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TH, EDUCATION,
VELFARE
Health Service
National Institutes of Health

A History of Cancer Control in the United States 1946-1971

Appendices

A History of Cancer Control in the United States 1946-1971

V. 4

Appendices

Prepared by the
History of Cancer Control Project,
UCLA School of Public Health
pursuant to Contract No. NOI-CN-55172,
Division of Cancer Control and
Rehabilitation, National Cancer Institute;
principal investigator,
Lester Breslow, M.D., M.P.H.

DEPARTMENT OF
HEALTH, EDUCATION,
AND WELFARE
Public Health Service
National Institutes of Health
National Cancer Institute
Division of Cancer Control
and Rehabilitation
DHEW Publication No. (NIH) 78-1519

APPENDICES

TABLE OF CONTENTS (APPENDICES)

UNIVERSITY OF CALIFORNIA, LOS ANGELES

APPENDIX 1

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO

SANTA BARBARA • SANTA CRUZ

SCHOOL OF PUBLIC HEALTH
LOS ANGELES, CALIFORNIA 90024

1 February 1977

Lester Breslow, Dean
UCLA School of Public Health
Los Angeles, California 90024

Dear Dean Breslow:

I am submitting with this letter final materials completed for a bibliography of Cancer Control in compliance with Contract Number USPHS-NCI-NO1-CN-55172: "History of Cancer Control". The contract called for identification, assembly and abstracting of more than 750 reports in the field of cancer control. These reports constitute an expanded bibliography of cancer control background, and are a valuable reference resource for history of cancer control. The reports and associated abstracts have been consulted, along with many other materials, in preparation of the final narrative manuscript of the project.

The reports are being transmitted in the following physical form:

a. 401 highly formatted abstracts in computer printout form. There are two sets, one aggregated by mode of cancer control (Set 1a) and one aggregated by state (e.g., Alabama, etc.) in which a cancer control project was located (Set 1b). These printouts were generated from the Databank of Cancer Control.

b. 356 informative but briefer abstracts also in two sets, as above, one aggregated by mode (Set 2a) and the other again aggregated by state (Set 2b).

We are also transmitting two additional physical materials. The first is computer tape that constitutes the Databank of Cancer Control (see below); the second are two microfiche, each containing 60 abstracts, from the Databank but in reduced form, readable through a simple, enlarging microfiche viewer.

DATABANK OF CANCER CONTROL

1. SEARCH PROCEDURES

The Databank searches, abstracts, and codes for computer input items important in the history of cancer control in the United States from 1946 to the present. Journal articles, books, interview transcripts, legislative records, and other materials have been considered for inclusion as items in the Cancer Control Databank. Most items considered to date have been journal articles.

The process of searching for items suitable for inclusion in the Programs section of the Cancer Control Databank begins with the scanning of bibliographies for titles which refer to cancer control programs. Items to which such titles refer are then themselves scanned. If appropriate (see "Cancer Control Databank Programs"), an item is Xeroxed and brought inhouse where a topsheet containing bibliographic and other information is affixed to it. A committee consisting of two or more members of the staff (and occasionally including other History of Cancer Control staff members) then decides if the item is, in fact, a program. If so, it is given an identification number, catalogued, abstracted and coded, edited and checked, and keypunched.

Titles found in Index Medicus (1946-1956 and 1960-present) and The Current List of Medical Literature (1957-1959) comprise the substance of the

Cancer Control Programs database. (Note: Index Medicus was not published from 1957 to 1959.) Subject headings found in these two bibliographies are the same or similar. Headings and subheadings searched and the years for which they are indexed are listed below. Subheadings are separated from major headings by colons. If the listing includes a subheading (e.g., "Breast: Cancer"), only titles catalogued under that subheading have been or will be scanned. Under "Breast: Cancer", for example, only titles catalogued under the subheading "Cancer" were scanned. All titles catalogued under the main heading, "Breast", will not be scanned. If only a main heading is listed, (e.g., "Cancer"), all titles catalogued under that heading have been scanned.

Index Medicus and Current List of Medical Literature headings and subheadings searched or in the process of being searched are: Cancer (1946-1956); Neoplasms (1957-present); Neoplasms, Specific Types (1960-present); Breast: Cancer (1946-1956); Breast Neoplasms (1957-present); Cervix: Cancer (1946-1956); Cervix Neoplasms (1957-present); Uterus: Cancer (1946-1956); Uterus Neoplasms (1957-present); Colon: Cancer (1946-1956); Colon: Neoplasms (1957-1964); Colonic Neoplasms (1965-present); Prostate: Cancer (1946-1956); Prostate: Neoplasms (1957-1963); Prostatic Neoplasms (1964-present); Lung: Cancer (1946-1956); Lung Neoplasms (1957-present); Sarcomas (1946-present); Carcinomas (1957-present); Carcinomas, Specific Types (1957-present); Tumors (1946-1956); Carcinogens (1957-present); Industry and Occupations (1946-1959); Occupations and Professions (1960-present); Industrial Hygiene (1946-1959); Industrial Medicine (1960-present); Hospitals (1946-present); Hospitals, Special (1968-present); Mass Screening Technics (1963-1967); Mass Screening (1968-present); Public Health (1946-present); Tobacco (1946-present); and Smoking (1957-present).

The computer tape contains the entire databank and with suitable computer equipment can reproduce the 401 abstracts in hard copy. This "deliverable" masks the fact that the tape, when associated with generic programs and procedures at the IBM 360/94 computer and the UCLA Campus Computing Network (CCN), is the core of an on-line, interactive bibliographic search and retrieval system in the cancer control field. The system is the Databank of Cancer Control (DOCC) and, when mounted at CCN in the time-sharing option (TSO), can be accessed from any desk telephone in the United States when coupled to a portable computer terminal. Search can be conducted on 12 variables. Features include on-line printing of full abstracts in any aggregation, as well as any element of the abstracts in the automatic list option. Hard copy can be ordered off-line as well as thermal (line at a time) copy at the terminal.

An experimental dissemination modality, computer output microfiche, was attempted, and was produced (two microfiche consisting of cancer control abstracts in the fields of administration, budget, legal, and therapeutic modality) and mailed to a selected list of cancer control professionals. Samples of each of DOCC microfiche 001 and 002 are attached and 25 copies of each are being physically transmitted with the materials mentioned earlier.

Appendix materials attached describe the method and procedure for sampling the broad cancer control literature, and definitions and codes for classification of cancer control reports.

Sincerely,

D. M. Wilner
Professor of Public Health

mw

Attachments:
1. Search Procedures
2. Program Definitions and Categories
3. Coding Manual
4. Search Variables and DOCC/Search Abbreviations (Menu)
5. Brief Definition of Cancer Control

Other materials were also searched to find items suitable for inclusion in the Cancer Control Databank. These include other printed medical bibliographies (e.g., Excerpta Medica), computer-generated bibliographies (e.g., Medline and Cancerline), books (e.g., National Program for the Conquest of Cancer), and special interest bibliographies submitted by other History of Cancer Control staff members.

2. PROGRAM DEFINITIONS AND CATEGORIES

The Search, Abstracting, and Data Input Systems team of the History of Cancer Control Project interests itself primarily in items concerning U. S. cancer control programs from 1946 to the present. These items were found, abstracted, coded and input into the online computer Databank of Cancer Control immediately accessible to anyone in the U. S. with a direct access computer terminal (telephone coupled via computer network). DOCC also disseminates its program abstracts via mail on microfiche and on hardcopy computer printouts.

Items may be journal articles, books, interview transcripts, legislative records, or any other materials describing cancer control programs. Programs must exist now or have existed in the past. DOCC is not interested in proposed programs.

DOCC defines cancer control program broadly to include any organized effort to control cancer. This does not, however, include research programs. Control programs make use of the results of research programs in order to reduce cancer or ameliorate its effects among human populations in the community. Control programs usually know in advance that what they are doing will work. Research programs do not.

In order to be a Databank of Cancer Control Program, an item must substantially describe a cancer control program. It must explain what the program does, rather than merely recording data collected by the program (although DOCC prefers to have the data too). Many items about tumor registries, for example, only relate cancer statistics. These are excluded from the Databank. An item which described the operation of a tumor registry, however, would be included.

The description of the program must include specific, preferably quantifiable, data of at least one of the following types:

1. Amount of money allocated by or to the program.
2. Facilities (e.g., clinics) built by or for the program.
3. Other resources (e.g., books, filmstrips) provided by or for the program.
4. Relationships between the cost of the program and benefit provided by it to the community.
5. Provisions of legislation passed or regulations enacted.
6. Population served by the program (e.g., 5,000 women between 21 and 55).
7. Area served by the program (e.g., Riverside and San Bernardino counties, California).
8. Utilization of the program's services (e.g., 53% of the target population).
9. Reduction of known carcinogens attempted or effected by the program (e.g., red dye number 2 was eliminated from all food sold in the U. S.).
10. Records kept by the program (e.g., the program recorded new incidence of cancer, five-year survival rates, and mortality rates).

The programs in which DOCC interests itself fall into five major categories:

1. Legislation and Regulation

   DOCC considers all cancer-related laws and regulations to be cancer control programs. Legislative and regulatory programs usually attempt to prevent cancer by eliminating or reducing known carcinogens.

2. Provision of Resources

   Among resources which may be provided are money, facilities, and information. Generally, these programs are effected by large organizations, such as the National Cancer Institute and the American Cancer Society.

3. Record Keeping

Descriptions of tumor registries comprise the bulk of these programs. For inclusion in the Databank, items in this category must describe a program's organization and its data sources, as well as tabulating its data.

4. Education

Included here are programs which teach cancer patients, cancer professionals, and the general public how to deal with cancer. This category also includes programs involving psychological and social interventions made at all stages of cancer control (prevention, screening and detection, diagnosis and treatment, rehabilitation, and continuing care). Examples of social and psychological intervention programs are teaching families of colostomy patients to accept the operation and using behavior modification techniques to train people to stop smoking.

5. Medical Interventions

Among programs in this category are those which use mammography to screen women for breast cancer and those which diagnose lung cancer by the use of radiologic techniques. DOCC is interested only in medical intervention programs involving the prevention, screening and detection, and diagnostic stages of cancer control. It does not accept for inclusion in the Databank medical intervention programs involving the treatment, rehabilitation, and continuing care stages of cancer control. There are two reasons for this. There is a plethora of items dealing with such programs. At present, there is a very limited Databank capacity. Also, most items about programs involving the treatment, rehabilitation, and continuing care stages of cancer control are more concerned with whether or not a particular medical technique works than they are with the control of cancer. Such programs are, in effect, research programs.

DATABANK OF CANCER CONTROL

3. CODING MANUAL

CONTENTS

I. Bibliographic Entry

(Publication Date is coded and included in the Bibliographic Entry abstract. The rest of the Bibliographic Entry is abstracted only. See IB for Publication Date codes.)

IA. Bibliographic Entry

(Abstract only. No codes except for Publication Date. See IB.)

IB. Publication Date

First two digits
01 - 12 = January - December

Second two digits
01 - 31 = First to Thirty-First Day of the Month

Last two digits
01 - 99 = 1901 - 1999
(01 = 1901 or prior; 99 = 1999 or beyond)
All six digits
000000 = Not Applicable
999999 = Unspecified

II. Site of Cancer

00- Not Applicable

01- Buccal Cavity and Pharynx (Oral): Lip, Tongue, Salivary Gland, Floor of the Mouth, Other and Unspecified Mouth and Pharynx

02- Digestive Organs: Esophagus, Stomach, Small Intestine, Large Intestine (Colon-Rectum), Liver, Pancreas, Other Unspecified Digestive Organs

03- Respiratory System: Larynx, Lung, Other and Unspecified Respiratory

04- Bone, Tissue, and Skin: Bone, Connective Tissue, Skin (Melanoma), Other and Unspecified Skin

05- Breast

06- Female Genital Organs: Cervix (Invasive), Cervix (in situ), Corpus Uteri, Ovary, Other and Unspecified Female Genital

07- Male Genital Organs: Prostate, Other Male Genital

08- Urinary Organs: Bladder, Kidney, Other Urinary

09- Eye

10- Brain and Central Nervous System

11- Endocrine Glands: Thyroid, Other Endocrine

12- Leukemia

13- Lymphomas: Lymphosarcoma and Reticulosarcoma, Hodgkin's Disease, Multiple Myeloma, Other Lymphomas

97- Six or More Sites

98- Other Sites (specify in abstract)

99- Unspecified Sites

III. Intervenor/Fundor

(Included in same abstract but coded separately. See IIIA and IIIB for codes.)

IIIA. Intervenor

00- Not Applicable

01- Federal Government Organization (U.S.)

02- State Government Organization (U.S.)

03- County Government Organization (U.S.)

04- Local or Municipal Government Organization (U.S.)

05- Foreign Governmental Organization

06- International Organization (e.g., WHO)

07- American Cancer Society

08- Other Voluntary or Consumer Agency

09- Private Foundation (e.g., Sloan-Kettering)

10- Professional Society or Organization (e.g., AMA)

11- Hospital and/or Clinic

12- Educational Institution

13- Prepaid Health Plan

14- Industrial or Business Organization

15- Physicians

16- Other Health Professionals and Paraprofessionals

17- Social and Related Workers

18- Other Individuals

97- "All" Intervenors

98- Other Intervenors

99- Unspecified Intervenors

IIIB. Fundor

00- Not Applicable

01- Federal Government Organization (U.S.)

02- State Government Organization (U.S.)

03- County Government Organization (U.S.)

04- Local or Municipal Government Organization (U.S.)

05- Foreign Government Organization

06- International Organization (e.g., WHO)

07- American Cancer Society

08- Other Voluntary or Consumer Agency

09- Private Foundation (e.g., Sloan-Kettering)

10- Professional Society or Organization (e.g., AMA)

11- Hospital and/or Clinic

12- Educational Institution

13- Prepaid Health Plan

14- Industrial or Business Organization

15- Physicians

16- Other Health Professionals and Paraprofessionals

17- Social and Related Workers

18- Other Individuals

97- "All" Fundors

98- Other Fundors

99- Unspecified Fundors

IV. Phase of Cancer Control

00- Not Applicable (includes research and registry programs not directly related to one of the following six phases of cancer control [prevention, etc.]; e.g., etiological research not directly to a cancer prevention program, mortality registries not directly related to treatment or other cancer control programs)

01- Prevention

02- Screening and Detection

03- Diagnosis

04- Treatment

05- Rehabilitation

06- Continuing Care

97- All Phases of Cancer Control

98- Other Phases of Cancer Control

99- Unspecified Phases of Cancer Control

V. Cancer Control Target and Sample Size

(Included in same abstract but coded separately. See following two pages for codes.)

VA. Cancer Control Target

00- Not Applicable

01- General Population

02- High Risk Population

03- Cancer Patient Population

04- General Patient Population

05- Medical or Other Health Field Student Population

06- General Student Population

07- Federal Government Organization (U.S.)

08- State Government Organization (U.S.)

09- County Government Organization (U.S.)

10- Local or Municipal Government Organization (U.S.)

11- Foreign Government Organization

12- International Organization (e.g., WHO)

13- American Cancer Society

14- Other Voluntary or Consumer Agency

15- Private Foundation (e.g., Sloan-Kettering)

16- Professional Society or Organization (e.g., AMA)

17- Hospital and/or Clinic

18- Educational Institution

19- Prepaid Health Plan

20- Industrial or Business Organization

21- Physicians

22- Other Health Professionals and Paraprofessionals

23- Social and Related Workers

97- "All" Cancer Control Targets

98- Other Cancer Control Targets

99- Unspecified Cancer Control Targets

VB. Sample Size

0000000 = None or Not Applicable (e.g., organizations, number of treatments as opposed to number of people treated)

0000001 - 9999997 = Exact Number of People

9999998 = 9,999,998 or More

9999999 = Unspecified (e.g.,

VI. Age

00- Not Applicable

01- Children Only (or mainly, at leat 2/3) under 12

02- Youth Only (or mainly at least 2/3) 12-17

03- Adults Only (or mainly, at least 2/3) 18-64

04- Elderly Only (or mainly, at least 2/3) over 64.

