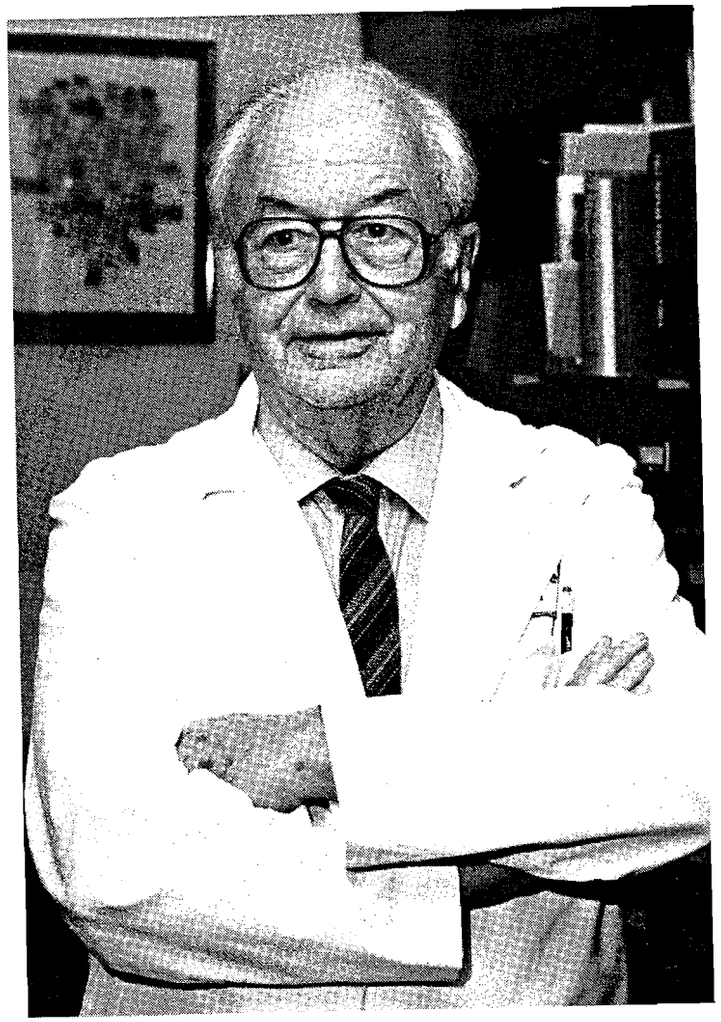
OPHTHALMOLOGY

ORAL HISTORY SERIES

A Link With Our Past



David Glendenning Cogan, MD
1987

David Glendenning Cogan, MD

The Howe Laboratory of Ophthalmology
at Harvard Medical School
The Massachusetts Eye and Ear Infirmary
and the National Eye Institute

An Interview Conducted by
Sally Smith Hughes, PhD
1989

With Introductions by
W. Morton Grant, MD
Lorenz E. Zimmerman, MD

The Foundation of the American Academy of Ophthalmology, San Francisco
Regional Oral History Office, University of California at Berkeley

It is recommended that this oral history be cited as follows:

David Glendenning Cogan, MD, Ophthalmology Oral History Series, A Link With Our Past, an oral history conducted in 1989 by Sally Smith Hughes, Regional Oral History Office, University of California, Berkeley in cooperation with The Foundation of the American Academy of Ophthalmology.

Cover & Title Page Design: Romaniello Design
Printed in the United States

The Foundation of the American
Academy of Ophthalmology
655 Beach St.
P.O. Box 6988
San Francisco, CA 94101-6988

Regional Oral History Office
The Bancroft Library
University of California
Berkeley, CA 94720

CATALOG CARD

COGAN, David Glendenning born 1908 Ophthalmologist

David Glendenning Cogan, MD: The Howe Laboratory of Ophthalmology at Harvard Medical School, the Massachusetts Eye and Ear Infirmary, and the National Eye Institute, 1990, xxvii, 256 pp.

Ophthalmology Oral History Series
The Foundation of the American Academy of Ophthalmology and
The University of California at Berkeley.

Son of Episcopalian minister, ophthalmologist mother, Boston area; undergraduate, Dartmouth College; medical education, Dartmouth and Harvard medical schools; internship, University of Chicago Clinics; residency, Massachusetts Eye and Ear Infirmary; faculty member, Department of Ophthalmology, Harvard Medical School, 1934-1974; staff member, Massachusetts Eye and Ear Infirmary,1935-1968; acting director, 1940-1943, and director, Howe Laboratory of Ophthalmology, 1943-1973; chairman, Department of Ophthalmology, Harvard Medical School, 1962-1968; surgeon-in-chief of ophthalmology, Massachusetts Eye and Ear Infirmary, 1962-1968; chief, Neuro-ophthalmic Section, National Eye Institute, National Institutes of Health, 1973-1985; Senior Medical Officer, National Eye Institute, 1985-date; associations with Dartmouth Eye Institute; World War II war gas research; Unitarian Service Mission to Germany, 1948; Atomic Bomb Casualty Commission, 1950; consultant, Los Alamos Medical Center, 1950-1955; Howe Laboratory history, staff, research (ophthalmic anatomy, biochemistry, pathology, neuro-ophthalmology), funding, politics; discussion of specific papers in ophthalmic pathology, neuro-ophthalmology, radiation and the eye, etc.; editor, *Archives of Ophthalmology*, 1960-1966; foundation of National Eye Institute; medical organization memberships; awards, citations, honorary lectures; associations with Drs. D.D. Donaldson, W.M. Grant, W.F. Hoyt, J.H. Kinoshita, V.E. Kinsey, C. Kupfer, T. Kuwabara, F.I. Proctor, C.L. Schepens, F.H. Verhoeff, J.H. Waite, F.B. Walsh, and others. Introductions by W. Morton Grant, MD and Lorenz E. Zimmerman, MD.

Interviewed in 1989 by Sally Smith Hughes, PhD.

ISBN 0-926866-05-2

OPHTHALMOLOGY ORAL HISTORY SERIES

Dohrmann Kaspar Pischel, MD	1988
Phillips Thygeson, MD	1988
Harold Glendon Scheie, MD	1989
Thomas David Duane, MD	1989
David Glendenning Cogan, MD	1990
Paul Boeder, PhD	1990
DuPont Guerry III, MD	In progress
A. Edward Maumenee, MD	In progress

The Foundation of the American Academy of Ophthalmology
San Francisco, California

Oral Histories Committee:
William H. Spencer, MD, Chairman
Arnall Patz, MD
H. Stanley Thompson, MD
Stanley M. Truhlsen, MD

Staff:
Susan Festa
Jill Hartle
David J. Noonan
Erica Perez
Licia Wells

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

Project Staff:
Willa K. Baum
Sally S. Hughes, PhD
Elizabeth Kim

DONOR REGISTRY

The oral history of David Glendenning Cogan has been made possible through the generosity of the following contributors:

Melvin G. Alper, MD
Chevy Chase, Maryland

Richard E. Appen, MD
Madison, Wisconsin

Anthony C. Arnold, MD
Los Angeles, California

David M. Bachman, MD
Washington, D.C.

J. Richard Baringer, MD
Salt Lake City, Utah

Roy W. Beck, MD
Tampa, Florida

Myles M. Behrens, MD
New York, New York

David P. Berry, MD
Spartanburg, South Carolina

Don C. Bienfang, MD
Boston, Massachusetts

William P. Boger III, MD
Concord, Massachusetts

Dan Boghen, MD
Montreal, Canada

Ronald M. Burde, MD
New York, New York

Leo T. Chylack, Jr., MD
Boston, Massachusetts

Edward M. Cohn, MD
Southfield, Michigan

Richard L. Dallow, MD
Boston, Massachusetts

Robert Daroff, MD
Shaker Heights, Ohio

Noble J. David
Coral Gables, Florida

J. Timothy Diegel, MD
Edina, Minnesota

Claes H. Dohlman, MD
Boston, Massachusetts

Joseph L. Dowling, Jr., MD
Providence, Rhode Island

Steven Feldon, MD
San Marino, California

Ephraim Friedman, MD
Boston, Massachusetts

Benjamin Frishberg, MD
Potomac, Maryland

Charles D. Fritch, MD
Bakersfield, California

John Gehring, MD
Wellesley Hills, Massachusetts

John W. Gittinger, Jr., MD
Chestnut Hill, Massachusetts

Evangelos S. Gragoudas, MD
Boston, Massachusetts

John F. Griffin, MD
Syracuse, New York

Steven V. Guzak, MD
Seattle, Washington

Dr. & Mrs. Robert S. Hepler
Encino, California

Lenworth N. Johnson, MD
Hershey, Pennsylvania

Otto H. Jungschaffer, MD
North Hollywood, California

David Kaufman, DO
East Lansing, Michigan

Marilyn C. Kay, MD
Brookfield, Wisconsin

Arthur H. Keeney, MD
Louisville, Kentucky

Jin H. Kinoshita, MD
Bethesda, Maryland

Martin P. Kolosky, MD
Washington, D.C.

Patrick J. Lavin
Nashville, Tennessee

Brendan D. Leahey, MD
Lowell, Massachusetts

Simmons Lessell, MD
and Irma Lessell
Boston, Massachusetts

Robert Lesser, MD
Waterbury, Connecticut

Byron S. Lingeman, MD
Wellesley Hills, Massachusetts

Eeva-Liisa Martola, MD
Wellesley Hills, Massachusetts

Bernard L. McGowan, MD
Framingham, Massachusetts

Joseph Michon, Jr., MD
Dorchester, Massachusetts

Roy J. Meckler, MD
Louisville, Kentucky

Richard P. Mills, MD
Seattle, Washington

Henry A. Mosher, MD
Cambridge, Massachusetts

Steven A. Newman
Charlottesville, Virginia

Edward W.D. Norton, MD
Miami, Florida

Patrick S. O'Connor, MD, PA
San Antonio, Texas

Jeffrey G. Odel, MD
New York, New York

George M. Olive, Jr., MD
Belmont, Massachusetts

Valerie Purvin
Indianapolis, Indiana

James Ravin, MD
Toledo, Ohio

John W. Reed, MD
Winston-Salem, North Carolina

Merrill J. Reeh, MD
and Elizabeth K. Reeh
Seattle, Washington

Karl Riemer, MD
Boston, Massachusetts

Joel G. Sacks, MD
Cincinnati, Ohio

Delia N. Sang, MD
Brookline, Massachusetts

Ralph A. Sawyer, MD
North Potomac, Maryland

Norman J. Schatz, MD
Key Biscayne, Florida

Alfred W. Scott, MD
Boston, Massachusetts

Richard G. Selbst, MD
North Andover, Massachusetts

William T. Shults, MD
Portland, Oregon

Richard J. Simmons, MD
Boston, Massachusetts

Barry Skarf, MD
Southfield, Michigan

Craig H. Smith, MD
Seattle, Washington

Richard L. Sogg, MD
Campbell, California

Robert H. Spector, MD
Atlanta, Georgia

Gregory I. Sugimura, MD
Ventura, California

John H. Thomas, MD
Boyton Beach, Florida

Martin Wand, MD
Hartford, Connecticut

Washington National Eye Center Foundation
Washington, D.C.

Frank Winter, MD
La Jolla, California

Jacqueline S. Winterkorn, MD
New York, New York

Thomas R. Wolf, MD
Tucson, Arizona

Shirley H. Wray, MD
Boston, Massachusetts

Robert D. Yee, MD
Indianapolis, Indiana

CONTENTS

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
Regional Oral History Office
University of California, Berkeley

William H. Spencer, MD
Chairman
Oral Histories Committee
The Foundation of the
American Academy of
Ophthalmology

March, 1988

INTRODUCTION

W. Morton Grant, MD

David G. Cogan, how do I introduce you? It must be as my friend, my hero, my knight in shining armor who has stood for the best in academic ophthalmology and for all the highest principles. Especially I want to introduce you as the very human being that you are.

But why do I think I know so much about you? It began one happy day in 1940 when I was still a medical student. I met David Cogan when he became my mentor in an elective course in the Howe Laboratory. He was so great, and made ophthalmic research so enchanting, that I never left, except for a year of internship. It has been fifty years since we met, fifty years in which I have had the great good fortune to have had David Cogan's friendship and his kind guidance, culminating in appointment as Harvard's first David Glendenning Cogan Professor of Ophthalmology. I submit that I should be able to offer some fairly reliable personal observations as a way of introduction.

I want to offer a brief characterization of David Cogan in a setting with scientific colleagues, and then to provide vignettes of his life outside of the laboratory and clinic.

In the scientific setting, David Cogan endeared himself to his colleagues by being always available with advice, with his rare gift for subtle guidance to help his students to think out their own solutions to their problems. Never dogmatic, always leading the way with well thought-out stimulating questions, David Cogan provided academic freedom and inspiration at its best. His relationship with his research colleagues is epitomized by his oft repeated question, "Is there anything I can do to help you?"

As a true and natural scholar, David Cogan has built bridges between ophthalmology and other clinical and basic sciences through both personal friendships and study of the literature. I recall an occasion when I found him studying biochemistry at 4 a.m. while others slept. With his wide-ranging interests, David Cogan has been one of the most highly respected ophthalmologists among people in other branches of medicine.

A highly successful means for encouraging discussion and sharing of scientific knowledge evolved as daily noontime brown bag luncheons at

which clinical and laboratory people met in David Cogan's laboratory-office for lunch and free-wheeling discussion. These luncheons, or the remembrance of them, have long been cherished by the many students, fellows, and career researchers of the Howe Laboratory, as well as by occasional visitors, who were also welcome. This bringing together of people with a variety of backgrounds and interests proved highly stimulating and productive. Jin Kinoshita, a biochemist destined to become director of all the ophthalmic research laboratories of the National Institutes of Health, was an outstandingly effective and enthusiastic participant.

When at home or elsewhere away from laboratory and clinic, David Cogan and his family have had a happy faculty of enjoying life in simple, unpretentious, and natural ways. In college at Dartmouth, David Cogan was a skier, and also played drums for the band. (He still practices on the piano.) When their children were young, the Cogans had a weekend farm in New Hampshire where the family enjoyed hiking, swimming, picnics, and winter sports. The Grant children were about the same age, and our family was many times invited to share in the festivities. The Cogans were wonderful hosts, and at times when scientists or students from other countries had unplanned weekends to spend by themselves in Boston, the Cogans would bring them to New Hampshire, where they certainly learned more about living in this country than they would have in a hotel in Boston. I can recall a professor from Germany learning to enjoy splitting firewood and using a chainsaw, and a Japanese student becoming Americanized, slightly.

Of later years, particularly since their move to the National Eye Institute, the Cogans have forsaken New Hampshire to vacation on a farm in Michigan left to them by Mrs. Cogan's parents.

Everyone who knows David Cogan knows how friendly he is, and knows what a delightful sense of humor he has, both in the form of extemporaneous humorous comments and humorous stories, never ever anything mean or unpleasant.

Among his personal priorities, David Cogan seems to have rated pocket money rather low on his list, allegedly sometimes finding that he has insufficient funds to pay the bill after eating out. Fortunately, Mrs. Cogan always has been prepared to come to the rescue. Happily, David Cogan's apparent indifference to pocket money has not been reflected in his regard for money raising for scientific research, at which he has been quite successful. For this, I and many former colleagues have good reason to be grateful.

July, 1990

INTRODUCTION

Lorenz E. Zimmerman, MD

During the period between the end of World War II and the beginning of the Korean conflict (1947-1950), I was a resident in pathology at Walter Reed Army Hospital, while Dave Cogan had already acquired such prestigious positions as director of the Howe Laboratory of Ophthalmology at Harvard Medical School, director of the ophthalmic laboratories at Massachusetts Eye and Ear Infirmary, associate professor of ophthalmic research at Harvard Medical School, and associate surgeon at Massachusetts Eye and Ear Infirmary. At that period of my life, I had no interest in either clinical ophthalmology or ophthalmic pathology, but I had developed a considerable, albeit transient, interest in metabolic disorders and especially disturbances in calcium metabolism. In my reading of Fuller Albright's many papers, the *New England Journal* CPC's, etc., I kept running into Cogan's name because he had made important observations on the development of corneal and conjunctival lesions in patients with hypercalcemia. In this connection, I recall reading about "Cogan's spots," but in a recent conversation with Dave, he doesn't recall having used that term!

I didn't actually meet the famous David Cogan until five years later when I was just beginning to learn something about ophthalmic pathology. Helenor Wilder, who had been responsible for ocular pathology at the Armed Forces Institute of Pathology for many years, had decided to retire, and I was given the opportunity to be her successor. She and Dave had been charter members of the Ophthalmic Pathology Club, which later was renamed in honor of Dr. Frederick Verhoeff, Dr. Cogan's mentor in ophthalmic pathology. In the spring of 1954, Helenor Wilder was so busy preparing to move to San Francisco that she asked me to represent her at the annual meeting of the Ophthalmic Pathology Club. That is when I first met Drs. Cogan and Verhoeff, as well as most of the other members. For several years thereafter, I would see Dave only annually at these pathology club meetings. I was from the beginning greatly impressed by how naively down-to-earth was this Harvard professor!

My first opportunity to get to know David Cogan more intimately came several years later, again in connection with a meeting of the Verhoeff

Society. It was the evening before our meeting was to begin. I received a phone call from Dr. Cogan who had realized only upon his arrival in Washington that he had neglected to make a hotel reservation. There were no rooms available at the hotel where our members were staying. My wife, Stasch, and I insisted that he come and stay with us. While we were waiting his arrival by cab, Stasch indicated she was a bit overwhelmed by the thought that she would have such an important Harvard professor as a house guest. Stasch still marvels at the fact that each morning Dave made up his bed and straightened his room so beautifully that no one would have thought it had been occupied.

That was a most fortunate beginning of a great friendship that has not only endured, but has grown in so many ways since then. After Dave's move to NIH in 1973, Stasch and I got to see more of Dave and his wife, "Did," and to know them so much better. We have often played bridge together, not only at home and while at meetings when rain kept us off the tennis courts, but also during international travel. We once played cards on the Oriental Express from Vienna to Munich and then from Munich to Geneva, taking time off only to partake in a picnic lunch of cheese that was so smelly that the conductors learned to stay out of our compartment! What a memorable way to travel from a joint meeting of the Irish and Bavarian Ophthalmological societies (in Munich) to a joint meeting of the European Ophthalmic Pathology Society with the Verhoeff Society (in Geneva)!

We have enjoyed the hospitality of the Cogans not only at their residence in the Bethesda area, but also in their summer home on a cliff overlooking Lake Michigan. There, this multifaceted man would also permit his guests to listen to his piano playing, which I believe relatively few of his colleagues know about.

The Cogans have so many interests, they know so much about history, archaeology, geography, natural history, and life in general, that they would be awesome were they not such gentle, humble people. It has been one of our greatest privileges to have made so many wonderful friends through ophthalmology, but none are more comfortable companions or loyal friends than the Cogans.

July, 1990

INTERVIEW HISTORY

Sally S. Hughes, PhD

This oral history of David Glendenning Cogan is the fifth in the Ophthalmology Oral History Series, a series consisting of comprehensive interviews with individuals who have made major contributions to American ophthalmology. Dr. Cogan enjoys world renown as former director of the Howe Laboratory of Ophthalmology and former chairman of the Department of Ophthalmology at Harvard Medical School, as a proponent of basic and clinical research in ophthalmology, and for his wide-ranging contributions in ophthalmic pathology and neuro-ophthalmology.

In the oral history, Dr. Cogan recounts his childhood as the only son of an Episcopalian minister and an ophthalmologist mother. An indifferent student in his early years, he was initially outshone academically by his older sister Mary. But at Dartmouth (1925-1928), he found himself, both scholastically and athletically. After three years as an undergraduate, he entered Dartmouth Medical School (1928-1930), and was among the fortunate three admitted to Harvard to complete his medical schooling (1930-1932).

When it came time to choose an internship, he accepted a classmate's suggestion to go "way out West," which to a Bostonian apparently meant Chicago. There he spent a year at the University of Chicago Clinics, finagling a nine-month rotation in neurology, to the detriment of other rotations. Thus were the seeds sown for a career-long fascination with neuro-ophthalmology. Chicago was also the beginning of another long relationship, this one with a pretty, young medical student, Frances Capps, whom he married in 1934.

In 1933, Dr. Cogan returned to Boston for a two-year residency at Massachusetts Eye and Ear Infirmary, where he encountered the formidable Frederick Herman Verhoeff and other luminaries of Boston ophthalmology. Verhoeff, who was director of the Howe Laboratory, became the formative influence in Dr. Cogan's professional life. Not only did Verhoeff convince his young admirer of the importance of research as an adjunct to ophthalmology, but he treated him virtually as the son, which biologically Verhoeff did not have. Because of their long association,

Dr. Cogan is in a prime position to swell the stores of Verhoeffiana and to amuse the reader in the process.

A year in Europe in 1937 on Harvard's Moseley Traveling Fellowship exposed the Cogans to the alarming Nazi buildup on one hand and to the pleasure and benefit of a research-oriented career on the other. By the time Dr. Cogan returned to the States, he had decided upon a career in academia.

When Verhoeff retired as director of the Howe Laboratory in 1940, Dr. Cogan was made acting director and then director three years later. He was thirty-five in 1943 and destined to hold the position for the next thirty years. The bulk of the oral history consists of Dr. Cogan's account of these years at the Howe Laboratory. We read of its illustrious staff—Morton Grant, Jin Kinoshita, Everett Kinsey, Toichiro Kuwabara and many others—whose accomplishments brought the Laboratory to the pinnacle of scientific achievement in ophthalmology. We also read of politics and personalities which led in the early 1970s to the migration of key members of the Laboratory to the National Eye Institute of the National Institutes of Health in Bethesda, Maryland. There they founded what they nostalgically referred to as Howe Laboratory, South.

Dr. Cogan followed in 1973, becoming chief of the Institute's neuro-ophthalmology section. He lured his reluctant wife south with promises of extended summer visits to their vacation home on the shores of Lake Michigan. In 1985, Dr. Cogan returned to his other professional love, pathology. At present, he is a senior medical officer at the National Eye Institute, where at age eighty-two he still puts in a full day's work five days a week.

Oral History Process

In preparation for the oral history, short interviews were taped in person and three by telephone with twenty colleagues and relatives of Dr. Cogan: Daniel M. Albert, MD; Melvin G. Alper, MD; Frederick C. Blodi, MD; Mary C. Bromage, PhD; Ronald M. Burde, MD; Jerry Chader, PhD; Frances C. Cogan, MD; Priscilla Cogan; Claes H. Dohlman, MD, PhD; David D. Donaldson, MD; Ephraim Friedman, MD; Morton F. Goldberg, MD; W. Morton Grant, MD; Jin H. Kinoshita, PhD; Carl Kupfer, MD; Toichiro Kuwabara, MD; Lawrence Merola; Abrahan Pollen, MD; Robert D. Reinecke, MD; and David Weeks. I am grateful to them for providing information which was instrumental in formulating questions and establishing content. I wish to give particular thanks to the Drs. Cogan for their hospitality in Leland, Michigan, to Dr. Ephraim Friedman, former president of Massachusetts Eye and Ear Infirmary, for arranging my visit to the Infirmary, to Joan Krizack, archivist at the Abraham Pollen Archives at the Infirmary, for guiding me to relevant documents and making the

Howe Library available for interviews, and to Beth Paterson, Dr. Cogan's assistant at the National Eye Institute, for arranging my visit to Bethesda and providing background material. I also thank Drs. Grant and Zimmerman for writing introductions. The annual reports of the Howe Laboratory for the years 1944-1972, written by Dr. Cogan, were of inestimable assistance in covering the laboratory's scientific achievements.

Seven interviews were recorded with Dr. Cogan, four in May, 1989 in his cluttered office at the National Eye Institute, and three in August, 1989 at the Cogans' summer home in Leland, Michigan. The Bethesda interviews allowed me to see Dr. Cogan, the ophthalmologist, in action, as a pathologist with his immense collection of slides and as a teacher at grand rounds. The Michigan interviews provided a more intimate and sustained association with Dr. and Mrs. Cogan, since I was staying at their comfortable, rambling home on the Lower Peninsula. It was a privilege and pleasure to share their wide-ranging mealtime conversations and enjoyment of the national beauty of the countryside where Mrs. Cogan's family has been summering for approximately a century, and to see their delight in a new puppy which, each told me confidentially, was to keep the other company.

The interviews were conducted by a fireplace fire which Dr. Cogan stoked enthusiastically with wood gathered on the property. Dr. Cogan was relaxed and forthcoming during the interviews, his ire rising only when we discussed the Laboratory's political problems. He occupied every waking moment between interviews, either puttering in his workshop, practicing the piano (he has recently started lessons), or composing a history of the Howe Laboratory on his new lap computer.

Transcriptions of the interviews were edited and mailed to Dr. Cogan, who reworked them extensively. Retyping the transcripts on his computer, he let my questions stand, but rewrote his answers, rewording and condensing them.

One of the themes that emerges from our discussions is the importance of the Howe Laboratory as a center for outstanding basic and clinical research in ophthalmology and as a place where intellectual exchange and freedom to pursue research interests were the modi operandi. Blessed with an exceptional staff of clinicians and basic scientists, the Laboratory under Dr. Cogan's guidance rose in the 1960s to a, if not the, leading position in ophthalmic research. Of particular pride to Dr. Cogan are his studies with Dr. Kuwabara on retinal vasculature and Dr. Kinoshita's research on sugar cataract. These are just two of the highlights of the Laboratory's diverse contributions in many fields of ophthalmology, representative examples of which are discussed in detail in the oral history.

The interviews also portray a modest but not self-effacing man remembered by his associates for his never-ending curiosity, high ethical standards, ability to guide others along scientifically profitable channels,

and to link findings in basic science and medicine. Dr. Cogan is at his best in small group settings, epitomized by his catalytic effect at the informal gathering of Howe Laboratory staff over brown bag lunches. By all accounts, he shone in the intimate, unstructured atmosphere of the Howe Laboratory in its heyday.

Admitting to a distaste for administration—he was unhappy as chief of ophthalmology at the Infirmary—as director he nonetheless orchestrated the expansion of the Howe Laboratory in terms of staff, scientific productivity, physical plant, service functions, and endowment. He says simply: "The Howe Laboratory was part of me and I of it."

What does he consider the highlight of his career? "I suppose the one word that says it all is freedom—freedom to pursue what I thought was right and promising, freedom to associate with persons whom I admire and respect, and freedom to identify with institutions that serve the common good."

July, 1990

Regional Oral History Office
University of California
Berkeley, California

The Foundation of the American
Academy of Ophthalmology
San Francisco, California

OPHTHALMOLOGY ORAL HISTORY SERIES
BIOGRAPHICAL INFORMATION

(Please write clearly, don't type. Use black ink.)

Your full name David Glendenning Cogan

Date of birth Feb. 14, 1908 Birthplace Fall River MA

Father's full name James Joseph Cogan

Occupation Clergyman Birth & Death dates ?

Mother's full name Edith Ives Cogan

Occupation Ophthalmologist Birth & Death dates ?

Spouse's full name Frances Capps Cogan

Children's full name Christy (dec); Ann (dec)
Frances Cogan Parson; Priscilla Cogan

Where did you grow up? Wakefield, MA.

Present community Chevy Chase MD

Education Dartmouth College, Dartmouth + Harvard Medical School
(Undergraduate, Medical School, Internship, Residency)
University of Chicago Clinics; Massachusetts Eye + Ear Infirmary

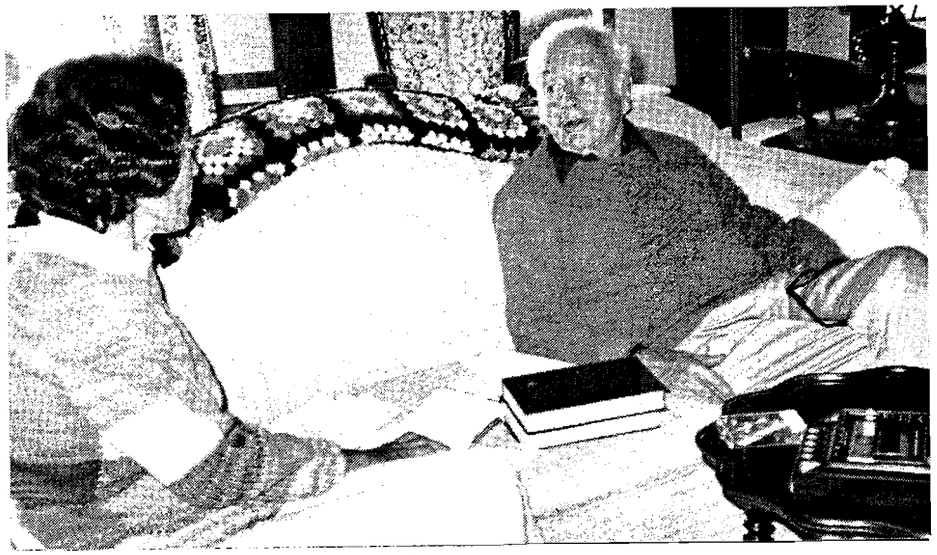
Occupation(s) Ophthalmologist

Areas of expertise Clinical Research in Ophthalmology

Other interests or activities Family; tennis; music; history

Active in which medical organizations? Ophthalmic Societies

Other organizations American Academy Arts + Sciences; Cosmos Club



The interview process,
Leland, Michigan, 1989

I. FAMILY BACKGROUND AND EARLY EDUCATION

Grandparents and Parents

[Interview 1: May 25, 1989, Dr. Cogan's Office, National Eye Institute, Bethesda, Maryland]

Hughes: Dr. Cogan, let's start with your maternal grandparents, where they came from, and what they did.

Cogan: I never knew my maternal grandmother. She died before I was born. Nor do I know what her maiden name was. My maternal grandfather was James Ives. My only remembrance of him was on his deathbed. I remember seeing this dignified elderly man with a white beard lying quietly in bed. He was living with my uncle, Jack Ives, in Andover, New Hampshire, in a large white house. In the house was a player piano which I was not allowed to use while he was living because of the disturbing noise. One morning at breakfast, I was told he had died. I rushed to the piano.

These grandparents came to Canada from England when my mother [Sarah Edith Ives] was twelve years of age. My grandfather had inherited a furniture store on Wigmore Street in London, but apparently failed in business. His real interest was in geology and geography. He brought to Canada his eight children and hoped to make a business out of selling maps that he made indicating territorial purchases by the United States government and the distribution of Indian tribes. My sister [Mary C. Bromage] and I have several of these extraordinary maps. He exhibited them at the 1892 World's Fair, but I doubt that they were much of a success financially.

Hughes: How about your paternal grandparents?

Cogan: I never knew my paternal grandfather or grandmother, Patrick and Elizabeth, except that they came from Ireland and lived in Brooklyn, New York. My grandfather was a cabinetmaker. Aside from my father [James J. Cogan], there were four daughters (Frances, Jane, Elizabeth, Margaret). My father rarely talked about his family.

Hughes: I know your father was a vicar or whatever the terminology would be.

Cogan: Interesting that you use the term "vicar" because most of my childhood was spent in Wakefield, Massachusetts, where he was an Episcopalian minister. Perhaps you had in mind Goldsmith's *Vicar of Wakefield* when you used the term. My father had been brought up a Catholic, but left the church for the Protestant ministry.

Rev. James J. Cogan

Hughes: Do you know why he converted to Episcopalianism?

Cogan: He never discussed it with me, but I know in later years he objected to Catholic dogmatism. He was an independent thinker and basically a scholar. We used to joke that he went into the

ministry so that he would only have to work on Sundays and spend the rest of the week with his books. Money meant very little to him.

Hughes: I gather he was somewhat distant.

Cogan: Yes. He was an ascetic who did not communicate very much. This had its good and bad points. It allowed us considerable freedom, and I appreciated that.

Hughes: Did your mother then fulfill the role of parent and housekeeper and wage earner?

Cogan: I was much closer to my mother. She was a warm person who liked people, and people liked her. Although of a temperament quite different from my father, she admired him and his scholarship. Yet they did not have much in common. That may have been the reason she persisted in medicine. She was eminently practical. As for her children, my sister and me, she felt they could do no wrong. She had that same sense of loyalty to anything English. It was all part of the family.

Hughes: Did she always practice medicine?

Cogan: She followed her older sister, Mary Ives, to the United States and like her went into medicine. She graduated from the Women's Medical School in Philadelphia.* I think that was its name.

Hughes: In medicine?

Cogan: In medicine. Why she went into ophthalmology I'd like to know. I don't recall ever asking her that, surprisingly. I wonder now why I didn't ask her more about it. I suspect she may have had a chance contact with some person or persons whom she admired and that directed her choice. She had the temperament of a family physician rather than a scientist.

Hughes: Did she encourage you to go into medicine?

Cogan: Not obviously. She neither encouraged me nor discouraged me. In fact, that was characteristic of both my parents. My sister and I were allowed to make up our own minds. But I think she was pleased that I chose medical school, and I know she was pleased that I went into ophthalmology. Perhaps that was subliminal pressure.

* The Women's Medical College of Pennsylvania.

Dr. Edith I. Cogan in her office, Wakefield, Massachusetts

Hughes: Tell me a little about her professional career. I know she was the first woman to be connected with the Massachusetts Eye and Ear Infirmary [MEEI].

Cogan: No, that's not quite right. At least one woman preceded her. That was Dr. Maud Carvill. She and my mother were close friends while my mother was on the staff of the Boston Dispensary before joining the Infirmary.

Hughes: Was she the first woman there?

Cogan: She might have been the first woman ophthalmologist at the Boston Dispensary.

Hughes: Did she ever talk with you about being a woman in medicine?

Cogan: She talked more about the difficulty of being a minister's wife than being a woman doctor. She loved her medical work and was sensitive to any suggestion that she was not fulfilling her role as a minister's wife. One of the few times when I remember her being angry was when the bishop told my father that a minister's wife should be a full-time church woman. Actually, she had been quite active in church affairs.

*Hughes: Priscilla thought that she also was, at times anyway, the principal breadwinner, that your father would have financial ups and downs.**

Cogan: That's right. Fiscal matters were of little concern to my father. He would rather spend money, when he had it, on the children or on my mother rather than keep it in his pocket or in the bank. I remember he bought a bicycle for me when my mother thought I should have earned it. Whether or not he was paid his salary did not seem important to him. Of course, that may be an exaggerated impression as we never discussed money so far as I recall. Somehow money was going to turn up.

Hughes: There were times when it didn't, then?

Cogan: That's right. Perhaps that's why my sister and I developed good work habits, because we both had outside jobs during our school years.

David and sister, Mary, c. 1910

Hughes: I noticed that as a child you lived in several different places. Was that because your father's work changed?

Cogan: We lived in Wakefield, Massachusetts, for most of my childhood years. We moved to Peabody, Massachusetts, when I was in my second year of high school. It was so difficult for my mother to

* Telephone interview with Priscilla Cogan, May 24, 1989.

move her practice that she continued to keep her office in Wakefield, about ten miles from Peabody.

Hughes: She was having to commute into Boston as well, wasn't she?

Cogan: Right.

Hughes: How much time did she spend in Boston?

Cogan: At the Infirmary, she was on duty three months of the year, possibly serving three mornings a week. On Saturday mornings, I was allowed to accompany her and wait while she was in the clinic. She also spent some time in a private Boston office which she shared with another woman ophthalmologist, Dr. Juanita Johns.

*Hughes: Your sister talked about those Boston trips. She apparently went along, too.**

Cogan: She probably did.

Hughes: Do you think those visits made any impression on you in terms of your choice of a career?

Cogan: Indirectly. What did impress me, I believe, was my mother's deep, almost unrealistic, respect for the Infirmary and its people. It was the same uncritical respect she had for the English royalty. When I rode by the Infirmary, I felt as though I ought to take off my hat. Dr. George Derby was the professor and chief of ophthalmology. He could do no wrong. It amuses me when I think of it. According to her, the system of the hospital was so efficient and everyone was dedicated to service.

Hughes: Did she remain more British than American?

Cogan: Yes, I think she did. Even her accent was British.

Childhood

Hughes: Where did you go to grammar school?

Cogan: I went to grammar school in Wakefield and continued there until my second year in high school when we moved to Peabody, Massachusetts. I was slow to adjust to the new school. My

* Interview with Professor Mary C. Bromage, Berkeley, California, December 26, 1988.

mother wanted to send me to Phillips Academy in Andover, but it seemed too expensive. My academic performance in the new high school was no more than average, a fact that contrasted with the top performance of my sister who was a year ahead of me. At graduation she was valedictorian and everything you would want in a student. "It's too bad that David can't be like his sister" is what I imagined the teachers thought. The question was whether or not I would ever get into college. The fact that I let it be known I wanted to play football if I ever made it did not enhance my chances of getting in. That was a ridiculous idea anyway since I was anything but an athlete. But for some reason unknown to me, the principal of the high school recommended me glowingly, and I was admitted to Dartmouth.

Hughes: Why Dartmouth?

Cogan: Well, I had spent a couple of summers in a boys camp operated by a Mr. Sanborn. He had two sons, one by the name of George who had been admitted to Dartmouth. At George's suggestion, I visited Dartmouth and fell in love with the place. I was impressed by the out-of-doors environment, the campus, and, of all things, the blazers the students wore. I had thought of trying for MIT [Massachusetts Institute of Technology], but felt sure I would not qualify. So Dartmouth it was at age fifteen.

Hughes: Were you interested in technical subjects?

Cogan: Perhaps. I didn't feel I was a scholar. I liked mechanical adventures. I liked my bicycle, and I was beginning to be infatuated with automobiles. I liked the freedom the machines gave me and liked the prestige of being the camp driver at the age of fourteen! Drivers' licenses were not required in New Hampshire at that time.

Hughes: Well, speaking of bicycles, Priscilla mentioned a trip that your parents allowed you to take. I think it is significant because it shows the independence you were allowed.*

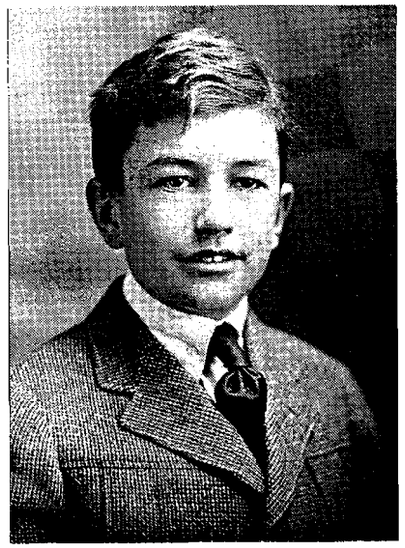
Cogan: Exactly. I must have been eleven or twelve years of age.

Hughes: Where did you go?

Cogan: To Provincetown on the tip of Cape Cod. My father had built a summer cottage there, along with founding a mission church, St. Mary's by the Sea. We spent our summers at Provincetown.

* Telephone interview with Priscilla Cogan, May 24, 1989.

Together with a friend of about the same age, I rode down the Cape about 120 miles. We had very little money in our pockets, spent a week, and rode back. What made the trip memorable for my parents is that I failed to keep them posted about my whereabouts. It rained all day on the way back. Come evening my friend stopped along the way with some relatives about twenty miles from Boston, but wet as I was I chose to continue. Having no light on my bicycle, I had to walk through most of Boston on police instruction. I arrived in Wakefield in the early hours of the morning, wet, tired, and content after some sixteen hours of continuous bicycling. To my surprise, my worried parents were up and waiting for me as my friend had called them from his stopping place in the early evening. It had never occurred to me my parents would be anxious. The event showed as much lack of insight as it did courage in a young boy.



Young David, c. 1918

Hughes: Is there anything else like that?

Cogan: Years later, after my sophomore or junior year in college, I secured a summer job on a Norwegian passenger ship as a deck steward and shipped off to Norway for several weeks. I had no plans and only about $10 in my pocket when I left. By a stroke of luck, which always seemed to come my way, I met a Norwegian medical student, Per Aasand, on board who invited me to stay with him for a week in Oslo and then at the family farm in the country. I have continued to be in touch with him since, and later I was of some help to the family by getting a job in the United

States for Per's sister. I returned, again as a deck steward, on the next return of the ship. The significance of these trips, I suppose, is the freedom to be adventurous in those days of the 1920s and the willingness of my parents to let me follow my whims.

Dartmouth College and Medical School, 1925–1930

Hughes: Did you begin at Dartmouth to get interested in anything particular?

Cogan: After a period of settling in, I did well. No one was more surprised than I. In fact, I became a sort of reference and support for my classmates in the dormitory. It was a novel experience.

Hughes: Were you working?

Cogan: Yes. I had a job there, too, in an eating house.

Hughes: I meant, were you trying? I got the impression that in high school you were taking academics rather casually.

Cogan: Yes. Perhaps I was reacting to my sister's superior performance. At Dartmouth I found myself, so to speak, not only scholastically but in sports as well. Unable to compete on the athletic field, I found the Dartmouth Outing Club a superb substitute. It was also competitive. Approximately 150 boys enter the competition in their freshman year.

Hughes: In what?

Cogan: In outing club functions—keeping the extensive chain of trails open, the cabins in repair and stocked, and logging the required miles in hiking or skiing. We were even recruited on one occasion to search a riverbank for a missing canoeist. If successful in the competition, we were elected to cabin and trail for the next year. We then had further competitive functions of a more sophisticated nature. I remember one of my assignments was to run the toboggan slide at the winter carnival. I had freshmen to help me. No Olympic functionary could have felt more important.

Hughes: Were you beginning to be interested in any particular kind of knowledge?

Cogan: Well, I knew early on that I wanted to go to medical school. By then I had jettisoned my earlier plan to be a farmer. Nor did I have any illusion about becoming a football player. Once I opted for medicine, all my courses, practically, were prescribed. This spared me the perturbation of having to decide on courses, a perennial problem for some of my colleagues in the liberal arts. The medical school candidate had to take prescribed courses and that was it.

Hughes: Were you forced to take the humanities as well?

Cogan: Some.

Hughes: But it was more a basic science emphasis?

Cogan: Right.

Hughes: How did it work? I know you transferred to Harvard for the last two years of medical school.

Cogan: At that time, Dartmouth provided only the first two years of medical school. One then had to transfer.

Hughes: Did you go four years as an undergraduate?

Cogan: I went for three years. My grades—they were in the Phi Beta Kappa range—permitted me to enter Dartmouth Medical School for what would have been my fourth year in college. Thus I received my college degree after my first year in medicine and had to transfer after my second year in medical school. I was fortunate in being one of the three admitted to Harvard Medical School for my final two years. Fortune seemed to be on my side. I was surprised but did not object.

Hughes: You were doing better by then?

Cogan: It hadn't been that way in the secondary schools.

Hughes: Were you interested?

Cogan: I loved it at Dartmouth.

Hughes: Not just in the outing club, but in what you were learning?

Cogan: Yes. Well, I had to drop out of the outing club competition at the end of my sophomore year in college to concentrate on my premedical studies. I even moved off campus for the same

reason. Fortune again gave me the opportunity to live in a private home of a lovely little old lady near the medical school. I remember her lace collar and tidy ways. I did small chores about the house in return for my room.

Hughes: Board as well?

Cogan: Not board. This lady, her name was Mrs. Wells, died during my second year there. I continued as custodian of the house until it could be sold. Then, the Congregational minister and his wife who lived next door invited me to occupy their third floor apartment in return for assistance in running the church's boys club.

Hughes: That was a benefit.

Cogan: It certainly was, and a very congenial relationship, too.

Hughes: Looking back on the courses before you went to medical school, are there any that stand out?

Cogan: Not that I recollect.

Hughes: Why don't you tell me about faculty members at the Dartmouth Medical School, those that stick in your mind.

Cogan: Dartmouth Medical School was great. There were only twenty students in the class and about an equal number of faculty members. We had a very personal relationship with the faculty. There was, for instance, the professor of anatomy in whose house we often had Sunday afternoon tea. It was so informal and delightful.

Hughes: What was his name?

Cogan: Fred Lord. One of his ancestors, also by the name of Lord, had been president of the college when medical training at Dartmouth was first officially recognized. The story is told that President Lord himself attended a class session and was so impressed that the next morning he ended prayers in the chapel by saying "and Lord, we thank Thee for the cerebrum, the cerebellum, and the medulla oblongata," or something to that effect. With that, medical teaching was approved at Dartmouth.

By the way, Dartmouth Medical School was the third medical school to be founded in this country. Its founder, Nathan Smith, then went on to establish medical schools at Yale and Bowdoin. It

was not without difficulty at that time. Medical students were thought to be a shabby lot, guilty of grave robbing and other social transgressions. But in my day it was a respectable adjunct of Dartmouth. I liked it and its rural setting so much that I felt lost when I transferred to the complex city environment of Harvard Medical School.

Hughes: That first two years of medical school at Dartmouth were the basic sciences, and you went into clinical work when you went to Harvard?

Cogan: Yes. Simeon Cantril and I had been close in college and in medical school at Dartmouth. We transferred to Harvard together. His father had a private clinic in Portland, Oregon. We talked about setting up practice there, Simeon in surgery and I in internal medicine, at the Cantril Clinic.

Hughes: That name rings a bell. Didn't he have something to do with radiation studies?

Cogan: That was Simeon in later days. How did you know that?

Hughes: I don't know. The name just clicked.

Cogan: He became head of the radiology department at the Swedish Hospital in Seattle. I'm impressed that you recognized the name. Simeon did not go into surgery after all. He developed hypertension and chose instead a specialty, radiology, in which he could better organize his life. He spent three years at the Sorbonne in Paris for his training in radiology. Among his later accomplishments was to be in charge of radiation health at Oak Ridge during the war. He would not tell me what he was doing then other than "winning the war."

In the meantime, I decided to go into ophthalmology. I felt it was hopeless for me to encompass medicine as a specialty. Practice required a knowledge of pneumonia, rheumatoid arthritis, infectious diseases, and so many diverse conditions that I would not be able to become expert in any. But here was the eye, only an inch in diameter. Certainly one could learn all about it in short order—another one of those illusions that determines our careers. And, of course, my mother's high regard for ophthalmology and ophthalmologists played a big role in my decision.

Hughes: Had she talked to you about her day-to-day practice? Did you have a feel for the vocabulary?

Cogan: No. Her comments were more related to people than to science. She was involved in her patients' private lives and their problems. She was temperamentally a private practitioner who happened to be an ophthalmologist.

Dartmouth Eye Institute

Hughes: Did you have any contact with the Dartmouth Eye Institute?

Cogan: I'm impressed. You seem to know a lot about these things.

*Hughes: Well, I talked with Paul Boeder, so I should.**

Cogan: I had a close relationship to the Eye Institute. In fact, Ken Ogle, the scientific member of the group, and I were particularly close friends. We took many trips together. On one trip, I introduced him to Betty Bartlett, who was later to become his wife. Many years later, I had the opportunity to introduce their daughter, Nancy, to her eventual husband, Dick Brubaker, now chairman of ophthalmology at the Mayo Clinic. At present, they in turn have a daughter in college at Wellesley. You know what I'm thinking. If only I had a chance to introduce her to some available resident, well...

Hughes: You'd like to carry it to the third generation.

Cogan: I might then get in the *Guinness Book of World Records* for matchmaking over three generations and still keeping it in the ophthalmic fold.

Hughes: So you decided on ophthalmology at Dartmouth. Was that the reason for the connection with the Institute?

Cogan: Probably. I even thought I might go back to Hanover and spend some time with the group there.

Hughes: Did you have a chance as a medical student to do any research there?

Cogan: No, except that some of Ken Ogle's optics brushed off on me.

Hughes: Was any of the Institute staff teaching in the medical school?

Cogan: No.

* Dr. Boeder was associated with the Dartmouth Eye Institute in the 1930s and early 1940s. See the forthcoming oral history in this series with Dr. Boeder.

Hughes: The Institute was completely separate from the Department of Ophthalmology?

Cogan: They were actually adversaries. You might say they did not see eye-to-eye.

Hughes: Do you know what the basis of the problem was?

Cogan: Well, the Dartmouth Eye Institute was founded by Adelbert Ames. He was more of a philosopher than a scientist. He got into the eye field through his observations in art and concluded that image-size difference in the two eyes might be a cause of eyestrain. He had little understanding of functional complaints in patients.

Eye symptoms are often simply a manifestation of fatigue, depression, or unhappiness, and are relieved by a change in venue, rest, and sympathetic understanding. Patients who went to Hanover were for the most part installed comfortably in the [Hanover] Inn, examined repeatedly at the Institute over a matter of days, and met with sympathetic counselors. They were given glasses with some assurance that their symptoms would go away, and for the most part they did. Dr. Ames and his optometric assistants had no doubt that correction of the image-size difference was responsible for the cures. Ogle, on the other hand, was involved in optics in the laboratory.

Hughes: So Ogle didn't have any connection with the work on aniseikonia?

Cogan: He was the one who designed the apparatus for measuring aniseikonia but played no role in interpreting the clinical results.

Hughes: Aniseikonia was quite a fad for a while, wasn't it?

Cogan: It was, and it had many wealthy and prestigious patients.

Hughes: It was Rockefeller money behind the Institute?

Cogan: Yes. I understand the Rockefellers gave $85,000 for it.

Hughes: [Alfred] Bielschowsky became associated with the Institute, but I guess you had long since gone. As I remember, it was not until the late thirties.

Cogan: In 1937. I got to know Dr. Bielschowsky quite well through our common interest in neuro-ophthalmology. He was a refugee from

Nazi Germany. I don't know what he basically thought about aniseikonia, but it was a nice position for him, and it added prestige to the Institute.

Hughes: Paul Boeder tells a funny story of walking with Bielschowsky because, I guess both being Germanic, Bielschowsky was his responsibility. Bielschowsky turned to him as they were walking, as I guess they did every day, and said, "Paul, what is aniseikonia?" So perhaps Bielschowsky wasn't too sure what he was getting into.

Cogan: Adelbert Ames was an artistic and charismatic person. He may have been an artist or a sculptor or both. He also had, I believe, degrees in theology and law. Interestingly, his son, Adelbert, is a well-known scientist in the visual field at the Massachusetts General Hospital. I don't think he was ever mixed up with aniseikonia.

Hughes: There were other people at the Dartmouth Eye Institute who went into ophthalmology. I'm thinking of Hermann Burian who was later at Iowa. Did your tenure at Dartmouth coincide with his?

Cogan: No. But subsequently I knew him. He came over to this country and found, as had Bielschowsky, a pleasant post at the Institute.

Hughes: Was he fleeing the Nazis as well?

Cogan: I really don't know.

Hughes: Did you have any knowledge of the [Walter B.] Lancaster situation?

Cogan: Dr. Lancaster was a great supporter of Ames' ideas initially. In fact, he told me once that he was the one who coined the name aniseikonia to reflect unequal images. He succeeded Bielschowsky and actually moved from Boston to Hanover.

Hughes: That was a disaster, I guess. He was not allowed to do what he had hoped to do. Were people at MEEI aware of that situation?

Cogan: I don't know that they were aware of the Lancaster situation, but my predecessor in the Howe Laboratory, Dr. Frederick H. Verhoeff, made it known that the whole philosophy of the Institute was a fallacy, if not chicanery. I suppose he was its most outspoken critic. He pointed out that reading across a simple page involved a 20% image-size difference. Yet the Hanover group claimed that size differences of 4% were producing

intolerable symptoms. He also delighted in hearing the case of a well-known public figure, whose name I will not divulge, who had the lenses replaced by accident in the wrong side of her glasses causing an *increase* in size difference without provoking any symptoms.

Hughes: What was happening to Lancaster?

Cogan: Lancaster was fairly elderly at the time. I don't know what happened intramurally, but he left after a year or so and returned to Boston. Then, too, there was ill will felt between Lancaster's Institute group and the ophthalmic department of the hospital. In fact, I found myself innocently thrust into the middle of the controversy. I was asked to come up to the hospital to operate on a patient with a detached retina. Not until I got there and examined the patient did I discover that the diagnosis had been missed at the Institute, and the patient had lost considerable vision as a result. Thus I had on my hands not only a detached retina to replace but a bitter patient to mollify.

To return to the Institute, it eventually folded up. I hold it against Ames that he gave Ogle only two weeks notice that he was through. Ogle had been a loyal supporter of Ames, and it was not easy to find a new post in optics. I tried to get a position for him at MIT, but optics was being phased out in favor of radiation. Finally, Ogle found a position at the Mayo Clinic. This allowed him to continue his studies and write several books on physiological optics, until his premature death from cancer some years ago.

Harvard Medical School, 1930–1932

Hughes: Let's go back to Harvard because I would like to know a bit more about what you were doing in those last two years of medical school, and who was teaching.

Cogan: In the abrupt jump from the rural setting of Hanover to the dispersed metropolis of Boston, I was lost. My record was not distinguished, but it apparently was passable. I don't think we had any grades. It was humbling because my record at the Dartmouth Medical School was first class.

Hughes: Was there anybody on the faculty who particularly impressed you?

Cogan: I was awed by all but felt close to few. The relationships between students and faculty seemed so impersonal. I was assigned,

however, to Dr. Edwin B. Dunphy as my advisor. He was a young ophthalmologist on the staff of the Infirmary and a friend of my mother's. We established a close rapport, although I don't recall any particular advice that I was given or needed. Later we were to have an intimate professional association.

Hughes: How big was the class?

Cogan: I think something on the order of 140 students.

Hughes: A big step from Dartmouth.

Cogan: Right. Transportation between the hospitals and the medical school was a problem. My father, with more generosity than discretion, bought me a Ford roadster. That helped my transportation problems but not my studies.

Hughes: Did you have exposure to ophthalmology as a medical student?

Cogan: I did. It was at the Infirmary, where I already felt at home. But, strangely, I don't remember much about the teaching. I do remember my cordial contacts with Dr. Dunphy, but it never entered my wildest dreams to think that some day we would share complementary roles in which he would be in charge of clinical operations and I in charge of research. However, it eventually came about, mostly by fortunate conjugation of circumstances. Later I would like to tell you about that in connection with the relationship of the Howe Laboratory to the Infirmary and Harvard Medical School.

Intern, University of Chicago Clinics, 1932–1933

Hughes: Why did you intern at the University of Chicago Clinics?

Cogan: By the time I was in the middle of my senior year in medical school, I had made up my mind to try for an ophthalmic residency at the Infirmary. It seemed to me appropriate, therefore, to take the required internship in some part of the country away from Boston. One of my classmates, Ed Gordon, suggested we go "way out west" to Chicago.

Hughes: That's way out west?

Cogan: I had that idea as a typical Bostonian. I don't think I had been west of Buffalo before. Thus several of us set out for that

metropolis during a vacation period. I was impressed by the University of Chicago Clinics. They were new, orderly, and on the Midway. I don't think I applied anywhere else. I was accepted to begin immediately after my graduation. Somewhat later, I was joined by Ed Gordon. Simeon Cantril signed up at nearby Michael Reese Hospital. My year at the clinics was eventful in ways I had not anticipated. Would you like to hear about it?

Hughes: I certainly would.

Cogan: Well, it was a medical internship arranged such that each intern would serve for three months on the following services: gastroenterology, nephrology, pulmonary diseases, and neurology. When I arrived, the chief of medicine was about to leave to return to research and the new chief, Dr. George Dick, had not yet arrived. When he did arrive, I was his first intern and in the unusual position of being able to orient him to some of the hospital's facilities.

I mention this because I and two fellow interns were hoping, somewhat deviously, to rearrange our schedules if we could get official permission. I wanted to take more neurology and less of the non-neurological rotations, whereas the other two interns, who were aiming eventually for surgery, had less interest in the neurological rotation. Dr. Dick allowed me to arrange it any way I wanted as long as all bases were covered. Thus it was that my medical internship was actually nine months of neurology and only three months of other non-neurologic medicine. Dr. Roy Grinker was chief of neurology and had just completed his textbook. All seemed to be working out as we had hoped.

Hughes: You were thinking of neurology in connection with ophthalmology, weren't you?

Cogan: Yes, the two specialties are interdependent. But then the first crisis occurred. Having finished his book on neurology, Dr. Grinker decided he would get further training in psychiatry and abruptly left for Vienna to be psychoanalyzed by Freud. I was left with a service that had scarcely any patients or other planned responsibility. "Dr. Cogan," I thought, "it serves you right. You manipulated the system and got what you deserve." What to do with all my available time now? Well, it wasn't wasted. I got some brain specimens from the morgue and learned a lot from dissecting them. And I read a lot.

But then I met a pretty medical student who was spending a quarter at the University of Chicago Medical School while on leave from Hopkins. She was spending her time in pediatrics. I

tried to convince her that I could be of great help to her. After all, an intern thinks he has a lot of practical knowledge whereas medical students know only theoretical book knowledge. Failing to impress her with that, I launched a further coup de grace. The hospital had one tennis court for the use of interns and residents. Medical students could play only on invitation. Both she and I liked to play tennis. That is how I got to know my wife-to-be. Her name was Frances Capps, the daughter of Clinical Professor Joseph A. Capps and the sister of Dr. Richard Capps who had been in the class ahead of me at Harvard. Dick had actually been one of those who encouraged me to take my internship in Chicago. The Capps' home was near the clinics. I became a frequent visitor, especially at dinnertime. Would you like to hear further ramifications of this story?

Hughes: I'd love to hear.

Cogan: Well, it did not begin quite as I told it. Rather it began with Dr. Joe Capps. The medical service held grand rounds on Wednesday afternoons when the interns presented a few interesting patients for the professorial staff, sitting in the front row, to discuss.

At one of these sessions, I presented a patient recuperating from typhoid fever. He was an Irishman who had been confined in a screened room for six weeks until he was free of typhoid bacilli. I prepared the protocol to be presented without notes and, as was the custom, in the absence of the patient. Then I signaled to have the patient brought in for the demonstration. The door opened, and we had the spectacle of a patient holding a lily on his chest and an expressionless face. He dominated the rest of the session. No one paid any further attention to what I had to say. The patient was willing and anxious to answer all questions and take over the discussion. After the presentations, Dr. Capps, feeling, I think, sorry for me, put his hand on my shoulder and suggested that I might accompany him home for dinner. That was when and where I really first met Frances. Only later did I learn how annoyed she was that her father had so disrupted her evening's homework.

