in San Jose for a long time. I asked Dr. Hogan to set up a committee at the university, but he passed it off, and the committee was never established. I didn't like that at all, but I couldn't do anything about it. I'm teaching now good infection prevention to the Proctor fellows.

Yesterday, on Channel 7, they had Ralph Nader on the harm that can come from contaminated cosmetics. There was a young woman who lost an eye, and her story was that she had Maybelline in a container, and she actually only scratched one eye when she was putting this on her eyelashes, and she got an opportunistic infection and lost the eye. She sued the Maybelline Company and got three and a half million for one eye.

Mrs.

Thygeson: It would almost be worth it. [laughter]

Thygeson: The loss of an eye in industry was twenty-five thousand dollars.

We knew of one man over at Pacific [Presbyterian Medical Center] who demanded that his herpetic eye be removed because of pain, and he got twenty-five thousand dollars for the unnecessary removal of the eye. Times have changed. It's millions now; it was thousands then.

I don't know how she scratched her eye, but I don't see the justification for suing the company for that. But the jury awarded it. Juries are now giving these tremendous awards, and lawyers get up to half of that.

Mrs.

Thygeson: And then they fret about the prices that doctors charge when they have to have this enormous protection against malpractice, which they didn't do then.

Thygeson: The administrator of Presbyterian Hospital in New York took an insurance policy out on me, and it cost eighty dollars. I didn't think that was justified, but that was New York.

Original Mission

[Interview 4: December 19, 1986] ##

Hughes: *Could you tell me what the original principal purpose of the Proctor Foundation was and perhaps still is?*

Thygeson: The purpose of the foundation, and the one that I presented to Mrs. Proctor when we were planning, was to form an institution or a laboratory for the study of infectious disease of the eye, particularly trachoma. At that time the etiology of trachoma was still in limbo. The mission, according to Dr. Proctor's will, was the study of trachoma and other infectious eye diseases. The mission was later enlarged to include related conditions, such as immunology as applied to infection. We went into a little bit of anatomy and a little bit of pharmacology, but the fundamental mission was infectious diseases of the eye.

Hughes: *You mentioned prevention last time.*

Thygeson: Yes, and preventive ophthalmology became a very important part of the mission. That was not in the original program that I presented to Mrs. Proctor, but it soon developed that preventive ophthalmology was very important and especially with regard to trachoma. We wanted to prevent children from getting trachoma.

Hughes: *When you modified or in this case enlarged the mission, did you discuss it with Mrs. Proctor?*

Thygeson: Oh, yes.

Hughes: *So she always knew what was going on.*

Thygeson: Yes, I was in touch with Mrs. Proctor every week by telephone and letter and pictures. So she was as familiar with everything about the laboratory as I could explain it on the telephone or write it or illustrate it by pictures.

Hughes: *Did she take an active interest and make suggestions?*

Thygeson: She never came to California again. She only made that one visit. She was really incapacitated by arthritis; she just couldn't travel.

Hughes: *I was wondering why you decided that the original members of the staff should be M.D.s rather than Ph.D.s, or was that a decision?*

Thygeson: There was no decision; it just worked out that way. Of course, we soon had Ph.D.s. Bill McEwen was a Ph.D. We had quite a number of Ph.D.s over the years. But the only thing that was settled was that the director should be an ophthalmologist who had an interest in infectious eye disease.

Hughes: *But you yourself weren't insistent that all the staff be ophthalmologists?*

Thygeson: Oh, no. No, in fact I felt the other way around. The value of an institute is that you have diversity in the staff and that you break down barriers between disciplines and so on. The trouble with a department is that you really get ingrown; the value of an institute is that you cross the departmental barriers. So we had consultants in dermatology, microbiology, physiology, and several others. Right from the first we wanted to take advantage of the university for consultants. The value of being tied into a university is that you have all these people that can be called upon for help. We did, you see, especially in microbiology and dermatology. We did in anatomy, too, and electron microscopy.

So there was never any idea of all ophthalmology. That's departmental more than Proctor.

Cooperative Research Projects

Hughes: *The association was really deeper than just consultants, was it not? I'm thinking of the joint research projects that you eventually got into.*

Thygeson: Yes, I had joint research projects in dermatology and microbiology.

Hughes: *Did those start at the beginning?*

The George Williams Hooper Foundation, UCSF

Thygeson: Well, very soon. I started working with one of Dr. [Karl F.] Meyer's associates very early, then he left, and I switched over to Dr. [Ernest] Jawetz. So that's how I got associated with Dr. Jawetz.

Hughes: *Jawetz was not Hooper; he was department of microbiology.*

Thygeson: He had been partially trained at Hooper.

Hughes: That's how you knew him?

Thygeson: That was one reason. He and Dr. Meyer were not the best of friends [laughs], but it worked out very well, and we had wonderful cooperation from Dr. Meyer's laboratory in chlamydial work. We had Miss [Bernice U.] Eddie, who was Meyer's first assistant, and she was just a wonderful source of information.

Hughes: She was a microbiologist?

Thygeson: Yes.

Hughes: Was she particularly interested in chlamydia?

Thygeson: That was her main interest, chlamydia.

Hughes: What was her scientific education?

Thygeson: I really don't know. I know she was not an M.D., so she was probably a Ph.D., but I actually don't know that.* She was a very shy person, very retiring. She was very, very helpful and friendly and all, but I didn't get to know her background very well. She coauthored many of K.F. Meyer's papers.

Hughes: You were consulting her.

Thygeson: All the time.

Hughes: But you weren't actually doing research together?

Thygeson: No, she told me how to do the things, because she knew how to cultivate the psittacosis agent, which I didn't know how to do. She taught me how to do that.

Hughes: Was Dr. Meyer ever in the lab at that stage, or was he strictly administration?

Thygeson: I never saw him in the laboratory, but he was very knowledgeable about the laboratory, so he must have been in the laboratory. It just happened that I never saw him there. I saw him in his office.

The Department of Microbiology, UCSF

Hughes: What was the nature of the work that you and Dr. Jawetz pursued?

Thygeson: He worked on any infectious eye disease we could get ahold of, particularly the chlamydia and herpes. But we also worked on epidemic keratoconjunctivitis, which is an adenovirus infection. We were on the lookout for any unknown disease entity, which we then tried to find the cause of.

* "Miss" Eddie was a Ph.D.

Hughes: Had he been interested in eye infections before he met you?

Thygeson: No.

Hughes: You were the one that led him in that direction?

Thygeson: Yes. I stimulated him, and then we got a hold of a very wonderful technician that we stole from the pathology department, Lavelle Hanna. She was a wonderful jewel. She was not a Ph.D.; she was an M.S., I think, but she was of Ph.D. caliber.* She just jumped right into the program in a big way. She was mainly responsible for the isolations of the chlamydia and the adeno viruses. Without her we couldn't have handled it.

Hughes: She came to you with that background? She knew how to culture chlamydia, for example?

Thygeson: No, she didn't have any experience, but she soon learned because we had Miss Eddie there to help us out.

Hughes: Did she remain with you for many years?

Thygeson: She remained with the department of microbiology. She was only a consultant to the Proctor. She was paid by the department of microbiology through NIH grants. She was so good that after awhile they put her on the fixed, on the hard money deal. She ended up her career in microbiology. She was really good.

Hughes: How did the collaboration work with the department of microbiology? What did they do, and what did the Proctor Foundation do?

Thygeson: We got the case material, and we did all the clinical work on the experiments. We did a lot of human inoculations—those days you could—and then we did a lot of the cytology, but the actual cultivations were all done in the department of microbiology by Miss Hanna.

Hughes: What was Dr. Jawetz contributing?

Thygeson: He was the real supervisor in the good sense. He wasn't just an observer; he really got in and did some work, too. But it was really Miss Hanna who was the one who did the work. Dr. Jawetz provided brains, especially on the write-ups. He was a very good writer, and he was a very good critic, too. We had a weekly meeting at which we went over everything, and he was very good at planning the program. We had some planning problems because we had to go out to the Vacaville prison** to do experiments.

Hughes: Is that where you were doing the inoculations?

* Miss Hanna held an M.A. degree.

** State of California Department of Corrections Medical Facility at Vacaville.

Thygeson: Yes. And we had to go down to the Indian country to get material and do therapeutic studies. So we had a rather complex program of investigation, etiology, and therapy, because we had a big therapy program on trachoma. It lasted a number of years.

Research with Human Volunteers

Hughes: *How did you go about setting up a program that required human inoculation? With the prison volunteers, did you approach the director of the prison?*

Thygeson: The Vacaville prison had a committee, based in Sacramento, that reviewed the project. Then if the project was accepted—

Hughes: *Was the committee reviewing it from the standpoint of scientific merit?*

Thygeson: Scientific merit and safety. Then it was approved also by the director of the hospital. He was a very knowledgeable psychiatrist. He approved it, and then we would call for volunteers. There were a lot of volunteers because this was a project the prisoners liked. I usually did the explanation of what the project was.

Hughes: *Directly to the prisoners?*

Thygeson: To the prisoners. We got them in a little room, and we'd have maybe twenty or thirty. We'd explain what the hazards might be. Then we'd look over the ones who volunteered; to see that the eyes were all right for the study. They were paid a certain amount—I've forgotten how much it was; it wasn't very much—for their work. We went out once or twice a week to examine the cases and so on.

Hughes: *Did you usually go?*

Thygeson: I always went.

Hughes: *Who else went along?*

Thygeson: Dr. Jawetz almost always went, and then Dr. [Chandler R.] Dawson very frequently went, and some of the other Proctor fellows would go with us. Dr. Dawson mostly, I guess, of the younger men.

Hughes: *This is trachoma that we're talking about now?*

Thygeson: It was chlamydia, and we were using inclusion conjunctivitis, not trachoma. We thought trachoma was too dangerous.

Hughes: *What were you trying to work out?*

Thygeson: We were trying to work out everything with regard to the experimental infection, the incubation period, how the organisms behaved in the incubation period, how long the natural history lasted, what the clinical signs

were, how long we could recultivate the chlamydia. Get as much information about the experimental disease as we could, and we did get a lot of information. On repeated inoculations there was an immunity that developed. The case would get well, and we'd inoculate it again, and there'd be a much milder disease the second time, and maybe the third time we wouldn't be able to get any infection at all.

Hughes: Why do you think the prisoners liked to participate in the project?

Thygeson: Vacaville had prisoners who were mentally a little different from the average prisoner. They were often just kind of social misfits, and they liked being able to do something that would be good for the world. It had a therapeutic effect; gave them something to think about. They liked it, and the director of the hospital liked it, but then it was all thrown out by the writer Jessica Mitford. She was able to squash the whole program.

Hughes: What did she do?

Thygeson: She campaigned against using prisoners for experimental work. She was successful in making it difficult for the prison authorities, so they cancelled it all.

Hughes: Do you remember when that was?

Thygeson: It was after we had finished our studies, so our studies went through without trouble.

Hughes: Was it the later fifties?

Thygeson: I would say early sixties probably.

Hughes: When consciousness was being raised as far as medical ethics was concerned.

Thygeson: Yes, the papers were full of how they were mistreating the prisoners. She was very successful in drumming up sentiment for the poor prisoners. Actually volunteering for experiments did the prisoners good. It was all a mistake to stop the volunteering.

Hughes: Across the country?

Thygeson: Yes, all over the country.

Hughes: The Public Health Service guidelines for the use of human volunteers in research were established in 1966.

Thygeson: It became much more difficult to get NIH grants using human volunteers.

Hughes: How did you get around that?

Thygeson: Well, they had a committee at UCSF,* and we just presented the material to this committee set up for animal and human experimentation.

* Committee on Research of the Academic Senate.

It wasn't any problem at that time. It became a problem later, but we had finished our work at that time.

Hughes: The committee at UCSF was set up because of the PHS guidelines?

Thygeson: I imagine that was so.* I didn't go to the committee meetings; Dr. Jawetz handled all of that. So I didn't have anything to do with that.

Hughes: You never had any problem getting your proposal approved?

Thygeson: No problem, and no problem at the prison. Everything worked fine; we had no complaints from the prisoners. They were all glad to cooperate.

Hughes: Did the tightening up on the use of human volunteers mean that you had to rely more on animal experimentation?

Thygeson: Oh, yes.

Hughes: If you couldn't get prisoners, you probably had difficulty getting any sort of volunteer.

Thygeson: Yes, that's true. I remember when adenovirus 8 was isolated from a case of epidemic keratoconjunctivitis, we wanted to test it experimentally. This virus does not affect animals; only humans are susceptible. We couldn't get a human volunteer in this country, so we got a volunteer in Japan and a volunteer in Italy.

Hughes: Just one in each country?

Thygeson: One in Japan, but in Italy [laughs] it turned out to be the whole hospital.

Hughes: How did that happen?

Thygeson: In Japan Dr. [Yukihiko] Mitsui was very careful about isolation. The volunteer was in a special room; everything was handled beautifully, and we had this experimental infection that went very well and eventually got well. But in Italy, Dr. Bietti was very careless, and the virus spread all over the hospital. They had to close the hospital. He got a reprimand from the state health department for letting this thing go through the hospital.

Hughes: Do you know what he did wrong?

Thygeson: No, I don't know for sure, but the virus spread all over the hospital.

Hughes: Then did you proceed to study those cases?

* In 1971 the Committee on Human Research was created. Its members were appointed by the chancellor on the recommendation of various campus groups, including the Committee on Research.

Thygeson: We didn't, but he did. We didn't go to Japan either, but Dr. Mitsui studied them and wrote a paper in collaboration.*

Hughes: *You contacted these two people because you knew that they were interested in keratoconjunctivitis?*

Thygeson: Yes. They were both good in infectious disease.

Hughes: *A one-case study would have stood up?*

Thygeson: Yes, because it was one hundred percent successful. If you have one hundred percent success, you only need one or two cases; you don't need a thousand.

Hughes: *That's still true?*

Thygeson: Yes, that's still true. As a matter of fact, in my book there are many more animals used in experiments than are actually needed. When you get one hundred percent or close to one hundred percent infection, the number that you need goes way down. You don't need a thousand guinea pigs. It's only when there's some doubt, when the percentage is around fifty percent or so, that you need a large number to mean something.

Hughes: *Did Proctor have its own animal colony?*

Thygeson: No, we had to use the university colony.

Hughes: *That was just a matter of purchasing animals?*

Thygeson: Yes, Proctor was assigned a room or rooms, and we had monkeys and guinea pigs and mice. Animal care was always a problem at UC because the facilities were never as good as they should have been. So it was always a problem, but we managed.

Hughes: *Were you worried about accidental infection?*

Thygeson: Yes, we had three accidental infections: one technician with adenovirus 8, one animal care handler with trachoma, and we had a technician with parakeet psittacosis. So we had three accidental infections, but nothing serious.

Hughes: *These were transmitted from the animals?*

Thygeson: No, the adenovirus 8 was a laboratory infection from handling the virus. The trachoma was from handling animals that were infected, and the psittacosis was from handling birds that were infected.

Hughes: *Were the infections due to poor technique?*

* L Hanna, E Jawetz, Y Mitsui, P Thygeson, SJ Kimura, A Nicholas. Continuing studies on the association of adenovirus type 8 with epidemic keratoconjunctivitis. *Am J Ophthalmol* 1957; 44:66-74.

Thygeson: The technician who was handling adeno 8 must have had poor technique because she had this accidental infection with adeno 8 and then later she had one with trachoma. So she must have been a little careless. I don't know anything about the third, the one handling the parakeet psittacosis, because that was over in the Hooper, and I didn't have any supervision of that except to take care of her eyes after she'd gotten infected.

Hughes: *The way I understand it, there are three broad areas of investigation at the Proctor, and they are: microbiology, immunology and uveitis, and experimental pathology. Is that the way you break down the three major research areas?*

Thygeson: That's the way it was, but experimental pathology is not active right now.

Hughes: *Why is that?*

Thygeson: Well, Dr. Hogan was the mainstay there, and he's dead. We had an immunopathologist for a short time, and he left for greener pastures. As the moment there isn't anybody in experimental pathology. But for the purpose of writing reports, there were these divisions into three areas.

Funding

Thygeson: The Proctor is at a much reduced level now because we've lost three staff members, Dr. O'Connor, Dr. [Khalid F.] Tabbara, and Dr. [Mitchell H.] Friedlander.

Hughes: *They have not been replaced?*

Thygeson: Financial difficulties at the moment prevent replacement.

Hughes: *Do you want to say more about the financial difficulties?*

Thygeson: Well, in part it was mismanagement, but more essentially it was the drop-off in NIH funds, which has hit everybody except those who are working with AIDS or something which has high priority. Mainly the drop-off in NIH funds, because the Proctor endowment was never really designed to do anything more than take care of one laboratory of infectious disease. It was NIH that supplied the money for the other laboratories. Without NIH, Proctor would still be one laboratory of infectious disease.

Hughes: *Do you remember when NIH came into the picture?*

Thygeson: I think it came in with what we did with Dr. Jawetz; that was our first NIH grant, I believe. After that we had grants for uveitis and a grant for almost everything. Then at one time we had a program which took in everything, all in one NIH grant.*

* Dr. Thygeson is referring to a seven-year Program Project Grant awarded to the Proctor Foundation in 1965 and replacing a number of individual grants.

Hughes: When was that?

Thygeson: That was when I was director.

Hughes: Was that a good thing for the Proctor?

Thygeson: Yes, very good; we didn't have to write all these individual grant requests.

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Thygeson: UC San Francisco has a big program grant for AIDS right now; I just saw it in the paper. So the program grants have been restored.

Hughes: Why were they dropped initially?

Thygeson: I suppose the idea was to weed out poor investigators who might have crept in on the program without being supervised properly.

Hughes: NIH would have had less discrete control.

Thygeson: Yes. Anyway, the program grant was very good.

Hughes: During the years you were director, money was not a prime problem?

Thygeson: No, we didn't have any real problems with money. But Dr. [G. Richard] O'Connor did*, and at the end he had a lot of problems with money, and Dr. Dawson, the present director, now has a problem with money.

The Sensory Disease Study Section and the Institute of Neurology and Blindness, NIH

Mrs.

Thygeson: Did you tell Sally about the study section, because I think that's a terribly interesting little chapter in your life.

Thygeson: Oh, yes. The NIH was preceded by a laboratory of infectious disease in Washington, D.C., the Hygienic Laboratory. I had quite a bit of contact with two of the workers there, Dr. Ida Bengtson, and the director, whose name I've lost now. It was from this laboratory that the NIH developed.

It was really a wonderful laboratory, but it did not have the money to put out grants. It was really a consulting laboratory; everybody could go there and learn, and it was really a very fine thing. I spent quite a bit of time there on trachoma research because Dr. Ida Bengtson was interested in it, and she had a lot of influence in the early trachoma work.

* Dr. O'Connor was director of the Proctor Foundation from 1970 to 1984.

When various institutes in the NIH were formed, one of them was neurology and blindness. I was on the first study section. It was called the Sensory Diseases Study Section. At the first meeting there were only two applications, and it took about a half an hour to go through these, and then we went home.

Hughes: Do you remember the year?

Thygeson: I don't remember the year, but I remember that I came in in the early morning, and I went out about four hours later. But anyway, we were told to go out and beat the bushes and get more applications, so I went. I remember coming home and I got ahold of Dr. [Francis A.] Sooy, and I told him he had to put through a grant on the sensory diseases of the ear. I persuaded him to do that. We beat the bushes, and at the next meeting we got a few applications.

That's the way the NIH developed, slowly, and soon became all powerful, you see, because all the research in the whole country was really based on the NIH. The Proctor Foundation wouldn't have been more than one laboratory without the NIH.

Hughes: Isn't neurology and blindness an unusual title for an institute?

Thygeson: It was because the eye is a sensory organ, you see.

Hughes: But the title of the institute doesn't bring that out.

Thygeson: They got it through Congress that way; a gimmick to get it through Congress.

Hughes: The "blindness" would get votes?

Thygeson: Mildred Weisenfeld of the National Council to Combat Blindness was the one who persuaded Congress to add the "blindness" to the "neurology institute." Later a separate eye institute was set up.

Hughes: Was that a disadvantage in the early days?

Thygeson: Yes, because the chief was a neurologist, and he was really not interested in eye. Dr. von Sallmann, after he retired from Columbia, came in there as the eye section chief, and he was very good. But still eye was a weak sister to neurology, so they really needed a separate eye institute.

Hughes: Were the other sensory organs well represented?

Thygeson: Well, in a way. Stroke became a very important part of the neurology section—neurology and stroke. The eye was minor compared to neurology. But there was plenty of money at that time, so everybody who was interested in research could get a grant. There wasn't any waiting list in those days.

Hughes: Could you comment on the difference in the style of application? I'm presuming that the application procedure was less sophisticated than it is now.

Thygeson: Yes, it was, and there were some pretty sloppily written applications. [laughter] Terrible. Now you see all the universities have little departments to tell you how to write a grant in good English. But in those days some of the investigators had pretty shoddy applications. They were good investigators, but they couldn't write a good grant request.

Hughes: Did you take that into account?

Thygeson: We took that into account, and, if there was a question, we'd go out and visit. I made many, many visits around the country to laboratories to talk about their grant requests. I think the term on the study section was six years, or something like that. I was reappointed for a second time and made chairman of the section [1955]. It became quite a chore, but you learned a lot doing that.

Hughes: Were there other ophthalmologists in the section?

Thygeson: Oh, yes, we had ophthalmologists, and we had a number of Ph.D.s in the section.

Hughes: Who were basic scientists?

Thygeson: Yes.

Hughes: What other specialties were represented in the section?

Thygeson: Initially we had an otologist. It was a very good learning deal, and I learned a lot in those early days, but it was a chore because I had to travel so much.

Hughes: Your associate director took over when you were away?

Thygeson: Yes, and then I traveled before I became the director. When I was in San Jose, in private practice, I had associates down there who would back me up in a very good way. I couldn't have done it otherwise.

Hughes: Did they know and did you know that was going to happen from the start?

Thygeson: No, I didn't know it was going to happen, but it worked out that way. Dr. Beard was my first partner, and he backed me up marvelously because when I was away he would really handle my patients.

Hughes: Do you know what the impetus was to the separation of ophthalmology into its own institute?

Thygeson: Well, just the fact that ophthalmology was low man on the totem pole, so a lot of push developed to separate them. Big push from Jules Stein. He was active in forming the eye institute.* He had a lobby in Washington. He was a real master of lobbying.

* For more on the formation of the National Eye Institute, see the interviews in this series with Thomas D. Duane, M.D.

Hughes: Did he pay it out of his own pocket?

Thygeson: I imagine he did; he put millions of dollars into ophthalmology.

Hughes: And into forming a distinct institute of ophthalmology?

Thygeson: Yes. So I would say he was probably the most important reason for doing that.

Hughes: Were you active in that campaign?

Thygeson: In a mild way, but in no way comparable. Dr. [Alfred E.] Maumenee of Baltimore was very active in that campaign.

Hughes: He was on the study section as well?

Thygeson: Later, he came on it. Then when the eye institute was developed, he was on the council of the eye institute, and Dr. O'Connor was on the council. I never was on the council. [National Advisory Eye Council of NIH].

Hughes: What does the council do?

Thygeson: It was like a dean and a faculty, and then the council had quite a bit of power in recommending programs. It was very important.

Hughes: Did the council select the membership of the study section?

Thygeson: Yes.

Hughes: I assume they were attempting to get individuals who were eminent as well as authorities in different subspecialties. Is that right?

Thygeson: Yes, that's right. It had its bugs, but really, considering everything, it was very effective. There was always the claim of favoritism, that they'd favor their friends and all that, but the charge really didn't amount to much. I've heard that complaint frequently.

Hughes: Was there often contention about whether to award a grant to a particular project?

Thygeson: Oh, yes. But it was a closed session, so it didn't get out very often. There were some pretty lively sessions. In many instances something would be approved and given a very low priority, so it couldn't be paid anyway. That was one way to get around it.

Hughes: Were you paying particular attention to external disease?

Thygeson: Yes.

Hughes: So everybody was there with a specific subspecialty in mind?

Thygeson: Within reason. There were a number who really didn't have any specialty; they were kind of all-round ophthalmologists, supposedly good in everything. The NIH had a tremendous effect on ophthalmology.

Infections Committees and Iatrogenic Disease

Hughes: *You mentioned last time that in the early days you asked Dr. Hogan to set up an infections committee at UCSF and that he didn't do that. Why didn't he?*

Thygeson: He was the first director of the Proctor Foundation, and I was appalled by the number of surgical eye infections. So I asked him to set up an infections committee of the type we had down in San Jose, which was very effective. He kind of went through the motions, but he never got around to forming it, and the reason, of course, was that he was not interested in infectious disease; he never was.

Hughes: *Why do you think there was so much infection in the eye department?*

Thygeson: Carelessness. In surgery you can be careless or you can be very meticulous, and he was careless. He didn't monitor the operating room the way we did down in San Jose. This was disturbing to me.

Hughes: *Were there particular infections that were more prevalent than others?*

Thygeson: Mainly postcataract infections.

Hughes: *Were you the impetus to the formation of the infections committee at San Jose?*

Thygeson: Yes.

Hughes: *How did that work? Who was involved?*

Thygeson: In the O'Connor Hospital, Dr. Les Grams was a pathologist who had an interest in microbiology, and he was very receptive when I went around to see him, and so we set up this committee. Grams did most of the work because he was on the spot and really monitored everything. If there was an infection, we went to find out what went wrong. So we really cut down infections in this San Jose hospital in a remarkable way.

Hughes: *Was that unusual at that time?*

Thygeson: Yes, but now every hospital has to have an infections committee; it's a rule now. Generally speaking the operating rooms are all well monitored now, but they weren't in the old days.

Hughes: *Iatrogenic disease was a problem in the ophthalmologist's office as well as in the operating room, was it not?*

Thygeson: Oh, yes, terrible, because epidemic keratoconjunctivitis was primarily spread in the doctor's office. I had a bad time trying to get the message over that it was a preventable disease. [laughter]

Hughes: *Was this a combination of poor technique and using solutions that were not sterile?*

Thygeson: Contaminated solutions and just poor handwashing. That was disturbing to me because when I started in ophthalmology in Dr. Finnoff's office in Denver, he laid it down on the line. He said, "You wash your hands between each patient, and you wash your hands ostentatiously in front of the patient. The patients must see you wash your hands." He really put that over. Well, that wasn't the rule in ophthalmology.

Hughes: *Wouldn't an ophthalmologist become suspicious when he had a series of patients getting the same infection?*

Thygeson: Well, it's surprising how difficult that is to get over. There was one ophthalmologist in New York who had eighty infections with adenovirus, and then he called on me for help. [laughter] He had to have eighty cases before he recognized it.

Mrs.

Thygeson: I want you to mention Danny's record in this connection.

Thygeson: Some eye surgeons have a bad record on infections, and some have a very good record, and Dr. Vaughan has a very good record, in part because I really went after him.

Mrs.

Thygeson: It is practically perfect, isn't it? He has never had a surgical accidental infection.

Thygeson: Yes. Then another one was Dr. Algernon B. Reese in New York on the staff at Columbia. He was a man in his fifties when I was there; he'd never had an infection, and the reason was that he was terribly meticulous. He wouldn't operate on any eye that had any inflammation or any possible infection. He wouldn't operate on anybody who had a cold. Very, very meticulous. Any case that had a red eye he'd send up to me for study, so he just never had any trouble. He's dead now, but I don't know whether he ever had an infection. He was, you might say, overly meticulous. He cancelled a lot of surgery at the last moment because the eye looked red, and he didn't want to take any chances.

Hughes: *Was that just his nature, or did he, too, have a mentor?*

Thygeson: He was an ocular pathologist, so he knew about infection and so on. He didn't want any infections.

Hughes: *Some of the basis of this problem probably is that infectious disease was not given sufficient emphasis in the residency programs.*

Thygeson: That's right; it wasn't taught. They thought ophthalmology was a surgical specialty, so surgery got the biggest piece of the teaching pie. And then every subspecialty got a piece of the pie, and external disease and refraction got the smallest pieces of the pie. They just weren't taught properly at all.

Hughes: *Is that still true?*

Thygeson: Well, see, I'm out of the picture now, but it probably is still true.

Hughes: *How did you get your message across?*

Thygeson: I only had limited success. [laughs] I didn't do as well as I should have.

Hughes: *You had some hard-core cases to deal with.*

Thygeson: Well, iatrogenic disease is a real problem. The surgeons are still much too careless about it. They'll operate on somebody with a cold. It takes quite awhile to get somebody on the schedule, and they don't want to cancel the case, and they do things that I wouldn't do. It's too bad, but it's a part of any medicine, I think, the prevention of iatrogenic disease.

Hughes: *Is preventive medicine another area that's not sufficiently emphasized?*

Thygeson: In the last five years preventive medicine has come up in a big way. There are books written about it, and the teaching is much better than it used to be. Preventive ophthalmology hasn't come up as much as I would like to see it come up. That's why I wanted it to be a part of the Proctor mission.

Hughes: *Could you say something in summary—we mentioned the three basic fields of research—about what you consider to be the significant contributions of the Proctor in each of those areas.*

Principal Contributions of the Proctor Foundation

Thygeson: I think the main contribution of Proctor has been in the external disease field, and in infection particularly, and in determination of etiology—finding the cause of the different infections and external disease—and teaching the fellows how to recognize a disease either by clinical appearance or by laboratory studies. So we've taught physical diagnosis and laboratory diagnosis. We feel that if you know the cause of a disease you can then give a proper prognosis. Treatment changes from time to time, so teaching treatment is less important than teaching diagnosis.

Hughes: *Is that an unusual emphasis?*

Thygeson: It is, yes. That is not the emphasis in the department of ophthalmology.

Hughes: *Treatment takes the upper hand.*

Thygeson: Yes, treatment comes first and diagnosis is often ignored, whereas so many mistakes are made by the wrong medication for the wrong disease. So we think we made some contribution in increasing the percentage of correct diagnoses. It is said that even now only about fifty percent of clinical diagnoses are proven correct by autopsy.

Hughes: Across the board?

Thygeson: Across the board, in general medicine. We wanted to improve the percentage of correct diagnosis in external disease.

Mrs.

Thygeson: Tell about Mas's sophisticated laboratory.

Thygeson: So we developed through Mas Okumoto, I think, one of the best laboratory setups for laboratory diagnosis of eye infection.

Mrs.

Thygeson: In the world.

Thygeson: He has proven to be a very wonderful teacher of the fellows and technicians and so on. He's had a big effect.

Hughes: That was the very first laboratory at the Proctor, was it not?

Thygeson: That's correct.

Hughes: So that started in 1947.

Thygeson: Yes.

Hughes: Do you remember when Mas came?

Thygeson: Let me change that; actually Mas didn't come in until a few years after that [1952].

Hughes: And he started the laboratory?

Thygeson: I had the laboratory going, and he was my technician. But Dr. Hogan had a pathology laboratory started in 1947, so he actually had the first Proctor laboratory. I started the laboratory for the diagnosis of eye infections also in 1947 in a room that Dr. Meyer gave me. Then Mas came in as technician, and he soon developed very rapidly.

Before we got Mas, I had a couple of technicians that didn't do so well. [to his wife] Remember that one girl that always had a chip on her shoulder? She was Jewish, and she thought everybody was casting aspersions on her ethnicity.

Mrs.

Thygeson: She stayed with us for two days.

Thygeson: We had a terrible time with her, but then when Mas came in everything went well.

Hughes: What is Mas's background?

Thygeson: He's American-born Japanese.

Mrs.
Thygeson: With a master's degree in microbiology.

Hughes: And he came to you with a master's degree?

Thygeson: He came right out of the University of California with an A.B. degree in bacteriology—

Mrs.
Thygeson: It's the only job he's ever held.

Thygeson: —and then he got his master's while he was working for us. He's the type of worker that wouldn't take time out for coffee or anything. You know how the Japanese are.

Hughes: Very industrious.

Mrs.
Thygeson: One week's vacation. He goes to our place in Tahoe with his family. Every year I beg him to take two weeks, but no.

Thygeson: He's so meticulous; if he promises you something, he never fails you. He's actually extraordinary. They don't make them that way any more.

Hughes: He is dealing with material that is submitted for culture and diagnosis? Is that what the laboratory is set up to do?

Thygeson: Yes. And he's the main teacher in the laboratory; teaches the fellows and so on. Then he's called on for consultation. People send him material. He has quite a wide reputation.

Hughes: Do the residents pass through that laboratory as well?

Thygeson: Not the residents, only the fellows.

Hughes: Why not the residents?

Thygeson: Well, there's been a drop-off in interest in external disease in the department. They're interested in everything else, but refraction and external disease are still at the bottom.

Hughes: That seems amazing to me, having the world's center in infectious disease of the eye at their doorstep.

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Thygeson: The chairmen of the department have not been interested. Dr. Hogan was not interested in infectious disease. He was interested in pathology, of course.

Hughes: Dr. Cordes before him was not either?

Thygeson: Dr. Cordes was remarkable. He was more interested than anybody else. Dr. Cordes was very good. But now the current chief, Steven Kramer, is not interested. He's interested in so many other things; it's just a matter of priority. He's doing a very good job, but not in external disease.

However, Proctor is teaching the residents, but not in the way that we used to. When I was there in the early days we had a weekly conference with the residents and two-hour seminars and all that. We don't have that any more. There is a one-hour weekly meeting with the residents that I think is still going on, but I don't have anything to do with it.

Hughes: They meet at the Proctor Foundation?

Thygeson: In the departmental clinic. I haven't been in on that for years.

Hughes: The conference is on external disease?

Thygeson: Yes. It's being carried on, but not in the way that it used to be or that I think would be nice.

Hughes: So the problem is being perpetuated of ophthalmologists being turned out who don't have a firm background in infectious disease?

Thygeson: That's true. But it's better than it used to be, but it's far from being what it ought to be. Now, of course, the Proctor fellows really get a big dose of infectious disease. And we've had about 170 fellows, so they're all around the world.

Hughes: They go back home and take that knowledge and set up laboratories to diagnose and treat infectious eye disease?

Thygeson: Yes, so there's been quite a spread out from Proctor. But not as much in San Francisco as it could have been, I think.

Hughes: What about other American departments of ophthalmology? Is it a similar story?

Thygeson: It's usually a similar story, but there are now maybe a dozen universities that have pretty good laboratories of infectious disease. So it's much better than it used to be.

Hughes: Are those laboratories usually run by microbiologists?

Thygeson: Usually, yes. But you see it's so much better than the early days, because there wasn't anything at all in those early days. There was over in Vienna, and for a little while there was in New York when Dr. [John Elmer] Weeks was

there, but otherwise pathology was much more interesting to ophthalmologists than microbiology. But in Europe there was quite a wave of ocular microbiology. It didn't really spread over to this country.

Hughes: Do you think that's somewhat because of the germ theory, which was a Germanic and French contribution?

Thygeson: Well, yes, because of Pasteur and Koch. So there was much more interest. There were isolated examples of American interest, but there weren't very many.

Hughes: Is it not true that surgery is a far more profitable subspecialty of ophthalmology than dealing with infectious diseases?

Thygeson: Oh, yes, and, of course, for the residents that come in, it's usually cataract extraction that they're interested in.

Basic Science Courses in Ophthalmology

Thygeson: I mentioned that in New York I had a hard time getting anybody interested in basic sciences. We started a course there, and I really had a hard time getting it started. But, of course, now it's quite different. Almost every university either has its own basic science course, or it participates in the Lancaster course or Stanford Basic Science Course in Ophthalmology. So it's entirely different. But even in basic science courses, microbiology doesn't have the place it ought to have.