05- 01 and 02 mainly or only

06- 02 and 03 mainly or only

07- 03 and 04 mainly or only

08- 01, 02, and 03 only

09- 02, 03, and 04 only

97- All Ages

98- Other Ages

99- Unspecified Ages

VII. Sex

00- Not Applicable

01- Males Only

02- 1 - 9% Female

03- 10 - 19% Female

04- 20 - 29% Female

05- 30 - 39% Female

06- 40 - 49% Female

07- 50 - 59% Female

08- 60 - 69% Female

09- 70 - 79% Female

10- 80 - 89% Female

11- 90 - 99% Female

12- Females Only

97- Both Sexes but unspecified percentage of either one

98- Other (e.g., transsexuals who change sex during the course of the program)

99- Unspecified

VIII. Race

00- Not Applicable

01- White Only (or mainly, at least 2/3)

02- Black Only (or mainly, at least 2/3)

03- Oriental Only (or mainly, at least 2/3)

04- Hispanic Only (or mainly, at least 2/3)

05- American Indian Only (or mainly, at least 2/3)

06- About 50-50 of any 2 (meaning less than 2/3 or each)

07- Non-White Only (or mainly, at least 2/3)

97- "All" Races

98- Other Races

99- Unspecified

IX. Socioeconomic Status

00- Not Applicable

01- Lower ~~Income~~ Mainly (2/3 or more)

02- Middle ~~Income~~ Mainly (2/3 or more)

03- Upper ~~Income~~ Mainly (2/3 or more)

04- 01 and 02 Mainly

05- 02 and 03 Mainly

06- 01 and 03 Mainly

07- 01, 02, and 03 but less than 50% of any one

97- 01, 02, and 03 and an unspecified percentage of any one

98- Other

99- Unspecified

X. Geographical Location of Program

00- Not Applicable

01- Canada

02- Alabama

03- Alaska

04- Arizona

05- Arkansas

06- California

07- Colorado

08- Connecticut

09- Delaware

10- District of Columbia

11- Florida

12- Georgia

13- Hawaii

14- Idaho

15- Illinois

16- Indiana

17- Iowa

18- Kansas

19- Kentucky

20- Louisiana

21- Maine

22- Maryland

23- Massachusetts

24- Michigan

25- Minnesota

26- Mississippi

(continued)

X. Geographical Location of Program (continued)

27- Missouri

28- Montana

29- Nebraska

30- Nevada

31- New Hampshire

32- New Jersey

33- New Mexico

34- New York

35- North Carolina

36- North Dakota

37- Ohio

38- Oklahoma

39- Oregon

40- Pennsylvania

41- Rhode Island

42- South Carolina

43- South Dakota

44- Tennessee

45- Texas

46- Utah

47- Vermont

48- Virginia

49- Washington

50- West Virginia

51- Wisconsin

52- Wyoming

(continued)

X. Geographical Location of Program (continued)

53- U.S. Possessions and Territories

54- Nationwide (U.S.)

55- Latin America

56- British Isles (including Eire)

57- U.S.S.R.

58- Europe (excluding the British Isles and the U.S.S.R.)

59- Asia (excluding the U.S.S.R.)

60- Africa

61- Australia and New Zealand

62- Antarctica

63- Worldwide

64- Off-World (e.g., Moon, Space Stations)

97- Worldwide and Off-World

98- Other (e.g., islands)

99- Unspecified

XI. Mode of Cancer Control

000- Not Applicable

A. Medical

101- General Medical Examinations (includes medical history, standard physical examination, and standard laboratory tests)
109- Unspecified Medical Cancer Prevention Modalities
111- Multiphasic Screening (for several diseases, including cancer)

112- Multiphasic Cancer Screening (for several types of cancer, but not for other diseases)

113- Cytological Smears (directed specifically toward the detection of cancer)

114- Thermography

115- Radiologic Screening (includes mammography and xeroradiology)

118- Other Cancer Screening Modalities
119- Unspecified Cancer Screening Modalities
121- Diagnostic Radiology

122- Biopsy

123- Palpation (by medical personnel)

124- Self Palpation
125- Endoscopy
128- Other Cancer Diagnostic Modalities
129- Unspecified Cancer Diagnostic Modalities
131- Surgery

132- Radiotherapy

133- Chemotherapy

134- Hormone Therapy

135- Immunotherapy

136- Hyperthermia

137- Acupuncture

138- Other Cancer Treatment Modalities
139- Unspecified Cancer Treatment Modalities
141- Physical Restoration--Head and Neck Involvement

(continued)

XI. Mode of Cancer Control (continued)

A. Medical (continued)

142- Physical Restoration--Breast, Extremities, Gastrointestinal, and Genitourinary Involvement

148- Other Medical Rehabilitative Modalities for Cancer

149- Unspecified Medical Rehabilitative Modalities for Cancer

151- Medical Palliative and Supportive Care

197- "All" Medical Cancer Control Modalities

198- Other Medical Cancer Control Modalities

199- Unspecified Medical Cancer Control Modalities

B. Psycho-Social

201- Clinical Behavior Modification

202- Inducing Entry into Health Care System

203- Encouraging Regular (Scheduled) Screening

208- Other Psycho-Social Methods of Motivating Cancer Control Target

211- Psycho-Social Rehabilitation and Readjustment

212- Vocational Rehabilitation and Readjustment

213- Social Services

214- Psychotherapy, Hypnotism, and Faith Healing

297- "All" Psycho-Social Cancer Control Modalities

298- Other Psycho-Social Cancer Control Modalities

299- General and Unspecified Psycho-Social Cancer Control Modalities

C. Education and Training

301- Education and Training

302- Testing the Efficacy of Education and Training Programs (includes surveys)

(continued)

XI. Mode of Cancer Control (continued)

D. Information Dissemination

401- Mass Print Media (includes mass distribution of pamphlets)

402- Professional Print Media (e.g., distribution of professional journals)

403- Electronic Media (e.g., films, tapes, television and radio broadcasts, computerized information dissemination systems)

404- Demonstrations and Exhibitions (includes posters and other graphic art forms)

405- Speeches and Conferences

497- "All" Information Dissemination Cancer Control Modalities

498- Other Information Dissemination Cancer Control Modalities

499- General and Unspecified Information Disseminiation Cancer Control Modalities

E. Legal/Political

501- Legislation

502- Judicial Decisions

503- Executive Orders

504- Regulations

505- Hearings and Recommendations

597- "All" Legal/Political Cancer Control Modalities

598- Other Legal/Political Cancer Control Modalities

599- General and Unspecified Legal/Political Cancer Control Modalities

F. Financial/Administrative

601- Allocation of Funds

602- Allocation of Manpower

(continued)

XI. Mode of Cancer Control (continued)

F. Financial/Administrative (continued)

603- Allocation of Facilities

604- Economic Disincentives

605- Economic Incentives

606- Planning Activities

697- "All" Financial/Administrative Cancer Control Modalities

698- Other Financial/Administrative Cancer Control Modalities

699- General and Unspecified Financial/Administrative Cancer Control Modalities

G. Record Keeping

701- Maintenance of Tumor Registries

798- Other Registry and Record Keeping Activities

H. Research

801- Etiological Research (not directly related to cancer prevention)

802- Research Directly Related to One or More of the Six Phases of Cancer Control (prevention, screening and detection, diagnosis, treatment, rehabilitation, and continuing care)

897- "All" Research Modalities

898- Other Research Modalities

899- General and Unspecified Research Modalities

I. "All," Other, and General and Unspecified Cancer Control Modalities

901- Environmental Modification (Public Protection)

902- Identification of People at Risk

997- "All" Cancer Control Modalities

998- Other Cancer Control Modalities

999- General and Unspecified Cancer Control Modalities

XII. Outcome and Conclusions

(Abstract only. No codes.)

XIII. Program Name and Dates

(Program Name is abstracted only. Program Initiation Date and Program Termination Date are Coded and included in the Program Name and Dates abstract. See XIIIB and XIIIC for Program Initiation Date and Program Termination Date codes.)

XIIIA. Program Name

Official program names will be preceded and followed by quotation marks. Abstractor-assigned program names will be preceded and followed by an asterisk.

XIIIB. Program Initiation Date

First two digits
01 - 12 = January - December

Second two digits
01 - 31 = First to Thirty-First Day of the Month

Last two digits
01 - 99 = 1901 - 1999

All six digits
000000 - Not Applicable
999999 - Unspecified

XIIIC. Program Termination Date

First two digits
01 - 12 = January - December

Second two digits
01 - 31 = First to Thirty-First Day of the Month

Last two digits
01 - 99 = 1901 - 1999

All six digits
000000 = Not Applicable
888888 = Ongoing
999999 = Unspecified

XIV. Program Information Source

(Abstract only. No codes.)

XV. References

(Total Number of References cited in Document is coded only. See XVA for codes. Number of References to Same Program is coded. See XVB for codes. Up to nine of the references to the same program are abstracted in the References to Same Program abstract. See XVC.)

XVA. Total Number of References Cited in Document

00- None or Not Applicable

01 - 97- 1 to 97

98- 98 or more

99- Unspecified

XVB. Number of References to Same Program

00- None or Not Applicable

01 - 97- 1 to 97

98- 98 or more

99- Unspecified (Includes references without titles)

XVC. References to Same Program

Begin first reference on card 101, second on card 201 , and so forth to card 901. A total of nine references to the same program may be keypunched.

M E M O R A N D U M

DATE: February 13, 1976

TO: Staff

FROM: Kerrick

RE: Zeros and Blanks

1) Continue using zeros as before in ID, Publication Date, Sample Size, Initiation Date, Termination Date, Total References, References to Same Program, and Journal Number.

2) Blanks should be coded with a line (——). Spaces should not be left empty.

3) Do not code blanks in the first block of coding spaces where you previously would have used zeros. Continue to use zeros in this situation.

4) Use blanks in subsequent blocks of coding spaces where you previously used zeros.

5) Continue to use zeros when they are part of a single code (e.g., 03, 210).

KF/Cp

M E M O R A N D U M

DATE: February 18, 1976

TO: Eva, Diane, Roger, Stewart & Michael

FROM: Kerrick

RE: Additional Code

On page 14 of your coding manual, under XI - Mode of Cancer Control; A - Medical, add 109 - Unspecified Medical Cancer Prevention Modalities. Please staple this memorandum to your coding manual.

KF/cp

June 16, 1976

DOCC

Databank of Cancer Control

SEARCH VARIABLES AND DOCC/SEARCH ABBREVIATIONS

Menu for Version of March 22, 1976

Instructions

1. The abbreviations given are minimum abbreviations. However, for clarity, you may type (for example) INTERVENOR instead of INT.

2. A search variable consists of both a short abbreviation AND a number. You must type them both for your searches to be successful.

DOCUMENT IDENTIFICATION NUMBER
abbreviated ID#
coded once per brief report

This search variable is really just the identification number of the particular document in which you are interested. You should search ID#, for instance, when you want to list only one or a few of a large number of brief reports which have met your previous search specifications.

SITE OF CANCER
abbreviated SIT
coded one to five times per brief report

00 - Not Applicable
01 - Buccal Cavity and Pharynx (Oral)
02 - Digestive Organs
03 - Respiratory System
04 - Bone, Tissue, and Skin
05 - Breast
06 - Female Genital Organs
07 - Male Genital Organs
08 - Urinary Organs
09 - Eye
10 - Brain and Central Nervous System
11 - Endocrine Glands

12 - Leukemia
13 - Lymphomas
97 - Six or More Sites
98 - Other Sites
99 - Unspecified Sites

INTERVENOR
abbreviated INT
coded one to three times per brief report

00 - Not Applicable
01 - Federal Government Organization (U.S.)
02 - State Government Organization (U.S.)
03 - County Government Organization (U.S.)
04 - Local or Municipal Government Organization (U.S.)
05 - Foreign Government Organization
06 - International Organization
07 - American Cancer Society
08 - Other Voluntary or Consumer Agency
09 - Private Foundation
10 - Professional Society or Organization
11 - Hospital and/or Clinic
12 - Educational Institution
13 - Prepaid Health Plan
14 - Industrial or Business Organization
15 - Physicians
16 - Other Health Professionals and Paraprofessionals
17 - Social and Related Workers
18 - Other Individuals
97 - Four or More Intervenors
98 - Other Intervenors
99 - Unspecified Intervenors

FUNDOR
abbreviated FUN
coded one to three times per brief report

00 - Not Applicable
01 - Federal Government Organization (U.S.)
02 - State Government Organization (U.S.)
03 - County Government Organization (U.S.)
04 - Local or Municipal Government Organization (U.S.)
05 - Foreign Government Organization
06 - International Organization
07 - American Cancer Society
08 - Other Voluntary or Consumer Agency
09 - Private Foundation
10 - Professional Society or Organization
11 - Hospital and/or Clinic
12 - Educational Institution
13 - Prepaid Health Plan
14 - Industrial or Business Organization
15 - Physicians
16 - Other Health Professionals and Paraprofessionals

17 - Social and Related Workers
18 - Other Individuals
97 - Four or More Fundors
98 - Other Fundors
99 - Unspecified Fundors

PHASE OF CANCER CONTROL
abbreviated PHA
coded one to five times per brief report

00 - Not Applicable (includes most research and registry programs)
01 - Prevention
02 - Screening and Detection
03 - Diagnosis
04 - Treatment
05 - Rehabilitation
06 - Continuing Care
97 - Six or More Phases of Cancer Control
98 - Other Phases of Cancer Control
99 - Unspecified Phases of Cancer Control

CANCER CONTROL TARGET
abbreviated TAR
coded one to three times per brief report

00 - Not Applicable
01 - General Population
02 - High Risk Population
03 - Cancer Patient Population
04 - General Patient Population
05 - Medical or Other Health Field Student Population
06 - General Student Population
07 - Federal Government Organization (U.S.)
08 - State Government Organization (U.S.)
09 - County Government Organization (U.S.)
10 - Local or Municipal Government Organization (U.S.)
11 - Foreign Government Organization
12 - International Organization
13 - American Cancer Society
14 - Other Voluntary or Consumer Agency
15 - Private Foundation
16 - Professional Society or Organization
17 - Hospital and/or Clinic
18 - Educational Institution
19 - Prepaid Health Plan
20 - Industrial or Business Organization
21 - Physicians
22 - Other Health Professionals and Paraprofessionals
23 - Social and Related Workers
97 - Four or More Cancer Control Targets
98 - Other Cancer Control Targets
99 - Unspecified Cancer Control Targets

SAMPLE SIZE
abbreviated SIZ
coded once per brief report

0000000 = None or Not Applicable (e.g., organizations)
0000001-2999997 = Exact Number of People
9999998 = 9,999,998 Or More
9999999 = Unspecified

You may search sample size ranges; for example, all brief reports with sample sizes between 100 and 1000 (S SIZ 100-1000).