Frances Capps Cogan

Cogan: By the time my internship ended in June 1933, we were tentatively engaged. I had not met my prospective mother-in-law or, more to the point, she me. It was her custom to spend the spring, summer, and fall in Leland, Michigan, where she supervised the building of her dream house on a cliff overlooking

Lake Michigan. This was such a major preoccupation that she had not come down to Chicago. Instead, Frances and I went up to Leland. Emma Christy Capps, she preferred to be called Christy, was a remarkable person, full of energy, independence, humor, and wide interests. I had expected as much, but I had not realized we had so much in common. Those few days are a memorable period in my life.

But I had contracted to be the camp physician on a canoe trip in the Lake of the Woods beginning the first of July. Accordingly, we three drove over Michigan's northern peninsula to the campsite in Wisconsin. Frances and her mother returned to Leland while I set out with a half-dozen boys and a camp leader to northern Minnesota and thence several weeks in the wilderness of the Lake of the Woods. Once we reached the Winnipeg River, I was released to return to Leland for a couple of weeks and then back East to begin my residency and reunion with my own family. Frances later left Leland for her final year of medical school at Hopkins. It had been an adventurous year.

Hughes: When did you get married?

Cogan: July 14, 1934. This was two weeks or so after she graduated from Johns Hopkins Medical School. I was in the middle of my residency at the Infirmary. You may recall that July 14th is Bastille Day or the French "Day of Liberation."

Hughes: Appropriate. Did she consider going into medicine?

Cogan: It was considered, but there were few openings for women at that time in conservative Boston hospitals. One possibility was at the New England Women's Hospital, but no program there appealed to her. Instead, she worked in the Howe Laboratory for a while and later at the MGH [Massachusetts General Hospital].

Hughes: Actually doing research?

Cogan: Yes.

Hughes: What type?

Cogan: Pathology, chiefly. We also prepared a paper for joint publication.*

Hughes: Yes. I heard that considerably later she was associated with Tufts. Priscilla said that it was because there was a certain family

* Cogan DG, Cogan FC. Dinitrophenol cataract. JAMA 1935; 213:354-60.

controversy over a color television set. So your wife said, "Alright, I'll go out and earn the money for a color television set."*

Cogan: That's hard to believe unless it was in jest.

Hughes: What research was she doing at Tufts?

Cogan: She worked with Dr. Russell Carpenter, professor of biology, on microwave cataracts in rabbits. The subject was heavily supported at the time because of the uncertainty as to whether cataracts resulted from ionizing radiation or from heat generated by the microwaves. It was important to know because radar was coming into wide use for all sorts of military and civilian purposes.

Hughes: What did she find?

Cogan: She and Dr. Carpenter did find that cataracts could be produced in rabbits, but the levels of microwave exposure necessary to produce the cataracts experimentally were much higher than those to which human beings might be exposed. The question as to an ionizing or thermal effect remained controversial.

Frances, or "Did" as she is generally known, liked working with Dr. Carpenter and his group, but I suspect it was as much because of the congeniality of her colleagues as it was because of curiosity about microwaves. She is a gregarious person who likes people association, and that is what she had in Dr. Carpenter's laboratory. She did not like working in isolation, as we discovered in later years when she tried her hand in electron microscopy. An electron microscopist has to be sort of a mole operating for the most part alone in a dark room.

Hughes: Did she learn that rather involved technique?

Cogan: Yes, through the gracious and patient instruction of that master of the technique, Dr. Toichiro Kuwabara.

Hughes: How long did she do electron microscopy?

Cogan: Maybe a couple of years. I must add it was a very trying time for both of us, 1962. We had lost our oldest daughter, Christy, through an auto accident in the summer after her graduation from the University of Michigan. Although the diversion of electron microscopy might not have been the ideal solution, it did enable us to be together in the Howe Laboratory much of the time

* Telephone interview with Priscilla Cogan, May 24, 1989.

and to have her participate in other activities of the Laboratory, such as our daily luncheon meetings. It served a purpose.

Hughes: How does, and did, she fill her time when she wasn't doing science or medicine?

Cogan: Did has a rich range of interests. Have you talked to her?

Hughes: No. That's what we're going to do tomorrow.

Cogan: Her family was not enthusiastic about her going into medicine. At their urging, she spent a year, after her graduation from Vassar, at the American School of Archeology in Athens. Her uncle, Ed Capps, a professor of classics at Princeton, was chairman of the managing committee of the Athens school and a well-known Greek scholar. She will tell you about this and her continuing interest in archeology when you talk to her.*

Hughes: Why didn't her family want her to go into medicine?

Cogan: It is hard to be an active doctor and raise a family even now, but much more difficult then. I know because I saw firsthand evidence of this in the case of my mother who tried to do both and was often frustrated.

Hughes: Well, Mrs. Cogan must have recognized that herself, from the course she took.

Cogan: You'd think so. But she was determined to carry through in medicine. The first thing she did after she returned from Greece was to register at Radcliffe for the courses she needed to get into medical school.

Resident, Massachusetts Eye and Ear Infirmary, 1933–1934

Hughes: Did you ever think of going elsewhere than Harvard for your residency?

Cogan: Only in the event that I would not be accepted for MEEI.

Hughes: Was the Depression having any influence on you?

* Interview with Dr. Frances C. Cogan, May 27, 1989, Chevy Chase, Maryland.

Cogan: Extraordinarily, not. I scarcely knew there was a depression. I was in Dartmouth Medical School when the crash occurred and in Harvard Medical School as it continued through 1932. I received fellowship support and had few expenses outside my studies. Then, through my internship and residency, my living expenses were covered by the hospitals involved. I don't recall having any major financial worries.

Hughes: You were paid as an intern?

Cogan: I don't think so. As a resident, I received $50 a month.

Hughes: But, of course, you had room and board.

Cogan: Yes, but I might add that the room at the Infirmary, accommodating four interns, was so small that the newest resident had to sleep in the upper berth of a double-decker bed. Lunches were often little more than cheese, crackers, and soup.

Hughes: Do you remember feeling strapped?

Cogan: I was used to it.

Frederick Herman Verhoeff

Hughes: What happened in your residency?

Cogan: It was a very busy time, of course, and full of practical experience. I found Dr. Verhoeff tremendously stimulating and provocative at the same time. At luncheon sessions, he always sat at the head of our table and raised questions about stereopsis, clinical cases, or whatever was on his mind at the moment.

Hughes: Did he recognize your interests and try to forward them?

Cogan: Indirectly. If he thought any of us had a good idea he would challenge us, but if you survived the inquisition he would be 100% on your side. If you did not, well, you'd better give up the idea. He was always more lenient to young persons such as residents than to his authoritarian peers. If he suspected subterfuge, unsubstantiated speculation, or naivetè, he could be devastating. He was especially vituperative when it came to the Dartmouth Eye Institute. We loved him for his frankness. He lectured me once on a matter of little interest to me, "I know you don't understand a damn thing I'm talking about, but it helps me to tell you about it anyway."

Hughes: Do you have any stories?

Cogan: I have many stories. Some of these were published in an imaginary letter I wrote him some time after his death.*

Hughes: You mentioned encountering him at lunch. Was there a table set aside where the residents and Dr. Verhoeff met on a routine basis?

Cogan: The eye residents had their own table with Dr. Verhoeff at its head. He usually came in a little late. It was one of our misdeeds to have the new resident occupy this head position unwittingly. We would watch for the expected reaction when Dr. Verhoeff appeared. [laughter]

Hughes: Were there other staff members at the table as well?

Cogan: Infrequently. It was a time for the residents and Verhoeff. Some of us were indoctrinated by his insistence on originality and direct observation in research. He once told me he didn't care so much what people thought as he did why they thought as they did. I think he was saying he was interested in observations rather than speculation. In pathology, he was more interested in analyzing a microscopic slide than in reading about the subject in a book. Could book knowledge be a handicap? I think he might have replied, it can be a handicap to originality.

J. Herbert Waite

Cogan: Dr. Waite was professor of ophthalmology and chairman of the department during my residency. He was at the Infirmary only infrequently and, on account of his deafness, communicated little with the residents. He received little financial support from the medical school and little assistance from the Infirmary to promote many of his ideas for postgraduate teaching and so on. He practiced in a private office about a mile from the Infirmary and did most of his surgery at the Deaconess Hospital another mile or two from the Infirmary. I suspect few residents, other than myself, got to know Dr. Waite well. My initial close contact with Dr. Waite did not augur well for our future relationship. I would like to tell you the story of that encounter but first tell you that I subsequently had a close association with him, an association that led me to have an understanding, respect, and affection for him. I believe he was not appreciated as he should

* Cogan DG. Frederick Herman Verhoeff – Personal recollections. Trans Am Ophthalmol Soc 1969; 67:96-109.

have been and I personally owe him a full measure of gratitude. I only hope he realized this before he died.

That encounter I referred to involved our planned wedding. The wedding was to be a big event for my mother-in-law. Did was an only daughter, so it was to be a once-in-a-lifetime affair. Moreover, it was to be held in Leland, Michigan, with many guests coming from Chicago. The scheduled date was July 14, 1934. Invitations had been sent out. I had made arrangements to have my duties covered at the Infirmary for the two weeks, which interns were customarily allowed each year. Then the explosion occurred. I was notified by Dr. Waite that a new ruling allowed interns to take off weekends only. I was incredulous and said in that case I would have to resign. I contacted Dr. Jonas Friedenwald at Hopkins, and he agreed to take me on in Baltimore in some capacity if I did resign from the Infirmary. I notified Dr. Waite of my intention, which I asked to activate immediately. He said he would ask the board of managers if they could make an exception to the ruling, which still had not been made public. The next day he called me with the "good news" that I would be allowed a week off. Why had Dr. Waite acted so seemingly arbitrary? I discovered that one of the senior residents had caused considerable difficulty by overextending his leave without notifying the hospital. Dr. Waite's ruling was the overreaction of an absentee administrator. Who would have thought at that moment I would one day be in private practice with Dr. Waite on the most congenial of terms?

Hughes: Did the neurology you had learned in Chicago serve you in good stead?

Cogan: It certainly did. At that time, there was little general interest in neuro-ophthalmology. My fellow residents were happy to refer all their neuro-ophthalmic patients to me. To see a wealth of material, it is not so much that you know a lot about a subject as it is that your colleagues know less.

Hughes: How many residents were there?

Cogan: I believe there were eight.

Hughes: Can you remember any of their names?

Cogan: Tom Carroll, Rod Irvine, Harold Gifford, Harry Messenger. We had a lot of practical experience at the Infirmary, especially in surgery, but little formal teaching. Instead, we organized among

ourselves Sunday morning sessions for the study of ophthalmic pathology. Self-instruction can be the best type of learning.

Hughes: That was enterprising.

Cogan: Residents now are so overloaded with clinical work in the hospital and responsibilities in their home life that they do not have time for such enterprises.

Hughes: But they did expect you to have a handle on the information, did they not? You were supposed to get it somehow?

Cogan: Yes, but neither the medical school nor the hospital felt it was its responsibility to teach the basic sciences in the specialties. That was our thinking when we later started the Harvard course for teaching the basic sciences in ophthalmology as a block either before or during the residency.

Hughes: Did you have any basic science instruction?

Cogan: Only what we organized ourselves.

George S. Derby

Hughes: George Derby was chief of ophthalmology when you were a resident?

Cogan: No. Dr. Derby died in 1931, whereas I did not come to the Infirmary until 1933. Dr. Waite succeeded him as chairman of the department. Dr. Waite and Dr. Paul Chandler had been his two associates in private practice. Actually, Dr. Derby committed suicide in his office. Apparently he was threatened with a lawsuit by an aggrieved patient. Dr. Derby was a very proud person and possibly tense as well. To be threatened with a suit, whether justified or not, apparently overwhelmed him. My mother, of course, knew him. She worshipped him.

Hughes: Who else of note was on the staff?

Cogan: Dr. Paul Chandler was the Infirmary's premier surgeon and authority on glaucoma.

Hughes: Did the residents get much surgical experience?

Cogan: They did, and it was well supervised by the senior visiting staff. Among the things I learned was that good surgery depends on

much surgery. It is not fair to the patient to operate only occasionally.

Hughes: Because you lose your touch so to speak?

Cogan: Exactly.

Hughes: When did you stop?

Cogan: I can't give you a specific date, but when I found my research activities in the laboratory preempted my time for surgery, I withdrew from taking the primary responsibility for surgery. I did continue to assist in the operating room for some time.

Hughes: Of course, there were plenty of others to do the surgery.

Cogan: Yes. Perhaps they were happy to have me withdraw.

II. THE HOWE LABORATORY YEARS, 1940–1973

Early History

Hughes: Do you think that Verhoeff served as a model for you in any way?

Cogan: There's no doubt about it. We had an unusually close relationship. I remember an occasion when he was presenting me with the AMA Howe Medal, and he announced that I had been like a son to him in ophthalmology. He had no sons and wanted someone to carry on in his image. We regularly went to meetings together; I as a satellite.

Hughes: Did that begin as soon as you finished your residency?

Cogan: Yes. He put me on the Howe Laboratory staff at a salary of $600. Per year, that is.

Hughes: Doing research?

Cogan: Yes, doing research for nine months of the year. Most of the other three months of the year I spent in Dr. Waite's office while he was away on vacation or ill. I also had the privilege of seeing a few private patients and consultations in the hospital.

Hughes: That was true from the very beginning?

Cogan: Yes.

Hughes: Was that common for physicians in the Howe Lab to have the privilege to see private patients at the Infirmary?

Cogan: Trygve Gundersen and Merrill King saw a few of their patients in the Lab.

Hughes: You talk as though it was not true later.

Cogan: When I later was director of the Howe Laboratory, I tried to formalize it. I set up a small consultation clinic so that full-time investigators in the Howe Laboratory could see private patients one or two half days a week. I thought it would enable them to maintain their clinical rapport, and it would supplement their income at the same time. It was also a service for the hospital, which otherwise was committed to the care of indigent patients only. I thought it functioned well, but some of the conservative staff feared it was the opening wedge for the entrance of the hospital into private practice. It became a bone of contention.

Hughes: Did you insist on the policy of having full-time investigators?

Cogan: I didn't insist on it, but it was my policy. As a matter of fact, I was making policy as I went along. There was nothing new in this. A fluid policy had been the history of the Howe Laboratory since its beginning. Would you care to have me give you a brief synopsis of the history of the Howe Lab?

Hughes: Please do, I believe you also wanted to say something about Dr. and Mrs. Lucien Howe.

Cogan: I never knew Dr. Howe personally. All I know is from hearsay and from what Mrs. Howe and their nieces have told me.

Dr. Howe was a prominent Buffalo ophthalmologist. He was financially well supplied. Mrs. Howe was his cousin. They had no children. I don't know what prompted him to establish the Laboratory at Harvard. I don't believe he had had any previous connection with Harvard or the Infirmary. I was told that his move to Boston disappointed some of the Buffalo group.

Judging from his portrait, Dr. Howe must have been an impressive figure. He was immaculately dressed and sported a flowing white moustache. According to the information I have, he and Mrs. Howe maintained a spectacular ménage in Buffalo, with some eight or nine servants. One took care of his horses, one was a groundskeeper, one or more cooks, and several maids who had to report each morning properly attired with clean aprons. He and Mrs. Howe presented quite a sight riding horseback through the park.

Let me tell you about Mrs. Howe whom I did get to know as she survived him into the early 1940s. Mrs. Elizabeth Howe had, I am told, not been very enthusiastic about the idea of a laboratory, but after Dr. Howe's death she adopted the Howe Library as her major interest. She made regular visits to the library to consort with her protégée, Mrs. Ada Messenger, who doubled as librarian and as the Howe Laboratory secretary. The relationship between Dr. Verhoeff on the one hand and Mrs. Howe on the other hand was anything but cordial. Mrs. Messenger's reports to Mrs. Howe did not help.

Hughes: Did Dr. Verhoeff have anything to say about either of the Howes?

Cogan: Nothing more than what you might predict. He felt Dr. Howe's reputation as an investigator was overrated. As for Mrs. Howe, he didn't want her to interfere with his operation of the Laboratory.

Hughes: Do you want to tell the story of Mrs. Howe coming to tea at your house?

Cogan: Actually we lived not far apart in Cambridge. On one occasion when she came to our house, I tried to intercede on Dr. Verhoeff's behalf, but found an impenetrable barrier. Because of severe deafness, Mrs. Howe depended on a trumpetlike hearing aid which she would insert in her ear for what she wanted to hear but remove when she did not want to listen. With my efforts to represent Dr. Verhoeff, she would remove the trumpet, put it in her bag, zipper up the bag, and look at me with a defiant smile. That was that.

In 1927, Dr. Howe gave Harvard Medical School a sum of $250,000, to be matched by an equal amount from university funds, to establish the Laboratory. There was no mention of the Infirmary in the deed nor, so far as I recall, any reference to the Department of Ophthalmology.* Dr. Howe stipulated somewhere that the director should have no compulsory teaching responsibilities and that a separate building for the Laboratory should be constructed as soon as possible.

Dr. Howe was appointed the first director and was provided with a room in the physiology department at the medical school. Space was also acquired at the Bussey Institute [Arnold Arboretum] for Dr. Clyde Keeler's study of genetics. Within a few months, however, Dr. Howe died, leaving an additional $250,000 to the Laboratory. Dr. Keeler's studies continued, but the room at

* Dr. Cogan is correct.

the school was locked up, and the Laboratory became essentially a paper organization for several years under the supervision of a committee that included the dean, I believe, Dr. [David] Edsall, Dr. Walter B. Cannon, Dr. Hans Zinsser, Dr. George Derby, and Dr. Frederick Verhoeff. An attempt was made to recruit Sir Stewart Duke-Elder, and he came from England to give a series of lectures, but he declined the position.

Then, in 1932, Dr. Derby prevailed on the committee to transfer the Laboratory to the Infirmary under a loose agreement whereby the Infirmary would provide space and Dr. Verhoeff would be the director.

Hughes: How did things change when Dr. Verhoeff became director?

Cogan: Dr. Verhoeff, whose researches had been chiefly in pathology, welcomed the opportunity to indulge in a long-time interest in physiology of visual perception. The Laboratory became the research arm of the Infirmary with a small staff and a budget of $30,000.

In 1940, Dr. Verhoeff reached the mandatory retirement age of sixty-five years. We appealed to the dean for an exception to the retirement rule but were told that similar appeals had been made for the eminent Walter B. Cannon, professor of physiology, and for Dr. Harvey Cushing, the neurosurgeon, but the rule was absolute. In fact, what the medical school authorities planned to do was to return the Laboratory to the school campus, and [they had] actually invited Dr. Ragnar Granit, the electrophysiologist of Sweden, to take the post. He declined since he was about to be made director of the prestigious Karolinska Institute in Stockholm. I happened to be on the spot, having recently returned from the Moseley Traveling Fellowship. I was thirty-two with few credentials and, a somewhat risky candidate. I was appointed acting director and that's how I became appointed on Dr. V's retirement, under the shade of a big question mark.

Hughes: Did Dr. Keeler continue?

Cogan: Dr. Keeler continued through the Verhoeff regime but had little contact with the rest of the Laboratory as his activities were elsewhere [at the Bussey Institute].

Hughes: Did Verhoeff have any connection with Phillips Thygeson?

Cogan: Of course, Verhoeff knew Thygeson and respected him. Sometime we might talk about the later relationship of the Howe Laboratory, the Proctor Foundation, and Thygeson.*

The staff of the Laboratory during Verhoeff's tenure was Merrill King in infectious disease, Trygve Gundersen in virology, Elek Ludvigh in physiologic optics, I in clinical physiology of the cornea, and an unusual person whom I shall not name.

Hughes: In what way was he unusual?

Cogan: In the first place, he worked only at night. He was building a spectroscope about the size of grand piano, but it never seemed to get finished. We were convinced that if it were ever finished, he would be out of a job. His major contribution perhaps was enlisting Elek Ludvigh into the Laboratory, for Ludvigh became Verhoeff's right-hand associate in studies on visual perception.

More on Verhoeff

Hughes: Physiologic optics was Dr. Verhoeff's prime interest?

Cogan: It was, after he took over the Howe Laboratory.

Hughes: More so than pathology?

Cogan: Right. He continued as consultant to pathology, but the operation of the pathology laboratory was turned over to Dr. Theodore Terry.

Hughes: Who had been trained by Dr. Verhoeff, is that not true?

Cogan: Yes.

Hughes: Were they on good terms?

Cogan: Yes, but they did surprisingly little in the way of collaboration.

Hughes: I'll read a quote from Dr. Harry Beecher's history of the Harvard Medical School. He credits Verhoeff with a role in the evolution of ophthalmology from "an empiric clinical specialty to an academic

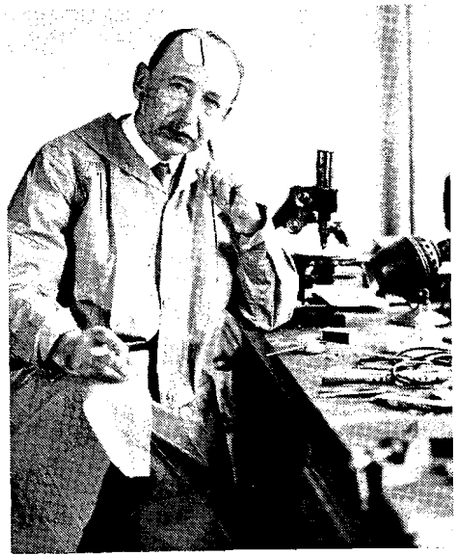
* See below, and also Phillips Thygeson, MD: External Eye Disease and the Proctor Foundation. Ophthalmology Oral History Series, A Link With Our Past. Interviews conducted by Sally Smith Hughes, PhD. Foundation of the American Academy of Ophthalmology, San Francisco, and the Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1988.

discipline resting on scientific foundations." * *Would you agree with that?*

Cogan: Those are actually my words.

Hughes: Why didn't Dr. Verhoeff become chief of ophthalmology at the Infirmary or chairman of the department at Harvard?

Cogan: He was an investigator of ideas not an administrator of people. This is an important difference.



Frederick Herman Verhoeff, 1934

Hughes: Say something about him as a physician and surgeon.

Cogan: Innovativeness was his hallmark. I don't know that you would claim him as a great surgeon. Yet he was a pioneer in surgery as he was in research. He introduced new techniques and methods of surgery.

Hughes: I read about his sliding technique for cataract extraction.

Cogan: Yes. Instead of removing the lens by tumbling, he would slide it out using Verhoeff forceps which he had designed. He also favored a keyhole iridectomy instead of the peripheral iridectomy.

* Beecher HK, Altschule MD. Medicine at Harvard: The First Three Hundred Years. Hanover, NH: University Press of New England, 1977, p. 437.

Hughes: Was it easier to slide it out?

Cogan: I don't know. We were taught the Verhoeff method and that was what the residents used.

Hughes: Did Verhoeff have ambitions to be chief of ophthalmology at the Infirmary?

Cogan: I think he would have been pleased to have it offered to him because there was strong rivalry between him and Dr. Derby. But I think he would have had the good sense to turn it down.

Hughes: How did he get on with the more clinical types?

Cogan: I heard a good deal about the mixture of awe and fear with which he was held by some of the clinicians, including my mother. He did not have time for such social amenities as saying hello or apparent recognition of his colleagues. He seemed to be thinking of something more important. Even though many of the staff were intimidated by him, they respected him.

Hughes: Did Dr. Verhoeff recognize the importance of giving an ear to you?

Cogan: I don't think that was the Verhoeff method. He was so intensely interested in what he was doing that he would rather talk about his interests than yours.

I remember I left the clinic one day to consult with him about a patient. I was then anxious to get back to the clinic, but he was anxious to tell me about one of his ideas. Noting my restlessness, he said, "I know you don't give a damn about what I'm saying, but it helps me to say it." Yet if you had what was a reasonably good idea and he could not come up with any serious criticism of it, he would thoroughly support you. He did support me and ultimately offered me a staff appointment in the Laboratory.

Hughes: Why was he so reticent to invite people to join the Howe Lab, and particularly somebody like you who was obviously interested in research?

Cogan: I think it is fair to say he was not interested in expanding the Howe Laboratory. He did little or no recruiting either of persons or funds. What was important to him was his physiologic optics.

A person by the name of Eugene McCarthy joined him for this study, but McCarthy's work habits were so different from Verhoeff's that effective collaboration never materialized.

However, McCarthy did recruit Elek Ludvigh who became an intimate collaborator with Verhoeff and backdrop for Verhoeff's ideas. That was a salutary arrangement but resulted, surprisingly, in few if any conjoint publications.

Hughes: Why do you suppose that Verhoeff was interested in McCarthy?

Cogan: That was really before my day, and I cannot tell you how McCarthy came into the Laboratory. McCarthy's project was building a spectroscope, which I don't think he ever finished. Nor do I know the circumstances under which he left the Laboratory.

Hughes: Did the several deans at the Harvard Medical School have much to do with the Howe Laboratory?

Cogan: Dr. Edsall, the dean at the time of the Howe donation, must have had considerable contact with it. After all, he accepted the funds and Dr. Howe's conditions. After the Laboratory was transferred to the Infirmary, the subsequent deans had, I believe, little to do with it. It was sort of out of their territory. I know that I succeeded in getting Dr. Ebert to visit the Laboratory only once and then only briefly when he had to cancel a luncheon I had arranged to have him meet our staff. It was a time when I needed help from the dean's office and did not get it.

Francis I. Proctor

Hughes: Could you tell me about the association of Dr. Proctor with the Howe Laboratory?

Cogan: It is my understanding that Dr. Proctor, a Boston ophthalmologist of some means, left a considerable fund in the Old Colony Trust for eye research. This was probably in the 1920s or earlier. The presumption was that the money was to go to the Infirmary because the executor of the estate was Mr. Russell Fessenden who was also chairman of the trustees of the Infirmary.

My first awareness of the fund was after World War II when Mr. Fessenden said the state of Massachusetts could no longer let the charitable trust stay unexpended and suggested that I apply for its contribution to the Howe Laboratory. By this time, Dr. Proctor had moved to New Mexico or Arizona on account of his wife's tuberculosis. There he became interested in trachoma among the Indians and tried to persuade Dr. Verhoeff to join him in its study. Sometime after his move to the West, Mrs. Proctor died,

and eventually he married again. In the course of time, he died [1939]. The second Mrs. Proctor, Elizabeth C. Proctor, was co-executrix of the estate. In my correspondence with her, I discovered she proposed offering the money to a foundation for the children of war-torn Europe. I think I dissuaded her from that as it would be such a small part of the overall effort. Only then did I learn that she was already in contact with Phil Thygeson, who had worked with Dr. Proctor, and had plans to set up a laboratory in San Francisco modeled after the Howe Laboratory.* As far as I was concerned, that was the end of my contact with the Proctor estate.

Hughes: Did the Howe Laboratory serve as a model for other eye institutions?

Cogan: Not that I am aware of. It has not been a popular modus operandi to have the clinical chief and the research director separate and equal appointments.

Hughes: Dr. Thygeson, I know, considers it to be very important.

Cogan: I do, too, but it's been difficult to persuade officialdom.

*Hughes: He is indebted to the Howe Laboratory for giving him the idea of setting it up under the dean rather than under the chairman of the Department of Ophthalmology.***

Cogan: I suppose a difference between the Howe and Proctor laboratories is the mission of the latter to concentrate on infectious diseases, Dr. Thygeson's lifelong interest, whereas the Howe Laboratory's studies have been more diverse.

Hughes: The Howe Lab did not have a specific mission in the sense of the Proctor Lab?

Cogan: That's right.

Hughes: What is its overriding purpose?

Cogan: I would say it's to bring basic science into clinical ophthalmology.

Hughes: Flexibility is important, would you say?

---

* See the oral history in this series with Dr. Thygeson for his view of the negotiations with Mrs. Proctor.

** The Proctor Foundation was subsequently placed under the chancellor.

Cogan: That was the Verhoeff philosophy, and I think it is mine. I'm for turning up the unexpected and then recognizing its importance.

Hughes: Would you care to give your opinion about Verhoeff's greatest contributions?

Cogan: He was the founder of ophthalmic pathology in this country. That's paradoxic because it never was his primary interest. He took the job at MEEI because it provided a source of income shortly after his graduation from Hopkins. The suggestion came from "Popsy" Welch, the professor of pathology and dean at Hopkins. When Verhoeff protested he didn't know pathology, the professor replied, "You can learn it as you do it." So Verhoeff came to MEEI to do the autopsies, mostly resulting from mastoid infections. Within a few years, however, he switched to eye pathology exclusively.

*Hughes: May I quote you on the subject of Verhoeff, and perhaps you could comment a bit further. The quote is from your obituary "letter" to Dr. Verhoeff: "Your generally unrecognized contributions would include the electrolysis method for treatment of detached retina, which preceded Gonin by several years (what a pity you did not appreciate the significance of the retinal hole!); the identification of retinoblastoma as a tumor peculiar to the eye; the concept of replacement for what is generally interpreted as fusion in binocular vision."**

What about the treatment of detached retina? Certainly Jules Gonin is most readily associated with that achievement. Why wasn't Verhoeff's contribution recognized?

Cogan: Dr. Verhoeff felt that he came close, at least, to discovering the surgical treatment of retinal detachment, but he failed to recognize the importance of sealing off the hole. He did introduce electrolysis for its treatment, however. Gonin recognized the significance of the hole, with the dramatic results he reported in 1929. That was twelve years after Verhoeff introduced the technique of electrolysis.**

Hughes: How was Dr. Verhoeff using electrolysis?

* Cogan DG. Frederick Herman Verhoeff – Personal recollections. Trans Am Ophthalmol Soc 1969; 67:96-109.

** For more on the history of retinal detachment surgery, see Dohrmann Kasper Pischel, MD: American Links with Germanic Ophthalmology, Retinal Detachment Surgery, San Francisco. Ophthalmology Oral History Series, A Link With Our Past. Interviews conducted by Sally Smith Hughes, PhD. The Foundation of the American Academy of Ophthalmology, San Francisco, and the Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1988.

Cogan: I really don't know the details of his technique.

Hughes: Could you explain Verhoeff's concept of replacement?

Cogan: I might compare it with hearing. If you block one ear, you do not hear as well as if both ears are unblocked. In other words, the input is additive. Not so with vision. The threshold for seeing is approximately the same with one or two eyes. The ultimate percept is a mosaic of selected points from each eye in which those points from one eye are replaced by corresponding points from the other eye. Does that make sense to you?

Hughes: Yes.

Cogan: It's quite different from other types of sensory input. Dr. Verhoeff felt this was one of his most important contributions to the concept of visual input.

Hughes: Which can be additive?

Cogan: Yes. It is thus not a matter of fusion as it is commonly referred to. Dr. Verhoeff removed "fusion" from his vocabulary.

Hughes: Did others?

Cogan: No. Even today I don't think people appreciate his concept. In fact, some call it "confusion."

Hughes: I also read, maybe it was your comment, that Verhoeff was not an advocate of the slit lamp or the Schiotz tonometer.

Cogan: I'll tell you why we thought he was not an advocate of the slit lamp. The world-famous publicist of slit lamp microscopy of the time was Alfred Vogt of Zurich. He drew conclusions freely about the underlying pathology, but according to Dr. Verhoeff, this was based on very limited knowledge. Dr. Verhoeff felt the importance of the slit lamp was overrated. The real reason for his bias, however, was that he could see everything with his magnifying loupe and did not need the slit lamp.

Hughes: What was his objection to the Schiotz tonometer?

Cogan: He was partial to a small hand tonometer, the Souter tonometer, which caused an indentation of the cornea best seen in the reflex from a window. He felt it was sufficiently accurate for him and it was very convenient. The patient didn't need to lie down for the test. Few ophthalmologists know about this tonometer. I have

one and find it useful for estimating the pressure of animal eyes and of human eyes that have irregularities of the cornea.

Hughes: You said in your letter that Verhoeff could be "frightfully devastating" at medical meetings. Would you care to relate one episode?

Cogan: I remember when he pitched into Peter Kronfeld at a meeting of the American Ophthalmological Society. The ostensible issue was whether Kronfeld had secured permission from his subjects to withdraw samples of aqueous humor for scientific purposes. But I suspected the real issue was Kronfeld's dogmatic manner of presentation of the subject.

Hughes: You mentioned going into private practice with Dr. Waite. What did you think of private practice?

Cogan: I enjoyed it. For a young doctor, it's very satisfying for one's ego to be in a position of authority. So I enjoyed it for a while. But there was so much we didn't understand or couldn't do anything about. I found that frustrating. I thought how exciting it would be to do more experimental work.

I had the opportunity to combine the two, thanks to Dr. Waite. I was in his office for three months of a year, seeing his patients while he was away, and then in the Laboratory for the rest of the year, seeing only a few private patients at the Infirmary. I must say the income was sufficient to support a limited lifestyle. Fortunately, my wife accepted the limitations and actually participated in many of my activities in the Laboratory. She was thoroughly supportive.

Moseley Traveling Fellowship

Cogan: Dr. Waite invited me to join him in full-time private practice. He proposed to enlarge his office considerably and made the position financially very attractive since he had one of the top ophthalmic practices in Boston. It was a big decision for me. I really wanted to stay in research, but at that time it was not the virile thing to do. To help in the decision making, or perhaps to avoid it, I applied for and received a Moseley Traveling Fellowship that permitted me to go abroad for a year.

The year was 1937, not a good year to go abroad with Hitler creating chaos in Germany and fear in Europe. On Dr. Bielschowsky's recommendation, I made my headquarters in

Basel, Switzerland, and did a small research project on color vision which was suggested by Professor Arthur Bruckner.

Hughes: What was Bruckner doing?

Cogan: He was chief of the clinic in Basel and interested in color perception. This was not of great interest to me, but my wife and I made enough observations on ourselves to show for our eight months in Basel.*

I also had an opportunity to study ophthalmic pathology with Dr. [F.] Rintelen. It was a distressing time to be in Basel on the edge of Germany. Refugees were trying to escape by all means possible. Some were shot trying to swim across the Rhine.

The year was of questionable scientific value, but it was full of adventures as my wife and I traveled about. What I did learn was that full-time research was more respectable abroad than it was in the United States. This encouraged me to pursue such a career when I returned to the United States. It meant financial sacrifices, but I respected the people who did it in Europe, shared their enthusiasms, and felt that the intellectual satisfaction would compensate for the rewards of a heavy involvement in practice. I wrote Dr. Waite of my decision. He was upset but graciously went along with my decision.

*Hughes: Priscilla referred to an incident when you were pulled off a train in Germany.***

Cogan: It was a momentous week in the fall of 1938. We had been spending the first part of the week in Munich. On Wednesday (I think it was), a general alert was called and reserves called up. All available trucks were commandeered. Thursday was very tense as I visited the eye clinic. One rumor was that a *Putsch* had been attempted against Hitler. The next morning we boarded a train for Czechoslovakia as the news was broadcast that the *Anschluss* with Austria had been effected. At the Czech border, I was discovered to have German money that was illegal to take out of the country. I was marched down to the local post office to forward the money to my anticipated point of return, while the train waited along with other trains loaded with tanks and military accoutrement to forestall any attempt of Czechoslovakia to come to the aid of Austria. In the meantime, my wife was closeted in the train compartment with a weeping

* Cogan DG, Cogan FC. Color fatigue in peripheral visual field. Ophthalmologica 1938; 96:137-56.

** Telephone interview with Priscilla Cogan, May 24, 1989.

Austrian woman, a Bulgarian perfume salesman, and a couple of other persons.

Hughes: What was the purpose of the Moseley Traveling Fellowship?

Cogan: It is a Harvard fellowship that provided $2,200 for an awardee to visit foreign clinics and laboratories abroad. I believe it was established when such postgraduate training was the thing to do. It is still a popular fellowship given, I believe, once a year with a stipend considerably inflated over what it was at that time.

Hughes: In medicine in general or just in ophthalmology?

Cogan: Not in ophthalmology. I know of no other ophthalmologist who has been a Moseley Fellow.

Hughes: Why was it awarded to you?

Cogan: Because I applied for it.

Hughes: Was it left to you to decide where you went on the fellowship?

Cogan: Yes. An attractive feature of the fellowship was the freedom to go where I wished. Also no thesis was required.

Hughes: Did you do research in any of the other countries you visited?

Cogan: No, but I spent some time in Groningen, Holland, with a young Dr. Bakker, who introduced me to the technique of whole lens culture. I was able to pass it on later to Everett Kinsey, who used it in his research in the Howe Laboratory. But I really didn't do any significant research myself while abroad.

Hughes: Then you came back to the United States; it was 1938. I'm a bit confused by the different academic titles of those early days. In 1935, you became clinical assistant in ophthalmology at the Infirmary, and in 1939, you became assistant surgeon in ophthalmology.

Cogan: Those were Infirmary appointments. Harvard appointments carried a professorial title to indicate their academic status.

Harvard Basic Science Course in Ophthalmology

Hughes: What happened when you got back from Europe?

Cogan: I found that Dr. Waite had arranged for me to organize a course in the basic sciences which was scheduled to begin in a couple of weeks. We had talked about this before I left for Europe. I had pointed out that medical students were given grounding in basic sciences in general, but nowhere was a prospective ophthalmologist ever given additional work in those basic aspects in which he was to make his career. At that time, the University of Pennsylvania had a training course that included the basic sciences along with clinical work.*

I proposed that we have an intense course of two to three months in the basic sciences of ophthalmology to precede a residency. So far as I knew, there was no such course available. But I had not given it much further thought until I arrived back in this country and found Dr. Waite had already scheduled it. I was frightened by the prospect. It had to be organized within a couple of weeks. A dozen or so students were already signed up for it. I recruited a faculty and emphasized that I hoped it would be hands-on instruction as much as possible rather than just lectures. Well, it turned out surprisingly well and became an annual course limited to about thirty students. With so many veterans returning after the war, there were more applicants than could be accommodated.

Dr. Lancaster then started a course in suburban Boston to serve the need. It was modeled after the Harvard course, except that it was largely lectures rather than hands-on teaching. After the first year, it was transferred to Maine where the students were housed at available schools, first at Westbrook Junior College outside of Portland and then at Colby College.

Hughes: How did you get faculty at such short notice?

Cogan: That was a problem. Dr. Ludvigh did the optics. Dr. Russell Carpenter supervised the orbital dissections. I did pathology and neuro-ophthalmology. The students did a lot of their own teaching. I've forgotten who constituted the rest of the faculty that first year. Most of the teachers were from the Howe Laboratory or the Infirmary. Dr. Carpenter was the exception. [As I've said] he was a professor of biology at Tufts University.

* See Harold Glendon Scheie, MD: Ophthalmic Surgery and the Scheie Eye Institute. Ophthalmology Oral History Series, A Link With Our Past. Interviews conducted by Sally Smith Hughes, PhD. The Foundation of the American Academy of Ophthalmology, San Francisco, and the Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1989.

Hughes: What was the format?

Cogan: It is described in an article I later wrote.* The course covered anatomy, embryology, pharmacology, biochemistry, neurology, and neuro-ophthalmology. My neuro-ophthalmology notes were later consolidated into two textbooks.**

Hughes: Oh, the books were based on the lectures?

Cogan: Yes, but they weren't written until some years later.

Hughes: Were there lectures and laboratories every day?

Cogan: Yes. We did brain modeling, ocular muscle modeling, anatomic dissection, and simple experiments every day.

Hughes: How many hours of laboratory work each day?

Cogan: Well, I'd say somewhere between one-third to one-half the time was laboratory work.

Hughes: It was quite a commitment on your part, wasn't it?

Cogan: Yes, it was. Comparable courses now are pretty much lecture courses. Laboratory work has been jettisoned in favor of lectures because of the time involved, but I don't think these can replace the advantage of doing the experiments and making models oneself. Our experiments and models were so simple I think people would be insulted if they were asked to do them now. Thus we made a model to illustrate eye movements out of a golf ball, chair casters, strings, and elastic bands.

Hughes: Was there a fee for the course?

Cogan: There must have been. Harvard charged for the announcements and registration.

Hughes: Where was it held?

Cogan: At the Infirmary.

* Cogan DG. Aims and aids in the teaching of basic sciences in ophthalmology. Arch Ophthalmol 1947; 37:428-32.

** Cogan DG. Neurology of the Ocular Muscles. Springfield, IL: Thomas, 1948. Cogan DG. Neurology of the Visual System. Springfield, IL: Thomas, 1956.

Hughes: So the Infirmary did have space.

Cogan: Limited space. Surprisingly, some of the Infirmary staff objected to the idea of giving any courses. It was claimed that courses might be used to replace residency training.

Hughes: The Lancaster course was never a competitor, or vice versa?

Cogan: No. It served the purpose of taking in a large number of students, which the Harvard course could not do. Most of our faculty also taught in the Lancaster course.

Hughes: What was the major difference between the two?

Cogan: The Lancaster course had the advantage of meeting in pleasant, vacationlike surroundings, the teachers were drawn from all over the country, and a large number of students could be accommodated. The disadvantage of the Lancaster course, on the other hand, was that it was largely a lecture course.

Hughes: My notes say that in 1966 the Infirmary assumed responsibility for the Lancaster course.

Cogan: Yes. First, I believe, under Parker Heath and later under Henry Allen.

American Board of Ophthalmology

Hughes: In 1937, you were certified by the American Board of Ophthalmology. Do you remember taking your boards?

Cogan: I remember taking the exams, but I don't recall any special story about them.

Hughes: Did you study for them?

Cogan: Probably. The only amusing incident I recall occurred some years later when I was examining a nervous candidate in neuro-ophthalmology. Trying to put him at ease, I asked him to explain the phenomenon of macular sparing. His reply was, "I never understood macular sparing, Dr. Cogan, and after reading your book I don't think you do either." [laughter]

I never did like examining candidates.

Hughes: Why didn't you like examining?

Cogan: I don't feel I can evaluate a person in a five-minute interview.

Sulfa Drugs and Penicillin

Hughes: I'm always interested in hearing if people remember when they first used sulfa drugs and penicillin. Do you remember?

Cogan: Sure, I do. It was 1938, shortly after returning from my travels in Europe, that my little niece developed a spiking temperature and stiff neck as a complication of mastoiditis. Her chances of survival seemed slim. But the University of Michigan Clinics, where she was hospitalized, had just received a small sample of the new [sulfa] drug—I think it was called prontosil—which she received with dramatic recovery. I believe she was the first patient in the University of Michigan Hospital to be treated with sulfonamides.

My second contact occurred somewhat later when this new wonder drug had become generally available. A patient of mine was struck in his only eye and blinded by a wooden peg while he was repairing some furniture. Examination of the eye showed the peg projecting through the cornea. The anterior chamber was filled with blood. The chance of infection and permanent loss of all vision seemed inevitable. Again the result of treatment with the antibiotic was dramatic, and the patient recovered to the extent of 20/50 vision. This seemed like a miracle, but the story does not end there, unfortunately. After discharging the patient and after I submitted a report to the insurance company, the patient, now somewhat inebriated, returned accusing me of collusion with the company. Apparently 20/50 vision is just under the border for less-than-full reimbursement. My pride in obtaining reasonably good vision created an angry patient. This is one of those socioeconomic problems which plague physicians. Had I known of the arbitrary cutoff level in the acuity scale, should I have perjured my report in the interests of the patient? That is one of those ethical questions that one does not hear about in medical school.

Hughes: Do you know when penicillin became available?

Cogan: Penicillin was somewhat later.

Hughes: During the war, wasn't it?

Cogan: Right.

Hughes: Because you weren't in a military setting, did you have trouble getting penicillin?

Cogan: I don't think I had much occasion to get it.

I was disqualified for military service because of hypertension. Our whole laboratory was turned over to war research.

Hughes: Nothing stands out in your mind in regard to antibiotics?

Cogan: Correct. Do you mind if I add a note about another matter that goes back to 1935?

Hughes: Please go ahead.

Dinitrophenol and Cataracts

Cogan: One of my first adventures in clinical research involved dinitrophenol. This drug was introduced in 1935 for weight reduction. It was quite effective. I happened to have a patient who developed cataracts after taking it in prescribed amounts. No previous instance of such a complication had been reported. While I spent several months trying to produce cataracts in animals with dinitrophenol (unsuccessfully, as it turned out), my fiancée, then a medical student at Johns Hopkins, discovered a second patient with cataracts after taking dinitrophenol. We were writing up the report* for the literature when the *New York Times* came out with the observation of cataracts in several Hollywood actresses who had been taking the drug. We were "scooped"; we had waited too long in trying to produce it experimentally. We then learned the value of making preliminary reports.

An amusing byline of this dinitrophenol business was the name of the patient and of his lawyer. His name was Martin Luther, his lawyer's name was Edmund Burke, an unlikely combination.

Hughes: Was clinical application always an ultimate goal of basic research at the Howe?

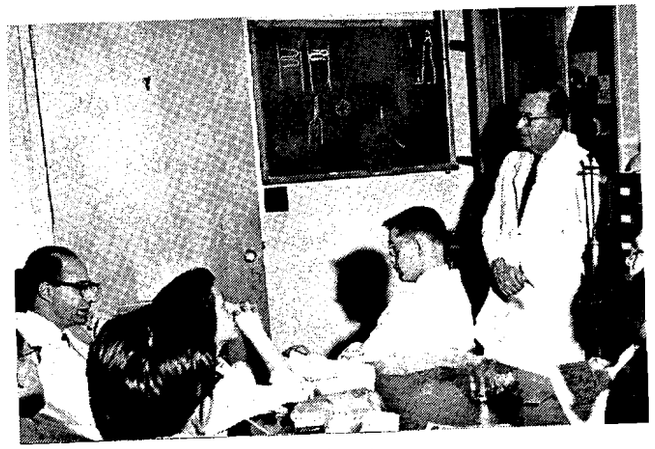
Cogan: For those of us trained as clinicians, it was always uppermost in our minds. The basic scientists, however, properly emphasized the importance of original observations whether or not they had

* Cogan DG, Cogan FC. Dinitrophenol cataracts. JAMA 1935; 105:793-5.

clinical relevance. Dr. Kinoshita's basic studies on aldose reductase, for instance, show how biochemical approaches may have profound clinical applicability.

Hughes: I understand that you always made an effort to show your science colleagues the possible clinical applications and expose them to ophthalmology.

Cogan: Well, we had lunch together every day. Some ten of us or so would sit down at the lunch table in the Laboratory. You could not help but be influenced by our diverse backgrounds. Dr. Kuwabara sparked a lot of discussion among the group.



Brown bag lunch at the Howe Laboratory

Hughes: Had Dr. Verhoeff anything comparable to the Howe Lab lunches?

Cogan: Except for Elek Ludvigh, Dr. Verhoeff did not have basic scientists in the Laboratory. They were all clinicians.

Hughes: Even a junior member of the Howe wouldn't have felt reticent to speak up?

Cogan: In our little gathering? I don't think so. If he did, we'd call on him. It was so informal there was no problem.

Hughes: I heard Dr. Grant did not come.

W. Morton Grant

Cogan: That's right. Morton liked to work in the solitude of his own laboratory, often with the door closed and guarded by a secretary. Yet he would join us if requested for a special discussion. But small talk was not in his manner of conversation. On the other, hand, he was always available to residents and others for discussion of professional matters. Moreover, he had a very special relationship with Dr. Paul Chandler, in which Dr. Chandler, the eminent eye surgeon, and Dr. Grant, the scientific investigator, met almost daily in the laboratory (over coffee) or in the operating room to discuss problems of glaucoma and studies on glaucomatous patients. This relationship, which continued until Dr. Chandler's death, is a model for integration of the clinic and the laboratory. Out of it came the text on glaucoma.* Few patients realize the benefits they gain from what goes on in the back rooms of research.

Characteristic of Dr. Grant was his modesty, competence, and thoroughness. I'm fond of recalling the following example of the latter. I had been examining a patient with gold deposits in the cornea, the first case of chrysiasis I had ever seen. As a matter of interest, I asked Dr. Grant to take a look at the patient without telling him that the patient had been receiving gold therapy for rheumatic arthritis. Well, he looked, and looked, and looked. I assumed he did not recognize the entity until he finally said, "I think these are gold deposits." He just wasn't going to commit himself until he had ruled out all alternatives.

Hughes: Do you know how Dr. Grant's interest in glaucoma arose? I know there was a debate in the thirties about the mechanism of glaucoma, whether it was an outflow or a secretion problem.

Cogan: You will laugh if I tell you my unlikely speculation on the basis of his original interest. You see, Dr. Grant's many talents include machine-shop work and domestic plumbing. (For instance, he air-conditioned his home and our original Howe Laboratory.) Now, glaucoma is a matter of mini-plumbing. Obstruction to outflow of fluid from the eye is the basis of glaucoma. In his meticulous way, Dr. Grant dissected the angle of the anterior chamber of enucleated eyes to identify the precise site of obstruction and developed a method for measuring its patency by increasing the hydraulic pressure of the fluid. The method,

* Chandler PA, Grant WM. Lectures on Glaucoma. Philadelphia: Lea & Febiger, 1965.

applied in vivo by a noninvasive technique, is called tonography. It provides a method for detecting glaucoma before glaucoma sets in.

Hughes: So his conclusion was that glaucoma is due to obstruction of the outflow rather than a problem of secretion?

Cogan: Yes.

Hughes: Tonography proved useful in discriminating among borderline cases of glaucoma?

Cogan: Right.

Hughes: Is there anything more to say about Dr. Grant's research?

Cogan: Dr. Grant has made an equally major contribution in his compilation of poisons to the eye. This has resulted in publication of his classic *Toxicology of the Eye*,* now in preparation for a fifth edition. This book is a compendium of poisons that affect the eye. It required a seemingly endless review of the literature and organization into a useful source of reference.

Hughes: Dr. Grant, I believe, was brought in during World War II to work on the mustard gas problem?

Cogan: Yes. Dr. Grant first had contact with us during an elective period of his senior year at Harvard Medical School. Organic chemistry had been a primary interest. As war broke out, the Howe Laboratory was assigned the project, along with other laboratories, to probe a possible prophylactic for mustard gas injuries. Morton joined us in this project immediately after completing his internship. Shall I tell you about this wartime research?

War Research

Hughes: What was the rationale for assigning that project to the Howe Laboratory?

Cogan: We were one of several laboratories assigned to look for an antidote or prophylactic treatment of mustard gas injuries and to be clearinghouses for study of other war gases that might be

* Grant WM. Toxicology of the Eye. Springfield, IL: Thomas, 1962.

used. You see, half the incapacitating injuries in World War I from mustard gas were eye injuries, but the symptoms did not come on for several hours after the exposure. It was our hope that interference in the damaging effects during this long latent period would prevent the injuries.

This turned out to be a false hope. Dr. Grant found that the mustard gas caused irreversible binding with the corneal protein within a few minutes of the exposure. Moreover, a vast number of experimental agents were ineffectual in undoing this binding under physiologic conditions. From that point of view, our mandated researches during the war were failures. In fact, I felt my role in the project as "responsible investigator" was so unproductive that I made a trip to Washington to declare my availability for military service. I declined the offer of being attached to the medical corps of Haile Selassie.

Hughes: Was mustard gas ever used in World War II?

Cogan: Fortunately not. But a convoy of ships did have a tanker filled with mustard that was bombed in the harbor of Bari, Italy, with spillage of the oil (mustard is not a gas at lower temperatures) that was catastrophic for the seamen who jumped off the bombed ship. At the time, we were privy to this super secret account, but a few weeks later we read more of the details in *Time*.

Hughes: Was the war gas research the impetus for Dr. Grant's subsequent interest in toxicology?

Cogan: Very likely.

Hughes: Tell me, if you please, the effect of the war on the Infirmary and the Howe Laboratory. For one thing, there must have been an exodus of personnel.

Cogan: Oh yes. Trygve Gundersen joined the army, and Dr. Edwin B. Dunphy, who was chief of the Infirmary and chairman of the Department of Ophthalmology, joined the navy.

Hughes: How about the optics research in the Laboratory during the war?

Cogan: I neglected to mention that a major part of the Laboratory's research was involved in stereoscopic methods of training for the navy. This was supervised by Elek Ludvigh, with assigned personnel from the navy and marine corps. In the early days of the war, range finding was done by stereoscopic visualization. Dr. Ludvigh and his group developed a dynamic system for

tracking aircraft during various types of attack. The apparatus for training exercises placed on various convoy ships was developed in the Howe Laboratory.

Radiation Cataracts

Cogan: My affair with radiation cataracts began during or soon after World War II when a number of physicists who had been working on the cyclotron developed a type of cataract compatible with radiation exposure. Yet they had been wearing film badges indicating insufficient gamma radiation to be cataractogenic. The fact was they had been exposed to neutrons for which there was at that time no means for measuring the exposure. There were twelve physicists involved, I believe, one of whom Dr. [David] Donaldson and I examined repeatedly and documented the progress of the cataracts. This was a most cooperative person by the name of Eric Clark who became a key figure in unraveling the mystery of the cause of the cataracts.

Hughes: *How did you know it was neutrons?*

Cogan: We weren't aware of it at the time. I was a member of the subcommittee appointed by the Atomic Energy Commission to coordinate the clinical abnormalities while the physicists proposed the neutron hazard.

Hughes: Now, this is postwar, right?

Cogan: This is postwar.

Hughes: Weren't you involved with radioactive isotopes and heavy water prior to World War II?

Cogan: I was involved, but the project was chiefly that of Drs. Kinsey and Grant who were studying fluid exchange between the eye and general circulation. I believe this was the first use of these tracers in the eye for this purpose.

Hughes: Please describe those experiments.

Cogan: The plan was to inject the isotopes into the circulation of rabbits and to measure the rate of entrance into the aqueous humor of the eye at equilibrium. The experiments with heavy water were relatively straightforward since quantitative measurements could be made on a fluid density column. But the radioactive measurements required obtaining the elements (sodium, potassium, and phosphorous) from the Harvard cyclotron and the assistance of physicists. One of the elements had such a short life that we had to take our rabbits and other paraphernalia to the cyclotron and perform the experiments there under unusual conditions for biologic experiments. Moreover, we were allowed only one short session because, so we were told, the cyclotron was to be "mothballed" on account of the approaching war.

Hughes: Was it?

Cogan: No, it was to be packed up and sent to Los Alamos, but that was a strict secret of which we remained ignorant.

Hughes: Who were the collaborating physicists at Harvard?

Cogan: Drs. J. J. Livingood and B. R. Curtis. I believe they were also about to move to Los Alamos for the duration.

The account of that brief meeting at the cyclotron laboratory might interest you. We arrived at the laboratory at the appointed time with rabbits, equipment, and personnel, but found time on the cyclotron was preempted by three New York jewelers who had invested in a large and expensive green diamond. Now a green diamond is said to be especially valuable, but the jewelers had learned that the green color might have been induced by radiation, in which case it would be only a surface coloration and they would not be able to cut it as they had planned. Fearing they might have been swindled, they were seeking help to determine whether it was a true or induced green diamond. I don't know how their investigation turned out, but it caused us anxiety lest we would not be able to follow through with our experiment. Actually, it turned out well due to the careful planning of Kinsey and Grant.

Hughes: Were you able to draw conclusions about the circulation of the aqueous?

Cogan: Yes. You'll have to read the papers.*

V. Everett Kinsey

Hughes: Will you tell me something about Dr. Kinsey?

* Kinsey VE, Grant WM, Cogan DG. Water movement and the eye. Arch Ophthalmol 1942; 27:242-52. Same authors. Sodium, chloride and phosphorous movement and the eye. Arch Ophthalmol 1942; 27:1126-31.

Cogan: V. Everett Kinsey pioneered the status of a biochemist attached to an ophthalmic department in this country. Actually, he was as much a physiologist as a biochemist. It took a lot of courage at that time to throw in his lot with clinicians.

Everett received his PhD in Pittsburgh in the late 1930s and joined us in 1940. He was my first appointment in the Howe Laboratory after I became director. His thesis involved glutathione, which is an important antioxidant in the lens. He had already established his practical interest in this field by studying cataractogenesis from infrared radiation, such as [that to which] workers in the steel mills of Pittsburgh might be exposed. His interests and ours coincided. It turned out to be a most productive and pleasant association.

At first, Dr. Kinsey worked with me on the physiology of the cornea, which was my primary interest at the time, a study for which we later received [1944] the Warren Triennial Prize given by the Massachusetts General Hospital. However, the war came, and it was necessary to convert to the war gas studies, which we have already discussed. Dr. Kinsey was a leader in these multiuniversity studies and emerged after the war as a talented organizer. Thus he was called upon to organize the large epidemiologic study confirming the role of excess oxygen in causing the blinding disease of infants known as retrolental fibroplasia.* In the Laboratory, he made important contributions to the fluid exchange in the eye and blood, before returning to his primary interest in lens metabolism which thenceforth was to be his major opus.

With our limited endowment at the Howe Laboratory, we were unable to obtain a tenure appointment for Everett, with the result that he moved to the Kresge Institute in Detroit where he continued his leadership in lens research until his untimely death in 1978. Along the way, Everett had established the stature of a basic scientist in clinically allied research to a high degree.

Hughes: How did Dr. Kinsey become interested in the problem of retrolental fibroplasia?

Cogan: Well, it was initially a Boston disease, occurring, paradoxically, in premature infants born at the prestigious Boston Lying-In Hospital. The condition was first documented by Theodore Terry, the then pathologist at the Infirmary. The pathology laboratory

* Kinsey VE, Zacharias L. Retrolental fibroplasia. Incidence in different localities in recent years and a correlation of the incidence with treatment given the infants. JAMA 1949; 139:572-8.

was adjacent to the Howe Laboratory. With funds provided by the Pew family, Dr. Terry asked Everett to supervise the research on this mysterious disease. Speculation was rampant as to its etiology. It seemed most unlikely that oxygen, which was necessary to save the lives of the premature infants, could have been responsible. This was, of course, proved to be the case after Kate Campbell of Australia first suggested it. Then followed Everett's epidemiologic evidence and the production of an experimental facsimile of the condition by Norman Ashton in England and Arnall Patz in Baltimore. Everett was given the Lasker Award for his role in the discovery.

Hughes: Did that discovery produce an immediate turnaround in the way premature infants were treated?

Cogan: Yes, indeed. Oxygen administration was kept to a minimum to save life and yet not produce retrolental fibroplasia. It was steering a course between Scylla and Charybdis.