Hughes: Can you tell me a little about the program at Lancaster and what you did in it?

Thygeson: I participated in the first session of the Lancaster course and for many years thereafter. The first session was held in the Boston City Library.

Hughes: Why was that?

Thygeson: That was because Walter Lancaster set it up that way.

Hughes: Who was he?

Thygeson: He was a very fine clinician at Harvard.

Hughes: It was his idea then?

Thygeson: There were actually two people who had the idea; one was Dr. [Theodore L.] Terry, and then Dr. Lancaster. Terry up and died [1946], and Lancaster carried on. So the course is named after him. I was given a week at half session, so I had half a day a week to cover the ocular infections, external disease.

Hughes: Who came to these courses?

Thygeson: People who wanted to get into residencies or who wanted to become ophthalmologists. Many flight surgeons got interested in ophthalmology during the war and wanted to become ophthalmologists. So this was really preparation for the American Board of Ophthalmology examination. Very popular, always had plenty of candidates.

Hughes: Can you remember what subjects were covered?

Thygeson: All basic sciences, and some that weren't basic, like motility, were taken up. But the principal emphasis was on pathology. That got two or three times as much emphasis as microbiology.

Hughes: Did they put you up?

Thygeson: Sure, I was put up in the Harvard Club there, and then later the course moved to Portland, Maine, in a little college there, and we were put up in the dormitories, and then later we went to Colby College in Waterville. Then we were put up in the Colby dormitories.

Hughes: Sounds very pleasant.

Thygeson: Yes, it was very nice, and I looked forward to it every year.

Hughes: Top people in the field were giving these classes?

Thygeson: Yes.

Hughes: Was it an opportunity for cross-specialty communication?

Thygeson: Not in the course.

Hughes: There wasn't a social part of it as well?

Thygeson: No. But with me it was always pharmacology, because I had Peter Kronfeld from the University of Illinois as my partner. He gave half the lectures, and I gave the other half, for six days. We had our subjects all set up for the six days—twelve lectures.

Hughes: Were the candidates required to attend the entire course? They couldn't pick and choose?

Thygeson: At first they had to attend the whole thing, and then later they allowed some choices. Somebody who had failed something for the American board could come in and get a refresher course. It worked out very well over the years.

Hughes: Is it still going?

Thygeson: Yes, it's still going strong. The Stanford course is going, too.

Hughes: Tell me now about the Stanford course.

Thygeson: Well, it was modeled after the Lancaster course and set up here in Palo Alto, California, at Stanford. It's participated in by the eye units in the Bay Area here, including Davis.* It is very good.

Hughes: *Who set that up?*

Thygeson: That was set up by Jerry Bettman.

Hughes: *Do you remember when?*

Thygeson: No, I don't remember, but he formed a foundation, The Pacific Coast Eye Foundation and I was one of the trustees for three or four years for the Stanford course.

Hughes: *What was the source of the funding?*

Thygeson: Well, the main source of the funding was the tuition, but there was some money given also—grants for some people. It wasn't very much, \$5,000 or \$10,000.

Hughes: *Did the teaching faculty come from the Bay Area?*

Thygeson: From the Bay Area, but there were some others. For instance, we always had a man from Denver, Phil Ellis, in pharmacology; and we always had a man on physiologic optics from the East or Iowa. Otherwise, it was mostly Pacific Coast people. Dr. Beard was in on it; Dr. O'Connor was in on it; Dr. Dawson.

Hughes: *Talking about external disease?*

Thygeson: It had everything, just the same as the Lancaster course.

Hughes: *I mean, the representatives from the Proctor were talking about external disease?*

Thygeson: The Proctor did almost all of the external disease. [tape interruption]

Principal Contributions of the Proctor Foundation (continued)

Hughes: *We talked some about what you considered to be the Proctor's really significant contributions, and you mentioned external disease. Do you want to say something about the other fields?*

* The course is sponsored by the University of California at San Francisco and at Davis, Pacific Medical Center, and Stanford University.

Electron Microscopy and Pathology

Thygeson: Yes. There were quite a number of contributions made by Dr. Hogan in electron microscopy and pathology, and a very good textbook that he made in collaboration with Dr. [Lorenz E.] Zimmerman.*

Hughes: *What is the title?*

Thygeson: One was an electron microscopy atlas,** and the other one was straight pathology, which has been carried on by Dr. Spencer, incidentally. So this is, I think, quite an important contribution in histology and electron microscopy.

Hughes: *He was the one who was particularly interested in pathology?*

Thygeson: Yes. He was the first Proctor director, and he really didn't follow the Proctor mission. I had to do that; I kept on the mission.

Hughes: *Was that a bone of contention when he was director?*

Thygeson: It was kind of a joke because we felt he didn't believe in the germ theory. [laughter]

Hughes: *Did you have to make excuses for the fact that you were putting some emphasis on anatomy?*

Thygeson: No, I just kept on the mission, and I kept in touch with Mrs. Proctor. Hogan didn't.

Uveitis

Hughes: *He was interested in uveitis as well, was he not?*

Thygeson: Yes.

Hughes: *Was that an interest of yours?*

Thygeson: Yes, as a matter of fact, he and I started a uveitis clinic, and that's still going. That turned out to be quite valuable, because uveitis was really in bad shape. The causes of uveitis were really mixed up. When I started, over in Europe uveitis was thought to be all due to tubercle bacillus, and over here it was all due to focal infection.

Hughes: *Everything was lumped under those categories?*

Thygeson: Yes, it really wasn't well worked out. I think in large part through the Proctor uveitis group it's become quite sensible. First of all, Dr. Hogan, and then Dr. O'Connor, played a large part in really putting some rationale to the uveitis

* MJ Hogan, LE Zimmerman. *Ophthalmic Pathology: An Atlas and Textbook*. Philadelphia, London: WB Saunders, 2nd ed., 1962.

** MJ Hogan, JA Alvarado, JE Weddell. *Histology of the Human Eye*. Philadelphia: WB Saunders, 1971.

problem, so now you can say this is an infectious type, like tuberculosis or syphilis or AIDS—AIDS is now a cause of uveitis—or it's an autoimmune disease like sympathetic ophthalmia. You can categorize the cases of uveitis, and it's much better for treatment if you know what the cause is.

Hughes: Did the uveitis clinic personnel work out that categorization?

Thygeson: Yes. I think Dr. Hogan and then Dr. Kimura were quite active in that, and Dr. O'Connor—those were the three. Right now it's Dr. [Robert A.] Nozik who's in charge of uveitis. I would say Dr. O'Connor had the most influence on uveitis, and he was a great lecturer, so he lectured all around the world on uveitis.

Hughes: And got uveitis on the map?

Thygeson: Yes. He's written quite extensively on uveitis, so I think that's quite a contribution of the Proctor Foundation.

Hughes: He came to the Proctor with a grant to do research on uveitis.

Thygeson: Yes. Proctor didn't have any money, so it was his grant that paid his salary. So he really came because of NIH.

Hughes: Were you on the NIH study section at the time that application came through?

Thygeson: Yes. I recruited him. He was working with Dr. von Sallmann, so I went to visit von Sallmann, and I saw Dick and liked his ideas and appearance and everything, so I recruited him for Proctor.

Hughes: He was a resident at the time?

Thygeson: He was what's called a clinical fellow at NIH.

Hughes: He had been working on uveitis?

Thygeson: That was his interest at the time. Toxoplasmosis was his main interest.

Hughes: Is there anything more you want to say about uveitis?

Thygeson: I think uveitis was and is really an important part of the Proctor Foundation, and, of course, it fits in, in a way, in infectious disease because toxoplasmosis is an infectious disease.

Hughes: Is that the main infectious cause of uveitis?

Thygeson: It's an important one. There are other infectious causes, fungal, viral, and, of course, you still have tuberculosis as a cause.

Hughes: Do you suppose that some of the reason that so little was known about uveitis was because of its location? The fact that it was the inner part of the eye?

Thygeson: Yes. It's hard to get at it. To get at it you either have to have autopsy material, the dead eye, or you have to puncture the eye to get material to study. Then you have, of course, the appearance—uveitis has many different clinical appearances and courses, and you can make a guess by the appearance. But often you have to have the aqueous punctate to study. Anyway, Proctor was in on all of that.

Hughes: *Why did the Viennese blame the tubercle bacillus?*

Thygeson: They don't any more. One reason, of course, was that tuberculosis was terribly common in Vienna, as it was in Denver in my early days. So there was a lot of tuberculous uveitis. I saw a lot in my early days in Denver. But they blamed cases on the tubercle bacillus that they couldn't find the tubercle bacilli in or thought ought to be there, without proper justification.

Hughes: *How did they make the diagnosis?*

Thygeson: On the clinical appearance, on granuloma. Tuberculosis is a granulomous disease, and if the eye had a granuloma, they'd call it tuberculous. But, of course, there are many causes of granulomous disease.

Now, I think, uveitis is on very firm ground. The main mistake that's being made now is the identification of uveitis with the steroids. The steroids are very effective in the treatment of the autoimmune type of uveitis, especially iridocyclitis. Many, particularly young, inexperienced ophthalmologists treat every case of uveitis with steroids, and that's murder for the infectious types of uveitis. Steroids make that type worse.

Hughes: *Is the mechanism understood?*

Thygeson: Oh, yes; steroids depress immunity. An infection goes wild when you block immunity.

Steroids

Hughes: *That's been another of your crusades, the judicious use of steroids?*

Thygeson: Yes, the misuse of steroids has been a terrible thing in ophthalmology. I've crusaded every since 1952 on the proper use of steroids. Steroids are a heaven-and-hell drug—heaven if used correctly and hell if used the wrong way.

Hughes: *Did you have trouble getting through with that message?*

Thygeson: Oh, yes, still have. With somebody like Dr. Vaughan that I had close contact with, I had no trouble at all because he understood right away, and most of the Proctor fellows I had no trouble with. But very poor success around the country.

Just a month ago I received a reprint of a paper from England* in which I was given the credit for calling attention to the bad results of steroids—that was referring to an article I wrote in 1953.** So it got around the world, and in a large part steroids are being used much better than they were, but they're still being abused.

Hughes: Is that because it's easy to prescribe a broad-spectrum drug?

Thygeson: Yes, and part of it's due to pharmaceutical companies, because they would provide antibiotics and steroids mixed. They'd say the antibiotics kill the infection and the steroids kill the inflammation. Well, it doesn't work that way because so many things are antibiotic resistant, and so many things need inflammation to effect a cure. The inflammatory reaction is the number one defense we have against all opportunistic infections. You knock that out with steroids and you get all these fungal infections. So there's a long way to go on the steroids; they're still being abused.

Hughes: Apparently there are a number of ophthalmologists who were sued because of postoperative infections—misuse of steroids. Did the legal profession get hold of you?

Thygeson: They tried to; I stayed pretty clear of that. I got into a few legal cases, but not many. I stayed out of the legal side pretty successfully.

Hughes: You were called in?

Thygeson: I was called in; I received letters requesting my services and so on, but I usually was able to get out of it. There were a lot of sad stories, particularly in herpes,*** and a lot of eyes lost unnecessarily.

Hughes: We talked about main line research problems at the Proctor Foundation; I'm wondering about some of the problems that were outside these main fields—ocular genetics and glaucoma, the biochemistry of tears, the histochemistry of blood leucocytes. How did these problems originate?

Biochemistry of Tears

Thygeson: I think the interest in tears was early a part of the Proctor interest because tears are so important in protecting the eye against infection. So the dry eye, called keratitis sicca, was an important Proctor project early in the Proctor scheme. Dr. [William K.] McEwen, who was a biochemist, and Dr. Kimura, as I remember, had an important study of lysozyme in the tears, showing that a drop-off in the lysozyme content of the tears was an early sign of keratitis sicca. Keratitis sicca is an autoimmune disease which affects women in about ninety-nine percent of the cases, and not men.

* D St Clair Roberts. Steroids, the eye, and general practitioners. *Brit Med J* 1986; 292:1414-15.

** P Thygeson, MJ Hogan, SJ Kimura. Cortisone and hydrocortisone in ocular infections. *Trans Am Acad Ophthalmol Otolaryngol* 1953; 57:64-85.

*** See: P Thygeson, MJ Hogan, SJ Kimura. The unfavorable effect of topical steroid therapy on herpetic keratitis. *Trans Am Ophthalmol Soc* 1960; 58:239-62.

Hughes: How do you explain that?

Thygeson: I explain it on the basis that women have a better immune system than men, and in autoimmunity the immune system recognizes small changes in the cells, damage to the cells. As a result of the damage, these cells become foreign bodies, and the immune system wipes them out. Well, in the case of keratitis sicca the lacrimal gland is often damaged by a viral infection, and the cells become foreign body and get wiped out, so the tears get shut off. It's a terrible disease, and it's very important to recognize it early and to do something about it. It's very damaging to the eye. We need the tears.

Hughes: What do you do?

Thygeson: We can use artificial tears, that's the main thing; we can shut off the puncta, so you conserve the tears that you have, and protect the eye against infectious agents and so on. It's still a problem, but much better than it used to be.

Ocular Genetics

Hughes: How did interest in ocular genetics originate?

Thygeson: Genetics in ophthalmology has always been important. A number of the eye diseases are known to be genetic, such as sympathetic ophthalmia, retinoblastoma, and other things are known to have a genetic origin. So we've always been interested in genetics. There was an ophthalmologist in Switzerland, Dr. [Adolph] Franceschetti at the University of Geneva, who had the best genetics laboratory in ophthalmology in the world, I guess. It was based on the eye, but he expanded it into other conditions, too. Switzerland is a hotbed of genetics because in the various valleys there's so much intermarriage, and so all these genetic abnormalities are much worse in Switzerland than they are anyplace else. Franceschetti took advantage of that and built up this wonderful laboratory; he was able to get really good pedigrees on all his patients.

Hughes: Which was not possible at the Proctor?

Thygeson: No, but we did quite a bit of it, as much as we could. We had Franceschetti over here; we had a lot of contact with Franceschetti. Genetics is very important in ophthalmology.

Hughes: Did he come over to do research?

Thygeson: No, mainly to lecture and so on. He was over here quite a number of times, and I was in his laboratory half a dozen times. He's now dead, of course, but he was very important.

Hughes: Another topic is glaucoma.

Thygeson: Glaucoma was not a Proctor subject, but we were interested in the fact that the Indians—we had a big Indian project—

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Thygeson: —don't have open-angle glaucoma. There's closed-angle and open-angle glaucoma, and the open-angle glaucoma—which is the most common type, about ninety percent—is genetic, familial. The Indians just didn't have it; didn't have the genes for that. We were interested in that and whether there was any genetic susceptibility to trachoma, so we worked on that. There wasn't any genetic susceptibility to trachoma, but there was to glaucoma.

The Indians had a lot of genetic problems: They had very high astigmatism, which was transmitted genetically, and they had squint, strabismus, which was of genetic origin. Myopia is also transmitted genetically, and the Indians had a lot of myopia, especially the Eskimos. The Eskimos had a lot of myopia, just like the Japanese have—nearsighted Japanese. So genetics is very important.

Hughes: *So it was really because of other studies that you got into genetics?*

Thygeson: Sure.

Histochemistry of Blood Leukocytes

Hughes: *How did the histochemistry of blood leukocytes tie in with ophthalmology?*

Thygeson: That came in as a part of the uveitis studies. The leukocytes, of course, are terribly important in controlling infectious disease. They also have diagnostic importance—leucocytosis, leucopenia—and then particularly eosinophilia, which is tied in with parasitic disease and allergy. So in the diagnostic laboratory you always look for eosinophilia in the scrapings from the eyes, that really give you a hint as to the allergic nature of the disease. Allergies and parasitic diseases have eosinophilia. And in the parasitic disease it's an allergy, too, because it's an allergy to a part of the body of the parasite that makes the eosinophils. So the eosinophil is a very important sign of allergy.

For example, there was a Pakistanian laborer who had come into the [Sacramento] valley, working over there in Stockton, and one of our Proctor graduates sent a slide to Mas Okumoto. In the slide it had eosinophils and inclusion bodies. Mas made a diagnosis of trachoma and vernal catarrh based on the slide alone, and he was right. The importance of cytology is hard to overestimate in ophthalmology.

Another thing, of course, is the masquerade syndrome, that is, tumors masquerading as infections. A tumor can undergo necrosis and liberate products that produce an inflammation just like an infection. So a tumor can be mistaken for an infection. I remember writing a paper on the masquerade syndrome and describing a number of cases that had been diagnosed as infections that really were tumors. Here cytology comes in very important, because you can recognize a premalignant or a malignant eye involvement on the basis of the cell type. A tumor cell is quite different from a normal cell, having a great big nucleus and nucleolus and very scanty cytoplasm. Well, you can recognize that in the laboratory and save a lot of time and trouble.

Hughes: Is cytology, with the current deemphasis of anatomy and histology, falling out of the educational background of the medical student?

Thygeson: I don't know about medical students, but it's an important part of the training of the fellows at Proctor.

Hughes: Would ophthalmology residents have a good background in cytology?

Thygeson: No, they wouldn't have much on that, but a little because the Papanicolaou smear test is used so much in gynecology. The medical student would have to see that, so he'd get an idea about the importance of tumor recognition in cytology rather than just by the biopsy. So I imagine a medical student nowadays would have some interest in cytology.

Cooperative Research Projects (continued)

Hughes: You mentioned the Proctor Foundation's associations with other departments at UCSF. You mentioned the Hooper Foundation and the department of microbiology, but I also understand that you had associations with the departments of medicine and dermatology, and the school of pharmacy, and also veterinary medicine at Davis.

University of California School of Veterinary Medicine, Davis

Thygeson: Oh, yes, we had a long association with Davis, and particularly with Dr. Robert N. Cello. Dr. Cello was a pioneer, a veterinary ophthalmologist, and he used to come to Proctor once a week. He always brought in one or two good cases of veterinary ophthalmology. He gave us a lot, and we gave him a lot, I think.

Hughes: Was there anything that you worked on with him?

Thygeson: Yes, we worked particularly on feline pneumoencephalitis, which is a chlamydia disease.

Hughes: That has ophthalmological symptoms?

Thygeson: Yes, it has an eye infection. And then I worked with him on zoonoses, and wrote a chapter with him on ocular zoonoses.

Hughes: That was for what?

Thygeson: That was for a seminar at South Lake Tahoe and for our book on preventive ophthalmology. He's already contributed his chapter, and I have mine still in preparation. *

* EH Friedlander, ed. *Prevention of Eye Disease*. New York: Mary Ann Liebert, Inc., 1988. Dr. Thygeson wrote the introduction, but the chapter on ocular zoonoses was not in the end included.

Department of Medicine, UCSF

Hughes: What about Proctor associations with the department of medicine?

Thygeson: The department of medicine was always concerned with uveitis, because the medical work-up was a part of the uveitis survey. So we always had a consultant from the department of medicine. I think there are three or four that I can think of over the years. The most important was Dr. Wallace Epstein, now in the department of medicine, who is supposed to retire this year.

Department of Dermatology, UCSF

Hughes: And dermatology?

*Thygeson: In dermatology we had a very close association with Dr. [Manuel Francisco] Allende, who has since died. Then with Dr. Bill Epstein. Dr. Allende and I had a long collaboration on various skin-eye problems.**

Hughes: Has there ever been tension between the dermatologists and ophthalmologists because their areas of expertise somewhat overlap?

Thygeson: Only in the case of blepharitis, where the dermatologists call everything seborrheic, and I divide it into three different types. That was the only time we tangled at all.

Hughes: They didn't buy the subdivisions.

Thygeson: Well, Dr. Allende did. We had an exhibit at the Academy of Dermatology, and Dr. Allende and I got a prize.

Hughes: Do you remember what the project was about?

Thygeson: It was skin-eye associations.

Hughes: In general?

Thygeson: Yes. We had one of these automatically revolving slide carousals.

Hughes: Was that a new things in those days?

Thygeson: Yes, that was new at that time. It attracted quite a lot of attention because it was constantly changing; we had it going all the time. So we got a prize.

Hughes: You considered these exhibitions at various medical associations quite important?

* See, for example: MF Allende, P Thygeson. Combined ocular and cutaneous manifestations of disease. *Postgraduate Med* 1955; 17:192-202.

Thygeson: Oh, yes, we did that often. We always had a couple a year. It was a very important means of getting a message over. You can do more by an exhibit than you can by a lecture, because in an exhibit people come by for three or four days; hundreds of people may come to look at the exhibit. You have a chance to talk to anybody specially interested.

Hughes: *Did you usually stay near the exhibit so that you could talk to people?*

Thygeson: Yes. The time that wasn't occupied by the lectures we would. It was a big chore—preparing the exhibit, exhibiting the exhibit, and taking the exhibit down. It was a tremendous chore, but it was so valuable, we did it. Proctor's always had an exhibit. We still do them now; there's always a changing exhibit on the third floor of the Medical Sciences Building [UCSF].

Hughes: *Which meetings did you usually have your exhibits at?*

Thygeson: At the Section on Ophthalmology of the AMA and the Academy of Ophthalmology. Those were the only two. Now there is ARVO, the Association for Research in Vision and Ophthalmology.

Hughes: *You mentioned the one at the Academy of Dermatology.*

Thygeson: That was only once.

School of Pharmacy, UCSF

Hughes: *What about Proctor associations with the school of pharmacy?*

Thygeson: In the school of pharmacy we worked with Dr. Sid Riegelman, who was a professor. He was interested in how to use medications in the eye, the sterilization of eye preparations, and the prevention of infections due to cosmetics and so on. So he and Dr. Vaughan worked together quite a bit. He died, as you probably know; he was drowned. A big loss. He was snorkeling some place and died in a water accident. He was a wonderful consultant for ophthalmology; he was very knowledgeable. He made preparations that were very valuable.

For example, when antifungal antibiotics came in, they were at first terribly irritating to the eye. Sid made a preparation that was tolerated by the eye so we could use amphotericin B, which was one of the first antifungal preparations, every half hour and get by with it. Formerly, you could only use it a couple times a day because it was so irritating.

Hughes: *Did he have any connection with the move to try to make ophthalmic solutions sterile?*

Thygeson: Oh, yes, he did.

Hughes: *When do you suppose that initial attempt was made?*

Thygeson: That was when Dr. Vaughan was a resident;* that was a long time ago.

Hughes: *Why was Dr. Vaughan particularly interested?*

Thygeson: We had these eye infections due to *Pseudomonas*, and they occurred because of contaminated solutions, so Dr. Vaughan took that on as a research project.

Hughes: *What was his connection with the Proctor Foundation at that stage?*

Thygeson: He was a resident in ophthalmology, and at that time I was teaching the residents; I taught them external disease. That was before the separation really occurred between the Proctor Foundation and the department of ophthalmology. I was responsible for all the early teaching of the residents in external disease.

Hughes: *He became interested in infectious disease?*

Thygeson: Yes.

Hughes: *He was never a Proctor fellow though, was he?*

Thygeson: No, he never was. I got Bill Spencer, who was a medical student at that time, interested in *Pseudomonas*. He did some studies and wrote a paper that got the Schering Award—I think he got \$500 or something like that. He was a fine student and showed early research capability.

Hughes: *What is the Schering Award?*

Thygeson: It's the award that the Schering Company gives to medical students every year for research projects.

Hughes: *In any field?*

Thygeson: Yes.

Hughes: *We talked about collaboration with the Hooper Foundation, but we didn't mention Dr. [Julius] Schachter. Do you care to say something about him?*

Thygeson: Dr. Schachter was a Ph.D. candidate under Dr. Meyer, so he was working on chlamydia, particularly psittacosis chlamydia, then known as bedsoniae, after Sir Sam Bedson. That's a name that's gone out of style now. In the process of Schachter getting his Ph.D., we ran into a number of common problems, so we got quite well acquainted. He's still a consultant for Proctor.

Hughes: *Did you have any particular research in common?*

Thygeson: I never did, but Chan Dawson did and still does. He's still collaborating with him now. They're going to have an international chlamydia meeting here within the year [1987], I think. I think Dr. Dawson and Dr. Schachter are convening that.

* Dr. Vaughan was a resident in the department of ophthalmology at UCSF from 1948 to 1951.

Hughes: *Do you know where it will be held?*

Thygeson: Here in San Francisco.

Trachoma (continued)

Hughes: *I know of Dr. Dawson's work with trachoma. Was that an outgrowth of the studies that you had been doing for so long?*

Thygeson: During the war the trachoma projects were all stopped because everybody had gone in the army. Then after the war I was called upon by the Public Health Service to investigate an outbreak in southern Arizona among the Papago. We went down there and found about thirty-five percent of the children had active trachoma. So we started the program up again—it had been originally started in 1938. That's when I wrote that atlas on trachoma.*

We had to have somebody in the field all the time, and I couldn't go in the field all the time, so I looked for somebody and went down to CDC [Center for Disease Control] in Atlanta and got hold of Dr. Dawson while he was still an epidemiologist for CDC. I got him assigned to the Indian work. That's how we got hold of him, through CDC.

Hughes: *He had been working on trachoma at CDC?*

Thygeson: No, he hadn't had any experience with trachoma at all.

Hughes: *You were looking for an epidemiologist?*

Thygeson: Yes.

Hughes: *How did you happen to know of him?*

Thygeson: I went down there when Dr. [Alexander] Langmuir was the chief, and we looked over all his boys, and I told him about my project. Dr. Dawson was interested, so we signed him up. He later became attached to Proctor as a fellow, and then he took a residency at UCSF. Then he stayed on at the Proctor, so his entire career has been with the Proctor Foundation.

Hughes: *The atlas you mentioned is called Trachoma Manual and Atlas, and it was published by the US Department of Health, Education, and Welfare, Public Health Service Publication No. 541, in January 1958, and was revised in 1960. Was it your idea to publish a manual?*

Thygeson: Yes.

Hughes: *Are those your photographs?*

Thygeson: Those are all my photographs.

* The atlas was published in 1958, see reference in text below.

Hughes: Do you know how widely it was used?

Thygeson: It was given to all the Indian Health Service doctors, and then to a lot of other people who were interested, too.

Hughes: I understand that [Arthur Ferguson] MacCallan's classification of trachoma is in general use all over the world.

Thygeson: Still is. It's been added to a little bit, a little more complex than I like to see it, but it's still being used.

Hughes: When was that first devised?

Thygeson: MacCallan was a World War I ophthalmologist; way back in those days.

Hughes: He was a trachoma expert?

Thygeson: Yes, an Englishman who did all his work in Egypt. He built the ophthalmic hospitals in Egypt. He was the grand old man of trachoma. He was a clinical man; he never was a laboratory man. He attempted to eradicate trachoma from Egypt, which is an impossible task.

Hughes: How was he proposing to attempt that in those days before the sulfas?

Thygeson: What it really amounted to was that his ophthalmic hospitals were surgical units and corrected the deformities caused by trachoma.

Hughes: But that wouldn't get at the cause.

Thygeson: No, it wouldn't get at the cause at all. He wrote two books, which I have.*

Hughes: Did you use his system as he had written it, without the later modifications?

Thygeson: Yes. Dr. Dawson used the modifications.

Hughes: Can you summarize what those are?

Thygeson: MacCallan's classification of trachoma was by stages one, two, three and four. Four was the healed stage, three was the scarring stage, two was the follicular stage, and one was the incipient stage. The newer classification defines stage two and where the follicles are, how many. It gives the intensity of the follicular reaction. I didn't use that; I just used stage 2a for the follicular and 2b for the papillary type. It was much simpler, and I really don't see the need for this super classification.

Hughes: Do you know what the argument is for the super classification?

Thygeson: To give a better idea of the intensity.

* AF MacCallan. *Trachoma and Its Complications in Egypt*. Cambridge: At The University Press, 1913; Trachoma. London: Butterworth and Co., Ltd., 1936.

- Hughes: *Apparently it was the modified system that was used by WHO's [World Health Organization's] Expert Panel on Trachoma?*
- Thygeson: Later on, when Dr. Dawson was on the committee. During my period on the committee it was not used.
- Hughes: *That maybe had some connection with the fact that you were on it.*
- Thygeson: I played a part. Dr. [Peter] Maxwell-Lyons was in a large part responsible for the WHO expert committees over the years, and he didn't believe in modifying the MacCallan classification; thought it was good just the way it was.

Research on Inflammation and Degenerative Ocular Diseases

- Hughes: *As late as 1976, the following statement was made in one of the Proctor Foundation publications: "A regrettable lack of knowledge in the field of inflammatory and degenerative ocular diseases has prompted our intense investigation of their pathogenesis." Is this touching on what we were saying before, that ophthalmologists in general were just not versed in these basic science aspects of ophthalmology?*
- Thygeson: Yes, particularly in infectious disease because, of course, they weren't microbiologists. Originally, they didn't understand the problem of immunity in regard to ocular infections.
- The first line of defense in the eye is the inflammatory reaction, before antibodies or anything else can develop. It takes a week or so before the antibodies can develop. So it's the acute inflammatory reaction that protects the eye from all these opportunistic infections. The general ophthalmologist did not understand this, but the microbiologist did, of course.
- Hughes: *Is the degenerative aspect part of the same pie?*
- Thygeson: No, that's really different. I really don't know just why they brought in the degenerative thing. The inflammatory side is terribly important because it's the basis by which the eye is protected against all opportunistic infections. That's why steroids are so bad, because they knock out the inflammatory reaction.
- Hughes: *So the Proctor Foundation was never particularly involved with the degenerative diseases?*
- Thygeson: Only in a minor way; degenerative conditions involving the cornea were of interest in the teaching of corneal disease, but not in the research projects.
- Hughes: *Degenerative disease was not part of the initial Proctor mandate.*

Thygeson: No, but it was part of the teaching program.*

Hughes: *Were there times when you had to adapt the research mission of the Proctor Foundation in order to fit in with the current national research trends?*

Thygeson: No, we never had that problem, but now if you want to go into AIDS and its eye involvement, there would be money available.

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Hughes: *Was there any scientist or physician who came into the Proctor with an interest that up until then had not been a research focus?*

Thygeson: I mentioned that Dr. Hogan was not interested in infectious disease, and that was a problem. Dr. O'Connor's interest was in uveitis, which was not part of the original mission, but he understood the mission, and he was very good about keeping the mission going. But even there we had a little trouble because as director of the Proctor Foundation he was mainly responsible for getting the fellows, so we got more uveitis-oriented fellows than we did regular infectious disease-oriented fellows, and that was a little disturbing. Now, with Dr. Dawson as director, we've gone back; the current fellows are infectious disease-oriented.

Hughes: *So the interests of the director of the Proctor Foundation have a real bearing on the direction of research?*

Thygeson: Oh, sure, because the director gets the fellows, so he's important.

Hughes: *Was it easier to get funding for work in uveitis than in external disease?*

Thygeson: It used to be. I don't know about the present situation, but at one time there were so few interested in uveitis that more funds were available.

Hughes: *Have you said enough about the major research contributions of the Proctor?*

Phlyctenulosis

Thygeson: One important disease that hasn't been touched on is phlyctenulosis. Phlyctenulosis was a terribly important disease in my early days.

Hughes: *Your early days at Proctor?*

Thygeson: No, in Colorado. In Colorado the majority of patients in our eye clinic at the university had phlyctenulosis—young children. A blinding disease. It's an allergy to the tubercle bacillus.

Hughes: *Why was it so prevalent in Colorado?*

* Parts of the discussion, which follows on the tape, of pressures to change the Proctor mission was incorporated in the text of a similar discussion appearing earlier in this volume.

Thygeson: Because Colorado was full of what were called "lungers," who had come from the East to recover in the Rocky Mountains. They used to think that the cure for tuberculosis was fresh air and sunshine. So the "lungers" from the East Coast all came to Colorado.

Our medical school was staffed by "lungers" from Hopkins and Harvard and so on. One of our best internists, James Waring, was a "lunger" from Hopkins. The children of "lungers" would get low-dose tubercle bacilli in their environment, and they were the ones who'd come down with phlyctenulosis.

Hughes: Why low-dose?

Thygeson: They don't get the full pulmonary disease. They get a tuberculous infection without the clinical pneumonia. Wherever tuberculosis was rampant, there you had phlyctenulosis.

Hughes: That explains the incidence of the disease in the Eskimos in Alaska as well.

Thygeson: Yes, it was terrible. The Eskimos had nine times the mortality rate from tuberculosis than we had down here. Phlyctenulosis was a terrible scourge for the Indians, and especially for the Eskimos. Bone and joint tuberculosis was rampant.

Hughes: Why the Eskimos?

Thygeson: The way they lived. They were cooped up all winter in—they weren't in igloos, but little one-room houses. So if one had an open tuberculosis, the whole family got it.

The Indians had a more outside life and a less familial life, and so they had less tuberculosis. But even so, they were running around ninety percent tuberculin reactors down in Arizona and New Mexico. So phlyctenulosis was also a terrible disease of the Indian children. You couldn't possibly remember what tuberculosis was; it was the white plague. Every family had a death from tuberculosis—consumption—in the early days. Even the operas were written around consumption.

Hughes: It was penicillin that brought it under control?

Thygeson: No, it was really isoniazid that brought it under control. Because of this Eskimo and Indian deal, the Proctor Foundation made a special two years' study of the Eskimos with Milo Fritz up in Alaska.

Hughes: Were you invited by the Public Health Service?

Thygeson: I was invited by Milo and the State Health Department of Alaska. Then the Public Health Service came in on it later. But what we did was to show that topical steroids would abolish the signs and symptoms of phlyctenulosis almost overnight. This was the main victory for steroids. We could stop

phlyctenulosis cold, but we didn't cure the tuberculosis which was in the chest, you see. Isoniazid came along and took care of the tuberculosis. They gave isoniazid to all the positive tuberculin reactors.

Hughes: When was that?

Thygeson: Our work was in 1952 and 1953 on steroids; then it was a little later that the isoniazid came in.

Hughes: Who was Milo Fritz?

Thygeson: Milo Fritz was the first ophthalmologist in Alaska. He was practicing in Ketchikan, the most southern town in Alaska. He later was in the air force, and was assigned to me down in Florida, so I became acquainted with him there. He was the one who got the phlyctenulosis project started in Alaska. He was the spark.

Hughes: He had been in Alaska prior to the war?

Thygeson: Yes, he'd been in Ketchikan. He was in the air force reserve, so he went in early into the war. He was in on the Aleutian campaigns. Then he went back to Anchorage, and he was still the only ophthalmologist in Alaska. He was the one that got the Alaska State Health Department interested in phlyctenulosis, and they financed our trip. So we published two or three papers.*

In Alaska it was the human tubercle bacillus that was at fault; there wasn't any bovine or avian tuberculosis. There was a very clear-cut connection between the pulmonary focus and the phlyctenulosis.

Hughes: Was that not true in the Indian population?

Thygeson: It was still true, although bovine tubercle bacillus probably came in on the Indian population. But the Eskimo [tuberculosis] was all human.

Hughes: How did these visits work?

Thygeson: We did our preliminary work at Sitka, at Mount Edgecumbe, where there's an industrial school for Eskimo children. We used that as the base, and from there we went around to various towns like Haidaberg, and so on. We rented an airplane and went around to the Indian villages. Dr. Fritz carried on in between our visits.

It was interesting that we could stop phlyctenulosis cold, so Dr. Fritz got through the Public Health Service that every Eskimo child with a red eye would get forty-eight hours of steroids, without any diagnosis. The nurse would use it. Of course, it worked out well, and we got into no complications. If we'd done that down here we would have gotten into complications. But we

* MH Fritz, P Thygeson, DG Durham. Phlyctenular keratoconjunctivitis among Alaskan natives. *Am J Ophthalmol* 1951; 34:177-84. P Thygeson, MH Fritz. Cortisone in the treatment of phlyctenular keratoconjunctivitis. *Am J Ophthalmol* 1951; 34:357-60. MH Fritz, P Thygeson. Phlyctenular keratoconjunctivitis among Alaskan Indians and Eskimos. *Pub Health Rep* 1951; 66:911-44.

didn't get into any trouble up there; they didn't have herpes. So we stopped the progress of phlyctenulosis in Alaska. Then the isoniazid came along and took care of the tuberculosis.