AGE
abbreviated AGE
coded once per brief report

00 - Not Applicable
01 - Children Only (or mainly, at least 2/3) under 12
02 - Youth Only (or mainly, at least 2/3) 12-17
03 - Adults Only (or mainly, at least 2/3) 18-64
04 - Elderly Only (or mainly, at least 2/3) over 64
05 - 01 and 02 only or mainly
06 - 02 and 03 only or mainly
07 - 03 and 04 only or mainly
08 - 01, 02, and 03 only
09 - 02, 03, and 04 only
97 - All Ages
98 - Other Ages
99 - Unspecified Ages

SEX
abbreviated SEX
coded once per brief report

00 - Not Applicable
01 - Males Only
02 - 1 - 9% Female
03 - 10 - 19% Female
04 - 20 - 29% Female
05 - 30 - 39% Female
06 - 40 - 49% Female
07 - 50 - 59% Female
08 - 60 - 69% Female
09 - 70 - 79% Female
10 - 80 - 89% Female
11 - 90 - 99% Female
12 - Females Only
97 - Both Sexes but an unspecified percentage of either one
98 - Other (e.g., transsexuals who change sex during the course of the program)
99 - Unspecified

RACE
abbreviated RAC
coded once per brief report

00 - Not Applicable
01 - White Only (or mainly, at least 2/3)
02 - Black Only (or mainly, at least 2/3)
03 - Oriental Only (or mainly, at least 2/3)
04 - Hispanic Only (or mainly, at least 2/3)
05 - American Indian Only (or mainly, at least 2/3)
06 - About 50-50 of any 2 (meaning less than 2/3 of each)
07 - Non-White Only (or mainly, at least 2/3)
97 - "All" Races
98 - Other Races
99 - Unspecified Races

SOCIOECONOMIC STATUS
abbreviated SES
coded once per brief report

00 - Not Applicable
01 - Lower Mainly (2/3 or more)
02 - Middle Mainly (2/3 or more)
03 - Upper Mainly (2/3 or more)
04 - 01 and 02 Mainly or Only
05 - 02 and 03 Mainly or Only
06 - 01 and 03 Mainly or Only
07 - 01, 02, and 03 but less than 50% of any one
97 - 01, 02, and 03 and an unspecified percentage of any one
98 - Other
99 - Unspecified

GEOGRAPHIC LOCATION OF PROGRAM
abbreviated GEO
coded one to five times per brief report

00 - Not Applicable
01 - Canada
02 - Alabama
03 - Alaska
04 - Arizona
05 - Arkansas
06 - California
07 - Colorado
08 - Connecticut
09 - Delaware
10 - District of Columbia
11 - Florida
12 - Georgia
13 - Hawaii
14 - Idaho
15 - Illinois

16 - Indiana
17 - Iowa
18 - Kansas
19 - Kentucky
20 - Louisiana
21 - Maine
22 - Maryland
23 - Massachusetts
24 - Michigan
25 - Minnesota
26 - Mississippi
27 - Missouri
28 - Montana
29 - Nebraska
30 - Nevada
31 - New Hampshire
32 - New Jersey
33 - New Mexico
34 - New York
35 - North Carolina
36 - North Dakota
37 - Ohio
38 - Oklahoma
39 - Oregon
40 - Pennsylvania
41 - Rhode Island
42 - South Carolina
43 - South Dakota
44 - Tennessee
45 - Texas
46 - Utah
47 - Vermont
48 - Virginia
49 - Washington
50 - West Virginia
51 - Wisconsin
52 - Wyoming
53 - U.S. Possessions and Territories
54 - Nationwide (U.S.)
55 - Latin America (includes Mexico, Central America, South America, and the West Indies)
56 - British Isles (including Eire)
57 - U.S.S.R.
58 - Europe (excluding the British Isles, the U.S.S.R. and Turkey)
59 - Asia (excluding the U.S.S.R. and Egypt and including Turkey)
60 - Africa (including Egypt)
61 - Australia and New Zealand
62 - Antarctica
63 - Worldwide
64 - Off-World (e.g., Moon, Space Stations)
97 - Worldwide and Off-World
98 - Other (e.g., Pacific islands)
99 - Unspecified

MODE OF CANCER CONTROL
abbreviated MOD
coded one to five times per brief report

000 - Not Applicable

100 - retrieves all brief reports coded 101 through 199 (MEDICAL MODES)

101 - General Medical Exam nat
109 - Unspecified Medical rev · ·odalities
111 - Multiphasic Screening (for several diseases, including cancer)
112 - Multiphasic Cancer Screening (for several types of cancer, but not for other diseases)
113 - Cytological Smears (directed specifically toward the detection of cancer)
114 - Thermography
115 - Radiologic Screening (includes mammography and xeroradiology)
118 - Other Screening Modalities
119 - Unspecified Screening Modalities
121 - Diagnostic Radiology
122 - Biopsy
123 - Palpation (by medical personnel)
124 - Self-Palpation
125 - Endoscopy
128 - Other Diagnostic Modalities
129 - Unspecified Diagnostic Modalities
131 - Surgery
132 - Radiotherapy
133 - Chemotherapy
134 - Hormone Therapy
135 - Immunotherapy
136 - Hyperthermia
137 - Acupuncture
138 - Other Treatment Modalities
139 - Unspecified Treatment M dal ies
141 - Physical Restoration - Head and Neck Involvement
142 - Physical Restoration - Breast, Extremities, Gastrointestinal, and Genitourinary Involvement
148 - Other Medical Rehabilitative Modalities
149 - Unspecified Medical Rehabilitative Modalities
151 - Medical Palliative and Supportive Care
197 - Three or More Medical Cancer Control Modalities
198 - Other Medical Cancer Control Modalities
199 - General and Unspecified Medical Cancer Control Modalities

200 - retrieves all brief reports coded 201 through 299 (PSYCHOSOCIAL MODES)

201 - Clincial Behavior Modification
202 - Inducing Entry into Health Care System
203 - Encouraging Regular (Scheduled) Screening
208 - Other Psychosocial Methods of Motivating Cancer Control Target
211 - Psychosocial Rehabilitation and Readjustment
212 - Vocational Rehabilitation and Readjustment
213 - Social Services
214 - Psychotherapy, Hypnotism, and Faith Healing

297 - Three or More Psychosocial Cancer Control Modalities
298 - Other Psychosocial Cancer Control Modalities
299 - General and Unspecified Psychosocial Cancer Control Modalities

300 - retrieves all brief reports coded 301 or 302 (EDUCATIONAL MODES)

301 - Education and Training
302 - Testing the Efficacy of Education and Training Programs (includes surveys)

400 - retrieves all brief reports coded 401 through 499 (INFORMATION DISSEMINATION MODES)

401 - Mass Print Media (includes mass distribution of pamphlets)
402 - Professional Print Media (e.g., distribution of professional journals)
403 - Electronic Media (e.g.,film, tape, television, radio, computer, telephone)
404 - Demonstrations and Exhibitions (includes posters and other graphicart forms
405 - Speeches and Conferences
497 - Three or More Information Dissemination Modalities
498 - Other Information Dissemination Modalities
499 - General and Unspecified Information Dissemination Modalities

500 - retrieves all brief reports coded 501 through 599 (LEGAL/POLITICAL MODES)

501 - Legislation
502 - Judicial Decisions
503 - Executive Orders
504 - Regulations
505 - Hearings and Recommendations
597 - Three or More Legal/Political Modalities
598 - Other Legal/Political Modalities
599 - General and Unspecified Legal/Political Modalities

600 - retrieves all brief reports coded 601 through 699 (FINANCIAL/ADMINISTRATIVE MODES)

601 - Allocation of Funds
602 - Allocation of Manpower
603 - Allocation of Facilities
604 - Economic Disincentives
605 - Economic Incentives
606 - Planning Activities
697 - Three or More Financial/Administrative Modalities
698 - Other Financial/Administrative Modalities
699 - General and Unspecified Financial/Administrative Modalities

700 - retrieves all brief reports coded 701 or 798 (RECORD KEEPING MODES)

701 - Maintenance of Tumor Registries
798 - Other Registry and Record Keeping Activities

800 - retrieves all brief reports coded 801 through 899 (RESEARCH MODES)

801 - Etiological Research
802 - Research Directly Related to One or More of the Six Phases of Cancer Control (prevention, screening, diagnosis, treatment, rehabilitation, and continuing care)
897 - Both Research Modalities
898 - Other Research Modalities
899 - General and Unspecified Research Modalities

900 - retrieves all brief reports coded 901 through 999 (SIX OR MORE, OTHER, AND GENERAL AND UNSPECIFIED MODES)

901 - Environmental Modification (Public Protection)
902 - Identification of People at Risk
997 - Six or More Cancer Control Modalities
998 - Other Cancer Control Modalities
999 - General and Unspecified Cancer Control Modalities

TOTAL NUMBER OF REFERENCES CITED IN DOCUMENT
abbreviated TRE
coded once per brief report

00 = None or Not Applicable
01-97 = 1 to 97
98 = 98 or more
99 = Unspecified (e.g., incomplete or ambiguous references)

You may search a range of references; for example, all brief reports containing between 5 and 10 total references (S TRE 5-10).

NUMBER OF REFERENCES TO SAME PROGRAM CITED IN DOCUMENT
abbreviated SRE
coded once per brief report

00 = None or Not Applicable
01-97 = 1 to 97
98 = 98 or more
99 = Unspecified (e.g., incomplete or ambiguous references)

You may search a range of references; for example, all brief reports containing between 2 and 7 references to the same program (S SRE 2-7).

FOR ADDITIONAL INFORMATION, PLEASE CONTACT:

Databank of Cancer Control
UCLA School of Public Health
10833 LeConte Avenue
Los Angeles, California 90024
Telephone: (213) 825-1240 or (213) 825-6110

5. BRIEF DEFINITION OF CANCER CONTROL

BEING USED BY

HISTORY OF U. S. CANCER CONTROL PROGRAMS PROJECT

The aims of the cancer control activities are:

- to identify potential cancer control methods or techniques which have been developed in research settings
- to conduct necessary testing of control methods and techniques in community settings
- to evaluate their applicability for widespread community use
- to promote the appropriate widespread community use of methods and techniques that are found applicable

These activities include: prevention, screening and detection, diagnosis and pretreatment evaluation, treatment, rehabilitation, and continuing care.

Cancer control includes developmental research, i.e., the identification of new methods and techniques and their field testing and evaluation in limited community settings, and community demonstration and application activities, i.e., the promotion of community-tested cancer control methods and techniques to ensure their appropriate application and use.

Cancer research seeks to find the means for combating cancer, where cancer control is concerned with identifying, community testing, evaluating, and promoting the means that are found.

From Working Group Report: National Cancer Program Planning Conference: Summary Report for Cancer Control. June 1975. USDHEW, PHS, NIH, NCI

PERSONS INTERVIEWED

	Name	Interview Date	Transcript	Tape Only	Notes Only
1)	Adair, Frank.	5/18/76	X		
2)	Adams, Lane W.	1976		X	
3)	Arje, Sidney	4/27/76	X		
4)	Baker, Carl	5/1/76	X		
5)	Barckley, Virginia	1976			X
6)	Batten, Grover	12/8/75		X	
7)	Berlin, Nathaniel	1976			X
8)	Bratic, Elaine	12/19/75	X		
9)	Brennan, Michael J.	4/26/76			X
10)	Breslow, Lester	12/2/75	X		
11)	Brown, Helene	1976			X
12)	Burney, Leroy	1976			X
13)	Cameron, Charles	5/22/76	X		
14)	Carlile, Thomas	1976			X
15)	Christopher, Paul	6/15/76	X		
16)	Coggeshall, Lowell	1976			X
17)	Cole, Warren	5/18/76	X		
18)	Copeland, Murray.	4/27/76			X
19)	Cullen, Joseph	9/30/76			X
20)	Davis, Alan	5/21/76	X		
21)	deHarven, Gerry	1976			X
22)	Dublin, Thomas (Telephone).	1976			X
23)	Dunn, John	4/1/76	X		
24)	Edwards, Margaret	11/3/75	X		
25)	Egan, Robert L.	4/76			X
26)	Endicott, Kenneth	5/19/76	X		
27)	Epstein, Samuel	12/23/75		X	
28)	Fink, Diane	11/4/75	X		
29)	Foote, Emerson	4/76			X
30)	Fowler, Evonne (Telephone).	9/9/76			X
31)	Frechette, Alred	6/16/76	X		
32)	Goldman, Lee	1976			X
33)	Hammond, Cuyler E.	1976		X	

	Name	Interview Date	Transcript	Tape Only	Notes Only
34)	Healey, John R., Jr.	4/76			X
35)	Heller, John R.	11/4/75	X		
36)	Henderson, Sourya	1976			X
37)	Hermel, Mortimer B.	4/27/76			X
38)	Holleb, Arthur J. (2 Interviews)	4/29-5/21/76	X		
39)	Horn, Daniel	1976	X		
40)	Hueper, Wilhelm C. (3 Interviews)	12/75	X		
41)	Isard, Harold	4/76			X
42)	James, Walter	4/26/76	X		
43)	Johnson, Craig	6/14/76		X	
44)	Kaiser, Raymond	4/27/76	X		
45)	Kolbe, Henry	6/17/76	X		
46)	Koss, Leopold	3/5/76	X		
47)	Kotin, Paul	1976	X		
48)	Kramer, Simon	1976			X
49)	Lasker, Mary	3/16/76	X		
50)	Lasser, Terese (Telephone)	9/5 & 9/9/76			X
51)	Lawrence, Sherwood	1976			X
52)	Lawton, Stephen; Glison, Joanna; Maher, Robert (Staff to Congressman Paul Rogers, Dem.-Fla.)	5/20/76	X		
53)	Lee, Richard	12/9/75	X		
54)	Leitman, Cynthia	9/17/76			X
55)	Levin, Morton	1976			X
56)	Lipworth, Leslie	1976			X
57)	Lombard, Herbert & Olive	6/18/76	X		
58)	Longmire, William P.	2/5/76		X	
59)	Mancuso, Thomas	4/27/76	X		
60)	Markel, William	5/21/76	X		
61)	Mayer, Andrew	11/17/75	X		
62)	McGrail, Richard	1976			X
63)	McSchulskis, Jack	11/6/75		X	
64)	Melton, William	1976			X
65)	Miller, Eugene	4/76	X		
66)	Miller, John R.	1976		X	

	Name	Interview Date	Transcript	Tape Only	Notes Only
67)	Mirand, Edwin	6/14/76	X		
68)	Murphy, Gerald	6/14/76	X		
69)	Neave, Charles	6/16/76	X		
70)	Papanicolaou, Mary	5/22/76			X
71)	Phillips, Harry	1976			X
72)	Present, Arthur	5/1/76	X		
73)	Quisenberry, Walter	12/8/75	X		
74)	Raucher, Frank J.	11/5/75	X		
75)	Rigler, Leo	8/12/76			X
76)	Robbins, Lewis C.	11/20/75	X		
77)	Roberson, William	11/6/75		X	
78)	Ross, Joseph	12/2/75		X	
79)	Ross, William	1976			X
80)	Saffiotti, Umberto	11/6/75			X
81)	Saunders, J. Palmer	1/13/76	X		
82)	Scheele, Leonard	12/17/75		X	
83)	Schoefield, Gerald	1976		X	
84)	Schottenfeld, David (Telephone)	1976			X
85)	Shannon, James A.			X	
86)	Shimkin, Michael	1/27/76	X		
	" " (Telephone)	8/9/76			X
87)	Sinon, Virginia (Telephone)	1/13/76			X
88)	Sklaroff, David	4/27/76			X
89)	Sloan, Margaret	11/3/75	X		
90)	Stemmerman, Grant			X	
91)	Strax, Philip	4/26/76			X
92)	Strickland, Stephen	12/17/75		X	
93)	Stronach, William	1976			X
94)	Taylor, Howard Jr.	4/27/76	X		
95)	Terris, Milton	2/3/76		X	
96)	Terry, Luther	4/27/76			X
97)	Vana, Josef	4/76		X	
98)	Vivona, Stefano	4/76		X	
99)	Warren, Shields	6/15/76	X		
100)	Warren, Stafford	1976			X

	Name	Interview Date	Transcript	Tape Only	Notes Only
101)	Wintrobe, Maxwell	11/3/76	X		
102)	Wood, David	11/11/76			
103)	Wood, J. Congdon (ACS)	11/27/76			
104)	Wynder, Ernst	11/26/76	X		
105)	Yarborough, Ralph W. (Senator) . . .			X	
106)	Yatsuhashi, Maseo	6/17/76	X		
107)	Zubrod, Gordon C.	11/20/76	X		
108)	Zwitman, Daniel (Telephone)	9/5/76			X

APPENDIX 3

INTERVIEW METHODOLOGY

An unusual methodological aspect of this Project was the deliberate decision (backed by budgeted funds) to conduct in-depth, tape-recorded interviews with living individuals who have contributed to the evolution of cancer control policy and programs.

Shortly after the Project began, a letter (attached) was sent to over 80 such potential subjects. Their responses helped to refine the list of individuals who would prove to be productive informants. The first wave of actual taped interviews was undertaken in November, 1975, prior to extensive literature research. Each subject received in advance an "interview framework," indicating fundamental questions upon which the interview would focus. Those interviews lasted from one to four hours each.

The Project consultants, upon reviewing two examples of such interviews, recommended certain changes in the process. Consultation was sought from UCLA Sociology Professor Eugene Levine, who conducted a simulation exercise in which all HCCP research/writer staff participated.

The majority of interviews were conducted in the spring of 1976. By that time considerable literature search, draft chapter development, and discussion had been completed. In general, these "second wave" interviews were more pointed and productive; the interviewers were better informed and maximized the time profitably to focus on issues of controversy or policy, and to expand on information gained from the published literature.

For the most part, a staff interviewer conducted in-person or telephone interviews with individuals whose viewpoints were vital to the chapters for which he or she was responsible. Obviously, some informants provided a wealth of information on a variety of topics; occasionally, two staff conducted the interview jointly; more commonly, a single interviewer attempted to cover the range of information, using questions compiled by other research staff as well as by himself or herself.