Fund Raising and the Howe Laboratory Reports

Hughes: I have a quote from your introduction to the 1946 Howe Laboratory report: "The beginning of 1946 marked the termination of most of the government projects and again the Laboratory came into its own. The transition presented, as was expected, many problems, both of an organizational and technical nature ..." Can you remember what you were thinking of?*

Cogan: I'm sure it was, how were we going to support the full-time staff and technical personnel once the government funds were discontinued. That must have been uppermost in my mind since we had only an income of $30,000 from the endowment.

Hughes: Would you like to comment on your feelings about fund raising?

Cogan: Fortunately, I had solid support from the Infirmary staff and other benefactors.

Hughes: I noticed that the list of donors in the Howe Laboratory reports got longer as time went on.

Cogan: Yes. We received a sizable $125,000 from one source, through the intercession of Mr. Murray Seasongood, and another benefaction

* Annual Report of the Howe Laboratory of Ophthalmology, Harvard Medical School, 1946, p.1. An annual report or reports will hereafter be referred to as the Howe Laboratory report or reports.

from the Scaife Foundation sufficient to endow a professorship as a result of Dr. John Carroll's contacts. Eventually, our endowment reached something of the order of $2 million.

Hughes: How actively did you pursue contributions to the Laboratory?

Cogan: Whenever the opportunity arose. I wouldn't say it was my forté, but our needs spoke for themselves. Since I gradually gave up my own surgical activities, I didn't have the grateful patient contacts, I had to depend on others' representation to patients. Dr. Paul Chandler was especially helpful in this regard. I also met regularly with the Lions clubs of Massachusetts, which made annual donations to the Laboratory. Especially satisfying were the contributions from an increasingly large number of the Infirmary staff.

Hughes: Tell me about the Howe Laboratory reports, which began in 1944. Were they your idea?

Cogan: Yes. They had the primary purpose of reporting our activities to the benefactors and documenting what we had been doing for our own benefit. But they also had the purpose of raising further support for the Laboratory.

Hughes: What was the circulation?

Cogan: The reports were distributed to the Infirmary staff and to anyone who showed an interest in what we were trying to do. We did not have a large mailing list.

Hughes: How did you write the Howe Lab reports? Was it off the top of your head or did you talk to various people in the Lab?

Cogan: I solicited a report from each member of the senior staff and then tried to integrate these summaries into a text that would be meaningful to the lay public as well as to our professional colleagues.

Hughes: Did you ever get the feeling that other institutions used the reports as a means of gaining information on projects that they might be interested in?

Cogan: I don't know about that. The accounts described work which had been done and rarely ideas that were in the offing.

Hughes: I understand that the director of the Howe Lab is appointed jointly by the Harvard Corporation and the trustees of the Infirmary. How well do you think this system worked?

Cogan: Of course, I would have to say I think it worked well. But you have to recall that the Howe Laboratory was established as an autonomous department at the medical school. The Infirmary came into the picture only later. Its trustees were in the nature of an approving body rather than an appointing body.

Hughes: Was it difficult to make the transition from wartime to peacetime?

Cogan: Of course, it presented problems, but it was with a sense of relief that we could now pursue research freely as opposed to targeted research. My biggest disappointment was in the matter of personnel, and the fault was probably mine. I was convinced the future of ophthalmic research required full-time personnel, and our space was allocated accordingly. But those who had been in the armed service assumed they would return to their old positions; in fact, the government mandated that.

Trygve Gundersen was a case in point. Trygve had been a close friend of mine socially and professionally, and I did not realize until too late that he was upset by the fact he would not have a place in the Laboratory on his return. I simply had failed to discuss it with him or he with me. I was learning the pitfalls of administration.

Hughes: What happened to Dr. Gundersen?

Cogan: He eventually moved to Boston University.

Recruitment

[Interview 2: May 26, 1989]

Hughes: What did you look for when you were selecting members of the Howe?

Cogan: Space, time, and availability of appropriate personnel were influential factors. For instance, for my first appointment I looked for a biochemist. Everett Kinsey was fortunately available. So was a recreation room adjacent to the Laboratory, and the time was ripe. Years later, when Dr. Kinsey had to leave because of the tenure restrictions, I naturally turned to the

biochemistry department and again was fortunate in finding Dr. [Jin H.] Kinoshita. Each in turn solicited his own staff. With the increasing reputation of the Laboratory, recruitment became easier.

Chance also played a most important role. The case of Dr. David Donaldson is a relevant illustration. After the war, Dr. Donaldson happened to take a course with us in which I taught neuro-ophthalmology using some photographs that I had hastily prepared. Pointing out that they were less than optimal, Dr. Donaldson volunteered to spend a couple of months with me preparing better material. That couple of months turned out to be several decades in which he combined clinical ophthalmology with instrumentation and developed a unique collection of photographic material that was eventually published in several atlases.

Hughes: *Dr. Donaldson told me his collection contained something of the order of 25,000 photographs.**

Cogan: By the way, you may be interested to know how Dr. Donaldson had become a sophisticated machinist at an early age. He was trained by old Henry Ford himself who, among his quaint customs, personally trained the sons of his executive officers in machine-shop work. "They should learn something useful before going to college," he said. Dr. Donaldson learned how to design and construct cameras among other things.

Hughes: *Is the photo collection available for use by others?*

Cogan: The stereoscopic atlases were sold on a cost basis. They provide a beautiful and instructive portrayal of ophthalmology and neuro-ophthalmology. When medical students ask my advice about going into ophthalmology, I show them a few Donaldson photographs; that often decides them positively.

Hughes: *What was there in the way of cameras for use in ophthalmology before the Donaldson camera?*

Cogan: Color photography was still in its infancy, and stereoscopic photography of the eye was practically nonexistent. Dr. Donaldson pioneered both.

Hughes: *I am interested in your comment [off tape] that those with a mechanical bent are often attracted to ophthalmology.*

* Telephone conversation with Dr. David Donaldson, May 17, 1989.

Cogan: I think that's true. Ophthalmology is probably second only to dermatology in its objectivity. Patients' accounts of their symptoms are not as important in ophthalmology as they are in other medical fields. Ophthalmology is objective. We have excellent examining instruments and can generally make diagnoses on what we see. I think that appeals to the visually minded.

Hughes: *Less guesswork perhaps?*

Cogan: In general, yes. It's not like percussing the chest or palpating the abdomen.

Hughes: *Was the tenure problem a real handicap to the development of the Howe?*

Cogan: It was most upsetting. We were told we could have only one tenure appointment for each $25,000 of endowment income per year. We lost Drs. Kinsey and Ludvigh on this basis. Later, "soft" money tempered the tenure restriction on the basis that NIH money would continue indefinitely. But "soft" money had its limitations. The time required to develop grant applications and the uncertainty of funding hampered our ability to attract new persons. Our only alternative was to increase our endowment.

Hughes: *Do you think that persons like Drs. Donaldson, Kinsey, Kuwabara, Kinoshita, and Grant would have been as successful in another environment?*

Cogan: I'm sure they would have been. They were all exceptionally talented persons.

Hughes: *Dr. [Carl] Kupfer told me that your policy, one that he has copied from you, was to allow new members of the Laboratory to have six months in which to decide on their line of research.* Is that what you remember?*

Cogan: In general, that's true. Dr. Kupfer is now in charge of an organization [National Eye Institute] that is several magnitudes bigger than the Howe Laboratory. He is a much better administrator than I was. Fortunately for me, I did not have the bureaucratic problems he has to handle. I was able to mix intimately with each investigator on almost a daily basis.

* Interview with Dr. Carl Kupfer, November 11, 1988, Berkeley, California.

Incidentally, Dr. Kupfer also illustrates the fortunate conjugation of space, timeliness, and availability. He came to the Howe from Hopkins and administered a training grant that was provided to us by NIH. Little did he or we realize that in the not too distant future he would be in administrative charge of NIH's entire National Eye Institute.

Hughes: *Would you say that your policy was to let investigators follow their own noses?*

Cogan: Very much so. I might try to subtly influence them towards clinical aspects of research, by presenting clinically relevant aspects of their work, but not directing them further. I assumed that they liked to be treated the way I appreciated being treated myself.

Independence of the Howe Laboratory

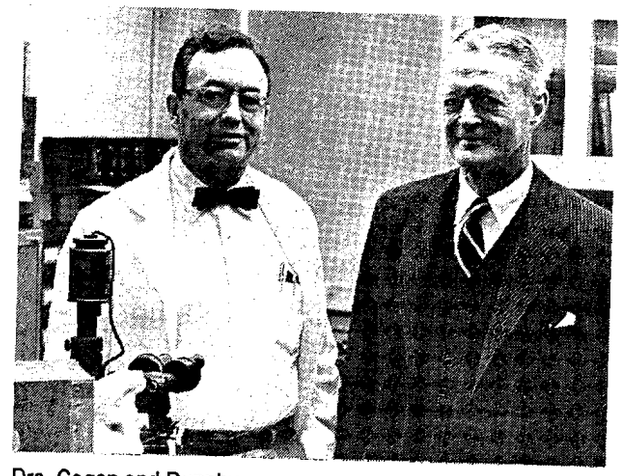
Hughes: *Why did you think it was important for the Howe Laboratory to be under the dean rather than under the chairman of the Department of Ophthalmology?*

Cogan: It seems to me the ideal administrator of research and the ideal chairman of the clinical department have such different motivations and responsibilities [that] they should not be combined in one person.

From my experience, I would like to illustrate the relationship I had with Dr. Edwin B. Dunphy, who was chairman of the Department of Ophthalmology while I was director of the Howe Laboratory. Dr. Dunphy was fully committed to running the clinical affairs of the hospital, the residency program, the nurses' program, patient matters, and innumerable functions involving people. He had no time to mix intimately with the problems that pertained to laboratory research. On the other hand, I was always available to the Laboratory personnel and had daily luncheon conferences with them at an unhurried pace.

One of my colleagues generously cited me as having a catalytic effect on my colleagues.* That would have been impossible if I had to cope with Dr. Dunphy's problems as well. Moreover, I had responsibility for raising money to support research, while Dr. Dunphy had to raise money for the hospital. There was no conflict of interest on this score. Each of us had our own

* Allen HF. Catalytic Cogan. Arch Ophthalmol 1966; 75:595.



Drs. Cogan and Dunphy
(Abraham Pollen Archives and Rare Book Library, MEEI)

repertoire of headaches and were thankful we did not have to handle that of the other. It was an unusual arrangement, unlike the customary pyramidal system where the chairman all too often hires a researcher to carry out the chairman's interest. The fact is our system worked well, but did not work as well when I reluctantly became chairman of the department while serving simultaneously as director of the Laboratory.

The common mistake, I believe, is to appoint a chairman because he is a good investigator or conversely appoint a director of research because he is an efficient manager. I think Dr. Howe must have set up the Laboratory with this in mind because he stated in the deed of his gift that the Howe director should have no clinical responsibilities other than those connected with research.

Hughes: *Do I take it then that if the Laboratory had been under the department chairman, as is the usual case, there would have been more control?*

Cogan: Yes, I think research, especially as it relates to basic research, would have been more constrained. But I was never able to convince Dean Ebert of the advantages of the system in the Howe Laboratory to warrant its preservation on my retirement. The dean, I suspected, visualized a double-headed monster with whom he would have to deal. He actually knew very little about the Laboratory and found it easier on my retirement to use the directorship of the Laboratory as bait for securing a new chairman for the department. I felt this was subjugating

research and the directorship to a subordinate place and naturally objected. The Laboratory staff felt the same way; most of the senior staff then left to set up what became known as the "Howe Laboratory, South" at the National Eye Institute in Bethesda.

Hughes: Does the Howe Laboratory own its space?

Cogan: No, it does not. It's interesting that you should ask that question because it was agreed in the terms of the Howe gift that a separate building would be constructed as soon as possible. Instead of that, the Laboratory was brought down to the Infirmary from its primary quarters in the medical school and the Bussey Institute, with the rather loose understanding that the Infirmary would provide the space. One of my problems in later years was the absence of any anticipation for expansion in this agreement.

Hughes: Well, the Laboratory did expand in 1956.

Cogan: Yes, it did, thanks to the benevolent cooperation of Mr. Henry Meyer, chairman of the Infirmary trustees, and the assistance of the Lions clubs of Massachusetts.

Hughes: Was there construction involved?

Cogan: Yes, we occupied one of the floors in the old outpatient building and later built a new penthouse on top of the building.

Services to the Hospital

Hughes: What about service functions of the Laboratory?

Cogan: Those of us who were clinicians were also staff members of the Infirmary. We served in the clinic just as did other volunteers.

Hughes: How many hours a week?

Cogan: Officially one day per week in the clinic, as well as committee meetings, surgery, consultations, and other hospital business.

Hughes: Did you participate in surgery as well?

Cogan: I did until 1962, when I was given permission to be relieved of all clinical activities because of my new position as chief editor of the *Archives of Ophthalmology*.

Hughes: Was it easy to be relieved of the clinical duties?

Cogan: Not really. The fear was that it might set an embarrassing precedent when others wanted to be similarly excused for their special assignments. But Dr. Dunphy was most sympathetic and allowed me to resign from the clinic.

Hughes: How did Dr. Chandler fit in?

Cogan: Aside from being one of the benefactors of the Laboratory, he was a close collaborator with Dr. Grant in clinical problems of glaucoma.

Hughes: Would photography, pathology, and tonography and some of the other activities in the Howe Laboratory provide services to the hospital?

Cogan: Absolutely. Dr. Donaldson's photography and Dr. Grant's tonography were major contributions to the Infirmary and to ophthalmology in general. Dr. Chandler once said, with his tongue in his cheek, "Dr. Donaldson's pictures are so good we can make a diagnosis without seeing the patient." [laughter]

Hughes: As you mentioned, Dr. Howe, in setting up the Laboratory, had tried to restrict its activities to research. Did you indeed find when you took on these added service responsibilities that they hurt the research effort?

Cogan: I think all those functions which you cited (and I would add neuro-ophthalmology as well) were allied to the research interests of the staff. They served research functions as well as service functions.

Hughes: Please tell me about tonography.

Cogan: Tonography is a method for measuring the outflow facility of fluid in the eye. It was developed by Dr. Grant, first on enucleated eyes, then on intact human eyes, as an indicator of a patient's risk for developing glaucoma or as a measure of his responsiveness to treatment.

Hughes: When tonography came along, was there ever danger that the flood of referral patients would upset the research program of the Howe Laboratory? I imagine the Laboratory could have been turned into a diagnostic glaucoma clinic since the need was there.

Cogan: That might have been a problem, but Dr. Grant was made chief of the glaucoma clinic, which was situated in the outpatient department. Most of the routine tonographies were done there rather than in the Laboratory.

American Optical Company and Bausch & Lomb Company

Hughes: For many years the American Optical Company and, I believe, the Bausch & Lomb Company provided money for the Laboratory. Was it usual in the immediate post-World War II years for ophthalmological institutions to get support from industry?

Cogan: My first effort to obtain support from industry on a more or less continuing basis was my approach to the American Optical Company. It was successful, and I was elated. To be sure, it was only $6,000 a year, but that was equivalent to an income from an endowment of $100,000. It provided a much-needed contingency fund for new investigators. Thus I was able to provide some support for Charles Schepens when he joined the Laboratory, fresh from abroad, to develop his method of binocular ophthalmoscopy for retinal detachment surgery. This seemed particularly appropriate since the instrument might have some commercial value for the company. So it did, and this culminated in the demise of the support to the Laboratory. Dr. Schepens founded an independent research and clinical organization [the Retina Foundation], with the director of research [Paul Boeder] at AO as its chairman.* My first venture in fund raising on a major scale thus came to a disillusioning end.

We then did secure some funds from Bausch & Lomb, but this was on a small scale, in the nature of consultation services rather than outright support.

Hughes: Was Harvard used to having this sort of arrangement with industry?

Cogan: I don't know, but I believe Harvard was leery of outside support from industry.

Hughes: What do you think of the tight relationship nowadays between departments of ophthalmology and industry?

* See the section in this oral history on Dr. Schepens for further discussion.

Cogan: It's dangerous but widespread. Once, when Elek Ludvigh asked me about patenting one of his inventions, I sounded out the then dean (who I think was George Berry). I suggested the medical school might hold the patent and cited [Harry] Steenbock's discovery of vitamin D as an example. I understood that income from that patented discovery supported most of the medical research at Wisconsin for years. The dean replied, "Yes, but when the patent ran out, the university requested another discovery to continue the support." I took that to be my answer and did not pursue it further.

Research on the Cornea

Hughes: Your Howe Laboratory reports begin in 1944, so I can't tell exactly when the work on the cornea began, but I'm guessing the late thirties, when you began the study of permeability of the cornea.

Cogan: Yes, the cornea was my initial interest in research, but my preliminary experiments were pretty haphazard until Dr. Kinsey joined me. Although his interest was chiefly in the lens, he graciously shared some of my interest and much of his time in corneal permeability studies.

Hughes: Why were you interested in the cornea?

Cogan: In my naiveté, I thought the cornea was a very simple tissue. Here we have a structure consisting of uniform connective tissue bounded by simple epithelium and endothelium with an interesting basement membrane. We ought to be able to know all about its physiology in short order. That was the illusion I was living by.

Hughes: It wasn't quite as simple as you originally thought, was it?

Cogan: Certainly not. I had published a paper on experimental production of bullous keratopathy that pointed up how little we knew about the permeability of the corneal epithelium.* After a few pilot experiments with Dr. Kinsey, we decided to go large scale, requiring a vast number of corneas. Because of their large size, cat corneas seemed most desirable, but we had neither the space nor funds for securing and housing the large number of animals which we required. Moreover, we did not need live animals, all we needed was excised corneas. It occurred to me

* Cogan DG. Experimental production of so-called bullous keratitis. Arch Ophthalmol 1940; 23:918-25.

that we might obtain the tissue from Boston's animal pound, where large numbers of alley cats were disposed of daily.

Well, to make a long story short, I did interview the responsible authorities and did get permission to get fresh tissue immediately after the animals were executed. Those whom I interviewed were not especially interested in my permeability project, but they were impressed by my claim, which I believe is correct, that cats do not get old-age cataracts. The possibility arose that we might find something useful in prevention of cataracts, in which they had a personal interest. I was thus given permission to get the tissue but not to remove the eyes on the grounds.

Thus it came about that Dr. Kinsey and I set up a routine whereby we would go to the pound armed with a large basket and a choice Havana cigar for the executioner and come away with the carcasses of a dozen or so cat bodies. This was our routine two to three times a week for months on end. In the Laboratory, we promptly removed the eyes, excised the corneas, and set up our battery of permeability experiments. This was poor man's research done at negligible cost.

Hughes: What would have happened to the cat carcasses if you had not taken them?

Cogan: They were picked up by a rendering company and their fur [was] eventually removed for lining gloves. Sounds gruesome, doesn't it? It would have been so much simpler for us to have removed the eyes on the spot, but that was not allowed.

Hughes: What were your experiments in the Laboratory?

Cogan: We tied the corneas on the ends of specially prepared pipettes filled with fluid of predetermined composition and immersed the loaded end of the pipettes in a flask of known fluid content. We had as many as twenty corneas set up at one time, with appropriate stirring devices. In this way, we used thousands of corneas for the experiments and controls in a machine-like operation.

Hughes: By the way, this was before the war?

Cogan: Yes. With the onset of the war, we had to stop most of these studies. I had hoped to continue them with the assistance of medical students and others not eligible for the draft, but a crisis arose when it was pointed out that one of these students was a

German-born refugee. It was my first confrontation with security regulations.

Hughes: Did the restriction apply to Japanese as well?

Cogan: I had no Japanese in the Laboratory at that time.

Hughes: So that stopped the cornea research until after the war?

Cogan: Not entirely. Dr. Kinsey had to stop because of his commitment to the war gas project. I continued to some extent with available personnel.

Hughes: Well, my understanding of what you were finding in the permeability studies is that the corneal epithelium and endothelium are permeable to fat-soluble substances, particularly electrolytes.

Cogan: You've done a lot of homework. Yes, the epithelium is permeable only to fat-soluble solutes, the stroma is permeable only to water solutes, and the endothelium appeared to be soluble only to fat-soluble solutes. Drugs, for instance, must have a biphasic solubility to penetrate the entire cornea.

Hughes: Why is it set up that way?

Cogan: The cornea must, of course, be transparent. A by-product of these permeability studies was to show that the transparency of the cornea depended in large measure on a continual osmotic removal of water from the cornea. We showed, for instance, that the corneal epithelium was far and away the most nearly perfect semipermeable membrane in the body. The key to transparency was a maintained deturgescence of the cornea effected by way of the surface membranes. The fundamental difference between the cornea and sclera, as far as transparency is concerned, is that the cornea is kept deturgesced, whereas the sclera, having no similar surface membranes, is fully turgid under physiologic conditions.

Hughes: How much of this was known before you started out?

Cogan: I believe the concept and its practical applications were new.

Hughes: There was some reference to glycerol to clear corneas.

Cogan: Yes, glycerol or any hypertonic solution will clear epithelial edema, but that is a minor application.

Hughes: Did any of the drug companies pick up on this research and try to produce products that were compatible with what you were finding in the cornea?

Cogan: I know our hospital pharmacist, who later formed his own company, did, but I'm not sure what others have done with it.

Hughes: It seems such a logical interest for the drug companies.

Cogan: I would have thought so. The studies were published in a series of papers covering different aspects of permeability, but I do not know how many persons read them. It was for these studies, however, that we were given the Warren Triennial Prize of the Massachusetts General Hospital.

Hughes: In the mid-forties, perhaps earlier since it is difficult to tell from the Howe Lab reports, you were studying vascularization of the cornea. Was that a logical next step?

Cogan: Only insofar as I was interested in all aspects of corneal processing. Among the cornea's extraordinary anatomic features is the fact that it contains no blood vessels. Yet the blood vessels in the adjacent sclera have no anatomic barrier to prevent them from growing into the cornea. How to explain this enigma? Our clinical and experimental evidence convinced us that the answer lay in the compactness of the cornea adjacent to the sclera. This simple hypothesis seemed reasonable to us, but I don't think it is the popular impression at present.

Hughes: What is the popular notion?

Cogan: Most persons now subscribe to a specific neovascularizing factor as popularized by Dr. Judah Volkman of Harvard's Children's Hospital. He and his followers propose a sort of specific growth substance, whereas our contention assumed a mechanical factor. Like so many divergent claims in biology, there may be truth in both points of view.

Hughes: Have Dr. Volkman or others found the stimulating factor?

Cogan: There are claims to this effect, but I have insufficient experience to evaluate them.

Toichiro Kuwabara

Hughes: After the war, you began to be interested in corneal lipogenesis. Is that true, and what was Dr. Kuwabara's role in these studies?

Cogan: After the war, fat deposits in the cornea became a prime clinical interest of mine. The experimental studies did not get off the ground, however, until Dr. Kuwabara joined me. I would like to tell you something about that extraordinary person and his crucial role in these and other studies.

I needed someone to prepare the histologic material for the experiments which I planned and for which I had secured a small sum of money from Boston's Junior League to hire a technician. Unable to find a suitable person in this country, I wrote to Dr. Harochi Ikui, an ophthalmic pathologist in Japan with whom I had had contacts while serving with the Atomic Bomb Casualty Commission in the late 1940s. He recommended a young pathologist, Dr. Toichiro Kuwabara, who would be interested. I replied I was looking for a technician, not a physician. Dr. Ikui replied (ambiguously) that Dr. Kuwabara's father was agreeable to have him come to the United States. Thus began my correspondence with Dr. Kuwabara, whom we got to know affectionately as Toichi, and an initial trial period of a year.

Despite our language barrier and what must have been a cultural shock for Toichi, it was obvious that interests and personalities bode well for a long-time association in ophthalmic research. There was a problem, however. After being in this country for a year, he had to return to Japan under the terms of the McCarran Act. Then began the six months or more waiting period while we made extensive arrangements for his return. Eventually we were successful, and Toichi arrived, this time with his wife and two little girls. Thus began our association of more than thirty-five years.

*Hughes: Dr. Kuwabara told me you negotiated some kind of congressional dispensation for him, with the help of Senator Leverett Saltonstall.**

Cogan: Yes.

* Interview with Toichiro Kuwabara, National Eye Institute, Bethesda, Maryland, May 23, 1989.

Hughes: Was the family thinking that living in the United States was going to be a lifetime proposition?

Cogan: I don't know. Japanese wives do, or did, pretty much what their husbands directed.

Hughes: And then what?

Cogan: Work on the corneal fat studies was going well. We found that the corneal cells had an extraordinary property to generate neutral fat when, under specific conditions, they were exposed to fatty acids. Dr. Kuwabara's lipid stains of excised and incubated corneas showed the process beautifully. We called it "aberrant lipogenesis." We were excited because the phenomenon had relevance not only to clinical manifestations in the cornea but to connective tissue throughout the entire body.

But then we had a blow. After an absence of several days at a meeting, I returned to the Laboratory on a Friday to find that Toichi had gone home with a "cold." Before leaving, however, he had an x-ray taken of his chest. When I reviewed the x-ray film, it was obvious, even to my inexpert eye, that he had tuberculosis with the characteristic cavitation in the upper pole of one lung. This was a crisis. Were it to be publicly known, he, as an alien, would have to return to Japan. And how about possible tuberculosis in the family? And what would Toichi's reaction be when he learned about the x-ray?

A weekend of flurried activity proved worthwhile. If one had to have tuberculosis, this was a good time to have it. Sanitoria were beginning to empty because of the successful treatment with streptomycin; endowed beds were available in private facilities. Through the gracious assistance of Dr. Henry Allen, a trustee of one of these sanitoria, a bed was acquired for Toichi. X-rays and other testing of the family showed no tuberculosis. By Monday, we had a definitive plan to present to Toichi. Six months or more of streptomycin treatment in the sanitorium and a concurrent self-taught course in histochemistry had a happy ending.

From Lipogenesis in the Cornea to Retinal Vasculature

Hughes: Was it after this episode in the sanitorium that the lipogenesis studies were developed?

Cogan: They were greatly advanced then. We were able to show that synthesis of neutral fat by connective tissue cells and some

epithelial and parenchymatous cells serves the purpose of removing free fatty acids, which are otherwise very toxic. Moreover, we were able to show that fatty degeneration, which was commonly believed to be a passive release of fat from the destroyed tissue, appeared on the other hand to be an actual synthesis of neutral fat from the liberated fatty acids. We also felt we could show in the scarred cornea a facsimile of atheromatous plaques of blood vessels.

Hughes: Are those concepts generally accepted?

Cogan: No. I think they have been generally ignored. We published our results and interpretations in a series of papers in the *Archives of Pathology*.* They generated little or no response. In a way, this is more disappointing than criticism. To receive criticism is often constructive but to be ignored leaves a void. Are our observations and conclusions as important as we think they are?

A serendipitous observation caused us to switch from corneal lipogenesis to retinal vasculature. The story as I recall it, but not necessarily the way Dr. Kuwabara may relate it, follows.** While subjecting excised corneas to various agents prior to exposing them to fatty acids, we used, on one occasion, trypsin. Included with the specimen was, by chance, a piece of retina. When Dr. Kuwabara subsequently stained the specimen, all that was left of the retina was a vascular network showing two types of cells in its capillary walls. It was immediately obvious that we had here a novel method for demonstrating the cellular components of the retinal capillaries. Specifically, there were cells embedded within the basement membrane as well as endothelium lining the lumina. We called the former "mural cells" to distinguish them from the usual pericytes that surround capillaries in most other regions of the body. Subsequent observations proved that the mural cells were the contractile cells for the retinal capillaries.

More significant than the simple microanatomy, however, was the subsequent observation that they were specific target organs for one type of diabetic retinopathy. Because of a close association with the pathology department of the Deaconess Hospital and the Joslin Diabetic Clinic, we were able to study a large number of diabetic eyes removed postmortem. The number was in excess of 3,000! It became apparent that the earliest pathological change

* Cogan DG, Kuwabara T. Lipogenesis by cells of cornea. Arch Pathol 1955; 59:453-6. Kuwabara T, Cogan DG. Experimental aberrant lipogenesis. II. Substrate factor. Arch Pathol 1957; 63:496-501. Kuwabara T, Cogan DG. Experimental aberrant lipogenesis. IV. Production of nonsudanophilic fat. Arch Pathol 1959; 67:34-8.

** Dr. Kuwabara expressed his viewpoint in an interview conducted on May 23, 1989 at the National Eye Institute, Bethesda, Maryland.

was a selective loss of the mural cells, resulting in distension of the capillaries, microaneurisms, and shunt vessels. The Kuwabara trypsin digest technique became a standard method for the histologic study of retinal vasculopathy and a starting point for the study of diabetic retinopathy.

Electron Microscopy

Hughes: When did electron microscopy enter the work on retinal vascularization?

Cogan: Dr. Kuwabara was a pioneer in electron microscopy of the eye. In fact, it was because of him, I believe, that Jelco, the Japanese Electron Microscope Company, established its offices in suburban Boston. He was the advisor and scientific beneficiary of the company.

Hughes: Do you remember when electron microscopes began to be used in scientific and medical research?

Cogan: Dr. Kuwabara tells me that RCA machines first became available about 1952. When the Howe Laboratory moved into its new quarters in 1956, arrangements were made to install one of the superior Jelco machines.

Hughes: The 1960 Howe Laboratory report says, "An electron microscope has now been finally acquired at the Howe Laboratory and was just put into operation at the end of the year," but the report refers to 1959, doesn't it?*

Cogan: Possibly as early as 1958.

Hughes: Were there electron microscopes at Harvard?

Cogan: There must have been. Don Fawcett, professor of anatomy at the Harvard Medical School, was an early authority on electron microscopy.

Hughes: How did Dr. Kuwabara set about learning electron microscopy?

Cogan: He learned it by doing it. That was always his approach. He was critical of himself and of others and persevered until he was able to develop superior results. He is a perfectionist. In 1962, he introduced my wife to the technique of preparing tissue for electron microscopy and cutting sections.

* p. 9.

Hughes: Did Dr. Kuwabara also develop some of the techniques, the fixation methods, and other steps in the elaborate protocol for preparing electron microscopy specimens?

Cogan: He developed his own technique for doing things. He did what he found worked well in his own hands. Of course, the whole process made a major advance when glutaraldehyde was used as a fixative instead of, or in addition to, osmic acid. Dr. Kuwabara has recently added a further refinement for demonstrating lipoprotein particles in tissues by combining osmic acid and formalin fixation to specimens.

Hughes: He told me as well that with electron microscopy he could see actual muscle fibers in the pericytes and mural cells. What forms the muscle fiber?*

Cogan: It is the interaction of actin and myosin which account for the contractility of muscle cells. Actin is also found in many other cells which are contractile but not ordinarily listed as muscle cells.

Hughes: I have a note that Michio Oikawa came from Japan to help Dr. Kuwabara with the electron microscopy.

Cogan: He was a fellow with us for two years. He was already a sophisticated electron microscopist. Would you care to hear an interesting story about him?

Hughes: Of course, I would.

Cogan: Prior to the war, Michio's father had held a position as secretary of the navy, or whatever the comparable position was in Japan, but had been removed from office because of his disapproval of the attack on the United States. Nevertheless, this military connection was sufficient, so I was told, to disallow young Michio to go to medical school. Thus his entrance into electron microscopy. I don't believe he ever acquired an MD degree.

Hughes: Well, the other name in connection with electron microscopy was Arnold Kroll.

Cogan: He was a fellow in the Howe Laboratory and a clinical trainee at the Infirmary. He is now on the staff of the Infirmary.

* Interview with Dr. Toichiro Kuwabara, May 23, 1989.

Sally, with your permission I would like to add a note about several members of the Laboratory who have become nationally prominent.

Jin H. Kinoshita

Cogan: Possibly the greatest single contribution of the Laboratory, at least from a therapeutic point of view, has been Dr. Kinoshita's elucidation of the underlying processes in diabetic complications and a rational direction for their treatment. But before describing these, I would like to tell you a bit about Dr. Kinoshita himself.

Jin's home was in California at the outbreak of the war. When Jin was seventeen, the Kinoshita family was interned at the Santa Anita Race Track along with 25,000 Nisei and other ethnic Japanese. The Kinoshitas were later relocated in the Midwest on a sugar beet farm. Thence Jin was given the opportunity to attend Bard College in the East. He distinguished himself in chemistry and on graduation was admitted to the PhD program in the biochemistry department of Harvard Medical School. The chairman of the department was Professor Baird Hastings, but Jin's special advisor was Professor Eric Ball.

I have previously narrated our recruitment of Dr. Kinoshita to the Howe Laboratory. I would only add it was our great good fortune. For he not only became an apostle for the position of a biochemist in an ophthalmic research laboratory (see the introductions to his Friedenwald Lecture)* but a model for others to follow. Together with devoted teams, he developed models for diabetic complications, demonstrated the osmotic effects in the lens and other tissues from sugar accumulation, proved the pathogenetic relation of the polyol to the presence of aldose reductase, and showed the beneficial effect of inhibitors of this enzyme. This was begun in the several years he was at the Howe Laboratory but was considerably extended when he moved to the National Eye Institute. There, in addition to his research, he has been scientific director but now plans to move back to California in semiretirement.

* Kinoshita J. Invest Ophthalmol 1965; 4:782.

Other Researchers

Cogan: Dr. Kupfer joined us in the late 1950s fresh out of his residency at Hopkins and was with us at least a decade. His multiple research interests included continuous recording of outflow of aqueous humor using a polyethylene tube implanted in the anterior chamber, measurement of transcorneal potential showing its dependence on the epithelium, electromyography of the ocular muscles in human beings, and, in collaboration with Dr. Jack Downer of University College, London, studies on optic nerve interactions with the lateral geniculate bodies. Dr. Kupfer was also in charge of the NIH training grant assigned to the Howe Laboratory. Toward the end of the 1960s, he left to become chairman of the Department of Ophthalmology at Seattle. In 1970, he was appointed to his present position as the first director of the new National Eye Institute.

Bob Reinecke's first contact with us, as far as I can recall, was when he was a summer student in the Laboratory. Later, he had his residency at the Infirmary with ongoing activities with us, and still later, he became a fellow and eventual staff member. During his dozen or more years on the staff, he pursued multiple research projects, adventures in teaching methods, and activities of an organizational nature. (His researches included demonstration of the prominent muscle involvement in temporal arteritis and other projects with Dr. Kuwabara. His teaching activities included programmed and computerized texts on refraction and strabismus. His organizational activities included operation of the Vision Information Center in conjunction with Harvard's Countway Library and the development of a random dot test for testing binocular vision and stereopsis.) Finally, he left to become chairman of the department at Albany and still later chairman at Jefferson. This year [1989], he is president of the American Academy of Ophthalmology.

During his residency at the Infirmary, Dr. [Herbert F.] Kaufman held a joint appointment in the Howe Laboratory for the study of infectious disease. This was initially directed toward toxoplasmosis, but with his interest in the cornea, it became more a study of herpetic keratitis. Out of this, in turn, came the breakthrough discovery that experimental herpetic infections could be controlled by a thymidine analog (iododeoxyuridine or IDU). It was similarly found to be effective in human beings, the first antibiotic for viral infections, not only for the eye but for viral infections elsewhere!

Hughes: I saw a note that Ephraim Friedman had done research on fluid transport in the retina.

Cogan: Dr. Friedman's experience in the Laboratory and his subsequent course were unusual. After his residency at the Infirmary, he probed several research outlets, especially physiology and pathology. He devised a system for photography of the choroidal and retinal circulation in animal eyes, with microscopic magnification and cinematic recording and a further method for measuring the relative blood flow in the two tissues by using radioactive tracers. With Dr. Taylor Smith, he pursued ophthalmic pathology and retinal detachment surgery.

With this background and a latent administrative talent, he was appointed to the chairmanship of the Department of Ophthalmology at Boston University. There, in short order, he became dean of the medical school. Not much later, he was appointed dean at Albert Einstein Medical School in New York, where he served for eight years. His most recent appointment was a return to the Infirmary in Boston to serve in a new post as president. His has been a kaleidoscopic career.

Hughes: Full circle.

Nonsyphilitic Interstitial Keratitis

*Hughes: I'd like to move on to nonsyphilitic keratitis. I found several papers, there might have been more, following up each time you added more patients.**

Cogan: Well, the definitive paper is a report not by me but by Drs. Barton Haynes, Muriel Kaiser, et al. here at NIH.**

Hughes: Well, let's start with yours. The first paper was published in 1945 and describes four cases. Will you tell me why you became interested in the syndrome?

* Cogan DG. Syndrome of nonsyphilitic interstitial keratitis and vestibuloauditory symptoms. Arch Ophthalmol 1945; 33:144-9. Cogan DG. Nonsyphilitic interstitial keratitis with vestibuloauditory symptoms. Arch Ophthalmol 1949; 42:42-9. Norton EWD, Cogan DG. Syndrome of nonsyphilitic interstitial keratitis and vestibuloauditory symptoms. Arch Ophthalmol 1959; 61:695-7. Cogan DG, Dickerson GR. Nonsyphilitic interstitial keratitis with vestibuloauditory symptoms. Arch Ophthalmol 1964; 71:172-5.

** Haynes BF, Kaiser-Kupfer MI, et al. Cogan's syndrome. Studies in thirteen patients, long term follow-up and a review of the literature. Medicine 1980; 59:426-41.

Cogan: While serving in the eye clinic, I happened to see the four young patients described in the initial report, with a mild and fluctuating inflammation of the cornea. They also had abrupt vertigo and deafness. This was an unusual combination of symptoms. Although congenital syphilis is associated with keratitis and deafness, the symptoms in these patients were unlike the acute keratitis and slowly progressive deafness in congenital syphilis. I found no description of such a condition in the literature. I reported the condition as "interstitial keratitis and vestibuloauditory symptoms." Four years later, I had contact with four more patients, whom I reported. By then, others had reported seeing patients with the entity, and the condition was coming to be known as "Cogan's syndrome." Apparently that was easier to address than the term I had used.

My fourth paper on the subject, reported with Dr. G. R. Dickerson, described the case of a young man who died of a necrotizing aortitis, adding to the growing evidence that the condition was some sort of vasculitis. Many cases were then appearing in the literature. In the definitive paper by Drs. Haynes, Kaiser, et al. in 1980, these authors examined many of my original patients and reviewed some 100 or so reported in the literature. They showed that the typical syndrome was indeed a manifestation of some kind of systemic vasculitis. With my name attached to the syndrome, I am repeatedly embarrassed that I know so little about the etiology of the condition. I take the liberty, therefore, of referring enquiries to Dr. Haynes who is an internist, now at Duke University, and continues his study of the syndrome.

Hughes: One of the points you were trying to make was to distinguish it from syphilitic keratitis?

Cogan: True.

Hughes: In syphilitic keratitis can you see the organism?

Cogan: Not when the patients are having the active disease. Spirochetes have been found, however, in the corneas of infected newborns, but they have no manifestations of the disease at that time.

Hughes: If you can't see the organism, it's all the more important to be familiar with your work.

Cogan: Maybe so. The consensus is that it represents an autoimmune disease. The interesting feature is that it affects the same organs

as does congenital syphilis, including the aorta as well as the cornea and auditory-vestibular apparatus.

Hughes: But the onset seems to be at a different pace.

Unitarian Service Mission to Germany, 1948

[Interview 3: May 27, 1989]

Hughes: In 1948, you participated in the Unitarian Service Mission to Germany. Could you tell me about that, please?

Cogan: The Unitarians had a noble idea of helping the world get back on its feet after the war. As you may know, they have much in common with the Quaker philosophy. Specifically, they sent several scientific (including medical) "missions" to foreign countries to make contact with civilian counterparts outside the military. I was fortunate to be invited to serve with the group to Germany. I think we were the first nonmilitary group that visited Germany in such a capacity.

We were a group of fifteen persons, representing a variety of specialties including general surgery, internal medicine, biochemistry, organic chemistry, anatomy, pharmacology, obstetrics and gynecology, anesthesiology, and other disciplines. Our chairman was Otto Krayer, professor of pharmacology at Harvard Medical School. He was originally from Germany and arranged the program superbly. We spent one week at each of ten universities, exchanging lectures and meeting with the faculties as a whole, with our particular counterparts separately, and with the students. Each of us had prepared ten lectures, from which the local faculty chose which ones they wished us to present. Thus I was asked to give two or three but never more at any one university. The German faculty gave an equal number of lectures. We also brought with us a small collection of books and journals for each of their libraries.

We had an evening banquet each week when we fraternized with our colleagues from the German faculty. We, of course, provided the food from the PX [post exchange], since the Germans were still on near-starvation rations. This was prior to the money reform, and Germany was still an occupied country. This evening session was most important in breaking down the suspicion that the Germans had of our motives. In fact, we discovered at our banquet in Freiburg that the word *mission* had aroused suspicion of our intent.

Hughes: The sessions were in German?

Cogan: Yes.

Hughes: So your German was good?

Cogan: It was alright. I've always liked the German language.

Hughes: Ability to speak German must have been one of the criteria for selection for the mission.

Cogan: I'm sure it was.

I might add that although the Unitarian motivation was to reestablish contact with the civilians, it was an extraordinary experience for us as participants. It is hard to believe the chaos of Germany at that time and the confused frame of mind of the people who had been under Hitler for over a decade and now were living under the Occupation, with which they had little communication.

Hughes: Were you focusing on ophthalmology?

Cogan: Yes. My lectures were all related to ophthalmology.

Hughes: Did you visit the ophthalmology departments at different universities?

Cogan: We had one day when we visited our counterparts. I did visit the ophthalmology departments on that day.

Hughes: Did that trip somehow lead to your later election to honorary membership in the German Ophthalmological Society and to your more recent several months in Munich as an awardee of the Alexander von Humboldt-Stiftung [1988]?

Cogan: In general, yes, but most particularly to a specific incident that occurred at that time. I'd like to tell you that story because it shows how trivial things can have such unexpected results.

Hughes: I'd like to hear it.

Cogan: Before the Hitler period, Heidelberg had been the sentimental as well as the scientific site for the annual meeting of the German Ophthalmological Society. The meetings were held in the castle [*Schloss*] overlooking the Neckar River. After Hitler took over in

1932 no further meetings were held; however, 1948 was to be the year when they were to begin again. The invitations were all out, Professor Ernst Engelking at Heidelberg had meticulously made all the arrangements including permission from the US Army for use of the castle. There were even prospects of some of the East German ophthalmologists being allowed to come. Then Professor Engelking was abruptly told, so he thought, that the castle would not be available to the Germans. There simply was no other place for them to meet. This was a catastrophe as far as Professor Engelking was concerned, and it happened just before my visit to Heidelberg.

When I heard the story, my thought was that this was my challenge as a member of the Unitarian mission to smooth relations between the United States military and the civilian organizations. So, with more bravado than I thought I had, I marched out to the Wiesbaden command and asked to see the general in charge. Well, of course, I never got to him, but I did see a most understanding colonel who got on the phone and found that this debacle was all due to a lack of communication. The United States order had been sent out because the Germans were no longer able to finance their regularly assigned evenings at the castle. If the ophthalmological society could support its use for their meetings, they could by all means have it. So it was arranged then and there.

I returned to Engelking who had never objected to orders from above (because of his poor English, I guess). When I told him the castle was his for the meeting, he thought I must have an "in" with Eisenhower. I could not disabuse him of the idea that it was all just poor communication. From then on I was a local hero. I was made an honorary guest of the society, put on the program as giving one of the major addresses, and met with the council. I'm sure this had a lot to do with my subsequent association with the German Ophthalmological Society.

Hughes: How did you find the state of German medicine in 1948?

Cogan: The isolation under Hitler and the deprivations of the war had taken their toll. Yet the Germans are a proud people, and we had to be careful not to give the impression of arrogance.

Hughes: Were they receiving foreign journals?

Cogan: Only a few. That is why we brought libraries with us to give each place we visited.

Hughes: What did they consist of?

Cogan: Books and journals.

Hughes: Had the Unitarians bought those books?

Cogan: They were donated.

Hughes: Remarkable.

Cogan: Yes. It was a an admirable venture on the part of the Unitarian Service Committee, all the more since none of us, so far as I know, were Unitarians. The group consisted of Protestants, Catholics, Jews, and agnostics. It was entirely a secular affair.

Hughes: Did the Unitarians do anything similar in Japan?

Cogan: Not that I am aware of. They did send a group to the Philippines. They sent missions only to those countries which requested them. I don't know how the host countries became aware of the availability of a mission.

Alexander von Humboldt-Stiftung Award, 1988

Hughes: Well, then, let's skip forward in time to the Humboldt-Stiftung Award.

Cogan: *Stiftung* means "foundation."

Hughes: What were you expected to do?

Cogan: The letter of invitation was a surprise to me. Apparently the foundation makes a half dozen or so senior scientist awards each year, inviting an American to spend up to eight months at some university for the purpose of study and research. It is prompted by gratitude for what this country's Marshall Plan did for Germany after the war.

Hughes: Are all the recipients Americans?

Cogan: I believe so.

Hughes: In medicine, or in any field?

Cogan: In various scientific fields, not especially in medicine. It is well supported and very efficiently arranged. The recipient is

provided with a small certificate (*Beweis*) which serves as a passport in traveling about Germany.

Hughes: Did you choose the laboratory that you were associated with?

Cogan: One has complete freedom to choose where he wishes to go and no restriction for a thesis.

Hughes: Why did you choose Munich?

Cogan: Both my wife and I are fond of Munich. The Alps are nearby. The university is prestigious. But, most especially, because I was familiar with Professor O. E. Lund, chairman of the department, and with Professor Fritz Stefani, the ophthalmic pathologist at the clinic. I planned to spend my time in pathology.

Hughes: In what laboratory?

Cogan: Ophthalmic pathology. The quarters were delightful, and my hosts were cordial to the extent of embarrassment. We also did some traveling, including a week in Berlin, East and West, attending the annual meeting of the Deutsche Ophthalmologische Gesellshaft, and a number of days in charming Würzburg with Professor Anselm Kampik.

Hughes: Did you actually do some research?

Cogan: In Munich, I spent most of my time reviewing pathology slides in Dr. Stefani's collection. This included the material that had been presented at the European meetings in previous years.

What a contrast the Munich clinic is today compared with my previous visit in 1948. Half the clinic was then in shambles after being struck by two bombs. Now it is a superb, well-equipped, modern eye clinic!

Hughes: Did you feel at home in the eye clinic?

Cogan: My hosts made me feel at home. I had hoped to return some of the cordiality two weeks ago by having Professor Lund be an honorary guest of the American Ophthalmological Society, but he had to cancel the trip on account of illness in the family.

Hughes: Does the German medical system still retain a hint of the old hierarchy in which the Herr Professor is very much above everyone else?

Cogan: To some extent perhaps, but I'm sure it's highly variable. The position of chief is much more coveted in Germany than it is in this country. It's the ambition of most young academic aspirants. It carries with it more authority. I think it results in a more efficient operation. Perhaps our commitment in the United States to democracy has gone too far.

Hughes: Does the professor practice ophthalmology?

Cogan: Yes. In general, he has the prime private practice whereas most of his junior staff are full time [academics]. There is a wide discrepancy in their incomes.

Atomic Bomb Casualty Commission, 1950

Hughes: Do you have memories of the bombing of Hiroshima and Nagasaki? Do you remember how you heard, and what your reaction was?

Cogan: That was one of the times in our lives when I guess we can recall just what we were doing when we heard the news. I was spending a couple of weeks at our summer retreat in Leland, Michigan. I was actually driving up our roadway with the car radio turned on. It was a shock. Curious how the brain records those essentials and nonessentials all mixed up at the time of a crisis. I can similarly remember just where I was and what I was doing when the news broke of Jack Kennedy's death.

Hughes: Did you immediately begin to consider the effects of radiation on survivors?

Cogan: We must all have had mixed feelings about dropping the bomb. Even [J. Robert] Oppenheimer did. I remember thinking how much better it would have been to bomb a deserted island off the coast of Japan and then announce the intent of bombing cities if the Japanese military did not capitulate. Of course, that was the free speculation of one who knew nothing about the availability of other bombs, etcetera. Subsequently it became apparent that our whole armamentarium was two bombs, and it was unlikely that others could be made in reasonable time.

Hughes: And they were not absolutely sure that those two bombs would work.

Cogan: I guess that's right.

Hughes: But you didn't immediately think "cataract" when you heard the news?

Cogan: No, I don't think so, Sally.

Hughes: Well, how did it happen that you were appointed to go to Hiroshima and Nagasaki in 1950 on the Atomic Bomb Casualty Commission?

Cogan: Because of my connection with cataracts in cyclotron workers, I was obviously a candidate. But, more significant, perhaps, I was available.

Immediately after the war, the Atomic Energy Commission [AEC] set up the Atomic Bomb Casualty Commission [ABCC] to survey the effects of the two bombs. When I arrived there in 1950, it was a joint operation with the Japanese and was largely run by epidemiologists from the States. My assignment was to set up an eye clinic to find out if cataracts had resulted from the radiation. I was reasonably certain, erroneously as it turned out, that we would not find cataracts because the cataractogenic dose was two or three times the lethal dose for whole body radiation. I assumed that any who had received enough radiation to cause cataracts would not have survived.

The plan was for me to go to Hiroshima for three months, set up the survey, and leave Dr. Samuel Kimura to continue the study for at least a year. The other person on the team was Dr. S. Forrest Martin, who was also to spend three months.

Hughes: Why was the Dr. Martin chosen?

Cogan: Of course, I knew Dr. Martin well. He was attached to the Howe Laboratory during the war to work with Dr. Ludvigh on antiaircraft range finding, but he had had no contact with radiation, and I was surprised he was assigned to be part of the team to go to Hiroshima. This apparently was Dr. Gundersen's influence, and I must say Dr. Martin was helpful in operation of the eye clinic at ABCC.

Hughes: Why was Dr. Gundersen involved?

Cogan: Dr. Gundersen emerged from the war an influential consultant in Washington administrative affairs and in veterans' services. In fact, I think he may have been initially sounded out as a candidate for the ABCC before I was approached.

Hughes: What about Dr. Kimura?

Cogan: Sam Kimura was at the time an ophthalmologist at the University of California, San Francisco, a co-worker with Phil Thygeson on infectious diseases of the eye. I believe I was responsible for his assignment, acting on the suggestion of Professor Fred Cordes, the head of the [UCSF] department.

Hughes: Kimura had no particular interest in radiation?

Cogan: That's right, but he was an excellent organizer and a key figure in our survey.

Hughes: Well, how did it work, once you got to Japan?

Cogan: Our first problem was finding the space and receiving the slit lamp biomicroscope and other equipment which we had ordered to precede our arrival. That took about two weeks. Then we had a strategic problem, an ideational difference between our plan and that of the epidemiologist in charge. Our plan was to see as many persons as possible who had been exposed and survived. We hoped for 1,000 patients in the first three months. The epidemiologist thought we were to make eye examinations only on those few patients whom they were calling in, possibly two or three per day. At that rate we would have seen only 100 or so persons, and the chance of finding radiation effects on the eye would have been slim. We finally agreed to do both, that is, examine their patients on whom they had extensive data and our patients on whom we had only such data as we solicited from the patients.

Hughes: What was the mechanism?

Cogan: Four or five Jeeps were assigned to ABCC, manned by Japanese. These persons and their vehicles made contact with designated persons to be brought in for examination. One knew the addresses where the persons to be contacted lived; one was the driver, and the other was an epidemiologist. A fourth person had made prior contact with the designated person. This seemed like an elaborate mechanism for a survey that we expected to be negative.

Hughes: Why didn't you expect to find cataracts?

Cogan: From our studies in Boston, we found the threshold dose of radiation to be the equivalent of 600 to 1,000 rads for single

exposures, whereas the lethal dose for whole body radiation was of the order of 300 to 400 rads. Thus if anyone received enough radiation to develop cataracts, he would not have survived. Our error was failing to consider the possibility of body shielding. This is illustrated by what I later called the saga of Hatsue Kimura.

Hatsue was on her way to a planned picnic with several of her classmates at the time of the bomb. She was standing in the front of a crowded streetcar near the center of Hiroshima. Her body was shielded by the bodies of several other persons standing in front of her, but her head was exposed to radiation from the bomb, which exploded 1,500 meters above ground. She developed loss of head hair indicative of her exposure, but no loss of body hair. Four years later, she began to complain of visual loss. She was then a kitchen maid working for ABCC, and we were asked to see her even before our equipment was set up. It was immediately apparent that she had the type of cataract which was compatible with that induced by radiation. Her history and age made this practically a certainty. This was then our first discovery of a positive eye effect of radiation and changed the entire outlook for our study.

Hughes: In the end, how many survivors did you examine?

Cogan: Well over 1,000. Cataracts were present in about 10% of the survivors who had been within one kilometer of the hypocenter. Cataracts occur only after a latent period which, in the case of Hatsue, was apparently three or four years.

Hughes: Is that the usual latent period?

Cogan: The latent period is an inverse function of the amount of radiation received; in other words, on the proximity to the bomb.

Hughes: How were you dealing with the language barrier?

Cogan: We had splendid help. There was no dearth of translators among the Japanese. Our secretary in the eye clinic was completely bilingual. Her home had been in California, but prior to the war, her parents had enrolled her, a girl of thirteen, in a girl's school in Hiroshima. This was apparently a common custom among Japanese-Americans. When the bomb went off, she extricated herself from the collapsed building and struggled through the park under the black cloud of smoke and amongst hundreds of sick and vomiting persons until she came to a river inlet. She got

across the river in a boat and finally got home several hours later. There were many such stories.

Hughes: What did they think had happened?

Cogan: Persons I talked to had, surprisingly, not heard the explosion. They first became aware of a high wind, collapsed buildings, and extensive fires of the predominantly wooden buildings. They had no suspicion of an atomic bomb. Hiroshima had not previously been bombed, but the newspapers had carried stories of the incendiary bombing of Tokyo. They assumed that was what had occurred to them. They called them Esso bombs.

Would you like to hear the story of a man who believes he was the first one in Hiroshima to discover it was an atomic explosion?

Hughes: Oh, I would love to hear it.

Cogan: His name was Dr. Shigeto. He was a radiologist but also the chief administrator of a hospital in Hiroshima. At the time of the explosion, he was waiting for a streetcar to take him to the hospital. Of course, all methods of transportation were eliminated by the bomb, and it took him several hours to get to his hospital. By then, the clinics were crowded with patients having a variety of injuries. His first thought was to get his portable power in shape for the x-ray machines. The next thing was to take pictures, but all the film seem to have been exposed even though it was wrapped in lightproof paper. But an accessory supply from the basement was intact. Only later, when he had time to think of the circumstance of the spoiled film, did it occur to him that the film had been exposed to radiation from an atomic blast. Of course, the rest of the world knew that an atomic bomb had been used, but the people of Hiroshima, deprived of outside communication, were ignorant of it. Only Dr. Shigeto knew.

Hughes: He put two and two together, with all the vomiting and other signs of radiation sickness?

Cogan: I'm sure that was true.

Hughes: Are there immediate effects on the eye in radiation sickness?

Cogan: Not as part of the general sickness syndrome. But we might have expected macula burns in those persons who were watching the parachute drop from the airplane when the blast went off. Surprisingly, we saw only one possible case. Perhaps the absence

of macula burns was due to the constricted pupil that the observers had on that bright, sunny morning. By contrast, there were a number of macula burns in observers of the Nevada tests, which were held at night when the pupils would have been dilated.

One patient reported to the clinic with optic atrophy, claiming it had been caused by the explosion. Well, it was in a way. He was watching the destruction of Hiroshima from a hillside at a safe distance of four kilometers from the hypocenter. His reaction was to consume a quantity of wood alcohol available in Sterno cans. He had blinded himself.

Hughes: What a terrible story.

Cogan: There were many such incidents.

Hughes: You were watching for all kinds of eye problems, not just cataracts?

Cogan: Right.

Hughes: But you didn't find much that was related to the bomb?

Cogan: You're right. I might add that we were not allowed to treat any patients.

Hughes: Why was that?

Cogan: General Douglas MacArthur thought that any treatment by Americans would tend to remove the initiative of the Japanese to develop their own resourcefulness. It was a general policy.

Hughes: What was the state of Japanese ophthalmology?

Cogan: That is hard for me to say. I visited a few clinics and private physicians; I sat in their offices and had tea with them. But I really couldn't make any judgment. On the other hand, I did get to know Dr. Ikui, the ophthalmologist at the Red Cross Hospital in Hiroshima. He showed me a number of interesting patients and pathologic slides. We kept in contact subsequently.

Hughes: Did you sense any resentment toward Americans?

Cogan: No. By contrast, there was a lot of resentment in the United States that America had used the bomb.

Hughes: The ABCC was a combination of Japanese and American personnel?

Cogan: Yes. At that time it was mostly Americans but became progressively Japanese and eventually entirely Japanese.

Hughes: Once you were there, you were left pretty free to conduct your studies as you wished?

Cogan: We were relatively free once we got over the initial hurdle of the number of patients we would examine. Then the finding of radiation cataracts in Hatsue Kimura gave a positive boost to our studies. Up to that time, 1950, no delayed effects of the bomb had been discovered. Lymphomas had not been reported nor any increased incidence of genetic abnormalities. It was a question of whether Congress would be willing to continue pouring money into ABCC if there were no positive results. Thus the finding of radiation cataracts was a strategic observation that had political significance.

Hughes: How much later did other delayed abnormalities appear? Years, wasn't it?

Cogan: Yes.

Hughes: To whom was ABCC responsible?

Cogan: Colonel Carl Tesmer was in charge. He was from the Armed Forces Institute of Pathology. But the ABCC was under the direct supervision of two epidemiologists who had had limited contact with biology. They were interested in statistics and being sure that the patients called in for examination were selected in properly random fashion. Our insistence on seeing a large number of persons was somewhat of an anathema to them.

Hughes: As you know, Dr. Cogan, I talked with Dr. Donaldson by telephone before I came here, and one of the things he told me was that he had been in Hiroshima. He wasn't quite sure when, but very shortly after you were there. Did you suggest that he go?*

Cogan: I believe so. It was so important to get good photographs of the cataracts.

* Telephone interview with Dr. David Donaldson, May 17, 1989.