Hughes: You were able to reach a significant portion of the population?

Thygeson: We got all the Eskimo population because we had the public health nurses who were scattered all over Alaska.

Hughes: I've heard people say that the public health nurses in Alaska were remarkable people. Did you find that to be true?

Thygeson: Yes, fabulous. There was one girl they called the Georgia Peach, who was from Atlanta, I think. She was at Haidaberg, and she ran this dispensary, and she could set broken legs and arms. She had a radio connection with the hospital in Juneau, and she could fly any serious case into Juneau from Haidaberg. She could get advice from Juneau on anything she couldn't handle. She was really the doctor of the little down of Haidaberg.

Hughes: So the nurses were rising to the occasion.

Thygeson: Oh, yes, they were good, and they seemed to like it; the responsibility and everything seemed to appeal to them.

Hughes: Was there ever any need to bring patients back to the Bay Area?

Thygeson: No, but they came back. When mobilization occurred for the Korean War, they inducted a bunch of Eskimos into the army from Alaska who had no active phlyctenulosis but they had terrible pulmonary tuberculosis—they had cavitation. How they got in the army I don't know, but about forty of them were sent up to us because of Dr. Vaughan. He was a consultant down at the Monterey army installation, Fort Ord. We examined about forty of these Eskimos who all had terrible scars from old phlyctenulosis, before the steroids had gotten in there. Most of them had signs of active clinical tuberculosis. This was before the isoniazid got in.

So they had to kick all these people out of the army—it was a terrible expense and waste—and send them back to Alaska. It showed the terrible ravage that tuberculosis had on the Eskimos; it was awful. The isoniazid completely turned around the whole picture. You can use isoniazid in children safely. In adults you get into trouble by liver damage, but in children the isoniazid works very safely.

Hughes: Why is there no damage to the liver in a child?

Thygeson: You know in the old days everybody used to get catarrhal jaundice. That was hepatitis A; it didn't do any damage; they all got well; the liver regenerated and all that. But if you get hepatitis in old age, you're dead. The liver can't make a comeback. That's like isoniazid damages the liver, but it doesn't permanently damage the liver.

Photography in Ophthalmology

Hughes: I understand that photography had quite a role in the study of phlyctenulosis. Were you taking photographs of these patients?

Thygeson: Oh, yes, we kept the Eastman Kodak Company supplied.

Hughes: It was certainly not the first time that you'd used photography?

Thygeson: I'd been using photography all the way from Colorado on, because to document your research project you had to use photography.

Hughes: Was photography not used much in ophthalmology in the thirties?

Thygeson: No, it wasn't used much, but there were certain people interested. For instance, Dr. Castroviejo was very much interested in photography, and he did quite a lot of pioneer work on photography of the cornea.

Hughes: But you'd been using the camera before that.

Thygeson: Oh, yes, I used it a lot. I kept on with the stereophotography, and I used stereophotographs for seminars. I felt that the camera was essential for external eye disease. You had to document.

Hughes: When did you begin to use color film?

Thygeson: In Colorado I used the Agfa starch granule film. Kodachrome was invented by two musicians, and it got taken up by Eastman Kodak [1935]. We got some of the first film while I was in New York. I have good color photographs taken in 1941 and 1942.

Hughes: Do you suppose those are the first in ophthalmology?

Thygeson: No, because Castroviejo had started to use color about the same time we did.

Hughes: I understand the Donaldson camera was also very useful in ophthalmology.

Thygeson: The Donaldson camera came into very good use because with that you could have stereoscopic slides for teaching. You could get fine changes in the cornea that you wouldn't get by straight photography. The Donaldson camera really made stereophotography good. I had been using a Zeiss stereocamera on the trachoma work before that, but in no way was it as good as the Donaldson camera.

Hughes: [Donald D.] Donaldson was an ophthalmologist—

Thygeson: At Harvard.

Hughes: Do you remember when that camera came in?

Thygeson: No, I don't remember the time. It must have been the sixties. It was an extremely useful instrument, and I still have one out in the barn [in Los Altos].

Hughes: Has it now been superceded?

Thygeson: At least it's not being used at Proctor, and I'm a little bit sad because it was so good. A marvelous camera.

Hughes: How extensive is your collection of ophthalmological photographs?

Thygeson: I've got thousands of photographs.

Hughes: Which date back to the Colorado days?

Thygeson: Yes.

Hughes: Are even those older ones still of teaching quality?

Thygeson: I use them occasionally. I used stereophotography in black and white in the study on trachoma in Mr. Brown in which we made a human inoculation of the [infectious] filtrate. Of course, without the photographs your documentation wouldn't be worth much. I still have those photographs of Mr. Brown around.

Hughes: Do you have a slide collection as well?

Thygeson: You mean pathology? Yes, I have one.

Hughes: Is that as comprehensive as your photography collection?

Thygeson: It's pretty good but not as comprehensive. [tape interruption]

Further Efforts to Ensure the Independence of the Proctor Foundation

Hughes: You have already described Dr. Saunders' attempt to combine the Proctor with the department of ophthalmology. Were there other attempts?

Thygeson: There was a second attempt made to do the same thing. The present chancellor [Julius R. Krevans] wanted to make Proctor a part of the department. He attempted to do that—Mrs. Proctor was dead—but Dr. Saunders helped me to squash that. So I don't think that will be brought up again, for the reason that we got from Mr. [James E.] Holst, the university attorney, that the donor's word is absolute; what he wants goes. If there's any change in the mission it has to be an action of the Regents, and it has to go through the attorney general of the state of California. It's a very wise thing, because donors want to be sure that their gifts are going to be followed out.

Hughes: When the second attempt came, and Saunders came to your aid, was it simply to restate Mrs. Proctor's wishes?

Thygeson: Yes, we had a meeting with [Julius R.] Krevans—he was then the dean. He had to back down because there wasn't anything else he could do.

Hughes: Has Dr. Saunders had any particular connection with the Proctor since then?

Thygeson: He's been very good; he got a little money for the building fund. He did one very good thing: He got the Proctor Foundation taken away from the dean and put under the chancellor. This has been a very valuable thing. It saves money and gives the Proctor a kind of a standing in the university outside the medical school. The Proctor is not in the medical school; it's under the chancellor.

Hughes: So it's a freestanding foundation?

Thygeson: Yes, it's much freer, much better.

Hughes: Why does that give it money advantages?

Thygeson: Well, NIH takes out about forty percent for overhead.

Hughes: So you don't have the overhead?

Thygeson: Some deal makes it much more advantageous to work through the chancellor than through the dean. The dean takes out more money, or something.

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Hughes: When the Proctor Foundation was being set up, you insisted on purchasing laboratory space from the university.

Thygeson: Yes. This is something I learned from my experience back East, because the Howe Laboratory had not purchased space and was dependent on the Massachusetts Eye and Ear Infirmary to lend space. It made the Howe Laboratory very vulnerable. So I learned that what you had to do is to buy the space, and then it becomes what's called endowed space. Nobody can take it away from you. It still belongs to the university, but the space committee can't reassign the space to somebody else.

Hughes: Could that aspect be challenged?

Thygeson: I doubt if it could be challenged. It's only been challenged once, and that was when the dean, William O. Reinhardt, was trying to find space for a new department, and he came down and asked us if we couldn't move Proctor from the space that we had up in the Medical Sciences Building down to 95 Kirkham. We said, "No." [laughter] That was the end of that.

Hughes: You had an opportunity, when the towers of the Health Sciences buildings were opened, to be in that space. Why did you decide not to?

Thygeson: Because we decided it would be much better to have the space that would be vacated by pathology, which was on the same floor and continuous with the space the Proctor Foundation already had in the Medical Sciences Building. It was much better to have a continuous space. So with some difficulty we got in on that.

I got one space free; one we didn't have to buy. They had to readjust the women's lounge to put in a new support for the building, so they had to revise that, and that left a pretty good-sized room which I managed to get for nothing. That's the only Proctor space we didn't have to buy. It's a good room for one secretary.

Hughes: *That was quite a coup, considering what a premium space is at that institution.*

Thygeson: Yes, we had to fight for it, but I got it.

The Proctor Foundation Advisory Committee

Hughes: *Could you tell me what the purpose is of the Proctor Advisory Committee, which was established in 1980?*

Thygeson: Do you mean the board of governors?

Hughes: *No, I don't think so. In 1980 the director of the Proctor Foundation, who at that time was Dr. O'Connor, and the Proctor Foundation's board of governors invited ten friends of the Proctor to serve on a committee.*

Thygeson: That's right. That's a common practice to have friends of the institutes, supposedly for raising money. That's actually what was done, but it didn't work out as far as any money's concerned. Right now it's dormant; it hasn't been reactivated.

Hughes: *Was it the intention that that committee would actually meet?*

Thygeson: Once a year.

Hughes: *Did that ever happen?*

Thygeson: Yes, we met two or three times, but nothing of value came out of it.

Hughes: *Just for the record, the people on that were Barbara Proctor Thompson—who was the niece of Dr. Proctor?*

Thygeson: The niece of Dr. Proctor. She and Frances [Proctor Wilkinson], another niece, were favorites of Dr. Proctor, but when Dr. Proctor got married, they didn't get along with his wife at all. Their brother did, so Harrison Proctor was a great friend of Mrs. Proctor, but the two nieces wouldn't have anything to do with her. [laughs] They contribute every year, about this time.

Hughes: *And then Charles Foster.*

Thygeson: Charles Foster's sister, Florence Foster, was a patient of mine. Charles Foster was a General Electric executive, and we thought he might be able to raise some money for us, but he wasn't able to. However, his sister is quite generous.

Hughes: *And then Ralph Heintz, whom you mentioned, and Harry Hind.*

Thygeson: Harry Hind was very important to the Proctor Foundation.

Hughes: *Jack Lange.*

Thygeson: Yes, he is the medical publisher.

Hughes: *John Saunders—we've spoken of him. George Stepovich.*

Thygeson: He was the Heintz lawyer.

Hughes: *Ruth Lee Thygeson and Dan Vaughan. So I think we've covered everybody.*

Early Proctor Foundation Laboratories

[Interview 5: January 7, 1987] ##

Hughes: *The Proctor Foundation embarked in 1947 upon a modest program of research dealing mainly with experimental pathology and microbiology. Where exactly was the laboratory?*

Thygeson: The laboratory started in a room loaned by Dr. K. F. Meyer, and then he gave us a sheep shed which we remodeled into a two-room laboratory.

Hughes: *Where was the sheep shed?*

Thygeson: It was next to Hooper, up the hill a bit, two nice laboratory rooms. So it started in a very modest way with one technician. Also Dr. Hogan had a preexisting pathology laboratory in the old library in Medical Sciences, room 318. So we started in a pretty small way. Of course, the Proctor Foundation was really supposed to be one laboratory dealing with infectious disease, and the expansion came primarily through NIH. Without NIH the endowment would have been sufficient only for one laboratory.

Hughes: *Do you remember when NIH came into the picture?*

Thygeson: I think we'd been going about ten years before NIH came in. We'd made some effort to raise other funds; we'd gone to the Commonwealth Foundation, and other foundations, too. We had some success, but not enough to really undertake the program that we had later. So NIH was really responsible for the development of the Proctor Foundation into its maximum form.

Hughes: Is that about the period when the electron microscopy lab came into operation, in the late fifties?

Thygeson: Yes. That came in also by NIH grants. That was supported almost entirely by NIH grants.

Hughes: What were the laboratories that preceded it?

Thygeson: Pathology and infectious disease.



At the formal opening of the Proctor Foundation Laboratories in 1954. 1 to r: Drs. Giambattista Bietti, Ludwig von Sallman, Phillips Thygeson, and Lopez Quinones.

The Infectious Disease Laboratory

Thygeson: We had a very good infectious disease laboratory, which was the core of the Proctor Foundation and the original idea. The original idea was to have one good laboratory of infectious disease, and then to take advantage of the other departments and institutes in the university for supplementary work which wouldn't cost Proctor any money. Proctor didn't have the money. So the big advantage of tying in with the university was to take advantage of all the basic science departments and to do collaborating research. But it was NIH that gave the real final stimulus to the Proctor Foundation.

Hughes: In those years before you became director [1959]—you, of course, had your private practice in San Jose—when you were at the Proctor, were you mainly in that infectious diseases laboratory?

Thygeson: Yes. This was a very good program for me because I had Tuesdays and Thursdays free from practice. The practice gave me financial support, three and a half days in San Jose, but Tuesdays and Thursdays I was free and nobody bothered me. I didn't have the committee work and all that I got into later on when I got to be director. So I had a very good situation for good laboratory work, and I think we did good laboratory work during that period of private practice.

Hughes: Was there a technician?

Thygeson: Yes. We had several technicians until we got our best one, the current one, Mas Okumoto. He's stayed with us all this time. He retires July 1, 1987.

Hughes: I've read of Mas referred to as the backbone of the Proctor.

Thygeson: Yes, he's been called Mr. Proctor Foundation because he really represents the mission of the Proctor Foundation. A very successful microbiologist, a very good teacher, a very good research man; he really epitomizes the best of the Proctor Foundation.

Hughes: Do you remember when he came?

Thygeson: 1952. I remember I'd had two technicians before he came. He wrote his thesis for his master's degree on steroids in 1952 while he was with us. He has a very wonderful bibliography on infectious disease.

Hughes: A wonderful teacher, is he not?

Thygeson: A wonderful teacher. A great fellow in every respect.

Hughes: What happened in the laboratory on the days that you weren't there?

Thygeson: I always had technicians, so I always had somebody to watch over animals and so on, but I modeled my work so that the main work was on Tuesdays or Thursdays. So I had the full day; came up early and left late, two wonderful days.

Hughes: Were you seeing patients?

Thygeson: No, no patients. Well, once in awhile they'd call me in as a consultant, but I had no formal patients up there at all. I always had a seminar on Thursday, so occasionally patients would be run in that I'd look at, but I had no practice or anything like that. It was a very successful time for me because I had those two days completely free, and my partners down in San Jose would take care of any emergencies for me, so I never got called at the university.

When I became director I was interrupted a lot of times, the way I had been in New York, which I didn't like very well. So I was able to do better work in those two days than I could do later because of the interruptions when I spent full time at the Proctor. I kept the laboratory going, but not in the same delightful way, because in the early years we really had a good time.

Hughes: Did you and Mas work as a team?

Thygeson: Yes. From the early days we had one big laboratory with two rooms. Mas was in the main laboratory, and I was in the smaller room, and Ruth Lee came up with me every day. She typed and edited and so on. So we had a kind of a three-man deal.

Hughes: It sounds like a wonderful time.

Thygeson: Yes, it was a good team. We also had a good teaching program, because we taught the ophthalmology residents too. Later on we talked only to the fellows, but in those days there were nine residents and we taught them external disease.

Hughes: That would have been during the period of Dr. Cordes, which implies that he was sympathetic to infectious disease as part of the education of an ophthalmologist.

Thygeson: Oh, yes, he was very broad-minded in regard to all the specialties. He didn't do anything himself except a little pathology, but he was very sympathetic to research. He wanted research to be done, the same way Dr. Wheeler did in New York.

Hughes: Was Dr. Hogan also sympathetic when he became chairman of the department of ophthalmology?

Thygeson: No, he wasn't sympathetic because he was interested in anatomy, histology, and pathology, but not in infectious disease. So the mission of the Proctor Foundation was not in his field at all.

Hughes: Was he responsible for cutting out the infectious diseases section of the residency training program?

Thygeson: Well, it may have happened during his time as chairman, but he was not sympathetic at that time. He became rather hostile to the development of the Proctor Foundation later on. He was a bad choice for director because he was not in sympathy with the Proctor mission, but Dr. Cordes wanted him to be director, and he'd worked with me for six months in New York on infectious disease and six months on pathology. He really chose pathology. During his time as Proctor director he did not interfere with my work, so that I was able to carry on the mission. But looking back, he was a bad choice for director.

Hughes: Do you think that some of that tension was a rivalry between the Proctor Foundation and the department of ophthalmology?

Thygeson: Yes, in a way, because Dr. Hogan had no sympathy for the mission, and he wanted to make the Proctor Foundation the laboratory of the department, which was not Mrs. Proctor's idea at all. So I had to fight that all the time, and that did create a little mild tension, but nothing serious.

The Board of Governors

Hughes: Dr. Hogan also was on the board of directors of the Proctor Foundation.

Thygeson: Yes, but he wasn't obstructing. He wasn't creative, but he didn't obstruct. He usually went along with any ideas I had. I think he kind of gave up on the idea that the Proctor Foundation would be the laboratory of the department. I think he was resigned that as long as I was living he couldn't do that.

Hughes: Who at the time was the third member of the board of directors?

Thygeson: The dean of the medical school, Francis Scott Smyth. He was absolutely fabulous because he sympathized with the mission of the Proctor Foundation. He was supportive in every way. He was the best board of governors' member we ever had. Saunders was the next best. And Dr. Cordes was very good, too. We had three good ones, but they've never been as good since.

Hughes: You say not as good in the sense that they weren't sympathetic to the mission of the Proctor?

Thygeson: That's right. One of them wanted to change over to optics, and another one wanted to change to physiology, and so on. They didn't sympathize with the mission. They were necessarily on the board of governors, so I had to really fight sometimes to maintain the mission, but we did, successfully.

Hughes: It was an unusual situation, was it not, to have a member, or perhaps two members, of a board of directors who were not necessarily sympathetic to the mission of the foundation they were directing?

Thygeson: They had to be on the board of governors, but if I hadn't been there the Proctor Foundation would have been completely absorbed by the department.

Hughes: Is this format typical of other medical institutions?

Thygeson: I don't think they need to do that, but it often happens that way. Anyway, as I say, we have been successful in maintaining the mission. We had to fight to do it, but we were able to maintain the mission. The mission is still strong.

But even the present board of governors is not sympathetic, because the chairman of the board of governors, a Ph.D., is interested in molecular microbiology and has no interest at all in clinical microbiology, which is the main purpose of the Proctor Foundation. So I have to maintain the mission.

Hughes: Is the other member of the board, Steven Kramer, also attempting to change the mission of the Proctor Foundation?

Thygeson: The rest of the board would change it if I weren't there, but I think they've kind of given up on any idea of changing it as long as I'm alive.

Hughes: *The present Proctor director, Dr. Dawson, is very much an infectious disease man.*

Thygeson: He's properly placed. Dr. O'Connor was not properly placed because he was interested in uveitis, which was not part of the mission. But he was sympathetic to the other fields, so it worked out pretty well. The only thing that went wrong with his tenure, besides financing, was the fact that too many of the fellows we got were oriented to uveitis, which was not a part of the Proctor mission. But part of uveitis is infectious, so we could still fit it in, but not the way we intended it to be.

The Electron Microscopy Laboratory

Hughes: *We spoke of the electron microscopy lab, which was completed in 1959. Was electron microscopy a new field for ophthalmology?*

Thygeson: Well, scattered around the country there were two or three areas in ophthalmology that were using it, but our electron microscope laboratory came by way of Dr. Hogan. He originated it and obtained NIH support. So he was responsible for that. That was his main interest at the time. He wrote a book on electron microscope histology of the eye.

The electron microscope laboratory has been very successful, and it's still going strong now; one of the best laboratories we have.

Hughes: *Did he actually do the electron microscopy?*

Thygeson: Yes. But he had help, of course. He had two technicians that went on to great things. One was Lynette Feeney. She's back in Missouri; she's very successful.

The other was Jorge Alvarado, a strong member of the department of ophthalmology. He takes care of the electron microscopes for the department; they have their own lab setup. He's from Costa Rica. He's had a very successful career, with some innovation, too, some original work that's been very valuable. So there are two graduates from the laboratory that have been very successful.

Hughes: *Did you ever take advantage of the electron microscopy lab in connection with your work on infectious disease?*

Thygeson: Well, I dabbled with it, and I went down and learned how to use the microscope and so on, but I never really carried through. However, Dawson has, and he's had a very successful EM experience on infectious disease, particularly on trachoma and herpes. He had a fellowship for a year of training on the electron microscope in England, so he became very adept at using the microscope and still is using it.

The Virus Laboratory

Hughes: Another laboratory, the virus lab, was established in 1967. What was the impetus?

Thygeson: The impetus was the work we started with the department of microbiology, particularly on herpes. We decided that it would be nice to have our own viral laboratory and not be entirely dependent on the department. So we were able to interview and get Dr. Jang Oh, who is an M.D./Ph.D., South Korean, who had very good training at the University of British Columbia and Washington and was very much interested in the eye.

Hughes: A microbiologist?

Thygeson: Yes, a microbiologist and pathologist. He's been very innovative. As the association with the department dropped out, our own viral laboratory went ahead. So Dr. Oh is still working very strongly with a very good laboratory. He and Dr. Dawson are working particularly on herpes. Dr. Dawson's work is now particularly chlamydia and herpes.

Hughes: Did the fact that there was now a viral lab mean that the viral disease aspect of the infectious diseases laboratory dropped out and was transferred? Hadn't you been working with viral diseases before the virus lab was founded?

Thygeson: Oh, yes, but [after the lab opened] we weren't dependent on the department of microbiology. All the specimens were funnelled to the department of microbiology, and then later on they were funnelled to Dr. Oh. So Dr. Oh took over.

Hughes: There were no specimens coming in from the department of ophthalmology?

Thygeson: Well, yes, there was a certain amount, but the specimens were mainly research specimens, animal and that type.

Hughes: Not clinical

Thygeson: It really was not designed as a clinical viral laboratory, although it did some, but it was mainly a research laboratory.

Hughes: What laboratory was handling the clinical material?

Thygeson: It was all Dr. Jawetz's laboratory in the department of microbiology, and Lavelle Hanna was the principal technician who did most of the laboratory work. She was kind of a chief technician and always had one or two helpers.

At one time we had a very active research program there, both on chlamydia and herpes, but all specimens went to the department of microbiology, not to the Proctor lab. Mas Okumoto was primarily interested and worked on bacterial and fungal infections. In fungal infections we had a very good deal, because I had been trained in fungal disease, and Mas had good training, and then we had a wonderful backup in the department of microbiology by Dr.

Carlyn Halde. She's still active there—a woman mycologist who was a great help, because any specimens that we were having trouble with she took over. A very good friend of Proctor Foundation. *

Hughes: Before the Proctor Foundation existed, what did the department of ophthalmology do with clinical material from cases of infectious diseases?

Thygeson: It went to the microbiology department.

Hughes: So it was a given that microbiology would handle anything to do with microbiology?

The Pathology Laboratory

Thygeson: Pathology was handled by the ophthalmology department, because a little laboratory of pathology was established by Dr. Cordes initially and then followed by Dr. Hogan.

Hughes: What was the impetus?

Thygeson: That was Dr. Cordes, who was interested in pathology.

Hughes: He wasn't satisfied to have the work done by the department of pathology?

Thygeson: No. In the early days pathology was the one research field that was really supported in ophthalmology. All the earlier researchers were primarily in pathology. That was the big field. Microbiology was pretty weak in the early days.

Hughes: Wouldn't you say that the Viennese ophthalmologists were mainly pathologists?

Thygeson: Yes, and Dr. Cordes went over to Vienna, and he had pathologic training under Ernst Fuchs, the great Viennese ophthalmologist. So Dr. Cordes was well trained in pathology, and he got Dr. Hogan.

Hughes: Another laboratory, much more recent, is the Elizabeth C. Proctor Memorial Research Laboratory, which was established in 1978.

Thygeson: That is in my old laboratory, the infectious disease laboratory, now in the Proctor Building at 95 Kirkham Street. I wanted to hold on to that laboratory for several reasons. One was that Mrs. Proctor helped design it, and when we built the laboratory, she looked it over, and she had a lot of comments. She had a little training in microbiology so her comments were very pertinent.

Hughes: This was on her one visit to the Proctor?

* Note added by Dr. Thygeson in editing: Dr. Halde gave the first Mas Okumoto Lecture in September, 1987, at a meeting of the Alta California Eye Research Foundation.

Thygeson: No, this was by mail and telephone. The plans were submitted to her, and she modified them. She also designed the library of the Proctor Foundation, room 318 in the Medical Sciences Building. It's a very successful design, I think—unusual design.

The Career Development Program

Thygeson: The laboratory developed a purpose, which was the training of young researchers. Can I tell you a story of how that started?

Hughes: Yes.

Thygeson: The Howe Laboratory at Harvard was an early laboratory of considerable success. After the war, the director, Dr. David Cogan, came up to Los Alamos to work. He was interested in atomic energy and the eye changes it causes—cataract and everything. So he visited Mrs. Proctor, and he sold her on the idea of a five-year program for developing young researchers—career development, in other words. The NIH has had a five-year career development program. She brought that up to me and to the Proctor Foundation, and I said, "Well, it's a great idea, but we haven't any money." And she said, "When I die you can sell my place here and use that money for career development," which is actually what we did. So the laboratory was set up for the purpose of career development, and the first man in was Mitch Friedlaender. His salary was paid from the sale of Mrs. Proctor's land and house, which was \$800,000 at that time, plus some NIH support.

Hughes: Was this the beginning of the fellows program?

Thygeson: No, it was the beginning of the career development program.

Hughes: Which is separate?

Thygeson: Yes, and that's based on the Elizabeth C. Proctor Fund from the sale of her house and land in Santa Fe. The first appointment went to Dr. Mitchell Friedlaender, with us for six or seven years, who is now at the Scripps Clinic in La Jolla.

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Thygeson: That was the purpose: We were going to train them for five years and then let them go on to other laboratories.

Hughes: Are these M.D. ophthalmologists?

Thygeson: Yes. They're ophthalmologists who have a fellowship which is extended to five years.

Hughes: How are they selected?

Thygeson: By a committee composed of the director and a couple others, usually three men.

Hughes: *The income from the sale of the property is sufficient to support the program?*

Thygeson: Well, we've had trouble. Because of the drop-off of NIH funds, we had to use the income from the \$800,000 to carry on the regular purposes of the Proctor Foundation, so we're in limbo right now.

Hughes: *So the program doesn't exist at the moment?*

Thygeson: It exists on paper, but we don't have anybody in that laboratory. Actually, Dr. Friedlaender is carrying on laboratory research that he's started. He comes up once a month from La Jolla to supervise it. But eventually, when times improve, that will go back to its purpose as a career development program.

Hughes: *Does it have to be in infectious disease?*

Thygeson: Well, it's logical because it's a memorial lab for Mrs. Proctor, so it has to be in the mission of the foundation. So as long as I've alive, of course, I push for infectious disease.

The Heintz Laboratory

Hughes: *Then there's the Ralph M. and Sophie K. Heintz Laboratory which was established in 1980.*

Thygeson: Yes. That was set up by a gift from Sophie Heintz. It's still kind of a token laboratory because the amount of money is insufficient. It's about \$175,000 now, and, of course, the income from that is only about five or six thousand a year, so it has kind of token support. But at Mrs. Heintz's death, then the income from her estate will be earmarked first for the Cecilia Vaughan Fellowship Program and second for the Heintz Laboratory.

Hughes: *What is the purpose of the laboratory?*

Thygeson: It's the mission of the foundation. It doesn't have the same push that the Elizabeth C. Proctor Laboratory has. It could be in immunology; it wouldn't have to necessarily be only in infectious disease. But the way we have it programmed now is that there will be two or three candidates considered, and then there'll be a choice, and there'll be a career development program. The chief—can't use the word director because it competes with the director of the foundation—the chief of the laboratory will be an ophthalmologist who is trained in basic sciences and in clinical ophthalmology. He would be much better trained, for example, than I was. See, I was really self-taught in the basic sciences.

The new chief of the Heintz Laboratory will have a real basic science background—probably an M.D./Ph.D. type—and then he'll have the main responsibility for the Cecilia Vaughan fellows, which will be a big program because there'll be plenty of money to handle that. But that all depends on the Heintz estate.

Hughes: At the moment then there's no physical laboratory?

Thygeson: It's a physical laboratory; it's Dr. O'Connor's old laboratory. It has a plaque; it's named the Heintz Laboratory, and it's been accepted by the Regents. So it's a named laboratory, but it's not functioning; it has no chief at the moment. It's being used for fellows. The eventual use will come through the Heintz estate.

Hughes: All through these interviews, you've emphasized the importance of the basic sciences to clinical ophthalmology. Do you see the Proctor as a basic science research institute with applications always to clinical medicine?

Thygeson: Well, I think it worked the other way. The original concept was that it would be run by ophthalmologists who would utilize the work of basic science departments in the university but not actually be productive in the basic sciences. Dabbling in basic sciences came later. The whole purpose was to utilize the basic science departments of the university, and that still is. You have to do that because Proctor hasn't got the money to develop in the basic sciences the way we had hoped to do. That's because of a fall-off in NIH funds.

Hughes: Your dream was really to have all the ancillary sciences within the Proctor Foundation?

Thygeson: Well, that was kind of a pipe dream because it was so dependent on NIH or on development of more private funds. Really the only big private funds that came through were the Heintz funds, and they're still not available. The university owns the Heintz gift of eighty-three acres down in Los Gatos, but there's no income from that. It's only at Mrs. Heintz's death and the sale of the property that the finances will come in. So that's all in the future.

Directors

Hughes: Turning to the administration of the Proctor Foundation, would you say something in a summary way about the nature of the research program under the four directors—Hogan, Thygeson, O'Connor, and Dawson. I'm thinking of the change in research emphasis which occurred under each director.

Thygeson: With Dr. Hogan first, the change developed right away because of his interest in pathology and electron microscopy and so on. That was unexpected; not in the original program. Then when I came in we went back to the original idea with infectious diseases the main focus. When Dr. O'Connor came in the

emphasis on uveitis picked up in a big way. Then with Dr. Dawson it came back to the original emphasis on infectious diseases, number one, but he's also sympathetic to uveitis.

Hughes: How much do you think the choice of Dr. Dawson as director had to do with his interest in infectious disease?

Thygeson: A great deal. That was really the deciding interest. The board of governors, not being interested in infectious disease, interviewed quite a number of others who were not interested in infectious disease, and I was able to stymie that.

Hughes: It takes a unanimous vote for the director?

Thygeson: Two out of three. Anyway, a search committee was set up, of which Dr. Lloyd Kozloff was the chairman, and we interviewed about twenty candidates. Dr. Dawson was the logical one to me because of his interest in the mission, but there were others who, if there hadn't been a mission, would probably have been much better because of fund-raising ability and personality. Dr. Dawson has a difficult personality, so he makes a lot of people mad, you see. His interest in the mission overcomes that, but he'll never raise money because he hasn't got the personality for that. Somebody else is going to have to raise the money.

Hughes: How did you feel about Dr. O'Connor's interest in uveitis, which was not directly part of the Proctor mission?

Thygeson: I liked very much what he did in uveitis because he really changed the whole concept of the etiology of uveitis, which was really in a mess when he came in. He really straightened things out and did a lot of very good research. But what I didn't like was the change in the fellowship program, bringing in the uveitis interest and cutting down on the interest in other infectious diseases.

Hughes: Could you comment upon your relationship with your assistant director, Dr. O'Connor, when you were director? Would you delegate?

Thygeson: Yes. Dr. O'Connor was really the perfect assistant director and then associate director. I could rely on him. I did an awful lot of travelling; I worked for the World Health Organization, the Indian Service, so I was away a lot. And I could really rely on Dr. O'Connor to keep the ball going.

Then I had a wonderful administrative assistant, who was Mrs. Rose Dow. She was superb; there's never been anybody like her. I had two strikes in my favor: I had a wonderful associate director, and I had a marvelous administrative assistant, so I could go away and have everything work fine. There's never been anything before or since like that.

Hughes: Is there always an assistant and an associate director?

Thygeson: Always one or the other. There have never been two.

Hughes: Is there any assumption that the associate director is in line for the directorship?

Thygeson: Well, it's a logical assumption. It couldn't be now because Dr. Oh is not clinical, and he could never be a director. Dr. Dawson was in line, and Dr. O'Connor was in line, but Dr. O'Connor became ill in his last years with ulcerative colitis. He travelled so extensively, and he lectured, so that he was hardly ever home the last few years, and the administration of the Proctor just took a nose dive.

Hughes: Was Dr. Dawson the associate director at that time?

Thygeson: Yes, but Dr. O'Connor wouldn't delegate to him at all. He'd delegate to his administrative assistant, who was no good; had to be let go. He had two of them, actually, who were inferior. So everything was in really bad shape. The grants-in-aid program, the NIH program, were neglected, and everything went wrong.

Hughes: An unfortunate period.

Thygeson: Very bad for the Proctor Foundation.

Hughes: How do you look upon yourself as an administrator? Do you think you were good at it?

Thygeson: Well, some people thought I was good at it. I didn't think so, but I had such good backup support that I overcame my own deficiencies in that. But I wasn't a born administrator, and I didn't like administration.

Hughes: Did you feel that you kept your finger in all the pies even though you were travelling around?

Thygeson: Yes, I think I worked hard at it, and I think I was very successful, especially in keeping the staff happy. I always had my door open so any member of the staff could come in and complain about anything he wanted to. Then we had a lot of social activity here, so we all got to be a big family. That hasn't happened since—or before. In that way I was successful.

Hughes: How do you judge yourself as a fund raiser?

Thygeson: Not a natural fund raiser, but the Proctor Foundation depended on the funds that I did raise. So the Proctor endowment was what I raised, and it's up to over thirteen million now. I couldn't go out and just raise money the way some people have been able to.

Hughes: It wasn't something that you felt comfortable with?

Thygeson: I didn't have the personality to really want to ask somebody for funds.

Dr. Vaughan is a natural fund raiser. He really doesn't have to raise them; the funds come to him. He was responsible for the Heintz fund, and he didn't ask for that; the Heintzes just gave the money to him. One of my old patients just left \$400,000 to Dr. Vaughan's little group down in San Jose for research. Then another of my patients, Miss [Margaret] Wibel, used to give a thousand or more every year, and she was going to leave money in her will to the

Proctor Foundation. But then she got mad because she gave a thousand dollars to the Proctor Foundation and the check was lost. Nothing was said about it—I didn't know about it; Dr. Vaughan didn't know about it—until she complained that she'd never gotten a letter or anything. So she had to write another check, and she never gave another nickel. When she died her estate was in the millions. That was bad administration on the part of Dr. O'Connor.

Hughes: Are there high points of your directorship that we haven't touched upon?

Thygeson: The high point in the directorship, I think, was to get in 1960 a consolidated NIH grant that took in all the activities under one grant, which was easy to administer; much better than the individual grants. That was the high point of my funding, so we had plenty of money at one time.

Hughes: Why did you decide to step down in 1970?

Thygeson: I reached retirement age. I was offered a chance to carry on, but I turned it down.

Hughes: Do you think we've covered the Proctor well enough?

The Future

Thygeson: Well, I might say that the future of the foundation will depend first on the Heintz estate, which will give plenty of money for the fellowship program and plenty of money for the Heintz Laboratory and some to spill over on the general purposes of the foundation. But it will mean that the Heintz Laboratory will come into function and probably could be the best-supported ophthalmology laboratory in the country. So it all depends on whom you get for the chief. If you get somebody who's really good, it could really remake Proctor Foundation.

Hughes: That decision is in the hands of the board of governors, or will there be a search committee?