More than 60 interviews were taped; most of these were transcribed and returned to the informant, who was then asked to correct any misspellings of names, any errors in transcription, and, most important--to indicate any statements to be deleted in a final typed interview product. In this way, some candid statements that an individual might have made about other persons could be deleted, without sacrificing the major substance of the interview experience.

The majority of taped interviews were conducted by Devra Breslow, who was principally responsible for researching and writing Book Two. Mr. Agran and Miss Morganstern each conducted at least 10 in-person taped interviews and many others by telephone. Dr. Ellwein conducted two taped interviews. Dr. Breslow participated in several interviews and conducted several himself, using notes only.

As drafted chapters neared refinement, some individuals were called again to verify statements made in the interviews. As a point of historical interest only, perhaps, the final three interviews were: a four-hour in-person taped interview with Dr. James Shannon, long-time Director of the National Institutes of Health (conducted by historian Arthur Viseltear, Ph.D.); a telephone interview (conducted by Mrs. Breslow) with former United States Senator, Ralph W. Yarborough, principal congressional architect of the

1971 National Cancer Act; and an interview (by Mrs. Breslow) with the Project Principal Investigator, Dr. Lester Breslow, concerning the principal factors, events, activities and policy issues affecting the California state cancer control program from 1947-67.

Beyond our own subjective assessment that in-depth interviews enrich the commissioned report and help establish its credibility, there are several points which might be useful to historians who consider this method.

- In general, subjects are most cooperative and informative in a familiar setting: a home, office, laboratory, with access to files or documents mentioned in the interview.
- Rarely is a "cold" interview very productive. A "cold" interview is one in which the informant has not had the opportunity to consider prepared questions for at least a week in advance of the actual interview, thereby allowing time to organize thoughts and to assemble supporting materials.
- Sexist as this may sound, male subjects appeared generally more comfortable speaking to women interviewers than to men interviewers.
- The interviewer can be <u>too</u> informed and, unless care is taken, thereby prejudice spontaneous remarks, or force the person being interviewed to move on too quickly to another topic.
- If the interviewer obviously enjoys the process, the subject is put at ease. Setting up and testing the tape equipment should be done smoothly and quickly, so the primary purpose of the taped interview is not obscured.

. The confidence of the informant in the discretion of the interviewer is paramount. While, on later review, the informant may strike out remarks he/she does not want permanently attributed, it is the interviewer's early task to inform the subject how the information is to be used, and the extent to which confidentiality will be maintained.

UNIVERSITY OF CALIFORNIA, LOS ANGELES

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SCHOOL OF PUBLIC HEALTH

HISTORY OF CANCER CONTROL PROJECT
1100 GLENDON AVENUE
SUITE 2050
LOS ANGELES CALIFORNIA 90024

September 19, 1975
j

Dear j:

The UCLA School of Public Health has been awarded an 18-month contract by the NCI to compile a History of Cancer Control in the United States, 1946-71. I am the principal investigator. A staff of research analyst-writers has been assembled. The bibliographic phase of the study is well under way.

In anticipation of launching the second phase of the study--75-100 in-depth interviews with key persons involved in cancer control efforts--we are seeking your guidance.

1) In your estimation, who are the individuals who should be subjects of such in-depth interviews? (Our aim is to select those individuals and agency personnel whose contributions scientifically, socially, or by virtue of other influence substantially advanced important cancer control programs--national, state, regional, local, professional, public or private).

2) Specify 5-7 scientific technological and/or social advances which have contributed to control of cancer or which are potentially contributory (for example: mammography; the emergence of voluntarism; the growing liaison between government and voluntary health agencies to promote cancer control; chemotherapy).

3) Identify any historical books, reports, monographs, or other documentation of a technological and/or social movement, which you believe our staff should review either for methodology or findings.

A return postcard is enclosed. Please specify on it the times when you are most easily reached by telephone and the numbers at which you may be reached. One of the project staff members will call you within 3 weeks for your responses to the above questions. If it is easier to write your reply, by all means do so, responding to me at the project address above.

The end-product of this study will be, first of all, an assessment of progress in cancer control and an analysis of factors contributing to or thwarting that progress, for immediate use by the Division of Cancer Control & Rehabilitation. We also hope to produce a book or series of articles for the general public.

We are excited about the possible value of this study and the process. We look forward to interviewing you in greater depth during the next 10 months, and we thank you in advance for your cooperation.

Sincerely yours,

Lester Breslow, M.D., M.P.H.
Dean, UCLA School of Public Health
Principal Investigator, HCCP Project

LB:bl
Encl. (1)

I N T E R V I E W

Interviewee: Dr. Lester Breslow

Interviewer: Larry Agran

Location: UCLA - School of Public Health

Date: December 2, 1975

Dean Lester Breslow
December 2, 1975

Larry: As I mentioned, I wanted this interview to be exclusively limited to the cigarette issue. I thought initially, perhaps, I could begin by trying to draw you back as far as your mind can take you with respect to your recollections, on a non-professional basis, of your first experience with cigarettes. Perhaps as a child or teen-ager.

Breslow: My first personal recollection about cigarettes was about the time I was an intern. I was then about 23 years old. Maybe it was a little before that time, but I think it was about that time when I thought that I would explore smoking. I bought a package of cigarettes and tried them out. I didn't especially care for them and I tried two or three different brands. I remember one in particular, Phillip Morris, which advertised that they were especially easy on the throat. After trying those two or three different brands, I gave it up and then I decided I'd try a pipe. So, I bought an inexpensive pipe and tried various kinds of pipe tobacco. Again, after a month or so of exploration I gave it up; it just irritated my throat. I may have smoked an occasional cigarette after that, but my experience was really limited to that period of exploration--it couldn't have lasted over a period of a few weeks--with both cigarettes and a pipe. I don't recall ever trying a cigar, although I might have done that too.

Larry: This period of exploration was a personal matter, not as a professional matter?

Breslow: Oh, no, it was purely personal. Other people were smoking so I thought I'd try it.

Larry: Was it attractive to you?

Breslow: No, it wasn't especially attractive.

Larry: Sounds like you made a fairly persistant effort.

Breslow: Well, I gave it a reasonable try.

Larry: You were 23. What year was that?

Breslow: That was in 1938-39.

Larry: At this time were there any suspicions at all professionally, among any of your colleagues, that smoking might not be healthy, not necessarily in an epidemiologic sense but, perhaps, looking at some of their patients and thinking that their coughing might be induced by cigarette smoking?

Breslow: Not among any of my colleagues either in the limited medical circles nor in epidemiologic or public health circles. To emphasize that point, I rec all a few years later, in approximately 1947 when I had become the chief of the Bureau of Chronic Diseases in the California State Health Department, I received routinely from the bureaucratic channel a request for comment on an educational pamphlet pertaining to the likelihood that cigarette smoking could cause a lot of terrible diseases. I remember reading

through that pamphlet and red penciling out implications that cigarette smoking and disease were related. This all appeared to me to be nonsense at that time. I don't even recall what diseases were mentioned.

Larry: But you remem-er it that strongly that you regarded it as not simply unproven, but actually as nonsense?

Breslow: Right.

Larry: This was 1947.

Breslow: Spring 1947. I was aware that there were indications and announcements by Overholt and Graham that cigarette smoking was an important factor in lung cancer, because they noticed in their patients that this was a very common thing. So many people smoked, woman as well as men, and lung cancer was basically appearing in men, that it didn't make sense to me that cigarette smoking could be cancerous.

Larry: Did you entertain other possibilities for lung cancer?

Breslow: Oh yes. The hypothesis that was most prominent in my mind at that time, and still remains a hypothesis was the substantial relationship between lung cancer and certain occupations, such as chromate ores and radioactive ores.

Larry: This was in a sense reinforced, I take it, by the experience with women.

Breslow: That's correct. That led nationally to the notion that it must be an occupational factor because of the very strong sex relationship. So we began making studies in the late 40's of the relationship between occupations and lung cancer because that was a very important hypothesis, and studies that were undertaken in the California State Department of Health in those days did contribute to the knowledge of this matter.

Larry: So this was the avenue in which you were pursuing? In other words, you were concerned about the lung cancer?

Breslow: I was concerned about lung cancer because it was very rapidly growing and it was evident by the latter 1940s that we were dealing with a long-term epidemic disease. That is what most impressed me about the phenomenon. Also it was a prototype, possibly, for other chronic diseases where the epidemic curve was not a matter of days, weeks, or months but of decades. I first got that notion in connection with lung cancer in observing that the disease was barely known in the 1920's and began to arise in the 30's and about the late 40's, it was obviously an epidemic swing.

Larry: Were you able to get much support among your colleagues for this concern?

Breslow: Yes, there was growing interest among people in chronic disease about epidemiology of lung cancer. I recall meeting Bill Hueper and others who were exploring occupational factors in lung cancer, and they stimulated my interests along that line of inquiry. About 1947-48, we were visited in Berkeley by a medical student named Ernest Wynder and he came with the

hypothesis that cigarette smoking was the factor involved with lung cancer.

Larry: Did he just show up? Was he introduced by someone?

Breslow: He came after some introduction, by himself or a telephone call or letter. He didn't just drop in. He came in rather suddenly with an obvious and very strong conviction that cigarette smoking was a factor in lung cancer. He got this notion from his association in St. Louis, where he had gone to medical school,with Evarts Graham. Wynder had undertaken a retrospective case control study and he came by to let us know that he was going to be visiting the hospitals in the Bay Area to interview patients and controls in regard to cigarette smoking practices as a part of his studies. We thought that he was a rash young man and asked whether a member of our staff could accompany him. Our staff member came back with a horrendous story of poor technique, so we decided that we ought to do a proper kind of a study. Consequently, we combined the cigarette smoking hypothesis with our occupational hypothesis in the studies we were about to undertake and did undertake, and publish in the early 50's. We were quite astonished with the results which were almost identical with those that Wynder was obtaining.

Larry: Your own studies that you mentioned, when were those published? How might we relocate them?

Breslow: The first study listed in my bibliography that refers to cigarette smoking was published in 1951, a publication in the Journal of the California State Departemnt of Public Health under the title "Does Cigarette Smoking Cause Lung Cancer?"

Larry: When Wynder came around and tried to interest you in his hypothesis, did you regard it still as far fetched or by that time did you already give more consideration to the cigarette smoking/cancer hypothesis?

Breslow: By that time, the hypothesis was being advanced nationally and we were aware that studies were underway, Wynder's in particular, so it seemed desireable for us to make our own study.

Larry: When you completed the study to that point, were you a believer?

Breslow: I began to come around to the notion. In 1950, it seemed to me that the retrospective and case control studies of the matter were vulnerable methodologically on the grounds of biased samples, people already having the disease, and select people being further interviewed. It did cause a certain amount of doubt about the significance of the restrospective studies. As I recall, I suggested to Harold Dorn that the issue was only going to be resolved when we had prospective studies, and I also suggested that there should be studies carried out by assembling large populations to obtain people's cigarette smoking histories before there were any illnesses. Then we would not have to be concerned with restrospective falsification and lack of memory. If several populations were studied in that fashion, then the evidence would be overwhelming. So quite early, it was evident to us that kind of study would be necessary. We then, of course, started those kind of studies.

Larry: Did yours proceed Horn and Hammond's?

Breslow: No. This idea was not unique to us, several people had the same idea. A whole series of studies--by Doll of the British physicians, a study

by Hammond and Horn of the American Cancer Society volunteers, and a study by Dorn of veterans and Canadian veterans. We had two studies in California--one of members of the American Legion, whom we selected because their age was such that they were coming into the lung cancer period and also because we could have access. The second was carried out among several occupational groups. So we carried out two of the prospective studies which along with five others were ultimately incorporated into the Surgeon General's report as the seven studies that were most compelling in relationship to cigarette smoking and lung cancer.

Larry: You were doing the study on occupational and the smoking at the same time, integrating the two?

Breslow: Yes. What we would do was interview the patients with lung cancer about their occupational backgrounds and about their cigarette smoking habits. The methodological advantage of the restrospective or case control type of study is that you can get information on several hypotheses. In fact, we had a paper on this discussing the advantages and disadvantages of case control versus long-term studies. The advantage of the retrospective case control studies is that the patients already have lung cancer and you can ask them about cigarette smoking, other kinds of smoking, about occupational exposures, or any other factor that you may think worthy of investigation, and you can do this in the same investigation. It is no more difficult to ask a few more questions once you've got to the person's bedside, where this kind of study is usually carried out, and then find a control and ask similar questions. So we were exploring the occupational hypothesis added in with the cigarette smoking hypothesis at the same time. In that study, we identified several occupations that we regarded as suspect and a positive factor in lung cancer. We, of course, also identified cigarette smoking as a factor.

The second set of studies we did, the long-term prospective studies, involved assembling large scale populations of men in selected occupations. We went to the unions, to industry and indicated that there was some reason to believe their occupation was a factor in lung cancer. So we carried out the study by getting from the unions or management the names of about 10,000 persons we could obtain and we simply entered them into our investigation, and ascertained subsequent mortality from lung cancer. There too, we were able to get information both about their particular occupations and about cigarette smoking.

Larry: Was the executive branch supportive of this?

Breslow: Executive branch of what?

Larry: State government.

Breslow: I don't think they were even aware of it.

Larry: No problems then?

Breslow: The only problem I remember was with one man who was a very prominent American Legioneer. He wrote to me expressing outrage that such a fool study was being carried out and that it was a waste of the taxpayer's money by the state, and what possible value could the maiden name of his mother be to any health study. I wrote back to him in a very nice way, enclosing another questionnaire and also a copy of the California death certificate. I pointed out that the questions we had asked on the ques-

tionnaire were identical in form and arrangement with those on the California death certificate and the purpose was to make it easy to identify the people who died so that we could study the relationship of death to the things we were asking about including cigarette smoking. He wrote back and said that he understood and he enclosed his questionnaire. Apart from that incident we had a little, but not very much resistance from the hospitals who were concerned with our talking with patients. Actually, we had very little resistance from the hospital systems.

Larry: When you were working on all of this, were you truly struck by the potential enormity of the human tragedy that was involved?

Breslow: Yes, I was struck by that curve. Almost every year one could see that it was still going up. In the late 40's and 50's it was the men, and then we became convinced that cigarette smoking was a factor but did not account for all lung cancer because at that time it was also overwhelmingly clear that a certain portion was due to other environmental exposure, particularly occupational. Then we understood why it was that women did not have lung cancer; that was because they did not take on smoking on a large scale until the 40's, whereas men did so before 1920. Although there was then practically no indication that the curve was rising in women, we predicted that there would be a rise probably in the 60's which was about 20 years later.

Larry: In the early 50's, when you and your colleagues around the nation were coming to the same conclusions, was there a sense of despair or optimism about it? In other words, was the expectation positive?

Breslow: In those days, my colleagues around the country expressed a feeling of excitement for the investigation. Here was a major epidemic of a new kind that man himself created and the evidence was now coming that a truly massive epidemic in disease and major killer in men was bound to occur also in women. The feeling that I had in the early days of the 50's was that we've discovered something. The policy implications and such came more slowly. If you examine some of my earlier writings, you may find that my recollection is not completely accurate as to when we began to emphasize the public policy aspect, but I think that as early as 1951 I raised the issue of campaigning against cigarette smoking. However, in 1951, it was only an idea to campaign against cigarette smoking as the evidence had to obviously be more compelling.

Larry: Let's say that by 1955 when the consensus among those who cared was pretty clear, and you take that as an important date and then look at the history of the cigarette policy, you have really at least a ten-year hiatus before Congress addressed itself to the issue.

Breslow: I recall writing a paper in 1955 entitled "Occupations and Cigarette Smoking as Factors in Lung Cancer," published in the American Journal of Health. By that time, as you can see, I was quite convinced that cigarette smoking was an important factor in lung cancer. I think that was the word we began to use. We staved away from the word "cause" because that was a word we realized as not being very acceptable in the scientific community, so we emphasized it as being a "factor" or sometimes a "positive factor." In some earlier papers you would see such words as "association," "relation," "factor," "positive factor," and then "cause," which was sort of the evolution that I recall.

To get at your question, by that time, many of us began to involve ourselves

a little in the public policy aspect, but the situation was such that we were pygmies compared to the industry and the prevailing attitudes and habits in America and other countries.

Larry: Were you ridiculed? Did you find your views being ridiculed by public testimony?