Hughes: He said that one thing that he did, that I guess you hadn't had the time to do, was to dilate the pupils to get good photographs.

Cogan: We did dilate the pupils, but we didn't have Dr. Donaldson's photographic ability nor his equipment.

Hughes: Do you have quite a collection of medical photographs of that period?

Cogan: I did take pictures in my fashion, using the then new stroboscopic flash apparatus, but the pictures were poor compared with Dr. Donaldson's.

Hughes: That apparatus wasn't developed by Dr. Donaldson, was it?

Cogan: He had built his own equipment. You know in Boston the Infirmary is right across the river from MIT where [Harold Eugene] Edgerton developed stroboscopic photography. Dr. Donaldson collaborated with him.

Hughes: Where are the photographic slides that Dr. Donaldson took in Hiroshima?

Cogan: I assume they are in the Donaldson collection at the Massachusetts Eye and Ear Infirmary.

Hughes: When Dr. Donaldson left, did he leave his collection in Boston?

Cogan: I believe so.

Hughes: When you returned to Boston after this period in Japan, did you do any follow-up work?

Cogan: I was a consultant to ABCC, but I don't think I was ever consulted. However, a Dr. Sinsky, who was at one time attached to the Howe Laboratory, did return.

Hughes: Looking particularly for cataracts?

Cogan: Yes.

Hughes: Do you know if he found more?

Cogan: I know he reported finding a sheen in the lenses of persons who had not developed cataracts. I was skeptical about it but have no further knowledge of it.

Radiation Effects on the Eye

Hughes: You said earlier that you could tell somehow that the cataracts were due to radiation. Was that because radiation cataracts look different from other cataracts, or was it because the time period was appropriate for them to have been caused by the bomb?

Cogan: It would be more correct to say that the cataracts are compatible with radiation. That is, they develop initially beneath the posterior capsule and are most marked at the posterior pole of the lens. In a young person with no other disease of the eye, they are highly suggestive of a radiation etiology, but it is imperative to have a history of radiation exposure to be sure. Only lawyers seem to think you can look at a lens and be sure the cataract was caused by radiation.

Hughes: Have you been involved in court cases involving radiation?

Cogan: Right now there is a case being tried in Chicago, where I have made a deposition as a voluntary amicus curiae. The case involves an employee at a nuclear reactor who is developing cataracts and would like to claim they are caused by chronic radiation exposure. The fact is he has always had to wear exposure badges, which have indicated a total exposure of only a few rads, whereas the cataractogenic level is in the hundreds of rads. No, I have avoided being directly involved in court cases, but I think it is the duty of those of us who have had contact with radiation to see that injustices are not done.

Hughes: How do they know to call on you?

Cogan: Well, because I represent one of the few people who have been involved in estimating the dose threshold level for cataracts in human beings. George Merriam at Columbia University is another one. My study was done many years ago with Dr. Knud Dreisler when we recalled many patients who had been treated by radiation to the face for acne and other patients who had to be radiated through the eye for tumors of the orbit. In the aggregate, we had a whole series of patients representing known amounts of radiation from small to large. That is how we determined the threshold dose for chronic exposure was of the order of 600 to 1,000 rads.

Hughes: When were they using radiation treatment for acne?

Cogan: Dermatologists used it many years ago, possibly in the 1920s.

Hughes: You published approximately twenty-four papers on the effects of radiant energy of many different kinds, ranging from x-rays through radar. Is there some general conclusion that you reached in this work?

Cogan: I suppose my conclusion is that the eye is a wonderful tissue on which to study the effects of different types of radiation. There's the surface effect of ultraviolet radiation which allowed us at one time to measure the action spectrum of ultraviolet keratitis. Then there's the effect of ionizing radiation on the lens which has given us insight on cataracts. There's the effect of infrared radiation which focuses on the retina and causes burns of the macula, as in eclipse blindness. And, of course, there is the use of light rays which are so important in the process of seeing but in excess may be used to induce the laser burns for sealing the retina in place. The eye just has so many aspects for the study of radiation. I guess that would be my conclusion.

Research on ultraviolet keratitis, 1945
(Abraham Pollen Archives and Rare Book Library, MEEI)

Hughes: Your studies involved microscopic observations as well?

Cogan: Yes, and then there were the practical problems of determining the relative biologic effectiveness of gamma rays versus neutrons in producing cataracts. All of this involves more than perhaps we should get into.

Hughes: Was your work used to establish safe levels of radiation exposure?

Cogan: I believe it is the standard reference for safe levels of chronic x-ray exposure as far as cataracts are concerned.

Hughes: Was most of your postwar work supported by the AEC?

Cogan: Much of it was.

Hughes: I am looking at a paper of yours which is called "Lesions of the Eye from Radiant Energy." The purpose you say is to describe the clinical characteristics of eye lesions caused by various types of radiant energy. Then you describe the effect of different types of radiation on different parts of the eye—the eye as a whole, the cornea, the lens, the retina, etcetera.*

Cogan: That represented the spectrum of radiant energy and a sort of cartoon to indicate the eye lesions. It is a summary of what you and I have been talking about.

Hughes: To this day, there's still controversy over what level of radiation is dangerous. I guess it's well known what acute dose will cause trouble, but there's still a lot of debate about low-level radiation.

Cogan: Some of the controversy is justified and based on difficulties of measurement in human beings. Such is the case, for instance, with microwaves where measurements on animals are meaningless. Much of the controversy is based on ignorance, hysteria, litigious and political obscurantism. Newspaper accounts contribute much to the popular confusion.

Consultant, Los Alamos Medical Center

Hughes: From 1950 to 1955, you were consultant at Los Alamos Medical Center. I imagine this was related to your work on radiation, particularly the Hiroshima experience?

Cogan: I'm sure it was related. The fact is, I was asked to set up a general service eye clinic in the Los Alamos Hospital and to organize a research program in the laboratories. In back of this was the general effort by AEC to continue Los Alamos as a community that would attract physicists and their families to stay there. After the war, of course, there had been an exodus of physicists to return to their home bases. This left a void of research talent and empty laboratory space. AEC wanted to

* Cogan DG. Lesions of the eye from radiant energy. JAMA 1950; 142:145-51.

recover some of the heavy investment it had made in the community by making it an attractive place in which to live and work. The hospital was one of these gestures and an eye clinic was a small facet of this attempt.

I agreed to come out for the summer months if they allowed me to arrange for one of our graduate residents or a fellow from the Infirmary to cover the rest of the year and rotate annually.

Hughes: And you did do that?

Cogan: They accepted the proposition, and John Goff, one of my fellows in neuro-ophthalmology, went out with me. He manned the clinic while I set up a research program in the laboratories. The rotation didn't work out quite as I had planned. At the end of John Goff's year, the hospital authorities said they liked him and he liked Los Alamos, so he stayed on and I no longer had the problem of finding successors.

My research activities revolved about the experimental production of cataracts in mice by neutrons. The physicist with whom I collaborated was Elizabeth Graves. She and her husband had a lot to do with the development of the atom bomb at Los Alamos during the war. In fact, her husband, Alvin Graves, was one of the three involved in the first laboratory explosion of atomic energy.

Hughes: The explosion at Trinity?

Cogan: No. This was well before Trinity. As I recall the story, they were slowly pushing the two elements of a bomb together by means of a screwdriver when the mass became critical and resulted in a miniexplosion. One of the three died. Dr. Graves and the other participant were injured but survived, and Dr. Graves continued at Los Alamos.

Hughes: Oh, I hadn't heard about that.

Cogan: No. I think few people know about it, although it is in the realm of public information. So far as I know, it was the first atomic explosion in the development of the bomb.

Elizabeth Graves was a physicist in her own right. Like so many atomic physicists, she seemed to me cavalier about her radiation exposure, especially since we knew so little about neutrons at that time and that is what we were working with. Also, she was pregnant at the time. But perhaps I was overly cautious.

There still was no way of measuring neutron radiation except by knowing the emission from a source. Hence we devised an apparatus to hold as many as twenty or more mice for each exposure, and we could vary the dose by having them at different distances from the target. Elizabeth Graves operated what was called a Cockcroft-Walton apparatus, while I followed up with examination for cataracts.

Hughes: Amazing.

Cogan: Yes, it was amazing, if I do say so. Also, it was a great opportunity for me to work with a sophisticated physicist and to have access with little effort to so many others. Thus I could drop in at coffee breaks and pose all sorts of naive questions to a roundtable of extraordinary talent. It was so unlike Boston where I could make contact with a physicist at MIT only by appointment and feel guilty that I was taking up his time. At Los Alamos, I even thought the physicists were as interested in hearing about my biologic experiments as I was in learning the elements of atomic energy.

Hughes: When you exposed these mice, you were really just interested in the effects on the eye?

Cogan: Yes. We were interested in determining the cataractogenic threshold for neutrons and their biologic equivalence to gamma rays.

Hughes: So you didn't look for radiation effects elsewhere?

Cogan: No. As a matter of fact, the mice were so oriented to the target that the rest of the body, aside from the head, received relatively little radiation. This was important so the animals would survive for our follow-up examinations.

Hughes: You mentioned that there was quite an exodus after the war. Was Edward Teller there at that point?

Cogan: No, but Enrico Fermi was a visitor while I was there.

Hughes: Had J. Robert Oppenheimer left?

Cogan: Oh yes.

Hughes: All the big names in physics had left?

Cogan: Except Alvin Graves. He may have been director of the laboratories at the time.

Hughes: Who was the director of the medical unit at Los Alamos?

Cogan: I forget the name of the internist who was chief at the hospital, but the head of Radiologic Health was a Harvard classmate of mine by the name of Tom Shipman.

Hughes: Was he down there permanently?

Cogan: I think so.

Hughes: Both Stafford Warren and Shields Warren were associated with radiation studies?

Cogan: Very much so. Both were associated with the AEC. Actually, Los Alamos was operated officially as part of the University of California, and I believe Staff Warren was the administrative head. Shields Warren, on the other hand, was the reference for the pathology of radiation effects on tissue. I knew both of them. Staff Warren and I used to meet as members of the scientific advisory panel of Research to Prevent Blindness. Shields Warren and I met through our coresidence in Boston. It was Shields Warren's Department of Pathology at the Deaconess Hospital in Boston that gave Dr. Kuwabara and me so much help in obtaining eyes for our diabetic studies.

Hughes: Didn't the corneas you were using in research come from the Deaconess?

Cogan: No, the diabetic eyes did come from the Deaconess and the associated Joslin Clinic.

Hughes: Wasn't Shields Warren also instrumental in establishing safe levels of radiation exposure?

Cogan: I am sure he was.

Hughes: How long were you at Los Alamos?

Cogan: Three months. It was a wonderful occasion to spend the summer months there with my wife and my four little girls. Los Alamos was still a closed city with all the required clearances and passes that had been in force during the war. We had to get out of the laboratory at five o'clock and could not get in the building on

weekends. So the entire family would regularly go down into the valley for horseback riding and camping. It was idyllic living.

Hughes: What happened when you were away from the Infirmary for three months at a time. Who sat in for you?

Cogan: My clinical duties were minimal at that time. Dr. Grant took care of what needed to be done administratively, but this was not much. Each person in the Laboratory had his own sphere of operations and was self-sufficient. In a way, it was disillusioning that the Laboratory got along so well without me.

Hughes: What was the protocol for getting clearance?

Cogan: A standard FBI investigation including interrogations of neighbors for evidence of unusual life-styles, associations, etcetera.

Hughes: Were they looking for Communist connections in particular?

Cogan: Perhaps.

Hughes: From 1951 to 1962, you were consultant instructor in ophthalmology at the US Naval Hospital in Chelsea, Massachusetts. How did that come about?

Cogan: I used to attend the eye clinic one afternoon a week, but there was nothing about that worth commenting on.

Consultant, Children's Hospital, Boston, 1954–1973

Hughes: From 1954, I guess right up to when you left Boston, you were consultant to the Children's Hospital. Was that a more meaningful relationship?

Cogan: My role was securing a full-time person to head up a department of ophthalmology there. One of our residents, Dr. Richard Robb, was an ideal person for this. He was interested in pediatric ophthalmology and showed administrative talent. I played a similar role for the Peter Bent Brigham Hospital in the appointment of Dr. Leo T. Chylack as its full-time head of the new eye department. Dr. Chylack had been a fellow in the Howe Laboratory and a resident of the Infirmary.

Hughes: Did you recognize the subspecialty of pediatric ophthalmology at the Infirmary?

Cogan: There were those who were interested in the field, but it was not the established subspecialty that it later became.

Hughes: At Children's and the Brigham, before the full-time operation was set up, would problem cases—I'm thinking of congenital glaucoma—have been referred to the Infirmary?

Cogan: Dr. Gundersen handled most of the cases. The two miles separating the institutions made consultations difficult.

Hughes: Do you have any feelings about subspecialization within ophthalmology?

Cogan: With the tremendous advances in knowledge and technology it is necessary, of course. It is an inevitable consequence of progress in all branches of medicine. I would have to say I am glad it was not so necessary in my early days.

Hughes: Do you think that it's impossible to handle the whole of ophthalmology now?

Cogan: In the academic world, it is virtually impossible—in private practice, perhaps not.

Hughes: Did the increased subspecialization have an impact on the content of the residency training program?

Cogan: Yes, but more particularly it has resulted in great expansion of the postresidency fellowship programs.

Hughes: Are the programs usually set up in such a fashion that you spend a unit on pediatric ophthalmology and a unit on retina, or is it more free flowing than that?

Cogan: I suppose it varies in different institutions. In the larger programs, residents rotate through the different subspecialties.

Hughes: Is there anything to be said about connections with any other hospitals in the Boston area?

Cogan: It had been my hope that with the establishment of separate departments in other hospitals, the Infirmary and Howe Laboratory would maintain their connections by rotation of our residents through these hospitals. This occurred to a variable extent. But it's paradoxic that the more successful the programs were in the other hospitals, the less they needed the Infirmary connection.

Hypercalcemia and Band Keratopathy

Hughes: It seems to me that you have done a fair amount of work on what I call metabolic disease. I know I missed papers, so I'm hoping you'll help me out. One that I looked at was published in 1948 and entitled "Hypercalcemia and Band Keratopathy." Do you remember how you came to be interested in that subject?*

Cogan: With my interest in corneal disease, I was intrigued by a group of patients who were developing unusual deposits of calcium in their corneas after taking excessive amounts of vitamin D for the treatment of rheumatoid arthritis. Unlike the usual band keratopathy which involves the central corneas preferentially, these deposits were in the periphery of the corneas. The patients also gave, for the most part, a history of having kidney stones and had increased calcium in the blood. This latter was a special interest of Fuller Albright, an internist at the Massachusetts General Hospital. Together we were able to study and report a number of patients with hypercalcemia from vitamin D and other causes. This had obvious medical implications, but it also seemed to fit our previous concept of fluid movement in the cornea. These were the bases for our interest in the subject.

Hughes: Are they calcium crystals?

Cogan: They are deposits of calcium phosphate or hydroxyapatite.

Hughes: Why are they localized in the cornea?

Cogan: You may remember our earlier observations on water removal through the epithelium of the cornea to the outside. This occurs maximally in the periphery of the cornea. The result is a concentration of electrolytes immediately beneath the epithelium. Calcium phosphate, which is normally in a metastable state, comes out of solution when thus concentrated. Moreover, the local alkalinization resulting from loss of carbon dioxide through the epithelium further favors precipitation of calcium phosphate.

Hughes: Can you see this band with the naked eye?

Cogan: Usually.

* Cogan DG, Albright F, Bartter FC. Arch Ophthalmol 1948; 406:24-38.

Hughes: What was your major point in this paper?

Cogan: To add to clinical medicine a potentially useful diagnostic sign. It also raises the possibility that we can see in the cornea what is probably taking place in the kidneys in the formation of kidney stones.

Hughes: How reversible is it when you lower the blood calcium?

Cogan: It gradually fades away.

Hughes: You recorded nineteen cases. Please comment on your coauthors.

Cogan: Dr. Albright was a world authority on calcium disturbances and a very colorful person. Dr. Bartter was one of his fellows.

Hughes: Why didn't you publish in a nonophthalmological journal?

Cogan: It was important, I thought, that ophthalmologists recognize this form of band keratopathy as a clinical sign of hypercalcemia. Moreover, I was sure Dr. Albright would refer to it in his medical publications.

Hughes: It was unusual to have vision impaired by these deposits, wasn't it?

Cogan: The band rarely involved the central portions of the cornea.

Hughes: You said that Fuller Albright was a very colorful personality. Would you elaborate, please?

Cogan: Aside from his scientific eminence, he had a way of putting things together in meaningful and often humorous terms. It was a pleasure to read what he wrote. But then he developed Parkinson's disease that became so severe he could scarcely walk or talk. Yet he continued to make rounds at the hospital and kept up interest in medical affairs. Finally, he insisted on having neurosurgery, which at that stage was still experimental. Following this surgery, he was completely incapacitated and remained so until his death one or two years later. It was a tragic affair.

Hughes: Did people in other specialties indeed pick up this band?

Cogan: Yes, it's a standard sign of hypercalcemia. Of course, it's no longer suggestive of vitamin D toxicity, which was responsible for

our original interest, since that mode of therapy is no longer in use. But it is a useful sign in hyperparathyroidism and other conditions associated with hypercalcemia.

At about the same time that we were observing it in the cornea, Dr. Frank Walsh in Baltimore was observing deposits in the conjunctiva. I believe we decided to publish our observations simultaneously in the same journal.

Cystinosis and the Eye

Hughes: Most of your papers were written by you as sole author or with only one or two others. But this paper has five names on it and it's called "Ocular Manifestations of Systemic Cystinosis." How did you become interested in this subject?*

Cogan: Again it was because we happened to have contact with persons (youngsters) who had this tragic disease. Cystinosis is a genetic abnormality wherein cystine crystals gradually accumulate in the body and are conspicuous in the corneal stroma. The disease at the time of our study was uniformly fatal in the first decade of life. The diagnosis was usually made by finding crystals in the bone marrow, but we pointed out that the appearance of the cornea was sufficient to make the diagnosis. We were able to demonstrate the crystals in the cornea of a fatal case. This was the first microscopic demonstration of the crystals in the cornea.

Hughes: Could you give me an idea what the technique was for demonstrating them?

Cogan: First, the tissue had to be fixed in alcohol since the crystals dissolve out in aqueous solvents. The thick sections were cut, mounted, and examined between crossed polaroids or by dark-field illumination. It is interesting that in the cornea the crystals have a characteristic needlelike configuration, whereas in the rest of the body they are either rectangular or hexagonal in shape.

Hughes: Was there a treatment for cystinosis?

Cogan: Not at that time.

* Cogan DG, Kuwabara T, Kinoshita J, Sudarsky D, Ring H. Ocular manifestations of systemic cystinosis. Arch Ophthalmol 1956; 55:36-41.

Hughes: In 1957, you published a paper in the Journal of the American Medical Association *reporting a case of cystinosis in an adult.* I gathered that it was very unusual to spot cystinosis in an adult.*

Cogan: It was unique. This was the first case.

Hughes: And you did it simply by spotting the crystals?

Cogan: Well, yes. The patient was referred to me by Dr. Linus Sheehan of Providence, Rhode Island. He had observed unusual deposits in the cornea of a person who reported only for a refractive problem. The crystals in the cornea and in the conjunctiva looked just like those I had seen with cystinosis, yet this patient obviously did not have the disease as we understood it. He was a tall young man, a PhD candidate, with no systemic abnormality. The patient allowed us to do a biopsy of the conjunctiva, and there we found crystals identical to those of the children with cystinosis. Solubility tests were also compatible with this.

The definitive proof, however, was obtained by Cornelius Hurlbut, professor of mineralogy at Harvard, by x-ray spectroscopy on a sample extracted from the conjunctival tissue.** This was indeed a benign form of cystinosis occurring in an adult. Since then, a dozen or more cases have been reported including an additional patient whom we included in the initial report.

The Eye and the Diagnosis of Nonophthalmologic Disease

Hughes: Is it true that one of your goals throughout your career has been to indicate to nonophthalmologists that the eye can be extremely useful in diagnosing general systemic disease?

Cogan: I think that's an important point. Ophthalmology naturally emphasizes surgery of cataract, glaucoma, detached retina, etcetera. But there is a tremendous field in which the eye reflects general disease unrelated to the surgical conditions. The eye provides an opportunity to study the general vasculature reflected by the blood vessels in the back of the eye, so many inflammatory processes reflected by what one sees in the front of

* Cogan DG, Kuwabara T, Kinoshita J, et al. Cystinosis in an adult. JAMA 1957; 164:394-6.

** Cogan DG, Kuwabara T, Hurlbut CS, et al. Further observations on cystinosis in the adult. JAMA 1958; 166:1725-6.

the eye, and a host of metabolic diseases. Medical ophthalmology has not had the high rating of its surgical counterpart.

Should ophthalmology be divided into medical and surgical ophthalmology similar to the division of neurology into the separate divisions of neurology and neurosurgery? In a way, neuro-ophthalmology is the forerunner of medical ophthalmology. Neuro-ophthalmologists are interested in the medical, rather than the surgical, aspects of the specialty. Temperamentally, they have a different approach to ophthalmology than the surgeons.

The idea of dividing ophthalmology into separate medical and surgical disciplines is not new. Dr. Fred Cordes of San Francisco suggested it years ago, but it precipitated so much opposition that it was dropped. I would have to admit it raises ponderous problems such as separate boards, societies, etcetera, if it were officially separated.

Hughes: Have you ever proposed the notion?

Cogan: Not officially. But some years ago Dr. [A. Edward] Maumenee and I did try to arrange our residency programs to accommodate those who were interested in medical ophthalmology as opposed to those who were headed for a practice. In Boston, we tried a paired residency where two residents would go through the program together. It did not work out very well and was dropped. I guess I concluded that if the need was there it would develop spontaneously. I think it has, and that is the role neuro-ophthalmology is playing. Certainly this subspecialty has become very popular in recent years.

Hughes: Is the differentiation somewhat in terms of the almighty dollar?

Cogan: That's a strong factor. Certainly a medical ophthalmologist's income would be considerably less than that of the surgical ophthalmologist. That's true in comparison of income of the neurologist and the neurosurgeon. There are just other amenities that some of us believe counterbalance the limitation of income.

Hughes: Have you been successful in getting across the message to other specialties that the eye is a valuable tool in general disease?

Cogan: Not as much as we would like. The fact that ophthalmology has been so downgraded in the medical school curricula is indicative of a low esteem held by those planning medical student teaching. Except for a very limited contact in the eye clinic and few, if any,

lectures, students have little contact with ophthalmology. They may get the impression that ophthalmology is little more than prescribing glasses.

Hughes: Is that true everywhere?

Cogan: It is widely prevalent.

Hughes: What is the argument?

Cogan: General surgery, internal medicine, pediatrics, obstetrics/gynecology, and psychiatry are the big heavies. The curriculum doesn't have time for ophthalmology. Fortunately, some medical students have developed contacts on their own, and I must say, ophthalmology has had in the past two decades superior candidates for the ophthalmic programs. But it has not had much assistance from the undergraduate medical school curricula.

Hughes: Have ophthalmologists tried to change that situation?

Cogan: Not with much vigor.

Hughes: I noticed that some of your papers, a minority I must admit, were published in nonophthalmological journals. The Archives of Pathology, Pediatrics, *the* New England Journal of Medicine, *and* Circulation *are some of the ones I noticed. Was the major point to get information from ophthalmology into other specialties?*

Cogan: One publishes where one thinks the greatest interest lies. I happen to be interested in general medicine and pathology as they relate to ophthalmology.

Hughes: Not because you wanted to communicate with other medical specialists?

Cogan: Not consciously.

Hughes: Is the same true of the basic science journals that you published in?

Cogan: Most of these were collaborative publications with basic scientists who chose the journal outlet.

Hughes: The Journal of Histochemistry *and the* Journal of Cytochemistry *seem to be the nonophthalmological journals that you most frequently published in.*

Cogan: Those were mostly with Dr. Kuwabara.

Hughes: How did you reach a decision about where you were going to publish?

Cogan: The senior author is the one usually responsible; after all, it is chiefly his project. There may be a personal bias. I know, in my case, I had a preference for the *Archives of Ophthalmology* having been an associate editor for twenty-five years and chief editor for eight years. As recently pointed out by Dr. Daniel Albert,* that was where a disproportionate number of Dr. Kuwabara's and my publications appeared.

Editorial Board, *Archives of Ophthalmology*, 1941–1966

Hughes: Is there an image projected by each of the major ophthalmological journals, the Archives, *the* American Journal of Ophthalmology, *and* Ophthalmology? *Do you expect to find a certain type of paper in one or the other?*

Cogan: The three journals you cite represent general interest articles for the ophthalmologist. There is considerable overlap. Historically, the *Archives of Ophthalmology* and the *American Journal of Ophthalmology* are the oldest and most prestigious. But in recent years, *Ophthalmology* has assumed an equal stature as the official publication of the American Academy of Ophthalmology. All three receive many more articles than they can publish. In the case of the *Archives*, our rejection rate use to be of the order of 50%.

Hughes: That much?

Cogan: At least that, probably more.

Hughes: Did you turn the papers down because of scientific deficiencies, or because they weren't written well?

Cogan: Both factors. If they were simply poorly written, the author was advised of this and invited to resubmit.

* Albert DM. Analysis of the *Archives* top-cited articles. JAMA 1988; 106:465-70.

Hughes: *1869 was when Hermann Jacob Knapp founded the* Archives of Ophthalmology and Otology. *Do you remember that he was simultaneously publishing a journal in Germany?**

Cogan: Actually Dr. Knapp had been editing that German journal at the time he immigrated to this country and before he founded a similar journal in this country. He was a well-established ophthalmologist in Heidelberg. Why he migrated is somewhat of a mystery.

Hughes: *Then, in 1929, the modern version of the* Archives *was founded.*** *Do you know the story of how it ceased being a Knapp enterprise?*

Cogan: As you know, Hermann Knapp was succeeded by his son, Arnold Knapp.

Hughes: *It was an impressive editorial board in 1929—George Derby, Sanford Gifford, J. Herbert Waite, William Zentmayer, and Francis Adler. Francis Adler was the chief editor who preceded you. Did any of those other men carry on until 1941 when you joined the editorial board?*

Cogan: I think not. I admired Francis Adler greatly. He was professor of ophthalmology at the University of Pennsylvania, sympathetic to basic sciences, author of a standard text on the physiology of the eye, and an aesthetic person with interest in music and the arts as well as in medicine. He was an ideal and devoted editor and planned on his retirement to devote most of his time to *Archives'* affairs. Unfortunately, the policy of the AMA for its specialty journals changed just at that time. Chief editors were to have a tenure of only ten years. Adler had served more than this. He was out and naturally upset to have to retire from the editorship. Do you remember what year this was?

Hughes: *Yes, 1960.*

Cogan: I had been on the editorial board for many years and was invited to become chief editor. At first I declined out of sympathy for the peremptory way the AMA had treated Adler. But I made a trip to Philadelphia and discussed it with Adler. He persuaded me to take it and continue some of the plans we had already discussed.

* Archiv für Augen und Ohrenheilkunde.

** The foregoing information on Knapp and the *Archives* is based on Snyder C. *Archives of Ophthalmology:* A 100-year survey. Arch Ophthalmol 1969; 81:605-11.

One of these plans, to which I was partial, was to have separate sections in the *Archives* for clinical articles and basic science articles. Some of the clinically oriented readers had criticized the *Archives* for having so many scientific articles, whereas the scientists were feeling more at home in such journals as *Investigative Ophthalmology* or whatever its name was at that time. I thought there should be a place where the two had at least some awareness of what was going on on the other side of the fence. My contribution, therefore, was to have two sections where each would know, by titles at least, what the others were doing. This was my mission. Other accomplishments, if I may call them that, were an introduction of a section for presentation of interesting cases, a photograph of the month by Dr. Donaldson, and historical vignettes by Charles Snyder. These latter were subsequently published in book format by Snyder.

Hughes: Our Ophthalmic Heritage? *Is that it?*

Cogan: Yes.

Hughes: *Was the historical section popular with ophthalmologists?*

Cogan: We received many favorable comments.

Hughes: *What was the reaction to dividing the journal into clinical and basic science contributions?*

Cogan: This wasn't exactly a revolutionary change, but we no longer had the criticism that prompted it. So I guess it was successful. At least I felt happy about it. But then, after a few years, I began to feel my mission was accomplished. The next few years, I was more or less defending the system, and at the end of seven years, I asked to be relieved of the chief editorship. It was taking more time than I felt it should from Howe Laboratory obligations. I resigned three years before my tenure was up.

Hughes: *Let's backtrack, if you don't mind. You became a member of the editorial board in 1941. Weren't you young for the position?*

Cogan: Perhaps. It coincided with the time I became acting director of the Howe Laboratory. The question is, why are persons put on an editorial board?

Hughes: *Yes, why are they?*

Cogan: I suspect in my case it was "influence peddling." Because of my position in the Howe Laboratory, I might steer worthwhile papers

to the *Archives*. At that time, there was considerable rivalry between the *Archives* and the *American Journal of Ophthalmology*.

By the way, in speaking of my contributions to the *Archives,* I forgot to mention one aspect which is perhaps the most significant. It was while Francis Adler was still chief editor. I suggested that we abolish the abstract section of the *Archives*, since this function was being carried out in other journals. I proposed that we subdivide ophthalmology into twelve categories and that each month contain an updated commentary on one category. Adler was pleased with the idea and helped me select review editors, each to serve for three years to prepare an up-to-date review of his field of expertise.

Hughes: Was that popular?

Cogan: It served a useful purpose. The reviewers found it an onerous task but allowed that it had been good for them once they had completed it. I think it was also useful for readers who wanted to have a critique by authorities in the various fields of interest.

Hughes: Who was on the board when you first joined it?

Cogan: I've forgotten.

Hughes: Was Adler chief editor?

Cogan: Yes.

Hughes: He was always chief editor during your tenure?

Cogan: Yes, until I succeeded him.

Hughes: How did Adler run the editorial board?

Cogan: We were referees on papers with minimal participation in policy matters. I don't recall ever having joint meetings of the board.

Hughes: What was the procedure? Surely not everybody read everything?

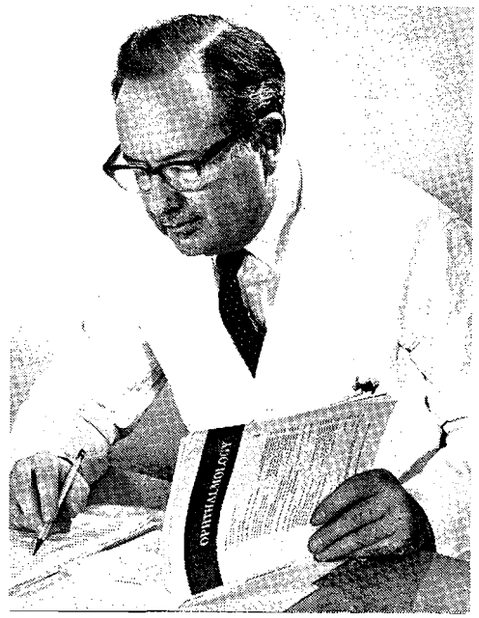
Cogan: Dr. Adler would distribute the papers to one or more of the board members who he thought would be most knowledgeable in the field of the paper. There were ten or a dozen of us on the board. If we, in turn, did not feel competent to evaluate the paper, we would refer it to someone who did.

Hughes: So a month could go by when you had no reading?

Cogan: Often much longer than a month.

Hughes: Was the task very time consuming?

Cogan: Only when I became chief editor.



Chief Editor,
Archives of Ophthalmology

Hughes: Why were you chosen to be chief editor in 1960?

Cogan: Perhaps because, as Adler claimed, I had been the most active member of his editorial board, or more likely, because I had been a Boston acquaintance of Dr. John Talbott, who as chief editor of *JAMA*, had much to do with determining policy of the specialty journals.

Hughes: Well, in 1952, the journal became the AMA Archives of Ophthalmology.

Cogan: Along with the other specialty journals of the AMA its name was changed. This was a source of confusion and lasted only a few years to the distress of libraries and other indexing systems. Dr.

[Morris] Fishbein, then the editor of *JAMA*, thought it would be a boost for the subspecialty journals.

Hughes: What was the role of the AMA?

Cogan: They handled the advertising and financing of the journals and prescribed the format to maintain some uniformity of the specialty journals.

Hughes: The AMA had nothing to do with editorial policy?

Cogan: Yes, they did play a role in policy as well. I found them frustratingly restrictive at times. For instance, I had a run-in with their legal department. I thought it was important to allow hospital unit numbers to be added to any case report so that others in later years could follow up the claims. I further thought this would guarantee that the authors would be entirely honest in their reports. The lawyers for the AMA were adamant in disallowing this. They would not even allow a patient's initials to be added to the case reports lest the AMA somehow become liable for a suit.

On the other hand, a policy which did seem to me appropriate was not to allow more than 40% of a journal to be taken up with advertising.

Hughes: Was that reciprocated in other specialty journals?

Cogan: I believe it was.

Hughes: There was some discussion of the actual placement of the ads in the journal?

Cogan: That came up recently while Dr. Fred Blodi was the chief editor. He objected, and we all did, to the distribution of ads throughout the journal. They had been concentrated theretofore in the front and back of the journal.

By the way, another contribution to the *Archives*, which I made as chief editor, was to have one or two editorials at the beginning of the journal. I wrote most of these, and they were time consuming. I don't know why I forgot to mention them previously.

Hughes: How did you choose your topic?

Cogan: Whatever seemed to be of current interest. I tried to introduce a leitmotiv in these editorials to counterbalance the heady articles which were to follow. Some dealt with clinical problems, some

with scientific matters. Others might have dealt with political issues dealing with support.

Hughes: Support for what?

Cogan: For ophthalmology or research. Such, for instance, was one in which I took issue with Senator Lawrence H. Fountain for his attacks on NIH.

Hughes: The editorial in Ophthalmology *nowadays is usually connected with the lead article and is almost always on a medical topic. But that wasn't true in your case?*

Cogan: They were variable.

Hughes: Do you remember who was on the editorial board when you became chief editor.

Cogan: I probably can't remember all so I won't cite any. But I would add I had a very efficient assistant by the name of Beezy Young. She had connections with the eye department at Hopkins before she came to Boston and so was familiar with the field.

Hughes: You told me off tape that when you were appointed chief editor, you asked to be relieved of your chiefship of the Thursday clinic days at the Infirmary. Were there other duties that you had to let go of once you became chief editor?

Cogan: No. Relief from the clinic duties was sufficient.

Hughes: Did you find being chief editor time consuming?

Cogan: I think I may have told you it was not too time consuming. But, as I think of it, it did occupy much of my otherwise free time, such as in the middle of the night when you're thinking of creative ideas. I think I became less productive in research because of the editorial preoccupations.

Hughes: Does the Archives *have an international reputation?*

Cogan: Of course, I would think so. Perhaps Derrick Vail, my friendly rival at the *American Journal of Ophthalmology* might have told you otherwise. I enjoyed sending him an article of mine occasionally with the comment that I would be happy to have him publish it since it had been turned down for the *Archives*. Then there were some articles which had to be published in the

American Journal [*of Ophthalmology*], such as the Jackson Memorial Lecture which was sponsored by the journal.

When Francis Adler was chief editor of the *Archives*, he had an agreement with Vail that neither would pressure authors of papers given at the American Ophthalmological Society to publish in their journal. I continued this policy.

Hughes: Would you say that Ophthalmology, *which is the successor to the* Transactions *of the Academy, has taken the lead from the* American Journal *and the* Archives?

Cogan: It is a fine journal, formerly edited by Paul Henkind and now Paul Lichter. It has a guaranteed subscription list since it is the Academy's official journal. It also has an appeal for those wishing colored plates, which only *Ophthalmology* provides cost free. It certainly is one of the big three, but I would have trouble rating them.

Hughes: Ophthalmology *doesn't print color plates very often, does it?*

Cogan: Every issue. Paul Henkind was interested in pathology and pushed the color reproductions for histopathologic papers.

Hughes: You mentioned that you appointed Henry Allen to the editorial board and that he followed you as chief editor.

Cogan: Correct.

Hughes: Did you have any role in his appointment as chief editor?

Cogan: I'm sure I did. He seemed like an appropriate candidate, with an impressive scholastic background, sufficient means to allow him to spend time in editorial work, and well motivated.

Hughes: John Talbott remarked in an article written when you became chief editor, "The appointment of Chief Editor and an assistant Chief Editor on a part-time basis is indicative of the emphasis the Board of Trustees has placed on the significance of this specialty journal. No longer will the direction be a volunteer assignment. It is a major task and has been so recognized." What was he meaning?*

* Talbott JH. Francis Heed Adler - David Glendenning Cogan. Arch Ophthalmol 1960; 63:907-8.

Cogan: I believe he was referring to the fact that I requested and received a small amount of money, I believe it was $5,000, to hire an editorial assistant in my office.

Hughes: Does that system continue today?

Cogan: I really don't know.

Hughes: From 1960 to 1967, the Archives *is reputed to have become the leading ophthalmological journal in the world.* How do you account for that?*

Cogan: I will neither affirm that nor deny it.

Hughes: The Transactions *of the Academy wasn't a contender at that stage?*

Cogan: That was quite a different journal at that time. It recorded what had been presented at the annual meeting. It was not a current journal. It was replaced, I think, by *Ophthalmology*, which is very much a current journal.

Hughes: I understand that in some journals there's a per-page cost.

Cogan: I don't know if that is true with any ophthalmic journals, but it is true in some chemistry journals. It is a bad policy.

Hughes: When you were reviewing manuscripts, what were you looking for?

Cogan: Originality, quality, and interest.

Hughes: So interest was part of it, too.

Cogan: A major part.

Hughes: It wasn't enough to be a good paper. It had to be on a topic of current interest?

Cogan: Yes. After all, the journal serves its readership.

Hughes: How much editing did you yourself do?

Cogan: I saw every paper that came in. I glanced through every paper and then distributed them to someone whose authority I respected in that field.

* [Anonymous editorial] The *Archives* [*of Otolaryngology*] salutes Dr. David Cogan. Arch Otolaryngol 1969; 89:689-90.

Hughes: Were you involved with rewriting?

Cogan: No. That would have been dangerous. I would send it back to the author with suggestions that it be rewritten.

Hughes: Were the suggestions along scientific-technical grounds?

Cogan: They could have been.

Hughes: Were you wary of manuscripts that touted new drugs?

Cogan: You mean a therapeutic drug?

Hughes: Yes.

Cogan: Dr. Grant was the authority on drugs. Being right across the hall from me, it was easy to get expert advice. I'd abide by his opinion over mine.

Hughes: Would you give particular consideration to somebody who had not done much publishing?

Cogan: I don't know that we would. The reader's point of view would be preeminent over everything else. We didn't feel it was our mission to promote any person on the basis of age or naiveté.

Hughes: But is it true that a young man or a woman can have a very hard time getting published for the first time?

Cogan: He has to learn how to present material on his own. That is an important aspect of getting his work recognized.

Pathology

Hughes: When and why did you become interested in pathology?

Cogan: I believe I am a visually minded animal insofar as I can comprehend things I see and feel rather than infer in the abstract. I admire persons like Jonas Friedenwald who can manipulate ideas, but they leave me far in their wake. I liked pathology in medical school because I could see what was happening under the microscope. Then, of course, my interest in ophthalmic pathology took a giant step forward through my contact with Dr. Verhoeff. In the clinic, disease to me was meaningful on the background of anatomy and pathology. [As I

mentioned] during my residency I organized Sunday morning sessions in pathology with my fellow residents. When Dr. Theodore Terry, the ophthalmic pathologist at the Infirmary, suddenly died, I took over the operation of the pathology laboratory for a while in addition to my Howe Laboratory functions. Eventually, I had to give this up because it was too time consuming, but it was something I enjoyed.

To answer your question, therefore, I think I would have to say pathology suited my temperament.

Hughes: I think immediately of your other interest, which is neuro-ophthalmology. Some parts of that are quite abstract in that you can't see into the brain, at least in most instances you can't.

Cogan: Still, neuro-ophthalmology is based on neuroanatomy. I think there is a lot of relevant objectivity in neuro-ophthalmology.

Hughes: So you could visualize?

Cogan: Right. Interpretation of eye movements, for instance, is also objective in that you know the mechanics of the muscle actions and their control within specific parts of the brain. It stands out in such contrast to fields like psychiatry, which don't appeal to me.

Hughes: Would you say, by and large, that ophthalmologists are visually minded?

Cogan: Perhaps. That's an interesting thought.

Shortly after the war, some twenty-five of us met at the Army Institute of Pathology in Washington for what was to become an annual meeting of the Ophthalmic Pathology Club. Still later its name was changed to the Verhoeff Society. It's been the one meeting of the year that I have prized. We exchange slides and return home with a cache of superb specimens that we share with our local associates. The meeting has been a model for several sectional meetings and for the European Ophthalmic Pathology Society. It made us proud that we could have a joint meeting with the Europeans celebrating our twenty-fifth meeting, while they were celebrating only their tenth.

Hughes: How did you choose who was to attend?

Cogan: We had to limit the membership to thirty because we were committed to use of individual microscopes. As vacancies became available, new members were selected after they had been invited

guests and presented a case. Still that was not sufficient room to accommodate the influx of trained ophthalmic pathologists. So sectional societies were formed, including the Eastern Ophthalmic Pathology Society, the midwestern society later named the Theobald Society, the western society named the Hogan Society, and a southern society that later merged with the Pan-American Society.

Hughes: Was the Verhoeff Society patterned after anything?

Cogan: No. It arose spontaneously and purposely avoided incorporation or other formalities.

Hughes: Whose idea was it to form the Ophthalmic Pathology Club?

Cogan: Benjamin Rones, John McLean, and Theodore Sanders.* Mrs. Helenor Wilder was in charge of the ophthalmic pathology at the Army Institute, but I don't think she was part of the organizing group.

Hughes: Did the Army Museum receive some of this material as well?

Cogan: Yes. Helly Wilder was a member of the group and representative of the Museum. She received a set of slides and deposited them in the Museum.

*Hughes: I read also that the Verhoeff Society meets periodically with the European Ophthalmic Pathology Society.***

Cogan: Every five years we have a joint meeting, alternating between this country and Europe. They are festive occasions.

Hughes: Is there any difference in approach?

Cogan: I believe the Europeans are somewhat hampered by the language difficulties. Possibly they may be more ritualistic on this account.

Hughes: But the way they go about pathology is essentially the way Americans go about pathology?

Cogan: It's the same the world over.

Hughes: Did Dr. Verhoeff always attend?

* Tso MOM, et al. Ex errato lux. Concept and development of the Verhoeff Society. Surv Ophthalmol 1986; 30:337-40.

** Ibid.

Cogan: Yes, he did. He and I used to come down to Washington together.

Hughes: Are there any stories connected with him in what was then the Ophthalmic Pathology Club?

Cogan: I'm reminded of one which is so unadulterated Verhoeffiana. "Freddie," as we used to call him, and I roomed together and would often discuss aspects of the meeting late at night. Now you may know that Verhoeff was a no-compromising atheist. He carried on one evening about the religious dogmatism of one of the members. I suggested that he, Verhoeff, had been brought up in an atheist environment and that the person whom he was castigating had been brought up in a devout home. I suggested that they were both simply mouthing Pavlovian patterns with which they happened to have been ingrained in early life. He replied that might be true for so-and-so, but it didn't apply to him. With that he turned over and went to sleep. That ended that. [laughter]

Hughes: Did the Howe Lab have any particular association with the pathology registry at the Army Medical Museum?

Cogan: That, of course, was before my day and before the days of the Howe Laboratory. Apparently, the registry was established by the Academy, following the persuasion of Dr. Harry Gradle of Chicago. Dr. Verhoeff was a consultant for reviewing slide material but not involved in the organizational aspects.

Hughes: Does the registry still exist?

Cogan: It certainly does.

Hughes: Who named the pathology laboratory at the Infirmary for you, and when did it occur?

Cogan: I think I know who was responsible for naming the pathology laboratory for me, but I am sure it was not the decision of only one or two persons. I prefer not to give any names. I know funds were contributed by more friends than I really knew I had. I am grateful beyond words and proud that it is in such capable hands as Dan Albert's and Ted Dryja's. The naming occurred sometime after I had left Boston.

*Hughes: I read that it processes more than 2,000 specimens a year, ranking it with the Armed Forces Institute of Pathology and the pathology laboratory at Hopkins.**

Cogan: That surprises me. To be sure, it is a much bigger operation than when either I or Taylor Smith was in charge.

Hughes: In what sense?

Cogan: The laboratory occupies about 2,000 square feet. The residents rotate through pathology. There is an active fellowship program and facilities for experimental pathology.

Hughes: What services does the pathology laboratory perform?

Cogan: Of course, there are the indispensable diagnostic services, such as examining tissue from the operating room. What is the nature of a tumor? Where does it come from? Is it malignant? These are critical questions that require expert opinion. Enucleated eyes tell us a lot about what has gone wrong and, in the question of sympathetic ophthalmia, for instance, what to expect in the patient's other eye. But after all this, there is the important research aspect. Dan Albert, for instance, has just reported at the American Ophthalmological Society, the successful production of retinoblastomas in mice, and Ted Dryja recently made the epochal discovery of the genetic basis of retinoblastoma and certain other tumors.

Hughes: Sounds as though things are moving in the direction of genetics.

Cogan: Very much so.

*Hughes: You used the term experimental pathology, which made me think of a comment that Dr. Chader made to me.** He felt that Dr. Kuwabara was responsible for introducing experimental pathology into ophthalmology.*

Cogan: At least, he was one of the pioneers in this country.

Hughes: Are you wondering whether he was the first?

Cogan: Experimental pathology goes back to the last century.

* Unidentified MEEI publication.

** Interview with Dr. Gerald Chader, National Eye Institute, Bethesda, Maryland, May 24, 1989.

Hughes: Was there anybody else in the early fifties in this country?

Cogan: The name of Friedenwald comes immediately to mind.

Hughes: Do you have anything to say on the place of diagnosis versus treatment?

Cogan: One refers jokingly to the doctor who is not very good at diagnosis but is great in therapy. That would not be very good medicine. There is no doubt that pathology provides the infrastructure for diagnosis, and sound therapy depends on diagnosis. Having said this, I would have to admit, however, that sometimes treatment has to be empiric. Thus steroids are used at times even when the diagnosis is uncertain.

Hughes: Has it made a difference in your practice of medicine?

Cogan: I can't conceive of practice without a knowledge of pathology.

Steroid Use in Ophthalmology

Hughes: Well, you mentioned steroids, and Dr. Thygeson is outspoken on the subject of their overuse in ophthalmology. Do you have any feelings on the subject?

Cogan: Nothing that is new or worthwhile.

Hughes: Were you willing to prescribe steroids?

Cogan: I believe I understand Dr. Thygeson's objection. Steroids can exacerbate infections, especially viral infections. Moreover, their use may mask an underlying disease process as, for instance, in the case of some tumors. Thygeson is correct in cautioning against the overuse of steroids.

*Hughes: I don't think he would rule them out entirely. In fact, I know him to have said that they were extremely effective in certain diseases. One I remember is phlyctenulosis, where he said steroids practically overnight had the problem in Alaska under control.**

* See oral history in this series with Phillips Thygeson.

Director, Ophthalmic Laboratories, Massachusetts Eye and Ear Infirmary, 1947–1956

Hughes: According to your curriculum vitae, from 1947 to 1956, you were director of the ophthalmic laboratories at the Infirmary. And then, as you say, you were also director of the eye pathology laboratory, and that was from 1954 to 1956. So there was a little overlap there. What are the ophthalmic laboratories?

Cogan: The ophthalmic laboratories included a number of entrepreneurial efforts by some members of the Infirmary to raise money for their own interests. These were competitive with what the Infirmary and the Howe Laboratory were already doing and led to confusion among the potential benefactors. The appointment was an effort to integrate all research efforts under one umbrella.

Hughes: So the director of ophthalmic laboratories was supposed to be in charge of all the laboratory work at the Infirmary?

Cogan: Yes. It aimed to prevent fractionation of the money-raising efforts, but I must add, it did not accomplish this goal.

Teaching

Hughes: Teaching is the next subject for discussion.

Cogan: We have already discussed the background of the Harvard Basic Science Course in Ophthalmology—how I found that Dr. Waite had committed me to running it within a couple of weeks after my return from the Moseley year abroad and how it became an annual event. It was a chore each year but always satisfying. I enjoyed it and learned a lot. The teacher learned more than the students.

Hughes: Dr. Alper and Dr. Donaldson told me the Harvard course is where they first encountered you. Was the Harvard course ever seen as a competitor of the Lancaster course?*

* Interview with Dr. Melvin G. Alper, Chevy Chase, Maryland, May 26, 1989. Telephone interview with Dr. David Donaldson, May 17, 1989.

Cogan: On the contrary, the Lancaster course was started as a sort of extension of the Harvard course for the large number of veterans that the Harvard course could not accommodate.* In fact, Dr. Lancaster invited me to be in charge of it for the first year, but I was already overloaded with other obligations.

Hughes: How long was Lancaster himself associated with the course?

Cogan: I believe only a year or two. Parker Heath soon took it over, and later, Henry Allen.

Hughes: Did Dr. Lancaster ever have a formal relationship with the Infirmary?

Cogan: That was before my time. I do not recall what the relationship was. I remember him attending clinical conferences. His chief interest was in optics. I believe his daughter has continued in this field.

Hughes: He was also associated briefly with the Dartmouth Eye Institute and aniseikonia.

Cogan: In addition, he was politically active in various ophthalmic organizations such as the American Ophthalmological Society.

I would really like to tell you about another course that had the same goal of emphasizing the basic sciences but was directed to undergraduate medical students rather than to graduates. It was during my last three or four years in Boston. Because of the popularity of ophthalmic residencies, medical students were already making applications for hospital appointments several years in advance, sometimes in their senior years and even their junior years. It was a time, so I thought, to introduce the teaching of basic aspects of ophthalmology to the dedicated few students who had already selected ophthalmology as a career.

Thus I organized a two-month elective course for not more than ten students. We met all day every weekday, using many of the same props that we had used in the graduate course. The faculty was almost entirely the Howe Laboratory staff. For the first couple of weeks, the faculty did much of the teaching, but gradually the students conducted their own classes on assignment and carried on the discussion with one or more of the faculty present. Emphasis was placed on ophthalmic anatomy, pathology, physiology, biochemistry, pharmacology—just those

* For further information on the Lancaster and Harvard courses, see the oral history in this series with Paul Boeder.

subjects which the students had had in medical school, but now applied to the eye. It was a stimulating experience for me and a pleasant association with a small group of students whom I got to know well. Some of those students have since become leaders in the field.

Is this type of course feasible in most curricula? Probably not. It requires more time than most faculty can afford to give and a cohort of dedicated students that has early decided on an ophthalmic career. It just happened to fit into the circumstances in Boston at the time.

Hughes: I gather that you like to teach.

Cogan: I find it challenging and the contacts very rewarding.

Hughes: Would you describe your style of teaching?

Cogan: As you saw from your attendance at the conference yesterday,* I like the informal give and take that one has in small group dialogues. I like the exchange with persons from other fields. Yesterday, it was Roscoe Brady who met with us to discuss some of the genetic enzyme defects which he has identified. Another meeting, which you were unable to attend yesterday, centered on my interest in optokinetic responses in human beings, but as guests, we had persons who work with analogous movements in animals. It was instructive.

Hughes: What is your ultimate goal in teaching?

Cogan: I would like to say it is to help others find the satisfaction I have had in ophthalmology. But I really think the true reason is that I learn a lot myself, and I find the contacts with students enjoyable and stimulating. I must say at NIH I miss contacts with students.

Hughes: Did your teaching responsibilities, particularly in those years when you were running the basic science course, interfere with your research?

Cogan: I'm sure they did, timewise. But they added to the research. When you're teaching, you are forced to organize your thoughts. Students ask pertinent questions that you may never have thought of.

* Grand rounds on the DAF syndrome, National Eye Institute, Bethesda, Maryland, May 1989.

Hughes: At some point, you turned over the supervision of postgraduate teaching to someone else in the Howe Lab. What became of the basic science course?

Cogan: Henry Allen took it over.

*Hughes: It was Dr. Howe's wish that members of the Howe Laboratory not be committed to teaching and other routine duties.**

Cogan: That's true. I find his prescience extraordinary for a person who had done little research himself. It is actually the philosophy of the intramural program of the National Eye Institute.

Hughes: These ideas were original with him?

Cogan: So far as I know they were, but I never knew Dr. Howe.

Hughes: Yet the Howe Lab has done considerable teaching, hasn't it?

Cogan: Yes, but I think Dr. Howe would have agreed that teaching of small groups, such as we did, is conducive to research.

Hughes: Who in the Howe Laboratory were involved in teaching?

Cogan: All the senior staff—Drs. Kupfer, Kinsey, Kinoshita, Kuwabara, Donaldson, Ludvigh, and Grant. I list Dr. Grant last because he was always reluctant to teach, as though he didn't like it, but he was one of the best and the most popular.

*Hughes: According to the Howe Laboratory report of 1949, a doctoral candidate for the first time was doing thesis work in the Howe Laboratory.***

Cogan: I think that must have been Frederick C. Merriam, working with Dr. Kinsey.

Hughes: Did the Howe have graduate students periodically?

Cogan: Rarely. I would have liked to have it occur more often, but the universities seemed reluctant to have PhD candidates do their thesis work in a clinical department.

* Beecher HK. p. 440.

** p. ll.

*Hughes: Dr. Donaldson remembered going over with you to the medical school dormitory to project slides.**

Cogan: Yes, we did that both at the [Harvard] college and at the medical school. The former was on special request, but the latter was on our initiative to create interest in ophthalmology at a time when ophthalmology was being largely jettisoned from the curriculum. Dr. Donaldson's 3-D projection of pictures of the eye and its diseases was seductively beautiful. We advertised these evening sessions by a note on the dormitory bulletin boards. When we added that the attendance would be limited to twenty-five persons, a full number of prospective attendees promptly signed up.

Hughes: Did you notice a sudden rush of applications for the ophthalmology residency?

Cogan: I was not in a position where I would have known, but it did coincide, in the 1960s, I believe, when applications throughout the country changed from a dearth to a superfluity of candidates. I'm sure what we did was only coincidental, at most a small pebble on the beach.

Hughes: Why the interest in ophthalmology?

Cogan: I really don't know. Perhaps because ophthalmology was announced as having one of the greatest remunerative returns.

Hughes: Maybe the popularity of the specialty also related to some of the technological breakthroughs that affected ophthalmology. I read of a preresidency year of research in the Howe Laboratory for physicians considering a career in ophthalmology. Was that something you instituted?

Cogan: I had in mind those persons who did not know they wanted to go into ophthalmology until their internship or later. By then, all the desirable residency programs would have been filled for the forthcoming few years. What would they do in the meantime? It seemed to me the time could be most profitably spent in a research laboratory. They would then enter their residency with a measure of sophistication. Some who did this claimed subsequently that this was the most valuable year or years of their career.

* Telephone interview with Dr. David Donaldson, May 17, 1989.

Nevertheless, this was a difficult point to get across. Most persons and their advisors thought it best to finish their residency and spend an optional year or so later even if it meant taking a less desirable residency. But postresidency fellowships are naturally attuned to clinical training rather than basic laboratory work. For this reason, I am partial to preresidency years if one is going to have laboratory experience. I could cite several instances to confirm this, cases where future administrators have had this period as their sole experience for fields in which they have had to have administrative charge. Or cases in which this period has allowed a continuation of research during the residency program.

Hughes: Did you have a role in accepting residents?

Cogan: The board of surgeons [of the Infirmary] accepted residents. I was on the board of surgeons.

Hughes: What were you looking for?

Cogan: I suppose the first consideration was to have someone who wouldn't cause trouble. Ethics and competence were equally prime considerations.

Hughes: How would you assess that?

Cogan: Personal interview.

Hughes: It's largely intuitive, isn't it?

Cogan: Yes. We probably made our quota of mistakes. There is a large gray area in the selection of candidates.

Hughes: Were you more interested in those candidates who seemed to have a more medical rather than a surgical interest in ophthalmology?

Cogan: When the candidate was still a medical student, as was often the case, we would be asked to recommend the type of internship. Well, my deputy chief was Dr. Carl Johnson, who recommended a surgical internship, whereas I recommended a medical internship. I thought it was good to leave the ultimate decision to the candidate.

Hughes: Did your medical orientation to ophthalmology cause tension with the surgically oriented staff?

Cogan: Not really.

Hughes: The last thing I have on the subject of teaching is the NIH training grant. When did it come into the picture?

Cogan: It was probably in the 1960s, that is, before the National Eye Institute was formed. I believe the Howe Laboratory was one of the first awardees of such a grant to ophthalmology by the Institute of Neurological Diseases and Blindness. It was most helpful.

Hughes: Did you administer it?

Cogan: I believe Dr. Carl Kupfer was its administrator, either through the Infirmary or the Howe Laboratory.

National Eye Institute

National Institute of Neurological Diseases and Blindness [NINDB]

Hughes: Dr. Cogan, you were a member of the advisory council of NINDB from 1955 to 1959. Do you know why you were appointed?

Cogan: Dr. Jonas Friedenwald had been on the council, he may have been the first ophthalmic representative, and died during his four-year term. I was a replacement.

Hughes: Why you?

Cogan: A good question. I suppose it was because the Howe Laboratory had become one of the recognized research laboratories at that time. Then, too, I had become identified with neuro-ophthalmology, and the granting institution was a neurologic institution. For that reason, I was probably better known among the grantors.

Hughes: Was there a limit to the number of years a council member served?

Cogan: I think it was four years.

Hughes: Do you remember who else was on the council?

Cogan: No, I don't, but I think it was intended to have two ophthalmologists among the professional members on the council of twelve, including neurologists. Six were lay persons

representing the public. Many of the latter were persons who had been active in the president's political party.

Hughes: Was there any stipulated ratio of neurologists to ophthalmologists?

Cogan: All that I remember was that there were supposed to be two ophthalmologists, but I may have been the only one when I replaced Dr. Friedenwald.

Hughes: Why wasn't it fifty-fifty?