Thygeson: Well, we're talking about that now. The last I heard was that Dr. Kozloff wanted a search committee for this, which I don't want. What I want is to select, say, three candidates and see how they perform for a year or two, then not name the chief until they're thoroughly proven. I don't want a search committee to go out and choose a chief because my experience with search committees is terrible. [laughs] I think the search committee is a bad idea.

Hughes: Why do you say that?

Thygeson: They have so many different opinions, by people who really don't understand the mission. I think that's the main thing—the search committee doesn't understand the mission. We ran into that precisely with Dr. Dawson. If I hadn't been there to really throw my weight around we would have come up with a director who had no interest in the mission at all.

Hughes: The way you would like to see it happen, the final decision would be in the hands of the board of governors after this year of trial?

Thygeson: Yes. But it looks as though there's so much red tape now in the university that it's almost inevitable that a search committee will be formed. I'd like that only to occur after there's been a good trial of one or two or three candidates.

Hughes: Do you have any candidates in mind?

Thygeson: Yes, we do. We have a wonderful group of fellows right now, and I can name three right out of our current group that would be candidates.

Hughes: Have you made these choices known to the search committee?

Thygeson: No. See, all we have at the moment is thirty thousand dollars for an extended fellowship for a candidate. We need much more than that to have an extended fellowship, but I think we're going to use that thirty thousand. Mrs. Heintz gave me that thirty thousand just for that purpose. Everything depends on how we handle the Heintz Laboratory, because it could be the best laboratory in the country in infectious disease. The future is good, but in the present it's pretty sad. We have a wonderful group of fellows right now.

We have had some wonderful summer student fellows, many of whom have gone into ophthalmology. One is chief at the University of Florida in Gainesville. Bill Spencer was a summer fellow for two years when he was a student, and Bill has made a wonderful success in ophthalmology.

Hughes: Is the fellows selection process pretty good?

Thygeson: Dr. Dawson, I think, was mainly responsible for selecting the present group. I wasn't on the selection committee, but they did a very good job, and I think we've got a wonderful group right now. I don't think you could do that every year.

Hughes: Anything else you'd care to say about the future of the Proctor Foundation?

Thygeson: I think everything depends on the mission, and, in fact, in my book all special foundations have to have a mission to survive. If they don't have a mission, they get absorbed or cast out, and you can name foundations that have gone that way. The Howe Laboratory was probably the worst example. It didn't have a separate mission, and it's just been absorbed. And the Knapp Foundation is nonexistent; just couldn't go on. That's happened to so many foundations that don't have a real mission. The Proctor Foundation has a mission that could last two hundred years, so I think the hope of the Proctor is its mission.

Preventive Ophthalmology

Thygeson: One of the new features of the mission is preventive ophthalmology.

Hughes: Which has always been an interest of yours.

Thygeson: Yes, it's always been.

Hughes: Are you then largely responsible for the reemergence of that concept?

Thygeson: Well, it may be so. We have a book in process on preventive ophthalmology.* Prevention of blindness has always been a thing, but preventive ophthalmology is rather new, to prevent eye disease that does not lead to blindness. So we're pushing that.

Hughes: Who is writing the book?

Thygeson: It's a group, mostly all Proctor fellows, and Mitch Friedlaender is the editor.

Hughes: You are contributing?

Thygeson: I'm contributing, and Dr. Vaughan is contributing.

Hughes: A different author will write each chapter?

Thygeson: Yes. Well, one author might write two chapters. Dr. Khalid Tabbara is writing a chapter; he may write two. Dr. Oh has a chapter on the future of vaccination as prevention, and Mas Okumoto and Dr. Halde have written a chapter on the prevention of fungal disease.

Hughes: And you?

Thygeson: I write the introduction, and I write a chapter with Dr. Cello in Davis on the prevention of zoonotic eye disease.

Hughes: Do you have a publisher?

Thygeson: We have a publisher who wants too much money, so we're looking around for a publisher. We have been quite successful in the past in getting publishers who will actually give some money, so we've made some money on publications. But this one's a pretty large deal that doesn't have too much appeal, so we may have to pay. We had another one on trachoma that we had to pay \$15,000, which was half the publishing cost.**

Hughes: Is that fairly standard in medical publishing these days?

Thygeson: Well, it varies. See, certain subjects—like AIDS, for example—make money, because everybody wants to buy the book. But preventive ophthalmology does not have appeal; I don't think we'd sell too many books.

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* MH Friedlander, ed. *Prevention of Eye Disease*. New York: Mary Ann Liebert, Inc., 1988.

** *Conference on Trachoma and Allied Diseases*. Sponsored by Francis I. Proctor Foundation for Research in Ophthalmology and Harvard School of Public Health. Menasha, Wisconsin: The Ophthalmic Publishing Co., 1967.

Thygeson: There is now much interest in preventive medicine, not necessarily preventive ophthalmology. Dr. Vaughan wrote the first chapter on preventive ophthalmology in any textbook.* I think there's plenty of interest in preventive medicine now, but it could be better, of course. The American Medical Association called the last decade a decade of preventive medicine.

Hughes: *Some of that probably is in response to the escalating cost of medicine.*

Thygeson: It's so much easier to prevent a disease than to pay for it. For example, in ophthalmology, if you prevent one corneal ulcer, you could save thousands of dollars, with the cost of hospitalization and everything. It's not necessarily the prevention of blindness; it's the prevention of eye disease which is very costly in actual cash and in taking people away from industry. About thirty percent of eye disease in the United States is preventable, and about sixty percent in the third world is preventable. So a lot can be done in terms of money and disability.

Hughes: *How sympathetic is NIH to preventive medicine?*

Thygeson: I've kind of lost touch with NIH, so I don't know. We're looking around to see about that. But I don't think our book will have a great sale.

The Proctor Fellows Program

Hughes: *The fellows program was founded in 1948. Perhaps you could tell me about the conceptual development.*

Thygeson: Well, there actually was no concept of a fellowship program in the early days. It came about spontaneously. The first fellow was a Turk, Necdet Sezer, who became the director of an eye institute in Istanbul. His interest was trachoma, so trachoma was the start of the fellowship program. Then it just naturally worked out from there.

Hughes: *1948 is when the Turk came?*

Thygeson: Yes, I think so. He was here for about a year.

Hughes: *We've had over one hundred and fifty fellows. The program was really set up for six fellows a year, but it varies yearly from five to eight. It started as a one-year deal, and it's been gradually extended to longer periods.*

Funding

Hughes: *Could you say something about the funding of the fellows program?*

* D Vaughn. Preventive ophthalmology. In: *General Ophthalmology*. D Vaughan, T Asbury, eds. Los Altos, California: Lange Medical Publications, 1986; 362-6.

Thygeson: Well, the funding of the fellowship program has always been a difficult one because Proctor had no funds of its own for a fellowship program. Of course, it will have with the Heintz money, so that'll be a tremendous advance. But we've been dependent on various sources, mainly NIH. NIH had a very good fellowship program. But there have been other sources, some private and some from other foundations. Really it's kind of a difficult program to get the funding going. For example, one of our fellows here is this year supported by the northern California chapter of the National Society for the Prevention of Blindness. That was due to Dr. Vaughan and Dr. Spencer, who were on the committee here. So Dr. Spencer and Dr. Vaughan enabled us to have this one fellow, who is very good.

Hughes: *The Cecilia Vaughan Memorial Fund is also targeted on the fellows program, is it not?*

Thygeson: That's entirely targeted on the fellowship program and mainly on the foreign fellows. Of course, when the Heintz money becomes available, that'll really revolutionize the fellowship program. It's hard to get an NIH fellowship, although we do have it, but it's hard to get.

Foreign Fellows

Hughes: *And its particularly hard to get an NIH fellowship for a foreigner, is that not true?*

Thygeson: Impossible for a foreigner. But now the Canadians have a fellowship fund, from the Medical Research Council of Canada, the name I've forgotten, which has enabled quite a number of Canadian fellows to come. One of them we have this year. Then the Japanese have had a partial fellowship support, and we had one fellow, Dr. [Hiroshi] Hatano, who was half supported by Japanese funds and half by the Cecilia Vaughan fund.

It's been a struggle to support these fellows, especially since it costs about thirty thousand a year now to support a fellow. It used to be twelve thousand. Costs have gone up so much that it's very difficult.

Hughes: *Is one of the principal purposes of the foreign fellow program to train individuals to go back to practice and research in their country?*

Thygeson: Yes, and preventive ophthalmology has been the main target. We don't expect the foreign fellows to do too well in research on the basic science side because they don't have the laboratory facilities. However, what they do is on the clinical—preventive ophthalmology. This is true except for the Japanese. The Japanese have been wonderful in providing laboratory facilities for returning fellows. We've had some wonderful Japanese fellows who have contributed a lot in basic science. But not the other fellows.

Hughes: *Are you thinking mainly of microbiology?*

Thygeson: Microbiology, immunology. With Dr. O'Connor the emphasis was on immunology.

Structure

Hughes: How are the fellows selected?

Thygeson: There's a committee, where the chairman is the director of the Proctor Foundation, and two or three staff members appointed by the director.

Hughes: So the format of the committee changes from year to year?

Thygeson: Yes.

Hughes: Do fellows come with a project in mind?

Thygeson: Some come with that in mind, and others select their subject in the first three months of their fellowship.

Hughes: So even if there's not a particular research emphasis, they still have a project?

Thygeson: Oh, yes, everybody has a project.

Hughes: Can you tell me how a typical day might go for a fellow?

Thygeson: His big time is spent on his research project, and then he has a series of lectures and seminars to attend, which come at odd hours. Thursday has always been a day for teaching, so all day Thursday is devoted to seminars and so on. On other days the noon hour is used for teaching.

Hughes: You're speaking of the seminars that you have given for so many years?

Thygeson: My seminars always came on Thursday. Currently it comes at ten thirty to twelve, but formerly it used to come in the afternoon.*

Hughes: The fellows are expected to attend all these seminars?

Thygeson: It's optional. It's a little bit of a fight to maintain interest, because there are other programs in the university that they may want to attend. There are a lot of seminars going on all the time, so there's competition.

Hughes: Are the fellows expected to present a paper at any time in their tenure?

Thygeson: Oh, yes, two or three times a year they have to prepare a paper—not in my seminar, but in the other seminars going on. There are special seminars having to do with the cornea, and the conjunctiva, and the uveal tract, and the retina, and so on. All that is covered every year, repeated every year.

* Dr. and Mrs. Thygeson stopped making their weekly trip on Thursdays to the Proctor Foundation in the fall of 1987.

Hughes: *I saw a list of the various seminars that are given,* and there was no day that went by when there wasn't one or two seminars on some aspect of ophthalmology.*

Thygeson: Yes. Lunchtime is often used, so they usually have a brown bag lunch and a seminar at the same time.

Hughes: *Has the department of ophthalmology always welcomed the Proctor fellows to participate in its activities?*

Thygeson: Always, except in the clinical, especially surgery. The Proctor fellows have no access to any departmental surgery.

Hughes: *Why is that?*

Thygeson: The legal tangle is the main thing, and also there's such a competition for surgical training that departmental residents always have first priority. If Proctor fellows come in to surgery, they come in as assistants. So surgery has never been a big feature of the Proctor fellowship program. When a fellow is particularly interested, then we farm him out to some particular corneal surgeon. For example, a girl from Puerto Rico, Carmen Santos, was interested in corneal surgery, so she was farmed out with a former fellow, who's over in Oakland, who does an awful lot of corneal surgery. So she spent two or three months with him.

Hughes: *A foreign fellow would be prevented by federal laws from actually performing surgery?*

Thygeson: Oh, yes, but they can assist and make clinical observations.

Benefits

Hughes: *Is there anything else you'd care to say about the fellows program?*

Thygeson: Well, I think the fellows program has been really one of the big mainstays of the Proctor Foundation. It's given an international flavor to the Proctor Foundation, which is not present in the department of ophthalmology at all. So the Proctor Foundation has become known internationally in a way that the department has not. The international program has been very successful. They have a little club in Japan.

Hughes: *Dr. Thygeson is showing me a plaque which says, "With fond memories and appreciation," and then there's a series of names, and at the bottom it says, "UC Eye Society in Japan, June 8, 1984." [tape interruption]*

Thygeson: I'd like to make one statement about the foreign fellows, particularly from Asia, and especially about the two Chinese women whom we had as fellows. One was a middle-aged woman, Shao Chen Li, who stayed for one year. We

* The fellows's work week is outlined in: *Francis I. Proctor Foundation for Research in Ophthalmology. Five Year Progress Report, 1975-1980, 56-8.*

didn't do too well with her because of her language difficulty. Then we had a young woman, Wenhua Zhang, who stayed two years, and she really learned English, and she did some remarkably good work on herpes.

Hughes: Did she come with experience in virology?

Thygeson: No, she started from scratch, and she developed clinically and in virology very well. She's back in Peiping at an institute of ophthalmology whose director was an old friend, Dr. Chang, who was a part of the team that made the first cultivation of the trachoma agent.

Hughes: Is that the connection?

Thygeson: That's one of the connections. I think the important thing is that the fellowship program was so good for Proctor because it gave international flavor, and we got so much from the individual fellows that we couldn't have gotten from just domestic fellows.

Mrs.

Thygeson: Are you talking about Wenhua Zhang?

Thygeson: Yes.

Mrs.

Thygeson: She was adorable.

Thygeson: The Japanese and the Canadian fellows were the most productive scientifically, but the other fellows had a very good influence. The reason the others were not so productive is that when they went home they didn't have any facilities for laboratory work. So our clinical teaching was more important for them than the laboratory.

Hughes: That emphasis was understood when they came?

Thygeson: Yes, and we accentuated the preventive medicine. I think, looking back, we did pretty well on the overall international portion.

Hughes: You say you've had over a hundred fellows.

Thygeson: I think we had close to one hundred fifty.

Mrs.

Thygeson: I think it's even more than that.

Hughes: Have they all gone home and continued one way or another in ophthalmology?

Thygeson: Not all, but the majority have.

Mrs.

Thygeson: About eighty-five percent.

Thygeson: Every year we have a fellows program, so that's been quite successful. With the international aspect we get material that we couldn't possibly get on the domestic side.

Hughes: *You're talking about research material?*

Thygeson: Yes, research material on diseases that no longer exist in the United States but are still present in the third world. Trachoma now has disappeared from the United States, so we're dependent on international sources for trachoma chlamydia. We have plenty of chlamydia, but it's not the trachoma type.

Hughes: *You said, perhaps off tape, that the fellows training program was probably the most important contribution of the Proctor Foundation.*

Thygeson: Yes, I think that's a fair statement.

The Cecilia Vaughan Fellowship Program

Mrs.

Thygeson: Have you covered Danny's Cecilia Vaughan?

Thygeson: I should say some more. The Cecilia Vaughan Fellowship Program was established after the death of Cecilia Vaughan, who was a very promising young woman killed in an auto accident. The Vaughan family friends made the first contributions, and this fellowship paid for a number of foreign fellows, full or partial support. When the Heintz estate comes through there'll be plenty of money for the Cecilia Vaughan Fellowship Program, which will probably be the most important fellowship program. It's less important now because the numbers are relatively small, but eventually it will be the major fellowship program. It's got a good history, and next September [1987] we're going to have a seminar devoted just to the Cecilia Vaughan fellows, up at Lake Tahoe.

The Association of Proctor Fellows

Hughes: *My next question concerns the Association of Proctor Fellows. Are they not the people who come to Tahoe on an annual basis?*

Thygeson: No, the Tahoe seminar in the past has been just the current fellows. The seminar's been too small. But every year the Proctor fellows association has a three-day meeting devoted mainly to unusual cases and unusual methods of handling cases, mostly clinical, with a smattering of basic science material.

Hughes: *The fellows themselves present the program?*

Thygeson: Yes, it's all done by the fellows.

Hughes: *What is the purpose of the Association of Proctor Fellows?*

Thygeson: The purpose is to maintain connection with the Proctor Foundation and also to support cooperative research between the fellows. There's quite a number of projects that can only be done by cooperation. There's sometimes not enough clinical material to have only one person do the research. It's rare to have, for instance, cat scratch disease involving the eye, so you can only really do studies if you have cooperation and collect enough cases.

Hughes: *Has this cooperation already occurred?*

Thygeson: It's under way. Especially in uveitis there are a number of cooperative studies. One on Behcet's disease and other types of uveitis. So the cooperation has been pretty good. Then the fellows are in communication by telephone or letter, and Mas is particularly in demand by the fellows because he knows so much about microbiology. Dr. O'Connor was very much in demand by former fellows on the uveitis cases.

The Alta California Eye Research Foundation

Hughes: *I read of the Alta California Eye Research Foundation.*

Thygeson: Yes, that's an important part of the Proctor program. I started that on a rather false assumption, but in a way it's worked out. My idea was that we should have a foundation which didn't have to follow the university rules and from which we could supplement salaries that were probably too low at the university standard by giving stipends for special reviews or seminars and so on.

It turned out it wasn't necessary because we had another technique. If we had a favorable dean, we could have an accelerated advancement. For instance, we couldn't get a good salary for Dr. Oh at first, but we obtained it by an accelerated promotion. So that technique kind of took the steam out of the Alta thing, but Alta turned out to be very valuable for support of lectures and books and other things like that.

Hughes: *Can you tap Alta for your book on preventive ophthalmology?*

Thygeson: Yes, but we haven't done any fund-raising at all, so really all the money we have is Mrs. Proctor's original endowment, which has grown, but we're thinking about having a fund raising program to enlarge it. We've turned over our house here in Los Altos to Alta.

Hughes: *Isn't there a connection with your house at Tahoe as well?*

Thygeson: Only in the sense that we use it for seminars. Every September we have an Alta seminar on a different subject each year, which is produced by the staff and the current fellows. We had one on preventive ophthalmology, from which we're making the book. But books came out of a number of the seminars—on immunology, on herpes.

Hughes: *This seminar is just for Proctor people?*

Thygeson: Only for Proctor.

Hughes: *Have we said enough about seminars sponsored by the Proctor Foundation?*

Thygeson: I think the first seminar in which virologists and ophthalmologists got together was at the Proctor Foundation, and a very good book came out of that. Proctor Foundation sponsored two seminars on trachoma, and books came out of each one.

Hughes: *Did those seminars pull from outside the university?*

Thygeson: Yes, those had Proctor and invited guests. We've had several international seminars or meetings, one on ocular immunology that Dr. O'Connor sponsored; one on herpes that Dr. Dawson and Dr. Oh sponsored; and there's another one on chlamydia that's coming out this year [1987]. There are others that have been sponsored by the Proctor Foundation, usually with help from Alta.

Hughes: *Does a publication usually emerge from these?*

Thygeson: About half of the seminars have had publications.

As of last year we have a Ruth Lee Thygeson fund, which is designed for the teaching of medical writing. That's just getting started. We've bought books on medical writing, and we're in the process of inviting an editor to talk to the group. The purpose is to stimulate better medical writing. [tape interruption]

Hughes: *What about the Elizabeth C. Proctor Research Professorship?*

The Elizabeth C. Proctor Research Professorship

Thygeson: Mrs. Proctor gave ten thousand dollars each to set up the Elizabeth C. Proctor Professorship and also at the same time a foreign fellowship. It happened that neither one worked out, in part because we made no effort to raise additional funds because the purpose evaporated. The professorship fund fell through because Proctor cannot have a professorship; only departments can have professorships.

Hughes: *That's a university regulation?*

Thygeson: Yes, so that went out. Then the Cecilia Vaughan Foreign Fellowship Program took over from that. So what we've done is to combine the two funds, to which only Mrs. Proctor and Ruth Lee and myself donated. We combined them into the Elizabeth C. Proctor Lecture and Laboratory Fund, just like the Heintz Lecture and Laboratory Fund. The first Elizabeth C. Proctor Lecture will come in September [1987], when the chief of the National Eye Institute, Karl Kupfer, will give a lecture. So that's a change in purpose.

Harry William Hind

Thygeson: Another donor is Harry William Hind. At the time of building an addition to 95 Kirkham, I went down to see Harry, who had been an old friend, and I asked him for support, and he gave eighteen thousand dollars to start our library.

##

Hughes: *Would you say something about his background and how you met originally?*

Thygeson: I met Harry through Dr. Vaughan, because Dr. Vaughan and Sid Riegelman from the School of Pharmacy had done a research project on a glaucoma drug, which was then prepared and marketed through Barnes-Hind Pharmaceutical. Harry has always been interested in ophthalmology because his company started on ophthalmic drugs.

Hughes: *Is it broader now?*

Thygeson: Much broader now, but that is the basis for his company, so he's always stayed interested. He's donated to both the department and to Proctor.

Hughes: *I talked to him twice on the phone and learned that Barnes-Hind was the first company to produce sterile ophthalmic solutions.*

Thygeson: Yes, and that started through the work of Dan Vaughan and Sid Riegelman, who were trying to prevent *Pseudomonas* ulcer.

Hughes: *Didn't this tie in with some of your statements about iatrogenic disease and contaminated solutions?*

Thygeson: Yes. Dr. Vaughan is very active in preventive ophthalmology, and he wrote the first chapter in a textbook on preventive ophthalmology.

Hughes: *Were you the one who originally got him interested in preventive ophthalmology?*

Thygeson: Well, I really think so, since he was a resident at the time I arrived here, and he became very much interested in *Pseudomonas* infections and prevention.

Hughes: *I understand that the Proctor Foundation ran clinical trials of Barnes-Hind products.*

Thygeson: In a limited fashion, mainly through Sid Riegelman and Dr. Vaughan. And maybe a few other tests. But Proctor has not been a test organization for any pharmaceutical house.

Hughes: *Why?*

Thygeson: Well, we felt leery of tying up too closely with any pharmaceutical house. I guess that's my worry.

Hughes: What were you worried might happen if you did do a lot of clinical trials?

Thygeson: Bad vibes amongst the profession if you tied in with pharmaceutical houses. We have many examples where there's been a close tie-up and had unfavorable publicity.

Hughes: Commercialization and research are not considered good bedfellows?

Thygeson: We wanted to stay away from that as much as possible. We have been successful in staying away.

Hughes: Yet Barnes-Hind and the department of ophthalmology have worked out some products that have been very useful to ophthalmology. Dr. Hind mentioned rose bengal.

Thygeson: Yes, actually Proctor cooperated on rose bengal.

Hughes: How is that useful?

Thygeson: Well, rose bengal is a dye which affects the epithelium selectively, so using it in corneal disease you can tell what part of the epithelium is involved, as in the dry eye syndrome. It has been very useful. Fluoroscetin was the standard dye used for corneal work, but it has limitations, and it doesn't stain small areas of damage to the epithelium, whereas rose bengal does.

Hughes: Who originated the idea of rose bengal dry eye?

Thygeson: I really don't know, but it's become very useful.

Hughes: One other thing that Dr. Hind mentioned was the use of epinephrine in glaucoma.

Thygeson: Yes, that was studied by Dr. Vaughan and Dr. Sid Riegelman. That's been very successful.

Hughes: Had epinephrine not been used to treat glaucoma previously?

Thygeson: Yes, it had been used previously, but it was an enlargement of the use, and the development of bacteria-free specimens, and so on.

Hughes: At what stage was the research when the drug came to the Proctor Foundation for testing?

Thygeson: It's all been done by the Proctor Foundation; the pharmaceutical houses just give financial support, not much scientific contribution.

Hughes: The synthesis of the drug would be done by the pharmaceutical company?

Thygeson: Yes, but the actual application and everything would be done by the university.

Hughes: Does that mean that the university would work out dosage and all of the details of clinical use?

Thygeson: Yes. Fluorescein turned out to be quite vastly successful, and Barnes-Hind did very well on it, and Sid Riegelman and Dan Vaughan got some royalties for that. The royalties didn't come to the Proctor Foundation.

Ralph M. and Sophie K. Heintz

Hughes: *Please tell me about Ralph Heintz and his scientific contributions.*

Thygeson: Ralph Heintz and Mrs. Sophie K. Heintz were old patients of Dr. Vaughan. The interesting thing, I remember, was that they wanted to be patients of mine, but I was running so far behind on appointments that they chose Dr. Vaughan. Dr. Vaughan was young and didn't have the backlog of patients that I had.

Ralph became very friendly with Dr. Vaughan, and one day Dr. Vaughan brought Ralph up here out on this patio, and we had a very lovely coffee time in which Ralph and I found we had many friends in common in the radio field. I'd been an old amateur radio man, and I knew these people. For example, Phil Scofield, who operated station 6BU in Palo Alto with me, became Ralph's chief engineer. And Ralph's best friend was Colonel Clair Foster in Carmel, who was an old friend of mine. We had many things in common—all the people we knew in radio—but we had never met.

Hughes: *Dr. Vaughan told me that he'd been trying to get the two of you together for a long time.*

Thygeson: We became very friendly with Ralph and Sophie. Sophie was tied in with radio all her married life, too, and she became a very good operator in amateur radio, station W6SH. So this was the start of a very close friendship. Ralph was an inventor, with four inventions in the Smithsonian Institute. His inventive life is pretty well detailed in his oral history.*

Hughes: *How many patents did he hold?*

Thygeson: I remember he had eighty paying patents and about two hundred that didn't pay off. He was very important during the war because his company produced a lot of the aviation material, for example, the automatic pilot and the Norden bomb sight and all kinds of things for the war effort. After the war he bought this land down in Los Gatos and set up his own laboratory, retired, and out of that came a number of inventions, particularly the ocutome, which has so much importance for the eye.

Hughes: *Tell me how that developed and with whom he collaborated.*

Thygeson: He worked with Connor O'Malley in Dan's office. O'Malley had the need for a very small cutting instrument that could be inserted into the vitreous of the eye, that could cut fibrous bands that are so terrible, particularly in diabetic

* RM Heintz. *Technical Innovation and Business in the Bay Area*. An oral history conducted in 1976 by Arthur Norberg, Ph.D. History of Science and Technology Program, The Bancroft Library, University of California, Berkeley, 1982.

retinopathy or hemorrhage. These bands pull the retina off—the scar tissue contracts and pulls the retina off. So by this ocutome you could cut these bands and save the retina from being pulled off. Then the ocutome's been useful for cutting tumor material in brain surgery.

Hughes: What does it look like?

Thygeson: It looks like a very tiny pencil. It has two small cutting knives to cut the adhesions or cut up a tumor.

Hughes: What had been used before the ocutome?

Thygeson: Scissors had been inserted; it was a very crude method.

Hughes: Has the ocutome been widely adopted in ophthalmology?

Thygeson: Yes, very widely, all over the world.

Hughes: Was Dr. O'Malley's role to point out the need for such an instrument?

Thygeson: Yes, and then Ralph did the engineering and the construction.

Hughes: Was that not his last invention?

Thygeson: Yes, that was in about 1984.

The family got half the royalties. The royalties were divided. It was a very good royalty; I think they're still getting it. It's a very effective instrument. I haven't kept up with it, but I think it's still very popular. There have been other instruments developed like that, for example, the instrument used on President Reagan on prostatic material has the same principle of a little cutting instrument that cuts the tissue.

Hughes: All derived from Mr. Heintz's work?

Thygeson: Well, I can't say that, but on the same principle of a very tiny instrument that has cutting knives. The urethral instrument also has a visual deal, which they don't have in the ocutome, so they can see what they're cutting.

Hughes: Anything more about Mr. Heintz?

Thygeson: I think the interesting thing is that because of his interest in ophthalmology and Dr. Vaughan, he and Sophie Heintz offered to set up this Cecilia Vaughan fund. Dan didn't ask him for it; it was volunteered by Ralph and Sophie. This gift comprises first all eighty-three acres of land, and then in Sophie's will an additional twenty acres, including the house, the laboratory, and all that, come to the Proctor Foundation.

Hughes: Are there plans to keep the laboratory in operation?

Thygeson: No, there are not plans except to sell the estate there. We toyed with the idea at the time when Proctor has a big program, but since Proctor's had to contract, it's too far away and too costly to do anything about it.

Medical Illustration and Photography

Hughes: *I have just one more Proctor-related question, and that's to do with medical illustration and photography.*

Thygeson: We've always had good medical illustration in Proctor, first by an artist whose name was Sylvia Ford.

Hughes: *Was she employed full time?*

Thygeson: Half in the Proctor and half in the department. Then she was followed by Joan Esperson, who has a new, married name now, Weddell. She's been with us for twenty years, I guess, still half in the department and half in Proctor.

Hughes: *Is that service available to anybody in the Proctor?*

Thygeson: Yes. Joan's overworked; she's having trouble now keeping up, but she's done everything from charts to certificates, and every kind of painting, and been extremely valuable. She hasn't done any photography at all.

Hughes: *Is there somebody who does photography?*

Thygeson: No. We had in the department a young woman who, as I remember, was totally employed by the department but who would contribute to Proctor. Her name is Diane Beeston. She developed the stereophotography that we like so much.

Hughes: *She developed it herself?*

Thygeson: Well, she introduced stereophotography to the West Coast. She didn't make the instrument; the main instrument was done by Dr. Donald Donaldson of Harvard. It was the Donaldson camera that she adapted to every sort of photography.

Hughes: *Had he used it for stereoscopic photography?*

Thygeson: Oh, yes. She took it over and brought it to the West Coast. So she interested me in stereo—although I had done stereo before—but she interested me in the Donaldson camera. So most of my later photography was with the Donaldson camera.

Hughes: *When was the Donaldson camera introduced?*

Thygeson: Many, many years ago. Donaldson's retired down in Florida.

Hughes: *That must have made a tremendous difference in ophthalmology.*

Thygeson: Oh, it's a marvelous teaching aid, and I use it every Thursday in my seminars.

Hughes: How many photographs do you think you might have in your collection?

Thygeson: I must have several thousand.

Hughes: Dating how far back?

Thygeson: They go way back to Iowa days.

Hughes: And what will become of that collection eventually?

Thygeson: It all goes to the Proctor Foundation, but I don't know who will utilize it. The Heintz Laboratory chief would be the logical one to use it.

V. MISCELLANEOUS TOPICS

[Interview 6: January 21, 1987]##

Private Practice in San Jose

Hughes: Dr. Thygeson, could you tell me why you decided to go into private practice in San Jose?

Thygeson: Well, that occurred because we decided to leave New York and come back to California. That was based on several reasons. One was Ruth Lee had a terrible ragweed hay fever allergy with asthma. Then I really didn't like the administrative job I had at Columbia. I was spoiled because I'd had such a good appointment before that allowed me to do the laboratory work that I wanted to do. And we liked California so much better than New York.

Hughes: Was there the possibility of getting a full-time academic position in ophthalmology?

Thygeson: No, in California everything was volunteer; there were no full time appointments anywhere on the West Coast. Of course, there were only a few on the East Coast at that time, too. Everything was volunteer. So it was logical that I had to do a private practice.

Initially I thought of Monterey, which is such an attractive place; I looked at that first. Then we decided on San Jose with Dr. Beard because he was a captain with me at the close of the war at Dibble Hospital. He was number two in my department in the army at Dibble.

Hughes: Had you talked while you were still in the army about setting up practice together?

Thygeson: Oh, yes. He wanted San Jose very much; he didn't want San Francisco, so I went along with him and changed from Monterey to San Jose, especially because it gave me a chance to work two days a week in San Francisco.

Monterey was a little far. So the arrangement with which we started out was that I would have Tuesdays and Thursdays at the university, and then I'd work Monday, Wednesday, Friday, and Saturday mornings in San Jose. It was a very good arrangement.

Hughes: The practice was small enough in the beginning that Dr. Beard could manage on his own on Tuesdays and Thursdays?

Thygeson: Yes, we had to build up our own practice. Practice came fairly fast because we were the only board-certified ophthalmologists in San Jose, so we attracted quite a number of patients locally and also from the towns around San Jose.

Hughes: Was one of the reasons why Dr. Beard chose San Jose because there weren't board-certified ophthalmologists in the area?

Thygeson: Well, that's probably one reason, and the other was that he didn't like San Francisco. He had a brother-in-law who ran a pharmacy in San Jose, so he knew San Jose pretty well. I didn't know San Jose, but it worked out very well, and the idea of having two days free in San Francisco without interruptions was very attractive to me. The interruptions ruin your laboratory work, because you can't stand interruptions and do continuous work.

Hughes: Dr. Beard must have been sympathetic with your ideas about setting up the Proctor Foundation and realized that it was important to you.

Thygeson: Oh, yes, he knew all about it; he'd met Mrs. Proctor, and he was very sympathetic to work at the university because he also worked at the university in plastic surgery.

Hughes: He went up on different days?

Thygeson: Yes. It worked out very well because we could spell each other on emergencies and so on. But it wasn't too long before our practice was too large, and we brought in Dr. Robert Cook first, who was a resident at UCSF, and then we brought in Dr. Dan Vaughan, who was also a resident at UCSF. So for quite a while we had a four-man unit, which is now enlarged to six or eight. But I thought it was very good for me, because I was able to do quite a lot of work in those two days in San Francisco.

Hughes: Were you practicing general ophthalmology?

Thygeson: General ophthalmology, but my specialty, of course, was external disease, so I got a lot of consultations in my specialty, and the word got around that I was interested in external disease, so it made the referrals very easy.

Hughes: Were you leaving the plastic surgery to Dr. Beard?

Thygeson: Yes, except that I had to assist him in all the surgery, and he soon developed a big surgical practice, so I was in the operating room a lot with him.

Hughes: What hospital were you using?

Thygeson: We did most of it at O'Connor Hospital; some at San Jose Hospital.

Hughes: *Is there anything more you'd like to say about private practice?*

Thygeson: I would say that private practice was very valuable as a financial support so that my work in the laboratory didn't cost the Proctor endowment any money. This was very valuable because Proctor didn't have much money at that time.

Hughes: *You didn't take any salary?*

Thygeson: No salary. And then it was also valuable because we were able to use the university departments—microbiology and the Hooper—for consultation and for collaborating studies. This was only possible because I was free for two days.

Then the private practice also furnished a lot of material for study. We had a big practice, and we saw a lot of unusual cases, and many of these were important in the university work. Of course, at the university we saw quite a lot of consultation cases on Tuesday and Thursday that were directly related to the laboratory work. It was the private practice in San Jose that enabled us to do special work on chlamydia and trachoma and herpes and adenovirus infection and so on, all because of the support of the private practice. It was very valuable to Proctor because it didn't cost the Proctor any money.

Hughes: *Of course, that changed; your salary when you became director came from the Proctor Foundation.*

Thygeson: Sure, well, Proctor funds gradually improved, but not too much more than inflation. Inflation kind of took care of everything. As Proctor income improved, inflation went up.

Hughes: *After you became director, the patients from the San Jose clinic continued to come?*

Thygeson: Oh, yes, we got quite a lot of cases.

Hughes: *Was there a problem replacing you in the private practice when you became full-time director in 1959?*

Thygeson: We were able to bring in three additional men,* all of whom had been trained at UC. Well, there was a Canadian. He'd done a fellowship at UC, but I think he was trained in Edmonton, Alberta.

Hughes: *Why did you invite these people to join?*

Thygeson: We knew all their capabilities, just like Dr. Vaughan's and Dr. Cook's.

In the Proctor Foundation's present financial stringency, we're turning to private practice. A good example is Gil Smolin, who formerly was on a university salary by way of NIH grants. The grants stopped, so he's now supporting himself from private practice the way I did. The idea is still valid.

* J. Winston Duggan, Marvin Quickert, John H. Sullivan.

We have, I hope, Caren Lawder coming to the Proctor, who's a former Proctor fellow, and she will support herself by private practice. The endowment really is enough to support only one good laboratory and provide a kind of back-up to the others. Otherwise we have to raise money some other way. Private practice is one source.