Breslow: I don't recall our being attacked on moral grounds, but the cigarette smoking industry began to respond quite early by forming a tobacco research council which did two things; one was to encourage the study of other factors in lung cancer, such as air pollution, etc. The other thing was to employ people like C.C. Little (and others) who did write polemics in the semi-scientific literature about the matter and ridicule it on scientific grounds. Further, they began to publish studies of others who began to look at the evidence critically and advanced notions as to why the evidence was not as compelling.

Larry: Did you find that kind of combat frustrating?

Breslow: Oh no. I found it exciting. There were a lot of statisticians in those days, and others, who began to point out alleged discrepancies in the evidence. I regarded it as an intellectual challenge and the thing to do was to bring forth more evidence and present it more compellingly.

Larry: How about later when you came up against the intransigence of the industry? I take it your stronger feelings were reserved for those "scientists" whom you believed to sell out as opposed to the vested economic interests. You just figured they were playing their role and it was perfectly understandable?

Breslow: Yes. I can understand why the cigarette manufacturers would employ advertising agents and polemicists to espouse their point of view because that's the kind of society we live in. In example of the evolution of feeling, I recall seeing on television in the 60's, Elmer Hess, the urologist of Erie, Pennsylvania who was then the president of the American Medical Association, smoking a cigarette while being interviewed by a news man and saying at the same time that cigarette smoking could not possibly cause lung cancer because he smoked cigarettes and he didn't have lung cancer. Approximately one year after that television appearance, I read in a newspaper that Elmer Hess had died of lung cancer and I had a peculiar feeling about that.

Larry: Graham died of lung cancer didn't he?

Breslow: Yes, I knew Evarts Graham quite well. It happened early in 1952 when President Truman appointed the Health Commission on which Graham was appointed to be a member. I joined the staff of the Commission and became the study director and, in the course of that work, I had many conversations with Evarts Graham in and around the sessions. I recall his discussing the question of cigarette smoking and lung cancer several times. We spoke of the medical student, Ernest Wynder, who I guess was still with him. I recall also that Evarts Graham had deeply stained fingertips from long exposure to tobacco in 1952. By that time he had quit smoking.

He was also at that time, like many of his colleagues, a very vigorous exponent of stopping cigarette smoking. Subsequently he died and the diagnosis was lung cancer.

I think, however, that the histologic type of the cancer in his case opened some question as to whether it was associated with smoking.

Larry: Did he have a lung removed?

Breslow: I don't recall, but that can be no doubt ascertained from Wynder. Wynder would know the complete detail and he might be worth getting information from.

Larry: Yes, I hope to meet with him over the Christmas vacation if he's out here.

Breslow: That's right, he's supposed to be coming out here.

Larry: If not, I'll catch up with him.

Let's get into the politics of the '64 Surgeon General's report. Dealing with the public policy realm, what were your expectations and did you quickly have to adjust your expectations? From the beginning, did you not expect to get too far too fast?

Breslow: I guess my expectations were always moderated by the knowledge and belief that we were going to have to contend with a mammoth industry and the economic thrusts of that industry in American life. My recollection is that in the late 40's we became interested in the problem; and in the 50's I explored with other colleagues of mine. Then in the later 50's and early 60's, the prospective studies provided the evidence that later came in. By that time, the Surgeon General's report came along. I recall testifying to the Surgeon General's Committee with Bill Cochran and other members of the group who were considering the evidence. That body had been carefully selected to eliminate all of those who had been caught up in investigating the situation themselves--everybody of any merit scientifically and involved had already come to some conclusion by that time. So the Surgeon General, in selecting a committee, had to pick people who were unbiased so to speak, which meant that they hadn't investigated the problem, so that their findings would not be subject to challenge by the industry. By that time, the early 60's, my feeling was that the time had come to mount a campaign against cigarette smoking. I don't remember exactly when, but increasingly I moved in that direction, like a lot of other people in the field did. By the early 60's we felt that the time had come to act. So we looked upon the Surgeon General's report, not so much as a scientific venture, although it was that--it was sort of a summary of the scientific work by a prestigous neutral body that could not be attacked because they had personally become involved with the studies--it was looked upon, not so much as a scientific venture, but as a public policy venture. That would make possible, we felt--I felt and I think others shared this view--the development of public policy in the country. So that was the feeling that I had toward it when it was being developed in '63-64' and was published. And then we found that it could be used for that purpose. So from that point on, I was no longer interested in the investigative aspect of cigarette smoking and lung cancer, although increasingly interested in the relationship between cigarette smoking and other diseases. That became a matter of scientific investigation. But with regard to cigarette smoking, now the time had arrived to begin a public campaign against cigaretts smoking.

Larry: Let me ask you about the '64-74' period. Let's say you ad been cigarette czar for this country, in 1964, assuming that they had such a post. You didn't have to contend with political problems. What kind of public policy would you have instituted?

Breslow: Well, I would have instituted the policy that I advocated at the First World Congress on Cigarette Smoking and Health. I don't recall exactly when that was, but it was in the middle 60's. I think probably we could even find the paper, or at least some fragments of it, that I presented at that time. That was the position that I would have advocated, or tried to carry out if I were as you say, czar for cigarette smoking in 1964. That position was that it was primarily an economic issue. Of course we wanted to educate the people on it. We have an obligation to give people the facts and seek to motivate them to do the things that are in their own personal health interest. So I am not denegrating the importance of education of the public. I think we have that as a social obligation, and so I strongly favor the development of educational programs, personal and mass education. But, to really get control of the situation, I was early convinced that the approach would have to be an economic one. Therefore, if I had been czar in those days, I would have initiated a Federal Government program to convert the use of the tobacco growing land in this country to other uses. The tobacco industry, like any other industry, consists of land, people, other capital resources that are put in, as well as the operating costs of the industry. If one is really going to change it in a reasonable time, then it seems to be that we have to approach the matter in the same way that we approached ship building in World War II. We had some ship building, but it was necessary to build a so-called Liberty Ship and a lot of other kinds of ships in a hurry, in those days. We did have the capital, the other resources necessary to build the ships that we needed to win World War II. It required a tremendous Government investment to do that and we made it. Now, I believe that we could and should do the same thing regarding the cigarette industry. We should acknowledge the fact that some land, some people, other resources are devoted to an economic persuit. It supports the livelihood of many people, and the way to approach it is economically, to offer a substitute. I would, for example, suggest that the land be converted to growing soybeans and other crops that are very much needed in this country to feed humans, to feed cattle, to use for overseas shipment, both humanitarian and economic exchange purposes. It would be a great advantage. So I would have done that in '64.

Larry: That deals with the supply side. What about the demand side? I don't know how many people were smoking at that time, 50 million or so habituated to one degree or another to cigarette smoking. How would you have dealt with that side?

Breslow: Well, with education as I mentioned. But, as long as you have in America an industry with a product that is being pushed upon people, that had a tremendous impact on what you call the demand side. In Los Angeles, in the 20's and 30's there was a transit system. I remember it even in the 40's, riding in some of the so-called red cars. So there was a transport system around which Los Angeles could have been built. It would have perhaps appeared a little different, I am not sure how much different it would have appeared, if we had those street cars, or electric cars plus buses. But the automobile industry in this country found that Los Angeles was a prize market. They began developing automobiles and passing legislation in the State Legislature to use tax money from gasoline to build freeways and other highways where automobiles could go. The industry built what you call the demand. So I don't think that one could have dealt with automobile driving in Los Angeles by educating people not to drive automobiles. They were relying on automobiles for transportation.

If one would have had to deal with it and to deal with it effectively it would have to be economically: that is, combat the automobile, and not by exhortation--what wasn't done very much, but by building a good transit system. Well now, 20 or 30 years later we are getting around to the point that we have to have a rapid transit system. Too bad in America things are based so much on short range returns in industry, both to those who are exacting a profit from industry and also those who are employed in the industry. This leads to a poor quality of life, whether it's health or transportation. We have not yet arrived at the point socially in this country where planning is effective. Social values are always put second behind narrow, short range economic interests. Cigarette smoking is a very good example. The Los Angeles transport system is another good example.

Larry: This is very useful. I appreciate this. If there is anything special that I may have forgotten, now would be a good point.

Breslow: No, I think only to round out the thing, that I advocated at the First World Congress on Cigarette Smoking in New York City, I can't remember the exact year, that we would one day have to approach the matter economically. The sooner we got around to that the better. One day, the people determining public policy in the country, whether in politics or other ways of influencing public policy, would have to get around to the economic approach. I would still like to see that. I will still advocate that as nationally the central thrust of endeavor. It's perfectly obvious that in the U.S. Congress, for example, there are just enough states, not very many if you count them, (one way three, another way six states and quite small states compared to California and New York), that hold up progress on the cigarette smoking issue because of their parochial, locally important industry. Until that issue is tackled and the Congress is willing to deal effectively with those few states, and their interests, then I don't think we are going to have very much progress. The Congress should come to the point of dealing with those states fairly. Their grandfathers, their fathers started out growing tobacco for cigarettes. You can't blame them for becoming caught in an industry that is producing deaths.

Larry: I take it that you considered view is that in addition to the moral questions that might be involved, that approach would be a very cheap buy-out on our part.

Breslow: Oh yes.

Larry: I mean the amount that would be necessary to deal with any hardship would be very small, indeed, when compared to the economic savings if nothing else.

Breslow: That's right. As a matter of fact, I am just now engaged in endeavoring to recruit someone to our school of public health who is particularly competent and interested--a very serious person--who would make this one of the central aspects of his work. If we are successful with that recruitment, I will be delighted.

Larry: That would be great. Well, again this is very helpful. I will no doubt want to get back to you with some specifics that I may have omitted or documents that I may want to request.

Breslow: My emphasis on the industrial and economic side--I don't mean in any way, however, to take away from my committment to education. Because I think we need to approach that as well.

Larry: I understand that. I didn't pursue it further because I know of your interests along those lines, and they are well documented actually.

APPENDIX 5

I N T E R V I E W

Interviewee: Dr. Charles Cameron

Interviewer: Devra M. Breslow

Location: Miami, Florida

Date: May 22, 1976

DEVRA: This is an interview with Dr. Charles Cameron. The interview was held on May 22, by Devra Breslow, at Dr. Cameron's home in Miami Beach, Florida. Dr. Cameron is the former Medical and Scientific director of the American Cancer Society from the period approximately 1946 through 1956. He subsequently became the Dean and later President of the Hahnemann Medical College in Philadelphia and for the last two years has been the Vice President for Development of the Papanicolaou Institute in Miami, Florida.

DEVRA: I think we should start out by describing your involvement in cancer control efforts....

(Personal)

CAMERON: I think it probably began in an emotional contact when I was an intern at the Philadelphia General Hospital between 1935 and 1937. It was a rotating internship, and part of my service was to spend two months on so-called cancer wards. Here were 50 men and 50 women in a very pitiable condition and it sort of got to me. From that time, I was caught up with this notion of doing something in the cancer field.

I remember that there was a researcher occupying a laboratory financed by the DuPonts, just about two blocks from the hospital. He had been a gynecologist. His wife had died of cancer and he had the backing of the DuPonts and so they set him up in a laboratory so he could find the answer. A cancer cure had been announced in Kingston, Ontario, and he sent me up there to investigate it. I came back with a very bleak report.

I had the experience of being confronted with a stampede of cancer patients from all over the country who were up there to get this drug, which proved in the long run to be of no value. But I think these experiences conditioned me in some way which I guess only a psychiatrist can explain.

After my surgical residency in Philadelphia, I went to Memorial Hospital in New York City. In those days, Memorial was at 106th Street and Central Park West, although during my tenure there, they moved down to 68th Street and York Avenue. There I came under the influence of a number of people who were really leaders in the field. There were people like Frank Adair, Hayes Martin, Fred Stewart. Then, of course, my future was really cast because I stayed devoted to the cancer cause for a long time. Just after my residency at Memorial, in 1941, I went into the Navy. I was at various posts in the Pacific and on ships for about 40 months. Then I wound up back at the Naval Hospital in Jamaica, in Long Island. When the Memorial people found I was there, they had me transferred to the Brooklyn Naval Hospital where there was a coterie of Memorial Hospital people. Indeed, Brooklyn was one of the hospitals where patients with cancer, anywhere in the Navy, were referred along with Bethesda and San Diego. I was again with the Memorial group and we were treating lots and lots of cancer. After I left the Navy (June 1946) I fully expected to go into practice.

However, I was offered a position with the American Cancer Society, which had been reorganized the year before. It had been the American Society for the Control of Cancer. I took it with the idea that I would probably be traveling a good deal and would see a lot of the country, and would decide where to go to practice. I had about decided on St. Petersburg, Florida, because I looked up the statistics very carefully and I found that it had the highest cancer death rates of any place in the country, obviously due to the age of the population. During that year, in the Cancer Society, I did travel a great deal, but the fact is I got promoted to the position of Medical and Scientific Director. I was 38 at the time. It was a very challenging and flattering thing. So I stayed.

I stayed for 10 years and during that time we had a great deal of fun in filling the lines with program. The Cancer Society was raising money at a phenomenal rate. We were able to get things underway which had not even been thought of a year or two before: programs in professional education, employing a number of publications of different kinds and then we produced a variety of motion picture films. We did a series of color television programs which went to eight different cities. We also had an elaborate public education program. (I'll come back to the Pap Smear but it proved to be the thing that we focused down on as something that would save lives here and now, and that we really ought to popularize.) Then, of course, there was the service program where local units provided assistance to cancer patients. The crowning piece of the whole thing was the research program because the Cancer Society was able to support research to a very limited extent during Dr. Little's tenure, when it was the American Society for the Control of Cancer. But when the money began to come in at the rate of $4 million, $10 million, and then $20 million dollars a year, one quarter of which was going to support cancer research, it became necessary to construct systems for evaluating the requests for grants-in-aid which began to flow in.

At that point, the Committee on Growth was organized. It was a committee of independent scientists under the auspices of the National Research Council. It flourished under very distinguished chairmen and included C.P. Rhoads. I thought it did a magnificent job. I rather regreted when that Committee was in effect disbanded and the Society took on to itself the task of forming advisory committees and having them do the peer review of the requests. However, that has proven to be highly successful and it therefore must be good.

The Cancer Society grew by leaps and bounds. After 10 years, I thought that perhaps I had contributed as much to it as I could in the way of ideas. It would be more or less a custodial position from there on, I thought. Most of those programs are still under way.

I therefore accepted a position as dean of a medical school in Philadelphia from which I was graduated. I went there in 1956. The school was not in very good shape academically, financially, or physically. It occupied some terribly old buildings, some of them over 100 years old, and all of

them conversions from previous uses. So in the next 17 years, we rebuilt the school and raised $58 million dollars, put up a number of new buildings and got it in pretty good condition. Then came the time of my retirement. Meanwhile, of course, I continued as a member of the Philadelphia Division of the American Cancer Society and served as its president for a term. Other than being a vice-president of the Philadelphia Board of Health, I didn't have much to do with the public or social aspects of disease control and it was simply as another board member that I functioned.

I was President of the Commission on Cancer Control of the International Union Against Cancer from its inception, which must have been in 1953 or thereabouts, for a period of 10 years. Rod Heller took my place; that was an opportunity to extend the principles that we had pretty well established in the Cancer Society on a worldwide basis. Indeed, one of our achievements was to demonstrate and actually do this by visiting countries and showing them how our cancer control program worked and providing them at cost with much of the materials we had developed. I think the Cancer Society's effort extended very broadly during the period of the mid 50's.

After I became dean of the medical school, apart from my local service with the Philadelphia unit of the Cancer Society, I did a fair amount of speaking at the invitation of old friends whom I had made during the years I was Medical and Scientific Director. They would ask me back frequently to give talks, particularly to their annual meetings. These were Divisions for the most part. I still do some of that, and I enjoy it very much. That's about the extent of my present contact with the organization. I am an Honorary Life Member of the Philadelphia and Florida divisions. Once in a while I am called upon to give advice to a couple of journals when an article about cancer appears.

DEVRA: Does the Pap Institute, where you are now the Vice-President for Development, have any concentration on cancer research of any kind?

(Pap Institute mission)

CAMERON: Yes. Cancer research is its sole mission. Previously, the Institute was known as "The Cancer Institute of Miami."