Cogan: Because it was originally a neurologic institute. The neurologists had graciously accepted us as a last minute expediency when the Institute was formed. We were correctly a minority party.

Hughes: What is there to say about those early years you were on the council?

Cogan: Well, I learned to have a lot of respect for the peer review system. I found it very impressive. My former fear that it was to be a rubber stamp for congressmen's pork-barreling was unwarranted. And the council was fair to ophthalmology.

Hughes: Did you feel that neurologists were listening to you?

Cogan: They listened, but I felt they always thought of ophthalmology in terms of neuro-ophthalmology. I found myself in the paradoxic position of representing ophthalmology as a predominantly surgical discipline with natural concern in problems of cataracts, glaucoma, retinal detachment, etcetera.

Hughes: I came across the name of [Ludwig] von Sallmann in connection with NINDB.

Cogan: Dr. von Sallmann was chief of the intramural eye section under NINDB. He was a scholar and delightful person, with all the charm of old school Vienna from whence he had migrated to the United States because his wife was Jewish. He was recruited from Columbia University to NINDB. His background was that of a clinician, but in this country, he emerged as one of the earliest experimental pathologists. When I came to Washington for council meetings I always spent some time with him.

Hughes: I'm not quite clear on the setup at NINDB. Was there a hospital?

Cogan: Yes. The [Warren Grant Magnuson] Clinical Center was established in 1952, and that is where von Sallmann's activities were.

Hughes: What was his interest?

Cogan: Mostly uveitis. He was also interested in radiation-induced cataracts.

Hughes: You apparently were chairman of the NINDB Training Grant Committee.

Cogan: This was the first training grant committee for ophthalmology under the supervision of NINDB.

Hughes: From my notes, other members of the committee were Percival Bailey (director, NINDB), Irving Leopold, and Ed Maumenee. Was there much money to allot?*

Cogan: I have poor recollection of the activities of this committee. The money couldn't have been very much.

Founding a Separate Eye Institute

Hughes: Would you tell me why people perceived a need for a separate eye institute?

Cogan: The budget allocated for ophthalmology was low in comparison to that for neurology, $18 million for eye in comparison to $90 million for nonophthalmology. However, understandably from a historical point of view, several persons, notably Dr. Everett Kinsey from the Howe Laboratory and Mr. Ulmer, a lawyer from Cleveland, felt ophthalmology could do better if it had its own institute. They secured Mary Lasker's endorsement but did not arouse much enthusiasm among ophthalmologists in general and were vigorously opposed by the NIH administration, including Dr. James Shannon, the powerful director of NIH. There the matter lay for a while.

Then Dr. Maumenee called together a meeting of several department heads to discuss what should be done about the problematic early appointment of residents, the feasibility of a matching plan for resident appointments, and the desirability of forming an Association of University Professors of

* Bailey P. America's first national program in eye research. Am J Ophthalmol 1955; 40:121-6.

Ophthalmology.* The meeting was held in a motel near the O'Hare Airport in Chicago. Present were Dr. Hogan from San Francisco, Dr. [Bernard] Becker from St. Louis, Dr. Maumenee from Baltimore, Dr. John McLean from New York, Dr. [Frank W.] Newell from Chicago, and me. Then came a snowstorm, which was unusually severe even for Chicago, and the airport was shut down. What was intended to be a brief in-out meeting turned out to be a two-day affair.

The extra time permitted an unscheduled discussion of a possible national eye institute. The chances seemed slim unless we could get the cooperation of the influential Dr. Jules Stein. Dr. Maumenee was also a personal friend of Senator Lister Hill. Even so, the chances seemed slim. We were all, therefore, much surprised when President Johnson signed a bill in 1968 authorizing the organization of the National Eye Institute.**

Hughes: Please explain who Stein was and how he fit into this.

Cogan: Much of the credit for the founding of NEI is due to Jules Stein and his Research to Prevent Blindness, Incorporated [RPB], which laid the groundwork. In case you may not know it, Jules Stein was a board-certified ophthalmologist in Chicago in the 1920s. But he had a competitive interest in orchestras, and these won out. He gave up ophthalmology for a brilliant career in the music world, culminating in the foundation of Music Corporation of America. Late in life, he returned to ophthalmology as a benefactor of ophthalmic research through his Research to Prevent Blindness, Incorporated.

*Hughes: I talked with David Weeks, who remembered a note that you had written to Jules Stein.*** He has never been able to find that note, so I can't tell you exactly what was in it. He looks upon that as the turning point in the struggle to establish an eye institute. Before that, there had been a lot of opposition to the eye institute, even among ophthalmologists. Your note apparently said something to the effect that you didn't feel that ophthalmology would ever fulfill*

* See the oral history in this series with Thomas D. Duane and the forthcoming oral history with A. Edward Maumenee for further discussion on the formation of the National Eye Institute.

** The National Eye Institute was founded on August 16, 1968 when President Lyndon Johnson signed Bill 812843 into law. (Cogan DG. The founding of the National Eye Institute: A personal recollection. Unpublished paper on deposit at the American Academy of Ophthalmology.) By January 1970 when Dr. Kupfer came to the Institute on a full-time basis as its first director, the National Eye Institute was finally established as an independent and fully operational component of the National Institutes of Health. (Kupfer C. The National Eye Institute. Am J Ophthalmol 1972; 74:1011-21.)

*** Interview with Mr. David Weeks, Las Vegas, Nevada, October 10, 1988.

its potential within NINDB, and that RPB should be brought into the picture to organize a campaign for a separate eye institute. Do you remember writing anything like that?

Cogan: I don't remember that specific note, but I did have considerable correspondence with Dr. Stein along such matters.

Hughes: When NEI finally came into being, it coincided with the cutback in government funds for research in the 1970. Were you aware of the financial struggles at NEI in those early days?*

Cogan: I have had mercifully little to do with the funding of NEI. But Dr. Kupfer has been very effective in representing ophthalmology's interests to Congress. I understand that NEI has fared much better than most other institutes over the years. As you recall, Dr. Kupfer had been in the Howe Laboratory before he became chairman of the Department of Ophthalmology in Seattle whence he was recruited for the directorship of NEI. The job is one of the most important positions in ophthalmology, with multiple responsibilities. Yet the salaries, including that of the director, are less than those of most university professorships. It requires a dedicated person. Of all the institute directors, Dr. Kupfer has survived the longest by far.

Hughes: Why do you think that ophthalmology, compared to certain other specialties, was rather slow in developing a strong research component? Or would you even agree?

Cogan: I would not agree. I believe ophthalmology has been a leader in the development of research potential.

Research to Prevent Blindness, Incorporated

Hughes: How did Research to Prevent Blindness fit into the picture?

Cogan: Dr. Stein formed this charitable organization to give and get support for research in ophthalmology. As he said once, ophthalmology needed a businesslike approach to raise funds for research. This was in the 1960s, before a separate eye institute was seriously considered. I remember he called a number of us, possibly thirty-five, together in his beautiful offices on Madison Avenue to discuss what the needs were. One of our major suggestions was that some sort of contingency support would be most useful to allow departments to take advantage of unanticipated research possibilities. This has been, and still is, a

* Interview with Dr. Carl Kupfer, Berkeley, California, November 9, 1988.

major contribution of RPB to eye research throughout the country. About this time, RPB authorized a survey, conducted by Dr. Thomas Duane, of ophthalmic research in the United States. This was published in book form in 1965.*

Hughes: I understand, both from Dr. Duane and Mr. Weeks, that somebody connected with Research to Prevent Blindness had some expertise in dealing with Congress, that he helped the ophthalmologists who were selected to present the arguments to Congress prepare their case. Does that ring any bells with you?

Cogan: I remember that Mary Lasker was very helpful in getting hearings before congressional committees. Perhaps Dr. Duane and Mr. Weeks were referring to a professional lobbyist who did a lot of preparation of the testimony. On the occasion I testified, I would have preferred to present material on my own.

Hughes: Is there a story connected with your congressional testimony?

Cogan: I testified only twice.

Hughes: Did you have butterflies?

Cogan: I don't recall that. The presentations had been thoroughly organized by RPB. They had been prepared by the lobbyist. Most of my suggestions were deleted as being unrealistic.

Hughes: What were your suggestions?

Cogan: I've forgotten what they were. I assume they pointed up areas of research which needed funding. Since I was not acting in any official position, I could suggest funding in excess of what the [Eye] Institute director requested. I remember it seemed odd to me that the director could not request funds over and above the president's recommendation. RPB was not limited by this.

Hughes: That limitation is true for all institutes?

Cogan: I think so.

Hughes: How does the president make up his mind what is necessary?

* Duane TD. Ophthalmic Research: U.S.A. A comprehensive national survey of eye research. A report to the trustees of Research to Prevent Blindness, Inc., 1965. See Dr. Duane's oral history in this series for discussion of the survey.

Cogan: I don't know. I suppose he gets advice from OMB [Office of Management and Budget].

Hughes: Why weren't ophthalmologists instantaneously enthusiastic about the Eye Institute? Why was there a hesitation?

Cogan: The natural fear of government in medicine, I suppose, the fear of socialized medicine.

Hughes: Would you look upon that snowstorm meeting as the turning point?

Cogan: I think it was, from our point of view. It initiated well; you might say it started the snowball rolling.

Hughes: You now had some unity among some of the leading figures within ophthalmology, which must have had an influence on the rank and file.

Cogan: It was the initiative of Dr. Maumenee in calling the meeting that resulted in the formation of the Association of University Professors of Ophthalmology.

Hughes: Was the primary impetus for the formation of that association lobbying for the Eye Institute?

Cogan: No, but it does have a vested interest.

Hughes: What are the concerns of the association nowadays?

Cogan: I cannot speak for present policy since I automatically lost my membership when I was no longer a department head.

Hughes: You continue to be a member of the board of Research to Prevent Blindness. There are several boards. Is that the scientific board?

Cogan: It's the scientific advisory panel.

Hughes: What are your responsibilities?

Cogan: We meet several times a year to evaluate applications for support and to discuss policies with the president, Mr. David Weeks. This concerns career development awards, foreign scholar awards, several named professorship awards, and the development awards. The processing is run in a very businesslike manner in accordance with the Stein prescription. RPB is constantly alert to approaches that cannot be covered by NIH.

Hughes: Do the proposals tend to be more innovative than those submitted to NIH?

Cogan: They do not have some of the restrictions inherent in government bureaucracy.

Hughes: In the early days, the scientific panel was not composed strictly of ophthalmologists?

Cogan: It never was.

Hughes: What is the thinking there?

Cogan: There are only two, or at most three, ophthalmologists on the panel. At present, aside from myself, there are Drs. Ed Norton and Ephraim Friedman. Dr. Lorenz Zimmerman, an ophthalmic pathologist, has also recently joined the panel. The others are eminent scientists in a variety of fields, making up a panel of about ten persons. It is an impressive group. One is a Nobel Laureate.

*Hughes: Is that George Wald?**

Cogan: No.

Hughes: I knew that Research to Prevent Blindness was instrumental in the founding of the Eye Institute, but I didn't realize it had an ongoing role. Does it consider that part of its mandate?

Cogan: Yes. It provides the citizens' committee to testify to Congress and employs a lobbyist to plan the approach. It is, of course, a private organization but maintains some contact with NEI. That is probably why I am on its scientific advisory panel. We meet in New York several times a year to discuss applications, nominations for support of professorships, and other policy matters. I have tried to promote removal of their administrative offices to Bethesda, but they point out they occupy rent-free some of Stein's old offices on Madison Avenue, and, moreover, their staff are New Yorkers.

Hughes: It would be hard for them to give up Manhattan, wouldn't it?

Cogan: Also hard to compete with free rent on Madison Avenue.

* Dr. Wald was a member of the panel in the sixties.

Hughes: Is it a large organization?

Cogan: It's the largest private organization supporting eye research.

Hughes: What are the approximate numbers?

Cogan: I cannot answer that off the top of my head. But I would say that their regular contribution of an uncommitted $20,000 to some thirty or more departments throughout the country is of inestimable value, as is also their funding of named professorships, foreign scholars, etcetera.

Hughes: Who is responsible for that policy?

Cogan: Mr. David Weeks is the president of RPB. He follows the Stein tradition of a businesslike policy, with reliance on peer reviews, scientific council advice, and ultimately trustee approval.

Hughes: Research to Prevent Blindness continues to have a lobbyist in Washington, does it not?

Cogan: Yes, it is thoroughly knowledgeable about political affairs. I don't think most ophthalmologists approve of lobbying, but it appears to be a fact of life.

A form of lobbying is RPB's biannual colloquium of research advances presented to members of the press. Some thirty or more journalists are invited to attend several days of presentations by scientists and ophthalmologists on recent advances in ophthalmic research.

Hughes: Have you been involved in these presentations?

Cogan: I was at one time.

Hughes: What sort of things have you presented?

Cogan: It was so long ago that I have forgotten the details. It must have had something to do with research expressed in a lay vernacular.

Hughes: Are these meetings well attended by the press?

Cogan: Those which I have attended in recent years had twenty-five or more journalists present.

Hughes: Do they then write on the subject that was presented?

Cogan: They report on selected topics which they feel are newsworthy.

Hughes: Do other specialties have arrangements like this?

Cogan: I suspect some cancer societies do.

More on Radiation Cataracts

[Interview 5: August 6, 1989, The Cogans' Vacation Home, Leland, Michigan]

Cogan: By coincidence I was asked, about the time Dr. Kinoshita joined us, to examine the crew of the *Nautilus*, the first atomic powered submarine, which was to be launched at Groton, Connecticut. The purpose of the study was to document possible lens or other ocular abnormalities in the crew before their residence beneath the North Pole for several months. I thought this might be an occasion to introduce Dr. Kinoshita to some of our clinical methodology. Together we spent two or three days at the submarine base doing slit lamp biomicroscopy and indirect ophthalmoscopy on fifty or more crew personnel and on an equal number of control subjects of about the same age from the General Dynamics Corporation who were not part of the crew. It was an intensive course in clinical methods for Jin and a pleasant opportunity for me to see a biochemist suddenly thrust into a clinical environment. I always felt free later at the Laboratory to show him patients who might have conditions in his field of interest. He knew how to use the examining equipment.

Hughes: After the submarine's prolonged stay under the ice, did you examine the men for cataracts?

Cogan: No. Apparently none developed ocular problems subsequently or else our records sufficed to handle what problems may have arisen. So far as I know, the protection from radiation sufficed to prevent cataracts. From Dr. Kinoshita's and my point of view, it was an interesting experience.

Hughes: A trial by fire.

Cogan: Something like that.

Hughes: I notice that the lens studies in the fifties were supported by the AEC [Atomic Energy Commission]. Was that because of a possible relevance to radiation cataracts?

Cogan: Yes. The AEC was, of course, operated by physicists who were well aware of the cataract problem from radiation exposure.

Hughes: Did you have an ongoing relationship with the people at the Harvard cyclotron because of the potential for radiation cataracts?

Cogan: No, but as I mentioned earlier, Dr. Donaldson and I repeatedly documented the progress of cataracts in Eric Clark, who had been exposed to the cyclotron [radiation]. He was possibly the most thoroughly studied of that group of twelve physicists who developed radiation cataracts.

Hughes: What is the latency between exposure and actual cataract formation?

Cogan: That depends on the amount of exposure. The heavier the dose, the shorter the period. With low doses, such as in the case of Eric Clark, it may be several years, whereas in heavier exposures, it may be a matter of months.

Hughes: Was that information instrumental in producing radiation-protective measures? Or had those already been put in place?

Cogan: Gamma radiation exposure was well known and monitored by Geiger counters. What was not known in those early days was the amount of neutron exposure since there was no way of measuring neutron radiation. That is why cyclotron workers were exposed to radiation without knowing it.

Lens Metabolism

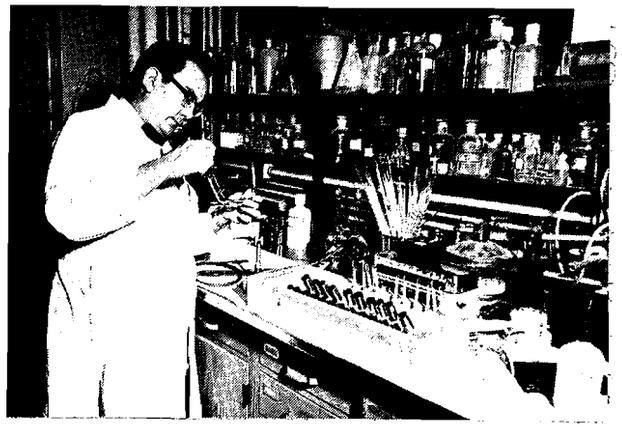
Hughes: I read in the 1954 Howe Laboratory report that the lens uses a direct oxidation reaction to break down glucose, as opposed to the more common Krebs cycle. Did that turn out to be a significant finding?*

Cogan: That certainly was. It was Dr. Kinoshita's key discovery, on which much of his subsequent research was based. As you point out, the lens uses the direct oxidative reaction (the so-called shunt pathway) for the breakdown of glucose, instead of the usual citric acid cycle.

Hughes: Is there a reason for that?

* p. 3.

Cogan: So far as I am aware, the reason is not obvious. One suggestion is that the avascularity of the lens has something to do with it. Perhaps the inability of the lens to dispose of its lactic acid requires a more direct method of oxidation. Whatever the reason, it appears to be tied in with the accumulation of sugar alcohols in the lens, another key observation reported a few years later by van Heyningen of England and extensively studied by Jin Kinoshita.



Dr. Kinoshita at work

Hughes: Who is van Heyningen?

Cogan: Ruth van Heyningen was one of a pair of English biochemists (the other was Antoinette Pirie) who were interested early in cataract formation. Kinoshita credits van Heyningen's demonstration of sorbitol in cataractous lenses as the event which prompted his concept of the osmotic basis for diabetic cataracts.

Hughes: Could you summarize the work that led to the understanding of how sugar cataracts are formed?

Cogan: According to the concept of Kinoshita and his collaborators, glucose enters the lens freely from the aqueous humor. Once it is within the lens cells and fibers, it is reduced to sorbitol, the sugar alcohol, and then escapes from the fibers so slowly, or is metabolized by the fibers so slowly, that in the presence of excess sugar (as in diabetes), it sets up an osmotic gradient that causes the fibers to swell. The swollen and eventually fragmented fibers are opaque and present as cataracts. Sugars other than glucose

are similarly cataractogenic. In fact, galactose is used instead of glucose for the experimental production of sugar cataracts.

Hughes: Were the swollen fibers evident by light microscopy?

Cogan: Dramatically so. I remember the luncheon session when Dr. Kuwabara showed the beautiful sections of swollen fibers in the lens of an experimental sugar cataract.

Hughes: Did he follow it with the electron microscope?

Cogan: Yes, and this showed that some of the fibers had become leaky, so that fluid was present outside the fibers as well.

Hughes: Was that new information?

Cogan: I think I am right in saying it was an entirely new concept. But then Dr. Kinoshita went further by exploring the possibility of preventing the cataracts by inhibiting the formation of the sugar alcohol in the first place. The sugar is reduced in the fibers by the enzyme aldose reductase. Dr. Kinoshita and his collaborators have now shown that sugar cataracts (and other diabetic complications) can be prevented experimentally by appropriate inhibitors of aldose reductase. Thus has come about a discovery of potentially great therapeutic significance.

Hughes: Was the basic laboratory technique derived from Kinsey's lens culture system?

Cogan: It was a useful part of the experiments. I wouldn't say it was crucial.

Hughes: I understand there was a continuing relationship between Dr. Kinoshita and Dr. Kinsey, who had moved to the Kresge Eye Institute in Detroit.

Cogan: It was a close relationship and a sharing of experience and interests.

Hughes: That implies that Dr. Kinsey's interest in the lens continued once he had left the Howe.

Cogan: Absolutely.

Hughes: Did he take it in similar directions?

Cogan: Not exactly. Kinsey's interests were more in electrolyte transport, whereas Kinoshita's interests became progressively directed toward the phenomenon of sugar cataracts.

Hughes: Dr. Kinoshita's work today, I understand, focuses on development of aldose reductase inhibitors. Did they become, very early on, an obvious possibility for the prevention of cataracts?

Cogan: I don't know just when the idea occurred to Dr. Kinoshita, but I know the first use of inhibitors had prevented the experimental cataracts dramatically. The first inhibitors were prepared by the Ayerst Laboratories in Canada, but the most widely studied has been the Pfizer product known as Sorbinil. There are now a number of pharmacologic companies in this country and in Japan which are exploring various inhibitors. A nontoxic inhibitor could revolutionize the treatment of diabetes not only for prevention of cataracts, but for other diabetic complications as well.

Sorbinil has not been approved by the FDA. That is why there is wide competition among drug companies to develop alternative inhibitors. A couple of inhibitors are already being used abroad but [are] not, as yet, approved by FDA for use in this country.

Hughes: But clinical trials are indeed in progress in this country?

Cogan: The National Eye Institute has several control studies in progress.

Clinical Trial of Sorbinil

Hughes: I read of a multicenter clinical trial of Sorbinil. Is it an ongoing project?

Cogan: Yes. This is a multiuniversity study. I believe some thirty centers are involved, with some 2,000 or so patients enrolled. The report of this study will probably be announced early next year [1990]. I believe clinical trials are also being conducted on a couple of other inhibitors as well. I understand the recruiting of patients for these clinical trials is not easy.

Hughes: Our drug regulations seem to be tighter than those of many other countries, which presents a problem, but it equally presents a problem if the regulations aren't tight enough. Then terrible things happen like the thalidomide disaster.

Cogan: That thalidomide story strengthened the conservatism of the FDA.

Hughes: What is your thinking about the problem of drug testing?

Cogan: I am aware of the pros and cons, but I think controlled studies such as NIH is conducting are the only way to go.

Hughes: What is your feeling about the FDA regulations?

Cogan: We are fortunate to have a regulatory body. It has a very difficult task.

Hughes: Is it too early to say what the promise of these trials is?

Cogan: Some trials have already been reported out; others are still operative. Even when the enrollment is closed, it may take years before conclusions can be drawn. The Sorbinil trial will be reported early next year.

Hughes: Are there known complications of Sorbinil?

Cogan: Rumor has it that a conjunctival and dermal reaction, known as the Stevens-Johnson syndrome, may result, but the definitive answer will probably be made when the final report is made. In any case, it must be a rare complication.

Hughes: What is the mechanism?

Cogan: I don't know what the mechanism is, but a similar complication has occurred with some antibiotics. Although rare, it is sufficiently serious so the FDA will not approve it.

Hughes: Do you know what Dr. Kinoshita will be pursuing when he moves to UC Davis?

Cogan: I believe he plans a semiretirement. I'm sure he will continue to work in some capacity, either at the university or as a consultant to pharmaceutical companies.

Hughes: To what extent can the work on sugar cataracts be related to cataract formation in general?

Cogan: That's a good point. Are the mechanisms for various types of cataract formation similar or not? Certainly the pathogenesis for diabetic cataracts is the same as that occurring with galactosemia, where the enzyme for metabolizing galactose is

lacking. Beyond that, the mechanism is probably quite different for different types of cataract.

Hughes: Is that enough on a very big topic? Is there something you'd like to add?

Cogan: I'd just like to say that it's one of the most important things that has come out of the Howe Laboratory and the National Eye Institute.

Hughes: Was it appreciated as such by pure biochemists?

Cogan: That I don't know.

Biochemistry in the Laboratory

Hughes: Dr. Kinoshita, I understand, was responsible for bringing in other biochemists, and I'm sure you had a part, too. I'm thinking particularly of Drs. Sidney Futterman and Abraham Spector. Why were they attracted to the Howe Laboratory?

Cogan: Dr. Kinoshita recruited his staff, and each seemed to fulfill a particular niche in the overall mosaic of the lens study and related observations on other ocular tissues. Dr. Futterman's studies, for instance, concerned especially the kinetic properties of retinal tissue. Dr. Spector concentrated on enzymatic control of lens protein metabolism. Dr. Kinoshita's staff included more than a dozen other members of his team including both PhDs and MDs involved in lens, cornea, and retinal biochemistry. Some had permanent appointments while others served as fellows in training.

Hughes: Was Dr. Kinoshita responsible for bringing in Dr. Chader?

Cogan: Yes, on the recommendation, as I remember it, of Dr. Liane Reif-Lehrer, who was already on Dr. Kinoshita's staff. Dr. Reif's interests were in retinal development, and Dr. Chader's wartime research had involved cortisol in the stress reaction. Thus began their joint project of cortisol control of retinal growth in the chick retina.

Hughes: What about interchanges with Dr. Kuwabara who was pursuing lens research on a morphologic level?

Cogan: The lens was only one of Dr. Kuwabara's many morphologic interests. At times his observations were very close to those of

the chemists and at times relatively remote. To those of us who had a viewer's interest in both, the morphologic aspects brought it all together.

Hughes: Would you say that the biochemical findings prompted Dr. Kuwabara to look for morphological signs?

Cogan: It worked both ways.

Hughes: So it wasn't a chance finding on Kuwabara's part?

Cogan: I'm sure much evolved from chance, possibly at a luncheon session, such as, "Wouldn't it be interesting to look at the lens of these eyes that develop cataracts from high glucose or galactose levels?" That afternoon, Dr. Kuwabara might receive just such a lens, and in the next day or so, microscopic sections would be shown to the group.

Hughes: What an exciting agenda!

Cogan: It was. Such events come about when small groups of persons with common interests but diverse approaches meet in an informal atmosphere of trust and mutual respect.

Hughes: It's exactly what you had hoped would happen, an interchange among basic scientists and also between clinicians and basic scientists.

Cogan: I should add the proviso of compatible personalities.

Hughes: And the right environment.

Cogan: An environment relatively free of bureaucratic restrictions or other encumbrances of research. In a recent statement, Dr. James Wyngaarden, former director of NIH, stated that he resigned in large part because he was overwhelmed by the bureaucratic restrictions imposed on research by a well-meaning but unwise Congress.

Hughes: That restricts the flow of information.

Cogan: That among other things.

Hughes: Yet when I talked with Dr. Chader, he was very supportive of the NIH environment. He said something to the effect that he was*

* Interview with Dr. Gerald Chader, National Eye Institute, Bethesda, Maryland, May 24, 1989.

given freedom to pursue his research in any direction. He did, however, mention that his group was now isolated from you and your group even though it was only yards down the road. It is not as good as the situation in the Howe Lab, where you were virtually next door to one another. Everybody speaks of the Howe Laboratory luncheons as being absolutely crucial to the exchange of information.

Cogan: I would agree the NIH environment has tremendous advantages, but freedom from paperwork is not one of them. Dr. Chader is now replacing Dr. Kinoshita as scientific director, which will take him out of the laboratory a good part of the time. It would be interesting to pose your question to him again in a year's time.

*Hughes: Another question I had was about the ophthalmic biochemistry meetings, which you initiated in 1956.**

Cogan: 1956 must have been the year that Dr. Kinoshita organized the meeting at the Harvard Faculty Club where some twenty-five or so biochemists and physiologists (and a few clinicians) gathered for a colloquium. Actually, the idea for such a meeting went back to an earlier period when Dr. Kinsey and I discussed the desirability of bringing together those relatively few basic scientists in ophthalmology. It was a time when it took a good deal of courage for PhDs to leave their academic departments for a clinical environment. We thought it would be some reassurance for them to know that others were doing it and found it satisfactory.

I do not remember whether or not we actually held such a meeting, but we did have something akin to it in the interurban meetings of the war gas groups during the war. It really got off the ground, however, only when Dr. Kinoshita took it over within a couple of years after he joined the Laboratory. It was a sudden success and is still an annual event, although no longer a strictly Boston affair. I attended the meeting which was held at beautiful Oakland University two years ago.

Hughes: Is it now a meeting with a traditional format?

* The interviewer is grateful to Dr. Kinoshita for supplying copies of correspondence relating to the first two ophthalmic biochemistry meetings: Cogan to Kinsey, June 17, 1955; Jonas Friedenwald to Cogan, June 20, 1955; Kinsey to Charles Snyder, November 14, 1955; Snyder to Kinsey, December 2, 1955. Copies of this correspondence and lists of participants in the first and second meetings are on deposit at the Foundation of the American Academy of Ophthalmology, San Francisco.

Cogan: I really don't know since I have not attended in recent years except for the Oakland meeting when I gave the Kinsey Memorial Lecture.

Hughes: On the lens work?

Cogan: On Dr. Kinsey particularly. I pointed out that Dr. Kinsey's wife, Irene, was also of great help to the Howe Lab in proofreading and manuscript preparation.

Hughes: This discussion reminds me of the Verhoeff Society. I'm wondering if that, too, wasn't patterned after the war gas exchange meetings.

Cogan: They all have a common pattern of informality. Most recently, we have the American Ophthalmic History Society, which is also pleasantly informal.

Hughes: That seems to me to be the Cogan style.

Cogan: I like it that way.

Hughes: Anything more that you care to say about biochemistry?

Cogan: I can't help but think back to the days when Dr. Verhoeff was so skeptical about having biochemists in the Laboratory. I am sure that he would now agree it has been a productive strategy.

Neuro-Ophthalmology

Cogan: I would like to say something about neuro-ophthalmology because this was one of the first of the subspecialties to be recognized [in ophthalmology] and one in which I have had a career-long interest. It encompasses that area between neurology and ophthalmology and is populated by an approximately equal number of persons from each discipline. It appeals to those who are interested in the medical, as opposed to the surgical, aspects of medicine. I was fortunate to be able to pursue my interest in the subject and in its teaching during the formative stages of its development. And then when I wrote a couple of books on the subject and made a few contributions to the field, I came to be looked upon as an authority. There's nothing like an assumed position of authority to be self-generating. It was easy for me to have a fellowship program at the Infirmary and the Howe Laboratory. My contact with these fellows has perhaps been the most satisfying experience in my professional life and a source of

pride as the former fellows have established their separate and, in many cases, prestigious careers.

Hughes: Could you tell me the general state of neuro-ophthalmology when you became interested in it?

Cogan: I can't say it had a very high rating among ophthalmologists in this country when I became interested in it. Ophthalmology, after all, is a surgical specialty. Many persons who today might become neuro-ophthalmologists opted then for neurology rather than ophthalmology. I, too, was fascinated by neurology but would have found it difficult to practice because of the low therapeutic yield at that time. Yet in my internship and later residency, I was never too far away from it and eventually found an outlet in neuro-ophthalmology. At that time, Frank B. Walsh and Donald Lyle were neuro-ophthalmologists, but there were very few others in this country. Yet abroad, especially in Germany, it was a recognized specialty as evidenced by the multivolume text of [Herman] Wilbrand and Alfred Saenger.

In the late 1930s, my contacts with Bielschowsky confirmed my respect for the subspecialty, and I became a teacher of this subject in our courses at the Infirmary. To my surprise, I was looked upon as an authority, and I had to try to measure up to that reputation. The fact that few ophthalmologists were interested in the subject meant that I had a wealth of patients referred to me, and unlike busy practitioners, I had the time to spend, which neuro-ophthalmic patients require.

Hughes: Wouldn't you say it also requires broader knowledge? A neuro-ophthalmologist has to extend his knowledge to the brain, while an ophthalmologist practicing general ophthalmology could say, "Well, my focus is the globe."

Cogan: It had to be of interest to a person who liked neurology but decided to go into ophthalmology. From an intellectual point of view, neurology must be the most attractive field in medicine.

Hughes: Why do you say that?

Cogan: It involves so many abstractions. The brain operates in so many mysterious ways that can be elucidated only by intelligent insight. One of these ways is the recognition and interpretation of disease states. This is the intellectual challenge of neurology.

Hughes: Do neurologists pay particular attention to the eye since it is an accessible part of the nervous system?

Cogan: Much more now than formerly. Ophthalmologists initially had the advantage of being familiar with examining techniques for eye movements and interpretation of abnormalities in the visual pathways, but now as many neurologists specialize in neuro-ophthalmology as do ophthalmologists.

Hughes: I've heard the terms "cog" and "noncog," meaning cognitive and noncognitive, used in not very accurate fashion to distinguish the two approaches that doctors can take to medicine. Surgeons represent the noncogs, and the internists, and I think Dr. David Cogan, the cogs. Is there a grain of truth in that distinction?

Cogan: I've not heard that expression before, but the distinction is probably correct. The surgeon sees a problem and wants to do something about it. Why spend valuable time discussing it at length? Neurologists, on the other hand, indulge in prolonged dialectics that may or may not come to a definitive conclusion. Fortunately, there is merit in both approaches.

Hughes: Frank Burton Walsh is a name that looms large in American neuro-ophthalmology. His textbook, Clinical Neuro-Ophthalmology, *came out the year before your book,* Neurology of the Ocular Muscles.* *Did you have contact with him?*

Cogan: Of course, I knew Dr. Walsh well and was fond of him, everybody was fond of him. He was a low-key physician who always reminded me, as he lectured, of a parson addressing his flock. The first edition of his book came out in 1947. It was already a classic when my book came out.

Dr. Walsh had been an internist in western Canada before he came to the United States. He had his ophthalmic training under [William H.] Wilmer at Hopkins. His large textbook, *Neuro-Ophthalmology,* became the standard text, full of personal anecdotes. Don Lyle's text came out about the same time or maybe earlier.

Hughes: Was Dr. Walsh an ophthalmologist as well?

Cogan: Yes, I believe he was one of Dr. Wilmer's first residents at Hopkins. He practiced in Baltimore and wrote his book at Hopkins.

Hughes: Did that book spur interest in the subspecialty?

* Springfield, IL: Thomas, 1948.

Cogan: I have no way of knowing. It was not an easy book to read.

Hughes: How much time was being spent on neuro-ophthalmology in residency training programs in the forties?

Cogan: So far as I recall, it was very little.

Hughes: Did you spend any time at Hopkins?

Cogan: Only for attendance at meetings.

Hughes: Had your interest in neurology arisen in Chicago?

Cogan: I was interested in neurology in medical school, pursued it in my internship, and found an outlet for it in my residency and subsequent teaching exercises. After writing two books on the subject, I found that I was considered an authority.

Hughes: Why was your initial focus the neurology of the eye muscles?

Cogan: Rather than vision?

Hughes: Yes.

Cogan: Because eye muscle neurology is easier to teach, and the book was simply an extension of my teaching notes. It took me several years to formulate my notes for a book on the visual system. As you know, there are two branches of neuro-ophthalmology. There's the motor branch and the visual branch. The motor group is easier for persons like me to comprehend because it depends on objective observation rather than abstract thinking. Abnormalities in eye movements can be seen, measured with extraordinary precision (speed, acceleration, fatigue, etcetera), and easily cataloged. Only later did I undertake to put my notes together on the visual system.

By the way, those two books were written up here in Michigan. The garage was too small for a modern car, so I took it over for my study and workshop. It was off-limits for the children. Only I and the chipmunks occupied it. I should have acknowledged them in my preface.

Hughes: I think of Bielschowsky as being particularly interested in eye muscles. Do you think there was any influence there?

Cogan: Some. I was impressed during my residency that here was a man who dared to make a career out of something that was not very popular in this country.

Hughes: What was the reception to Neurology of the Ocular Muscles?

Cogan: The sales and demand for subsequent editions would suggest that it was well received, but, of course, many persons might have purchased it simply to have on their shelves. The publisher told me it set a record for republications and copies. Between republications there was a brisk black market for the book. Of course, behind this seeming popularity was a marked increase of interest in the subject of neuro-ophthalmology. I think I had more feedback from neurologists than from ophthalmologists.

Hughes: I'd like to quote a section of your preface. You say, "the familiar truism to the effect that clinical findings can be properly interpreted only with a knowledge of anatomic structures and physiologic principles is nowhere more cogent than in the case of the ocular muscles." What did you mean by that?

Cogan: I meant that to interpret eye movement abnormalities, one had to know the underlying connections in the brain and the way the control systems interacted. Of course, ideas that I professed in the 1940s seem elementary today. I would not advise one to purchase my book on the muscles today. A much better text is that of neurologists R. John Leigh and David S. Zee.* It is a magnitude jump over my text.

Hughes: In what sense?

Cogan: In the up-to-date knowledge of the neuroanatomy and methods of measurement.

Hughes: In 1956, you wrote a second edition because, you said, of the recent upswing of interest in neuro-ophthalmology among students. Do you have an explanation for that upswing?

Cogan: Neuro-ophthalmology took off. Neither Frank Walsh nor I understood why. It did correlate with a great increase in the number of applicants for ophthalmic residencies and of their qualifications, judged by their medical school records. Dr. Walsh and I were, of course, pleased by it and profited in it in the sense that we were invited to give lectures and participate in meetings.

* Leigh RJ, Zee DS. The Neurology of Eye Movements. Philadelphia: FA Davis Co, 1983.

Hughes: Could you compare and contrast what you were doing in neuro-ophthalmology as compared with what Dr. Walsh was doing?

Cogan: Dr. Walsh was, I believe, a full-time neuro-ophthalmologist and received, I presume, some support from the Wilmer Institute. At least it would have been difficult at that time to be a full-time neuro-ophthalmologist without receiving outside support since one sees very few patients and many of these are nonchargeable. My situation was similar in that I was supported in part by the Howe Laboratory and similarly operated a fellowship program in neuro-ophthalmology, but it differed in that I was not a full-time neuro-ophthalmologist. My other activities occupied the majority of my time.

Hughes: How does William F. Hoyt fit into the story?

Cogan: It must be said that Bill Hoyt has played a major role in establishing neuro-ophthalmology in this country. His fellowship program and his joint operation with neurologists and neurosurgeons have had a profound effect on the post-Walsh direction of the subspecialty. He is a scholar, charismatic teacher, and prolific writer. He is perhaps best known for his collaboration with Walsh in revising the Walsh text and coming out with a three-volume edition.

Hughes: What is the status of neuro-ophthalmology nowadays?

Cogan: It's recognized as a highly desirable specialty in an academic department. The possibility of starting out in practice outside of an academic center without outside means of support is still limited. In contrast to several decades ago, a neuro-ophthalmologist is a respected colleague and not some sort of dropout.

Hughes: Is it a subspecialty that has a strong research component?

Cogan: Decidedly so. It now attracts not only ophthalmologists and neurologists but physiologists as well. At NEI one of the largest sections is a neuro-ophthalmic research unit devoted to eye movements. A few years ago, David Hubel and Torsten Wiesel of Harvard were awarded the Nobel Prize for their researches on representation of the visual system in the occipital cortex. John Dowling's work on the retina, recently summarized in his book,

emphasizes the analogy of the eye receptive system to that of brain processes.* In the research area, there is no sharp distinction between physiology, neuroanatomy, neurology, and ophthalmology.

Hughes: Have you always been interested in the fact that the eye is a way of getting at the brain?

Cogan: In a vague sort of way. Yet I had no awareness or appreciation of its closeness until the work of Hubel and Wiesel.

Hughes: It seems to me that there is a certain pattern in your thinking in terms of the eye leading to more general applications. You saw the work in biochemistry, for example, as being applicable to diabetes in general and also to atherosclerotic processes. Similarly, you thought neuro-ophthalmology could be used as a means to discover more about brain processes.

Cogan: You suggest that I did have some preformed idea beforehand. I'm not sure I was ever conscious of that. A clinical background and natural curiosity about body and mental functions makes one ask questions. I think you're giving me credit for a prescience which I don't deserve. Thank you all the same.

Hughes: At least you had the ability to appreciate the wider applications of your findings because you definitely did make those associations later on. Whether you started out with that thesis, I suppose, is debatable.

Cogan: The fact is that we are presented with many opportunities which can lead to the inevitable ...

Hughes: I don't think it is necessarily inevitable. I don't think everyone is able to see from the immediate problem to the broader problem. I would certainly give you credit for being able to do that.

*You mentioned Lawton Smith, and I know from reading the Howe Laboratory report for 1958** that he spent a year at the Howe Lab. Did that turn out to be a profitable year?*

Cogan: At that time, I had two fellows spending a year with me, Lawton Smith from Hopkins and Andy Gay from Washington University (St. Louis).

* Dowling J. The Retina. Cambridge, MA: Bleknap Press, 1987.

** p. 7.

It was a memorable year. Anybody who knows Lawton would expect no less. He and Andy Gay would see all my neuro-ophthalmic patients before I did. This meant a thorough workup! Lawton told me once that he liked to see two neuro-ophthalmic patients a day, one in the morning and one in the afternoon. Then, in the late afternoon, he would write them up, usually filling three or four pages of single-space typewriter notes with discussion and references for each case. He established such excellent rapport with patients that they became participants in their problem, even when he introduced some of his ebullient evangelism in the process. He was also an enthralling teacher.

Of course, there were problems. We had only a small examining room in the Laboratory, which also was used by several of us when we could get in. Then there was the matter of typewriters. Lawton did all the typing himself and at such speed it was nothing less than spectacular. In his youth he had been a state champion typist. With Andy Gay's equanimity and my secretary's understanding, it was a profitable and fun year if that is not putting it too glibly.

He returned to Hopkins to finish his residency, then on to Duke, and finally to Miami where he has established himself as a leader in neuro-ophthalmology, an organizer of a popular course on the subject, and a founding editor of *Clinical Neuro-Ophthalmology*.

Hughes: Andrew Gay was a Heed Fellow assigned to neuro-ophthalmology, maybe in the same year?

Cogan: Yes. He was the other of the pair of neuro-ophthalmic fellows of that year. Fortunately, Andy Gay was a person who could go along with Lawton's dynamism without being upset. It might have been overwhelming for anyone else. We used to sit back, listen, and be amused by the energetic Lawton and learn a lot in the process. I looked upon Andy's relationship to Lawton as the catcher of a team on which Lawton was the pitcher.

Hughes: Did Gay continue in neuro-ophthalmology?

Cogan: Yes. He returned to St. Louis, coauthored a book,* and later moved to northern Maine where, I believe, he practices with emphasis on neuro-ophthalmology.

Hughes: Did he have any particular interest that year at the Howe?

* Gay AJ, et al. Eye Movement Disorders. St. Louis: Mosby, 1974.

Cogan: That was a practical training year without specializing in any one neuro-ophthalmic area.

Hughes: You alluded to the association with the Department of Neurology, but I'd like to hear more in terms of the people in the department who were particularly interested in what you were doing in the Howe, and also about the referrals which must have gone on. Am I correct in that?

Cogan: Ray Adams was chief of neurology for most of my period of activity in neuro-ophthalmology. We were good friends and had good rapport professionally, but I must say it was never as close as that which Bill Hoyt had in San Francisco with the department there. I tried to attend the weekly neurology rounds and occasionally presented a case for discussion. Dr. Adams and I published several papers together on ocular motor apraxia.*

I had perhaps closer working relations with the neuropathologists, Drs. Charles Kubik and Pierson Richardson, with both of whom I had several collaborative projects. Then, too, there was Miller Fisher, a specialist on vascular disease of the brain, with whom I studied a number of patients who had visual or ocular motor symptoms.

Hughes: Was the Department of Neurology operating through the Massachusetts General Hospital [MGH]?

Cogan: It is an integral part of MGH. The Infirmary is administratively separate from the MGH, but the two work closely together in the outpatient department.

Hughes: Harmoniously?

Cogan: Very.

Cogan: The neurologic connection was greatly enhanced when Dr. Wray joined me in 1968. Dr. Wray is an English neurologist who had trained at Queen's Square in London. She had planned, I believe, to stay with me for only a year, but the arrangement was so mutually satisfactory that I was able to persuade her to stay indefinitely—that is, for the subsequent seven years until my retirement. She then succeeded me in the Howe Laboratory until she eventually moved over to the neurology department at the Massachusetts General Hospital where she is at present.

* Cogan DG, Adams RD. A type of paralysis of conjugate gaze (ocular motor apraxia). Arch Ophthalmol 1953; 50:434-42. Cogan DG, Adams RD. Correspondence: Balint's syndrome and ocular motor apraxia. Arch Ophthalmol 1955; 53:758.

From my point of view, the close association with Dr. Wray could not have been more pleasurable and profitable. The complementary roles of ophthalmologist and neurologist serve them both and serve the patients as well. We saw each patient together, usually in the company of one or more fellows. It was a great learning experience. Dr. Wray was also an excellent organizer, much better than I. She gradually took over the organization of the postgraduate course in neuro-ophthalmology, increased the participation of her neurologic colleagues, and made it *the* outstanding teaching course in the country. Dr. Wray is now recognized as a leading authority in the field of neuro-ophthalmology; or should it be ophthalmo-neurology?

Hughes: You wrote on a number of different topics in neuro-ophthalmology. Were those papers usually provoked by patients?

Cogan: They were mostly clinical subjects involving patients with new or unexpected abnormalities. Many were collaborative studies. Such was the report with Ray Adams of an adult patient with ocular motor apraxia (I had long been interested in the congenital variety.) or the postmortem findings in a patient with unilateral internuclear ophthalmoplegia, which I reported with Charles Kubik.* Such anecdotal reports were more common than they are now, where reports of series of patients are more the custom.

Hughes: Well, of course, you did at times report series of cases. I'd like you to say a little more about congenital ocular motor apraxia, which is probably better known as Cogan's syndrome, type 2.

Cogan: I happened to see several young persons who had peculiar thrusting movements of their head when they suddenly looked at an object of attention on one or the other side. This had not been recognized as an ocular abnormality. I believe it was considered some sort of a tic. I discovered it was actually due to a defect in moving the eyes to either side on command or when the child's attention was suddenly alerted to one side. Yet random or semiautomatic movements to either side were intact. Thus it fell into the neurologic category of abnormalities called apraxia, where reflex movements are retained whereas volitional movements are impaired. The abnormality was present in the early life of my patients, so I assumed it was congenital, whence the name congenital ocular motor apraxia.

Hughes: What is its basis?

* Cogan DG, Kubik CS, Smith WL. Unilateral internuclear ophthalmoplegia. Report of eight clinical cases with one postmortem study. Arch Ophthalmol 1950; 44:783-96.

Cogan: Since the eyes cannot be rapidly turned to one side voluntarily (these are called saccadic movements), the child turns his head to fixate an object but the vestibular system counter-rolls the eyes in the direction opposite to the head rotation. The head has to overshoot the mark, causing the characteristic head thrusts. If the child is held in one's arms and rotated around a vertical axis, the eyes will deviate to the side opposite the rotation instead of developing a nystagmus. This is an easy confirmation of the syndrome.

Other aspects of the syndrome are the predominance in boys, the spontaneous improvement in the first decade of life, and the preservation of slow following movements and of normal vertical movements. It was later discovered that the syndrome sometimes occurs in siblings, occasionally in patients with Gaucher's disease, and, rarely, with midline abnormalities of the central nervous system. Usually, however, it is a benign abnormality. Its pathology is obscure. All this may sound complex, and all the foregoing signs did become evident only over a period of several years, but it is actually a simple and easily understandable entity once one understands the neurophysiology of eye movements.

I reported the first four cases in the Academy's Jackson Lecture in 1952.* Since then, many cases have been reported in the literature. I have seen some forty or more patients myself. So it is by no means a rare entity.

Hughes: How does it come about?

Cogan: Apparently there is a delay or absence in the development of the pathways which mediate voluntary horizontal eye movements. The surprising feature is its spontaneous improvement during the first decade of life and later.

*Hughes: In 1980, you wrote a paper on the course of the syndrome.***

Cogan: The paper to which you refer describes one of the patients in my original report whom we have been able to reexamine periodically from infancy to the present age of thirty-five years. This has been a documentation of the gradual improvement into adult life, with almost complete resolution of the syndrome.

* Cogan DG. A type of congenital ocular motor apraxia presenting jerky head movements. Am J Ophthalmol 1953; 36:433-41.

** Cogan DG, Chu FC, Reingold DB, Tychsen L. A long-term follow-up of congenital ocular motor apraxia. Case report. Neuro-ophthalmol 1980; 1:145-7.

Hughes: Dr. Cogan, what do you think of having the syndrome named after you?

Cogan: My thoughts on eponyms are described in an editorial I once wrote on the subject.* Ideally, a syndrome is best named in terms of the etiologic agent or the site of its defect such as enzyme abnormality or anatomic part. If this is not known, the best alternative is a name that incorporates the significant clinical features. If this, too, is impracticable or too cumbersome, an eponym is justified as a temporary expedient until the etiologic agent is identified.

In the case of the syndrome under discussion, my original designation was congenital ocular motor apraxia with jerky head thrusts but later shortened to congenital ocular motor apraxia or, as some have suggested, to COMA. I would not opt for an eponym in this case, and I believe it is being phased out.

Hughes: Who originated the eponym?

Cogan: I don't know. I would guess it began simply by referral to the syndrome which Cogan described.

Hughes: This brings up the question of Cogan's syndrome, type 1, which is nonsyphilitic interstitial keratitis.

Cogan: Nonsyphilitic interstitial keratitis and vestibuloauditory symptoms. This is a cumbersome term, but is the most succinct designation that I could think of that incorporates the entity. It might be shortened to IKVA, but that would not be meaningful to anyone who wasn't familiar with what we were talking about. So it has been dubbed Cogan's syndrome until we know more about its etiology. Present investigations of the syndrome, spearheaded by Dr. Barton Haynes at Duke University, suggest some underlying vasculitis as the etiologic factor.

Hughes: Dr. Cogan, tell me of the work behind Cogan's syndrome, type 1.

Cogan: Over a relatively short period of time, about 1945, I happened to see four patients who had a smoldering type of interstitial keratitis and who developed shortly before or afterwards a rapid onset of deafness and vestibular signs similar to Ménière's disease.** I was unfamiliar with the entity and found no

* Cogan DG. The rise and fall of eponyms. Arch Ophthalmol 1978; 96:2202-3.

** Cogan DG. Syndrome of nonsyphilitic interstitial keratitis and vestibuloaudiotry symptoms. Arch Ophthalmol 1945; 33:144-9.

description of it in the literature. Theretofore, the combination of interstitial keratitis and deafness always suggested congenital syphilis, but the type and course of the keratitis and deafness were entirely different from that in my patients, and, moreover, there was no corroborative evidence of syphilis in my patients. It was apparently a new entity, or at least one that had not been described before.

In my initial report, I described four patients, and later four more, occurring predominantly in young women.* Some years later, I reported a patient with the typical entity, except that he was a teenage male who died of a necrotizing aortitis.** This patient added to the accumulating evidence suggesting an underlying vasculitis as a cause of the syndrome. A large number of cases, more than a hundred, then were reported in the literature, culminating in a definitive review of the subject by Haynes et al. in 1980.*** Haynes, who made his initial study at NIH, is now professor of immunology and rheumatology at Duke University. He has now had personal contact with forty patients with the syndrome.

The condition is responsive to steroid therapy, but once the deafness has occurred, it is irreversible. The keratitis usually resolves with only mild impairment of vision.

*Hughes: Dr. [Harold G.] Scheie, in his oral history, referred to the Cogan-Reese syndrome.****

Cogan: That is an entity consisting of peculiar nodules on the iris in association with glaucoma. Dr. Algernon Reese and I were attending a meeting of the Verhoeff Ophthalmic Pathology Society when we discovered that each of us had an unusual specimen with this entity. Some pathologists have interpreted the nodules as nevi. Since our report, several other cases have been described in the literature, but since so little is known of the etiology, they have been designated by the eponym which you mention.

Hughes: If they are not nevi, what are they?

* Cogan DG. Nonsyphilitic interstitial keratitis with vestibuloauditory symptoms. Arch Ophthalmol 1949; 42:42-9.

** Cogan DG, Dickersin GR. Nonsyphlitic interstitial keratitis with vestibuloauditory symptoms. A case with fatal aortitis. Arch Ophthalmol 1964; 71:172-5.

*** Haynes BF, Kaiser-Kupfer MI, Mason F, Fauci AS. Cogan syndrome: studies in thirteen patients, long-term follow-up, and a review of the literature. Medicine 1980; 426-41.

****p. 225.

Cogan: I have interpreted the nodules as contraction folds on the surface of the atrophic iris.

Hughes: In the 1965 Howe Laboratory report, you mentioned Dr. Stephen Fricker who at that time was a resident. He had developed what was termed a synchronous detector for detecting signals in the eye and brain resulting from light flashes. For several years, the synchronous detector was mentioned in the Howe Lab reports.*

Cogan: These were early explorations of what is now known as visual evoked responses whereby electric signals are recorded over the occipital lobes that result from flashes of light in the eye. It is an objective way of evaluating the intactness of the visual pathways even in the presence of opacities in the ocular media which otherwise prevent the usual methods of testing. With his engineering background, Dr. Fricker was in a position to refine these electric recording devices, such as the synchronous detector, and to apply them to clinical problems.

More to the point perhaps is how Dr. Fricker came into the field and ultimately became an ophthalmologist on the staff of the Infirmary. I would like to tell you about it. It was sometime in the late 1960s when the possibly harmful effects of radar were of concern to the air force. At a meeting at the military base in Rome, New York, Dr. Fricker and I found ourselves cochairmen of a group assigned to prepare a report on the possible effects of radar on the eye, he representing the physical aspects and I the clinical aspects of radar. This brought us together for a sharing of experiences. I became as interested in his experiments at the Lincoln Laboratories of MIT [Massachusetts Institute of Technology] where he bounced radar off the aurora borealis and the moon, as he became interested in the biology of the eye. In fact, he became so interested that he left the Lincoln Laboratories and joined the Howe Laboratory for the purpose of studying electric recording devices for ophthalmology.

The problem was we did not have the space for his oversize equipment at the time. Innovative as he was, he then converted his garage in suburban Boston to a laboratory, with his wife and infant son as test subjects. Later, he moved into the Howe Laboratory as his instrumentation became smaller and as we acquired additional space, but it did not stop there. In the course of time, he decided to go to medical school; he was then in his middle thirties. This was followed by the usual internship and residency at the Infirmary and ultimate return to the Howe

* p. 8.

Laboratory as a medical staff member and expert in electric recording of visual functions and of ocular movements. He is now the chief of the strabismus clinic at the Infirmary. That's the saga of Dr. Fricker; an aberrant reply to your question about his role in the Lab.

Hughes: I was wondering specifically about his synchronous detector.

Cogan: I guess I would have to say I don't know enough about the technology to give you a clear answer.

Hughes: It didn't become a fixture of neuro-ophthalmic examination?

Cogan: Yes, it did. Visual evoked potentials and electroretinography, to which Dr. Fricker made substantial contributions, are now standard methods of examination for suitable patients.

Hughes: One of your aims in neuro-ophthalmology was to connect the clinical and experimental approaches. How successful do you think you were?

Cogan: I think I was in a position to bring persons like Dr. Fricker into the field, and I made a few contributions myself, such as the studies of the optokinetic response, but I would not rate my personal role in the technology as outstanding.

Hughes: What about treatment in terms of neuro-ophthalmology?

Cogan: Neuro-ophthalmology shares with neurology in general limited therapeutic successes. Its strength is largely diagnostic rather than therapeutic. From that point of view, it is in a privileged position. Neuro-ophthlmology has all the advantages of participating in the really exciting aspects of establishing a diagnosis without the onerous tasks that are all too often involved in treatment of neurologic conditions.

Hughes: In 1970, you helped to organize a postgraduate course in neuro-ophthalmology in the Boston area.

Cogan: I would have said I had given courses in neuro-ophthalmology considerably before 1970, but it was about that time that Dr. Shirley Wray took over the organization of the course and made it a supercourse.

Hughes: Does it continue?

Cogan: I don't think it does.

Hughes: Was the general format similar to the neuro-ophthalmology that you had taught in connection with the basic science course?

Cogan: Yes, but much expanded, with increasing use of video displays and perhaps fewer direct patient demonstrations, which were always a problem to arrange.

Neuro-Ophthalmology Papers

Hughes: Unless you have anything more of a general nature to say, I have a few neuro-ophthalmological papers to discuss. They may not be the key ones, so please feel free to add to this.

Cogan: You're very conscientious, Sally. I must add that many of your questions refer to events years ago and may strain my recall.

Hughes: Well, I hope I'm accurate. One of them is written with Charles S. Kubik, whom you mentioned before, and William L. Smith. I don't know that name.

Cogan: He was a resident at that time.

Hughes: In ophthalmology?

Cogan: In ophthalmology. In general, the residents contributed greatly in gathering cases and participating in their workup. It is important to make appropriate acknowledgement. Sometimes a resident's name is tacked onto an authorship not only by way of acknowledgement but for his future benefit. It may be the only chance he has, if he later goes into practice, of having his name on a publication.

Hughes: In this case, you are the senior author, and the title is "Unilateral Internuclear Ophthalmoplegia." I gather that the emphasis was on the unilateral because the bilateral condition was well recognized.*

Cogan: That paper illustrates the system of authorship order.** I had collected a series of patients with unilateral internuclear ophthalmoplegia, including one patient, who was the first to show the pathologic lesion in the medial longitudinal fasciculus. It allowed the significant conclusion that whereas the bilateral

* Cogan DG, Kubik CS, Smith WL. Arch Ophthalmol 1950; 44:783-96.

** See discussion p. 206.

variety was usually due to multiple sclerosis, the unilateral lesion had a vascular basis. I wrote the paper but incorporated the postmortem findings on that one case in consultation with the neuropathologist, Dr. Kubik. Dr. Smith had examined one or more of the patients with me and showed an unusual interest in the subject. The order of the authorship was obvious. I have to add, however, I did have a problem in getting the manuscript off Dr. Kubik's desk.

Hughes: He had better things to do than review papers?

Cogan: Not that. You see, Dr. Kubik was a perfectionist in editing as in his work. He always thought there might be some other aspect that he might improve. He was a model coauthor, but two years was a long time to wait for the return of the manuscript. [laughter]

Hughes: Another paper, which became your most requested as sole or single author, is entitled "Ocular Dysmetria, Flutterlike Oscillations of the Eyes, and Opsoclonus," which was published in 1954. Are you familiar with the paper by Dr. Albert in which he documents the most cited papers in ophthalmology?** Your paper was the twenty-first most cited.*

Cogan: Yes, I remember Dr. Albert reporting it. Dr. Kuwabara was the most frequently cited author.

Hughes: That's right.

Cogan: That's appropriate because Dr. Kuwabara has contributed so much to so many papers. He frequently was a junior author because he added the histologic and electron microscopic support of various studies, but his role was crucial.

Hughes: What was the subject of the Kuwabara paper that was most cited?