The U.S. Indian Health Service

Thygeson: The association of Dr. Beard, and later with Dr. Cook and Dr. Vaughan, enabled me to be away from the office for extended periods for work with the Indian Service and the World Health Organization. This was very valuable to me and would only have been possible because our office was able to take care of my problems when I was away. So we very early in private practice again started trachoma work with the Indian Service. We were able to develop a good eye service for the Indians. Previously the eye care for the Indians had been done either by referral to private practitioners in the area or by general practitioners.

Hughes: Were you the first ophthalmologist on the Indian Service?

Thygeson: No, the first one was Dr. Proctor.

Hughes: Oh, of course.

Thygeson: But I was number two or three. One thing we did was with help from Douglas Powers (he was an Indian Service employee) we were able to get money from Congress to set up ophthalmological appointments in the Indian Service. This was new; previously there weren't any full-time ophthalmologists in the Indian Service.

Hughes: Was this right after the war?

Thygeson: Shortly after the war. It was trachoma that enabled the Public Health Service to really expand the eye care for the Indians, not only for trachoma but for all other eye problems, and give first-class eye care for the Indians.

Hughes: Over what area were you providing the services?

Thygeson: My area covered the entire United States and Alaska. The principal service was in the Southwest—Arizona and New Mexico—but it also spilled over into Montana and Idaho and Oregon—the western Indian area. My consultation work extended into Florida and the Middle West, but the actual ophthalmological work was really in the Southwest.

Hughes: Were there hospitals in each of these areas that were set up by the Indian Service?

Thygeson: There were two main Indian hospitals, one in Gallup, New Mexico, and one in Phoenix, Arizona. These were the real consultation hospitals, and then we also used the public health hospital here in San Francisco for Indian care.

Hughes: Did you bring people from a distance to San Francisco?

Thygeson: Yes. There were enough funds available for transportation, so it was quite satisfactory. And then there were optometrists brought in for prescribing glasses for the Indians.

Hughes: Were you impressed with the level of ophthalmological care, compared to what you might see in private practice?

Thygeson: Yes, I was very much impressed because previously all work had been done by general practitioners who had only a smattering of knowledge about ophthalmology. They took care of emergencies and so on, but many problems were completely neglected in the early days. Even general practitioners did cataract extraction. Seems unbelievable, but they got by with it.

Hughes: How did ophthalmologists feel about working for the Indian Service?

Thygeson: Well, it was a time when physicians were supposed to put in a year or two for military service. So half of the ophthalmologists that came into the Indian Service, came in just for two years. The other half were more or less permanent.

Hughes: Did they end up being pleased with the experience?

Thygeson: Well, the Indian Service was very attractive to many people because they liked the western life and the Indians, and they liked the art of the Indians, so it pleased many ophthalmologists and their families.

I look back on those days with the Indian Service with much pleasure because I think we were really accomplishing something.

Hughes: When you went out on field trips, would arrangements be made to collect patients at a hospital, or some location, knowing that you were arriving?

Thygeson: Yes. We worked generally with the medicine man of the area; had very good cooperation with the Indian medicine men, who were colleagues—they felt they were colleagues.

Hughes: Did that mean that the medicine man was often present when you saw the patient?

Thygeson: Oh, yes. We always had the permission of the chief of the tribe of the area and the cooperation of the medicine man. You had to do that, otherwise you'd be lost. We also had to have the cooperation of the principal of the Indian school in the area. They didn't like very well to have their classes disrupted, so we had a little problem occasionally with them.

But as I look back on it, we never had an unpleasant experience with the Indians in fifty years. The Indians were very cooperative. They looked upon the Public Health Service doctors as their doctors; the doctors belonged to the Indians. We had none of the racial antagonism that developed a little bit later with the young Indians. I can't think of a single unpleasant episode.

Hughes: Were they good patients in the sense that they followed directions?

Thygeson: In a sense. We worked mostly with children on the trachoma work, and they were very, very good. But the older Indians were not always so good at following directions. For example, I remember we did cataract extraction on a couple of old Indians at Fort Apache, and we had them in bed. In the first twenty-four hours they took off. [laughter] We finally traced them back to their homes. They could see blurred images—didn't have any glasses—and that's all they wanted. They didn't want to read or anything, so they were perfectly satisfied with the outcome.

Small things didn't bother them. For example, if they had a foreign body in the eye or herpes or something like that, they didn't seek medical attention. It had to be something major before they would seek any help. That was some good and some bad, because some foreign bodies were neglected because they didn't complain enough about them.

Hughes: Were clinics or hospitals so placed that Indians didn't have to come great distances?

Thygeson: In a sense, yes, but, for instance in the trachoma work, we generally went out in a team and went to an area and examined all the children.

Hughes: To a village, for example?

Thygeson: Yes. In the Apache area we'd go and examine everybody. At Fort Apache we examined, I think, about five thousand in perhaps two years.

Hughes: Young and old?

Thygeson: Yes, we examined everybody. The Apaches had one of the worst areas of trachoma, so we thought we'd take in everybody. One reason for that is that trachoma was often transmitted by the grandmother to the young child because the grandmothers often took care of the young children. The grandmothers often had flare-ups of trachoma, just the way old people often have a flare-up of tuberculosis, and would transmit it to the young children. So we had to examine everybody.

Hughes: Were you also trying to teach them preventive care?

Thygeson: Yes. We were not too successful in doing that, however. But overall I think we were able to reduce trachoma. It's no longer of any importance in the U.S. There are a few smatterings of very mild cases among the Indians, but that's all. Formerly it was a blinding disease.

Hughes: How long did the average treatment go on?

Thygeson: Well, in the early days the treatment went on for as long as a year; that was before the sulfonamides came in. When the sulfonamides came in, the treatment time was reduced to about three weeks.

Hughes: What were you using before the sulfonamides?

Thygeson: A copper sulfate stick.

Dr. Proctor set up at Fort Apache, using the old enlisted men's barracks, a school for the severe trachoma cases. They were kept there during the school year and went to school every day, and we were able to treat the children every day with the copper sulfate stick. The nurses did most of this treatment. The Public Health nurses were very good, so the ophthalmologists really had supervisory control but didn't do the actual treatment.

When the sulfonamides came in, the whole picture changed because this was a specific treatment, whereas copper sulfate was very poor treatment. It started back in Egyptian times, the historical treatment for trachoma.

Hughes: *Did copper sulfate actually kill the trachoma agent?*

Thygeson: What it did was change the epithelium of the conjunctiva to a more resistant form so that it crowded out the chlamydia.

Hughes: *But it took a year or so to do that?*

Thygeson: Yes. It worked very well, but the limitation was that it was hard to get anybody to put up with a year of treatment. So it worked, but only selectively, whereas the sulfonamide worked beautifully in all the severe cases of trachoma, but less well for the very low-grade cases of trachoma.

Hughes: *In these surveys you were making, were you picking up cases other than trachoma?*

Thygeson: Oh, yes, for instance, cataracts were brought into the major hospitals for cataract extraction.

Hughes: *Was the Indian hospital in Gallup, New Mexico providing a service similar to that of Fort Apache?*

Thygeson: That was initially the main consultation hospital for all eye care, and then the new Phoenix hospital was built, and it became the more important eye service.

Hughes: *These are general hospitals?*

Thygeson: General hospitals, but the eye services were a very important part. The eye problems have been very important in the whole Indian period. Even back in historical times, the military surgeons coming into the Indian areas always noted the eye problems of the Indians. The Indians had eye problems very early, in the 1800s.

Hughes: *Is that because of their life style?*

Thygeson: That was trachoma. We're not sure just how this trachoma developed in the Indians. There are two theories: one is that the Indians brought trachoma from Mongolia or Siberia over through Alaska and to the whole Indian area,

that the Indians always had trachoma. The other theory is that the Spaniards brought trachoma over into Mexico and that trachoma spread over the Indians out from Mexico.

Hughes: So it's the same question that you hear debated about syphilis, whether that was endemic in the New World or brought in by Europeans?

Thygeson: Yes. But I think it's pretty definite that syphilis was endemic in the Indians because the bones of the old Incas show syphilitic changes. That theory is a little bit like evolution—it's disputed, but it's pretty definite that the Indians had syphilis.

Hughes: Dr. Bettman told me about trachoma meetings at the Indian hospital at Gallup, New Mexico, which he said he attended.

Thygeson: Oh, yes, that's right. We had an annual trachoma meeting to which all the major trachoma workers in the country came. We had three-day teaching sessions, and Dr. Bettman came up three or four or five times. He talked mainly on the traumatic injuries to the Indian eyes and how to handle them. Trachoma was not the only subject, but all the Indian eye problems, even including refraction, were brought up. This was really a teaching session.

Hughes: Who came to those meetings?

Thygeson: Everybody, including nurses, took time off.

Hughes: People who were working with the Indians?

Thygeson: Social workers and optometrists came too. They were quite important meetings, I thought.

Hughes: Dr. Bettman also mentioned problems getting glasses for Indians.

Thygeson: Yes. Some tribes, like the Navaho, were able to fund their own glasses. They got glasses rather cheap, wholesale. But others had to really scrounge around for private funds to supply glasses. So it varied from area to area. The Navahos were the best, because they were able to provide money for their own glasses.

Hughes: Were they a wealthier tribe?

Thygeson: Yes. The Navahos got money from oil and coal and uranium, so tribal funds were better.

Hughes: I'm surprised that glasses weren't supplied by the Indian Service.

Thygeson: Well, in a few instances they were. But the Indian Service was never really affluent, and the money that came in for the trachoma work, there was a little difficulty in using for glasses.

Hughes: Did you receive a salary?

Thygeson: No, but I received a per diem. As I remember, it was fifty dollars a day plus transportation and sometimes a per diem to cover costs at a motel or dormitory. We often stayed in the boarding schools.

Hughes: *Did the fact that the children were in the school away from their parents for a year at a time cause problems?*

Thygeson: Yes, it did cause psychological problems. There was always an attempt to bring the schools to the Indians as much as possible.

I just saw in the paper that originally the Indian Service wanted to get rid of all the schools and make all the Indians go to regular public schools. The Indians didn't like that; the Indians liked to have their own schools and their own hospitals and all that. So there was a problem there.

Hughes: *Is anybody connected with the Proctor Foundation occupied with the trachoma control program amongst the Indians, or is that no longer a problem?*

Trachoma Programs

Proctor Foundation Programs Abroad

Thygeson: It's no longer a problem, and as far as I know there's no connection. However, the international trachoma program is very much a part of the Proctor deal, and Dr. Dawson is very active in that.

Hughes: *Is that mainly Tunisia and Egypt?*

Thygeson: Tunisia and Egypt are the two main areas, but not the only.

Hughes: *Proctor fellows on occasion cover those countries?*

Thygeson: Yes, we have two of them over in Egypt right now.

Hughes: *How is that project funded?*

Thygeson: It was originally funded in part by the World Health Organization, but mainly by NIH. There was a program, Public Law 480, by which the United States sold food, like grain and so on, to Egypt, for example. Then the Egyptians would pay for that in local currency, which would not be sent out of the country but could be drawn upon locally. So these trachoma programs were funded by drawing on those funds. Public Law 480 has run out now, as far as I know.

The present program at Proctor is funded by a grant from the Clark Foundation, set up by a woman whose husband ran a big cosmetics organization. Not Revlon, but a rival to Revlon. That's quite extensive support because it takes in the School of Public Health in Berkeley also, so they've got anthropologists and social workers and everybody mixed up in this current trachoma program.

World Health Organization Expert Panel on Trachoma

Hughes: In the 1950s and sixties you were on WHO's Expert Panel on Trachoma.

Thygeson: Yes. The World Health Organization became interested in trachoma and related diseases as a part of their world interest in infectious eye diseases. They were also interested in onchocerciasis and in vitamin A deficiency. I think those were the three main interests. I was on the first expert committee, in which we outlined the problem and got started on trachoma control programs.

Hughes: What countries were targeted in that initial phase?

Thygeson: All the North African countries were targeted but also some of the southern European countries, including Portugal, Spain, Italy, Yugoslavia. There was trachoma there, but in a rather mild form, much less prominent. North Africa was pretty bad; Egypt was the worst.

Hughes: Why should that be?

Thygeson: Well, it's economic. Trachoma is a disease of poverty and also of climate. Flies spread the disease. So if you combine poverty and tropical conditions, you've got a pretty bad area for trachoma. If you did nothing but just improved the economics, you could completely change the trachoma picture. You wouldn't have to treat the trachoma; it would just disappear on the basis of personal hygiene and fly control.

Hughes: Were you attempting to do that?

Thygeson: Well, we didn't have much luck on the economic side, but we did on the treatment of trachoma and especially on control of the secondary infections. North Africa had periodic epidemics of bacterial conjunctivitis which spread the trachoma. We were able quite effectively to control these epidemics and stop the spread of trachoma. But trachoma's still a problem in North Africa. It's alleviated, but it's in no sense handled properly.

Hughes: Did WHO have groups in all those areas that you mentioned?

Thygeson: Yes. WHO provided pretty good support.

Hughes: Was there a system of hospitals that it used?

Thygeson: In a sense, yes, because in Tunisia they had an institute of ophthalmology, and in Algiers they had an institute of ophthalmology, and in Egypt they had the Giza Memorial Laboratory. It worked out pretty well.

Hughes: Were the institutes established at about the same time as the Giza Memorial Laboratory [1925]?

Thygeson: The Tunisian one was, but the one in Algiers had been established some time before.

Hughes: Did you go to North Africa while you were on the panel?

Thygeson: Yes.

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Thygeson: My job was really as a consultant rather than an active worker, but I did a lot of surveys, and I surveyed the laboratory work. One year I made a survey of the whole Mediterranean area, and another year I surveyed South America.

Hughes: How did you survey such vast areas?

Thygeson: Well, of course, I did what I could do in a couple of months. I got an idea of the general problem.

Hughes: This was by talking to people and looking at reports?

Thygeson: And going to the laboratories.

Hughes: Then would you report back to the expert committee?

Thygeson: Yes, and I made a report back to the chief. There was one person designated finally to run the trachoma work, and in a large part it was Dr. Peter Maxwell-Lyons. He was in the department of virology of the World Health Organization.

Hughes: An American?

Thygeson: No, he was an Englishman. He was the main force behind the whole trachoma work. He was on the first expert committee, and he followed through and became the WHO liaison with the U.N. Secretariat.

Hughes: Was he responsible for choosing the membership of the expert committee?

Thygeson: Not the first one, but after the first one.

Hughes: Who else was on that first committee?

Thygeson: I remember Roger Nataf from Tunisia and Dr. Bietti from Italy and Dr. Graham Scott from South Africa. There were about eight or ten on the committee.

Hughes: These were people whose names were well known?

Thygeson: Who were interested in trachoma.

Hughes: Were people assigned geographic areas?

Thygeson: They were assigned later by the chief as he thought necessary. At the expert committees a lot of work was done to prepare source material. So one of my jobs was to write working papers. So I'd come before the expert committee meeting, and I'd prepare on different subjects what the current status was.

Then the committees would review that and other preparation and decide what the program should be for the next year. One time I remember spending two months every day working on these papers. I wonder now how my friends back home allowed me to do that.

Hughes: They knew it was important.

Thygeson: They didn't object.

Hughes: So you were abroad during those two months, writing the papers?

Thygeson: In Geneva.

Hughes: What about nursing staff and laboratory staff—ancillary staff?

Thygeson: WHO had no laboratories and no nurses of their own, but they supported by grants laboratory projects.

Hughes: In the institutions that we already mentioned?

Thygeson: Yes.

World Health Organization Trachoma Reference Centers

Thygeson: Then eventually they set up what they called reference centers. They had done that for influenza; I think that was the first reference center set up.

Hughes: That was under the Rockefeller Foundation, was it not?

Thygeson: It may have originated there, but it became a WHO function. The main one was set up in Copenhagen.

Thygeson: In the trachoma work, the main one was set up in the Proctor Foundation [1965]. Then smaller units were set up, one in London and another one, I think, in Tunisia.

Hughes: What did those centers do?

Thygeson: They had all the strains of chlamydia that had been isolated and the pathologic specimens and the microbiology of the secondary infections, all the organisms that were found in the eye.

Hughes: People in the field would send specimens in?

Thygeson: Yes, and would get a report from the reference center. It was quite effective, and it was very good for Proctor Foundation.

Hughes: You were director of that center?

Thygeson: Originally, and then Dr. Dawson took over.

Hughes: Does it still exist?

Thygeson: Yes, it still exists in a modified form. I think it's now a [World Health Organization] Center for the Prevention of Blindness—it has a different name.

Hughes: *That makes it more comprehensive?*

Thygeson: Yes. But it's still functioning.

Hughes: *I know you went many places with WHO. Is there a story to be told about those assignments?*

Thygeson: Well, I went on many assignments, all of them very interesting to me. It was a means of world travel.

I think one interesting feature was that WHO had a travel agency, and they made all the arrangements for your travel. They insisted that I travel as a professor because the American doctor was supposed to be wealthy, and the hotel rate would be raised about ten or twenty percent for American doctors, whereas the American professors were well known to be poverty-stricken. And they were at that time.

Hughes: *So you were Professor Thygeson?*

Thygeson: I was a professor. We were given WHO diplomatic passports, and all arrangements were made by the WHO. It was very effective. They had agents all over, in almost all the cities, and they would make arrangements for your hotel and everything.

Hughes: *Mrs. Thygeson usually went with you?*

Thygeson: Often, not always.

Hughes: *You were in Geneva for some months, were you not?*

Thygeson: Yes, I was preparing reports and working papers for the expert committees. We had about eight or ten expert committees on trachoma that I remember. I prepared papers for all of them, I think.

Hughes: *There are that many expert committees for geographic reasons?*

Thygeson: About every year they formed an expert committee.

Hughes: *Serially.*

Thygeson: But they did cover different areas. Initially all these expert committees were in Geneva; then they started having them in different geographic areas. They had one in Tunisia, and they had one in India, and they had one in Yugoslavia. They went around.

Hughes: *Did the membership remain international?*

Thygeson: Yes. Initially the Russians were not in on it. The Russians wouldn't have anything to do with WHO in the early days; then the Russians came in.

Hughes: *Why were they opposed to WHO?*

Thygeson: I really don't know. I guess it was money; the WHO got money from all the member nations—it was optional. Russia did not come in at first.

Hughes: *Trachoma is a problem in Russia?*

Thygeson: It was. I don't really know what the status of it is now, but it once was quite a problem.

Hughes: *Where else did you go with WHO?*

Thygeson: The two biggest trips I made were visiting all the countries in the Mediterranean area, and then visiting all the trachoma areas in South America.

Hughes: *For the purpose of a report in each case?*

Thygeson: Yes, and deciding whether trachoma was a problem. It was a terrible problem in Brazil, especially in northeastern Brazil. But it wasn't much of a problem in any of the other South American countries. There were millions of cases in Brazil.

Hughes: *Did WHO step in in Brazil?*

Thygeson: Yes, but the interesting thing was that Brazilians handled that very well themselves. They put on a sulfonamide program that was very effective. They treated over a million cases with sulfonamides, and they didn't get into any trouble. We got into trouble here with some sulfonamide complications, and there was trouble in Australia with sulfonamides.

Hughes: *What can happen?*

Thygeson: Well, they get an allergic syndrome that causes scarring, called erythema multiforme major, a skin-mucus membrane complication of drug therapy. We had a few cases in our Indian area, but in Australia they had three cases with very severe complications—I don't know whether they resulted in death or not—but anyway the public health service of Australia cancelled the whole trachoma program.

Hughes: *Then what was used for treating trachoma?*

Thygeson: I really don't know. They just stopped treatment. Maybe trachoma subsided by itself.

Hughes: *You were also in Taiwan, Manila, Belgrade, and Jerusalem.*

Thygeson: Jerusalem was, of course, part of the Mediterranean tour. Taiwan and Manila were separate deals, and then I went to American Samoa. That was not under WHO; that was under the Department of the Interior. The medical care of American Samoa was a function of the Department of the Interior, just the way it used to be in the old trachoma days when the Department of the Interior ran the medical service for the Indians, before the Public Health Service took over. The program in American Samoa was bad, because they really didn't have anybody who knew how to handle the medical program, so it was very poorly done.

Hughes: *That seems inefficient, to have a medical program outside the Public Health Service.*

Thygeson: It was the old way. I don't think it was well done.

Hughes: *Were you making a survey of American Samoa?*

Thygeson: We actually did a survey and a three-week trachoma treatment program.

Proctor Foundation Conferences

Hughes: *The Proctor Foundation sponsored two international conferences on the clinical and laboratory aspects of trachoma, one in 1966 and the other in 1970.*

Thygeson: Yes, and they were both based in Asilomar and were very enjoyable. We had trachoma people from all over the world, and we did something I'll never do again. After the first meeting, which incidentally resulted in a very important volume containing the minutes and papers,* we invited the members from abroad to have a little secondary meeting at Tahoe. We put them all up by our place in Tahoe and with friends, and we fed them, and Ruth Lee and Krissie [Thygeson] and her husband at that time really pitched in and put on a real program.

Hughes: *That sounds like quite a task.*

Thygeson: It really was; we'd never do it again. It was a killer, but we had a lot of fun. But the second one we didn't do at Tahoe—just Asilomar.

Hughes: *Were the reports you put out looked upon as the latest information on the treatment and study of trachoma?*

Thygeson: At that time, yes.

The International Organization Against Trachoma

Hughes: *Do you know how the International Organization against Trachoma originated? I know it was founded in 1930 and that Nicolle was the first president.*

* The May 1967 issue of the *American Journal of Ophthalmology* (63:2) was devoted to publication of the papers presented at the first conference.

Thygeson: Well, there were these two separate organizations that coalesced. One was the French organization against trachoma which usually held a meeting in connection with the French ophthalmologic society, and then the International Organization against Trachoma, which met only in connection with the International Congress of Ophthalmology, so it had less frequent meetings. The purposes were the same, and there was a sense of duplication.

They were eventually combined to make the International Organization against Trachoma. It's still active, except that it's branched out to any trachoma-related diseases. Dr. Dawson's been very active in that. The most active members abroad were Roger Nataf from France, and from Italy Giambattista Bietti. Those were the two really active people.

Hughes: *Do you remember Nicolle being active when you were with him in Tunis?*

Thygeson: He and Victor Morax started the international organization with A. Cuenod. It had a journal, the *Revue Internationale du Trachome*. It was the first journal against a specific disease, so historically it is important. [tape interruption]

The International Organization had a gold medal that was given out periodically. Dr. Dawson received a medal, I received a medal, and the Proctor Foundation received a medal. It was the first laboratory that ever received the trachoma medal. The *Revue Internationale du Trachome* is still going strong.

K. F. Meyer was a pioneer in psittacosis work, and as you know, psittacosis is a chlamydial disease. At one time he was very much interested in trachoma, and I had a number of consultations with him, and I had some difference of opinion. He thought that psittacosis belonged to a different group from the trachoma agent, whereas I thought trachoma belonged to the psittacosis group. We called it the trachoma-lymphogranuloma psittacosis group of agents, and Dr. Meyer wouldn't buy putting trachoma in that group.

Hughes: *On what grounds?*

Thygeson: He could not find serologic evidence that the trachoma cases showed antibodies to the group, whereas we had found such evidence. The reason he didn't find it was that he had very mild cases of trachoma that he studied, and we had very severe cases, so we could show antibodies while he couldn't. We turned out to be right; he was wrong on that. But he was an important member of that first international trachoma meeting that we had.

Hughes: *Was there anybody else outstanding at that first meeting?*

Thygeson: Dr. Dawson was there as a young man, and Dr. Barrie [R.] Jones from London, Bietti, Nataf, Maxwell-Lyons—all the important trachoma people were there. In one of these books I have a picture of the group taken out on the beach in Asilomar; there's about seventy-five or a hundred. [tape interruption]

Symposium on Keratoconjunctivitis

Thygeson: We also had a symposium on viral keratoconjunctivitis, which included trachoma because at that time we thought trachoma was a viral disease. This was a symposium sponsored by the National Institutes of Health, and it comprised the Sensory Diseases Study Section and the Virology Study Section of the NIH. We met in 1955 at the Proctor Foundation in San Francisco, and, as a supplement to the *American Journal of Ophthalmology*, we had the transactions of the meeting.* So in a historical sense this was the first meeting of the virologists and the ophthalmologists—at least in this country. I'm sure in the world, too, because I don't think there was any other symposium.

Hughes: *That was your idea?*

Thygeson: I was the chairman of the Sensory Diseases Study Section, and I persuaded the NIH to sponsor it.

Hughes: *Do you want to say anything about the participants?*

Thygeson: The participants in virology were all the famous names of the United States, and one interesting feature was that we all got acquainted. I think it was the first time the virologists and the ophthalmologists ever got together. We had a dinner down here in Los Altos out on the patio, preceded by a rather hair-raising drive to the big trees and then down the side of the mountain in a big bus. [laughs] I doubt if any of the participants will forget that drive.

Hughes: *Were all the participants working on keratoconjunctivitis?*

Thygeson: No. Viral keratoconjunctivitis was the subject of the meeting, and all the participants had some interest in it, but not all were actually working on the subject. At that time the adenoviruses had just been recognized, and so the disease epidemic keratoconjunctivitis kind of took the front stage in the symposium.

Hughes: *Did the relationship between the virologists and the ophthalmologists continue?*

Thygeson: Very much, but on an individual basis. For example, one of the participants was Dr. Jack Snyder of the School of Public Health at Harvard, and as a result of this meeting he became very much interested in chlamydial disease and sponsored a big program at Harvard on chlamydial disease.

* The symposium on viral keratoconjunctivitis. *Am J Ophthalmol* 1957; 43:2 (April).

Published Papers

Seborrheic Blepharitis, 1947 and 1954

- Hughes:* You wrote a paper with Joseph S. Gots and Morris Waisman called, "Observations on *Pityrosporum ovale* in seborrheic blepharitis and conjunctivitis."* Could you say something about what was known at the time and then subsequently about the relationship of *Pityrosporum* to blepharitis?
- Thygeson:* Blepharitis was an important, but little studied, ocular disease. In the dermatologic world it was almost entirely confused with seborrheic dermatitis. We were able at Drew Field to make a study of the etiology of blepharitis, and in the course of that study we noted the very common presence of *Pityrosporum ovale* and *orbiculare*, two yeast forms, which were so characteristically correlated with the seborrheic form of blepharitis that we thought there must be some etiologic significance. Now, we could grow the *Pityrosporum*, but we couldn't produce any disease in animals with it, and we were afraid to do human inoculations. So all we had was the clinical correlation, but I must say I felt very strongly that there was an etiologic relationship.
- That theory did not get over very well, and for many years it was not considered very pertinent. But in the last few years it's been revived, so there are now at least two papers indicating that there is a real tie-in between *Pityrosporum* and seborrheic dermatitis.
- Hughes:* A few years later, in 1954, you wrote another paper on seborrheic blepharitis,** this time with Daniel Vaughan, and again you postulated the role of *Pityrosporum*.
- Thygeson:* Yes, at that time we were very sure that *Pityrosporum* had an etiologic role, but another study by a Proctor fellow, Aleksandar Parunovik, rather disputed this concept.
- Hughes:* Wasn't one of the problems that there were no susceptible experimental animals?
- Thygeson:* That's right. We inoculated a number of monkeys. We couldn't reproduce seborrheic dermatitis in animals; it's probably a human disease that does not transmit to animals. But now I feel pretty sure that *Pityrosporum* is etiologic in seborrheic dermatitis.
- Hughes:* What about the differential diagnosis of seborrheic blepharitis?
- Thygeson:* We were able to study quite extensively the different forms of blepharitis, and we found three main types: one, a staphylococcic type, which was most important because it had associated corneal changes; two, a diplobacillary type, which was of lesser importance because it had no corneal changes; and,

* *Am J Ophthalmol* 1947; 30:1484-1494.

** Seborrheic blepharitis. *Trans Am Ophthalmol Soc* 1954; 52:173-88.

three, the seborrheic blepharitis, which is mainly cosmetic—didn't have much in the way of symptoms. Really the only important one was the staphylococcal type. But, anyway, we could differentiate on clinical grounds the three main types of blepharitis which were sometimes associated. We sometimes had a staphylococcal and seborrheic together in the same person.

Hughes: Which one was treated with selenium sulfide?

Thygeson: It was the seborrheic one, the only one that responded to selenium—beautifully.

Hughes: Was there a treatment for the other forms?

Thygeson: Well, diplobacillary was pretty easy because you could use zinc sulfate or sulfonamides or almost anything, and did very well, but the staphylococcal type was very resistant. We had a hard time, and we resorted to staphylococcal toxoid, among other things.

Hughes: Did the toxoid work?

Thygeson: Yes, the toxoid was very useful. The toxoid knocked out the corneal changes, but it often did not affect the organisms in the lid margin. We made our own toxoid. But toxoid had a great interest until the antibiotics came in. The antibiotics proved more effective than the toxoid.

Hughes: In the paper that you wrote with Dr. Vaughan, you suggested the use of the eye in etiological studies, since you found the lid margin to be a good place to have a pure culture of the agent. Do you remember that remark?

Thygeson: I don't remember that remark, and I wouldn't think that the human eye would be a good place for any experimental work.

Hughes: You specified the lid margin. According to my notes, you suggested use of the eye in etiological studies since the lid margin has a high incidence of pure disease.

Thygeson: Well, that is true in the sense that you could very often find pure seborrheic disease on the lid margin, where it was difficult to find it pure elsewhere on the skin or scalp.

Hughes: So I expressed that wrong.

Thygeson: Yes, therefore, for study, yes, but not for experimental inoculation.

Acute Central Ulcers of the Cornea, 1948

Hughes: In 1948, you published a paper on acute central ulcers of the cornea, based on fifty cases which you observed since 1932. What treatment did you use before the antibiotic era?*

* P Thygeson. Acute central (hypopyon) ulcers of the cornea. *Calif Med* 1948; 69:18-21.

Thygeson: Before the antibiotic era, the main cause of central ulcer was the pneumococcus. It produced an hypopyon keratitis. That's pus in the interior chamber. It was difficult to treat, but we treated it in two ways: one was to do a paracentesis to let out the primary aqueous so that the secondary aqueous could come in with its antibodies. Then we'd cauterize the advancing border of the ulcer. This had moderate success, but when the antibiotics came in, penicillin was much superior to that. So in the early days we had trouble.

Hughes: *Did you use local anesthetic with the cautery?*

Thygeson: Oh, yes. I carried around in my bag a little copper cautery and an alcohol lamp. The pneumococcal ulcer was so common in Colorado days that I always carried that around with me.

Hughes: *Why do you think it was prevalent in Colorado?*

Thygeson: In Colorado we had the soft coal mines, and the miners were very susceptible to pneumococcal ulcer. They're exposed to foreign bodies all the time, and it is thought that they don't get enough sunlight and that their resistance might be down because of poor vitamin content. They don't get the sun to produce vitamin D.

Hughes: *I saw a reference to optochin.*

Thygeson: Optochin was really one of the first chemotherapeutic agents. The pneumococcus was very susceptible to optochin, and it's still being used to differentiate between a pneumococcus and a streptococcus. But it's a very toxic substance, so when it was used systemically it caused terrible problems. It killed the pneumococcus, but it killed a lot of cells, too. But it could be used in the eye quite successfully, so optochin powder would be put right in the ulcer, and it was useful. It was one of the first specific chemotherapeutic agents.

Hughes: *Do you know anything about the history of it?*

Thygeson: No, I don't, but I think it was of European origin.

Hughes: *Sulfonamides were used before penicillin came in?*

Thygeson: The sulfonamides were much less successful compared to penicillin. Penicillin became very important. Before penicillin we used with some success type-specific rabbit antipneumococcal antiserum instilled into the eye.

Hughes: *Which you also were making?*

Thygeson: Yes, we also made that. We made that by injecting rabbits.

Hughes: *Nowadays would substances like that be available commercially?*

Thygeson: No, I don't think so. They used to make rabbit antibody for systemic use in pneumococcal pneumonia, but that's all gone. The pneumococcus is still important, but not as important as it used to be because most corneal ulcers are now caused by opportunists, fungi and so on. The whole picture on corneal ulcer has changed.

Hughes: *Why?*

Thygeson: Because of the steroids being introduced. Steroids lower the resistance so that fungi and various opportunistic bacteria come in that never used to come in at all.

One of the programs at Proctor is to change the method of prescribing steroids so that this doesn't happen. At one time practically every ophthalmic solution or ointment had steroids in it. I went back to the FDA to protest this, and I had no luck in getting the combination out. But I did have luck in preventing the distribution to the general practitioners of the ointments and solutions. That stopped quite a bit of the corneal ulcers produced by the general practitioner or pediatrician.

Hughes: *Why wouldn't the FDA listen?*

Thygeson: Well, my presentation probably wasn't good enough, but I remember the chairman said, "We have to be very careful; this is a fifteen million dollar industry, and we don't want to disturb it." I resented it very much.

Hughes: *Were acute central ulcers also a problem of solution bottle contamination?*

Thygeson: Yes, they were much later when steroids entered the picture. But originally contamination of solutions by *Pseudomonas deruginosa* was a real problem. It took a tremendous dose of *Pseudomonas* to produce the ulcer, so you had to have a solution that was really teeming with organisms. Then you had to have a break in the epithelium larger than usual, because the eye has quite a bit of resistance to opportunists. When the steroids came in, the resistance was broken down, so *Pseudomonas* ulcers became a dime a dozen. The one thing I've fought against for so many years is the unwise use of the steroids.

Hughes: *What was the theory of having steroids part of so many ophthalmic preparations?*

Thygeson: It was a pharmaceutical house concept that every inflammation in the eyes should be treated by an antibiotic to kill the infection and steroids to eliminate the inflammation. Totally wrong concept because it didn't work out that way. So solutions and ointments and everything were combined antibiotics and steroids.

In veterinary ophthalmology it was worse because they combined four things: They had an anesthetic, and then they had the antibiotic and the steroids, and often they had a vasoconstrictor—four different things to put in the eye in one preparation. So they had a terrible time; horses got very extensive corneal ulcers. Fortunately that has gone out of style.

Hughes: *Did you have any confrontations with the steroid industry?*

Thygeson: Yes. I wasn't very popular with the steroid industry. [laughs] Of course, I've had a lot of confrontations in my life.

Hughes: *In what ways did they confront you?*

Thygeson: The steroids were a miracle drug because they stopped all the pain and symptoms. They were called the heaven and hell drug—the initial heaven because they stopped the inflammation and the pain, but long term it was hell because you could lose the eye. Use of steroids in herpes was the worst because they stopped all the symptoms of herpes, and the patient felt so much better, and then they'd lose the eye a month or two later.

Hughes: *How did the pharmaceutical houses that were making steroids express their opposition to your opinions?*

Thygeson: They wrote papers, and a lot of their ophthalmologic supporters wrote papers. I remember in New York City I was practically attacked by two supporters of the steroids. One of them grabbed me by the lapels and shook me. So I had quite a lot of opposition.

Hughes: *How could an ophthalmologist ignore the evidence that you had?*

Thygeson: Now, of course, it's accepted. But it takes quite a long time to get anything controversial accepted. I was going against a miracle drug that stops pain and inflammation.

Hughes: *Of course, it wasn't that you were against use of steroids in toto.*

Thygeson: No, steroids were a miracle drug in phlyctenulosis, but they were a disaster in infectious disease in which the infection was in the eye. In phlyctenulosis the infection was in the lungs, and the eye was secondarily involved in an allergic reaction, and steroids worked beautifully, no complications.