Dr. Pap came here in 1961. He had the idea of building an institute of cytology which would embrace considerably more than the morphologic cytology that he built his reputation on. So his idea was to bring in men of different disciplines to work on the cell from different angles. Well, he was here only four months before he died. Most of the people he had brought here--technicians, medical doctors, and Ph.D. scientists--drifted away during the following year when nothing seemed to be going on. So the place stood as a shell really. One exception: Before Dr. Pap arrived there had been a cancer detection center operating there, a cytologic diagnostic laboratory with some superb technicians. They are still there and they examine 25,000 specimens a year. You see, in those days, cytology was not quite accepted generally. The Pap Institute was

one of the few places where doctors, particularly in Central and South America, could send specimens. They sent them by mail and that kind of material still comes in in large numbers. So the diagnostic laboratory has been there since before Dr. Pap arrived and it has continued.

Dr. Schultz came in 1968; he is a biochemist by training and career experience. So the Institute has tended to grow along very basic lines; for example, an important part of the program are electron nuclear magnetic studies of molecular structure of tissues. Then, a very interesting development has to do with the automation of the Pap test. This is not new anymore, but Dr. Leif's approach to it is unique, as he is really a biomedical engineer. It looks as though we may have something that is going to be exciting and will permit the screening of 20 times as many specimens as a technician can do in the space of an hour. This is now in its fourth model. I expect something is going to come out sooner or later, maybe within two years. The specimens have now extended beyond the vaginal secretions. The cytology laboratory always did exam specimens from other organs. But I remember Dr. Pap trying, during his later years at Cornell, to get material from the breast, because after all our results were not very good even with radical treatments. Now Dr. Leif has succeeded in developing a pump which will obtain cells from about 50 percent of most breasts. With just this information, it now becomes necessary to go to patients with known breast tumors and find out how accurate this system is going to be.

All we've been trying for at the moment is to get the material and then with the addition, of course, of mammography and xeroradiography, we have other means to make it appear as though breast cancer is going to go the way of cervical cancer. I'm looking into the crystal ball now, but this material from Milan, where the group treated breast cancer patients after surgery with the combination of three chemotherapeutic agents, looks as though we're going to really make a dent in breast cancer for the first time.

That's so much for the Pap Institute and its general character. It is a very basic institute, one of the few independent, freestanding laboratories devoted solely to basic studies in cancer research. It makes it a little hard to raise money because they don't treat patients, but nevertheless we are examining the cancer phenomenon at levels which are essential to the ultimate solution.

DEVRA: Was that original intent of the Pap Institute?

CAMERON: Not exactly. I would say that that was a more recent evolution of the direction. Dr. Pap--we must remember that he was trained as an M.D. with a Ph.D. from a German institute--was an endocrinologist essentially and he did some very basic work in endocrinology. To be sure, his later years at Cornell were concerned not with physiology, but with the identification of various cell types as they appeared in body secretions. So he was not the kind of renaissance man that was particularly broad in the field of cytology, although he is called the father of cytology. Cytology in a broader sense is a very encompassing term. He was concerned with structural cytology or anatomy. But he would have liked to have developed it branching out from structure into function so that perhaps in time,

the Institute would have turned out much as it has. Whereas when Dr. Schultz came in, he knew little about cytology, and he was trying to get at biochemical phenomena within the cell which seemed to be characteristic--differentiating between normal and cancer cells.

DEVRA: When you were the medical director at the American Cancer Society, what do you recall were the significant policy decisions and related significant events, debates, and defeats pertaining to cancer control.

(Significant Policy Decisions)

CAMERON: There was one which might be controverted by a number of people when you would talk to. Dr. _____, for example. There were two camps. They became apparent because the man who preceded me as medical and scientific director was a very scholarly fellow by the name of Ashley Oughterson. He was from Yale. He had grown up in the ivy halls and he was an academician with a capital A. The result was that he was slow in making a decision. The laymen who had been responsible for the reorganization of the American Cancer Society, like Elmer Bobst, Albert Lasker, and Jim Adams, were getting impatient and wanted to get the program moving. So they finally got impatient enough with dear Dr. Oughterson to send him off and they made me the acting medical and scientific director. We made lots of mistakes, but we did make things move. That was partly the result of the brashness of inexperienced youth, I guess. The point I'm leading up to is that was the beginning of a schism, with the medical and scientific group on one side and the laymen on the other side. And I must tell you this, I think that apart from some very outstanding people like Adair and the fellow whose father is one of the founders of the American Society for the Control of Cancer, a gynecologist....

DEVRA: Howard Taylor.

(Attitudes toward Physicians)

CAMERON: Yes, and people like that who really secretly, I think, sympathized with my position, the fact is that the rank and file of the medical and scientific group in the Cancer Society were more or less politically motivated. They had come up through the ranks. One of their leaders became president of the AMA. I thought that they really didn't know too much about cancer, and I also thought that they were very, very fearful of the strength of the Memorial Hospital group. Frank Adair preceded them, but they would never appoint anybody else from the Memorial group. Gradually that gave way and you got people of real stature like Sidney Farber and George Pack in the Society, but that was after my time.

In my 10 years there, I had to put up with what I thought was a rather reactionary medical and scientific group. They were not leading the way by any means, but the laymen picked it up and did. It was the laymen who conceived the research program and who really engineered its operation.

Here's an example. I had heard about cytology when I was at Memorial Hospital; that would have been before World War II. The monographs of Papanicolaou and Traut were published in 1943; they helped to make some converts. By the time I got into the Cancer Society in 1946, the pathologists by and large were still of the opinion that this was a "flash in the pan," and not to be taken anymore seriously than any aspirate of fluid. So I got talking to Dr. Pap about this.

I came to know him quite well. We would meet, as I said in the eulogy I wrote for the JAMA, at the Plaza Bar near East 68th at least twice a month, and we would have long discussions about cytology and eventually about his future. At any rate, as a result of my conversations with him and discussions with people whom I really regarded highly, like Howard Taylor, Joe Meigs, who was doing superb work with Ruth Graham in Boston, I got the idea that this was a great opportunity to save lives. So I pushed the Cancer Society into backing this full tilt. We put it in our publications for doctors and we made much of it for the laymen by means of films, pamphlets; I wrote some Public Affairs pamphlets about it. Well, the cumulative effect was that the opposition gave way faster than I think it otherwise would have. In 1948, I called the First National Conference on Cytology, at the Somerset Hotel in Boston. We invited 100 people and we paid their way. We divided them as nearly as we could between those who were for it and those who were skeptics or against it. It was a lively argument for the two days of the conference. I think that perhaps did something to persuade the profession that cervical cytology was here to stay.

DEVRA: How did the Toledo study get going? The 10-year study in Toledo using the Pap Smear?

(ACS Goals)

CAMERON: I don't remember too much about that. The one I know a little more about was the one in Memphis which was primarily sponsored by the NCI, the first effort to perform Pap tests on a large population. One decision made, really at the persistence of Mary Lasker--she said we're not spending enough and we're the ones to agitate for it--so she really steered the membership and the staff into organizing the citizens' testimony. We were trooped down to Washington to appear before Fogarty's and Lister Hill's committees every fall, at budget-making time. We would organize a testimony giving much of it ourselves but calling on people who were not related to the Cancer Society also. I remember Farber, before he was anything more than a member of the Massachusetts Division, coming down and being the most effective spokesman we ever had. I think the National Cancer Institute people felt very grateful to the Cancer Society for picking this up, exercising the effective leadership role which they did in increasing the federal appropriation, which did increase very remarkably. Beginning with $700,000 in 1938, the appropriation which established the National Cancer Institute, which was the first of the institutes, to last year in excess of $700 million. I think the Cancer Society had a lot to do with that. And then, of course, there was the behind-the-scenes work of Mary Lasker, which ought to be recognized.

Anyhow, the thing that was significant in this relationship between the Cancer Society and the National Cancer Institute was that we realized that we were dealing with something that was responsible for incredible loss of life, much of which was needless. If we had any feeling of competition, we'd better submerge them and work together in order to do this great noble thing. I really think there was a very high and healthy motivation on both sides. The other thing was that the personalities involved at that time, happened to just fit together like hand and glove. Rod Heller, I love. I see him occasionally. Then there was a wonderful guy named Ray Kaiser. Ray Kaiser was the director of the cancer control program at NIH during much of my time at ACS. Again, we were warm, personal friends. Everybody at NIH, I thought, was just great. There was Harold Stewart, who was the pathologist, and there were the people in the grants evaluating section. We just enjoyed each other's company and got along famously. We evolved programs together by just simply sitting around tables.

I remember how we got the first breast self-examination film started. We remembered a monograph that had been written by Fred Stewart which showed that when tumors of the breast were less than two centimeters in diameter, 70 percent of women would be alive at the end of 5 years after treatment. We reasoned not enough attention was being given to size. Let's go on the rule that the smaller the better. Small doesn't necessarily mean early, but we hoped that it does in most cases. So we got some more of the literature data together and thought we were on solid ground, and then got the real breast experts like Cushman Haagenson and Dr. Frank Adair to give us some good advice on examination of the breast. (I forgot to tell you that when I came out of the Navy and went with the Cancer Society in June 1946, I said I would do so only on the condition that they would give me enough time to practice at the Memorial Hospital. And they did. So I was a member of Dr. Adair's staff for 6 or 7 years out of my 10 with the Cancer Society.)

DEVRA: Did you actually have private patients?

(ACS-NCI Collaboration)

CAMERON: Yes: I had an office on 73rd Street and saw private patients, but the Cancer Society was growing so fast and the demands made on me were so great, that I thought it was unfair. I frequently would operate on a patient and then duck out of town, leaving the patient under another doctor's care. This wasn't good. So I gave up the Memorial thing, recognizing that the world is full of good surgeons but I seemed to have a little flair for administration, so I would stay with it. I never regretted that decision. The collaboration between the ACS and the NCI was firm and established early, and I think that a lot of it was due to our sense of mission.

DEVRA: Those are some of the promotive aspects. What were some of the deterrents or risks of this relationship between the two?

CAMERON: At first, I had some reluctance to see the Cancer Society eclipsed by the sheer volume of money which was going to come from the

NIH. I expressed this reservation to the Board. I said, "we're going to be skunked. People are going to say if we're giving all this money in taxes, why do we have to give it out of our philanthropy? Mrs. Lasker had no patience with that argument. She said, "there will never be enough," so we did go right down and did what we could in Washington. But I thought it was a hazard. Every once in a while, I do hear it pop up again. When you begin to talk about the National Cancer Act and what has happened to appropriations since 1970, you hear the question more frequently: "What is going to happen to the Cancer Society? Is it going to be left way behind?" Well, of course, the ready argument there is that the Cancer Society does some things better than public health agencies can ever do, such as educate the lay people. Secondly, the NCI is able to fund less 50 percent of its approved requests for grants-in-aid, so that there is still an enormous need for that 25 percent of $100 million which the Cancer Society is now putting up each year.

This is hard to express, but I think we were apprehensive about the NCI only at an early point in the evolution of the Cancer Society. We were not working closely very long with the staff in Washington until we came to have very considerable respect for them. We realized right off the bat that they were not the people who got into public health work because they weren't able to practice medicine, but they were really superior individuals. I don't think we ever quite succeeded in communicating this to the rank and file of our Board. I think the Board always regarded the people in the public health sector as "country cousins," and I don't know that this interfered with the growth of the program, but certainly at the staff level it did not exist.

DEVRA: I want to go back to that observation. Anybody looking at the roster of the membership of the board of the American Cancer Society, in the early days when it was another agency, would realize that it is a very elitist organization. Mainly east coast physicians of some distinction, certified, and operating some in private practice but not in universities. And the laymen, of course, were also what we might consider socially elite. I wondered whether this sort of social attractiveness of these people would strengthen or weaken the ability of the Cancer Society to attract into it ordinary middle-class and lower-class individuals, and certainly a broader perspective of the medical profession? (ACS cultural set)

CAMERON: Well, that's very astute. I tell you the truth, that aspect of it never occurred to me up to this moment. But I think that there is much in what you say, and if it existed, it was a holdover from the days when the Cancer Society was established as the American Society for the Control of Cancer in 1913, and you had the leaders in the organization who were really of the Mrs. Astor, Mrs. Cleveland, etc., elite. I don't think I could detect when I arrived on the scene that that was proving to be a deterrent to the popularization of the movement or its message. I know that there was a great deal of tender concern all the way for the masses.

But I tell you what was a difficult problem. When Dr. Little was the Director for the American Society for the Control of Cancer, he was also the Director of the Jackson Memorial Laboratory in Bar Harbor, Maine. He had achieved distinction as a geneticist, and his name was well respected in scientific circles. He got the idea that there would be formed the women's arm of the Cancer Society, which could be called the Women's Field Army. I was told that at one time they actually wore uniforms. The women had the time. They certainly had the inclination. So the Women's Field Army got underway. The Society was divided into seven geographic regions in those days. (That was abandoned for a while. Now I understand that they are back to it.) As a means of decentralizing, the seven regions were each headed by a regional commander. Within the regions were the divisions, roughly corresponding to states; there was the state commander and the local unit commander. Well, it formed a great crowd of women and I must say that I admired those women who were regional commanders, and I remember each of them very clearly.

DEVRA: Did they sit on the Board?

CAMERON: No, there was not one of them on the Board.

DEVRA: Why?

CAMERON: Well, you see what happened was that there was a rather violent . . . During the war years, the Board got more or less infiltrated by people who were determined that this organization was a "sleeping giant." I suppose it started out with one individual, like maybe Elmer Bobst, coming on the Board and saying this has great potential, let's get my friend Mr. So-and-so on, like Jim Adams. And from there it started. They got one of their friends after another on the Board. They were all people of tremendous gusto and enthusiasm, and style, and most of them had a good deal of influence. Like John Reed Kilpatrick, the great president of the Madison Square Garden, a tremendously powerful man, and "Wild Bill" Donovan and Emerson Foote, the whiz kid of advertising.

However, when these people came on they had contempt, no sympathy for the Womens' Field Army. They thought they were a lot of do-good amateurs. The regional commanders were making a salary (not much) and most of the State commanders were also. Now you must remember that when you're talking about tight division organization, as you have now in most of the divisions, nothing like that existed then. There were no professional fund raisers, professional writers or anything else. This was all labor of love. These women had a convention in Biloxi, Mississippi. I joined the Society in June, this convention was in October. It was a big thing. (We occupied all of a big hotel there.) But it was a disaster, really because many of the Board attended it and did not care for the Ladies' Garden Club style....

DEVRA: I think it was around 1946 or 1947.

CAMERON: It was in the fall of 1946. It just didn't leave a good taste. At any rate, the decision was made, high up, that the Womens' Field Army was kaput and would be eliminated as conveniently as possible. And it was.

As the Society began to raise more money, they began to employ more professional people at the state and regional level. The regions were disbanded so that the organization was left without one of its very strong organization components. As I said, very gradually, but as a result of a concerted plan, the Womens' Field Army was whittled down until it became a ghost. Now out of deference to some of the people who had worked most of their lives for it, like Mrs. Harold Milligan, was not all eliminated, and a few of the older field army commanders remained the few years left before retirement. They were kept on in one capacity or another, but their strength was gone.

(Pap Smear) (Mrs. Mosiman...)

DEVRA: Let's go back just a bit. You've talked about the tension between the medical/scientific group and the laymen over promotion of such a thing as the Pap Smear. I must say that. The other day when I read the 1957 annual report (which was the year after you left), there was one very significant paragraph. This was the declaration stating that this year was the year of uterine cancer.

David Wood was the president. It outlined all the things the ACS had done in the previous 15 years. It showed how the Society had dragged itself up in past years. Then now, finally, they were decided they were going to announce that the Pap smear is here to stay, but you know some people look upon this as being ludicrous. What was it really like dealing with people like Dave Wood in those days?

CAMERON: Well, of course, Dave was a powerhouse in the Society in those days. What he said was accepted pretty much as gospel. After all he was a pathologist and one of the very few we had on Board. We used to come away from meetings terribly depressed.

Here is an example of what the staff had to contend with: I remember sitting with the staff one weekend in the office of the Cancer Society, we were then on Beaver Street downtown. We had a map of the United States. We were going through the country by metropolitan population groupings, and we were sticking pins where we thought there were enough people to support a cancer center. (Detection Center)

DEVRA: A detection center?