Cogan: It probably was the one where we reported the trypsin digestion method for making mounts of the whole retina permitting for the first time analysis of the cellular components of the capillaries.***

* Cogan DG. Arch Ophthalmol 1954; 51:318-35.

** Albert DM. Analysis of the *Archives* top-cited articles. JAMA 1988; 106:465-70.

*** Dr. Cogan is correct, the reference is: Kuwabara T, Cogan DG. Studies of retinal vascular patterns. Part I. Normal architecture. Arch Ophthalmol 1960; 64:904-11.

Hughes: I guess your main point in the paper on ocular dysmetria, eye oscillations, and opsoclonus was to distinguish the three conditions?

Cogan: That paper coincided with a renaissance in understanding cerebellar functions in general. I was merely documenting the cerebellar signs in the eye movements, which were, and those which were not, indicative of cerebellar lesions.

Hughes: You pointed out that ocular dysmetria was characteristic of cerebellar lesions but opsoclonus was not.

Cogan: That is right.

Hughes: You also stated that ocular dysmetria is frequently missed, or was at the time frequently missed. Why would that be?

Cogan: It may be a subtle manifestation and not appreciated unless one actually looks for it. It is then obvious.

Hughes: And you think this paper made ophthalmologists more alert to dysmetria?

Cogan: That was my hope.

Hughes: Then there's a paper written in 1968 entitled "Opsoclonus, Body Tremulousness, and Benign Encephalitis." Do you remember that?*

Cogan: I remember that well. It was a report of several patients who developed chaotic eye movements (opsoclonus) in association with profound truncal ataxia, and cerebrospinal fluid signs of mild encephalitis.

*Hughes: Then a paper with Robert D. Yee and John Gittinger, called "Rapid Eye Movements in Myasthenia Gravis."***

Cogan: This was a report of several patients showing unusual eye movements, which we called "quiver movements" and interpreted as being diagnostic of myasthenia gravis. We might have called them "twitch movements," but I had already attached that name

* Arch Ophthalmol 1968; 79:545-51.

** Arch Ophthalmol 1976; 94:1083-5.

to certain lid movements in myasthenia gravis.* The two may, however, be related. The lid twitch sign consists of a momentary overshoot of the lid as a person with myasthenic ptosis changes his gaze from downward to the primary position. It is also essentially pathognomonic of myasthenia.

Hughes: Is that what has been called the "Cogan lid twitch sign"?

Cogan: Yes.

Hughes: Are the two movements used as diagnostic signs?

Cogan: I have found them very useful when present.

*Hughes: Then there is an interesting paper on visuospatial dysgnosia, which was published in 1979.** The condition consists, I gather, of a disorientation or a loss of awareness of one's physical surroundings?*

Cogan: This was a report of a dozen or more patients who had organic lesions in the parieto-occipital areas of their nondominant hemispheres and who presented with visuospatial disorientation and related symptoms. It is usually associated with left hemianopia.

Hughes: You describe the case of a patient who was a professional of some type, a very articulate man. He gave a fascinating account of being able to visualize the library, yet was completely incapable of driving himself there.

Cogan: I think you may be referring to a physician who regularly had lunch at the Harvard Club not far from his office. He said he could visualize the front of the club and the entrance to his office but had lost all ability to know how to get from one place to the other.

As I remember, he had had a stroke but was perfectly able to carry out complicated investments and to participate intelligently on the university board of governors of which he was a member.

Hughes: Was he still in practice?

* Cogan DG. Myasthenia gravis. A review of the disease and a description of lid twitch as a characteristic sign. Arch Ophthalmol 1965; 74:217-21.

** Cogan DG. Visuospatial dysgnosia. Am J Ophthalmol 1979; 88:361-8.

Cogan: I don't believe so. However, I did have another patient, a surgeon, whom I did not include in the series but who had a similarly placed lesion and similar symptoms. He offered the following comment in jest. He said he could still remove a gall bladder if he could find out where it was in the abdomen. What he meant was that he would have no difficulty in recognizing an object but did not recognize its relationship in space.

Hughes: You wrote a chapter entitled "Ophthalmic Manifestations of Systemic Vascular Disease" for a book edited by Lloyd H. Smith, Jr., a name I know well because he is at UCSF. I believe one of the points that you were trying to make was that the eye is a very good means for studying the living vascular system.*

Cogan: That was a booklet, rather than a chapter, in the series that Dr. Smith was editing. Actually Dr. Smith (we called him Holly) asked me originally to write a booklet on medical ophthalmology for the series. I felt this was too broad a subject; I confined it to the vascular system.

Hughes: Were you successful in spreading the word to nonophthalmologists?

Cogan: Perhaps in a small way. To accept such an assignment is, however, rewarding in that it forces one to organize his thoughts carefully on a subject when he knows they are to be exposed in the marketplace, so to speak.

Hughes: Well, at least you could have been assured of an interdisciplinary audience.

You say in your preface, "Some chapters stress signs and symptoms while other chapters stress disease entities." Was that a fairly characteristic way for you to approach a subject? Did you do that, for example, in your neuro-ophthalmology books?

Cogan: Possibly so. I guess I was trying to accommodate both ways a reader might approach the subject. However, I can't say it was done consciously.

Hughes: In some chapters you were talking about diagnostic signs and in others you were placing emphasis on the disease per se?

* Smith HL (ed). Major Problems in Internal Medicine. Vol. 3, Philadelphia: WB Saunders, 1974.

Cogan: I imagine that a typical reader's first approach would be to find out what the signs and symptoms meant and then the disease category.

Hughes: I think you're answering my question. Apparently the approach was not a burning issue with you, one that you were trying to press in all your publications?

Cogan: I guess I did not have that missionary goal.

Hughes: When you were teaching medical students, were you aware of stressing the importance of the eye as a means of diagnosing more widespread problems?

Cogan: I took every opportunity to do just that. My aim was to have the medical students come to that conclusion on their own.

Hughes: Do you think that ophthalmology gets sufficient coverage in medical school?

Cogan: Not at all. It's been practically removed from the curriculum in favor of medicine, surgery, pediatrics, and psychiatry. That's why I started, before I left Boston, an elective course of two months in basic ophthalmology, hoping the word would get around that ophthalmology was a lot more than just prescribing glasses, that it was a really exciting part of medicine.

Hughes: Well, then a paper I heard you present at grand rounds on this subject when I was in Bethesda, entitled "The DAF Syndrome." Would you like to say something about the syndrome?*

Cogan: The name is an acronym for downgaze paralysis, ataxia of the limbs, and foam cells throughout the body. It has been better known as Niemann-Pick, type C, a name given to it before there was a means for identifying the enzyme deficit which has come to be accepted as the hallmark of the various types of Niemann-Pick [N-P] category. This type C has no such deficiency and by definition should not be included in the N-P group. Since its etiology is nevertheless unknown, I thought it should have a name giving its cardinal clinical features, whence the DAF name, until its cause was ascertained. I had the temerity to suggest such a name because the eye movement disturbances is such a prominent and early sign.

* Cogan DG, Chu FC, Bachman DM, Barranger JA. Neuro-ophthalmol 1981; 2:7-16.

There is some evidence now that an intracellular cholesterol esterifying enzyme defect underlies the condition. If corroborated, the name DAF may give way to a more meaningful term.

Hughes: You seem to have written a number of papers on conditions stemming from enzymatic defects. Would you say that is a specific interest of yours?

Cogan: This has been an exciting quarter century for identification of enzyme defects in various genetic diseases. My role has been a side issue in describing the ocular signs and symptoms.

Hughes: Would you include cystinosis?

Cogan: Yes, but I'm not sure the enzyme defect in cystinosis has yet been established.

*Hughes: One last paper, and this is related to Niemann-Pick, "Macula Halo Syndrome."**

Cogan: This is a name I gave to a condition characterized by a ringlike opacity in the back of the eye in or around the macula. I first described it in conjunction with Dr. Daniel Federman in 1964 under the title, "Retinal Involvement with Reticuloendotheliosis of Unclassified Type."** Since then, several other cases have been described and an enzyme deficiency discovered that puts it legitimately into the category of a type of Niemann-Pick disease. The paper to which you refer is based on a reevaluation of the original patient and description of another patient who came to our attention.

Hughes: Does it surprise you that it doesn't impede vision?

Cogan: That is the most extraordinary feature. Since no case has come to microscopic examination, we really don't know where the opacity is located, that is, whether it is in the retina or behind the retina.

Hughes: Has your work been used to diagnose Niemann-Pick disease?

Cogan: I think it has been of help in delineating some of the types with eye signs. [tape interruption]

* Cogan DG, Chu FC, Barranger JA, Gregg RE. Arch Ophthalmol 1983; 101:1698-1700.

** Cogan DG, Federman D. Arch Ophthalmol 1964; 489-91.

Hughes: Dr. Cogan, I just read you a list of topics in neuro-ophthalmology that I picked out of the Howe Lab reports, twenty-five conditions that you have examined. I'm sure there were more as well. Is there anything you care to say in summary?

Cogan: Your list cites various neuro-ophthalmic activities in the Howe Laboratory through 1972. It does not include projects continued at NEI.

Charles L. Schepens

[Interview 6: August 7, 1989]

Hughes: Dr. Cogan, I thought we'd talk about Dr. Schepens, who came into your life right after the war. Maybe you'd start out with telling me how he got to Boston.

Cogan: The Schepens saga is an interesting story, about which I have mixed feelings. It would make a grand novel in the Marquand style. It might begin with the dramatic escape from the Nazis over the Pyrenees into Spain, with a family of four children, and thence into England. The key figure would be an ophthalmologist with a mission to develop a self-luminous binocular ophthalmoscope for use in retinal detachment surgery, not in England, but in America, where ophthalmologists still depended on outmoded direct ophthalmoscopy in the operating room.

With an attractive personality, a noble goal, and an allegedly great respect for the Howe Laboratory, Schepens had no difficulty in persuading us to grant him a fellowship in the Howe Laboratory. There he succeeded in developing his instrument and assisting retinal surgeons in the workup and surgery of their patients. Once his reputation was established, he developed his own clientele and cornered the market, so to speak, with a businesslike strategy. After two years of the fellowship, it was obvious that the Howe Laboratory had served his purpose. He, therefore, moved on to head up a retina service under the Infirmary's auspices and, somewhat later, an independent organization of his own under the name of Retina Foundation. With a staff and clientele of his own, he and the Foundation prospered. His ophthalmoscope, manufactured by the American Optical Company, became standard equipment.

Hughes: Had he actually originated binocular indirect ophthalmoscopy?

Cogan: I know there is some question about that, but, from our point of view, he is the one who brought it to Boston.*

Hughes: What is your impression of his surgical skill and judgment?

Cogan: I believe he was a good surgeon. Moreover, he and his colleagues introduced several innovative methods for treating retinal detachment.

Hughes: Even after he'd set up the Retina Foundation, he continued to see patients at the Infirmary?

Cogan: He'd see private patients in his offices at the Foundation but used the Infirmary for his surgery.

Hughes: How long did that continue?

Cogan: Indefinitely.

Hughes: When Schepens created the Retina Foundation, he made Paul Boeder chairman of his board. Do you know anything about his thinking in making that appointment?

Cogan: I think I do. Before Schepens appeared on the scene, I had arranged for the American Optical Company to contribute annually $6,000 to the Howe Laboratory. It was my first successful acquisition of outside support for the Laboratory, and I was proud of it. I do not know what went on behind the scenes, but the company soon withdrew its support to the Howe Laboratory in favor of supporting the Retina Foundation. Boeder was then head of research at American Optical, and I assume his appointment to Schepens' board had something to do with this transfer of support from us to the Retina Foundation.

Hughes: Did American Optical profit by the association?

Cogan: I presume they thought they might. I'm not criticizing the company, that's business. I just thought it was mischievous for Schepens to use in this way the organization that had given him his start.

On the other hand, one has to admire his courage and initiative and conclude that he contributed substantially toward establishing the modern methods for examining and treating

* For more on Charles Schepens and the development of the binocular indirect ophthalmoscope, see the oral history in this series with Paul Boeder.

retinal disease. At the same time, one has to admit his methods strained what was considered at that time to be questionable tactics. They inevitably led to the detriment of the Howe Laboratory and the Infirmary and split the loyalties of the staff. Among others, I was disappointed, but in retrospect, I can now see it was merely the avant garde of what is happening to medicine in general. We are now living with advertising, fund-raising tactics, and conflicts of interest that would have been unacceptable in the era of years passed. I would say Schepens was ahead of the times. I found it disillusioning and stressful.

Hughes: What difference do you think it made that Dr. Schepens as a personality didn't fit the image of the conservative Bostonian or New England physician?

Cogan: Perhaps your implication is correct, but I doubt it because I received protests about his public relations from other parts of the country and even from his native Belgium.

Hughes: A quote from Henry Beecher and his history of the Harvard Medical School, a quote which I understand is actually from Dr. David Cogan, "Perhaps more than any other development, his [Schepens'] efforts signalized the end of ophthalmology as a single specialty and its beginning as a collection of integrated subspecialties." You're referring to the retina service or the Retina Foundation?*

Cogan: I can understand your confusion. This refers to the retina service, which Dr. Schepens organized in the Infirmary in 1948 at the suggestion of Dr. Dunphy and me. It is said to have been the first such service in this country.** It certainly did not refer to the Retina Foundation, founded in 1950,*** which was physically and administratively separate from the Infirmary and from the Harvard Medical School.

Hughes: Do you think the recognition of subspecialties within ophthalmology is a necessary trend?

Cogan: It applies to many subspecialty services as a result of increasing knowledge and technology. I see no alternative.

* Beecher HK. p. 443. Dr. Cogan wrote the section on ophthalmology in Beecher's history.

** Typescript, March 9, 1949. The Howe Laboratory of Ophthalmology. (AC51) Abraham Pollen Archives and Rare Book Library, Massachusetts Eye and Ear Infirmary, Boston, MA. Hereafter referred to as Pollen Archives, MEEI.

*** Francis S. Hill to Walter A. Rosenblith, January 2, 1968. MEEI President's Office. (AC49) Pollen Archives, MEEI.

Dr. Cogan's Simultaneous Positions

Hughes: In 1962, you became simultaneously Henry Willard Williams Professor of Ophthalmology, head of the Department of Ophthalmology at Harvard, chief of ophthalmology at the Infirmary, and director of the Howe Laboratory. Well, you had been director of the Howe Laboratory since 1940. How successful did you find this unification in one person of these posts?

Cogan: I did not like it. It came about when Dr. Dunphy had to retire at the age of sixty-five. I was on the search committee for a successor, which is usually a guarantee that you will not be appointed. My conviction was, and is, that the clinical chief should not be simultaneously the director of research. But the committee was unable to find a suitable candidate and asked me to take on the chiefship temporarily while the search continued. I objected but accepted insofar as it was to be temporary. That "temporary" continued for six and more years.

Although I had a very able deputy in Dr. Carl Johnson, the new position nevertheless took time and thought away from my research responsibilities, and I do not think it was a good idea. The worst part of it was that it set a pattern for successors to the chiefship when I ultimately retired from Boston. Until then, I had not realized that in accepting the position, even though supposedly temporary, I was undoing the uniqueness of the double type of administration which we had in the Infirmary–Howe Laboratory arrangements. If I had realized this at the time, I would never have accepted the position under any condition.

Subsequent appointees have insisted on combining the two positions with the result that the Howe Laboratory's integrity, and Dr. Howe's wishes, have been violated.* After several ultimata to the dean, I finally succeeded in resigning from the chiefship at the Infirmary but offered to continue as departmental chairman at Harvard. I thought the strictly teaching and research affairs could well be done under the aegis of the Howe Laboratory so that the chief of the Infirmary would be free of the purely academic responsibilities to concentrate on the demanding Infirmary functions. This possibility was somewhat unorthodox and was turned down. I was actually

* The independence of the Howe Laboratory, so important to Dr. Cogan, has recently been restored with the appointment of a separate director. (Personal communication from Dr. Cogan, February 6, 1990.)

pleased it was not accepted, but it might have been an interesting adventure in academic organization. That was 1968.

Hughes: Do you think that the reason for turning down the offer, namely, that it was against the historical precedent, was really the full story?

Cogan: No. I suspect the directorship of the Howe Laboratory was to be used as bait for recruiting a new chairman of the department and chief at the Infirmary.

Hughes: Did your optimism about the future of the Howe Laboratory turn out to be warranted?

Cogan: I think the status of the Howe Laboratory has been downgraded as a result by not having a separate director whose primary function is administration and support for the Laboratory. After all, the clinical chief's primary responsibility has to be to the hospital affairs. In anticipation of this subjugation of the [Howe] Laboratory, most of the senior staff of the Laboratory moved to Bethesda on my upcoming retirement [1973].

Hughes: What about town-gown problems during this period?

Cogan: This is omnipresent in Boston, but I was spared many of the problems because everyone knew I was in a volunteer position and did not want to continue as chief at the Infirmary. I did have a confrontation with some members of the staff when I set up a small consultation service at the Infirmary for private patients, staffed by members of the Howe Laboratory. I thought it served a useful purpose for patients, who would otherwise be turned away, since they were above the means test. It also benefited the reputation of the hospital, and partly supported those who staffed it. Since it raised issues that did not seem to warrant exhaustive and exhausting confrontations, it was discontinued.

Hughes: I would like to quote from a document entitled, "The Department of Ophthalmology at the Harvard Medical School and the Massachusetts Eye and Ear Infirmary: Organization and Development." It's undated and unsigned, but I believe it dates about 1973. Do you recognize this document?*

[Pause while Dr. Cogan reads the document.]

* Massachusetts Eye and Ear Infirmary President's Office. (AC49) Pollen Archives, MEEI.

Cogan: No. I've never seen it before. As you point out, it is unsigned but there is a note in the upper corner indicating it was directed to "CTW" who was the hospital administrator. It is a thirteen-page typescript full of pious generalities that have little substance. It reads like a political candidate's barrage of promises for utopia under his jurisdiction. I suspect the windmill being attacked was my recommendation that the Laboratory maintain its separate identity and not be placed under the clinical chief. It reflects a belief that research is a luxury rather than a serious concern.

Hughes: One of the arguments I heard put forward against the independent status of the Howe Laboratory was that the funding situation changed when it shifted to NIH. NIH looked askance at laboratories which were independent of the larger organization, in this case the Harvard department. Did you ever feel that your chances for support from NIH were jeopardized by the fact that the Howe Laboratory was not under the department?

Cogan: I would have thought just the opposite. The Howe Laboratory was, of course, under the larger framework of the medical school, and NIH has always favored research rather than direct support of clinical or even training functions. I had operational contact with NIH since the middle 1950s, and the Howe Lab had a high rating for support from NIH. A peer review rating solicited by Research to Prevent Blindness regularly put the Howe Laboratory as number one or number two on its list of fifty or more institutions in this country as having optimal research potential [in ophthalmology].

Hughes: What was the basis for the rating?

Cogan: A rating determined by peer reviews.

Hughes: A number of people believe, and it's indeed my impression, that one of the reasons for the great strength of the Howe Laboratory from the forties into the seventies, that is, during the Cogan reign, had a lot to do with you yourself, and also with the relationship, which you have mentioned in these interviews, with Dr. Dunphy. It was a unique circumstance, and, of course, in that equation should be mentioned superior investigators—Dr. Kinoshita, Dr. Kuwabara, and others.

Cogan: You properly point out the favorable circumstance. I would like to emphasize again the separate and equal roles that Dr. Dunphy and I played, he in charge of clinical management and I in charge of research operation. We each had our own responsibilities in fund raising, policy, and strategy for our own division, and each

was glad that he didn't have to wrestle with the other's problems. It was a system that was favorable.

Hughes: Mrs. Cogan, since you are listening in, let me ask you how much you were drawn into the affairs of the Howe Laboratory?

Mrs. Cogan: I was very much interested and shared Dave's conviction that he should not be chief at the Infirmary. Every time I saw Dean Ebert I would remind him of that. I would say, "You know, I don't see why you take people who are interested in research and put them in a position as clinical administrator. You're wasting their talents." I saw them doing that with Dave when they insisted he become chief of the Infirmary and chairman of the Department of Ophthalmology. I wasn't a bit in favor of it.

Hughes: Did it in fact hurt his research?

Mrs. Cogan: Jin Kinoshita once made the comment to me that the Howe Laboratory was not the same while Dave was the Infirmary chief. He didn't have as much time to attend the brown bag lunches; conversations drifted into discussions of baseball games and other nonophthalmologic subjects.

Hughes: Dr. Cogan, is it true that you didn't have sufficient time for the affairs of the Howe Laboratory when you held those several positions?

Cogan: I felt like a bigamist must feel who has to divide his time between a couple of wives. I don't recommend it. I'm sure many junior staff members with a research interest must feel that way now when hospital demands for them to see many patients give them little time to do research in the laboratory.

Hughes: What do you think the solution is?

Cogan: To have research divisions staffed by persons who are not overwhelmed by clinical obligations.

Mrs. Cogan: That's rather an extreme statement. There should be overlapping activities in the clinic.

Cogan: Yes, but not their primary responsibility.

Mrs. Cogan: Didn't Morton Grant see many patients?

Cogan: As an Infirmary staff member, he served in the public eye clinic one half day a week and saw many patients in the glaucoma clinic which was part of his research activities. In fact, he was ultimately on the board of surgeons. I shouldn't give the impression that a person in research should divorce himself from the clinic. In fact, quite the contrary. I don't think his clinical duties should encroach too heavily on his time for research.

Hughes: What is the function of the board of surgeons as opposed to the board of managers, which is now called the board of trustees?

Cogan: The board of surgeons at the Infirmary comprised senior clinicians who discussed such clinical matters as resident appointments, and policy matters that affected the clinical operations. The trustees, on the other hand, comprised chiefly lay people who were concerned with the financial affairs and public relations.

Hughes: How effective do you think you were as an administrator?

Cogan: I would not rate myself very high.

Hughes: Did you have any ideas that you wished to implement when you came in as chairman and chief?

Cogan: I had been brought up to have a lot of respect for the Infirmary, and I wanted to repay a sort of debt for what it had done for me. I don't know that I had any specific plans other than to help find a successor as soon as possible. I did, however, think some full-time clinical staff was going to be necessary, for which the small consultation clinic was a trial balloon.

Hughes: Dr. Kinoshita credits you with changing ophthalmology from a position subservient to the Department of Surgery to a full-time academic department. Would you comment on that?*

Cogan: I think that reflects Jin Kinoshita's magnanimity rather than the facts in the case.

* Telephone interview with Dr. Jin Kinoshita, July 5, 1989.

More on the Howe Laboratory

Hughes: Was it deliberate on your part as director that very little of the Howe Laboratory endeavor was devoted to surgical research? Or was it that the configuration of personalities wasn't particularly interested in surgical research? The only exception that I can think of is Dr. Schepens.

Cogan: Sally, I think I made the point somewhere in my annual reports that it is important to appoint a person of promise and let him develop his ideas as he sees fit. It just so happened that more persons who joined the Howe Lab were interested in the medical aspects and the basic sciences than in surgery. I guess that is true of research in general. I suppose what is called a good administrator or a good manager would have set a pattern and picked persons to fit into that pattern. It was the other way around with me.

Hughes: Did the very fact that the Howe Laboratory was doing basic research serve as a thorn in the side of the clinical staff, and did the clinically oriented people have trouble recognizing the importance of the Howe Laboratory endeavors?

Cogan: I don't recall any antagonism on that score. In fact, I think the clinicians thought it was nice to have a research program going at the Infirmary even though they might not participate in its operation. It was no expense to them except insofar as they made voluntary contributions to the Lab, and I think they appreciated the service functions that we performed for them and for the Infirmary in the form of photography, glaucoma studies, neuro-ophthalmic consultations, and presentations of relevant research at various meetings. No, I think it was a salubrious relation of the Howe Lab with the great majority of the staff. Some of them, like Dr. Chandler, even developed a close participating relationship.

Hughes: Dr. Kinoshita pointed out that members of the Howe Laboratory had received the two major awards in research sponsored by the Association for Research in Vision and Ophthalmology. The Proctor Award was awarded to you, Dr. Grant, Dr. Kinsey, and Dr. Spector. Both the Proctor and the Friedenwald awards had been received by Drs. Kuwabara, Futterman, and Kinoshita. This*

* Telephone interview, July 5, 1989.

seems to me to be an astounding number of prestigious awards in a small laboratory. Other than just the prestige and the pleasure of receiving such awards, did it make any tangible difference in the future of the Laboratory?

Cogan: Of course, it added to the prestige of the Laboratory and encouraged basic scientists to work in clinically related laboratories. I was proud of the recipients.

Hughes: In your 1967 Howe Laboratory report, you spoke of seeking the means to establish a research professorship, which is a theme for many years in the Howe Laboratory reports. How successful were you?*

Cogan: I was anxious to have an additional tenure position in the Howe Laboratory. Tenure positions, that is, having the rank of associate professor or full professor, depended on the size of the endowment. Thanks to the intercession of a former fellow in the Howe Laboratory, Dr. John Carroll, we were able to obtain funds for a professorship from the Scaife family of Pittsburgh. This is what became the Cogan Professorship. It was formerly held by Dr. Grant and is now held by Dr. Daniel Albert.

Hughes: Say a little more, if you don't mind, about your philosophy of collaborating in research. What role did you play?

Cogan: Each collaboration was an ad hoc affair. With problems in radiation, we collaborated with physicists at MIT or Harvard. Dr. Donaldson collaborated with Dr. Edgerton of MIT in matters of camera construction. I collaborated with the neurological colleagues at MGH. I assume you made reference to collaboration outside the Howe Laboratory.

Hughes: No, I wasn't.

Cogan: Within the Laboratory or Infirmary, collaboration was the natural expression of common interests. Persons working on similar problems were often parts of a project team.

Hughes: What I was trying to get at is, when you were collaborating with, for example, Dr. Kuwabara, was one of you generating the ideas and the other doing the work? Was there any pattern that you can identify?

* p. 21

Cogan: There was no simple routine. Many of our intramural collaborative undertakings arose from discussions at our brown bag luncheons. Collaborative projects arose informally. It is hard to say where ideas come from. In the case of Dr. Kuwabara and me, they often reflected the combined input of a pathologist and a clinician. I did not realize at the time how important those brown bag sessions were. I have tried to introduce something similar at NEI, but the size of the latter has made it difficult to develop such cross fertilization. There is advantage in smallness.

Hughes: What emphasis did you place on attracting young people into a career in medicine?

Cogan: Our contacts were almost entirely with persons who had already committed themselves to medicine or biologic research. Our efforts then were to share with them the enthusiasm we had for what we were doing.

Hughes: There was a period, I understand, when many of the potential residents in ophthalmology chose to spend a year in the Howe Laboratory before they went into their residencies. How did you handle those young people coming into the Laboratory?

Cogan: They would get a rundown on the various activities in the Howe Laboratory and then choose the investigator with whom they wished to work. It was their choice and the investigators' consent. I don't think we tried to direct them into any one area.

Hughes: Did you spend time with them? You hear, particularly on the European scene in the past, that the professor was a very distant individual who really had very little personal contact with anybody in his department. I'm suspecting that was not the case in the Howe Laboratory.

Cogan: Nor is it so true in purely research or basic science departments. In clinical departments, the head of service is kept so busy with running the hospital that he doesn't have the time to spend with his junior staff. This is one of the reasons I think it is important to have a full-time research person in charge of research.

Hughes: And the time available.

Cogan: An investigator needs to have someone in an authoritative position who will listen to him, criticize his project if necessary or encourage him if appropriate, and show an interest in what he is doing.

Hughes: In 1971, you mentioned an executive committee formed "to coordinate the Infirmary's research activities with those of the Howe Laboratory." Dr. Grant was appointed overall research administrator for the Infirmary. Was that an important committee?*

Cogan: It was an attempt to bring all the research activities under one administrative head. It might have been an important committee, but like an earlier appointment when I was made director of all the Infirmary's laboratories, it carried no authority with it and did not amount to much.

Hughes: Why was the Retinitis Pigmentosa Laboratory separate from the Howe?

Cogan: I thought it should have been part of the Laboratory, but the donors and the recipients apparently felt that they had greater independence and prestige by having their own separately endowed and named laboratory. I was not consulted.

Hughes: Why would anybody agree to set it up independently?

Cogan: Because a considerable sum of money was available.

Hughes: Presumably the money could have been funneled through the Howe Laboratory.

Cogan: I would have thought so. I can understand a benefactor wanting to maintain his separate identity by having a separate organization. Although I opposed its separateness in an attempt to prevent fractionation of research activities at the Infirmary, I should add that it is an ongoing and productive research laboratory.

*Hughes: I read that in 1971, Dr. Grant refused to participate in an NIH training grant renewal.** Is there a story behind that?*

Cogan: If my memory serves me correctly he became fed up with the paperwork and bureaucracy.

Hughes: Was the training grant indeed renewed in 1971?

---

* Howe Laboratory Report, 1971, p.1.

** Henry F. Allen to W. Morton Grant, May 11, 1971. MEEI President's Office. (AC49) Pollen Archives, MEEI.

Cogan: I don't recall.

Hughes: Had the training grant been previously awarded to the Howe Laboratory?

Cogan: Yes. I believe we were one of the first in the eye field to be awarded a training grant from NIH. I believe it was administered by Dr. Kupfer when he was in the Laboratory. It was a very useful source of support for fellows and those temporarily attached to the Laboratory.

Hughes: Could you use it for individuals who came to the Lab just before their residency?

Cogan: Yes, if they had research potential. It was very useful for that.

Hughes: Was the fellow who received the support able to train in any subspecialty that he cared to?

Cogan: It was necessary that he find a preceptor among the staff who would accept him. Persons like [Leo T.] Chylack, [Richard F.] Brubaker, and [David M.] Worthen are examples of former trainees who have come to occupy prominent positions in ophthalmology. The training grant had a flexibility that is not available in most types of grant support.

Hughes: How did you feel about Dr. Claes H. Dohlman succeeding Dr. Allen as director of the Howe Laboratory, chief of ophthalmic services, and departmental chairman?

Cogan: Dr. Dohlman is an impressive and gracious person. He had been an effective head of the cornea clinic. I was disappointed that I could not convince him of reinstating the dual appointments of having equal and separate persons for the clinical and research operations, but then I was soon to leave Boston and felt it was none of my business. I did feel the cornea service needed him more than the chiefship did. As you may know, the personnel of this clinic have been charged with unethical conflicts of interest recently. I doubt that would have been true if Dr. Dohlman had continued as the clinic's chief. Withal, I have had most cordial personal relationships with Dr. Dohlman during his tenure as chief of the ophthalmic service. He has recently resigned from the positions which he held, although he is still a relatively young person.

Hughes: Do you know his reasons?

Cogan: I do not, but I do know there was a considerable controversy in the traditional town-gown relations at the Infirmary. I think the Howe Laboratory suffered by not having a director who had the time to spend with the investigators.

Hughes: Do you think there's a chance of the Howe Laboratory being restored to an independent status?

Cogan: Only if some legal action were taken showing that Harvard had not fulfilled the stipulations of the founder's intent.

Short of that, it is encouraging that the new chief of ophthalmology and director of the Howe Laboratory, Dr. Fred Jakobiec, is a basic investigator. At the moment, it looks like a major center for ophthalmic pathology is in the offing.

Hughes: Did you have a role in his appointment?

Cogan: I was consulted.

Hughes: Do you attribute the recent decline of the Howe Laboratory strictly to the Laboratory's lack of independence?

Cogan: It needs a full-time director whose prime concern is for research and who has the time and inclination to meet regularly with the staff. Most of all, it needs independence to probe methods of furthering its aims with minimal extraneous restraints. At least, this was the formula that was successful in the Laboratory's heyday.

Hughes: Do you think it would have been an advantage for the Howe Laboratory to have had a specific mission along the line, for example, of the Proctor Foundation with its emphasis on external eye disease?

Cogan: I can't answer that, Sally. You have now been mixed up in both laboratories. What do you think?

Hughes: Dr. Thygeson has ideas about future financial support and continuing the mission. If indeed those two things happen, particularly the independent financial support, then I think it has a good chance. However, it's a complicated problem. The Proctor, even though it has a similar structure to the Howe in its direct line to the chancellor rather than to the department, nonetheless is very much influenced by the circumstances of the medical center of which it is a part. There's a new dean coming in and a new

chancellor soon to be on the way. Who's to say what their attitude will be towards an independent laboratory?

Cogan: In retrospect, I believe it would be wise to have a board to help in maintaining the Laboratory's independence, but until this was threatened at the time of my prospective retirement, I didn't feel the need for it.

Hughes: The Proctor does have a board that consists of the director of the Proctor Foundation, the department chairman, and the dean of the medical school. They don't always have unity; that's been one of the problems.

Cogan: One of the mistakes we made, and perhaps it is true for the Proctor, was that we did not have an outside board. There was no member of the Howe family available or other party representing the Howe interests. It is too bad because it was a unique system that guaranteed an enviable stature for ophthalmic research.

Hughes: What about Dr. Grant?

Cogan: Dr. Grant resigned some years ago but continued for some time visiting the Infirmary once a week or so. He tells me he is working on the fifth edition of his book on toxicology at home.

Hughes: The importance of the Howe Laboratory reports has been implicit in our discussions, and I certainly would underline that they have been absolutely crucial to my preparations for these interviews. Did you enjoy writing them?

Cogan: I can't say I enjoyed writing them, but I enjoyed having written them. My concerns were that I was giving appropriate reference to the contributions of each staff member, that I was describing what seemed to be important in terms that were understandable to laymen but still not overly simplistic for more sophisticated readers, and that I properly acknowledged *all* those who had helped us financially. I wanted them to be historical records, but, frankly, they were appeals for private funding.

Hughes: Do you think they were indeed instrumental in fund raising?

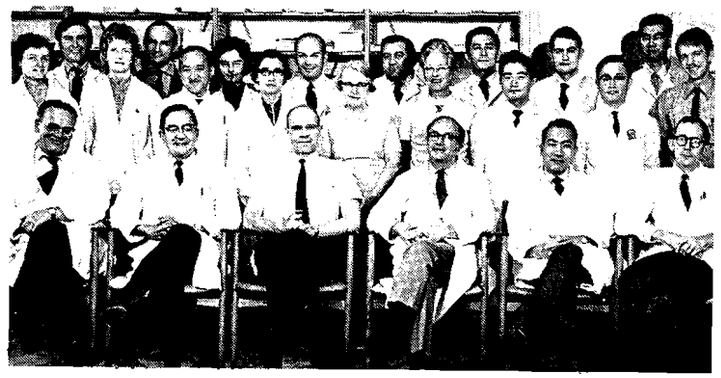
Cogan: I'm sure they were. As you can see from the acknowledgements, our outside support increased markedly with the years. The reports were our instruments of public relations, our substitute for fund-raising campaigns. It was because of the outside support that we were able to expand our annual activities and to more than double our endowment.

Hughes: I wonder if the new director could be convinced to reinstate the Howe Laboratory reports?

Cogan: The support of the Howe Laboratory will undoubtedly be acknowledged in the annual reports of the Infirmary, but so long as the Laboratory is no longer a separate and independent department, its contributions, scientific and financial, will not have the separate identity which was represented in the reports to which you refer. The last report was 1972.

Hughes: What, then, happened to the private support to the Laboratory?

Cogan: So far as I know, it doesn't exist.



Howe Laboratory staff,
c. 1970

Hughes: Is there anything in summary that you care to say about the Howe Laboratory?

Cogan: Well, not exactly in summary. I would simply like to reiterate that the Howe Laboratory was a unique organization that gave stature to research at the Infirmary and the Harvard Medical School. It was able to play a significant role in that tripartite aim of patient care, teaching, and research. It also served well the goals of enthusiastic investigators who would probably not otherwise have been brought into the field. I believe it has left a heritage and funds that will continue to the common good. The fact that it no longer has a separate director and has thereby

been submerged into a larger framework saddens those of us who remember its halcyon days.* It shares the fate of other Harvard departments like the Huntington Cancer Center and the Thorndike Memorial Laboratories that started out as vigorous, independent entities that were ultimately submerged in a hospital administration. I suppose laboratories are like people, they come and go and leave their mark. They serve a useful purpose and then pass on.

Hughes: Well, we hope that's not the case with the Howe.

* Subsequent to this interview a separate director of the Howe Laboratory, Dr. Fred Jakobiec, was named.

III. MISCELLANEOUS TOPICS

Chief, Neuro-Ophthalmic Section, 1973–1985, and Senior Medical Officer, 1985–present, National Eye Institute

The Move to Bethesda

[Interview 7: August 8, 1989]*

Hughes: You were not the first of the Howe Laboratory to go to Bethesda. Would you tell me, please, who went and what the circumstances and arrangements were?

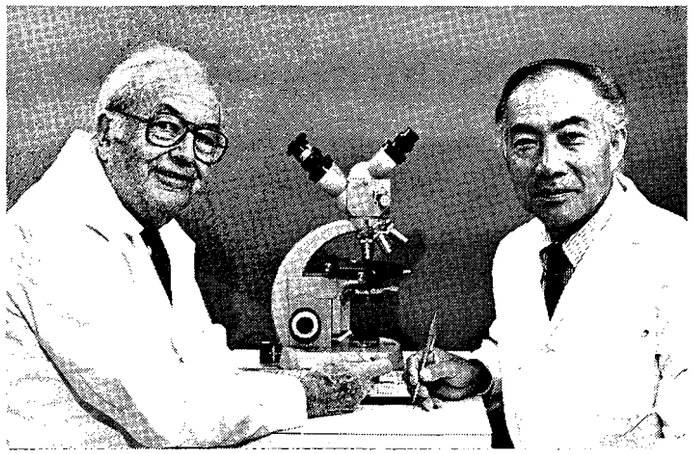
Cogan: The National Eye Institute [NEI] became a reality in 1970, and Dr. Kupfer was appointed its first director. You will recall that Dr. Kupfer was in the Howe Laboratory for seven years before he went to Seattle to be chairman of the department there. His task at NEI was to build up an intramural as well as an extramural program. Accordingly, he invited the core group at Howe to move to Bethesda, there to be a part of the nucleus of the intramural program. As we have related, it coincided in time with our disillusionment over the Howe Laboratory's future. Thus Drs. Kinoshita, Kuwabara, Chader, Fukui, and Mr. Merola left for Bethesda as a group, where they came to be known as the Howe Laboratory, South. Dr. Kinoshita was appointed chief of the Laboratory of Vision Research.

* For better chronology, portions of interview 4, recorded on May 28, 1989, have been incorporated here.

I had planned to stay in Boston at least until my mandatory retirement at age sixty-five, two years hence [1973]. But when the dean told me I could take a sabbatical year, even though it was my last year at the Howe, I, too, packed up for Bethesda for what was to be a trial period. The dean and I differed on what we thought should be the future of the Howe Laboratory, and he may have been glad to get rid of me.

Hughes: What were you prepared to do?

Cogan: I planned in Bethesda to continue my pathology studies with Dr. Kuwabara. Once there, I found a greater need for development of neuro-ophthalmology. Thus I postponed my planned full-time participation in pathology, and with Dr. Kupfer's concurrence, set up a section of neuro-ophthalmology.



Drs. Cogan and Kuwabara, National Eye Institute

Hughes: Were you seeing patients?

Cogan: Yes. It became a very active service with a fellowship program and close association with the neurologic institute, especially with Dr. David Zee. Several of my former students in Boston became fellows with me, notably Drs. John Gittinger and Robert Yee, who are now departmental chairmen at their local universities. One fellow, Dr. Fred Chu, came for the anticipated two-year period and stayed seven. All in all, it was a very pleasant, privileged, and productive period for me. I, too, stayed on longer than I had anticipated and am still there, sixteen years later.

Hughes: Was the setup similar to that at the Howe?

Cogan: Yes. It was the same system but in an expanded form. I now had excellent video facilities for documenting study cases, I had closer relationships with the neurologic departments, and, best of all, it was Dr. Kupfer, rather than I, who had the budgetary worries.

Hughes: Were the closer relations with neurologists a matter of personalities?

Cogan: No. It was rather physical proximity of our activities and also the fact that so many of the patients were referred for study purposes.

Hughes: Please compare and contrast the National Eye Institute with the Boston environment.

Cogan: At NEI we gained some things and lost some things compared with Boston. We gained access to a wide range of medical conditions that were being studied in depth. We had the opportunity to probe diagnoses and forms of therapy under controlled conditions, often with in-house hospitalization. I should add that the attitude of most of the patients was also a plus factor. The patients had to be referred to NIH by their private doctors who had as much as said to them, "We cannot help you further; perhaps NIH will accept you for study." Since, once accepted, the patients did not pay for these services, they seemed grateful for what little we might add. It was a pleasant and relaxed relationship of doctors and patients.

On the other hand, NIH differs from a university setting in that there is a paucity of undergraduate teaching. Those of us who liked teaching missed the contacts with medical students. Also, the insistence on protocol approval for any study was a lengthy and time-consuming procedure in comparison with the Boston experience as was the commitment to ordering procedures. It was not unusual for an order to be placed but delivery take place so much later that the person ordering the item had already left the institutes. All of this seems to be inherent in the load of government regulations.

Perhaps the greatest disadvantage at NEI was the separation of the basic scientists from the clinicians by being in separate buildings. Since NEI arrived late on the scene, all the space in the [Warren Grant Magnuson] Clinical Center had been allocated to other institutes. NEI had to take what space it could get. The basic researchers were assigned to a building some distance

away. This resulted in a dichotomy of the clinical and basic science personnel.

Hughes: I believe in your particular case it was left up to you what you did in terms of research. Is that true of NIH as a whole?

Cogan: I was in a privileged position and cannot speak for NIH as a whole. I looked upon my activities, both in neuro-ophthalmology and pathology, more as service functions rather than primarily research functions. Thus they did not require the strict adherence to protocols of experimental projects.

Hughes: Does that mean protocols that have been approved by an ethics committee?

Cogan: Yes. I was bound by a code of ethics for dealing with patients but not the scrutiny which is necessary for use of new drugs or other therapeutic means.

Hughes: Were you limited to neuro-ophthalmology?

Cogan: No. Having had wide experience in general ophthalmology, I was often asked to participate in other studies as well.

Hughes: I'm sure it had something to do with your seniority and standing in the field, too.

Cogan: I could shift between neuro-ophthalmology and pathology freely.

Hughes: How were the other Howe Laboratory people reacting to the new environment?

Cogan: They seemed happy in continuing the work they had begun in Boston. Being basic scientists, they were not as restrained by protocols as were those working in the clinical fields.

Hughes: Did they find the research environment as pleasant as they had at the Howe?

Cogan: If you asked them, I believe they would reply in the affirmative while citing some of the disadvantages.

Hughes: Do other institutes combine research and clinical work in the same building?

Cogan: I believe most of the institutes which were in place before NEI came on the scene have more research space in the clinical center.

Hughes: Does the administration recognize the virtue of having clinical and research facilities in proximity?

Cogan: They recognize it but are under pressure from all the present institutes and formation of new institutes. I proposed to Dr. James Wyngaarden, head of NIH, that we bid for the convent property (which came on the market) to house an integrated eye unit. He was at first favorable to the idea, but we were outbid by the Cancer Institute, and they in turn were outbid by the Hughes Foundation which has now converted it into a novel teaching facility. The ultimate solution for an integrated NEI would still seem to be a single building for housing its clinical and basic sciences.

Hughes: Did Dr. Grant consider going to Washington?

Cogan: He remained in the Howe Laboratory for a while and was active on the Infirmary staff and then retired after a few years. He did return once a week or so, but gave this up and more or less severed his contacts with ophthalmology until recently. In the past year or so he was prompted by Dr. Fred Jakobiec to prepare another edition of his *Toxicology*. This he is doing largely at home. He has declined to let me explore the possibility of getting him a Fogarty Fellowship at NIH where he could work on his book and participate in the many ophthalmic studies that are in progress at NIH.

Hughes: It seems an ideal place to write his book.

Cogan: He would have unsurpassed library facilities and a wealth of interested colleagues.

Hughes: Do you think he will be enticed eventually?

Cogan: No. The last word I had from him was to the effect that he liked it at home, which he spelled with a capital H.

Living in the Washington Area

Hughes: Your roots in the Boston area are pretty deep. On a personal level, how was it to change your place of residence?

Cogan: After so many of the Howe group left for Bethesda, Boston wasn't the same for me and time was too short for me to start building a new group. It seemed logical from a professional point of view to

join them. This is what I did as soon as I could put my affairs in order and arrange for some continuity for those who remained.

My social life, of course, changed a lot, but that has never been very important to me. My concern was, of course, for my wife, for whom the move would have been much more disruptive. Then, too, I was not sure how it would work out for me in Bethesda. The decision was for me to go there alone for a while, live on the campus, and travel between home and Bethesda frequently. I did that for some months, then my wife came down for a temporary stay. She found it tolerable, and we eventually bought our present house in Chevy Chase. The Washington area is barely tolerable in summer, so my wife spends two or three months up here in Michigan while I commute several times during the summer months.

Hughes: *Had she not gone to Michigan for long periods in the summer when you were living in Boston?*

Cogan: Perhaps not for such long periods. The move to Bethesda was much harder for her than for me. She is still nostalgic about Boston. In fact, we looked at apartments in the Cambridge area a couple of years ago with a move in mind but did not follow up on it.

Hughes: *Dr. Friedman told me that there had been talk of you returning to Boston.**

Cogan: Well, we found the apartment we wanted. It was on the Charles River, near Harvard Square and the Widener Library, and easily accessible to Boston. Just what we wanted, except the price. We had been exposed to the "Rolls Royce" of apartments, which spoiled us for anything less. We decided to stay in Chevy Chase.

Hughes: *You mentioned, last night, occasions that sounded very pleasant to me, namely, the small group that you meet with periodically, which includes ex-Senator William Fulbright. It makes me think your social life isn't entirely a desert.*

Cogan: You are referring to our customary coffee sessions when my wife and I meet with Senator Fulbright, journalist Henry Mitchell, and several others for a few hours on Sunday mornings. You are right, Sally, these are enjoyable sessions in the milieu of a nonmedical group. I didn't mean to imply that I was antisocial. It is simply that there is so much to be done I have to be

* Interview with Dr. Ephraim Friedman, MEEI, Boston, January 23, 1989.

parsimonious of my time. I also enjoy mixed doubles in tennis and infrequently an evening of bridge. So I am not a hermit.

Hughes: *Do those Sunday morning sessions consist of informal conversations or do you come with a topic in mind?*

Cogan: Entirely informal. The senator lost his wife a couple of years ago and seems to enjoy this socializing, as we do. I have rarely met a person with as interesting a background as he has had, a quarterback on the Arkansas football team, a Rhodes scholar, a college president at the age of thirty-two (University of Arkansas), a senator who looked as much across the Atlantic as he did toward his home state, and a liberal with whom I can identify. Then, too, we have in common problems of getting old.

Hughes: *Do you have any thoughts on the subject of growing old?*

Cogan: I'm against it. On the other hand, it does have its interesting aspects. I would just as soon not be part of it. Dr. Chandler once told me it was hell to grow old. Well, I think it is if you fight it. Better to accept your limitations, I keep telling myself. Up here in Michigan, I am depressed that I can't do what I used to do physically.

Hughes: *What did you used to do that you can't do now?*

Cogan: Gardening, carpentry about the house, hiking through the woods, beating my daughter in tennis.

Hughes: *It was your daughter, Priscilla, who gave me a feeling that the summers in Michigan when she was a child were hardly spent lounging around, that you had quite a program of activities, many of which involved strenuous labor.* What would you say was behind that?*

Cogan: It was a pleasurable contrast to the sedentary life in Boston, but it was not all physical. This is where I did a good deal of writing and now have the assistance of a portable word processor. I also have a small sailboat, which you may have noticed in the garage.

Hughes: *You may notice your physical restraints, but I certainly am not aware of any mental restraints connected with aging.*

Cogan: They're there alright—forgetfulness and all premonitory symptoms of Alzheimer's disease. People of my age are generally

* Telephone interview May 24, 1989.

preoccupied with the possibility of Alzheimer's disease. Of course, they all have it, just a minor manifestation of it. There is only a gray area between aging symptoms and frank Alzheimer's. Both have the amyloid and fibrillary plaques in the brain that characterize the disease. It's just a quantitative difference between the normal and the demented.

Hughes: Dr. Cogan, would you say something, please, about your life in Boston as compared to Washington.

Cogan: We lived in Belmont, a suburban town, sometimes called a bedroom community because it had no industries. It didn't even have a liquor store. Belmont is full of academic people from Harvard, MIT, and Tufts. Our neighbors had wonderfully diverse interests. Across the street was an economist who has since received a Nobel Prize. Down the street was a professor of mineralogy, and another a professor of political science, and another a professor of electrical engineering. I don't mean to be snobbish, but it was great to have such a mixture of interests within easy walking distance. They in turn attracted other persons with interesting interests.

I'll never forget a small dinner party with a neighbor and his invited guests, the Norbert Wieners of cybernetics fame. I challenged the latter's claim for a knowledge of anatomy, whereupon he traced the course of a red blood cell from the toe to the brain. I think he was correct, and I did not challenge him further. In Washington, on the other hand, there is not the permanence in the neighborhoods nor the backyard friendships which we had in Belmont.

Since my wife is just coming down the stairs, let's ask her what she thinks about living in Washington compared with living in Belmont.

Mrs. Cogan: If I had my choice, I'd go back to Boston.

Hughes: Why?

Mrs. Cogan: It's an academic town. It's a much more interesting place to live. Too much politics in Washington.

Hughes: You don't like that part of it?

Mrs. Cogan: I liked being able to go to the Widener Library, which is just about as good as the Congressional Library. In the Congressional Library, one doesn't have stack privileges, unless, of course, you're a senator or high official, and you really can't take books out. If I were a famous authoress, perhaps I could. At Widener, which has just as good a collection, I had a stack pass due to Dave's position, and that was great. I also had so many interesting friends.

On a recent visit to Belmont, Mary and Lucien Pye gave a dinner party for us attended by many of our former neighbors. They were such interesting persons. Lucien himself, for instance, was a sinologist, born in China, and author of several books on China. Also present was Mark Hyman, a polymer chemist, who has designed and built several solar energy homes, the Hurlbuts, [he] a mineralogist with whom Dave had several joint publications, and the Scannells, [he] a surgeon become editor. We don't seem to have that in Washington, at least not in our neighborhood.

Cogan: We do have close friends in the Washington area of whom we are fond, but they are not concentrated conveniently in our neighborhood. I think that is what we miss.

Hughes: Is it any interest to you to be in the political center of the country?

Cogan: I guess not. My wife was active in Belmont politics where she was a conscientious town meeting member, but politics in the Washington area is vastly different.

Memberships

[Interview 4: May 28, 1989, Dr. Cogan's office at the National Eye Institute]*

Hughes: You belong to a number of scientific, ophthalmological, and nonscientific societies. Did you ever hold office in any of them?

Cogan: I served officially in various ophthalmological societies but not more than my conscience demanded. They were not a major part of my existence. To be passed over for an office was always a double blessing. It blessed me, and it blessed the society.

* Although recorded earlier in the interviewing process, interview 4 is topically more appropriately placed here. It includes the final portion of interview 7.

Hughes: Would you care to talk about any of them?

Cogan: Well, I have varied recollections. I like to be involved in the organization of a new venture. My position has allowed me to initiate a number of them. Right now I am enthusiastic about the American Ophthalmic History Society, which I started three years ago in conjunction with the National Library of Medicine. Once an organization is under way, I'm happy to sit back and be just a participant. I object when it becomes too highly organized. I would cite the Verhoeff Society and the Ophthalmic History Society as being relatively free of officialdom.

Hughes: Why did you found the American Ophthalmic History Society?

Cogan: Actually, the idea occurred to me at a meeting of the centennial committee of the Academy. The chairman suggested that we take specific assignments for the event scheduled for a decade hence [1996]. I felt this was premature as some of us, including myself, would probably not be alive then. It seemed to me more to the point to establish at this early date a cadre of potentially interested persons and probe methods for presentation of historical material. The chairman and one or two others with whom I discussed it after the meeting shared my feeling.

So, when I returned to Bethesda, I approached the chief of the history section of the National Library of Medicine, Dr. John Parascandola, and together we began the annual meeting, now in its third year. It is a closed meeting at present, but my idea is that the Academy may take it over for an open meeting and that some of the better presentations may be collectively re-presented at the Academy centennial. I have solicited the advice of Dan Albert, Fred Blodi, Andy Ferry, and Marshall Parks. Together we are still exploring possibilities for the future.

Hughes: You haven't been much interested in history in the past?

Cogan: I certainly have not been the contributor to the field as have many of my colleagues, notably those whom I have just cited. I just happened to be in favorable proximity to the National Library of Medicine to fulfill the need for such a venture, which in itself, has generated a lot of latent interest. What has surprised me is the number of young persons who are interested in this aspect of ophthalmology.

*Hughes: On your curriculum vitae you've checked for discussion the American Academy of Arts and Sciences.**

Cogan: This society was founded, I believe, in the eighteenth century to be a counterpart of the American Philosophical Society in Philadelphia. Benjamin Franklin may have had something to do with it. It is a highly intellectual organization, meeting monthly for dinner and presentation of some high-level topic by an eminent lecturer. My wife and I enjoyed these meetings while we lived in Boston.

I was nominated by Dr. Verhoeff. He had been the only ophthalmologist who had been a member previously and was anxious, I believe, that I would follow his precedent.

Hughes: What specifically was Dr. Verhoeff hoping that you would transmit?

Cogan: Dr. Verhoeff never had a son. He had two daughters, one of whom died tragically in her late adolescence. I think I served the role for him as a substitute son; he often said that. He always treated me as though I were one of his family.

Hughes: Did he have a specific ambition for you?

Cogan: I don't think so. Possibly he wanted me to be his ophthalmological heir apparent. I don't know whether I fulfilled his expectations or not, but ...

Hughes: Do you think your high sense of ethics in some way was inspired by his example?

Cogan: Curiously, I've never identified him with ethics, but what you suggest is correct. He demanded complete honesty of his colleagues, but I would not say he was a missionary in ethics.

*Hughes: I got the idea from Dr. Donaldson, who spoke of both you and Dr. Verhoeff as being overly concerned with your ability to back up everything that you stated professionally.***

Cogan: With Dr. Verhoeff that was certainly true. I would like to think it was true for me as well.

---

* Dr. Cogan was asked to select for discussion some of his memberships and appointments from a list filling almost two pages of his curriculum vitae.

** Telephone interview with Dr. David Donaldson, May 17, 1989.

Extracurricular Appointments

Hughes: Moving to extracurricular appointments, of which there are many, one that you checked is the George Washington University Study Group.

Cogan: I was chairman for a short while of a study group on the ocular effects of microwaves. Are they a potential cause of cataracts in human beings? That is still a controversial question. Microwaves are, of course, involved in satellite tracking and in various military and civilian uses. We're all being exposed to microwaves to some extent. The question is, are we being exposed in a sufficient amount to cause cataracts? This issue is still being presented before the courts of law.

The work done by Dr. Russell Carpenter's group, which included my wife, showed conclusively that cataracts could be produced in rabbits by microwaves. However, since the exposures necessary to cause cataracts also burns the eyelids, microwaves under any practical conditions are unlikely to cause cataracts. No comparable burns have occurred in human beings who have been exposed to radar equipment.

Hughes: Why is it difficult to establish definitively?

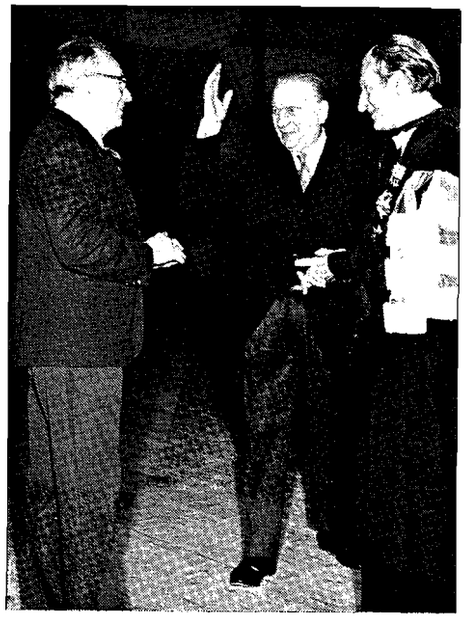
Cogan: It's a problem in measurement. The amount of radiation to the lens of the eye varies with the shape of the head or whatever else is in the environment of the eye, including any measuring instrument. Microwave dosimetry is thus quite unlike that of other portions of the electromagnetic spectrum where the inverse square law applies. The fact remains that there is no incontrovertible proof that cataracts have ever been observed in human beings from microwaves, whatever lawyers and juries might conclude.

Honors

Hughes: Characteristically, you have checked only one award to discuss. That is the Gonin Medal, which you received in 1974. I have heard that's an extremely prestigious, if not the most prestigious, medal in ophthalmology. Is that true?

Cogan: Derrick Vail called it the Nobel Prize of ophthalmology. Lest I seem ungracious, however, I would like to acknowledge at least a few other awards. These would include prominently the Research to Prevent Blindness Award, which I received early on, along with Drs. Kinoshita, Kuwabara, and Grant. Then were the Humboldt Award from Germany [1988] and the honorific Doctor of Science Award from Duke University [1989].

But you ask about the Gonin Medal. [Jules] Gonin of Lausanne was identified as the one in the late 1920s who revolutionized surgery for retinal detachment. I did retinal detachment surgery at the Infirmary for a while after spending time in Utrecht with Professor [Henricus J. M.] Weve during my Moseley Traveling Fellowship. I believe I did introduce Weve's method of using indirect ophthalmoscopy during the procedure when I returned to Boston, but the surgery was never of great interest to me.



Dr. Cogan receiving the Gonin Medal from the Deputy President of France and the President of the International Congress of Ophthalmology, Paris, 1974

Hughes: Was there a citation with the award?

Cogan: Yes. The award was made in Paris at the meeting of the International Congress of Ophthalmology, but the actual ceremony was held at a later date in Lausanne.

Hughes: Was there a lot of ceremony with it?

Cogan: It was very formal. I remember after I made the usual acknowledgements in French, the audience's sigh of relief when I switched to English for the scientific part of my talk. [laughter]

Hughes: Did the citation mention your early retinal surgery?