Aureomycin in the Treatment of Herpes Simplex Corneae, 1950

Hughes: *Another paper, "Aureomycin in the treatment of herpes simplex corneae," was written with Michael Hogan.**

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Hughes: *You described twenty-four cases of herpes that were treated with aureomycin. Was aureomycin a new drug in 1950?*

Thygeson: Yes. Aureomycin was, I think, the first broad-spectrum antibiotic, and it was touted as the answer to all the problems because it took care of all bacteria and the larger viruses, and herpes was caused by a larger virus.

* *Am J Ophthalmol* 1950; 33:958-960.

The authority on aureomycin was Dr. Perrin Long of Baltimore. He came out here and gave us a lecture on aureomycin, which was very impressive. Dr. Braley, who had been using aureomycin, wrote us that it worked fine on herpes. He sent us a supply of aureomycin borate—aureomycin which was in a borate solution—which was well tolerated by the eye.

So Dr. Hogan and I decided to run a series. You said it was twenty-four cases? As I remember it was larger. I remember we each eventually had fifty cases of dendritic keratitis which we treated with aureomycin borate. The idea was that if it didn't get better in a week, we would then change to the standard method of treating herpes, which was to remove the epithelium, with or without some cauterization. The aureomycin worked so beautifully that we hardly ever had to do anything else; it was almost like a miracle. But we found out by experiments on mice that aureomycin had no effect on the herpes virus. All we were doing was placebo therapy. Herpes is remarkably susceptible to placebo therapy.

Hughes: Can you explain that?

Thygeson: Well, it's all in the literature, in Shakespeare, about the cold sores that come on after a fight, emotional upset. Herpes comes on after a fever and after exposure to ultraviolet light. Herpes virus is triggered by at least four different methods, mostly fever. Emotional upset is second; ultraviolet light is third; trauma is fourth.

Anyway, it doesn't much matter how you treat herpes; so long as you don't use steroids it gets well. If we believed in the aureomycin, the patient believed, and we got a remarkable effect. It shows that placebo therapy can be very effective, and what it is is psychoimmunology. Stress lowers the immune system; placebo elevates the immune system.

Hughes: How long did it take for you to realize that it was a placebo effect?

Thygeson: Not too long, because we immediately went to the mouse experiments and showed that herpes virus was not influenced by aureomycin.

Hughes: Were you the first to notice this placebo effect?

Thygeson: No, Dr. Braley in Iowa City was, I think, the first. Anyway, we take advantage of the placebo effect in herpes all the time.

Traumatic Hyphemia, 1950

Hughes: In 1950 you published a paper with Crowell Beard, "Observations on traumatic hyphemia," which is hemorrhage in the anterior chamber. You treated a number of cases. Traditionally there had been very poor prognoses for these cases?*

* *Trans Pac Coast Oto-Ophthalmol Soc* 1950; 31:145.

Thygeson: Well, the trouble was the matter of the secondary hemorrhage. The eye could be destroyed by a secondary hemorrhage that came on a week or so after the initial injury. All we did was to try and prevent the secondary hemorrhage by immobilizing the iris and putting the patient to bed and keeping down strain for a week or ten days. Then the danger of the hemorrhage would be over.

Hughes: *How did you immobilize the iris?*

Thygeson: By atrophine.

Hughes: *That was generally successful?*

Thygeson: Yes, we did pretty well on that. It wasn't earthshaking, but we got a little better results than some of the general ophthalmologists did.

Superficial Punctate Keratitis, 1950 (continued)

Hughes: *In a paper entitled, "Superficial punctate keratitis,"* you objected to the use of that term for quite a range of diseases and defined it more precisely.*

Thygeson: Yes. The term superficial punctate keratitis had begun to be used very broadly for any spotty keratitis. But, as I said earlier, originally the term had been used by Ernst Fuchs in Vienna to describe a special disease which we now know as adenovirus 8 keratoconjunctivitis or epidemic keratoconjunctivitis. How I got interested in it was through Dr. Finnoff. Dr. Finnoff, my chief, had been trained partly in Vienna, and Fuchs had told him about this disease, superficial punctate keratitis. Dr. Finnoff had never seen a case, however.

So when I got my first case of what is now known as SPK, I asked him what it was, and he said, "Oh, that's the Fuchs superficial punctate keratitis." Of course, it wasn't at all; it was a completely different disease. But I decided that the name, superficial punctate keratitis, characterized the disease so beautifully that I wanted to keep it, and then describe all the other punctate lesions as epithelial keratitis due to staphylococci, or whatever the cause, and retain this one disease. So I wrote quite extensively on this, and Dr. Braley in Iowa City wrote quite extensively, too. Then Barrie Jones in London objected to this term, SPK, and he added my name to it, to differentiate it from the other types of spotty keratitis.

Hughes: *So you have a disease named after you.*

Thygeson: I have a disease named after me, but not by me. [laughs]

Hughes: *Have people now accepted your nomenclature?*

Thygeson: Yes, now it's an established disease. When I write about it, I leave my name off, but very often my name is put on it by others. There's a question of whether Dr. Braley's name should be on it, too. He came in after I had showed him the cases, so he came in secondarily.

* JAMA 1950; 144:1544-8.

It's an interesting disease. It's undoubtedly a viral disease, but we haven't proven the etiology yet. Three different viruses have been isolated from it, but none of the three is the cause. A herpes virus, an adenovirus, and a zoster virus were isolated from the disease, but we're sure that none of the three is etiologic. We're still working on it.

Hughes: *Is it prevalent?*

Thygeson: No, fairly rare. I've probably seen fifty or sixty cases in my life.

The Trachoma-Psittacosis-Lymphogranuloma Venereum Group, 1951

Hughes: *The following year you published a paper on the trachoma-psittacosis-lymphogranuloma venereum group of viruses.**

Thygeson: That was simply a summary. Dr. Meyer objected to having trachoma mixed and in with psittacosis-lymphogranuloma venereum. He wouldn't accept trachoma as a member of the group.

Hughes: *Did he remain firm on that throughout his life?*

Thygeson: No, he changed.

Hughes: *When you presented the serologic evidence?*

Thygeson: What really changed his mind was the isolation of the trachoma chlamydia, and that was obviously the same group as psittacosis.

Hughes: *That was done by the group in China?*

Thygeson: Yes.

Hughes: *In 1945, according to this paper of yours, a Russian named Moshkovsky proposed the term chlamydozoaceae.***

Thygeson: He was a taxonomist, and he proposed having a group of chlamydozoaceae. He started the taxonomy, the use of the term chlamydia to take in the group. The rival was bedsonia, after Sir Sam Bedson, who cultured the psittacosis agent. They wanted to give him credit for the group—they called it the psittacosis group—and called it bedsonia. That went out of favor after a while. But Meyer touted that term bedsonia for a long time, and then he gave it up.

* The trachoma-psittacosis-lymphogranuloma venereum group of viruses: the chlamydozoaceae. *Am J Ophthalmol* 1951; 34:7-34.

** *Uspekhi Souremennoi* 1945; 19:12.

Cortisone and Experimental Herpes Simplex Keratitis, 1951

Hughes: You wrote a paper with H. O. Geller and Ariah Schwartz on "Effect of cortisone on experimental herpes simplex keratitis of the rabbit," published in 1951.* Who is Hildegard Geller?

Thygeson: She is the first technician I had in California. Schwartz was a resident. We showed that cortisone had a very deleterious effect on experimental herpetic keratitis. And then a number of papers arose at Proctor: Dr. Kimura and Mas Okumoto wrote a very good paper showing the same thing.**

Around the world there were quite a number of papers showing that cortisone and herpes didn't do well together. But this didn't get into the general ophthalmologic picture as it should have. The reason was that cortisone stopped all the symptoms of herpes, so patients were initially very delighted—until they lost their eyes.

Hughes: Was it this work on herpes simplex that was the first to warn you of the possible dangers of overuse of steroids?

Thygeson: What happened was that we started using steroids in San Jose on a group of herpetic cases. We had four in one week, and three did beautifully, and one lost his vision but not his globe. So that immediately warned me; that's why we went to the rabbit. We were the first, I think, to recognize the bad effects of steroids in herpes. That paper went around the world, and I think did quite a bit of good. Didn't get to everybody the way it should have, but it got to a lot of people. It got me into hot water with the drug houses.

Hughes: That was 1951. Is that about the time that steroids were coming into wide use?

Thygeson: In 1952 they became very widespread. In 1951 they were commercially available. In 1951 I had to get them from Sharing by way of Dr. Braley. Anyway, my biggest controversy was steroids, the heaven and hell drug.

Ocular Manifestations of the Dermatoses, 1952

Hughes: You presented a paper, "Ocular manifestations of the dermatoses," at a symposium in 1952.***

Thygeson: That was a review article, mostly based on the skin-eye problems that I'd seen in New York and San Jose. There were no original experiments done there.

Hughes: Were you unusual in ophthalmology for working closely with dermatologists?

* *Am J Ophthalmol* 1951; 34:885-8.

** SJ Kimura, M Okumoto. Effect of corticosteroids on experimental herpes simplex keratoconjunctivitis in the rabbit. *Am J Ophthalmol* 1957; 43, pt.2: 131-4.

*** *Trans Am Acad Ophthalmol Otolaryngol* 1952; Sept-Oct.:737-50.

Thygeson: I think it was unusual. I started working with a dermatologist in the army air force. In the Proctor Foundation, I had a very good tie-in with a young dermatologist, Francisco Allende, who unfortunately died of a heart attack when he was in his thirties. [tape interruption]

Criteria of Cure in Trachoma, 1952

Hughes: *In 1953 you published a paper called, "Criteria of cure in trachoma, with special reference to provocative tests." * The gist of the paper was differentiating between healed trachoma and trachoma of low activity.*

Thygeson: That was based on the finding by three observers, of which I was one, that steroids made trachoma worse. Hugh Ormsby up in Toronto, Canada, noted that steroids made inclusion conjunctivitis worse, so we found out that in the case of a totally healed trachoma, if we used steroids it wouldn't flare up, but if there was any residual trachoma the steroids would blow it up. We advocated using steroids as a means of deciding whether the trachoma was really cured or whether there was still latent trachoma, because there is a very low-grade trachoma that will last a lifetime and then blow up in old age. Just like tuberculosis will smoulder and then blow up in old age when your immune system becomes bad.

At that time it was very important that immigrants coming into the United States be trachoma-free. We were going to use the steroids to test out these questionable trachoma cases. The steroids had one advantage, that it's much easier to treat an active trachoma than a very mild trachoma. So that was the purpose of the paper.

It worked out that we didn't need this test because the immigration people threw out the trachoma deal. Trachoma no longer is important because there's a cure for trachoma now. One time it was considered incurable, like leprosy. So the test became obsolete.

Hughes: *Was it used for a while?*

Thygeson: Yes.

Nontuberculous Phlyctenular Keratoconjunctivitis, 1954

Hughes: *You were the sole author of a paper published in 1954, "Observations on nontuberculous phlyctenular keratoconjunctivitis." ***

Thygeson: I considered this an important observation because most cases of phlyctenulosis at this time were due to tuberculosis, but some cases were not due to tuberculosis. We decided that most cases of nontuberculous phlyctenulosis were of staphylococcal origin. There were other causes, too, like *Coccidioides immitis*, but they were very rare. The staphylococcal type did not respond to steroids, and the reason was that the focus of the infection was

* *Revue Internationale du Trachome* 1953; 30:450.

** *Trans Am Acad Ophthalmol Otolaryngol* 1954; Jan-Feb: 128-132.

in the eye. So when you used the steroids, you'd blow up the local infection. We had to get rid of the staphylococcus in the eye, and then the phlyctenulosis would subside. That's now generally accepted.

Cytology of External Ocular Disease, 1955

Hughes: Then you wrote a paper with Sam Kimura on "The cytology of external ocular disease." That was published in the AJO in 1955 and was really an updating of the paper that you'd published in 1946.***

Thygeson: Yes. We felt that the cytology was a very valuable adjunct to external disease examination because, first of all, you could tell right away between an infection and an allergy by the presence or absence of eosinophils. Then you could tell right away by the cytology whether you had a bacterial infection or a viral infection because the bacterial had polys [polymorphonuclear cells] predominant and the viral had mononuclear cells predominant.

Then in the case of the masquerade syndrome, where tumors masquerade as infections, a scraping would show tumor cells. That was an easy differentiation in the masquerade syndrome between a tumor and an infection. The other one was in herpes, because in herpes they have what's called the Tzanck test—after a Hungarian dermatologist—which shows multinucleated giant cells in herpes, which you don't get in any bacterial infection. That's still a very valuable test; the dermatologists use it all the time.

Hughes: Did the ophthalmologists pick up on your cytological analysis?

Thygeson: Yes, it's still being used all the time, because it's a very simple test for herpes.

Hughes: In this 1955 paper you made three new observations. One of them was on the Greeley type of keratoconjunctivitis.

Thygeson: Yes. That turned out to be an adenovirus 3 etiology.

Hughes: Did you realize that at the time?

Thygeson: Not initially, not in the first paper, but we recognized it shortly after the adenoviruses were recognized as a group of viruses.

Hughes: What is "Greeley type"?

Thygeson: Well, in Greeley there was a very large municipal swimming pool, and there was an epidemic of conjunctivitis that originated there. Swimming pool conjunctivitis is now almost entirely adenovirus. At that time, when this big epidemic occurred, an epidemiologist, Dr. Aiden Corkburn, had enough sense to draw sera, acute and chronic sera, which he stored. Then when the adenoviruses were worked out, he went back and showed that this was an adeno 3 infection. So what was called the Greeley conjunctivitis turned out to be cases of pharyngeal conjunctival fever or adeno 3 conjunctivitis.

* *Am J Ophthalmol* 1955; 39:137-45.

** The cytology of conjunctival exudates. *Am J Ophthalmol* 1946; 29:1499-1512.

Hughes: *The second new observation was made on Newcastle disease conjunctivitis.*

Thygeson: Newcastle disease, which is a viral disease of fowl—chickens and turkeys, birds in general—has a conjunctivitis that lasts about ten days, and it has a mononuclear [cell] exudate. So it was easy to diagnose on the basis of the history and cytology, as well as on the clinical appearance.

Hughes: *The third observation was on several inflammations due to fungi.*

Thygeson: Fungi originally were not very important in the eye since most of them were opportunists. The main thing was to recognize fungi on the basis of history and on the basis of cytologic observation. Fungi are easy to see with a microscope; they're large things, so all you need is the scraping; no problem in diagnosing.

Hughes: *The discussion of the paper was also published, and one discussant mentioned that it was probably better that the ophthalmologist examine corneal scrapings himself rather than send them to a lab, because most labs weren't set up to identify fungi.*

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Thygeson: The general laboratories are now doing much better, but at least in the early days the labs did not do well on ophthalmic material. It was much better to do it yourself.

Hughes: *Where were these labs?*

Thygeson: They're just general hospital laboratories, the laboratories that are available to the general practitioner. The technicians were not specialized. Mas Okumoto is a good example of a specialized technician.

Hughes: *But in the early days there were very few Mas's, weren't there?*

Thygeson: That's right. In our practice down in San Jose, I always did the cytology because I had been brought up on the microscope.

Hughes: *Did you eventually train technicians?*

Thygeson: Not in San Jose, but, of course, we helped to train Mas.

Cytologic Diagnosis of Trachoma, 1955

Hughes: *You published a paper in 1955 entitled, "The cytologic diagnosis of trachoma." **

Thygeson: There are many ways to diagnose trachoma by inspection, you see. If the disease had typical scars, it was very easy. But there are some cases that might give a little difficulty, and then the examination of the contents of the trachoma follicles would be important. The trachoma follicles are soft and easily expressed, so if with a ring forceps you press out the contents of the

* P Thygeson. *Revue Internationale du Trachome* 1955; 32: 421-36.

follicle and stain them with giemsa, it has a very characteristic cytology, with a predominance of large germinal central cells, which is entirely different from the nontrachomatous follicle.

This is one paper that nobody seemed to pay any attention to, and nobody ever confirmed or denied this one. The only paper I ever wrote in which I got no contest. It's a very valuable test, I think.

Hughes: *You also showed in that same paper how cytology could be used in the determination of cure of trachoma.*

Thygeson: Oh, yes, because in a case of trachoma that's cured you don't get any characteristic cytology at all. We always made scrapings before we decided to stop treatment entirely.

Etiology of Epidemic Keratoconjunctivitis, 1955

Hughes: *In a paper published in 1955 you talk about a new virus called the "Trim agent."**

Thygeson: Trimborn was a steward on the American President Line, and he showed up at the U.S. Marine Hospital with keratoconjunctivitis, and from this our group isolated a virus which turned out to be a new type of adenovirus, adeno 8.

Hughes: *In the paper there was a reference to APC viruses.*

Thygeson: That's obsolete terminology for the adenoviruses.

Hughes: *It must have stood for something.*

Thygeson: Adeno-pharyngeal-conjunctival. That really means type 3 adenovirus.

Hughes: *The Trim virus turned out to be the etiological agent?*

Thygeson: Yes, the prototype of the cause of epidemic keratoconjunctivitis.

Hughes: *Now I'm switching to papers that weren't in your collected bound works. They are the ones that you singled out as being important.*

Pharyngoconjunctival Fever, 1957

Hughes: *One published in 1957, "Sporadic cases of pharyngoconjunctival fever: In Northern California, 1955-1956.**"*

Thygeson: This just illustrated the fact that the adenoviruses could occur in isolated cases rather than in epidemics. So we could find mainly type 3 or type 8, but not necessarily in epidemic form.

* E Jawetz, SJ Kimura, L Hanna, VR Coleman, P Thygeson, A Nicholas. Studies on the etiology of epidemic keratoconjunctivitis. *Am J Ophthalmol* 1955; 40:200-9.

** *Am J Ophthalmol* 1957; 43:98-101.

Hughes: Were the virologists listening to you at this stage?

Thygeson: Yes. They listened to Dr. Jawetz mainly. Incidentally, Miss Hanna was the worker who did the actual cultivation. She should receive credit for the actual isolation of the virus.

Hughes: You said before that she was a remarkable technician.

Thygeson: Yes, she was very good. She had Ph.D. knowledge without the degree to go with it.

Epidemic Keratoconjunctivitis, 1957

Hughes: Then a paper the same year, with you as sole author, "Office and dispensary transmissions of epidemic keratoconjunctivitis,"* which we've talked about. That was the solution bottle problem.

Thygeson: Yes. We had an epidemic in New York and an epidemic in San Francisco. In the case of New York, the transmission was generally by way of the solution bottle, and in San Francisco the transmission was probably by fingers and by the tonometer, rather than by the solution bottle.

Hughes: Were you able to get the word out quickly and generally that this was an iatrogenic disease?

Thygeson: I had surprisingly little luck in that respect. I used to point out that in our office in San Jose we had many referrals of adenovirus infections sent to us, and we had no transmission in the office, for three reasons: we washed our hands between patients, we sterilized our tonometers, and we used separate droppers whenever we used solutions. Well, I taught that, but we turned up with an epidemic in the veterans' hospital in San Francisco where the transmission occurred in the glaucoma clinic. The reason it happened there was that there was no wash basin handy so there was no washing of hands between patients. So my teaching wasn't very valuable.

Hughes: I suppose there was resistance in the medical profession to the idea that it could be causing disease. Do you think that was perhaps part of it?

Thygeson: Well, it was really carelessness, which I didn't have because—I think I told you—Finnoff told me that one of my first duties was to wash my hands ostentatiously in front of the patient and between patients. He wanted the patient to see you wash your hands.

Recurrent Erosion of the Cornea, 1959

Hughes: There was a paper in 1959 entitled, "Observations on recurrent erosion of the cornea."**

* *Ophthalmol* 1957; 43:98-101.

** SJ Kimura, L Hanna, A Nicholas, P Thygeson, E Jawetz. *Am J Ophthalmol* 1957; 43:14-16.

Thygeson: We thought this was an important observation because in my early days I was taught that the proper way to treat a recurrent erosion was to cauterize it, either with carbolic acid or trichloroacetic acid. Of course, this cautery damaged the stromal fibers, and it turned out to be totally unnecessary. What we did was to remove all loose epithelium around the erosion with a little curette. Then we would patch the eye with a pressure dressing for forty-eight hours, and then we would put a little bland ointment in the conjunctival sac every night and caution the patient to open his eyes gently in the morning and not to tear the epithelium loose. Then we would follow the patient with the slit lamp until all signs of epithelial abnormality had disappeared. Then there would be no more recurrences. So when I left practice, there were no current cases; they had 100 percent healing rate. I avoided all the cautery. They still are teaching cautery in some places. I read to our fellows the other day an example of the use of cautery—totally unnecessary.

Hughes: *In this country?*

Thygeson: In this country.

Ophthalmological Problems of the American Indians, 1959

Hughes: *Then a paper with Dr. Dawson, published in 1959, "Ophthalmological problems of the American Indians." * Is that a review paper?*

Thygeson: This was a review paper which described principally the diagnosis of very mild and very early cases of trachoma in children, before the more obvious signs, like pannus and scars, had developed.

Hughes: *Did you use the corneal exudates as a tester?*

Thygeson: We used that as a help, but it was mainly observation. We could make the diagnosis especially by the development of micropannus, before gross pannus had developed. But we found there were many cases of early trachoma that were missed by the general medical men because they didn't recognize those early signs. That was the main purpose of the paper.

Inclusion Conjunctivitis, 1962

Hughes: *In 1962 you published a paper entitled, "Isolation of viruses from inclusion conjunctivitis in the newborn".***

Thygeson: This was one of the early isolations of chlamydia, and Miss Hanna actually did the real work on that. Her name really should have come first.

Hughes: *It does come first.*

* *Trans Pac Coast Oto-Ophthalmol Soc 1959:49-62.*

** L Hanna, J Zichosch, C Dawson, P Thygeson, E Jawetz. *Am J Ophthalmol 1962; 53:774-80.*

Chronic Conjunctivitis, 1963

*Hughes: Then just one more paper that you've checked as important, and that was written with Dr. Kimura and published in 1963 on chronic conjunctivitis.**

Thygeson: This was a paper in which we tried to determine the etiology of a large number of cases of chronic conjunctivitis. Perhaps the most important part was that we recognized a special syndrome of a chronic conjunctivitis that was characterized by upper tarsal papillary hypertrophy and by epithelial keratitis of the upper third of the cornea with occasional filaments. We called that chronic conjunctivitis with filaments.

Dr. [Fred] Theodore of New York discussed the paper. About six months after our paper at the Academy came out, Dr. Theodore called this entity superior limbal keratoconjunctivitis, and that's the name that has stuck. This was a new entity. Dr. Theodore deserves the credit for giving it the paper name.

Hughes: Are there any other papers that you want to mention?

Thygeson: I think that pretty well covers everything.

American Ophthalmological Journals

Hughes: I noticed in looking at your bibliography that a lot of your papers were published in the AJO. Was there a reason that you chose that journal?

Thygeson: I started in Denver helping Dr. Jackson, who was the editor of the AJO, and reviewing papers. In my early career I got associated with it; I was on the editorial staff. So I was more tied in with AJO than with the Archives of Ophthalmology. Those were the two American ophthalmology journals at that time. I later became acquainted with Dr. Knapp of the Archives, and then I published a number of things in the Archives, but the AJO was my favorite, and I get it free. I have to pay for the Archives.

Hughes: You get it free because of your editorial service?

Thygeson: Yes, because I was on the editorial staff.

Hughes: Do the Archives and the AJO reach different audiences?

Thygeson: They did. The AJO was much more clinical, and the Archives published a lot of basic science articles which were not accepted by the AJO. But it's pretty well ironed out now; they're both about the same.

Hughes: You were also on the editorial staff of the Survey of Ophthalmology.

* *Trans Am Acad Ophthal Otolaryngol* 1963; 494-517.

Thygeson: I think on the *Survey* only for a year or two, but I was on the *AJO* for a long time.

Hughes: *Did you have a lot of manuscripts to review?*

Thygeson: Well, I had plenty of manuscripts. It was a little difficult, I'd say. I was glad to get off.

Hughes: *How did the review process work?*

Thygeson: Initially everything depended on the editor; he just got advice from the reviewer, and he could take it or leave it. Then it was broadened so that usually three people reviewed a single manuscript, and it's become much more arduous than it used to be. I'm not sure that was a great advance.

At one time the *AJO* and the *Archives* had to compete for papers; there weren't enough papers to go around. Now there are too many papers to be published.

Hughes: *What is the burden of the Survey of Ophthalmology?*

Thygeson: I've kind of lost track of that. That has no research papers at all; it's just a good review journal. I haven't looked at a copy for years.

Hughes: *How do the Transactions of the Academy of Ophthalmology fit into the picture?*

Thygeson: That no longer occurs. The Academy has a new journal called *Ophthalmology*. The *Transactions* were important, but they could be republished in any other journal if you wanted to. Now you can't do that; the Academy has its own journal. It's a good journal now, *Ophthalmology*.

Hughes: *Does that go automatically to the entire membership of the Academy?*

Thygeson: Except the retired ones. I don't get it.

The Academy is a very expensive outfit. The membership is over a hundred dollars, annual dues, and then if you go to the meeting, they charge you another hundred dollars registration fee.

Medical and Scientific Organizations

The American Academy of Ophthalmology, The American Ophthalmological Society, and the Section on Ophthalmology of the American Medical Association

Hughes: *What are the differences between the American Academy of Ophthalmology, the American Ophthalmological Society, and the Section on Ophthalmology of the AMA?*

Thygeson: There's a big difference. The American Ophthalmological Society is the oldest and the most prestigious society, and its membership is limited. You have to write an acceptable thesis. So it has a smaller membership, and a lot of the old folks belong to it. Not too many of the young people come until they've been in practice for quite awhile or until they've been doing university work.

Hughes: *What did you write your thesis on?*

Thygeson: I wrote mine on ophthalmia neonatorum. I was the youngest member of the American Ophthalmologic Society for the reason that Dr. O'Brien, who was the chief at Iowa, told me I had to join. He proposed me, and Dr. Jackson seconded it, so I had to write the thesis, which I did, and it was passed. It wasn't a very good thesis incidentally. [laughter]

Hughes: *You had a lot of weight behind you.*

Thygeson: I had to do it, so I came up with it. But initially the American Ophthalmological Society was best because of its theses. It was not so good on the caliber of the individual papers; there were too many case reports and not enough serious studies. That has very markedly improved in later years.

The American Academy of Ophthalmology has improved very much. It originally was tied in with nose and throat; that was a real handicap. Now it's a separate organization. The Section on Ophthalmology of the AMA originally was the best—the best attended and the best material by far—but it has gone downhill. It just gradually deteriorated, as opposed to the Academy and the American Ophthalmological Society.

Hughes: *Anybody who's a member of the AMA can belong to a section?*

Thygeson: Yes.

Hughes: *Is that perhaps why it degenerated, too large?*

Thygeson: Yes, it's too large, and I guess that's the main reason it's gone downhill.

Hughes: *Is there a difference in audience and a difference in quality between the papers that are likely to be given at the Academy versus those at the American Ophthalmological?*

Thygeson: There used to be a difference in quality because the AOS had too many case reports, but that's gradually changed. The American Academy has gotten too big, so it's undergoing a bit of the trouble that the Section on Ophthalmology had. There's a tendency now for smaller meetings, and what happened is that at the American Academy the various groups in the Academy have set up their own sectional meetings, and that's damaged the Academy. So the corneal surgeons have one section, the microbiologists and immunologists have another section, and the uveitis people have another. That's taken away from the general meeting.

Hughes: Were any of these societies ever of particular assistance—or the opposite—to the Proctor Foundation?

Thygeson: No, not directly. In Colorado it was different because Dr. Jackson and Dr. [William H.] Crisp were editors, so at Colorado there was a clear-cut association with the *American Journal of Ophthalmology*. In New York Dr. Knapp founded the Knapp Foundation, and he had been editor of the *Archives*, so there was a close relation.

Now, out here there isn't any close relation, except Dr. O'Connor was on the editorial staff of the *American Journal of Ophthalmology*; he came in after I dropped out. So there was a relationship to Dr. O'Connor. At the Pacific Medical Center there is a much closer relation to the Academy with Dr. Spencer.

The Pacific Coast Oto-Ophthalmological Society

Hughes: What about relations with the Pacific Oto-Ophthalmological Society?

Thygeson: That's kind of a poor cousin to the AOS. It has turned into a kind of a social society where the members go mainly to have fun and to meet their peers, and the quality of the papers has deteriorated. Dr. Vaughan told me that they weren't even going to publish their transactions this year. So it's gone downhill. That's inevitable because you have eye, ear, nose and throat meeting together, and that's passe. So it's on the way out.

Hughes: There's no talk of splitting the specialties?

Thygeson: If there is, I don't know about it.

Hughes: Did you use to attend their meetings?

Thygeson: Yes, and I was guest of honor twice, I remember. Dr. Vaughan was president; Dr. Cordes was president; and Dr. Hogan was president. So its early history was quite good; it's just gone downhill.

Hughes: Have you given courses through the Academy?

Thygeson: Yes, I gave courses for twenty-five years.

Hughes: Are the courses usually associated with the annual meeting?

Thygeson: Yes, always associated with it, and very well done. They were started by Dr. [Harry Searls] Gradle of Chicago, and they've turned out to be a very valuable part of the Academy, more important than the general meeting. Dr. Spencer has been very effective in sparking these courses. The Academy also has a very good exhibit section. We at Proctor Foundation had numerous exhibits there, very good.

The Judicial Council of the American Academy of Ophthalmology

Hughes: Are you familiar with the functions of the Judicial Council of the Academy?

Thygeson: Not any more. I was vice-president at one time.

Hughes: What sorts of things did you do?

Thygeson: Well, we had all-day meetings, either before or during the regular meeting, in which all the various committees—the Academy had committees for everything—made their report, and the editor of the *Transaction* made his report. The Academy had a little endowment fund, and it distributed this money for various sources—a little bit of a grant-and-aid deal. It was kind of a boring day but I went through it several times.

In general the Academy is doing quite well now, but I haven't been to any of the meetings for the last few years.

The National Council to Prevent Blindness

Hughes: You were on the Scientific Advisory Committee of the National Council to Combat Blindness.

Thygeson: That was an organization which started first for the study of retinitis pigmentosa. It was started by a committee of sufferers from retinitis pigmentosa, and it's been broadened to include all other eye diseases.

It has been sparked by Miss Mildred Weisenfeld of New York, who is blind. She's still going strong, and she's pretty old now. I usually hear from her at Christmas. The Council gives out grants and aid which are usually from \$3,000 to \$8,000, rather small amounts, which are very useful for young researchers who are just starting. The idea is to get them started before they can get an NIH grant, and it's had a very good effect. The Proctor's had quite a few of those grants.

The Group in Ocular Microbiology and Immunology

Hughes: I read about the Group in Ocular Microbiology and Immunology, which I believe you set up very early. Did that coincide with starting the practice in San Jose?

Thygeson: I don't remember just when that was, but it was a national group that had a very interesting early history in the sense that one university at a time would be host. We hosted the first one, and we gave a dinner and provided all the facilities.

Hughes: At the Proctor?

Thygeson: Yes. And then I think we next went to UCLA, and we went to Florida University, and we went around the country. But finally it worked out as a part of the Academy of Ophthalmology program, coming before the general Academy meeting. But it was much nicer when we went around the country

to different universities because we saw what they were doing. The only trouble was that the expense of transportation and everything was too much, and it was much easier when you went to the Academy because it only cost one day more. But it was very successful.

Hughes: Was it your idea?

Thygeson: Yes. It's still going, and I think it's been very successful over the years.

Hughes: It doesn't publish its proceedings?

Thygeson: Only a summary, abstracts of papers, which appear in the *American Journal of Ophthalmology*. A lot of interesting work has been reported, usually in a preliminary form.

Hughes: What is the total membership?

Thygeson: Well, we usually run seventy-five to a hundred. Originally it was around thirty or thirty-five, but it doubled.

The Peninsula Eye Society

Thygeson: Dr. Beard and I decided that we should have a local group, so we started what we called the Peninsula Eye Society. It's still going; it's quite large now. Originally we had three members.

Hughes: You and Dr. Beard, and who was the third?

Thygeson: Phil Jordan, a local man from San Jose. Gradually we brought in more members; Dr. Bettman was a member.

Hughes: Was this ocular microbiology?

Thygeson: No, this was general ophthalmology.

Hughes: What happened at those meetings?

Thygeson: We always had a guest speaker who gave a regular paper, and then we had case reports. It was usually a single evening's program. I don't know what it is now, but it started as an evening program.

Hughes: Do most of the ophthalmologists on the peninsula come?

Thygeson: When I left it, it was all the ophthalmologists on the peninsula, also down in Gilroy, Watsonville, Santa Cruz. Gilbert Smolin is a member. He's from South San Francisco. So it took in the whole area, San Mateo and—

Hughes: As far south as Monterey?

Thygeson: Well, not officially. As far south as Watsonville, I would say. Often Monterey men came in, but not officially.

The Association for Research in Ophthalmology

Hughes: How active were you in the Association for Research in Ophthalmology?

Thygeson: I used to attend all the meetings, never failed, and I was chairman for one year and was on the council for three years. I received the Association's Proctor Medal in 1950. I got Mrs. Proctor to give money for a Proctor Medal, which is still in existence. It is given to established investigators.

Hughes: I didn't realize that was awarded by the Association.

Thygeson: Well, there are several Proctor lectures; there's only one Proctor Medal, and that's by the Association.

That was the only time Mrs. Proctor turned me down. When the price of gold went up, the amount of money she had given had not been invested in stocks so it didn't increase any, so there wasn't enough money to pay for the gold in the medal. So I asked her for an additional five or ten thousand dollars, and she refused me. She said, "Medals are for kids. No grown man would have anything to do with a medal." [laughter] That was it. She was uninterested. That was the only time she ever refused me.

Hughes: Is the Association effective in encouraging research in ophthalmology?

Thygeson: Yes, it's very effective. It's terribly important, I would say. It's probably the most important of all the societies for the actual basic research. Clinical research comes in particularly well in the Academy.

Hughes: Did you ever hold a position with the American Ophthalmological Society?

Thygeson: The only position I ever held was on the committee to review theses. I was on that committee for three years, but I was never an officer of the AOS.

Honors

Thygeson: I received the Howe Medal of the AOS at one time [1949].

Hughes: Was that given for a specific piece of research?

Thygeson: I don't think it was given for a special piece of research. The Research Medal that I got [1936] from the AMA Section on Ophthalmology was given for the trachoma work that I had done, and the Chibret Medal [1966] from the International Organization against Trachoma was for trachoma work. Then I got the Castroviejo Medal [1984] from the Castroviejo Society for the work on external disease.

Hughes: You don't remember any research being cited when you received the Howe Medal?

Thygeson: Well, that's a funny thing. I missed the meeting at which it was awarded, and it was one of the rare times I missed a meeting, so I was given the medal at the Academy by Dr. Dunnington later on, so there was no statement at the time I received the medal.

Hughes: *Would you say that is probably the most prestigious award in ophthalmology?*

Thygeson: Well, the AOS members think so. [laughter]

Hughes: *What would you say is the most important medal in ophthalmology on the international scale?*

Thygeson: I think the Gonin Medal in retinal detachment is probably the most important one.

Hughes: *But that can only be given in retinal detachment.*

Thygeson: Yes, that's true. I don't know any international medal that's special. There are a lot of international lectures that are very important. The medals have been given quite freely in this country, I would say.

The Pan American Ophthalmological Society

Hughes: *You were also a member of the Pan American Ophthalmological Society.*

Thygeson: Yes, and I gave papers at two or three meetings, but I wasn't an officer or on any of the committees, so I was never active in that society.