CAMERON: No, this was a reproduction of the Memorial Cancer Center of trained specialists devoted to cancer. Well, when we unveiled this thing, we just about got "run out on the rail," because this was the rankest kind of socialism, if not worse. It got very short shrift. There was our beautiful map with pins in it--and it got nowhere.

DEVRA: The reaction was exclusively from the medical and scientific group?

CAMERON: Yes, that's the one we had to get approval from. I think that if I had been perhaps more courageous, I would have risked my neck and gone around them, but we didn't do that. We worked along and figured that if our ideas were good, eventually they'll recognize them. But they were against the idea of cancer detection centers. I must admit that cancer detection centers have not proven overwhelmingly successful, but neither has the old cliche about "every doctor's office is a cancer detection center."

DEVRA: Then it was a cliche?

CAMERON: It was. It didn't really do anything for anybody except to keep this spreading monster of the clinic in check. And that was what they were afraid of. They were afraid this whole movement would get into a big super-clinic business. That's precisely where we are today, with the development of comprehensive cancer centers.

DEVRA: Why do you think that we have that now?

CAMERON: Well, I don't think that we have it. I think they're still struggling. But I think the concept which was clearly outlined in things that I read back in 1970 and 1971. There would be aggregations of specialized facilities within availability of the majority of the population, so that even poor people who couldn't go the the established cancer centers would be able to go to a place where special oncology services were available. That's what I think it was. I think in Florida it is tending to move in that direction. For a long time, they risked going through exactly the same type of opposition, although this was recognized beforehand, and I think a lot of the ground was cut from under the opposition because of the very cautious way that Gordon Zubrod, for example, has enlisted participation of all these people. I think the idea is good because I've seen what has happened with the development of a small hospital up in Buffalo, New York, State-supported, of course. I think the notion of the cancer center is a valid one as I have always thought the notion of a cancer specialist is good. You can't be a cancer specialist in the sense that George Pack was, where he did everything. He treated all kinds of cancer except I guess brain tumors, and treated them both with radiation and surgery in the early days and that was Dr. Ewing's notion, too. I think it died early as both specialties became so enormously suspecialized.

DEVRA: But you had this conception of comprehensive cancer centers in 1948?

CAMERON: This would have been 1948.

DEVRA: And you presented this to your medical and scientific board, and what kind of reaction did you get? Invasion of privacy in the practice of medicine?

CAMERON: No, it wasn't quite that bad. Any philosophy was rooted in this concept: Who would you rather have your stomach removed by, a man who does 200 gastrectomies a year or a man who does 6, given that they are of equal competence to begin with? But they wouldn't buy that, they would not listen to the logic of it. Their argument was that there is no such thing as a cancer specialist. After all, how much more expert can you become doing 20 breast operations a week than if you do 2 a week. So that the surgery of cancer is the province of the surgeon, period. That was the logic of it. They were talking about surgery as a technique and not as a disease-oriented treatment discipline.

DEVRA: Let me go back just a little bit. This Board did not have general practitioners on it, but it must have had general surgeons.

CAMERON: General surgeons, yes, and gynecologists, and a radiologist or two.

DEVRA: And this is at a time when the tremendous proliferation into subspecialties of surgery had been really just beginning. Yesterday, Dr. Holleb gave some reference to that even to this day, the domination of the Board by surgeons still exists. They realize now that they don't even have any medical oncologists on the Board. I get the impression that through the years the Society had been medically dominated by surgeons, pathologists, radiation therapists and some diagnostic radiologists. Very few internists, which we now call medical oncologists, were into it. That to me was another aspect of the kind of exclusiveness. I probably shouldn't become too preoccupied with this, except that the more I get into it, the more people I meet including Mrs. Lasker, and the more I recognize that this is an extremely elitist organization. It's only been in the last dozen years or so that you find people who are not in what we call the upperclass society, even at the local levels. It obviously in many communities must be an extremely desirable organization with which to be affiliated; only a person of some esteem in the community gets himself really involved. Let me give you an example. In my home town, Manchester, New Hampshire, Stewart Keay, the treasurer of the local bank, was on the Cancer Society Board for years. Now why would a man like that, a banker, get himself all involved in this? He must have been on the Board for 15 or 20 years. There must be some local payoff, it occurs to me, for the physicians as well. I would assume that for the physicians, there is a considerable local payoff.

(Medical-Lay Tensions)

DEVRA: The other person whose name comes up is Norm Bailey. I don't recall any physicians from the local province. Can you think of any other real crises that occurred during your tenure? You mentioned the schism between the Board--the medical side and the lay side--which I gather took a number of forms. Dr. Adair referred the other day to an episode involving Eric Johnston. Do you remember that?

CAMERON: I don't remember that specifically. I remember Mr. Johnston's plight there, but I don't know the specifics of the problem.

DEVRA: It sounds to me again as if as though there is a break between the laymen and the medical and scientific people. Did you find this schism to be persistant throughout your tenure as the Director?

CAMERON: I felt it did. I think that there probably was a lessening of it toward the end. Well here's a little anecdote, that I really hate to tell you because I seem to be bearing down on Dr. _____, But Dr. Pap had about given up on cytology. He'd returned to some rather basic work with chromosomes, and Herbert Traut arrived on the scene at Cornell....

DEVRA: This was in the 30's?

(Pap & Traut story)

CAMERON: Yes, and he got interested in cytology, being a professor of gynecology, and so he began to consort with Pap. As a result of this collaboration, Traut's clinics began to send Pap a lot of new work. The results were fantastically accurate. Traut was much impressed and he climbed on the bandwagon. As a result the two published the monograph in 1943--a beautiful collection of colored plates. Traut moves on to become the professor of gynecology at the University of California. After I had been in the Society--in my second year--I designed a silver medal to be given by American Cancer Society to the person who had made an outstanding contribution to the solution or control of the cancer problem. The medal was sometimes given to a basic scientist and sometimes to a guy who was prominent in control. I sketched a little picture of St. George and the dragon on one side and some lettering on the other. I gave it to Tiffany's designers. They came up with this beautiful medal which the Society still gives today, I think. Now the medal the first few years went to, I have forgotten whom, but about the fourth or fifth year I said it was time that we gave the medal to Papanicolaou. Dave Wood said, "Well if you give it to Pap, you'll have to give it to Dr. Traut." I said, "Traut was a latecomer. I mean, it was Pap who devised the thing and his supremacy in this field just could not be questioned." Well, it was questioned. Pap just about blew his top when I told him he was going to share the award with Dr. Traut. I got him quieted down, and he appeared very gracious at the ceremony when it was given to the two of them.

DEVRA: I have asked several people this question and I'm interested in your perception, since you and Pap were obviously very good friends. He was born in Greece. Do you remember whether he spoke with an accent or not?

CAMERON: Oh, a very decided accent, and it was one reason why he was reluctant to appear in public. He was a shy man to begin with, but of all of the people who had something to say that I have known, he was the most reluctant dragon. He just would not accept invitations. He would get them from all over the world, of course, to come and receive honors and make presentations. However, he did come down to my school in Philadelphia and speak once. I must say that he spoke English, but he spoke with such an accent that it was difficult for people who were not a little bit familiar with him to follow him easily.

DEVRA: Do you think he was made self-conscious by his academic peers or by people of the American Cancer Society, the Board and other staff who obviously thought.....?

CAMERON: In a sense, yes. I must say that although he was a noble man in the strict sense of the word, in my book, he adopted a deferential attitude toward people who were. . .and again, I think this was part of the European academic tradition. For example, Joe Hinsey he disliked Hinsey because he thought Hinsey was the man at Cornell, once dean and then later chairman of the Joint Administrative Board, who was keeping him down. And Hinsey always said to me, "You know I never kept him down, I have pushed him every possible way I could, and tried on many occasions to keep him with us." But, Pap would have this sort of ingratiating kind of approach to people like Mr. Bobst and Mrs. Lasker, who were very much interested in seeing that he was secure in his retirement.

DEVRA: But he thought he was sort of subservient and so on?

CAMERON: Something like that.

DEVRA: Do you think this was something that he sort of acquired over the years especially since he came here as a European?

CAMERON: No, I think it was in the later experience, (non-acceptance of the smear) but you are probably right.

DEVRA: He published his first paper in 1928. Why didn't anybody pay any attention to it?

CAMERON: Now that surely was an example of the idea whose time hadn't come. But what I have often pondered is that when he came into this country, he had very little money. He and Mary lived in a miserable little town that I later lived in, called Rutherford, New Jersey. She did almost menial work in a department store-I heard he did too for a while until he landed a job--with an anatomy unit at Columbia University. From there, he went on to Cornell, where he stayed. But, he was carrying letters of introduction around to important people. Here's a poor man in a new world. He doesn't know how he's going to survive or where he's going to land. I think he developed an attitude which became part of his character.

DEVRA: Now it's interesting that some of the laymen on the Board obviously recognized that he had made an enormous contribution to cancer control, but how persuasive were they on the Board or even dealing with you, in pushing forward his accomplishments, his need for recognition, or the need for the application?

CAMERON: Well, you know you didn't need to be persuasive as far as I was concerned, because I had had this earlier contact with Papanicolaou. I saw, quite independently, its value. If I were to give my interpretation of their role I would say that they saw cytology come to be accepted, saw its results as demonstrated in Toledo and Memphis, and elsewhere, and then they later became more enthusiastic boosters of Papanicolaou than they'd ever been before. I think that was the proper sequence of it. I think everybody is trying to claim fame for having recognized this great achievement early.

DEVRA: The first paper was written in 1928. A man in Romania by the name of Babes also published a very similar interpretation. The monograph came out in 1943. The government, the medical profession, even the Society fooled around for a long time. Last year, I think the statistics were that 91 per cent of American women by 1970 were found to have had at least one Pap Smear. Now this is almost 50 years. You're a scientist and a physician. Do you think that this is a normal amount of time for a major scientific advance to have some impact?

CAMERON: No. I regret it extremely, because as you pointed out, I think lots of lives were lost needlessly because of the delay in its acceptance. Remember that the pathologists are the most reactionary of all the specialists, at least that's what I hear, and this required them to learn a new technique really, if they were going to be good at it. A lot of them picked it up by on-the-job training, but here they were, ingrained in tissue pathology and looking at the aggregates of cells -- the architecture of tissue. It's just not in the nature of the beast to say, "Well, something is going to be better," although the evidence, I think was clear. Now, Pap's little paper...(left out at beginning of side 2 of tape)

(Rod Heller)

DEVRA: I want to be as kind to Rod as I can be, but Rod was one of the last of the Mohicans. The public health service guy, who by virtue of longevity or administrative accumulated experience, achieved a position of this distinction. They were VD people if I remember correctly, but nevertheless, Heller did a super job with the organization and the resources which were available to him. And, of course as you know, he went over for a brief period as President at Memorial.

DEVRA: Did you know Jean Weddle in those days? She went with him probably about the time that you left.

CAMERON: No, I don't remember her.

DEVRA: Any other things about the form and substance of the NCI/ACS relationship--what were some of the plus factors?

CAMERON: We had the usual exchange of staff people. For example, Dr. Adair who was very active in the Cancer Society, and president for three years, was the Chairman of the Cancer Control Group Study Section in Washington. I became a member of that study section, and subsequently became a member of other sections usually having to do with the awards and fellowships. And then when the medical school cancer education program became popular, we made the awards for medical schools there. Then we (the Society) had people on our medical and scientific committees, such as Dr. Heller. I think Ray Kaiser was on one of them. There was that degree of administrative exchange.

DEVRA: It was a smaller cancer world in those days?

CAMERON: Oh yes, quite so. You see, the National Cancer Institute and the whole NIH at that time had nothing corresponding to a board of directors as it does now. It was hard for them to bring anybody but staff people on our Board, but we did. On the other hand, we had Board members whom we would like to see honored by heading various study sections. Some were.

DEVRA: Were there any deterrents or hazards in this relationship? If so, how did you overcome them?

CAMERON: You know, they say, we tend to forget pain and unpleasant things, but I cannot at this moment remember any serious difference of opinion which we had with them in the development of the program during the 10 years I was with the ACS. If they showed no interest in something, we might say, for example, that maybe it's time to have a conference on lung cancer. If they didn't agree to tackle such a subject, then we would say, "Okay, we'll have our own cancer conference," which we did.

DEVRA: But you invited them to participate.

CAMERON: Yes indeed. Some of them did attend as observers, but not as participants.

DEVRA: Were the tobacco industry people there?

CAMERON: We never knew. We didn't invite them; certainly we didn't invite them to the first three. I don't think that the tobacco people came.

DEVRA: What role did the ACS take with respect to the cigarette and lung cancer issue?

(Cigarette Smoking Issue)

CAMERON: There had been sporadic reports. However, the reports were pretty well clinically oriented and they were almost always retrospective things. The next thing was that we got a request for grant from Ernst Wynder. He was then if I remember rightly a senior at Washington University

School of Medicine, and he was going to work this out with Evarts Graham. I might say that we were always trying to think up cute things to do. For example, we decided to give Evarts Graham a silver medal for his great achievement in doing the first pneumonectomy and found out early in the game that the guy he did it on was still living. So we invited them both. It was a spectacular performance. This was 18 years after the operation. But Wynder had a little project going on, and I think that they were actually going to try to study carcinogens in smoke. However, whether, it was statistical or if it got down to this chemical thing I don't recall, but we gave the grant to Wynder. Just about that time was the beginning, I don't think that award particularly stimulated it though, because obviously this had been developed in Cuyler Hammond's for some time.

DEVRA: And he was already with the Cancer Society?

CAMERON: Yes, he was. He had come a few weeks before I came in 1946. He had been brought in by Dr. Oughterson, who had admired his work with the Atomic Bomb Casualty Studies in Japan. Well, he and Dan Horn, who was another nice guy with a lot of imagination, had cooked up the idea that what we needed was a prospective study. They designed this thing and I gave them every support I could. It had no problems, we rode it right through, and it was done with no consultation with the NIH people. Again, we may have been sensitive to their relationship to the government, and therefore the government's concern about the effect on the tobacco industry. So, we thought that we, as an independent organization, were best suited to do this.

DEVRA: That's an interesting concept. You figured that the government's hands were tied and therefore they shouldn't get into this business of studying something that might be killing people?

CAMERON: Well, let me say I think there may have been enough people in the Congress who made themselves heard. That had an effect. For example, I can remember testifying before a Select Committee chaired by Congressman Delaney. He was from Massachusetts and he was studying additives in food, preservatives, sweeteners, colors, and so on. We had something we thought was perhaps suspicious enough to be banned. I was called to offer the testimony. I don't think I had any direct knowledge of the data, but it was an accumulation of what I had read. I presented it. Now here was something that was in the nature of a pesticide, which was getting into a dairy product. The Congressman may have been an unusually ignorant man, but he was from a southern state and he jumped on me with both feet. Now this was uncalled-for, because when you are there giving testimony, you don't have to get personal, but he was violent and he called me some mean things.

DEVRA: Was this Mr. Fogarty?

CAMERON: No, this was some unknown, but the point was that he was reflecting the farmers' interest in this thing.

What I have cited this incident for is to illustrate what I think may have happened in this whole tobacco thing. Government really didn't want to know enough about it so that it would have its conscience burdened by the knowledge that it was dragging its feet. They were fearful that the evidence would be overwhelming and the government would have to take steps which were going to hamper industry, take people out of work, deny the government revenue, and it was just too awful to contemplate. But here was a situation where I think again (and the Pap Smear was another one,) the evidence had to build up to a certain critical weight in this pan in order to tip it. I don't know how you would determine what that weight is, but evidently it is much greater than what we would like to see. There was this unquestionable economic situation so far as the government's posture in the whole situation.

So we went ahead with this and got volunteers to do the study, and, of course, that was one of the things we were criticized for by the tobacco interests, maybe not entirely by just tobacco interests. There was one statistician at Mayos who was very critical of the message, and I think there was another at the University of Cincinnati. However, the volunteers had a very circumscribed and simple task. The information they had compiled was then correlated with certificates of deaths occurring among the men in the study. Within 15 months, I think, the results began to show convincingly. Well here was, so far as I know, the first very large-scale prospective study. Since then, others have been done which seem to support it.

From the point of view of maybe not being entirely original but sort of clinching the thing from the biometric approach, the Cancer Society ought to have all the credit really. The Society undertook to do this thing on its own. Subsequently, of course, there were people who came in and served in some unofficial way as advisors, people like dear Dr. Harold Dorn, who later did his own study under government auspices.