Cogan: No. My contribution to that was negligible.

Hughes: Is the award strictly for work in retinal work?

Cogan: I believe so, but not necessarily for surgery.

Hughes: What year did you visit Weve?

Cogan: In 1938. My wife and I rode our bicycles from Basel to Utrecht where we stayed for a couple of months while on the Moseley Fellowship. At that time, to ride along the French-German border was an experience!

Hughes: Was Gonin still alive?

Cogan: He had died in 1934. He was one of Lausanne's most famous personages.

Hughes: As you know, it took him many a year to convince ophthalmologists that his approach to retina detachments was the correct one.

Cogan: So I've been told.

Hughes: Had Weve actually studied with Gonin?

Cogan: I don't recall.

Hughes: There's about a page of honorary lectures in your curriculum vitae. One you checked to discuss is the Verhoeff Lecture, which you gave in 1969.

Cogan: I must have checked that because it is replete with anecdotes that might be of interest. After Dr. Verhoeff died, there were many obituaries, including one which I wrote.* When I was later asked to present the Verhoeff Lecture at the American Ophthalmological Society to highlight his life and times, I had to think of something different from just another obituary. In the

* Cogan DG. Frederick Herman Verhoeff – Personal recollections. Trans Am Ophthalmol Soc 1969; 67:96-109.

middle of one night, I conceived of writing him a letter encompassing some of the anecdotes with which his life had been surrounded, but which had rarely been put in print. I thought he would have approved of this, and it would be of interest to the audience. I believe it succeeded in getting across some of the aspects of his life which would otherwise have been lost. In a way, it was sort of an oral history.

Hughes: Then, there's the Susan Alper Lecture in 1979.

Cogan: That lecture was significant to me for several reasons. In the first place, I had a long-time friendship with Dr. Melvin Alper, and after my move to Bethesda an especially close relationship to the Alper family. It was while we were living in Chevy Chase, not far from the Alpers, that their twenty-three-year-old daughter, Susan, was killed in an auto accident. This mirrored our experience of the 1960s when our daughter, Christy, of about the same age, had been killed in an auto accident. The common tragedy made our families especially close. Finally, Dr. Alper ran a monthly neuro-ophthalmic conference at the Washington Hospital Center where he was chairman of the department. The lecture gave me an opportunity to document collectively some cases of visual and ocular motor defects from cerebral lesions. With his usual liberality, Dr. Alper allowed me to give a three-hour lecture instead of the usual one hour, the first such for the lectureship and the first for me.

Hughes: Who established the Susan Alper Lecture?

Cogan: The many friends of Dr. Alper. He is popular among his colleagues in medicine and among his patients. With all his interest in neuro-ophthalmology, he carries on a heavy surgical practice.

That lecture in 1979 coincidentally marked a transition point for me at NIH. Theretofore I had been chief of the section on neuro-ophthalmology. Since then my interests have been primarily with the microscope rather than with patients.

Hughes: Why did you have a resurgence of interest in pathology?

Cogan: When you reach your middle seventies, you want a little more time to yourself. When you are seeing patients, you are a public servant and the telephone is your adversary. Then, too, Dr. Kuwabara and I began to talk about putting a text together on some aspects of pathology.

Hughes: Then the Japanese Ophthalmological Society Lecture [1985]?

Cogan: I've given the Japanese Ophthalmological Society Lecture twice. When I was invited for the second time, a couple of decades later, I wondered if they realized I had given the lecture previously. Dr. Kuwabara advised me to accept it any way, which I was inclined to do because Japan was so pleasant to visit and the Japanese colleagues were so cordial. It was an experience for both my wife and me.

Hughes: When you were invited to give these honorary lectures, how did you choose the subject?

Cogan: That depended very much on the anticipated audience. In Japan, it was important to present a subject with pictures because of the language barrier. There were also personal reasons. For instance, my last lecture in Japan was on fat metabolism because that involved Dr. Kuwabara. In Japan, I had difficulty with humor that I use to spice up lectures. That was hard to get across to the ever-polite Japanese audience. In fact, when Dr. Ikui was my host at one of the lectures, he asked me to forewarn him if I were attempting a joke so that he could evoke the appropriate responses. [laughter]

Hughes: The last lecture that you checked was the Kinsey Lecture [1987].

Cogan: This was a memorial lecture set up at Oakland University where Everett spent his last years. He had developed some cardiac symptoms and died of a complication of cardiac arteriography. The lecture was especially meaningful to me since we had grown up together scientifically in the Howe Laboratory. He was my first appointment and established a prototype for biochemists in a clinical department. Both he and his wife were closely associated with my family in those early days of our careers. His premature death was a shock.

Associations with the Academy

Hughes: What about your associations with the Academy? I understand that you have held a position on the museum committee.

Cogan: That was a very minor position. Actually, I have never held an office in the higher echelon of the Academy. The nominating electorate probably knew that I would not be a very effective officer and that I did not covet such a post. I think we both

profited by that decision. Actually, however, I have served the Academy in some less official capacity. I gave the Jackson Lecture on one occasion. I have been the honored guest on two occasions and participated in some of its teaching functions and most recently served on the museum committee and the centennial committee. I feel guilty I have not done more.

Incidentally, in my Jackson Lecture given back in 1952, I presented the entity of congenital ocular motor apraxia with jerky head movements.* This has come to be known as Cogan's syndrome, type 2 because, I suspect, the name I suggested was too cumbersome. An alternate name, the acronym COMA, has other ambiguities, [as I've already mentioned].

At a later date, when I was one of President Maumenee's honored guests [at the Academy], I gave a followup of this entity, pointing out the occasional familial occurrence of the syndrome. This meeting was memorable for me as it was to be my farewell to ophthalmology, for I had envisaged what Dr. Norman Ashton has aptly called a "pre-posthumous" lecture. Just before the meeting, I had developed a gastrointestinal obstruction which was shown radiologically to be due to a mass in the colon. I had little doubt that it was malignant and had made arrangements for surgery immediately on my return to Boston. A colectomy showed it to be a benign lipoma. This is probably of little general interest, but, of course, it meant a lot to me.

Hughes: Do you want to say more about memberships, societies, lectures?

Cogan: My chief service to the Academy now is to provide an infrastructure for its centennial in 1996 through the American Ophthalmic History Society.

Hughes: Will you write something for the centennial?

Cogan: I don't expect to be around at that time.

Hughes: Ten years ago, the Academy separated from the otolaryngologists. Did you have a part in that separation?

Cogan: I had no part in it, but I felt it was overdue.

Hughes: Why did you think separation was a good idea?

---

* Cogan DG. A type of congenital ocular motor apraxia presenting jerky head movements. Am J Ophthalmol 1953; 36:433-41.

First meeting of the American Ophthalmic History Society, National Library of Medicine, 1988

Cogan: The combination might have been appropriate at one time when the specialty of eye and ear was the rule for financial reasons, but that has long passed. Few persons now practice combined eye and ear together. The specialties actually have little in common. I think the combination in a separate hospital such as the Infirmary is similarly an anachronism.

Hughes: Are you aware of any of the problems in splitting the Academy into two separate institutions?

Cogan: I'm not aware of any, but I would not be in a position to know about them if there had been any. Prior to the split, I heard predictions that attendance [at the annual meeting] would drop off markedly, and the commercial exhibitors would decline to show their wares. So far as I can see, I don't think either of these objections has been borne out.

Hughes: I read a poll that was conducted in 1973 by the Academy, and, surprisingly, only 54% of the respondents were in favor of splitting into two academies. I wonder what the resistance was?

Cogan: I remember one comment: "We served together as residents in the same hospital, and it's nice to get back to see your former associates in the other specialty." This seemed to me a fatuous argument.

Hughes: I know there were some economic problems, too. Apparently the money was in a common kitty and it was a question of how to divide it up.

What do you consider yourself first and foremost? A clinician, a surgeon, a researcher, or a teacher? Or something else?

Cogan: I believe I have been fortunate enough to be a little of each at different times of my life. I suppose I am a professional polygamist.

Hughes: Let's leave it at that.

Cogan: Do you know the story of the man who had been widowed twice? He said he wanted to be buried precisely between his two wives, but if that were not possible he would be willing to be tilted just a little bit toward Tillie. [laughter]

Hughes: Which occupation do you tilt towards? I don't think it would be toward surgery.

Cogan: You are probably correct. I enjoyed surgery when I was doing a lot of it, but not when I had to cut back.

Hughes: What about taking care of patients?

Cogan: I think I inherited from my mother an interest in people and being able to help them. At first, it was in the care of patients. Later, it was, I believe, in helping young investigators. I think this was recognized by the Association for Research in Vision and Ophthalmology when they established a lectureship in my name for young investigators. It was the first of three lectureships to be given antemortem. I wondered at the time if it was not premature.

I've forgotten what your question was. I hope my freewheeling comments are somehow relevant.

Current Issues in Ophthalmology

Hughes: An entirely different sort of question: What do you think of profiting financially from medical research?

Cogan: Conflict of interest is a major issue right now. It is a dangerous policy to have an investigator profit financially from his research unless it is for the institution.

Hughes: Is there a reason that ophthalmology is in the news these days, rather than another specialty?

Cogan: Ophthalmology is properly being discredited because of the way some surgeons are advertising their trade in the popular media. It is really repulsive to one who was brought up in a strict code against advertising. Ophthalmology is reduced to a business. The epitome occurred a couple of years ago when a small group of doctors doing the controversial radial keratotomy sued the Academy and its officers for suggesting the procedure was still "experimental" and needed further control studies to determine its safety or validity. The basis was restraint of trade. Had the suit been successful, it would have bankrupted the Academy and its officers.

Hughes: Do you think the trend is reversible?

Cogan: I don't have any crystal ball, but the transgressions, even though they are from a minority, are an invitation to governmental restrictions on medicine. We are already facing a disproportionate cutback on governmental remuneration for ophthalmic surgery.

Hughes: What is your opinion of that?

Cogan: It's an inevitable reaction.

Hughes: What about optometry, which is another current issue in ophthalmology?

Cogan: Ophthalmology's criticism of optometry has been much the same as is now being leveled against some of the advertising practices which we have just cited among some ophthalmologists. They not only advertise their trade, they operate commercial stores, they make profit on the sale of glasses, and call themselves doctors, which they have a right to do, but the public misinterprets this as meaning medical doctors. In short, they accept gross conflicts of interest which, until recently, have been unethical for ophthalmologists.

These criticisms are not valid where optometrists are salaried persons working in a clinic with ophthalmologists. This was the case at the Infirmary, where it worked well. The optometrists did the refractions, which they are trained to do, whereas the ophthalmologists handled the medical and surgical aspects, which they are trained to do. The question is how to make this mandatory. I think it is fair to say that optometrists don't like it because of the fear that they are then second-class citizens with lower incomes than they can make on their own. Interestingly, some of the ophthalmologists who are most critical of optometrists came into ophthalmology through optometry.

Hughes: Were you ever aware of prejudice against women or minorities in your long association with the Infirmary?

Cogan: I was aware of the difficulties that women had. I had some reason to know because my mother was on the staff, first at the Boston Dispensary and later at the Infirmary. It was not a prejudice, though, it was the difficulty with competitive obligations at home and other interests. It was just harder for them.

Hughes: In what ways?

Cogan: I think the public at that time was skeptical about women operating on them. Perhaps that was a prejudice. As a result, women did not do as much surgery. My mother, for instance, did only minor surgery which could be done in the office. Also, the married women ophthalmologists had to move when their husbands moved, and it was not easy to start a practice in a new city. Now persons who are married by the time they get through medical school have different problems. Can they get an internship in the same city? Is it fair to take an inferior position just to be in the same city as your husband, or the reverse?

Hughes: Were there lean times in the Cogan family when your mother was reestablishing her practice?

Cogan: She never had a large practice, and the family moved only once while she was in practice. On this one occasion, she maintained a limited practice in the first city since it was only about ten miles away. The small income which she had from her practice was important.

Hughes: Harvard Medical School does not have a sterling record in regard to admission of female medical students. It was very late in accepting women. Was there a similar policy at the Infirmary in regard to female residents?

Cogan: I was chief of ophthalmology at the time and had a very promising candidate, Deborah Pavan. I remember the debate. We could train only a limited number of persons. Was a woman the best investment? Was it not true that more women than men subsequently abandoned the profession? What would we do for quarters in our tightly occupied hospital? On the other hand, there were great opportunities for women in ophthalmology, such

as pediatric ophthalmology, ophthalmic pathology, etcetera. But above all was the excellence of the candidate. The case for Deborah Pavan won out. She was the first woman resident and has gone on to be a recognized authority in virology. Now I would guess that about half the residents are women.

Hughes: I can think of a number of women ophthalmic pathologists in the past, but are there any notable ones today?

Cogan: I suppose Georgianna Dvorak-Theobold is the prime example [from the past]. She was at the Illinois Eye and Ear Infirmary where the pathology laboratory and the midwestern ophthalmic pathology society are named in her honor. Another outstanding woman in ophthalmic pathology was Helenor Wilder, who, as a technician at the Army Medical Museum, became so knowledgeable as to be the recognized ophthalmic pathologist. More than this, she was the first to identify *Toxocara canis* as the cause of a severe endophthalmitis, and to identify *Toxoplasma* as a cause of intraocular infections. Currently outstanding women in ophthalmic pathology are Jean Campbell at the Mayo Clinic, Merlyn Rodrigues at the University of Maryland, and Barbara Streeten at Syracuse, New York.

Hughes: What criteria did you use when you were deciding whether or not to try a new drug or a new procedure on a patient? How could you decide that it was safe?

Cogan: That's a difficult question. Certainly you would try it on animal eyes first. Contrary to what the animal rights people say, a computer cannot tell you whether a new drug will or will not be toxic.

Perhaps I can answer your question by giving you an illustration of a case in which cysteamine was used on a human eye for the first time. The patient was an infant with cystinosis who was having considerable pain as a result of cystine crystals eroding through the corneal epithelium. Cysteamine was being used systemically in cystinosis, but I wanted to get a high concentration on the surface of the eye. I proposed to use drops on the eye. Dr. Joseph Shulman provided me with the solution. I first tried it over a twenty-four-hour period on a rabbit cornea. It produced no irritation. I then tried it on a monkey eye, with similarly negative effect. Then I tried it on my own eye, which was also negative. I then felt free to try it on the patient. The results were promising, but we had to discontinue the procedure because it was violating a certain NIH ruling that a protocol had to be prepared and approved before any experimental procedure

was done on patients. We had violated the rules. Later, Dr. Muriel Kaiser-Kupfer prepared a protocol and study that confirmed the positive results. This gives you some idea of the procedure and complications in trying out a new drug.

Publication

Hughes: What importance do you place on publication, in relation to other aspects of your career?

Cogan: It is important to prepare papers for publication. In the first place, it is good discipline. Public exposure tells you something about yourself and your work. It must be something more than just table talk. You have to ask yourself, is it original? Has it properly acknowledged the work of others? How well is it written? Publications are important for promotions or acceptance at other institutions. In many cases, the papers are the justification of money spent on you.

Hughes: Did you feel pressured to publish?

Cogan: I felt it was necessary to report work that had been funded by outside agencies. It was a progress report aiming for continuation of the support. This was especially true for NIH grants where decisions were made largely on the basis of the quality of publications.

Hughes: The list of publications at the end of each Howe Laboratory report was a prominent feature. It usually went on for pages.

Cogan: Publications are what one passes on to posterity.

Hughes: Did you ever need to urge members of the Howe Lab to publish?

Cogan: At the extremes are two types of people: Those who you have to urge to publish and those who you have to suppress. The persons who you have to urge to publish are often the most qualified. Dr. Grant, for instance, was reluctant to publish his work. You could be sure that anything he did publish was thoroughly considered before it was sent off to press. On the other hand, there are those who have such an urge to publish that the quality of their publications is questionable. Of course, there is a big gray area in between.

Hughes: Were there times when you had to suggest to somebody that perhaps it wasn't the time to publish?

Cogan: Yes, and times when I solicited advice on the wisdom of publishing my own papers.

Hughes: When you were writing a paper, was it a creative process? Or was it simply a factual presentation of the results?

Cogan: The creative period, if I may call it that, was already past by the time the paper was being written. The creative period was while you were doing the work. That was when you would wake up in the middle of the night with an idea, or you might be teaching and some new insight occurred to you. That was the creative period. Writing was a matter of organization and self-criticism.

Hughes: Did it ever occur to you that you had to stop and get more data?

Cogan: Often. That's part of writing.

Hughes: Many of your papers were written with Dr. Kuwabara. How did that work? English is not his first language.

Cogan: I did most of the writing of our conjoint publications, but he was the senior author when he made the major contribution.

Hughes: Was it ever difficult to determine who should be the first author?

Cogan: I'm not aware that it was. I hope others would agree with me.

Hughes: How did you determine the sequence of authors on a paper?

Cogan: That can be a sticky problem and an important one. When a person on an academic ladder comes up for promotion, his or her publications are carefully considered, with emphasis on the times he has been senior author. I think the senior author should be the one who initiates the idea, makes the major contribution to the project, and writes the paper. The order of the other authors is not so important. If there is an equal contribution to a study, it has been my policy to alternate seniority in multiple publications. Thus several series of my publications have been alternatively Cogan-Kuwabara and Kuwabara-Cogan. I am not aware of our having had any questions in the Howe Laboratory publications on this score.

Hughes: Did you have any role in the sixties in the genesis of Medicare or Medicaid?

Cogan: No, emphatically.

Hughes: What do you find the most effective way of disseminating ideas?

Cogan: Publications and participation in symposia with large audiences.

Hughes: There are probably many ways.

Cogan: I think it is important to address groups of persons who are not strictly in your own field. It is a challenge to present your work using a vernacular and mode of expression that will be understood by someone who may be unfamiliar with the subject you are presenting. There are, of course, occasions when the opposite is true also, such as ARVO [Association for Research in Vision and Ophthalmology] where you address persons who are in your field.

Hughes: Why do you single out ARVO?

Cogan: Because ARVO represents the specialized fields in a seminar format. To understand the sessions, you have to be familiar with the acronyms and techniques which the essayist is demonstrating.

Hughes: Did you ever withhold information until you had an opportunity to publish it?

Cogan: I think you tend not to make public announcements of original observations until you have already submitted your manuscript. It may not yet be published, but it has been submitted. Dr. Kuwabara may have told you that he showed the new trypsin digest technique to a visitor to the Lab before it was published. I was a bit shocked when two weeks later I received a brief manuscript from this former visitor describing the technique. I was chief editor of the *Archives* at that time and advised him that his paper was simply what he had learned at the visit. He graciously agreed to withhold his paper until the original report was published. This was naiveté and not dishonesty.

Hughes: What would you say was the policy of the Howe Laboratory in terms of sharing information?

Cogan: In our daily luncheon sessions we felt we could be entirely open. Rarely did we have an experience with visitors like that cited above.

Controversies and Changes in Ophthalmology

Hughes: Have you been involved in any controversies in ophthalmology?

Cogan: Many.

Hughes: Could you give me a few illustrations?

Cogan: It seems to me we have cited a number, both scientific and administrative. The matter of corneal neovascularization, the problem of microwave cataracts, the presence of consultation services in a hospital, the fragmentation of support within the Infirmary framework, the disciplinary action for staff derelictions, etcetera. I doubt that this is what you had in mind.

Hughes: Were you ever involved with the issue of fee splitting?

Cogan: I think Boston was exceptionally free of that. George Derby had been adamant against it and set a pattern that prevailed long after he was gone. I know of only one instance when something analogous to fee splitting occurred. That person was peremptorily discharged and left for Canada.

Hughes: What criteria did you use to set fees?

Cogan: The standard fee for an office visit in private practice was $10, with no charge for nurses, doctors, and ministers. In suburban areas, like my mother's practice in Wakefield, Massachusetts, the fee was $5 per visit. My fee for cataract extraction was the standard $300 when I was in private practice.

Hughes: Did the Infirmary have a fee schedule?

Cogan: I don't know what the outpatient fee was, although sixty cents seems to jog my memory. The Infirmary was not in the market for private patients. Potentially private patients were given a list of private doctors with offices scattered about the city. Only the chief of service had an office in the Infirmary to see private patients and that only occurred when Dr. Dunphy became chief.

Hughes: What would you say are the major changes in ophthalmology in your professional career?

Cogan: There are, of course, many changes. Residents are now paid a substantial salary. They are now employees of the hospital.

Fellows now abound in the various subspecialty clinics, and in some places have taken away some of the responsibilities of the residents. There are more full-time senior staff members in the clinics.

Of course, there have been major technological changes. Binocular indirect ophthalmoscopy is as much used as direct ophthalmoscopy. Automatic field testing and automatic refraction are commonplace. Cataract extractions with intraocular placement of an artificial lens are now a one-day affair. Retinal biopsies are becoming frequent.

Hughes: Do you have feelings about the heavy reliance of present-day ophthalmologists on instrumentation?

Cogan: They make for precise documentation, but they are only aids.

Hughes: What about losing sight—that's a pun that wasn't really intended—of the patient because the doctor is so busy with instrumentation?

Cogan: That's right. There's no substitute for listening to the patient's history. This can be especially important in neuro-ophthalmology.

Hughes: Why particularly in neuro-ophthalmology?

Cogan: Because neuro-ophthalmology depends heavily on subjective information. In this respect, neuro-ophthalmology is more akin to general medicine than are most other aspects of ophthalmology.

Hughes: Of course, there have been tremendous changes in the basic sciences as well. Can you think of ways in which scientific breakthroughs have made a difference in your research?

Cogan: What has changed is the knowledge of what underlies various disease entities—the genetic basis for congenital defects, enzyme deficiencies, immunologic abnormalities, and the like. The eye is such a beautiful organ in which to see these aberrations.

Understanding the basic processes is understanding the disease. That's fundamental.

Hughes: Did Medicare have any impact on your work in the Boston area?

Cogan: No, I've been comfortably spared from such fiscal problems. I know it is profoundly affecting hospital administration. Viewed over the fence, however, it seems to me we're headed for some health care system similar to those in Canada and Europe. And

some of those who are protesting the loudest are just the ones whose excesses are responsible for bringing it about.

Medical and Surgical Ophthalmology

Cogan: Ophthalmology has been properly a surgical specialty, but it seems to me that there is an attractive medical side with a potential division comparable to that of neurology and neurosurgery. I believe I have personally stressed the medical aspects because that is the less well-developed branch and to me the most interesting. I realize it is not as remunerative, but that is perhaps less important in an academic setup than it would be in private practice. So I don't think those of us who have been in a privileged position should generalize what should be done for others.

Neuro-ophthalmology is perhaps the present substitute for medical ophthalmology as a subspecialty. It appeals to persons who would be medical ophthalmologists if there were such a discipline. Years ago, Dr. Fred Cordes of San Francisco suggested a split with, I believe, two separate boards of certification, but it caused such a furor that it was dropped. Then some years later, Dr. Maumenee and I tried to set up residency programs that might serve the separate goals, but these were not effective either.

Hughes: Why were people opposed to the idea?

Cogan: I think many felt they might be barred from doing surgery, and they didn't want to lose this right. As far as the trainees were concerned, I suspect the residents did not feel that they were virile ophthalmologists unless they had the whole battery of surgical experience.

Hughes: Do you think the monetary differential between the ophthalmic surgeon and the medical ophthalmologist has been a barrier?

Cogan: That is, of course, a very real factor. But the same applies to the differential in the incomes of neurologists and neurosurgeons, I believe.

Hughes: Perhaps these moves for more government control, which is particularly directed to surgical fees, will in the end benefit medical ophthalmology by reducing the difference between the two branches.

Cogan: The future will decide. I don't see it as a burning question at the moment.

Hughes: As a young man, had you thought that you would spend more of your career doing surgery than it has turned out?

Cogan: Of course, an ophthalmic residency is largely surgical training. When I finished, I was an average ophthalmologist and did surgery as we all did. I continued while I was in private practice. But as I became involved in laboratory work and less in practice, I felt it was not fair to the patient to do surgery unless I was doing a lot of it. One's dexterity depends in part on the amount of surgery one does. I continued to assist in surgery but not to have the primary responsibility. Since I had no secretary, I was happy to be relieved of the many telephone calls that are involved with surgery.

Hughes: When you were doing the work on the retina with Dr. Kuwabara, did you find it useful to be doing some retinal surgery? Or did surgery seem completely separate from what you were trying to do in the lab?

Cogan: They are worlds apart. When you put on a sterile gown in the operating room, you are a different person than you are in the laboratory.

Hughes: I am thinking of the town-gown problem again. I presume that most of the town people are surgeons, is that not true?

Cogan: I think it is.

Hughes: Did you ever feel yourself at a disadvantage because you're better known as a medical ophthalmologist than as a surgical ophthalmologist? Did that mean a lessening in political clout?

Cogan: I wasn't aware of it as a lessening of political clout, but I was aware of the disadvantage in not having contact with surgical patients who constitute the majority of benefactors for research. I had to depend on the good will of the surgical staff to represent our interests to them.

Hughes: Do you think sufficient weight is given to the medical aspects of ophthalmology in the residency training programs?

Cogan: I don't know how sufficient it is, but neuro-ophthalmology has been incorporated in training programs. That was not the case in my early days.

Hughes: Did you attempt to counteract the deficiency, if that is what it is?

Cogan: Not really. If it is desirable, I believe it will come about by itself or else optometry will take it over.

Hughes: Do you have anything to say about clinical trials?

Cogan: They are a costly necessity for the evaluation of therapy. Controlled observations are the only way to get something more than anecdotal opinions. The National Eye Institute is heavily involved in these multiuniversity studies which can be done most effectively only by an agency with the resources of the government.

Hughes: Have you yourself conducted clinical trials?

Cogan: No, but I am grateful to those who set them up with statistical controls, safeguards for the patients, and severe problems of recruitment.

Major Clinical and Scientific Problems in Ophthalmology

Hughes: What do you think is the major clinical problem in ophthalmology today?

Cogan: I would put macular degeneration at the top of the list. This is a cruel loss of central vision commonly affecting people in middle life or older, people who were planning to catch up on reading in the leisure of old age and now lose just that function. Visual aids are only moderately helpful.

Hughes: And if I asked the same question about the major scientific problem in ophthalmology?

Cogan: I would have difficulty in answering that question. Certainly, retinal degenerations would be well up on the list, but so would the genetics of ocular tumors, the pathogenesis of optic neuritis, and the role of autoimmunity and viral infections in ocular inflammation.

Relaxation

Hughes: On a different plane, what do you do to relax?

Cogan: I try to play tennis once a week. I'm not much of a player, but I like it. Since my wife is a good player, it has its social as well as physical aspects, and it is not the time-consuming game that golf is. Also, piano playing has become a late-in-life pastime. I may get up in the middle of the night and play for an hour or so. In this I have been inspired by Frank Wilson's delightful book *Tone Deaf and All Thumbs?** in which he claims to have taken up the piano at middle age. He is a neurologist at Stanford who has become a specialist in the biology of music and is active in the summer Aspen festivals. He graciously gave us an evening's entertainment at NIH as part of a cultural series of lectures and demonstrations.

Hughes: Your wife plays, too, I understand.

Cogan: Both tennis and the piano.

Hughes: You're currently doing research for a paper on music?

Cogan: Right now, Frank Wilson has me working on the subject of neuro-ophthalmologic diseases of musicians for possible presentation in one of his courses. In the past, I have had several professional or near-professional musicians, including the famous Randall Thompson, who developed brain lesions that prevented them from reading music, but did not prevent them from composing, conducting, or playing by ear. This has sparked my search for the medical records of famous composers who have developed neurological diseases. It has become a free-time preoccupation of mine.

Another mode of relaxation is the care and maintenance of our vacation house in Leland, Michigan. This is a frame house, now more than fifty years old, and needs constant repair. My wife spends the entire summer months there, whereas I come and go. I complain about the many tasks I have there, but basically I like it. Then, too, it is a place where I can do some of my writing in a relaxed environment.

* Tone Deaf and All Thumbs?: An Invitation to Music-making for Late Bloomers and Non-prodigies. New York: Viking, 1986.

Hughes: But you never stay the whole summer?

Cogan: Not yet.

*Hughes: Here's a quote that I think is very apt. The source is David G. Cogan. "A man who loves his work always has it with him. I think he's unrelaxed when he doesn't. Why, the cruelest thing you can do to a man is to expect him to sit on the beach and leave his work back in the laboratory."**

Cogan: I can't go on a pure vacation and be happy for more than a few days. I have liked to travel but only in conjunction with some work.

Aging

Hughes: Do you have any more thoughts on the subject of aging?

Cogan: I have reason to give it considerable thought. My own incapacities are increasing rapidly and on schedule. In a way, they are interesting, but I would gladly do without them.

Along with loss of memory, the most distressing aspects for a person of my age is loss of friends and colleagues. Of the six of us who came down to NIH from the Howe Laboratory, only half are now active here. Of my twenty classmates at Dartmouth Medical School, only three are now living. I know because I have been the class secretary.

Hughes: Do you have thoughts about retiring?

Cogan: Each time I'm given a party I wonder if it isn't a retirement party and telling me something. I'm reminded of Vannevar Bush's statement that he had retired four times and was thinking of retiring again. [laughter]

To ease my conscience, I have several times offered to retire if I could only keep my room at NIH with the excellent microscope, video recording apparatus, my IBM personal computer, and parking place. I have also been blessed with an excellent assistant in the person of Elizabeth Paterson who has helped me put my files in some sort of order for the time when I might retire or die.

* Contemporaries: David G. Cogan, MD. Mod Med 1969; 36:16-20.

The Good Physician

Hughes: What do you think makes a good physician?

Cogan: Oh, that's a tough question. The public wants a person with a pleasant bedside manner, one who is sympathetic and listens to you. Those certainly are desirable traits, but competence and curiosity are also essential.

Hughes: How do you define competence?

Cogan: If he's a surgeon, he must have appropriate judgment and dexterity. He must know when not to operate as well as when to operate. If he is an internist, he must have a vast fund of knowledge and an empathetic understanding of his patients' needs. I would expect him to be a good diagnostician and up to date on therapeutic means. It is very difficult for the public to select a good doctor.

A few years ago, John Pekkanen wrote a book that attempted to solve the problem of choosing a doctor. The book had a terrible title, *The Best Doctors in the US*, but had a logical approach.* For each specialty, the author canvassed the heads of ten outstanding departments, asking for the names of five to ten persons in their specialty whom they would recommend. The final list was made from a compilation of names that were suggested by several of the interviewees. It is not a perfect system, but it is preferable to making a choice on a neighbor's recommendation.

Hughes: Is there a consensus that the physicians listed are indeed the ones who are most competent?

Cogan: The main objection was the inflammatory title. Then there was an absence of persons who probably should have been included. Some of these were included in a subsequent edition.

Hughes: Do you agree with the choices?

Cogan: In neuro-ophthalmology, for instance, they were all top people. I suggested a few names that were not listed in the first edition and which were included in the second edition. I also asked that my name be deleted as I was not in practice.

* Pekkanen J. The Best Doctors in the U.S.: A Guide to the Finest Specialists, Hospitals, and Health Centers. New York: Seaview Books, 1979.

Hughes: If you were starting off today afresh, would you choose medicine as a career?

Cogan: It is true that medicine has lost a lot of its appeal, but I think it still has a lot going for it. One has the satisfaction of serving a humanitarian end. It has technology comparable to that of the physical sciences. There are many aesthetic yields, and the financial returns are adequate after the heavy investment of schooling. I think I would still choose it, but if I knew I were to be an ophthalmologist, I would elect a preparation in the scientific mode rather than in the liberal arts.

Hughes: Do you think you would have been interested in the liberal arts if you hadn't had a liberal education at Dartmouth and a wife who is interested in the liberal arts?

Cogan: Of course, I have been extraordinarily lucky to have a wife who guarantees continued contact with liberal arts. Nevertheless, my contribution is in the scientific arena and that is where I should be heavily entrenched.

The Cogans with daughters, Priscilla and Polly, at dedication of the Cogan Library, National Eye Institute

Hughes: Does medicine have the excitement it did when you first entered it?

Cogan: In retrospect, I think there is still much room for excitement. A recent survey at Harvard indicated that on entering medical school some 70% or more of the students stated they were excited about world affairs and their possible contribution to its welfare. By their senior year, however, they had turned to technological and scientific aspects with only 20% supporting their earlier motivation.

Hughes: What about making money?

Cogan: I'm sure that had much to do with their motivation, especially with those who had a family to support and a large debt upon graduation.

Contributions and Regrets

Hughes: What do you think is the greatest contribution of the Howe Laboratory?

Cogan: I believe it is the cadre of persons with a research orientation who have been brought into the field of ophthalmic research. I am thinking of basic scientists like Drs. Kinsey, Kinoshita, Kuwabara, and Chader, and clinicians like Drs. Kupfer, Grant, and myself who have had opportunities to pursue their interests in research. This is so much more significant than any specific discovery.

Sally, we were interrupted just now by a telephone call which incidentally illustrates the foregoing. The call was from a medical student by the name of Nick Hogan, PhD, requesting advice about an ophthalmic residency. Nick was a sophisticated virologist who had been working with D. Carlton Gajdusek a number of years on slow virus disease when he joined our laboratory at NIH in ophthalmic pathology. This decided him on a career in ophthalmic research, but he opted first for an MD degree, which he is now about to get. I am not sure what he will ultimately decide to do, but he represents a function of laboratories like the Howe and the one at NEI of bringing persons with a basic science background into ophthalmology.

Hughes: Very apt. What regrets do you have?

Cogan: I wouldn't be honest if I didn't say I have had disappointments. We have had fellows or staff members who have disappointed us. There are some for whom we had high hopes for careers in ophthalmic research but who have become totally immersed in private practice. We have had a few who took advantage of our freedom to set up empires competitive with our Laboratory and detrimental to the Infirmary and the Howe Laboratory. They have created severe political strains, phenomena which are accepted in business but traditionally foreign in academic life.

Hughes: What do you consider your own greatest contribution?

Cogan: My contribution has been, I believe, taking advantage of opportunities that arose fortuitously in my area of activity. I inaugurated the basic science course in 1938, the first of its kind in the United States, because it happened to be needed at that time. I converted the Howe Laboratory to a full-time staff because it was timely to do so. I established neuro-ophthalmology in Boston at a time when it was not recognized as a worthwhile subspecialty. I promoted the Socratic method of dialogue between clinician and basic scientist because the setup of the Howe Laboratory gave me the opportunity to do so, and now I have organized an Ophthalmic History Society which, I believe, responds to a need of the times. I took advantage of opportunities that were at times risky. They might have been passed over as the easier thing to do. Many were successful, a few were not.

This is a vague answer to your question—purposely vague because only time will tell what will be the greatest contributions.

Hughes: One final question. What have you most enjoyed doing?

Cogan: You ask such difficult questions, Sally. I enjoyed teaching. Was that "most"? I enjoyed the excitement of original research and the solution of its inherent questions. I remember the excitement when Dr. Kinsey and I were discovering things about the permeability and transparency of the cornea. We would call one another up in the middle of the night if we had new ideas. I participated in the excitement of others over work with which I may have had nothing to do. The discovery of the role of aldose reductase in diabetes was as exciting to me as though it were my own work. The Howe Laboratory was part of me and I of it. It was an institution with which I could identify.

Hughes: Of your decades of teaching, both on the formal course level and in connection with your work in the Howe Laboratory, what one thing would you like your associates to remember?

Cogan: That's a terrible question.

I think I would rest comfortably if they said, "He had his assets, he had his liabilities, he made mistakes, and he made contributions, but the contributions and accomplishments outweighed the mistakes."

Hughes: Of all the knowledge that you have imparted to others, what do you consider to be the most important?

Cogan: To transit some of my enthusiasm for the aesthetic, scientific, and humanistic possibilities in the study of the eye and its connections to the brain. I guess I would leave it at that.

Hughes: Do you have any regrets?

Cogan: Of course, we all have regrets. Not all my problems had happy endings. Not all the people turned out to be the paragons I had expected. Not all the opportunities that came my way could be explored—the other path so to speak.

Hughes: What are the highlights in your professional life?

Cogan: I suppose the one word that says it all is freedom—freedom to pursue what I thought was right and promising, freedom to associate with persons whom I admire and respect, and freedom to identify with institutions that serve the common good.

Hughes: Is there anything that you'd care to add before we stop?

Cogan: I would like to thank you, the Academy, and others involved in giving me the opportunity to see this mirror image of a career and of the institutions with which I've been associated for these many years.

The Cogans and friend, 1989

APPENDICES

CURRICULUM VITAE

Name David G. Cogan, MD

Date of Birth February 14, 1908

Place of Birth Fall River, Massachusetts

Nationality U.S. Citizen

Education

Dartmouth College	A.B.	1929
Harvard Medical School	M.D.	1932
Chicago University Clinics	Internship	1932-1933
Massachusetts Eye and Ear Infirmary	Residency	1933-1935

Professional Experience

Senior Medical Officer, Clinical Branch, National Eye Institute, National Institutes of Health, 1985-date

Chief, Neuro-Ophthalmic Section, National Eye Institute, National Institutes of Health, 1973-1985

Henry Willard Williams Professor of Ophthalmology, Harvard Medical School, 1962-1974; Emeritus, 1974-date

Chairman, Department of Ophthalmology, Harvard Medical School, 1962-1968

Professor of Ophthalmology, Harvard Medical School, 1955-1974

Surgeon in Ophthalmology, Massachusetts Eye and Ear Infirmary, 1954-1962

Consultant, Children's Hospital Medical Center, Boston, 1954-1973

Director, Eye Pathology Laboratory, Massachusetts Eye and Ear Infirmary, 1954-1956

Consultant-Instructor in Ophthalmology, U.S. Naval Hospital, Chelsea, Massachusetts, 1951-1962

Consultant, Los Alamos Medical Center, New Mexico, 1950-1955

Director of Ophthalmic Laboratories, Massachusetts Eye and Ear Infirmary, 1947-1956

Associate Surgeon in Ophthalmology, Massachusetts Eye and Ear Infirmary, 1945-1954

Associate Professor of Ophthalmic Research, Harvard Medical School, 1943-1955

Director, Howe Laboratory, Harvard Medical School, 1943-1973

Acting Director, Howe Laboratory, Harvard Medical School, 1940-1943

Assistant Professor of Ophthalmic Research, Harvard Medical School, 1940-1943

Assistant Surgeon in Ophthalmology, Massachusetts Eye and Ear Infirmary, 1939-1945

Clinical Assistant in Ophthalmology, Massachusetts Eye and Ear Infirmary, 1935-1939

Assistant in Ophthalmology, Harvard Medical School, 1934-1940

Extracurricular Appointments

Editorial Board, *Archives of Ophthalmology*, 1941-1960; Chief Editor, 1960-1966

Committee on Ophthalmic Consultants, National Research Council, 1949-1958

Consultant, Executive Council, Committee on Research, National Society for Prevention of Blindness, Inc., 1951-date

Consultant, World Health Organization, 1953

Graduate Medical Training Grant Committee, National Institute of Neurological Diseases and Blindness, NIH, 1953-1955

Committee on Courses for Graduates, Harvard Medical School, 1954-1958

Honorary Medical Staff, Los Alamos Medical Center, New Mexico, 1955-1959

Member, Advisory Council, National Institute of Neurological Diseases and Blindness, NIH, 1955-1959

Committee on Radiation Cataracts, National Research Council, Division of Medical Science, 1955-1960

Chairman, George Washington University Microwaves Study Group, 1956

Administrative Board of the Faculty of Medicine, Harvard Medical School, 1959-1962

Member, Advisory Council, Panel of Ophthalmology, Medical Radio System, 1960

Member, Board of Directors, National Society for Prevention of Blindness, Inc., 1962-1965

President, Massachusetts Eye and Ear Infirmary Alumni Association, 1968

Chairman of the Trustees, Association of University Professors of Ophthalmology, 1968

Consulting Chief of Ophthalmology, Massachusetts Eye and Ear Infirmary, 1968-1973

Member, Advisory Board of Editors, *Journal of the Neurological Sciences*, 1968-1977

Editorial Board, *Investigative Ophthalmology*, 1969-1972

Member, National Advisory Eye Council, NEI, NIH, 1969-1972

Chairman, Committee on Basic and Clinical Research, National Society for Prevention of Blindness, Inc., 1969-1971

Editor, *Albrecht von Gräefes Archiv für Klinische und Experiementelle Ophthalmologie*, 1972-1978; Managing Editor, 1978-date

Consultant with Office of Associate Director, Collaborative and Field Research, National Institute of Neurological Disease and Stroke, NIH, 1973

Scientific Advisory Committee, Research to Prevent Blindness, Inc., 1975-date

Honorary President, Second International Congress of Neuro-Ophthalmology, 1973

President, Association of Ophthalmic Alumni, Armed Forces Institute of Pathology, 1978-1979

Guest of Honor, American Academy of Ophthalmology, 1978

Visiting Committee, Department of Psychology, Massachusetts Institute of Technology, 1978

Guest of Honor, Association for Research in Vision and Ophthalmology, 1978

Guest of Honor, Pennsylvania Academy of Ophthalmology and Otolaryngology, 1979

AOA Visiting Professor, Albany Medical College, 1980

Guest of Honor, Fourth International Congress of Neuro-Ophthalmology, 1982

Consulting Editor, *Journal of Neurological Science*, 1983

Trustee, Massachusetts Eye and Ear Infirmary, 1984

Memberships

American Academy of Arts and Sciences

American Neurologic Association, Honorary

American Academy of Ophthalmology

American Ophthalmological Society

American Medical Association

American Society for Clinical Research

Association for Research in Vision and Ophthalmology

Massachusetts Medical Society

National Society for the Prevention of Blindness

New England Ophthalmological Society, President 1960
Canadian Ophthalmology Society, Honorary
Swedish Ophthalmology Society, Honorary
Irish Ophthalmological Society, Honorary
Washington, D.C. Ophthalmological Society, Honorary
Japanese Ophthalmological Society, Honorary
Campagnie des Mousquetaires de l'Armagnac
Cosmos Club
Club Jules Gonin
Deutsche Ophthalmologische Gesellshaft, Honorary

Honors

Moseley Traveling Fellowship, 1937
Warren Triennial Prize of the Massachusetts General Hospital, 1944
New England Ophthalmological Society Annual Prize, 1953
Knapp Medal of the American Medical Association, 1955
Hektoen Silver Medal of the American Medical Association, 1961
Howe Medal, American Ophthalmological Society, 1965
Howe Gold Medal, University of Buffalo, 1967
Mackenzie Medal, University of Glasgow, Scotland, 1968
Trustees Award for Outstanding Ophthalmic Achievement, Research to Prevent Blindness, Inc., 1969
Gonin Medal, International Council of Ophthalmology, 1974
First Derrick Vail Award, 1976
The Johns Hopkins University Centennial Fellow, 1976
First Castroviejo Medal, 1978
Honorary Award, Association for Research in Vision and Ophthalmology, 1979
NIH Director's Award, 1983
Alexander von Humboldt-Stiftung, 1988
Lighthouse Pisart Vision Award, 1988
Honorary Doctor of Science, Duke University Medical School, 1989
Michaelson Medal, Israel Academy of Sciences, 1989

BIBLIOGRAPHY

1. Cogan DG: Uveoparotid fever. Case report. *Am J Ophthalmol* 18:637-640, 1935.
2. Cogan DG, Cogan FC: Dinitrophenol cataract. *JAMA* 105:793-795, 1935.
3. Cogan DG, Cogan FC: Cataracts and dinitrophenol. *N Engl J Med* 213:345-350, 1935.
4. Cogan DG, Cogan FC: Color fatigue in peripheral visual field. *Ophthalmologica* 96:137-154, 1938.
5. Cogan DG: Accommodation and autonomic nervous system. *Arch Ophthalmol* 18:739-762, 1939.
6. Cogan DG, Cogan FC: Recovery time from color fatigue in peripheral visual field. *Ophthalmologica* 96:267-276, 1939.
7. Cogan DG: Ophthalmology. Medical progress. *N Engl J Med* 221:62-65, 1939.
8. Cogan DG: Tetanus and the prophylactic use of antitoxin following injuries of the eye. *Am J Ophthalmol* 222:1365-1367, 1939.
9. Cogan DG: Ophthalmology. Medical progress. *N Engl J Med* 222:716-720, 1940.
10. Cogan DG: Experimental production of so-called bullous keratitis. *Arch Ophthalmol* 23:918-925, 1940.
11. Cogan DG: Waltzing guinea pigs with particular reference to ocular movements and righting reflexes. *Arch Ophthalmol* 24:78-82, 1940.
12. Cogan DG: Self-luminous placido disc. *Am J Ophthalmol* 23:914-915, 1940.
13. Cogan DG: Popular misconceptions pertaining to ophthalmology. *N Engl J Med* 224:462-466, 1941.
14. Cogan DG: Some ocular phenomena produced with polarized light. *Arch Ophthalmol* 25:391-400, 1941.
15. Cogan DG: Some practical considerations pertaining to corneal edema. *Arch Ophthalmol* 25:552-556, 1941.
16. Cogan DG: Bullous keratitis: With particular reference to the pathology of experimental corneal vascularization. *Arch Ophthalmol* 25:941-968, 1941.
17. Cogan DG: A simplified entoptic pupillometer. *Am J Ophthalmol* 24:1431-1433, 1941.
18. Kinsey VE, Grant WM, Cogan DG: Water movement and the eye. *Arch Ophthalmol* 27:242-245, 1942.
19. Cogan DG, Kinsey VE: The cornea. I. Transfer of water and sodium chloride by osmosis and diffusion through the excised cornea. *Arch Ophthalmol* 27:466-476, 1942.
20. Cogan DG, Kinsey VE: The cornea. II. Transfer of water and sodium chloride by hydrostatic pressure through the excised cornea. *Arch Ophthalmol* 27:696-704, 1942.
21. Cogan DG, Kinsey VE: Physiologic studies on the cornea. *Science* 95:607-608, 1942.
22. Kinsey VE, Grant WM, Cogan DG: Sodium, chloride and phosphorous movement and the eye. *Arch Ophthalmol* 27:1126-1131, 1942.
23. Kinsey VE, Cogan DG: The cornea. III. Hydration properties of excised corneal pieces. *Arch Ophthalmol* 28:272-284, 1942.
24. Cogan DG, Kinsey VE: The cornea. IV. Hydration properties of the whole cornea. *Arch Ophthalmol* 28:449-463, 1942.

25. Cogan DG, Kinsey VE: The cornea. V. Physiologic aspects. *Arch Ophthalmol* 28:661-669, 1942.

26. Cogan DG: The waltzing (circuling) phenomenon in rabbits. *J Comp Psych* 35:111-117, 1943.

27. Cogan DG: Clearing of edematous corneas by glycerine. *Am J Ophthalmol* 26:551, 1943.

28. Cogan DG: Lewisite burns of the eye. *JAMA* 122:435-436, 1943.

29. Kinsey VE, Drinker P, Cogan DG: Measuring eye flash from arc welding. *JAMA* 123:403-404, 1943.

30. Cogan DG, Kinsey VE, Drinker P: Infra-red therapy of flash keratoconjunctivitis. *JAMA* 123:883, 1943.

31. Cogan DG, Hirsch EO, Kinsey VE: The cornea. VI. Permeability characteristics of the excised cornea. *Arch Ophthalmol* 31:408-412, 1944.

32. Cogan DG, Hirsch EO: The cornea. VII. Permeability to weak electrolytes. *Arch Ophthalmol* 32:276-282, 1944.

33. Cogan DG, Grant WM: An unusual type of keratitis associated with exposure to N-butyl alcohol (butanol). *Arch Ophthalmol* 33:106-109, 1945.

34. Cogan DG: Syndrome of non-syphilitic interstitial keratitis and vestibuloauditory symptoms. *Arch Ophthalmol* 33:144-149, 1945.

35. Cogan DG, Grant WM: Correspondence. Keratitis due to N-butyl alcohol. *Arch Ophthalmol* 34:248, 1945.

36. Cogan DG, Robbins LL, Aub JC, et al: Superficial "burns" of skin and eyes from scattered cathode rays. *Radiology* 46:1-23, 1946.

37. Holt M, Cogan DG: The cornea. Permeability of the excised cornea to ions, as determined by measurements of impedance. *Arch Ohthalmol* 35:292-298, 1946.

38. Cogan DG, Kinsey VE: Action spectrum of keratitis produced by ultraviolet radiation. *Arch Ophthalmol* 35:670-677, 1946.

39. Cogan DG, Grant WM: *Biochemistry, Pharmacology and Toxicology. Ophthalmology in the War Years.* vol I (1940-1943), Weiner M (ed), Chicago, Year Book Pub, Inc, 1946.

40. Cogan DG: Aims and aids in the teaching of basic sciences in ophthalmology. *Arch Ophthalmol* 37:428-432, 1947.

41. Cogan DG, Grant WM: Correspondence. Corneal and conjunctival pigmentation among workers in hydroquinone. *Arch Ophthalmol* 38:847, 1947.

42. Cogan DG: Neurologic significance of lateral conjugate deviation of the eyes on forced closure of the lids. *Arch Ophthalmol* 39:37-42, 1948.

43. Cogan DG: Students' model for demonstration of action of the extraocular muscles. *Arch Ophthalmol* 39:92-93, 1948.

44. Cogan DG, Albright F, Bartter FC: Hypercalcemia and band keratopathy. Report of nineteen cases. *Arch Ophthalmol* 40:624-638, 1948.

45. Cogan DG, Grant WM: *Biochemistry, Pharmacology and Toxicology. Ophthalmology in the War Years.* vol II (1944-1946), Weiner M (ed), Chicago, Year Book Pub, Inc, 1948.

46. Cogan DG: *Pediatric Ophthalmology. The Child in Health and Disease.* Grulee CG, Grey RC (eds), Baltimore, Williams & Wilkins, 1948.

47. Cogan DG: *Neurology of the Ocular Muscles.* Springfield, IL, Thomas, 1948.

48. Cogan DG, Loeb DR: Optokinetic response and intracranial lesions. *Arch Neurol Psychiat* 61:183-187, 1949.

49. Cogan DG: Vascularization of the cornea. Its experimental induction by small lesions and a new theory of its pathogenesis. *Arch Ophthalmol* 41:406-416, 1949.

50. Cogan DG: Studies of the clinical physiology of the cornea. The interrelationship of corneal tugescence, epithelial edema, bullous keratopathy, and interstitial vascularization. The Third Frances I. Proctor Lecture. *Am J Ophthalmol* 32:625-633, 1949.

51. Cogan DG, Grant WM: Treatment of pediculososis ciliaris with anticholinesterase agents. Report of a case. *Arch Ophthalmol* 41:627-628, 1949.

52. Cogan DG: Nonsyphilitic interstitial keratitis with vestibuloauditory symptoms. Report of four additional cases. *Arch Ophthalmol* 42:42-49, 1949.

53. Cogan DG: A simple method for temporary closure of the lids. *Arch Ophthalmol* 42:188, 1949.

54. Cogan DG: A new method for studying endothelial regeneration. Biomicroscopic observations of normal and vitamin C deficient animals. *Ophthalmologica* 118:440-443, 1949.

55. Cogan DG: Endogenous intraocular fungus infection. *Arch Ophthalmol* 42:666-668, 1949.

56. Cogan DG, Martin SF, Kimura SJ: Atom bomb cataracts. *Science* 110:654-655, 1949.

57. Cogan DG: Untersuchungen zur klinischen physiologie der hornhaut: Die beziehungen zwischen hornhautepithelödem, keratopathia bullosa und interstitieller vascularisation. Bericht über die 54. *Zusammenkunft der Deutschen Ophthalmologischen Gesellschaft* 54:6-13, 1949.

58. Cogan DG: Hypercalcämie und bandförmige keratopathie. Bericht über die 54. *Zusammenkunft der Deutschen Ophthalmologischen Gesellschaft* 54:111-114, 1949.

59. Cogan DG: Propositos e auxilios do ensino das cienias basicas em oftalmologia. *Ophthalmologia Ibero-America* 11:13-17, 1949.

60. Cogan DG: Principos de patologia de la cornea. *Anales de las Sociedad Mexicana de Oftalmologia* 22:87-98; 167-174, 1948. 23:32-37, 1949.

61. Cogan DG: Lesions of the eye from radiant energy. *JAMA* 142:145-151, 1950.

62. Donaldson DD, Cogan DG: A fixation aid for slit lamp biomicroscopy. *Arch Ophthalmol* 43:555-556, 1950.

63. Cogan DG: Book review. *Vision: Its Development in Infant and Child,* by Gesell A, New York, Hoeber, 1949.

64. Cogan DG: Symposium. Primary chorioretinal aberrations with night blindness. Pathology. *Trans Am Acad Ophthalmol Otolaryngol* 54:629-661, 1950.

65. Cogan DG: Book review. *Practical Neurological Diagnosis,* by Spurling RG. *Arch Ophthalmol* 44:353, 1950.

66. Cogan DG, Kubik CS, Smith WL: Unilateral internuclear ophthalmoplegia. Report of eight clinical cases with one postmortem study. *Arch Ophthalmol* 44:783-796, 1950.

67. Cogan DG, Martin SF, Kimura SJ, et al: Ophthalmologic survey of atomic bomb survivors in Japan, 1949. *Trans Am Ophthalmol Soc* 48:62-67, 1950.

68. Cogan DG: Japan - after the bomb. *Harvard Alumni Bull* Jan 13, 1951.

69. Cogan DG: Book review. *The Cerebral Cortex of Man: A Clinical Study of Localization of Function,* by Penfield W, Rasmussen T, New York, Macmillan, 1950.

70. Cogan DG: Applied anatomy and physiology of the cornea. *Trans Acad Ophthalmol Otolaryngol* 55:329-359, 1951.

71. Cogan DG: Editorial. Registry of interesting cases. *Arch Ophthalmol* 45:507, 1951.

72. Cogan DG, Donaldson DD: Experimental radiation cataracts. I. Cataracts in the rabbit following single x-ray exposure. *Arch Ophthalmol* 45:508-522, 1951.

73. Cogan DG: Editorial. The Atlantic City meetings. *Arch Ophthalmol* 46:1-3, 1951.

74. Cogan DG: Recurrent bilateral scleritis of many years' duration with unusual corneal infiltration (Fat?). *Arch Ophthalmol* 46:341-342, 1951.

75. Cogan DG: Divergence paralysis and paralysis of the near reflex associated with a malnutrition syndrome (presumably Wernicke's polioencephalitis). *Arch Ophthalmol* 46:436-437, 1951.

76. Cogan DG: Radiant energy. In *Systemic Ophthalmology.* Sorsby A (ed), St. Louis, Mosby, 1951.

77. Cogan DG, Donaldson DD, Reese AB: Clinical and pathological characteristics of radiation cataract. *Arch Ophthalmol* 47:55-70, 1952.

78. Cogan DG, Ginsberg J: Representation of corneal and conjunctival sensation in the central nervous system. *Arch Ophthalmol* 47:273-275, 1952.

79. Cogan DG: Editorial. Inter-group research. *Arch Ophthalmol* 47:415, 1952.

80. Cogan DG: Book review. *The Clinical Pathology of the Eye,* by Samuels B, Fuchs A, New York, Hoeber, 1952. *Arch Ophthalmol* 47:413, 1952.

81. Cogan DG, Donaldson DD: Correspondence. Optokinetic responses. *Arch Ophthalmol* 47:538, 1952.

82. Cogan DG, Goff JL, Graves E: Experimental radiation cataract. II. Cataract in the rabbit following single exposure to fast neutrons. *Arch Ophthalmol* 47:584-592, 1952.

83. Giller H, Cogan DG: Papilledema as the outstanding sign in meningeal hydrops. *Arch Ophthalmol* 48:557-566, 1952.

84. Cogan DG: *Pediatric Ophthalmology. The Child in Health and Disease.* Grulee CG, Eley RC (eds), Baltimore, Williams & Wilkins, 1952.

85. Cogan DG: A type of congenital ocular motor apraxia presenting jerky head movements. *Am J Ophthalmol* 36:433-441, 1953.

86. Cogan DG, Dreisler KK: Minimal amount of x-ray exposure causing lens opacities in the human eye. *Arch Ophthalmol* 50:30-34, 1953.

87. Cogan DG, Adams RD: A type of paralysis of conjugate gaze (ocular motor apraxia). *Arch Ophthalmol* 50:434-442, 1953.

88. Cogan DG, Donaldson DD, Goff JL, et al: Experimental radiation cataract. III. Further experimental studies on x-ray and neutron irradiation of the lens. *Arch Ophthalmol* 50:597-602, 1953.

89. Cogan DG: Clinical physiology of the retina. *Bull Ophthalmol Soc Egypt* 46:484-498 1953.

90. Cogan DG: Lesions of the eye from radiant energy. *Bull Ophthalmol Soc Egypt* 46:458-473 1953.

91. Cogan DG, Victor M: Ocular signs of Wernicke's disease. *Arch Ophthalmol* 51:204-211, 1954.

92. Cogan DG: Ocular dysmetria, flutter-like oscillations of the eyes and opsoclonus. *Arch Ophthalmol* 51:318-335, 1954.

93. Cogan DG, Barrows LJ: Platybasia and the Arnold-Chiari malformation. *Arch Ophthalmol* 52:13-29, 1954.

94. Cogan DG, Kuwabara T: Metastatic carcinoma to eye from breast. *Arch Ophthalmol* 52:240-249, 1954.

95. Cogan DG, Kuwabara T: Lipogenesis by cells of the cornea. *Science* 120:321-322, 1954.

96. Norton EWD, Cogan DG: Spasmus nutans. A clinical study of twenty cases followed two years or more since onset. *Arch Ophthalmol* 52:442-446, 1954.

97. Brockhurst RJ, Hynes EA, Cogan DG: Crystalline dystrophy of the cornea. *Arch Ophthalmol* 52:458-459, 1954.

98. Cogan DG, Kuwabara T: Fat formation and foam cells following injection of brain tissue into the cornea. *J Neuropathol Exp Neurol* 14:95-107, 1955.