Hughes: *Would you say that Central and South Americans place the same emphasis as North Americans on research in ophthalmology?*

Thygeson: No, their emphasis is almost entirely clinical, but they have had some very good clinical meetings. They had one in Mexico City that was really good. They had one in Argentina which was not good [laughs]; I went to that.

Hughes: *So good training can be had in ophthalmology in Central and South America?*

Thygeson: I'm not sure about that. I don't think the quality of the literature coming out of South America is very good, but there are some very skillful ophthalmologists, surgically speaking. The Latinos are very good surgeons, incidentally.

Hughes: *That means dexterity?*

Thygeson: Yes, manual dexterity. Just like the Filipinos; the Filipinos are awfully good surgeons.

Hughes: *How about the Japanese? I think of them being dexterous.*

Thygeson: I don't know, because I've never seen Japanese surgical work, but I've admired the Filipinos.

Hughes: What is the problem with the work coming out of South and Central America?

Thygeson: Well, they haven't any institutes or laboratories that really are devoted to research. For example, Penedo Burneir Institute, the largest eye institute in the world, is at Campinas, Brazil, and they have laboratories, but they're vacant. So no laboratory research comes out of there at all.

Hughes: Is that due to lack of government funding?

Thygeson: I suppose so. I don't think they have any counterpart of the NIH.

The American Society of Microbiology

Hughes: You are a member of the American Society of Microbiology?

Thygeson: Yes, of course, that's a very important society. I used to go to the meetings regularly and never failed to get good information.

One of the most important things I got out of the meetings was the meeting in New York when Peter Olitsky and Albert Sabin gave a marvelous exhibit on toxoplasmosis in animals. This was before any human cases had been found. They showed the intracellular nature of this organism and gave the pathology very well. It was really a wonderful presentation.

I was lucky to see the first human case. The first human case was also in New York, a baby that died at about the age of three months with a massive involvement of the brain and retinas. Three pathologists, A. Wolf, D. Cowen, and B. H. Page, were able to show "toxoplasma" in the retina and brain.

Hughes: This was in the thirties?

Thygeson: Yes, 1937, and the interesting thing was that we were called from the eye institute at Columbia to examine this child, so we were able to see this massive involvement of the retina. We had no idea what it was; we knew it was a granulomatous lesion; so it was very exciting when the three pathologists came up with the "toxoplasma" finding.

I was lucky to see the second case, too, in St. Louis. I was visiting in St. Louis. Dr. [Henry] Pinkerton was a friend of mine from Harvard, and he had a case with marked lymph gland involvement. He had found "toxoplasma" in the lymph nodes, so he asked me to see the case with him. So I got to see the first and second cases of toxoplasmosis. This interested me because Nicolle had told me about and showed me material from the gundi, which is a little reptile in Tunisia in which Nicolle had first seen the *Toxoplasma gondii*. So I had three observations on "toxoplasma."

The Sensory Diseases Study Section of the National Institutes of Health

[Interview 7: February 4, 1987]##

Hughes: We mentioned in one of the earlier interviews your work with the Sensory Diseases Study Section of NIH, but I don't remember it being emphasized that you were chairman in 1955.

Thygeson: Yes, I was chairman there for two terms, I think.

Hughes: Is that a matter of appointment by the rest of the group?

Thygeson: No, it's an appointment by NIH.

Hughes: Was money readily available?

Thygeson: Well, there wasn't so much money, but there were fewer demands on the money. The NIH grants had not received proper attention, so the number of applications was small. So there was actually plenty of money considering the small number of applications.

Now it's really a mess because there are many more applications than money, so NIH has fallen into bad days as far as available money. The other thing is the change in the character of the support. For instance, clinical ophthalmology originally had some support by NIH, but now it's all really molecular biology; that's the big thing that takes up the money. A clinical research project doesn't have much of a chance for funding, except for AIDS.

Hughes: I read that you were chairman of the Vision Research Training Committee. When was that, and where did that fit into the scheme of things at NIH?

Thygeson: I have a hard time remembering that, but I think that was just an offshoot of the development of the Sensory Diseases Studies Section. The committee has changed its name several times.

About that time there was the development of training grants, which was a completely new deal, which allowed for great expansion in the training of ophthalmologists. That was all funded through NIH. It was a wonderful thing, but it got out of hand because pretty soon there were many more people trained than should have been trained. Now there are still some minor training programs, but they're very minimal compared to what it was at one time.

Hughes: Did the Proctor Foundation receive some of those training grants?

Thygeson: Oh, yes. Of course, the department of ophthalmology received more; about three quarters of the residency program in ophthalmology was funded by NIH. Proctor received funding mostly in the way of fellowship grants, for one or two years.

Hughes: Anything else about your work with NIH?

Thygeson: The NIH is still helpful to Proctor. There's an NIH training grant now in progress for fellowship training in basic sciences, but it's at a much lower level than formerly. Inflation is such that the cost of supporting fellows and salaries has doubled or tripled or quadrupled, so the same amount of money doesn't go very far. Just for example, when I was research director at Columbia our whole program, including salaries and everything, was funded on \$80,000. Now \$80,000 doesn't pay the salary of one professor. So you can see the terrible role of inflation, which has been very hard in the last few years on the Proctor Foundation. Salaries have gone up, but income has stayed more or less stable.

The Estelle Doheny Eye Foundation

Hughes: You were a trustee of the Estelle Doheny Eye Foundation of Los Angeles.

Thygeson: I was for five years in the 1970s, but I resigned. The monthly trip to Los Angeles took a whole day, and I just felt I couldn't afford that.

Hughes: Is that foundation set up in any way that's similar to the Proctor Foundation?

Thygeson: It has some similarities, but it's really a service organization, not a research organization. They do research, but it's minor compared to the actual services that they do, whereas in the Proctor Foundation, research is the number one thing and service is secondary. But the original money was just the same; both started on a \$500,000 endowment. Doheny has received money from the large Doheny Foundation, so it's done pretty well financially. And then it gets a lot of money from service, from the private practice doctors, which we don't have at Proctor at all. So it differs in many, many respects.

Hughes: What was your primary role as a trustee?

Thygeson: I was supposed to take the know-how that I'd gotten at Proctor to Doheny.

As far as I can remember the only good thing I did was to help persuade the foundation to move in with the University of Southern California instead of being totally isolated at St. Vincent's, a Catholic hospital, where it started. I think that's the best thing I did. I felt that Doheny had to be tied in with a university if it was going to have long-term success. It couldn't survive as part of a Catholic hospital—not that I'm anti-Catholic. [laughs]

Mrs.

Thygeson: It was really frustrating; I can tell you that.

Hughes: To try to get them to understand the need for university affiliation?

Mrs.

Thygeson: To do anything. Very, very tight Catholic hierarchy there.

Thygeson: Well, St. Vincent's did very well for Doheny in the very initial stages, but then the foundation outgrew the hospital. It just became evident that there wasn't any future for Doheny except with a university, and the University of Southern California was the logical place because the original founder of the Doheny Foundation was A. Ray Irvine, who was formerly a professor at the University of Southern California. UCLA already had the Jules Stein Institute, so it didn't need the Doheny. It was logical, and it's worked out very well. Doheny is doing very well right now.

Hughes: *Does the Doheny have the same relationship with the department of ophthalmology that the Proctor has?*

Thygeson: It has a much closer relationship than we have with the department of ophthalmology.

Hughes: *Because the Doheny is mainly a service organization?*

Thygeson: Yes. The clinical side is much closer than the research side.

Consultant to the Food and Drug Administration

Hughes: *Another thing that you have done is a consultancy with the Food and Drug Administration which began in 1973.*

Thygeson: Yes. I didn't do so well there. [laughs]

Mrs.

Thygeson: You did pretty well.

Thygeson: I was advocating two things: I wanted to get rid of mixed drugs, where there's more than one drug in an ointment or a solution; I think those are very dangerous. And I wanted to minimize the use of steroids, which are very dangerous for the eye. I wanted to prevent the distribution of samples containing steroids to all the doctors, pediatricians, and general practitioners, because these doctors didn't know anything about the eye. Faced with a red eye, they would put steroid-containing material into the eye, and then after the damage has been done they would call the eye doctor. My purpose in going to the FDA was to try and prevent that. I got nowhere in cutting out the mixed drugs.

Hughes: *What was the argument for mixed drugs?*

Thygeson: Economy, so the doctor wouldn't have to write two prescriptions, the patient wouldn't have to buy two different ointments, and it would be easier to control. The pharmaceutical outlook at that time was to cure the infection with an antibiotic and stop the inflammation with a steroid, neither of which really worked out because the antibiotics have not been as successful as originally thought. The steroids are a heaven-and-hell drug, doing temporary good and long-term damage. So the combination was very bad. The only thing I got out of it was that I stopped the distribution of free samples containing steroids.

Hughes: *That's a considerable accomplishment.*

Thygeson: But I didn't have any luck in stopping the manufacture of combined preparations.

Hughes: *What role was the pharmaceutical industry playing in this?*

Thygeson: Well, at the meeting one prominent doctor got up and said, "We have to be very careful because this is a fifteen-million dollar industry, and we don't want to hurt it." I resented that because I thought the safety of the eye was more important than the fifteen-million-dollar industry.

Hughes: *You became a consultant to the FDA because of wanting to make an inroad in these particular areas?*

Thygeson: I was invited to join, and then I made myself a little obnoxious by doing something that would damage the pharmaceutical industry income. So the drug houses were unhappy with my presentation—as you can imagine.

Hughes: *How long were you a consultant for the FDA?*

Thygeson: A few years, not very long, in the late 1970s.

The International Congress of Ophthalmology

Hughes: *Did you usually go to the meetings of the International Congress of Ophthalmology?*

Thygeson: I went to a number of them, but I didn't go to every one. The international congresses were a mixed bag because they had so many people there, and the papers were so short—ten minutes allotted to a paper—that you really couldn't get anything out of them. What you really got was to meet the various people, and you could get more out of a conversation than you could out of the meeting. So in a way it was more economical to stay home and read the papers as they came out. They'd be a year or so late, but you'd eventually get the right thing.

Not all the conferences were bad; they varied. Some were remarkably well done, and they split up into sections so that more time was allowed to some features that you were interested in. It was a smaller group, and you could ask questions. I think I told you [off tape] about the smallest group, on the prevention of blindness, in the 1937 International Congress in Cairo, when at eight o'clock in the morning there were only three people starting off the meeting.

Hughes: *Beginning in 1950 the Congress introduced panel discussions. Did you ever participate in any of those?*

Thygeson: I think I did, but I can't remember. I remember a panel at the Pan American Congress, but I've forgotten about the International. I think I can only remember three or four of those. I presented short papers, but I don't remember too much about a panel.

Hughes: *You and Roger Nataf wrote a paper for the eighteenth International Congress in 1958.**

Thygeson: He was from the Institute of Ophthalmology of Tunis.

Hughes: *You reported on the etiology of trachoma. Do you remember that paper?*

Thygeson: Yes, I remember that paper very well. That was a mixed-up paper, because Nataf and I were so far apart, and we had different ideas. We were designated by the committee to get together, so we got out a joint paper which really had some funny aspects.

At that time I knew what the etiology of trachoma was, because we had done the elementary body studies, but Nataf did not know, and he was under the supposition that the elementary bodies were rickettsiae. So you can imagine what kind of a mixed paper that was.

The Registry of Ophthalmic Pathology

Hughes: *Have you had any particular dealings with the Registry of Ophthalmic Pathology in Washington?*

Thygeson: Only in the sense of comparative ophthalmology. I contributed a few cases, and then I have all the material showing animal models of ophthalmic disease. I was always in touch with Dr. Zimmerman, who was the chief there, and he was always an authority on pathologic materials. So if we got into trouble, we'd always consult him. He's still living somewhere; I don't know quite where; he's retired. He was the great authority on ophthalmic pathology.

Hughes *He wrote a very nice history of the registry.***

Thygeson: Yes, we were tied in with him in one way because he and Dr. Hogan wrote a book on pathology.***

Dr. Zimmerman spent a lot of time out here, and we knew him and his wife very well. We also knew his predecessor, Helenor Campbell Wilder Forster, who was the first ophthalmic pathologist at the Armed Forces Institute of Pathology. She's still living here in San Francisco.

Hughes: *Is she a physician?*

* P Thygeson, R Nataf. Etiologic problems in trachoma. *Revue Internationale du Trachome* 1958; 2:83-227.

** LE Zimmerman. Registry of ophthalmic pathology, past, present and future. *Trans Am Acad Ophthalmol* 1961; 65:51-113.

*** MJ Hogan, LE Zimmerman. *Ophthalmic Pathology*, 2nd ed. Philadelphia: WB Saunders Co., 1962.

Thygeson: No, she was a technician who married Colonel William H. Wilder, who was the director of the Armed Forces Institute. Forster was her second husband; he was a lawyer. She was largely self-trained, but she's one of these people who really went to town. I don't know what she got in the way of degrees, but I'm sure she got a lot of honorary degrees.

We have a number of examples of people like that. In New York we had an immigrant who started as a janitor in the New York Eye and Ear Infirmary who became so important and so valuable in teaching that they awarded him an honorary medical degree. He was an immigrant from Hungary or some place like that. He had no regular degree, but he had all these honorary degrees.

Mrs.

Thygeson: One of the interesting things was that all Helly's colleagues in medicine with M.D.s on their names, and ophthalmology—that's three years more—would say, "Have you asked Helly? Have you shown it to Helly?" They always wanted to know what she thought.

Thygeson: She was very highly respected.

Mrs.

Thygeson: They were all crazy about her.

Thygeson: There have been about four or five women in ophthalmology who have been unusually successful.

Helly married Mr. Forster and then moved to San Francisco. She was a consultant at UCSF for many years, and then she retired. [to Mrs. Thygeson] She must be in her nineties, don't you think?

Mrs.

Thygeson: Older than we are?

Thygeson: I think so. The first time I met her I thought she was middle aged, and I thought I was pretty young. [laughter]

Mrs.

Thygeson: You're probably right; I just haven't seen her for so long.

Thygeson: She's been retired from the Armed Forces Institute for many, many years, and they gave her a big send-off at the time she retired. That was a long, long time ago.

Honors (continued)

Hughes: *We've talked about the medals that you've received. Is there anything you'd like to add?*

Thygeson: I told you what Mrs. Proctor said about her medal.

Hughes: Yes.

Mrs.

Thygeson: Medals are kids' stuff.

The Chibret Gold Medal for Trachoma Research, 1966

Thygeson: I have the Chibret Medal, which was from the International Organization against Trachoma, and then the Castroviejo Medal from the Castroviejo Society. I have five medals.

Hughes: *What is Chibret?*

Thygeson: Chibret is a French pharmaceutical firm that supported the International Organization against Trachoma and paid for the journal, the *Revue Internationale du Trachome*.

Hughes: *When did Chibret's support start?*

Thygeson: It goes back quite a ways, to the early days of the International Organization against Trachoma. Chibret was kind of the NIH of France; it funded a lot of meetings and research programs. The French didn't have an NIH. The Chibret Medal doesn't mean too much. It's awarded every year.

Hughes: *Just in trachoma?*

Thygeson: Yes. Dr. Dawson has it. There were two original things: The Proctor Foundation was awarded the medal, and that's the first time an organization was awarded the Chibret medal. The other thing is that it's made of gold, and it's unusual for the French to allow any gold to get out of the country. [laughter]

The Castroviejo Medal, 1984

Hughes: *Tell me why the Castroviejo Medal was awarded, and also about the Castroviejo Society.*

Thygeson: The Castroviejo Society was in honor of Ramon Castroviejo, who was a pioneer in corneal transplantation, so it's really a society of corneal transplanters. I don't know how I got in on this because I've never done a transplant on a human eye. The medal was given to me [1984] for research in external eye disease.

Mrs.

Thygeson: He never liked surgery.

Hughes: *Although you did a fair amount of plastic surgery at one time.*

Thygeson: I did a lot more assisting in surgery than the actual surgery. I always assisted Dr. Beard in plastic surgery, so I had my hands in the operating room an awful lot, but I wasn't a real surgeon in the sense that my surgical practice was limited, mainly muscle surgery. I let Dr. Beard do all the cataracts because he was so much better than I was.

Hughes: Was that a matter of dexterity?

Mrs.

Thygeson: Judgment.

Thygeson: Dr. Beard was really a natural-born surgeon. He knew how to use his hands and his brains both. I was a bit of a worrywart about my surgery because I worried too much about unhappy side effects and so on. He was a better surgeon because he didn't worry. But I worried more about surgery than I did about anything else.

Hughes: Weren't you doing plastic surgery at Valley Forge and then later out here?

Thygeson: Yes, we were doing surgery every day in those times, and I did my share of surgery, but I did more assisting.

Mrs.

Thygeson: Just never did enjoy it.

Thygeson: I didn't enjoy plastic surgery; I didn't like putting in all those sutures. It could take an hour or an hour and a half to put in sutures, and I really got tired of that. Got tired of taking out the sutures, too. In those days every suture we put in we had to take out.

Hughes: Isn't there a difference in temperament between a research person—which I think of you as being—and the surgical mind?

Thygeson: A surgeon is born a surgeon, like Dr. Beard, because he really loved it. And the corneal surgeons love it. It's really the difference between a surgeon and an internist; there's a big difference between a surgeon and an internist. Ophthalmology's supposed to be a surgical specialty, but actually surgery's only about twenty percent of the ophthalmologist's work. There's much more medical ophthalmology than surgical ophthalmology, and I was always more interested in the medical side. I never wanted to be a surgeon; I just got caught up in it in ophthalmology.

Hughes: It's probably telling that your initial impulse was internal medicine, don't you think?

Thygeson: Yes. I was not a born surgeon.

Hughes: That twenty percent surgery that you spoke of in ophthalmology today, that's less than it has been historically, is it not?

Thygeson: It varies. Certain people would become great surgeons, like Dr. Wheeler and Dr. Knapp in New York. Their surgical practice would build up in referrals so that they would soon be doing more than the usual twenty percent. A few of them did nothing but surgery, and they would have associates do the refraction and everything like that. That was pretty rare, and still is pretty rare, but it does happen. For instance, Dr. Maumenee in Baltimore developed a big surgical practice, and I doubt if he does any other type of ophthalmological work.

The Distinguished Service Award of the International College of Surgeons, 1963 ##

Hughes: *In 1963 you received a distinguished service award from the International College of Surgeons.*

Thygeson: I just received it, that's all; I didn't do anything about it. Surgery was not my field. I didn't seek this award in any way.

Certificate of Commendation from the Public Health Service, 1971

Hughes: *In 1980 you received a plaque and a certificate of commendation from Dr. Emery [A.] Johnson for your fifty years of assistance to the Indian Health Service.*

Thygeson: That's something I'm proud of. As I can show you, it's not given too often by the Public Health Service. It covers the fifty years of service that I had as consultant for the Indian Service. So that goes way back to about 1927 or '28 when I started working with the Indians. In fact of all the things that I've gotten, that's the most interesting to me because it really represents an awful lot of work in those fifty years.

Physical Diagnosis

Hughes: *I believe you wanted to say something about the importance of physical diagnosis.*

Thygeson: I wanted to say that in my teaching program—which is really based on a weekly conference which was started in Colorado and kept on all through the war and right up to the present time—I've always stressed the importance of physical diagnosis. One reason is that I had such good training in physical diagnosis at Stanford Medical School. So I taught and I wrote about all the eye signs that can be used in diagnosis.

In a way it's much more important to make a correct diagnosis than it is to treat, because diagnosis doesn't change, whereas treatment changes periodically. Furthermore, if you make a correct diagnosis you can usually give the correct treatment, whereas if you get the wrong diagnosis you're going to do the wrong treatment. So my opinion was that every Proctor

fellow ought to be grounded in real study of physical diagnosis so that he could use the history and laboratory findings secondarily, but everything really based on physical diagnosis. That's what I'm still teaching.

Hughes: What is your feeling about present-day emphasis on laboratory tests?

Thygeson: I think they've gone overboard on the laboratory, because if you know your physical diagnosis you can usually make a very good diagnosis and prognosis just based on history and physical diagnosis, and the laboratory is a pure supplement; you don't rely on that supplement.

But in general medicine now the laboratory and technical aids have overbalanced physical diagnosis, which is sad because physical diagnosis has slipped. The young doctors don't know physical diagnosis. They make diagnoses on the basis of laboratory findings, and laboratory findings can be wrong. There's a much greater chance of error in a laboratory procedure, unless it's done two or three times. You can diagnose a thyroid disease that doesn't exist on the basis of a laboratory finding. In fact, there was a good article in one of the journals on the diagnosis of nonexistent disease. It was based on faulty laboratory findings which convinced the doctor that there was a disease that actually didn't exist.

One example is my own case after I'd retired, when I was reappointed research professor for three years. You had to have a physical exam each time for reappointment, and the electrocardiogram showed that I had cardiac damage, arterial blockage in one of the coronary arteries. I was reappointed anyway in spite of that, but then the next electrocardiogram showed that didn't exist, so I never had any heart trouble at all. I have had several cardiograms since then without any—so I could have been diagnosed in a faulty way just on the basis of a laboratory test.

I've always felt that it's wrong to make a diagnosis on the basis of one laboratory finding. The social implications can be terrible. Can I tell you an anecdote about New York?

Hughes: Yes, please do.

Thygeson: Dr. Reese referred a young man to me with a diagnosis of gonorrhoeal ophthalmia. He'd been treated by Dr. Reese with a sulfonamide. When he got to me I couldn't get any culture, so I couldn't prove that he'd had gonorrhoeal ophthalmia. But this young man was told that he'd had gonorrhoeal ophthalmia, so he was trying to figure out where he'd got the gonorrhoea. He had recently been married to one of the socialites in New York, so he thought he'd gotten it from one of her friends. His wife heard about it, and this led to a divorce. It undoubtedly was not gonorrhoeal ophthalmia; it was meningococcal ophthalmia. So here was a faulty laboratory examination—

Hughes: Leading to social tragedy.

Thygeson: Yes, and this has happened two or three times in my practice, where meningococcal ophthalmia has been confused with gonorrhoeal ophthalmia.

We had a hospital suit in which the parents of a baby with an ophthalmia sued the hospital because of the negligence on the part of the hospital to let the baby get this infection. This infection turned out to be inclusion blennorrhoea—not gonorrhoea—and I was able to show that it wasn't the hospital that was negligent; it was the parents who had contracted the chlamydial disease who were negligent. So I saved the hospital a couple hundred thousand dollars.

There are quite a lot of cases in my experience where a faulty laboratory diagnosis can lead to a lot of trouble. I feel that a large number of eye diseases can be diagnosed on physical examination alone, and furthermore you can tell which diseases are an emergency, which should be treated right away, and others which should have no treatment at all or which you can take your time treating. If you didn't know your physical examination findings, you could get into trouble. You could miss an emergency, or you could overtreat a thing that's going to get well anyway.

Hughes: Are residency programs today not emphasizing this approach?

Thygeson: That's right. I think there's a real deficit in the training of residents. Even here in our own university I think there's faulty training on that. I can't do anything about it, just deplore it, but I'm glad that I was able to train quite a few people that way. I had a nice letter just the other day from Dr. Phenizy Calhoun who's retired from Emory University in Atlanta. He was one of our fellows in New York that I helped to train. He became very good in external disease, and I have had a number of letters from old fellows and residents who seem to have appreciated the training I gave.

The program is that of the training pie, surgery now gets the number one slice, refraction gets the bottom slice, and external disease gets the next to the bottom. So in the priority of the training deal, external disease and refraction are the poorest taught. Actually for the general ophthalmologist who's out in practice, it's refraction and external disease that furnish most of his patients and income. If you're a good refractionist, you can survive and practice even though your surgery may not be so good. But people appreciate good refractions.

Hughes: Is there any move within the various ophthalmological associations and societies to change the emphasis?

Thygeson: Well, there's some minor evidence of that, but in general the emphasis has all been put on corneal surgery, which is to me the least important of all because if you have to do corneal surgery that's the end of the deal. You ought to be able to prevent the necessity for corneal surgery. That's one reason why I'm emphasizing preventive ophthalmology rather than curative ophthalmology. You can prevent more diseases now that would lead to corneal surgery.

Fee-splitting

Hughes: In the first interview you referred to a fee-splitting problem in San Jose which you and Dr. Beard eliminated, but you didn't go into any detail.

Thygeson: In the early days of ophthalmology there were two unfortunate rackets. One was the fee-splitting between optometrists and ophthalmologists when the optometrist referred a case to an ophthalmologist for surgery, and the other one was the kickback to the ophthalmologist who prescribed glasses. The optician would kick back the difference between the wholesale and retail price of the glasses; he'd give the difference to the ophthalmologist. In my view, and in the view of Dr. Edward Jackson, this was dishonest because the patient didn't know this. If the patient had known it, it wouldn't have been so bad because fees for refraction were low, so that the combined thing was just about the right amount for refraction anyway. It would have been all right if the patient had known.

The fee-splitting was all the way through medicine, because the general practitioner referring a case to a surgeon would get a kickback from the surgeon. The lawyers would do that, too; if they'd refer a case to a trial lawyer, there would be a kickback to the referee. Well, you see this was unethical and bad, and to be a member of the American Ophthalmological Society you had to swear that you would not accept kickbacks for surgery or rebates for glasses. The same thing is true of the American College of Surgeons; to be a member you could not accept either one of these two rackets. Also to be on the American Board of Ophthalmology you had to swear that you did not engage in these two practices.

Dr. Jackson crusaded to the whole Rocky Mountain area; you couldn't be a member of the Colorado Ophthalmological Society if you accepted rebates. Well, Dr. Jackson had such prestige that he was able to clean up the whole area.

When Dr. Beard and I got to San Jose, this rebating and everything was rampant, and we just cleaned it up.

Hughes: How did you do that?

Thygeson: We got next to Dr. Fred Borden, the president of the county medical society, who was a nose and throat. We got him on our side and he, as president of the county society, really cleaned it up. The rebating there was pretty bad. The pharmacies would rebate to the doctor; the funeral homes would rebate to the doctor, and the orthopedic appliance people would rebate. This was really a disgrace to the profession.

Hughes: Did Dr. Borden eliminate the problem simply by making it impossible for membership in the county society if you were doing it?

Thygeson: It wasn't written down, but, like Dr. Jackson, he had this tremendous local prestige, and he was very vocal. So at the county society meetings he really told them. We really got it started, but it was Fred Borden who carried it on. As far as I know there's nothing going on like that in San Jose at the moment.

Hughes: *What about elsewhere in the country?*

Thygeson: I suppose that the fee-splitting is going on, but I don't know about it.

San Francisco Bay Area Ophthalmology

Hughes: *When we were talking about the Proctor Foundation, I don't believe we mentioned its role in the division of ophthalmology at UCSF becoming a full department.*

Thygeson: What's now the department originally was a division of surgery and the prestige of the Proctor Foundation was enough so that it enabled Dr. Cordes to request and receive status as a department. So it was the Proctor Foundation that provided the prestige to do that.

Hughes: *Do you remember when that was?*

Thygeson: It was within a year after the Proctor was founded, probably 1948 or '49.

At Stanford, ophthalmology is still a division of surgery.* And that's a joke, because it never should be in surgery; it should be in medicine, because most ophthalmology is medical. It's a joke when they make it a division of surgery; it's wrong. Ophthalmology at Stanford really hasn't lived up to expectations because of that. It's inferior compared to UC or Pacific Medical Center. Pacific has really come up in a big way because the dean of the school, Bruce Spivey, is an ophthalmologist. Spivey is the one who really brought up Pacific, and now Bill Spencer is the big man behind the throne there at Pacific. So with Spivey and Spencer, Pacific has really gone ahead, and it's got a very fine training program and a good research program. They say they think they're doing better than UC. [laughter]

Hughes: *Is the difference between being a division and a department simply clout?*

Thygeson: No, because as a division of surgery everything has to go through the chief of surgery. Ophthalmology always gets squeezed because it's the bottom of the list, the bottom of the surgical specialties. You just can't go anywhere financially unless it's a department. That's what's happened at Stanford.

Hughes: *Why has ophthalmology at Stanford tolerated that situation?*

* In 1988, the division achieved departmental status.

Thygeson: They haven't had the ear of the dean, for one thing, and then the division heads in ophthalmology at Stanford have been pretty weak sisters. The last strong one was Dohrmann Pischel. He was the last one that had any clout. He was good, but he was not forceful in the sense that Fred Cordes was. Fred could really push and fight and yell and scream.

Hughes: *From what Dr. Pischel told me, he himself wasn't particularly convinced of the necessity for the division to become a department.* He felt that the division was getting pretty much what it needed—of course, that's many years ago.*

Thygeson: Dohrmann lacked the perception that Fred had that ophthalmology had to expand into all the subspecialties. So it was a matter of perception. Dohrmann was a surgeon and not interested in medical ophthalmology, and Fred was not a very good surgeon but terribly interested in pathology. Fred was underrated. He didn't have anywhere near the brains that Dr. Hans Barkan had, but he had wonderful drive.

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Thygeson: He wasn't the cultured gentleman that either Dohrmann or Hans Barkan was. He was a poor boy who really developed. He was a difficult character of the old German-type school, but he knew what he wanted, and he went after it.

Before he died he did receive quite a lot of recognition. He became president of the American Ophthalmological Society. But for a long time he was underrated. He was so very good when we were setting up the Proctor Foundation because he understood the importance of a foundation with a mission.

Hughes: *Do you think that some of the present success of ophthalmology at the Pacific Medical Center is because nowadays it is more closely tied in with research? I'm thinking of the Smith-Kettlewell Institute.*

Thygeson: Yes. [Arthur J.] Jampolsky, the institute director, is a real go-getter, too, a little bit on the order of Fred Cordes. He's pushed and gotten money and developed programs, rather remarkable.

Hughes: *Is there an attempt to tie in the program of the department of ophthalmology and Smith-Kettlewell?*

Thygeson: Oh, yes, they've got a good relationship there. But Spivey is the main spark for the whole program.

Hughes: *Is he a man who appreciates the necessity of the basic sciences in ophthalmology?*

Thygeson: I think he's done pretty well because there's quite a lot being done there. Spivey has a national outlook; for instance, he's really taken over the Academy.** The Academy's really a one-man deal now. It's always been

* See the interviews in this series with Dr. Pischel.

** Dr. Spivey is executive vice-president of the Academy.

that way; there's always been one man who really sets the standard for the Academy. Harry Gradle was one of the first, and there was another man from Mayo, Bill Benedict. Harry Gradle and Bill Benedict ran the Academy, set all the standards. So everything the Academy is was really started by these two men.

Hughes: Has Dr. Spivey changed the Academy's direction?

Thygeson: Yes, he's changed the direction, I think, much more in teaching special courses, and then literature and medical care. Spivey's had quite a broad outlook on the Academy. The Academy's now the number one eye society in the country.

Hughes: Usurping the place of the American Ophthalmological Society?

Thygeson: Oh, yes, and also the Section on Ophthalmology of the AMA. The Academy is now number one. It didn't used to be. When I went in, the Academy was looked down upon because it was eye, ear, nose and throat, a combined deal. Most ophthalmologists look down on anybody doing nose and throat. I think I told you what Dr. Finnoff said about the specialties.

Hughes: I don't think you said it on tape.

Thygeson: My first chief, Dr. Finnoff, looked down upon eye, ear, nose and throat as a combined specialty he could not tolerate. He rated the specialties with ophthalmology first, and when down the list and got down to urology and proctology, and the next one was chiropractic, and at the bottom was nose and throat. [laughter] That was a common feeling, so the Academy was very much handicapped by its association with nose and throat. Of course, they're now separate.

Hughes: Why did it take so long; they've only been separated since 1979.

Thygeson: They took so long because they developed quite an endowment, and there was a terrible squabble over the division of the endowment. As eye came up, nose and throat was going down, but nose and throat wanted half the endowment. So nose and throat put up a big fight to prevent the division of the two. I really don't know how they divided the endowment, but it was a financial squabble.

Hughes: The Pacific Coast Oto-Ophthalmological Society remains a dual society.

Thygeson: Yes, it remains a dual society, and it's gone downhill in a big way. It's now a social society.

Hughes: Do you think it's because of the membership of the two different specialties?

Thygeson: Yes, it's no longer possible to combine the two.

Optometrists

- Hughes:* Do you have any comment to make about the relationship of ophthalmologists, optometrists, and opticians, other than what you've already said about fee-splitting?
- Thygeson:* There's always been a squabble between the optometrists and the ophthalmologists because the optometrists want to take over eye problems just the way dentists have taken over the mouth—oral surgery is now a dental thing. Optometry wanted to do the same thing for the eye, take over everything—particularly medical ophthalmology and surgery—and do exactly as the dentists did. The dentists did it very successfully; they just took over. You never have a nose and throat man fooling around in the mouth, but they used to. Oral surgery used to be done by nose and throat men, but not any more. The dentists have been very successful in doing that, and the dental oral surgeons are at least as good as any of the M.D. surgeons. It was a very successful takeover, I would say. The optometrists have not been successful.
- They tried in Australia to take over, and for awhile they were successful. They prevented the appointment of anybody with an M.D. as a professor of ophthalmology in any of the Australian universities. That was overruled fairly recently. In Berkeley, the [University of California] School of Optometry has been very successful, and it can grant doctorates and other advanced degrees, which the department of ophthalmology at UCSF cannot do. In the army, the optometrists have taken over eye care, so the optometrists have been partially successful.
- Hughes:* Has it always been a crusade of the ophthalmological societies to try to restrain the optometrists?
- Thygeson:* There's always been an attempt to restrain them. There are two societies that I know of that are really devoted to trying to control the optometrists and prevent them from prescribing medications, but with only moderate success. There are some states that allow the optometrists to write prescriptions even though they don't have an M.D. degree. I used to be worried about it, but in my old age, it's like state medicine, I've stopped worrying about it.
- Hughes:* Apparently in 1953 there was a controversy when the AMA put forth revised principles of medical ethics. One thing the new ethics called unethical was dispensing glasses for profit.
- Thygeson:* Yes. This followed the rebate deal. When the rebate deal became illegal, then the doctors countered by forming their own optical companies and making money out of glasses surreptitiously. It was really the same deal. The AMA tried to stop that but not with great success.
- Hughes:* Was it the AMA that originally tried to stop the rebate business?
- Thygeson:* Yes. The AMA was really responsible for that. Rebating I think is illegal in all states now.

Hughes: This is rebating in general, not just in ophthalmology?

Thygeson: I don't know about the other specialties, but I know it's in ophthalmology. There's very little rebating going on in general. For instance, all that racket about the kickbacks on drugstores, I think that's all been eliminated.

More on American Ophthalmological Societies

Hughes: When you refer to the AMA, are you actually meaning the Section on Ophthalmology?

Thygeson: Yes. The section used to be very, very important. It's gone downhill. It did a lot of good things. Dr. Jackson told me that he had gone to fifty consecutive AMA meetings.

Hughes: The fact that it's gone downhill, you said before, was largely because of numbers, but is there more to it than that?

Thygeson: I think the section didn't allow enough time for delivery of papers and wasn't progressive enough, not like the Academy with all the ancillary sections and exhibits. Ophthalmology was a minor affair in the AMA meeting, and people were much more interested in having just a pure eye meeting. The Academy just overran; it's now the number one. Everybody goes to the Academy—except me.

Hughes: Where does that leave the American Ophthalmological Society nowadays?

Thygeson: That still goes on. It's a prestige organization, and it's improved; much better papers than there used to be. At one time I remember Dr. [Jonas] Friedenwald complaining about the American Ophthalmological Society. He said, "There was no paper presented there that couldn't have been presented twenty-five years previously." He was a real basic science research man, and he couldn't stand the case reports that were coming out of the AOS. But it's improved very, very much.