99. Cogan DG: Remarks made on the acceptance of the Proctor Medal Award, June 1954. *Am J Ophthalmol* 39:3-4, 1955.

100. Cogan DG: Experimental implants of conjunctiva into the anterior chamber. *Am J Ophthalmol* 39:165-172, 1955.

101. Cogan DG, Kuwabara T: Lipogenesis by cells of cornea. *Arch Pathol* 59:453-456, 1955.

102. Cogan DG, Adams RD: Correspondence. Balint's syndrome and ocular motor apraxia. *Arch Ophthalmol* 53:758, 1955.

103. Cogan DG, Fink R, Donaldson DD: X-ray irradiation of orbital glands of the rabbit. *Radiology* 64:731-737, 1955.

104. Cogan DG, Freese CG, Jr: Spasm of the near reflex. *Arch Ophthalmol* 54:752-759, 1955.

105. Cogan DG, Kuwabara T: Fat deposition in the cornea and lipogenesis. *Acta XVII Concilium Ophthalmologicum* 1:523-535, 1955.

106. Cogan DG, Kinsey VE: Criticism of the deturgescence theory of corneal transparency. *Acta XVII Concilium Ophthalmologicum* 1:483-489, 1955.

107. Cogan DG: Action of x-rays and radioactive substances on ocular tissues. In *Modern Trends in Ophthalmology*. Sorsby A (ed), London, Butterworth, 1955.

108. Cogan DG: Corneoscleral lesions in periarteritis Nodosa and Wegner's granulomatosis. *Trans Ophthalmol Soc* 53:321-344, 1955.

109. Cogan DG, Kinsey VE, Becker B: Obituary. Jonas S. Friedenwald, MD, 1897-1955. *Arch Ophthalmol* 55:155-157, 1956.

110. Cogan DG, Kuwabara T, Kinoshita J, et al: Ocular manifestations of systemic cystinosis. *Arch Ophthalmol* 55:36-41, 1956.

111. Cogan DG: *Neurology of the Ocular Muscles.* ed 2, Springield, IL, Thomas, 1956.

112. Lincoff HA, Cogan DG: Unilateral headache and oculomotor paralysis not caused by aneurysm. *Arch Ophthalmol* 57:181-189, 1957.

113. Tucker DP, Steinberg AG, Cogan DG: Frequency of genetic transmission of sporadic retinoblastoma. *Arch Ophthalmol* 57:532-535, 1957.

114. Cogan DG, Kuwabara T: Experimental aberrant lipogenesis. I. Serum factor. *Arch Ophthalmol* 63:381-386, 1957.

115. Cogan DG, Kuwabara T, Kinoshita J, et al: Cystinosis in an adult. *JAMA* 164:394-396, 1957.

116. Kuwabara T, Cogan DG: Experimental aberrant lipogenesis. II. Substrate factor. *Arch Pathol* 63:496-501, 1957.

117. Cogan DG, Gorman JJ, Gellis SS: An apparatus for grading the visual acuity of infants on the basis of opticokinetic nystagmus. *Pediatrics* 19:1088-1092, 1957.

118. Cogan DG, Kuwabara T: Experimental aberrant lipogenesis. III. Tissue factor. *Arch Ophthalmol* 64:23-33, 1957.

119. Cogan DG, Henneman PH: Diffuse calcification of the cornea in hypercalcemia. *N Engl J Med* 257:451-453, 1957.

120. Cogan DG: Comparison of fat deposits in the cornea and blood vessels. *Proc N Engl Cardiovas Soc* 15:37-38, 1956-1957.

121. Cogan DG, Hurlbut CS, Kuwabara T: Crystalline calcium sulphate (gyppsum) in scleral plaques of a human eye. *J Histochem* 6:142-145, 1958.

122. Cogan DG, Kuwabara T, Hurlbut CS, et al: Further observations on cystinosis in the adult. *JAMA* 166:1725-1726, 1958.

123. Cogan DG: Some objective and subjective observations on the vestibulo-ocular system. *Am J Ophthalmol* 45:74-78, 1958.

124. Cogan DG, Kuwabara T, Parke D: Epibulbar nevoxanthoendotheliomas. *Arch Ophthalmol* 59:717-721, 1958.

125. Cogan DG: Amaurotic family idiocy. A case for eponyms. *N Engl J Med* 258:1212-1213, 1958.

126. Cogan DG, Kuwabara T: Some common artifacts in the retina. *J Histochem* 6:290-293, 1958.

127. Cogan DG: Editorial. Through a glass, darkly. *N Engl J Med* 259:247, 1958.

128. Cogan DG: Ocular effects of radiation. *N Engl J Med* 259:517-520, 1958.

129. Cogan DG, Kuwabara T, Richardson PE, et al: Histochemistry of the eye in metachromatic leukoencephalopathy. *Arch Ophthalmol* 60:397-402, 1958.

130. Cogan DG, Kuwabara T, Silbert J, et al: Calcium oxalate and calcium phosphate in detached retinas. *Arch Ophthalmol* 60:360-371, 1958.

131. Reinecke RD, Cogan DG: Standardization of objective visual acuity measurements. Opticokinetic nystagmus vs. Snellen acuity. *Arch Ophthalmol* 60:418-421, 1958.

132. Cogan DG, Kuwabara T: Lipid keratopathy and atheroma. *Circulation* 18:519-525, 1958.

133. Cogan DG, Fricker SJ, Lubin M, et al: Cataracts and ultra-high-frequency radiation. *Arch Ind Hlth* 18:299-302, 1958.

134. Cogan DG: Radiant energy. In *Systemic Ophthalmology*. ed 2, Sorsby A (ed), St. Louis, Mosby, 1958.

135. Kuwabara T, Futterman S, Cogan DG, et al: Dehydrogenases in the retina and Müller's fibers. *J Histochem Cytochem* 7:67-68, 1959.

136. Kuwabara T, Cogan DG: Experimental aberrant lipogenesis. IV. Production of nonsudanophilic fat. *Arch Pathol* 67:34-38, 1959.

137. Cogan DG, Kuwabara T: Ocular changes in experimental hypercholesteremia. *Arch Ophthalmol* 61:219-225, 1959.

138. Sweebe EC, Cogan DG: Adenocarcinoma of the meibomian gland. A pseudochalazion entity. *Arch Ophthalmol* 61:282-290, 1959.

139. Cogan DG, Kuwabara T, Howard J: The nonelastic nature of pingueculas. *Arch Ophthalmol* 61:388-389, 1959.

140. Cogan DG, Kuwabara T: Histochemistry of the retina in Tay-Sachs disease. *Arch Ophthalmol* 61:414-423, 1959.

141. Cogan DG, Kuwabara T: Further observations on lipid crystals in tissue. *J Histochem Cytochem* 7:80-82, 1959.

142. Cogan DG, Kuwabara T: Arcus senilis. Its pathology and histochemistry. *Arch Ophthalmol* 61:553-560, 1959.

143. Smith JL, Cogan DG: Internuclear ophthalmoplegia. A review of 58 cases. *Arch Ophthalmol* 61:687-694, 1959.

144. Norton EWD, Cogan DG: Syndrome of nonsyphilitic interstitial keratitis and vestibuloauditory symptoms. A long-term follow-up. *Arch Ophthalmol* 61:695-697, 1959.

145. Cogan DG: Editorial. The symposium on the transparency of the cornea. *Arch Ophthalmol* 61:679-680, 1959.

146. Cogan DG, Smith JL, Zieper IH: Observations on ophthalmodynamometry. *JAMA* 170:1403-1407, 1959.

147. Cogan DG: Symposium. Ocular effects of ionizing radiation and radiotherapy. Basic concepts: Orientation. *Trans Am Acad Ophthalmol Otolaryngol* 63:429-432, 1959.

148. Cogan DG, Smith JL: Optokinetic nystagmus: A test for parietal lobe lesions. A study of 31 anatomically verified cases. *Am J Ophtalmol* 48:187-193, 1959.

149. Cogan DG: Convergence nystagmus. With notes on a single case of divergence nystagmus. *Arch Ophthalmol* 62:295-299, 1959.

150. Cogan DG, Gorman JJ, Gellis SS: A device for testing the visual acuity in infants. *Sight-Sav Rev* 29:80-84, 1959.

151. Smith JL, Cogan DG: Ataxia-telangiectasia. *Arch Ophthalmol* 62:364-369, 1959.

152. Kuwabara T, Cogan DG: Tetrazolium studies on the retina: I. Introduction and technique. *J Histochem Cytochem* 7:329-333, 1959.

153. Cogan DG, Kuwabara T: Tetrazolium studies on the retina: II. Substrate dependent patterns. *J Histochem Cytochem* 7:334-341 (Sept) 1959.

154. Smith JL, Cogan DG: Ophthalmodynamometric diagnosis of acute renal hypertension in pulseless syndrome. *Am J Ophthalmol* 48:326-329, 1959.

155. Cogan DG: Ocular effects of radiation. *Arch Ind Hlth* 20:293-296, 1959.

156. Cogan DG, Kuwabara T: Focale senile translucency of the sclera. *Arch Ophthalmol* 62:604-610, 1959.

157. Smith JL, Gay AJ, Cogan DG: The spasticity of conjugate gaze phenomenon. *Arch Ophthalmol* 62:694-696, 1959.

158. Cogan DG, Smith JL, Zieper I, et al: Nystagmus retractorius. *Arch Ophthalmol* 62:864-867, 1959.

159. Cogan DG, Kuwabara T: Experimental lipogenesis by purkinje fibers. *J Biophys Biochem Cytol* 6:519-522, 1959.

160. Smith JL, Cogan DG: The ophthalmodynamometric posture test. *Am J Ophthalmol* 48:735-740, 1959.

161. Cogan DG, Kuwabara T: Lipid keratopathy and atheroma. *Trans Am Ophthalmol Soc* 56:109-122, 1959.

162. Cogan DG, Kuwabara T: Ocular pathology of cystinosis with particular reference to the elusiveness of the corneal crystals. *Arch Ophthalmol* 63:51-57, 1960.

163. Cogan DG: Radiation cataract in man. Symposium on the delayed effects of whole body radiation. Walter Reed Army Institute of Research and Operations Research Office, 1960.

164. Kuwabara T, Cogan DG: Tetrazolium studies on the retina. III. Activity of metabolic intermedicates and miscellaneous substrates. *J Histochem Cytochem* 8:214-224, 1960.

165. Cogan DG: Editorial. Something new on the horizon. *Arch Ophthalmol* 63:909, 1960.

166. Cogan DG, Lubin M, Curtis GW, et al: Effects of ultra-high-frequency radiation on animals, *Arch Ind Hlth* 21:555-558, 1960.

167. Cogan DG, Kuwabara T: Aberrant lipogenesis. *Nutrition Rev* 18:225-227, 1960.

168. Cogan DG, Kuwabara T: Tetrazolium studies on the retina: IV. Reductase in ocular tissue. *J Histochem* 8:380-384, 1960.

169. Cogan DG: Editorial. What the ophthalmic pathologists talked about. *Arch Ophthalmol* 64:319-320, 1960.

170. Cogan DG: Editorial. What some anatomists are talking about. *Arch Ophthalmol* 64:321-323, 1960.

171. Cogan DG: Editorial. Cell migration and autoradiography. *Arch Ophthalmol* 64:479, 1960.

172. Goldstein JE, Cogan DG: Diabetic ophthalmolplegia with special reference to the pupil. *Arch Ophthalmol* 64:592-600, 1960.

173. Cogan DG: Editorial. Another Georgia incident. *Arch Ophthalmol* 64:649, 1960.

174. Cogan DG: Editorial. Sensitization of the eye, Freund's adjuvant, and uveitis. *Arch Ophthalmol* 64:653-654, 1960.

175. Cogan DG, Kuwabara T: Studies of retinal vascular patterns. I. Normal architecture. *Arch Ophthalmol* 64:904-911, 1960.

176 Cogan DG: Hemianopia and associated symptoms due to parietotemporal lobe lesions. *Am J Ophthalmol* 50:1056-1066, 1960.

177. Cogan DG: Some aspects of fat degeneration. The transparency of the cornea. *Symposium Organized by the Council for International Organizations of Medical Sciences*. Duke-Elder S, Perkins ES (eds), Oxford, Blackwell, 1960.

178. Cogan DG: Neutral fat synthesis by nonadipose tissue. In *Diseases and the Advancement of Basic Science*. Beecher HK (ed), Cambridge, Harvard University Press, 1960.

179. Cogan DG, Kuwabara T: Retinal dehydrogenases. *Trans Am Ophthalmol Soc.* 57:153-157, 1960.

180. Cogan DG: Editorial. Hypoparathyroidism, pseudohypoparathyroidism, and pseudopseudohypoparathyroidism. *Arch Ophthalmol* 65:1, 1961.

181. Hill K, Dodge PR, Cogan DG: Ocular signs associated with hydranencephaly. *Am J Ophthalmol* 51:267-275, 1961.

182. Cogan DG: Editorial. Publication explosion. *Arch Ophthalmol* 65:319-320, 1961.

183. Cogan DG: Editorial. Russian ophthalology. *Arch Ophthalmol* 65:320-322, 1961.

184. Cogan DG: Editorial. Manuscript references. *Arch Ophthalmol* 65:473, 1961.

185. Cogan DG, Poppen JL, Hicks SP: Ganglioneuroma of chiasm and optic nerves. *Arch Ophthalmol* 65:481-482, 1961.

186. Toussaint D, Kuwabara T, Cogan DG: Retinal vascular patterns. Part II. Human retinal vessels studied in three dimensions. *Arch Ophthalmol* 65:575-581, 1961.

187. Cogan DG: Editorial. Headaches. *Arch Ophthalmol* 65:619-620, 1961.

188. Cogan DG: Editorial. Colored illustrations. *Arch Ophthalmol* 65:620, 1961.

189. Cogan DG, Kuwabara T: Orbital reaction to sponge (invalon) implant. *Arch Ophthalmol* 65:646-647, 1961.

190. Kuwabara T, Carroll JM, Cogan DG: Retinal vascular patterns. Part III. Age hypertension, absolute glaucoma, injury. *Arch Ophthalmol* 65:708-716, 1961.

191. Cogan DG: Editorial. The pathologists again. *Arch Ophthalmol* 66:1-2, 1961.

192. Cogan DG: Editorial. Future ophthalmologists. *Arch Ophthalmol* 66:155-156, 1961.

193. Cogan DG: Editorial. School vision testing. *Arch Ophthalmol* 66:156, 1961.

194. Cogan DG: Blackouts not obviously due to carotid occlusion. *Arch Ophthalmol* 66:180-187, 1961.

195. Cogan DG: Editorial. A matter of editorial policy. *Arch Ophthalmol* 66:299-300, 1961.

196. Cogan DG: Editorial. Ocular muscles. *Arch Ophthalmol* 66:300-301, 1961.

197. Cogan DG, Toussaint D, Kuwabara T: Retinal vascular patterns. IV. Diabetic retinopathy. *Arch Ophthalmol* 66:366-378, 1961.

198. Cogan DG: Obituary. Dr. Edward Chiles Sweebe. *Arch Ophthalmol* 66:451, 1961.

199. Cogan DG, Merola L, Laibson PR: Blood viscosity, serum hexasamine and diabetic retinopathy. *Diabetes* 10:393-395, 1961.

200. Cogan DG: Editorial. Cataracts caused by corticosteroids. *Arch Ophthalmol* 66:455, 1961.

201. Cogan DG: Editorial. Ophthalmic heritage. *Arch Ophthalmol* 66:456-457, 1961.

202. Goldstein JE, Cogan DG: Lateralizing value of ocular motor dysmetria and skew deviation. *Arch Ophthalmol* 66:517-518, 1961.

203. Cogan DG: Editorial. Photosensitization and cataracts. *Arch Ophthalmol* 66:612-613, 1961.

204. Kuwabara T, Cogan DG: Retinal glycogen. *Arch Ophthalmol* 66:680-688, 1961.

205. Cogan DG: Editorial. Electron microscopy. *Arch Ophthalmol* 66:772-773, 1961.

206. Cogan DG: Editorial. Inborn errors. *Arch Ophthalmol* 66:771-772, 1961.

207. Cogan DG: Nystagmus. In *Traumatic Medicine and Surgery for the Attorney*. London, Butterworth, 1961.

208. Cogan DG: Papilledema. In *Traumatic Medicine and Surgery for the Attorney*. London, Butterworth, 1961.

209. Toussaint D, Kuwabara T, Cogan DG: Extravascular lesions of diabetic retinopathy. *Arch Ophthalmol* 67:42-47, 1962.

210. Cogan DG: Editorial. A decade of NINDB of NIH. *Arch Ophthalmol* 67:120-122, 1962.

211. Cogan DG: Treatment of herpetic keratitis. *Arch Ophthalmol* 67:122, 1962.

212. Cogan DG: Triparanol and medical reporting. *Arch Ophthalmol* 67:397-398, 1962.

213. Cogan DG: Prevention of blindness. *JAMA* 180:238-239, 1962.

214. Reinecke RD, Kuwabara T, Cogan DG, et al: Retinal vascular patterns. V. Experimental ischemia of the cat eye. *Arch Ophthalmol*67:470-475, 1962.

215. Cogan DG: Corneal vascularization. *Invest Ophthalmol* 1:253-261, 1962.

216. Cogan DG: Editorial. Aerospace problems. *Arch Ophthalmol* 67:564, 1962.

217. Cogan DG: Editorial. AFIB and ophthalmology. *Arch Ophthalmol* 67:546-548, 1962.

218. Cogan DG: Editorial. Herpetic keratitis and viral antibiosis. *JAMA* 180:966, 1962.

219. Cogan DG: Editorial. Chromatin, chromosomes, and ophthalmology. *Arch Ophthalmol* 67:697-700, 1962.

220. Goldstein JE, Cogan DG: Sclerocornea and associated congenital anomalies. *Arch Ophthalmol* 67:761-768, 1962.

221. Cogan DG: Anatomy of lens and pathology of cataracts. *Exper Eye Res* 1:291-295, 1962.

222. Cogan DG: Editorial. Entre aveugles. *Arch Ophthalmol* 68:1-2, 1962.

223. Cogan DG, Simmons RJ: Occult temporal arteritis. *Arch Ophthalmol* 68:8-18, 1962.

224. Cogan DG: Editorial. Conference on diabetic retinopathy. *Arch Ophthalmol* 68:2-3, 1962.

225. Cogan DG: Editorial. Cardiac glycerides and glaucoma. *Arch Ophthalmol* 68:155-156, 1962.

226. Cogan DG, Kuwabara T, Richardson EP: Pathology of abiotrophic ophthalmoplegia externa. *Bull Johns Hopkins Hosp* 111:42-56, 1962.

227. Cogan DG: Editorial. Lasers. *Arch Ophthalmol* 68:156-157, 1962.

228. Cogan DG: Retinal architecture and pathophysiology. The Sanford R. Gifford Lecture. *Am J Ophthalmol* 54:347-363, 1962.

229. Cogan DG: Taking the malignancy out of malignany glaucoma. *Arch Ophthalmol* 68:301, 1962.

230. Cogan DG: Editorial. Lens symposium at Oxford. *Arch Ophthalmol* 68:302-303, 1962.

231. Cogan DG: Editorial. Therapeutic hypopituatarism for diabetic retinopathy. *Arch Ophthalmol* 68:579-580, 1962.

232. Knox DL, Cogan DG: Eye pain and homonymous hemianopia. *Am J Ophthalmol* 54:1091-1093, 1962.

233. Cogan DG, Kuwabara T: Pathology of cataracts in mongoloid idiocy. A new concept of the pathogenesis of cataracts of the coronary-cerulean type. *Doc Ophthalmol* 16:73-80,1962.

234. Cogan DG: Nystagmus. Strabismus. *Symposium of the New Orleans Academy of Ophthalmology*. Haik GM (ed), St. Louis, Mosby, 1962.

235. Cogan DG: Editorial. Subclavian steal syndrome. *Arch Ophthalmol* 69:2-3, 1963.

236. Cogan DG: Editorial. Tumors of the eye and adnexae. *Arch Ophthalmol* 69:144-146, 1963.

237. Cogan DG: Editorial. The Stiles-Crawford effect in modern dress. *Arch Ophthalmol* 69:285-286, 1963.

238. Cogan DG: Editorial. High cost of prosperity. *Arch Ophthalmol* 69:286, 1963.

239. Cogan DG: Editorial. NIH under attack. *Arch Ophthalmol* 69:422-423, 1963.

240. Cogan DG: Editorial. Retinotoxic drugs. *Arch Ophthalmol* 69:545-546, 1963.

241. Cogan DG: Editorial. On viewing the eclipse. *Arch Ophthalmol* 69:690-692, 1963.

242. Cogan DG: Editorial. Solar eclipse. *JAMA* 185:38-39, 1963.

243. Cogan DG: Editorial. New departments. Arch Ophthalmol 70:1, 1963.

244. Cogan DG, Kuwabara T: Capillary shunts in the pathogenesis of diabetic retinopathy. *Diabetes* 12:293-300, 1963.

245. Cogan DG: Book review. *Journey Round My Eye,* by Vajda A, London, Hutchinson & Co, 1962. *Arch Ophthalmol* 70:287-289, 1963.

246. Cogan DG: Editorial. Visual problems of space travel. *Arch Ophthalmol* 70:141-142, 1963.

247. Cogan DG: Dissociated nystagmus with lesions in the posterior fossa. *Arch Ophthalmol* 70:361-368, 1963.

248. Cogan DG: Editorial. Most missed diagnosis. *Arch Ophthalmol* 70:446-447, 1963.

249. Cogan DG, Mount HTJ: Intracranial aneurysms causing opthalmoplegia. *Arch Ophthalmol* 70:757-771, 1963.

250. Kuwabara T, Cogan DG: Glycogen in the retina. *Ann Histochem* 8:223-228, 1963.

251. Cogan DG: What in my opinion and experience is an ideal program in research training, and by what pedagogical methods is this best accomplished? *Proceedings of the Second Conference on Graduate Training in Ophthalmology*. United States Government Printing Office, 1963.

252. Cogan DG: Development and senescence of the human retinal vasculature. Doyne Memorial Lecture, 1963. *Trans Ophthalmol Soc UK* 83:465-489, 1964.

253. Cogan DG: Editorial. Feature photo. *Arch Ophthalmol* 71:1, 1964.

254. Cogan DG: Editorial. Horse serum and horse sense. *Arch Ophthalmol* 71:155, 1964.

255. Cogan DG, Dickersin GR: Nonsyphilitic interstitial keratitis with vestibuloauditory symptoms. A case with fatal aortitis. *Arch Ophthalmol* 71:172-175, 1964.

256. Cogan DG: Editorial. Conference on diabetes. *Arch Ophthalmol* 71:295-296, 1964.

257. Cogan DG, Kuwabara T, Moser H: Fat emboli in the retina following angiography. *Arch Ophthalmol* 71:308-313, 1964.

258. Cogan DG: Editorial. Scientific exhibits. *Arch Ophthalmol* 71:453-454, 1964.

259. Cogan DG, Federman DD: Retinal involvement with reticuloendotheliosis of unclassified type. *Arch Ophthalmol* 71:489-491, 1964.

260. Cogan DG: Medical intelligence. Current concepts. Diabetic retinopathy. *N Engl J Med* 270:787-788, 1964.

261. Cogan DG: Editorial. The tissue committee. *Arch Ophthalmol* 71:610, 1964.

262. Cogan DG: Editorial. Heterozygosity. *Arch Ophthalmol* 72:1-3, 1964.

263. Cogan DG: Editorial. Woods-Friedenwald Memorial. *Arch Ophthalmol* 72:153, 1964.

264. Goldstein JE, Cogan DG: Exercise and the optic neuropathy of multiple sclerosis. *Arch Ophthalmol* 72:168-170, 1964.

265. Cogan DG, Kuwabara T: Ocular pathology of the 13-15 trisomy syndrome. *Arch Ophthalmol* 72:246-253, 1964.

266. Cogan DG: Editorial. Commemorating the AOS centenary. *Arch Ophthalmol* 72:295-296, 1964.

267. Cogan DG: Editorial. Melanoma or no. *Arch Ophthalmol* 72:451-452, 1964.

268. Cogan DG: Editorial. Verhoeff Society. *Arch Ophthalmol* 72:589-590, 1964.

269. Cogan DG, Kuwabara T, Young GF, et al: Herpes simplex retinopathy in an infant. *Arch Ophthalmol* 72:641-645, 1964.

270. Cogan DG: Joseph Almarin Capps, 1872-1964. *Harvard Medical Alumni Bull* 39:32-33, 1964.

271. Cogan DG: Digitalis and glaucoma. *Arch Ophthalmol* 72:742, 1964.

272. Cogan DG, Donaldson DD, Kuwabara T, et al: Microcystic dystrophy of the corneal epithelium. *Trans Am Ophthalmol Soc* 62:213-225, 1964.

273. Cogan DG: The retina. Physiologic and anatomic considerations. *University of Miami Neuro-ophthalmology Symposium*. Smith JL (ed), Springfield, IL, Thomas, 1964.

274. Cogan DG: Brain stem and cerebellum. Anatomosemiologic considerations. *University of Miami Neuro-ophthalmology Symposium*. Smith JL (ed), Springfield, IL, Thomas, 1964.

275. Cogan DG: Brain lesions and eye movements in man. In *The Oculomotor System*. Bender MB (ed), New York, Harper & Row, 1964.

276. Cogan DG: Editorial. Alexia without agraphia and the splenum. *Arch Ophthalmol* 73:2-3, 1965.

277. Cogan DG: Editorial. London letter. *Arch Ophthalmol* 73:2-3, 1965.

278. Goldstein JE, Cogan DG: Apraxia of lid opening. *Arch Ophthalmol* 73:155-159, 1965.

279. Cogan DG: Editorial. Editorial help from the American documentation institution. *Arch Ophthalmol* 73:154, 1964.

280. Cogan DG: Obituary. Joseph A. Capps, MD (1872-1964). *The News of Massachusetts General Hospital* 24:6-11, 1965.

281. Cogan DG: Editorial. Two views of choroidal angiosclerosis. *Arch Ophthalmol* 73:309, 1965.

282. Cogan DG: Editorial. Do oral contraceptives have neuro-ophthalmic complications? *Arch Ophthalmol* 73:461-462, 1965.

283. Cogan DG: Editorial. The curse of babel. *Arch Ophthalmol* 73:609-610, 1965.

284. Cogan DG: Ophthalmic manifestations of bilateral non-occipital cerebral lesions. *Brit J Ophthalmol* 49:281-297, 1965.

285. Cogan DG: Editorial. The AMA and the specialties. *Arch Ophthalmol* 73:763-764, 1965.

286. Cogan DG: Editorial. Current concepts of myasthenia gravis. *Arch Ophthalmol* 74:1-3, 1965.

287. Cogan DG: Editorial. Vale Vail. *Arch Ophthalmol* 74:3, 1965.

288. Cogan DG: Editorial. The good and the bad of research support. *Arch Ophthalmol* 74:149-150, 1965.

289. Cogan DG: Myasthenia gravis. A review of the disease and a description of lid twitch as a characteristic sign. *Arch Ophthalmol* 74:217-221, 1965.

290. Cogan DG: Editorial. Letter about Japan. *Arch Ophthalmol* 74:294-296, 1965.

291. Goldstein JE, Peczon JD, Cogan DG: Intraocular pressure and ophthalmodynamometry. *Arch Ophthalmol* 74:175-176, 1965.

292. Cogan DG: Editorial. Chloroquin toxicity. *Arch Ophthalmol* 74:297, 1965.

293. Cogan DG: Editorial. Dislocated lenses and homocystinuria. *Arch Ophthalmol* 74:446-447, 1965.

294. Cogan DG: On the presentation of the Friedenwald Award to Dr. Kinoshita. *Invest Ophthalmol* 4:780-781, 1965.

295. Cogan DG: Editorial. Reticular system. *Arch Ophthalmol* 74:593-594, 1965.

296. Cogan DG: Editorial. Association of University Professors in Ophthalmology. *Arch Ophthalmol* 74:740, 1965.

297. Kuwabara T, Cogan DG: Retinal vascular patterns. VII. Acellular changes. *Invest Ophthalmol* 4:1049-1058, 1965.

298. Cogan DG, Kuwabara T: Photoreceptive abiotrophy of the retina in the elkhound. *Pathol Vet* 2:101-128, 1965.

299. Cogan DG: Heredodegenerations of the retina. Neuro-ophthalmology. *Symposium of the University of Miami and Bascom Palmer Eye Institute*. vol II, Smith JL (ed), St. Louis, Mosby, 1965.

300. Cogan DG: Editorial. Botanical hazards. *Arch Ophthalmol* 75:1, 1966.

301. Cogan DG: Researches into vision. *Arch Ophthalmol* 75:155-156, 1966.

302. Cogan DG: Editorial. Primum non nocere. *Arch Ophthalmol* 75:456-457, 1966.

303. Kupfer C, Cogan DG: Unilateral internuclear ophthalmoplegia. A clinicopathological case report. *Arch Ophthalmol* 75:484-489, 1966.

304. Cogan DG: Editorial. Red and expert. *Arch Ophthalmol* 75:737-738, 1966.

305. Cogan DG, Kuwabara T, Moser H, et al: Retinopathy in a case of Farber's lipogranulomatosis. *Arch Ophthalmol* 75:752-757, 1966.

306. Cogan DG, Kuwabara T: Retinal vessels: Anatomy. *Acta Council Ophthalmologicum* 1:95-103, 1966.

307. Cogan DG: Editorial. Chloroquine retinopathy. *N Engl J Med* 275:730-731, 1966.

308. Cogan DG: Congenital ocular motor apraxia. *Can J Ophthalmol* 1:253-260, 1966.

309. Cogan DG: Ocular correlates of inborn metabolic defects. *Can Med Assoc J* 95:1055-1065, 1966.

310. Cogan DG: International congress of ophthalmology. Munich, August 14-19, 1966. *Arch Ophthalmol* 76:763, 1966.

311. Cogan DG: Foreword. In *Atlas of External Diseases of the Eye.* vol I, Donaldson D, St. Louis, Mosby, 1966.

312. Cogan DG, Kuwabara T: The eye. In *Histology.* Greep RO (ed), New York, McGraw-Hill, 1966.

313. Cogan DG: *Neurology of the Visual System.* Springfield, IL, Thomas, 1966.

314. Cogan DG: Congenital nystagmus. *Can J Ophthalmol* 2:4-10, 1967.

315. Cogan DG, Kuwabara T: The mural cell in perspective. *Arch Ophthalmol* 78:133-139, 1967.

316. Cogan DG, Kuwabara T: Myelin artifacts. Am J Ophthalmol 64:622-626, 1967.

317. Cogan DG: Editorial. Moonshine, earthshine, and vision. *Arch Ophthalmol* 78:695-696, 1967.

318. Cogan DG, Kuwabara T: Scientific exhibit: Vascular pattern of the retina. *Acta Concilium Ophthalmologicum* 2:1203, 1967.

319. Cogan DG: Sphingolipidoses. *Trans Soc UK* 87:871-872, 1967.

320. Cogan DG: Ophthalmologic problems in diabetes mellitus. In *Diabetes Mellitus: Diagnosis and Treatment.* vol II, Hamwi GJ, Danowski TS (eds), New York, American Diabetes Association, 1967.

321. Cogan DG: Extraocular muscles and their diseass. Neuro-ophthalmology. *Symposium of the University of Miami and Bascom Palmer Eye Institute.* vol III, Smith JL (ed), St. Louis, Mosby, 1967.

322. Cogan DG: Editorial. Lighting and health hazards. *Arch Ophthalmol* 79:2, 1968.

323. Cogan DG: Editorial. The Fountain report or the fine line between efficiency and effectivity. *Arch Ophthalmol* 79:321, 1968.

324. Kuwabara T, Quededo R, Cogan DG: An experimental study of dichloroethane poisoning. *Arch Ophthalmol* 79:321-330, 1968.

325. Cogan DG, Kuwabara T: The sphingolipidoses and the eye. *Arch Ophthalmol* 79:437-452, 1968.

326. Cogan DG: Opsoclonus, body tremulousness, and benign encephalitis. *Arch Ophthalmol* 79:545-551, 1968.

327. Cogan DG: Correspondence. Editor's note on catheterization versus antecubital vein method for injection. *Arch Ophthalmol* 80:414, 1968.

328. Cogan DG: Book review. *Atlas of Fluorescence Fundus Angiography,* by Chikano S, Shimizu K, Tokyo, Igaku Shoin Ltd, 1968.

329. Cogan DG: Lighting, eyestrain, and health hazards. *Sight-Sav Rev* 38:73-76, 1968.

330. Cogan DG, Kuwabara T, Friedman E: Retinal vasculature. *Microvasc Res* 1:115-132, 1968.

331. Cogan DG: Down-beat nystagmus. *Arch Ophthalmol* 80:757-768, 1968.

332. Cogan DG: Corneal edema: Introduction. *Int Ophthalmol Clin* 8:523-526, 1968.

333. Cogan DG: Retinal and papillary vasculitis. The William MacKenzie Memorial Lecture. *The Wiliam MacKenzie Centenary Symposium on the Ocular Circulation in Health and Disease.* Cant JS (ed), St. Louis, Mosby, 1969.

334. Cogan DG: Pseudoretinitis pigmentosa. Report of two traumatic cases of recent origin. *Arch Ophthalmol* 81:45-53, 1969.

335. Cogan DG: Obituary. Frederick Herman Verhoeff. In memorium. *Arch Ophthalmol* 81:300-302, 1969.

336. Kuwabara T, Kinoshita J, Cogan DG: Electron microscopic study of galactose-induced cataract. *Invest Ophthalmol* 8:133-149, 1969.

337. Cogan DG, Souders TB: Some clinical lessions from histopathology. *Trans Penn Acad Ophthalmol Otolaryngol* 22:5-22,1969.

338. Cogan DG: Editorial. So the blind may "see." *N Engl J Med* 281:215-216, 1969.

339. Cogan DG: Position statement on diabetic blindness. *Science Writers Seminar in Ophthalmology,* Bevery Hills, Nov 1969.

340. Cogan DG, Reese AB: A Syndrome of iris nodules, ectopic Descemet's membrane and unilateral glaucoma. *Doc Ophthalmol* 26:424-433, 1969.

341. Cogan DG: Frederick Herman Verhoeff, personal recollections. Frederick H. Verhoeff Lecture. *Trans Ophthalmol Soc* 67:96-109, 1969.

342. Cogan DG: Foreword. In *Fundamentals of Ophthalmology. A Programmed Text.* Reinecke RD, Herm RJ, New York, Appleton-Century-Crofts, 1969.

343. Cogan DG: Book review. *Diabetes and the Eye,* by Caird DM, Pirie A, Oxford, Blackwell, 1969. *N Engl J Med* 282:517, 1970.

344. Cogan DG: Book review. *Fluorescence Photography of the Eye,* by Rosen ES, New York, Appleton-Century-Crofts, 1969. *Arch Ophthalmol* 83:795-796, 1970.

345. Cogan DG, Wray SH: Internuclear ophthalmoplegia as an early sign of brainstem tumors. *Neurology* 20:629-633, 1970.

346. Hallett M, Cogan DG: Episodic unilateral mydriasis in otherwise normal patients. *Arch Ophthalmol* 84:130-136, 1970.

347. Cogan DG: Internuclear ophthalmoplegia, typical and atypical. *Arch Ophthalmol* 84:583-589, 1970.

348. Cogan DG, Kuwabara T, Moser H: Metachromatic leucodystrophy. *Ophthalmologica* 160:2-17, 1970.

349. Cogan DG, Guzak SV: Fluorangiographic pattern of macular edema and retinal seelling in postmortem eyes. *Am J Ophthalmol* 71:291-297, 1971.

350. Cogan DG: Book review. *Stereoscopic Atlas of Slitlamp Biomicroscopy.* vols I, II, Braley AE, et al, St. Louis, Mosby, 1970.

351. Cogan DG: Congenital anomalies of the retina. *Birth defects: original article series* 7:41-51, 1971.

352. Cogan DG, Kuwabara T: Tumors of the ciliary body. *Int Ophthalmol Clin* 11:27-56, 1971.

353. Cogan DG, Kuwabara T: Growth and regenerative potential of Descemet's Membrane. *Trans Ophthalmol Soc UK* 91:875-894, 1971.

354. Donaldson DD, Cogan DG: *Ocular Fundus in Systemic Diseases.* New York, Medcom, Inc, 1971.

355. Cogan DG: Heredity of congenital ocular motor apraxia. *Trans Am Acad Ophthalmol Otolaryngol* 76:60-63, 1972.

356. Cogan DG: Benign vacuolated melanomas of the ciliary body. In *Contemporary Ophthalmology Honoring Sir Stewart Duke-Elder.* Bellows JG (ed), Baltimore, Williams & Wilkins, 1972.

357. Cogan DG: Radiant energy. In *Modern Ophthalmology.* ed 2, Sorsby A (ed), Butterworth, London, 1972.

358. Cogan DG: Editorial. The lens, cataracts, and galactosemia. *N Eng J Med* 288:1239-1240, 1973.

359. Cogan DG, Truman JT, Smith TR: Optic neuropathy, chloramphenicol, and infantile genetic agranulocytosis. *Inv Ophthalmol* 12:534-537, 1973.

360. Cogan DG: Visual hallucinations as release phenomena. *Arch Ophthalmol* 188:137-150, 1973.

361. Cogan DG, Kuwabara T: The eye. In *Histology.* ed 3, Greep RO, Weiss L (eds), New York, McGraw-Hill, 1973.

362. Cogan DG: Paralysis of down-gaze. *Arch Ophthalmol* 91:192-199, 1974.

363. Cogan DG: Tumors of the optic nerve. In *Handbook of Clinical Neurology*. vol 17, Vinken PJ, Bruyn BW (eds), Amsterdam, North Holland, 1974.

364. Cogan DG: Ophthalmic manifestations of systemic vascular disease. In *Major Problems in Internal Medicine* 3:1-67, 1974.

365. Char DH, Hollinshead A, Cogan DG, et al: Cutaneous delayed hypersensitivity reactions to soluble melanoma antigen in patients with ocular malignant melanoma. *N Eng J Med* 291:274-277, 1974.

366. Cogan DG: Editorial. The corneal arcus. *N Eng J Med* 291:1356, 1974.

367. Cogan DG, Kuwabara T, Donaldson DD, et al: Microcystic dystrophy of the cornea, a partial explanation for its pathogenesis. *Arch Ophthalmol* 92:470-474, 1974.

368. Cogan DG, Kuwabara T, Kolodny EH: A variant of Tay-Sachs disease. *XII Concilium Ophthalmoligicum*, 1974.

369. Cogan DG: Basic mechanisms of ocular motility. (Panel discussion), *Wenneigren Center International Symposium*, 1974.

370. Cogan DG: Ocular involvement in disseminated intravascular coagulopathy. *Arch Ophthalmol* 93:1-8, 1975.

371. Robinson WG Jr, Kuwabara T, Cogan DG: Lysosomes and melanin granules of the retinal pigment epithelium in a mouse model of the Chediak-Higashi Syndrome. *Invest Ophthalmol* 14:312-317, 1975.

372. Cogan DG: Lighting and health hazards. *Symposium: The Occupational and Health Effects Associated with Reduced Illumination.* HEW Publication (NIOSH), 1975.

373. Char DH, Cogan DG, Sullivan WR, Jr.: Immunologic study of nonsyphilitic interstitial keratitis with vestibuloauditory symptoms. *Am J Ophthalmol* 80:491-494, 1975.

374. Appen RE, Wray SH, Cogan DG: Central retinal artery occlusion. *Am J Ophthalmol* 79:374-381, 1975.

375. Wand M, Smith TR, Cogan DG: Cholesterosis bulbi: The ocular abnormality known as synchysis scintillans. *Am J Ophthalmol* 80:177-183, 1975.

376. Cogan DG, Wray SH: Vascular occlusions in the eye from cardiac myxomas. *Am J Ophthalmol* 80:396-403, 1975.

377. Kuwabara T, Cogan DG, Johnson CC: Structure of the muscles of the upper eyelid. *Arch Ophthalmol* 93:1189-1197, 1975.

378. Zee DS, Cogan DG, Robinson DA, et al: Analysis of eye movements in members of a family with late-onset, dominantly inherited cerebellar ataxia. *Trans Am Neurol Assoc* 100:98-103, 1975.

379. Cogan DG: An introduction to eye movements. In *The Nervous System.* vol 2, Tower DB (ed), New York, Raven Press, 1975.

380. Kuwabara T, Perkins DG, Cogan DG: Sliding of the epithelium in experimental corneal wounds. *Invest Ophthalmol* 15:4-14, 1976.

381. Cogan DG: Fibrin clots in the choriocapillaris and serous detachment of the retina. *Ophthalmologica* 172:298-307, 1976.

382. Sanderson PA, Kuwabara T, Cogan DG: Optic neuropathy presumably caused by vincristine therapy. *Am J Ophthalmol* 81:146-150, 1976.

383. Wray SH, Cogan DG, Arnason BGW: Experimental allergic encephalomyelitis; Passive transfer by the intraocular injection of sensitized cells. *Arch Neurol* 33:183-185, 1976.

384. Cogan DG: V. Everett Kinsey: A biographical sketch. *Invest Ophthalmol* 15:334-335, 1976.

385. Yee RD, Cogan DG, Zee DS: Ophthalmoplegia and dissociated nystagmus in abetalipoproteinemia. *Arch Ophthalmol* 94:571-575, 1976.

386. Cogan DG: Neuro-ophthalmology and medical ophthalmology, a dialogue. *Arch Ophthalmol* 94:393-394, 1976.

387. Yee RD, Trese M, Zee DS, Kollarits CR, Cogan DG: Ocular manifestation of acute pandysautonomia. *Am J Ophthalmol* 81:740-744, 1976.

388. Cogan DG, Yee RD, Gittinger J: Rapid eye movements in myasthenia gravis. I. Clinical observations. *Arch Ophthalmol* 94:1083-1085, 1976.

389. Cogan DG: Obituary. Ludwig Von Sallman. *Vision Res* 16:885-890, 1976.

390. Cogan DG: Ocular manifestations of spongy degeneration. *Birth defects: original article series.* vol XII, 3:527-534, 1976.

391. Wray SH, Cogan DG, Kuwabara T, et al: Adrenoleukodystrophy with disease of the eye and optic nerve. *Am J Ophthalmol* 82:480-485, 1976.

392. Yee RD, Savage DD, Cogan DG, et al: Plasma cell infiltration of the conjunctiva associated with pancytopenia, dermatitis, and polyclonal gammopathy. *Am J Ophthalmol* 82:486-491, 1976.

393. Yee RD, Cogan DG, Zee DS, et al: Rapid eye movements in myasthenia gravis. II. Electro-oculographic analysis. *Arch Ophthalmol* 94:1465-1472, 1976.

394. Zee DS, Yee RD, Cogan DG, et al: Ocular motor abnormalities in hereditary cerebellar ataxia. *Brain* 99:207-234, 1976.

395. Fishman ML, Bean SC, Cogan DG: Optic atrophy following prophylactic chemotherapy and cranial radiation for acute lymphocytic leukemia. *Am J Ophthalmol* 82:571-576, 1976.

396. Cogan DG, Robins SM: Neuro-ophthalmic disorders. In *Human Health and Disease.* Federation of the American Society for Experimental Biology, 1977.

397. Cogan DG, Kuwabara T: Papilledema. *Exp Eye Res*, Suppl, 419-433, 1977.

398. Yee RD, Cogan DG, Thorp TR, Schut L: Optic nerve compression due to aneurysmal bone cyst. *Arch Ophthalmol* 95:2176-2179, 1977.

399. Cogan DG: Immunosuppression and eye disease. First Vail Lecture. *Am J Ophthalmol* 83:777-788, 1977.

400. Kollarits CR, Moss ML, Cogan DG, et al: Scleral calcifications in hyperparathyroidism: Demonstration by computed tomography. *J Computer Assisted Tomography* 1:500-504, 1977.

401. Kuwabara T, Cogan DG: The eye. In *Histology*. ed 4, Weiss L, Greep RO (eds), New York, McGraw-Hill, 1977.

402. Cogan DG: Enigmas and hypotheses. In *Eye Movements.* Brooks BA, Bajandas FJ (eds), New York, Plenum, 1977.

403. Cogan DG, Yee RD: Experimental myelin intrusion in the nerve head. *Invest Ophthalmol Vis Sci* 16:461-468, 1977.

404. Cogan DG: Introduction to "special senses in aging." *Proceedings of Symposium on Biology of Special Senses in Aging at the University of Michigan.* Ann Arbor, 1977.

405. Gunkel RD, Cogan DG: Colorimetry by a new principle. *Arch Ophthalmol* 96:331-334, 1978.

406. Cogan DG: Opsoclonus. In *Handbook of Clinical Neurology*. Vinken PJ, Bruyn GW (eds), Amsterdam, North Holland, 1978.

407. Cogan DG: Ophthalmic pathology in America (1928-1978). *ARVO*, 1978.

408. Cogan DG: Coloboma of optic nerve with overlay of peripapillary retina. *Br J Ophthalmol* 62:347-350, 1978.

409. Cogan DG: The rise and fall of eponyms. *Arch Ophthalmol* 96:2202-2203, 1978.

410. Cogan DG: Updating the inborn errors of metabolism affecting the retina. *Trans of the Pacific Coast Oto-Ophthalmological Society* 59:23-37, 1978.

411. Karp LA, Streeten BW, Cogan DG: Radiation-induced atrophy of the meibomian glands. *Am J Ophthalmol* 97:303-305, 1979.

412. Chu FC, Reingold DB, Cogan DG, Williams AC: The eye movement disorders of progressive supranuclear palsy. *Ophthalmology* 86:422-428, 1979.

413. Chu FC, Reingold D, Cogan DG: Letter to the Editor: A maneuver to elicit vertical dolls' eye movements. *Am J Ophthalmol* 87:724, 1979.

414. Cogan DG: Visuospatial dysgnosia. *Am J Ophthalmol* 88:361-368, 1979.

415. Reingold DB, Chu FC, Cogan DG, et al: A computerized testing facility for clinical study of versional eye movement control. *Computers in Ophthalmology* April:220-222, 1979.

416. Cogan DG: Some neuro-ophthalmic syndromes. Trans Penn Acad Ophthalmol-Otolaryngol 32:130-138, 1979.

417. Chu FC, Kuwabara T, Cogan DG, et al: Ocular manifestations of familial high-density lipoprotein deficiency (Tangier disease). *Arch Ophthalmol* 97:1926-1928, 1979.

418. Cogan DG, Chu FC, Reingold DB: Notes on congenital ocular motor apraxia: associated abnormalities. In *Neuro-Ophthalmology*. Glaser J (ed), St. Louis, Mosby, 1980.

419. Cogan DG, Schulman J, Porter RJ, et al: Epileptiform ocular movements with methylmalonic aciduria and homocystinuria. *Am J Ophthalmol* 90:251-253, 1980.

420. Cogan DG, Chu FC, Reingold DB, Tychsen L: A long-term follow-up of congenital ocular motor apraxia. Case report. *Neuro-ophthalmology* 1:145-147, 1980.

421. Cogan DG: Book review. *Ophthalmology, A Short Textbook. N Engl J Med* 303:1375, 1980.

422. Haynes BF, Kaiser-Kupfer MI, Mason P, Fauci AS: Cogan syndrome: Studies in thirteen patients, long-term follow-up, and a review of the literature. *Medicine* 59:426-441, 1980.

423. Cogan DG, Chu FC, Gittinger J, Tychsen L: Fundal abnormalities in Gaucher's disease. *Arch Ophthalmol* 98:2202-2203, 1980.

424. Cogan DG, Chu FC, Bachman DM, Barranger J: The DAF syndrome. *Neuro-ophthalmology* 2:7-16, 1981.

425. Chu FC, Reingold DB, Cogan DG: Lid-triggered synkinesis. *Ophthalmology* 88:1019-1023, 1981.

426. Cogan DG, Chu FC, Reingold DB, Barranger J: Ocular motor signs in some metabolic diseases. *Arch Ophthalmol* 99:1802-1808, 1981.

427. Waybright EA, Selhorst JB, Chu F, Cogan DG: Sublingual angiomas and the blue rubber-bleb nevus syndrome. Letter. *Arch Neurol* 38:784-785, 1981.

428. Cogan DG, Chu FC, Bachman DM, Barranger J: The DAF Syndrome: A Brief. In *Neurogenetics and Neuro-ophthalmology*. Huber A, Kline D (eds), Amsterdam, Elsevier/North Holland, 1981.

429. Cogan DG: Stroke. In *Neuro-Ophthalmology*. vol II, Lessell S, van Dalen JT (eds), Amsterdam-Princeton, Excerpta Medica, 1982.

430. Cogan DG, Chu FC: Ocular manifestations of myasthenia gravis. In *Neuro-Ophthalmology*. Smith JL (ed), 1982.

431. Cogan DG, Chu FC, Reingold DB: Illusory movement during head rotation with normal eye movements. In *Nystagmus and Vertigo: Clinical Approaches to the Patient with Dizziness*. Honrubia V, Brazier MA (eds), New York, Academic Press, 1982.

432. Cogan DG, Chu FC, Reingold DB: Ocular signs of cerebellar disease. *Arch Ophthalmol* 100:755-560, 1982.

433. Bachman DM, Rodrigues MM, Chu FC, Straus SE, Cogan DG, Macher AM: Culture-proven cytomegalovirus retinitis in a homosexual man with the acquired immunodeficiency syndrome. *Ophthalmology* 89:797-804, 1982.

434. Cogan DG: The ocular fundus and hypertension. In *Hypertensive Cardiovascular Disease: Pathophysiology and Treatment*. Amery A (ed), The Hauge, Martinus Nijhoff Pub, 1982.

435. Cogan DG: Book review. *Walsh and Hoyt's Clinical Neuro-Ophthalmology. N Engl J Med* 307:901-902, 1982.

436. Cogan DG, Kuwabara T: Abstract. The retina as a possible model for gray matter vasculopathy. *Ann Neurol* 12:74, 1982.

437. Stowens DW, Chu FC, Cogan DG, Barranger JA: Oculomotor deficits in Gaucher disease. In *Gaucher Disease: A Century of Delineation and Research*. Desnick RJ, Gat S, Grabowski GA (eds), New York, AR Liss, Inc, 1982.

438. Cogan DG, Chu FC, Barranger J: Macula halo syndrome. *Trans Am Ophthalmol Soc* LXXX:184-192, 1982.

439. Cogan DG: Foreward. In *The Neurology of Eye Movements*. Leigh RJ, Zee DS (eds), Philadelphia, FA Davis Co, 1983.

440. Chu FC, Rodrigues MM, Cogan DG, Neva FA: Leishmaniasis affecting the eyelids. *Arch Ophthalmol* 101:84-91, 1983.

441. Cogan DG: Book review. *The History Ophthalmology*. vol 1: Antiquity, by Hirschberg J; translated by Blodi FC. *N Engl J Med* 308:973-974, 1983.

442. Cogan DG: Book review. *Computerized Tomography in Neuro-Ophthalmology*, by Moseley IF, Sanders MD. *Journal of Computer Assisted Tomography* 7:604, 1983.

443. Green MH, Sanders RJ, Chu FC, Clark WH Jr, Elder DE, Cogan DG: The familial occurrence of cutaneous melanoma, intraocular melanoma, and the dysplastic nevus syndrome. *Am J Ophthalmol* 96:238-245, 1983.

444. Chu FC, Reingold DB, Cogan DG, Hunt SMJ, et al: Clinical studies of color vision with Gunkel's chromagraph. *Arch Ophthalmol* 101:1232-1235, 1983.

445. Optican LM, Zee DS, Chu FC, Cogan DG: Open loop pursuit in congenital nystagmus. ARVO abstract. *Invest Ophthalmol Vis Sci* 24(Suppl):271, 1983.

446. Zee DS, Chu FC, Leigh RJ, Savino PJ, Schatz NJ, Reingold DB, Cogan DG: Blink-saccade synkinesis. *Neurology* 33:1233-1236, 1983.

447. Cogan DG: Combined diseases of the eye and brain. *Fortschr Ophthalmol* 80:360-361, 1983.

448. Cogan DG, Chu FC, Barranger JA, Gregg RE: Macula halo syndrome, variant of Niemann-Pick Disease. *Arch Ophthalmol* 101:1698-1700, 1983.

449. Chu FC, Rodrigues MM, Cogan DG, Fauci AS: The pathology of idiopathic midline destructive disease (IMDD) in the eyelid. *Ophthalmology* 90:1385-1388, 1983.

450. Cogan DG: Internuclear Ophthalmoplegia. *Bull Soc Belg Opt* 208-I:205-210, 1983.

451. Cogan DG, Kuwabara T: Comparison of retinal and cerebral vasculature in trypsin digest preparations. *Br J Ophthalmol* 68:10-12, 1984.

452. Cogan DG: Gangliosidosis AB. *Neuro-ophthalmology* 4:65-68, 1984.

453. Cogan DG, Kuwabara T, Kolodny E, Driscoll S: Gangliosidosis and the fetal retina. *Ophthalmology* 91;508-512, 1984.

454. Chu FC, Rodrigues MM, Cogan DG, Barranger JA: The pathology of pingueculae in Gaucher's disease. *Ophthal Paediatr Gene* 4:7-11, 1984.

455. Cogan DG: Discussion on paper by Cobo LM, Haynes BF: Early corneal findings in Cogan's syndrome. *Ophthalmology* 91:907, 1984.

456. Cogan DG, Rodrigues MM, Chu FC, Schaefer EJ: Ocular abnormalities in abetalipoproteinemia, a clinicopathologic correlation. *Ophthalmology* 91:991-998, 1984.

457. Cogan DG, Kinoshita JH, Kador PF, et al: Aldose reductase and complications of diabetes. *Ann Int Med* 101:82-91, 1984.

458. Cogan DG: Visual disturbances with focal progressive dementing disease. *Am J Ophthalmol* 100:68-72, 1985.

459. Cogan DG, Witt ED, Goldman-Rakic PS: Ocular signs in thiamine-deficient monkeys and in Wernicke's disease in humans. *Arch Ophthalmol* 103:1212-1220, 1985.

460. Cogan DG: Academy exhibits and the Museum. *Arch Ophthalmol* 103:189, 1985.

461. Antonakou-Chrousos G, O'Neill JF, Cogan DG: Absence of the near reflex in a healthy adolescent. *J Ped Ophthalmol Strabismus* 22:76-77, 1985.

462. Antonakou-Chrousos G, Reingold DR, Chu FC, Cogan DG: Habitual head turning in spasmus nutans: An oculographic study. *J Ped Ophthalmol Strabismus* 22:113-116, 1985.

463. Cogan DG: Thoughts on reading "Immortal Words." *Dartmouth Medical School Alumni Magazine*, Spring, 1985.

464. Cogan DG: Raccoonteurs. *Dartmouth Medical School Alumni Magazine*, December, 1985.

465. Cogan DG: Update remarks on the control of eye movements. *Eye Science* 2:9-11, 1986. Presented at the International Ophthalmologic Conference in China, Guangzhou, 1985.

466. Cogan DG: Historical perspectives. In *Optical Radiation and Visual Health.* Waxler M, Hitchins VM (eds), Boca Raton, CRC Press, 1986.

467. Chrousos GA, Oldfield EH, Doppman JL, Cogan DG: Prevention of ocular toxicity of carmustine (BCNU) with supraophthalmic intracarotid infusion. *Ophthalmology* 93:1471-1475, 1986.

468. Chrousos GA, Matsuo V, Ballen AE, Cogan DG: Near-evoked nystagmus in spasmus nutans. *J Ped Ophthalmol Strabismus* 23:141-143, 1986.

469. Cogan DG: The infirmary and Mr. Lincoln. *MEEI Alumni News*, Fall, 1986.

470. Tso MO, Sanders TE, Zimmerman LE, Cogan DG: Ex errato lux. Concept and development of the Verhoeff Society. *Surv Ophthalmol* 30:337-340, 1986.

471. Cogan DG: Book review. *NIH: An Account of Research in its Laboratory and Clinics.* Stetten D Jr, Carrigan WT (eds). *Arch Ophthalmol* 104:1447, 1986.

472. Cogan DG, Kuwabara T: Accessory cells in vessels of the paranatal human retina. *Arch Ophthalmol* 104:747-752, 1986.

473. Cogan DG: Lest we forget: Notes on reading Verhoeff's early correspondence. *Surv Ophthalmol* 32:131-135, 1987.

474. Cogan DG: Alzheimer's syndrome. *Am J Ophthalmol* 104:183-184, 1987.

475. Cogan DG: Book review. *Ophthalmic Pathology: An Atlas and Textbook.* ed 3, Spencer WH (ed). *N Engl J Med* 316:887, 1987.

476. Rabinowitz-Dagi L, Antonakou-Chrousos G, Cogan DG: Spasm of the nea reflex associated with organic disease. *Am J Ophthalmol* 110:582-585, 1987.

477. Cogan DG: Book review. *The University of Michigan Department of Ophthalmology,* by Henderson JW. *Arch Ophthalmol* 105:468, 1987.

478. Cogan DG: Letter of Tribute to Edward W.D. Norton: *Am J Ophthalmol* 103:3, 1987.

479. Porrino LJ, Ho VW, Palombo E, Kennedy C, Hogan N, Kuwabara T, Cogan DG, et al: Abstract. Columnar organization of metabolic activity in the striate cortex of hemi-Parkinsonian monkeys. *Neurology* 37(Suppl 1):340, 1987.

INTERVIEWER BIOGRAPHY

Sally Smith Hughes

She graduated from the University of California, Berkeley, in 1963 with an A.B. degree in zoology, and from the University of California, San Francisco, in 1966 with an M.A. degree in anatomy. After completing a dissertation on the history of the concept of the virus, she received a Ph.D. degree in the history of medicine from the Royal Postgraduate Medical School, University of London, in 1972.

Her previous positions have been postgraduate research histologist, the Cardiovascular Research Institute, University of California, San Francisco, 1966-1968, and medical historian conducting the NEH-supported History of Medical Physics Project for The Bancroft Library, 1978-1980.

She is presently an interviewer on medical and scientific topics for the Regional Oral History Office, and the author of *The Virus: A History of the Concept.*

INDEX

A

B

C

D

E

F

G

H

I

J

K

L

M

N

O

P

R

S

T

U

V