Hughes: Is there any particular reason for that improvement?

Thygeson: Well, there's more competition, the younger group coming in, and it's still a very prestigious organization, and everybody wants to be a member. Dr. Dawson just now applied for membership, and he has to write a thesis. He wasn't going to have anything to do with it until he became director. Now to be a director of the Proctor Foundation without being a member of the AOS is unthinkable. To be a chairman of any department, you really have to be a member.

Hughes: He was reluctant to belong before?

Thygeson: Yes. He decided not to be, and Dr. Vaughan decided not to be a member.

Hughes: Because of its poor reputation?

Thygeson: Yes, and because it was very expensive. The admission fee was very high, and the meetings were always held in very expensive places. It just cost too much money considering what you got out of it. In my case I was told by Dr. O'Brien, "I'm proposing you for the AOS, and now you write a thesis," which I did.

Hughes: He thought membership was necessary to get ahead in ophthalmology?

Thygeson: He wanted every member of his department to be an AOS member. It was because that was the prestigious place. So I wrote the thesis, and I became the youngest member of the AOS.

Hughes: Is the thesis really considered seriously both by the person who writes it and the people who receive it?

Thygeson: Oh, yes, that was the one good thing all the way through. There was a thesis committee set up, and it was usually composed of the few very well-trained ophthalmologists in basic sciences, and they were very critical. I was on this committee just before the war started, and I was horrified because the committee turned down all the theses that were submitted that year. I thought that was going a little too far.

Hughes: What numbers are we talking about?

Thygeson: Six or eight. I thought there ought to have been one or two out of that that could get by.

Hughes: Do you remember who else was on that committee?

Thygeson: I remember Dr. [Francis Heed] Adler was the chairman of the committee. I don't know how my thesis got through, but it went through all right. I guess the committee didn't understand what I'd written [laughs].

Hughes: Is there any feedback about the thesis?

Thygeson: You can do it again if you fail the first time.

Hughes: But they don't comment in specific about the content of the thesis?

Thygeson: No.

Medical and Ophthalmological Training

*Hughes: In 1953 the Council on Medical Education and Hospitals of the AMA decided that all prospective ophthalmologists must take a year's approved internship. At the same time it decreased the number of approved internships, and in a paper that I read by Alan [C.] Woods, he seemed to feel that this was harmful to ophthalmology. **

Thygeson: Yes, I remember talking to him about that. He felt that ophthalmologists should be picked out right out of medical school. Alan put a lot of emphasis to the teaching of ophthalmology to medical students. Then he would pick out the brightest and the best and offer them a residency right out of medical school, without going through internship, and he thought he got the best candidates. He thought if they got into an internship, then the surgeons or the medical people would steal the high-class candidates and take them away from ophthalmology.

He had a point there, because there were some good people lost to ophthalmology who got steered into other specialties or into surgery. My point was that a doctor who hadn't had an internship was totally unfit to take care of a patient. I thought an ophthalmologist who hadn't had an internship was about like an optometrist. He wouldn't recognize the importance of systemic disease in ophthalmology. So in a way I thought that the ideal thing was one or two years internship before going into ophthalmology.

Hughes: Wouldn't there also be a problem in convincing the faculty of the medical school to leave enough room for ophthalmology in a medical student's education?

Thygeson: That was always a fight. Ophthalmology was often at the bottom of the list, and in some universities there wasn't any ophthalmology teaching at all. In some places, like Iowa and Hopkins, because of O'Brien and Woods, then they'd give a lot of emphasis to teaching ophthalmology, but generally speaking ophthalmology was slighted. The trainee coming out of medical school had practically no knowledge of ophthalmology. It was really bad, I think. I think the internship is very important.

I had a rotating internship; in one year you went through all the specialties, surgery and medicine. That was better than nothing, but one year is hardly enough. I felt that I had been slighted on general medicine, but then in the army I got really good training in general medicine. The army experience was very good for me.

Hughes: Did the fact that Stanford in the thirties awarded the M.D. after the internship year indicate that it thought that the internship year was essential?

* AC Woods. The role of the Academy in the changing pattern of medical practice. *Trans Am Acad Ophthalmol Otolaryngol* 1955; 608-16.

- Thygeson: Yes. That was really a function of Dr. [Ray Lyman] Wilbur, who was a professor of internal medicine before he became president of Stanford. I think his feeling was that a medical student is unfit to practice until he's had at least a year of hospital experience.
- Hughes: *I know that system was true at UC as well, but was that common practice elsewhere?*
- Thygeson: No, it wasn't, and Hopkins didn't have that, and I thought it was bad.
- Hughes: *I believe that all American medical schools nowadays award the M.D. degree after four years of medical school.*
- Thygeson: They do, but as far as getting a license to practice, that's a different matter. Almost everybody takes one or two or three years of postdoctoral training. Somebody going out to practice ophthalmology without postdoctoral training is really unheard-of now.
- Hughes: *In this paper by Alan Woods, he intimated that the AMA had always acted in the interest of ophthalmology. Was he thinking of more than this problem of the cream of the crop being siphoned off to other specialties?*
- Thygeson: That was his main idea. He successfully utilized this idea because he picked out the good ones, and he had remarkable success. The Hopkins boys were chairmen of departments all over the country. He was probably the most successful of the chiefs of ophthalmology in distributing his residents around the country.

The Doctor-Patient Relationship and the Placebo Effect

- Hughes: *Would you say something about the type of relationship that you sought to establish with your patients?*
- Thygeson: I tried to teach the art of medicine, which was that half the battle was the patient's relation to the doctor, and that the doctor acted as a placebo. The doctor assumed the responsibility for the care of the eye disease, took that burden away from the patient and the family. For a good doctor, half the battle was establishing the proper relationship with the patient. If the patient was just another case, why, the patient felt that and the family felt that, and it was an unsatisfactory tie-in.
- Since the specialty of psychoimmunology has been established, we know that the doctor is the placebo, that a good doctor can handle a patient and disease in a much better way than the pure research doctor, who just looks at everything as another problem in research.
- Hughes: *So you've always stressed the importance of a personal relationship with patients?*

Thygeson: Yes, and I was always interested in the placebo effect, because many diseases are susceptible to the placebo. It doesn't matter what medication is used, as long as the patient and particularly the doctor think that it is effective.

Herpes is a disease that's very placebo-related. Dr. Hogan and I ran a series on aureomycin borate. We were convinced on the basis of a Johns Hopkins lecture by Dr. Perrin Long, who was the principal infectious disease man in the country, that aureomycin, a new antibiotic, was the answer, that it was a broad-spectrum antibiotic that had a remarkable ability to handle bacteria and larger viruses. I really believed it at that time.

So Dr. Hogan and I set up this series on herpes, that we would treat cases of dendritic keratitis with aureomycin borate three or four times a day for at least a week. If there was no response, we'd go on to the regular treatment which, in my case, was denuding the epithelium containing the virus. But what we found out was that all our patients were getting well in less than a week, and we were really excited about this because it was a remarkable effect. Then when we treated aureomycin against herpes virus in the mouse it didn't work. So it was all placebo.

Now we know that placebo elevates the immune system just the way stress reduces immunity, and herpes virus is very susceptible to placebo. Some diseases are not. For instance, with gonorrheal ophthalmia you can't get anywhere with placebo.

I've always been interested in the placebo. Dr. O'Connor used to complain because my treatment was usually to remove the drugs that the patient had been on and then to give the patient the diagnosis and prognosis. Usually the prognosis was good because I knew what the natural history was, and the drugs he'd been using were full of side effects, so they did better without the drugs. Dr. O'Connor used to complain that I was going too far on this placebo effect. I took away more drugs than I gave.

Hughes: Did aureomycin prove to be effective against large viruses?

Thygeson: No. It was just another broad-spectrum antibiotic, like terramycin or tetracycline.

Hughes: Why do you suppose the idea originally arose that it was effective against large viruses?

Thygeson: Probably this placebo effect; they probably used herpes as a test.

The Proctor Los Altos Library

Hughes: Would you say something about the Proctor Los Altos Library?

Thygeson: The Proctor Los Altos Library was set up on the basis of my own personal library that I'd collected over the years, plus the books that I was buying as part of the teaching program. I always have one or two new books to present at the seminar. I paid for this out of my social security income.

When I left practice there were no contributions from doctors to social security; we were not a part of the social security unit. But in the work I'd done for the Indian Service and for NIH and so on, they had been subtracting a certain amount of money for social security. I hadn't paid any attention to that, so I was surprised when I got a check for—

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Thygeson: —\$300 a month, which is the lowest form of social security, and I used that to buy books. I'm still doing that in a minor degree, so I added those books to what we have here at home, and then in connection with the seminar I would send away for all pertinent reprints. I had a student to do the bookwork. He'd send away, and then I'd get these reprints, and we used them for teaching. So I added these all to our library here, and I got a lot of donations of historical manuscripts and so on. I've got the barn out there filled with reprints—thousands of them. Then I gradually fell off on that; I don't send away for reprints any more. If I see anything, I just duplicate it in San Francisco.

Hughes: *What about books?*

Thygeson: I'm still buying books, but in a minor degree. What I'm doing is putting a hundred dollars a month into what we call the Ruth Lee Thygeson Fund, under Alta California Eye Research Foundation, and then she can buy any pertinent books that can be used for teaching. She's interested in editing and speech, so our initial books—three or four of them—have been to do with medical editing. She's taken that on, and she's bringing in the editor, James Ransom, from the Lange Publishing Company to give a little course to the fellows. So that's where part of my social security money's gone.

Hughes: *What eventually will become of the library?*

Thygeson: My hope is that when we get the Heintz Laboratory established, the chief of the Heintz Laboratory will have charge of the library here at home, and what he or she does with that I don't know, but it'll be available. It will be ancillary to the Harry William Hind Library. The Harry William Hind Library has limited value because there's limited space available, so all the old stuff is thrown out after five years—reprints and books and everything. I've been collecting those and putting them in my barn. Dick O'Connor filled his basement with discards. I guess they're still in his basement. But I have most of them here.

My hope is that the combination of the Heintz Laboratory, the Harry William Hind Library, plus our library here will make a research and historical library that will be available. I have a lot of historical books that I collected. I've always been interested in medical history.

Hughes: *Your collection is wider than just ophthalmology?*

- Thygeson: It goes into medical history, yes. I've got a lot of interesting medical history that deals mainly with microbiology and the history of infectious disease, in addition to infectious eye disease. I was going to be a microbiologist, so I started collecting everything about microbiology.
- Hughes: *You mentioned Marie Hui-Shi Feng off-tape. Do you want to say anything about her?*
- Thygeson: Marie Feng was the wife of a fellow of mine at Columbia. Her husband, whose name was S. P. Chang came to me at Columbia from the Peking Union Medical College in China and worked in my laboratory in external disease. The first thing I knew his wife arrived unannounced. She really took over the laboratory. She was a forceful woman. [laughs] You could write a book about her, and Dr. Vaughan could tell a lot about her because he knew her back in China.
- I followed her all these years, and she's retired up in Canada. She was going to endow a fellowship for Chinese women. She wrote Dr. Vaughan that she was going to do that, but nothing developed. She was a very successful and skillful practitioner.
- Hughes: *Was it unusual to have a woman in a prominent position in ophthalmology in China?*
- Thygeson: I suppose it was. She was good, and she was a little bit like Madame Chang-Kai Chek, the same type of forceful woman. She was very beautiful and very imperious, and she demanded respect and attention. She had Dr. Vaughan running circles for a while. Courtney Vaughan didn't like it too much. [laughs] We still keep in touch with her; Dr. Vaughan keeps in close touch with her. She did some research in our laboratory, but I don't remember that it amounted to too much. She was really a very good clinical ophthalmologist.
- Hughes: *Are you speaking of the laboratory at Columbia?*
- Thygeson: At Columbia, and she also did a little work for about six months in California on trachoma. I don't remember that any publications came out of that.

Changes in Ophthalmology

- Hughes: *Would you like to say something about the changes, trends, and major advances in ophthalmology over your career?*
- Thygeson: Of course, there's been a tremendous change in ophthalmology over the last fifty or sixty years, and in my specialty of external disease, we had big changes. In gonorrheal ophthalmia we would lose eyes; it was unusual to be able to save an eye. Now, of course, you shouldn't lose an eye. In infectious disease we had corneal ulcers, particularly with the pneumococcus, and we had no means of treating them successfully, so we had scars resulting from those that now can be avoided.

We used some procedures that were useful. For instance, in pneumococcal ulcer we would cauterize the advancing border of the ulcer, and this was helpful, and then we'd do a paracentesis which would bring in the secondary aqueous that had antibodies in it, which was helpful. Then a little later we found that optochin, which is ethyl hydrocuprine, one of the first chemotherapeutic agents and still being used as a test specific for pneumococcus—we found that we could use the powder in the eye, but we couldn't use it systemically; it was too toxic for systemic use.

Hughes: When did that come in?

Thygeson: I guess when I was in Iowa [1931-1936]. It was really the first specific treatment that came in, and it was useful. Then later on we had type-specific pneumococcal rabbit antiserum.

Hughes: How effective was that?

Thygeson: Reasonably effective if we had the right type. We had to type all our antisera. We had a special typing for it. It was called the swelling technique; the capsule of the pneumococcus would swell if you had the right type antiserum. So we could type a pneumococcus directly from the lesion. We had to use rabbit antiserum because the antibody in the rabbit was small enough so it would get into the cornea. We couldn't use the other types of antiserum.

Then when sulfonamides came in that was the first real advance, and then when penicillin came in this was a complete change. In the early days diphtheria of the eye was rampant, and we don't see that at all now. One interesting thing to me was that herpes simplex, involving the eye in the form of the dendrite, was nothing but a fever blister and got well by itself or by placebo in most cases. Now it's a terrible thing because of overtreatment, particularly with steroids. Many infectious diseases have changed character. Scarlet fever often involved the eye, and you got a membranous conjunctivitis; you don't see that any more.

Hughes: Are these changes due to the widespread use of antibiotics?

Thygeson: In some cases. I don't know why scarlet fever has disappeared; I often wonder. I know why diphtheria has disappeared, because we have immunization. There used to be a lot of meningococcal meningitis, and in certain cases of meningitis you would have a vitreous abscess; the infection would extend into the eye, and you'd have a vitreous abscess. You don't see that any more.

So there's quite a number of changes, but big changes occurred in about 1938 when the sulfonamides came in. The big change for the worse was in 1952 when the steroids came in. All the opportunists that we knew existed but didn't cause any disease now became disease producing. The corneal ulcer, which was once caused by just pneumococcus, now every opportunist you can name has produced a corneal ulcer, all on account of steroids.

Hughes: What about changes precipitated by new instruments in ophthalmology?

Thygeson: We've always had a problem with the instrumentation. For instance, instruments producing radiation were used quite widely in ophthalmology in the beginning. Vernal catarrh was treated with radium—very bad. Temporary success, long-term terrible results. X-rays were used frequently in dermologic conditions around the eye, which would result in cataract.

Hughes: *This was in the course of your career?*

Thygeson: Yes. Dr. Jackson felt terribly bad about the use of radium on vernal catarrh because he knew the disease got well by itself with time. They were using radium and producing scars; it was terrible. He realized that, and he was one of the first to believe in preventive ophthalmology. Jackson was way ahead of his time.

Hughes: *How late do you think radium and x-rays were used in the treatment of diseases of the eye?*

Thygeson: They were used for quite a number of years.

Hughes: *After the war?*

Thygeson: Yes. Eventually they were abandoned. Cataract was one of the reasons for abandoning them. Radiation was used for the treatment of recurrent pterygium and tumors, and a lot of cataracts developed from the treatment. Radium adapters were used, were put on the eye, for the treatment of pterygium, and we had temporary success because vessels were obliterated, but then the long-term effect was bad.

Hughes: *Cancer as well as cataract?*

Thygeson: Radiation started being used in cancer early on.

Hughes: *Did it perhaps induce cancer as well?*

Thygeson: It might have in regard to leukemia, but I don't think it induced it in much else. There was a lot of unwise medication. I think I realized generally what was going wrong and generally avoided it myself.

I remember doing one bad thing. There were a lot of papers on the use of radiation of the thymus to prevent thymic death in children undergoing operations, such as tonsillectomy or for muscle difficulties, and we didn't know then the role of the thymus at all. I had never used x-ray; I knew that there was something wrong about it, but this one nurse demanded that I use it when her own child was having a muscle operation. I got a radiation man to do this, and I knew it was wrong. Nothing happened that I knew about, but this was absolutely the wrong thing to do because the thymus is tied in with immunity, and it was terrible to irradiate the thymus; it was very wrong.

Hughes: *Did you realize the immunological tie-in?*

Thygeson: No, not at that time. The thymus was just a body—I had no idea what it was for.

Hughes: *What was the origin of your skepticism about the use of radiation?*

Thygeson: Because I'd seen the long-term bad results. I was always kind of anti-therapy; I didn't want to use anything that I didn't know the side effects.

Hughes: *[laughing] I would say that's a theme of these discussions.*

Thygeson: Anyway, it wasn't very long before Robert A. Good was the one to show that the thymus was absolutely essential in the immune process.*

Hughes: *This was postwar?*

Thygeson: Postwar. I remember reading the first papers by Good.

Mrs.

Thygeson: I remember telling a new doctor that you were a therapeutic nihilist. He said, "Well, he shouldn't be."

Thygeson: I got into some trouble about it; people thought I went too far overboard in cutting off medications. Dr. O'Connor worried about my therapeutic nihilistic tendency.

Hughes: *What was his particular concern?*

Thygeson: I thought that uveitis was far overtreated, and in my early days I was getting much better results with minimal treatment. Dr. O'Connor was giving all the modern treatment. For instance, steroids were used by him then, and toxoplasmosis was treated by two medications including steroids. I was doing better without them.

Hughes: *So Dr. O'Connor is not as skeptical about the use of steroids as you are?*

Thygeson: He gradually learned. The interesting thing was when he developed ulcerative colitis, the treatment for which usually was steroid enemas, and I knew that that was wrong. I urged him not to submit to that, and he didn't, and he's doing very well now.

Mrs.

Thygeson: That nearly lost us our grandson, Nels Marcus Thygeson.

Thygeson: Yes, steroid enemas caused bowel perforations in my grandson. The gastroenterologist didn't understand the melting effect of steroids on collagen. We did in the eye, but the gastroenterologist didn't yet know that. Perforating corneal ulcers are a dime a dozen because of the use of steroids.

Ernest Lawrence of the Lawrence Berkeley Laboratory died from perforations of ulcerative colitis at Stanford Hospital. They didn't realize this side effect, and he perforated and died of peritonitis. I remember Dr. Saunders saying that Lawrence died a steroid death. He should never have had the treatment.

* RA Good, HE Gabrielsen, eds. *The Thymus in Immunobiology*, New York: Harper and Row, 1964.

I think I was successful in getting Dr. O'Connor to avoid steroid treatment. He's thanked me for it several times. I had more success in removing medication than I had in treating.

Hughes: Do you have anything to say about the decline of the general ophthalmologist today?

Medical Ethics

Thygeson: I think that there's been a tremendous decline in ethics and in medicine in general. In my early days there was a feeling of service, that the doctor had an obligation to see his patients get well, and monetary considerations were minimal. Nobody went into medicine to make money because it wasn't a very lucrative field, and then this obligation of doctors to the clinic patient...For example, Dr. Knapp, who was a pioneer in ophthalmology in New York, insisted that the doctor should make his money in practice in the morning, and the afternoon he should devote to free clinics or to research or to university work. That didn't last too long; when I got to New York [1936] that was on the way out. But the history was there.

I remember the man who was my co-director, Jack Dunnington, would work Saturday afternoon at the free clinic at New York Eye and Ear Infirmary in the early days. Devoting a Saturday afternoon to a free clinic—nobody would do that now. So there's been a decline in medical ethics.

This was before I was in medicine, but the story went around about James J. Hill, the great railroad tycoon. His wife had appendicitis, and he wanted the best, and the best was the Mayo brothers. So he had the Mayo brothers come up to St. Paul, and they did the operation up there very successfully. Then James J. Hill was so pleased that he sent the Mayo brothers a check for \$10,000, and they returned it and said, "We make our own charges," and they charged him \$25,000. [laughter] The program then was that every patient was investigated according to their income, and they were charged according to their income.

Nobody charged a patient in advance. The wealthy patients supported the poor patients. That isn't true any more. Every office took care of free patients. In San Jose we had a certain number of free patients.

Hughes: Where would you put your finger on the source of this decline in ethics?

Mrs.

Thygeson: This greed?

Thygeson: It's all through society now. The lawyers are the worst. It's all through the professions; there is a decline in public service. There are islands of service you can always find, but in general in the professions money comes first; service comes second. That's true of the country as a whole. There's a falloff in morality. In the old days there was peer pressure to keep you in line. Then, of course, parents were strict. For instance, Ruth Lee's mother wouldn't let

her go out by herself until she was sixteen years old. My mother didn't want me to go out with stenographers. [laughter] So you see what the change has been.

Just the other way around now. Now the kids have peer pressure to do the wrong things.

That permeates society, and I don't know what you can do about it. All you can do is try to teach morality. Fortunately, the Proctor Foundation fellows are really immune to that.

Two Papers on Steroids, 1960 and 1950

Thygeson: Here are two papers that I think may have a lasting effect. The first one was a paper in the *Transactions of the American Ophthalmological Society* entitled, "The unfavorable effect of topical steroid therapy on herpetic keratitis."* This was 1960, and I think this was the first paper showing the unhappy aspects of steroid therapy in herpes.

Hughes: *Did people pay attention to the publication at the time?*

Thygeson: To a certain extent, but not anywhere near as much as I had hoped. The other paper is just the opposite, showing the favorable effect of steroids in eye disease, "Cortisone in the treatment of phlyctenular keratoconjunctivitis."** Dr. Fritz of Anchorage and I were the first to show that steroids had a remarkable and immediate beneficial effect when used topically in phlyctenular keratitis in the Eskimo. At that time the Eskimo had blinding phlyctenulosis. We were able to completely reverse the cases of phlyctenulosis in the Eskimo even before tuberculosis had been controlled.

Hughes: *What had been the treatment for phlyctenulosis before the appearance of steroids?*

Thygeson: There had been no effective treatment at all. In fact, the only beneficial effect that I knew of was to improve the nutrition of the child so that the tuberculosis would improve. But we had no topical treatment that was of any value except the use of atropine that cut down the photophobia of the disease.

The steroids had a miracle effect, stopping the phlyctenulosis overnight, and only requiring two or three days use, so we didn't get into any complications at all. These papers show the two effects of steroids, one anti-allergic in phlyctenulosis, and then the very unfavorable effect in herpes. So as I look back, those were the two papers that really show the extremes of the use and misuse of steroids.

* P Thygeson, MJ Hogan, SJ Kimura. *Trans Am Ophthalmol* 1960; 58:239-62.

** P Thygeson, MH Fritz. *Am J Ophthalmol* 1950; 34:357-60.

Major Contributions

Hughes: If you had to pick out two or three other contributions in the course of your career, what would they be?

Thygeson: I think one was showing, before the chlamydia had been cultivated, that the elementary body of the trachoma inclusion was the cause of the disease. That was probably the most important thing. Then the other thing I look back on is the Proctor Foundation and the development of a training program in external disease. That was one of the important things. I look back on the overall good cooperation we had with the University of California, and the good cooperation we've had from the residency programs at Iowa and Columbia and California. Also the students in the Lancaster course in Maine sent very nice letters showing that they at least appreciated some of the material we were able to teach.

I've been very lucky because I had good partners in San Jose who let me travel and do research that I couldn't have done if I were in practice by myself. Then the fact that my wife always backed me up and was not a money-grabber. She always supported me and worked with me, always went to the Proctor Foundation with me, still does. Then my two partners who stayed with me, Dr. Beard and Dr. Vaughan. I think I've been very lucky.

Ruth Lee Thygeson

Hughes: Because Mrs. Thygeson is reluctant to speak for herself, perhaps you can elaborate on the role that she has played in your career.

Thygeson: We were married when I was a sophomore in medicine. When I began writing papers, she always edited them. So I was able to turn over pretty rough material, and she really made a paper out of it. Every paper I've written, she has edited. Then she extended her editing to many of our colleagues—Dr. Vaughan and all the Proctor fellows at the University of California. So she's had quite a career in editing, and she's still going strong. Without her cooperation I would have been in trouble.

She is a wonderful manager, too. The only reason we have a place at Lake Tahoe and have this place here is that she managed all the family finances. I never was any good at that type of management.

Hughes: Did you seek her advice when you were director of the Proctor Foundation? I'm thinking particularly of financial matters.

Thygeson: Not so much on financial matters, but every other time we had her advice. In the founding of the Proctor Foundation, she was very important. She was a good friend of Mrs. Proctor and Dr. Proctor. She typed all the letters and papers having to do with the origin of the Proctor Foundation, so we think she's very important. In the few times that we've gotten into trouble in the Proctor Foundation, such as the attempted takeover by the

department—we've had two attempted takeovers, like the industrial takeovers—we were able to fend them off, and she was very active in that because she had all the papers concerning the foundation of the Proctor.



c. 1980s. 1 to r: Dr. Daniel Vaughan, Dr. Max Fine, Dr. Mathea Allansmith, Dr. Thygeson, Mrs. Thygeson, and Dr. Richard O'Connor the third Director of the Proctor Foundation.

More on the Proctor Foundation

Hughes: I remember your talking about one which Dr. Saunders was involved in, but I don't recall the second one.

Thygeson: The second one was when Dr. [Julius R.] Krevans became dean. He decided that the Proctor Foundation should belong to the department of ophthalmology, so he called a meeting to do this, and we were able to stop that.

Hughes: By pointing to the original documents?

Thygeson: Yes. Of course, there isn't any future problem on that because the university program now is that the donor's wishes are paramount. You can't change the mission of the donation without action of the Regents of the University of California and going through the attorney general of the state. So that cuts

out any further attempts, and we will never have any further attempts that way. That's a big relief, and that's due to the university attorney, Jim [James E.] Holst, because he's laid down the law on that. Nobody's going to try a takeover again.

Incidentally, I can't say too much how important the Regents' attorneys were, because we've had three wonderful attorneys in Berkeley who, whenever we got into trouble, came to the rescue. Even on this last thing where the Proctor Foundation had this \$500,000 deficit, Mr. Holst came to the rescue, and I may say that Chancellor Krevans also came to the rescue at that time, so we're coming out of that.

Hughes: How did they help you solve that problem?

Thygeson: They advanced money for the audit so we didn't have to pay for the audit, and they made an arrangement for paying off the \$500,000 in five years. That's what we're doing right now; we're down to about \$200,000. It would have been pretty sticky if we hadn't had cooperation. Now we're going to prevent any comparable thing because we're going to have an audit every two years, and we're going to have a board of governors' meeting twice a year, just to prevent any future problem that way.

Hughes: In the past how often has the board of governors met?

Thygeson: Once a year, but it was a kind of a token deal. They took the word of the director. Chancellor Sooy told us that our mission was supervisory and let the director direct. Well, we let the director direct a little too much. It was an unfortunate situation.

Hughes: So now at the board of governors' meetings the board actually goes over the books?

Thygeson: Yes, and then in one more year we'll have an audit of all the books.

Hughes: The audit prior to this has not been done on a regular basis?

Thygeson: No, the first audit we ever had was when Dr. O'Connor resigned. We had no problem when I was the director because we were always running a surplus; we always ran a twenty or thirty thousand surplus.

Hughes: Whose idea was it to have a periodic audit?

Thygeson: Well, I'd say it was my idea. I took the brunt of this \$500,000 because I'm the treasurer. I should have demanded that I get an examination of the books, but Dr. O'Connor was very secretive and didn't want to show the books.

Hughes: And there'd been no history of need.

Thygeson: Well, the NIH had partially dried up, and we knew that we were spending more than we had, but I didn't anticipate the amount. My figure was between fifty and a hundred thousand. I was completely wrong. I don't think Dr. O'Connor realized that either.

Dr. O'Connor was not a good administrator, and he had terrible help. I had good help. Really the burden of the administration depends on what used to be called the administrative assistant. It's now got another name, but it's the same idea. Usually it's a woman who really has control of everything, and the director is just kind of supervisory. I had a marvelous administrative assistant, Rose Dow, and she's still living. I didn't appreciate her properly when she was there, but I do now.

I might say a word about the Heintz Laboratory. It's the hope of the Proctor Foundation because it will have a good laboratory space, and it will have adequate funding, and it will have the services of the Cecilia Vaughan fellows, which is very important, and also the services of the fellows in the regular Proctor program.

We're just in the process of developing a program for the Heintz Lab, and we should get that underway shortly. This will make it possible to have one more good laboratory working on the mission of the Proctor Foundation. Originally all we hoped for was one good laboratory. The expansion of Proctor was due to NIH. Now that NIH is drying up, Proctor has to retract and get more private funds. But I think basically with the current endowment, which yields about a half a million dollars a year, plus the Heintz endowment, which will probably be equal, we will have a million dollars of hard money to work on every year, plus what we can get from NIH and private funding.

So the Proctor Foundation is in decline, but not dead. It's inevitable that it would have a decline because of the aging staff. You have to replace them with young people, and right at the moment we cannot replace them because of finances, but we will. So the future's good. It's like the opposite of steroids: The steroids have a good present but bad future, and we have a bad present and good future.

One other thing concerns our book on preventive ophthalmology that's in the works. We figure that about sixty percent of eye disease in the third world is preventable, and about thirty percent of eye disease in the developed world is preventable. So one of the missions of the Proctor Foundation, and particularly of the Heintz Laboratory, will be the expansion of the work on preventive ophthalmology. There's a lot of good work on preventive medicine going on now, so we're going to do the same thing for preventive ophthalmology. As far as I know there's only one other eye unit that's working on preventive ophthalmology, and that's at Hopkins. My last days will be on preventive ophthalmology.

One of my current interests is the Alta California Eye Research Foundation, started some fifteen years ago by Dr. O'Connor, Ruth Lee, and me, with an initial grant from Mrs. Proctor. It was established as an extramural unit to support the Proctor Foundation in ways not possible under university regulations. It has contributed to the Proctor Foundation in supplying special lectures, symposia, monographs, etc. Currently under the presiding of Dan Vaughan, it is expanding, and this fall [1987], it will sponsor an international seminar at Lake Tahoe.

Hughes: Well, this is our last interview. It has been a delight to talk with you.

Controversies in Ophthalmology*

Thygeson: During my long career in ophthalmology, I became engaged in controversies, some rather bitter. Looking back on these controversies, I am impressed by my high "batting average." Only in a few instances was I on the wrong side of the argument.

My first major controversy had to do with the role of staphylococci in external disease. I maintained that staphylococci were an important cause of conjunctivitis, blepharitis, and marginal and epithelial keratitis. Professor Lindner of Vienna, a world-renowned figure in ophthalmic microbiology, would not accept this role for the staphylococcus, which he believed was a saprophyte in the eye because it could not grow in epithelium as an epithelial parasite.

Other controversies concern (1) the role of the inclusion body in trachoma, (2) the role of the placebo, (3) the uses of the corticosteroids, (4) the treatment of herpetic infections, (5) the prevention of intraocular infections.

The Fifty-Plus Years of a Kodachrome Seminar

Thygeson: Starting back in Colorado days (1928), I gave a special seminar based on photographs, mainly stereoscopic, of important external eye diseases, mainly infections. The seminar was continued at Iowa and Columbia and during the war years in Florida at Drew Field Air Base and at Valley Forge General Hospital, and finally at the University of California in San Francisco. The scope of the seminar has been steadily enlarged to include microbiologic specimens and pertinent reprints from the current literature. As given in the school year of 1986-87, the seminar consisted of four parts: (1) two microscopic preparations from Mr. Mas Okumoto's laboratory, (2) a case report from the Valley Eye Clinic given by Dr. D.G. Vaughan, (3) a short review of current literature given by Paul Rhordan-Eva or John Sheppard, and (4) eight or nine slides of important Proctor cases, some of historical significance, given by me. Occasionally during the year important historical figures in ophthalmic microbiology, such as Morax, Lindner, [John Elmer] Weeks, [Theodor] Axenfeld, etc., were presented.

* Dr. Thygeson wrote the two following sections after the taped interview sessions had concluded.



Phillips and Ruth Lee Thygeson
June, 1984

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APPENDICES

CURRICULUM VITAE

Name Phillips Thygeson, MD

Date of Birth March 28, 1903

Place of Birth St. Paul, Minnesota

Nationality U.S. Citizen

Education	<i>Degree</i>	<i>Year</i>
Stanford University, California	A.B.	1925
Stanford University, California	M.D.	1928
University of Colorado	Oph.D.	1930
University of Colorado	M.S.	1933

Professional Experience

Professor of Ophthalmology, Emeritus, University of California, San Francisco, 1970-
Professor of Ophthalmology and Director, Francis I. Proctor Foundation for Research
in Ophthalmology, 1959-1970

Clinical Professor, University of California, School of Medicine, 1949-1959

Associate Clinical Professor, University of California, School of Medicine, 1947-1949

Medical Corps, Army of the United States, 1942-1946 (Colonel, Medical Reserve,
Retired)

Professor of Ophthalmology and Co-Director, Institute of Ophthalmology, Presbyterian
Hospital, Columbia University, 1939-1942

Assistant Professor of Ophthalmology, Columbia University, 1936-1939

Assistant Professor of Ophthalmology, University of Iowa School of Medicine,
1931-1936

Honors

- 1936 Research Medal, A.M.A. Section on Ophthalmology
- 1949 Howe Medal, American Ophthalmological Society
- 1950 Proctor Medal, Association for Research in Ophthalmology
- 1951 Certificate of Merit, Exhibit on Ocular Manifestations of the Dermatoses, American Academy of Ophthalmology and Otolaryngology
- 1963 Distinguished Service Award, International College of Surgeons
- 1966 Chibret Gold Medal for Trachoma Research, International Organization Against Trachoma
- 1971 Certificate of Commendation from Dr. Emery A. Johnson, Assistant Surgeon General of the U.S. and Director, U.S. Indian Health Service, in recognition of many years of assistance to the Indian Health Service
- 1971 Honorary Degree (LL.D.) of the University of California, San Francisco
- 1984 Castroviejo Medal, Castroviejo Society

Memberships

- Expert Panel on Trachoma, World Health Organization
- International Organization Against Trachoma
- Scientific Advisory Committee, National Council to Combat Blindness, Inc.
- Editorial Staffs: American Journal of Ophthalmology and Survey of Ophthalmology
- American Medical Association
- American Academy of Ophthalmology and Otolaryngology
- Association of Research in Ophthalmology
- Pacific Coast Oto-Ophthalmological Society
- American Ophthalmological Society
- American Board of Ophthalmology, 1946–1952
- California Medical Association
- Pan American Ophthalmological Society
- Society of American Microbiologists
- Sensory Diseases Study Section, National Institutes of Health, USPHS (Chairman), 1955
- Lancaster Course in Ophthalmology, Colby College, Maine
- Consultant, Division of Indian Health, USPHS
- Treasurer, Board of Governors, Francis I. Proctor Foundation for Research in Ophthalmology, 1947–
- Vision Research Training Committee, National Institutes of Health (Chairman)
- Director, World Health Organization Reference Center for Trachoma, 1965–1970
- Trustee, Estelle Doheny Eye Foundation, Los Angeles, 1966–1976

Trustee, Alta California Eye Research Foundation, 1969–

Trustee and Section Chief, Basic Science Course, Stanford, 1970–1975

Consultant, Bureau of Drugs, FDA, 1973

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