DEVRA: Well then you established a committee. Howard Taylor told me that he was chairman even though he was a gynecologist. You established a Committee on Tobacco and Cancer. Again, while the government is doing relatively little about this, not issuing statements and not doing much research. How effective was this Tobacco and Cancer Committee in actually translating these observations into some kind of program?

CAMERON: I would say that it was not terribly effective from the point of view of originating ideas itself. It wasn't considered an earth-shaking important program. What it did was to act as a sounding board and give us the incentive that we needed in order to go ahead. And what did go ahead mean? Well, it meant that we would produce printed material for laymen on one hand and professionals on the other. We did produce a good deal of material. Some of it was shown by some rather courageous television stations. Other things included statistical material for use by speaker's bureaus. We did work with the American College of Chest Physicians in trying to compile professionally correct publications for physicians, and we did a great deal in the motion picture line for lay audiences, in trying to persuade them that we had a real health hazard.

DEVRA: You went through the educational group, but what about any policy statements or resolutions?

CAMERON: I suppose they would be certainly in minutes or archives of the Cancer Society, but I do remember that we had one which we feared was going to split our membership on the board but it did not. We found a remarkably uniform agreement on the matter of the hazard of cigarette smoking. Even people would sit there and smoke while voting. We encountered no problem as far as intra-board controversy was concerned on this issue.

DEVRA: And they felt you had to press on?

CAMERON: Yes.

DEVRA: Did they recognize the government's resistance or was this spelled out to them?

CAMERON: Not very clearly. We were sensitive to the point where we didn't choose to make much of it because we were "chicken."

DEVRA: How did Cuyler feel about all of this?

CAMERON: I think that he and all of us were tremendously proud that this announcement at the AMA convention in San Francisco, somewhere around 1954, attracted as much attention as it did. We were sort of lionized in press conferences all over the place. Then Cuyler became infused with an almost missionary passion which I did not share. I looked at the thing as sort of an intellectual challenge. I thought, "How can we persuade people that this is bad or how could we maybe get the manufacturers to make safe cigarettes, or do something?" But Cuyler became impassioned. For example, I can remember his standing up at a meeting and saying," these lives are on my conscience." I just couldn't feel that involved in it, although I certainly felt involved enough in the cervical cancer issue, when we were working with the Pap Smear.

DEVRA: Did you smoke cigarettes?

CAMERON: Yes.

DEVRA: When did you stop smoking?

CAMERON: I didn't stop.

DEVRA: You still smoke?

CAMERON: Yes, I smoke maybe one with my first cocktail and then maybe three or four through the end of the evening, and that's about the extent of it. Then when I was at Hahnemann, we went through all kinds of exercises like smoking clubs held at the YMCA, hypnosis, etc. I really don't know what is good, but I guess different things are best for different kinds of personalities. However, I don't get the impression that we're making very much headway. My hospital was across the street, in Philadelphia, from a Roman Catholic high school and those kids (mostly under 15) were not on the bottom step when they came out of school before they lit a cigarette.

I was already reaching for some kind of program where we would pitch it effectively. How do you do it effectively is the question. How do you reach kids who are still perhaps in grammar school?

DEVRA: How about industrial carcinogens? What position did the ACS take with respect to this?

(Industrial Carcinogens)

CAMERON: That brings us back to the National Cancer Institute. Hueper was holding forth at the NCI and we respected him greatly. There wasn't much teeth in the government's program to be sure, but on the other hand here again was a situation where evidence had to accumulate. I think we felt that the NCI had preeminence in this field, particularly with its intrmural laboratories back up. Therefore we did very little in this field. I can't think of any one thing where we...

DEVRA: The Society had gone into industrial education. Was that during your period?

CAMERON: Probably not. No, the only thing I can remember in that respect was that we would go in and encourage our people, as part of the total program, to go into the Bell Telephone Company and talk to their employees about the importance of breast self-examination, etc. This was health education in a very broad sense, but I don't remember any industrial education program.

DEVRA: Did you fund research into industrial carcinogenesis? I could look that up in the record, but I wondered how high of a priority it had or how much scientific interest there was.

CAMERON: I would say that we probably began to but I don't think that it was an important part or excited anybody. I don't think we realized then that the problem was as widespread as it is.

DEVRA: Did the ACS get into the food additive issue during your tenure?

(Food Additives)

CAMERON: No. Something I had written attracted a member of the staff of Delaney, and he said that I might be a good one to testify, so I did. But as I said it was only on the basis of what reading I had done on the subject of food additives and it was a rather general testimony. It did, however, offend a representative of the food lobby. He sort of put me down. But I do not recall any other role which the Cancer Society played.

As I said, the research program was extremely broad, and both in the Committee on Growth and in its successor within the ACS organization, there was a committee or section on carcinogenesis, so I just assume that they have supported some work in industrial carcinogenesis.

DEVRA: We've pretty much covered the Pap Smear story, although if you have some more things that you would like to. . .

CAMERON: There might be something that would be interesting for you to put in. When the ACS met in New Orleans, about four years ago, for the International Conference on Uterine Cancer, it brought together a lot of people from all over. I was to give a keynote address. I said to Art Holleb, "Can't you find that original program of that 1948 meeting in Boston?" Well, by golly he did; it was the only one we had, so I returned that to him. Now that might be an interesting exhibit along with Pap's original paper which appeared in Nature. Somebody gave Mrs. Pap a picture of the Battle Creek Sanitarium, which was where that paper was presented.

(Single Diagnostic Test)

DEVRA: Do you recall the ACS taking any position on identifying or promoting a single cancer diagnostic test? Do you remember the Huggins brouhaha, and the Penn test? There was a big movement in the late 40's to find a single test to diagnose cancer. Ray Kaiser diverted a great deal of his resources of his program into research for this. Do you remember anything about that?

CAMERON: Well, I think I'm on pretty thin ice here now. My memory is not so keen on this subject, but the thing I remember most of all is that I was personally persuaded that we did not know enough to approach the matter of a test. Also, that it was foolhardy to waste time trying to. I was strongly prejudiced against proponents of tests, because they seemed to fall into a pattern: technician-level people or doctors who had a rather simplistic view of what was needed. This aided my very biased, and probably a correctly biased opinion about this approach. The Cancer Society then never espoused any tests and showed very little enthusiasm in backing any organized structured studies in this direction.

DEVRA: In general, would the medical and scientific Board respond to the kind of leadership you were provided--you, the staff, rather than they initiating ideas and coming to you for implementation?

(Staff Initiative)

CAMERON: I think it would be quite fair to say that the program which evolved is a result of their response to suggestions which came from the staff. As you say, they were practicing surgeons or physicians and they were terribly busy. Most of them had less knowledge of clinical cancer than I did, so I think they fell early into a pattern of compliance with our staff-initiative. Now when it touched their nerves, such as the issue of cancer clinics or centers--and they were opposed to the idea of cancer specialization and opposed to the big centers which threatened the private practice--then they would rise up and react to put me down. I must say this, however, I think that there were some people who thought that I was safe and wasn't going off half cocked, but I think there were other people who were always looking at me afraid that I would do something that would embarrass the Cancer Society.

DEVRA: Now these are mainly medical people that we're talking about?

CAMERON: Yes.

DEVRA: What about the laymen?

CAMERON: The laymen seemed to think that I was doing a good job. I think it was laymen that put vitality into the Society, in the first place. They supported me right down the line. When Oughterson left, they were faced with a replacement for him. It was, of course, logical that I would try to fill in. I think I was the only M.D. there. Cuyler was there and a guy who went up to be the director of public health education in New York State, but otherwise I was that for about four months. Then at a meeting the Board, they sent me word that they had elected me permanently as Medical and Scientific Director. I know that it was the laymen who were the persuasive ones. I don't think there was any strenuous opposition to me, but I think that the old medical people felt that they probably could get someone who was more mature, with more academic background, and less inclined to be so enthusiastic.

(Quackery)

DEVRA: Do you remember how the ACS got into the quackery business, whether you initiated the concern or whether this was pressure from the government or other sources?

CAMERON: My impression is that the government took its cue from the ACS. The staff development at the NCI was a little bit behind the ACS, in that it was after 1945-6 when they were building up their staff up on a more elaborate departmental structure. We got into the cancer quackery business on a rather emotional basis. We had a very pathetic call from a man in Long Island whose wife had cancer of the breast. She had gone to Maine to get Wilhelm Reich's therapy. Then he told me about this thing, "this box," so I got curious and read what I could about it, and read some of Reich's writings. I was just amazed. Then we heard of another case of a woman whose brother was a state legistator. She had had cancer of the cervix and had gone down to Texas to have the Hoxsey, so then we got interested in Hoxsey, and found out how horrendous that treatment was. The next thing was that Hoxsey was being tried. We were called down to be witnesses as to how worthless it was. That started a whole train of things. We began to collect then any kind of data on new and unproved methods of treatment. Then a committee of the Board was formed which acted as sort of a liaison between the staff activity and Board policy on this matter. I think the California Cancer Commission was extremely active in this field very early and did a great deal to stimulate our own interest in that whole area. They had a special problem in Southern California and they responded well to it, and so we adopted much of their philosophy, thanks to David Wood.

When something got popular on the horizon, people would write the Cancer Society and ask us, "What do you know about this treatment." We would send out individual letters, until it got so voluminous that we would have statements on the Society's position mimeographed and mailed out.

Then the idea evolved that the Cancer Society would from time-to-time issue position statements on the more popular agents that were hot at the moment. We just had a new agent crop up here in Miami, where we were asked to express our opinion.

DEVRA: Do you remember somebody named Irene Bartlett? Did she come from the National Cancer Institute?

CAMERON: The name is familiar.

DEVRA: I was told by Lois, of the ACS now, that she was preceded by a woman named Irene Bartlett who came from some federal agency. Was the feeling that there was a vacuum in governmental responsibility for quackery?

CAMERON: Precisely, yes.

DEVRA: But you had to kill it?

CAMERON: Right and we barged in. I tell you who was very helpful at the time. There was an attorney who was employed by the AMA who had the AMA files together. They had very complete information, even more complete than we had, and maybe even more complete than we ever did have. We relied heavily on him and their information sources for information we needed quickly in order to arrive at a position.

DEVRA: I'm intrigued when I was looking through The Truth About Cancer, which you wrote, and am interested in a number of things. One is a very substantial chapter on quackery. What was the stimulation for you to write this book?

(Books re: Cancer Control)

CAMERON: There have been some good books written about cancer for the laymen, but when I would pass one around to the members of my family I would find that they couldn't get through it. So we got the idea that the Society should write a book that would be really pitched to someone at a 15-year old level. It was the idea to get the message not to the person who had graduated from college and who might be expected to follow some of the things, but down to the little housewife.

DEVRA: Did you write it?

CAMERON: Yes, every word.

I've started another one called What You Always Wanted to Know About Cancer, but I haven't been able to work very hard on it. I may really get into a retirement situation soon and maybe I can then. There are some splendid books however, such as Pat McGrady's book, The Savage Cell, and then there is one that I have read recently called The Seige of Cancer, and The Patchwork Mouse. I think The Patchwork Mouse is interesting for another reason. It's based on a deception involving a

distinguished institution, but the other books suffer from this fact. Authors get the idea that people understand what a cell is. I have suffered over chapter one of my book trying to make the definition of a cell clear as a concept, with differentiation flowing out of it. Then you really can't avoid discussing DNA, which is the ultimate replicating unit. That's where we begin. I thought to write that book to give two ideas: One the idea of the complexity of the problem, and two, some practical information about cancer's natural history.

DEVRA: What sort of impact did the book have?

CAMERON: I'm told it sold 70,000 copies, which Prentice Hall considered good for a book of that kind. I do have copies in my library (all packed up to move) in seven languages.

DEVRA: Do the royalties go to the Cancer Society?

CAMERON: The royalties do go to the Cancer Society. A paperback came out in about 1965, and I don't know how well that sold. I think by that time, the bloom was off the rose. There were other books coming out, so it probably didn't do that well. MacMillan brought that one out, but I haven't really been corresponding with them. I wrote to Prentice Hall about my new idea and got practically a form letter rejecting it, saying that their interests have turned in other directions now.

DEVRA: What role did the ACS play in stimulating the interest in research about chemotherapy?

(Chemotherapy)

CAMERON: Well, you know, I think that the answer is not going to be very satisfying to you.

The interest in chemotherapy was stimulated in me personally in the last days of my Naval service. We heard of nitrogen mustard and how it had been discovered accidentally, and Dr. Rhoads was very active in the Army Chemical Warfare Division at the moment. It was early introduced to Memorial, and since we were a Memorial group at the Brooklyn Nurse Hospital, I got some of it and used it on a kid with a testicular tumor which had metastasized to his lung. He had been sent to Brooklyn as a terminal situation. Later his tumor disappeared and he was going to New York on his liberty days. Now, this was a miracle which I had never seen before.

When I came into the Cancer Society, not long after that experience, chemotherapy was being very avidly persued by Rhoads. Now I think the man's name was Gilman, who came up with the nitrogen mustard therapy, but I don't think he was a prominent clinician; I think he might have been in Baltimore. But at any rate, he didn't carry it with gusto like Rhoades and Farber did. So Rhoads sold the whole concept of chemotherapy to Mr. Sloan and Mr. Kettering. He convinced them that Mr. Kettering's notion was a right one--that if you applied enough engineering know-how

and put enough money in--you push the answer out as you would in an industrial problem. I can remember a film which they made, which is a most effective film, called "The Weeds and the Grass." The weeds were the cancer and the cancer was invading in the grass all the time. You put this proper chemical on the broad leaves of the crab grass which soaks it up and is preferentially killed. It's a beautiful analogy.

DEVRA: Sloan made this film?

CAMERON: Sloan-Kettering paid for it, I believe. It's still a beautiful film.

I became very enthusiastic about chemotherapy. I made no bones about my enthusiasm. I went around the country with two hamsters Dr. Farber had given me. They both had experimental cancer planted in the cheek pouch as had their litter-mates. Then they were given one of the early chemotherapeutic agents. In these two hamsters, the tumors disappeared, and their litter-mates went on to die of their cancers. I held them up everywhere I went as an example of the cure of the cancer "patient." I was absolutely convinced that if an experimental drug would cure an experimental tumor in an experimental animal, then the day would come when pills were going to be used to cure cancer in humans.

Mr. Sloan asked me, around that time, whether I thought this was a solvable problem. He was getting pretty deep into it. I said, "yes, it's solvable, but not in our time." I should have stopped at the answer "yes"--but I went on. Now I have to eat some of that, because we now have 10 kinds of cancer which were wholly incurable when I was a resident, but which are now being cured although not consistently. The thing with the Kennedy boy to me is the most exciting of all.

DEVRA: Well, you had a personal interest. How did you translate this interest to policy and programs in the American Cancer Society?

CAMERON: I wasn't in a position to do it because my title was Medical and Scientific Director. I was an arm's length removed from the scientific aspect of it. Remember I told you that I opposed the dissolution of the Committee on Growth. Well, that didn't carry any weight. The ACS went ahead and they formed their own research division within the Society's staff structure.

There were kinds of things that I was able to do, though. For example, there were certain types of radiobiologic research fellows which I thought we needed in this country, particularly as therapeutic radiology was beginning to separate from diagnostic radiology, movement which we tried to encourage in every way we could. But we were not going to be the determining factor, because we were not considered a professional organization. That always rose up. It was part of the "unrapport" which developed between me and the Medical and Scientific Committee, because they felt, in the preparation of our materials, even though it was done with expert guidance, we were intruding in an area which was properly that of strictly professional organizations like the county medical organization or the national AMA.

DEVRA: I think they're improving, because next week in San Francisco is the first American Cancer Society National Conference on Radiation Oncology. It has taken all this time.

CAMERON: Well, there was very little I could do personally. The Cancer Society, during the days of the Committee on Growth, showed a very healthy interest in chemotherapy research, new drug development and testing. The National Cancer Institute already had taken a lead in this because of their larger resources.

DEVRA: Did you encourage that lead? Send people down to testify and other mechanisms?

CAMERON: I definitely encouraged it. Then there were other developments such as Dr. Farber's growing interest in childhood cancers. This led to the establishment of the Childrens' Cancer Research Foundation in Boston, the Jimmy Fund.

Here is another interesting development: the Cancer Society had what I considered almost a paranoia about the growth of an organization which could conceivably be competing. Farber did this so adroitly that it was never a matter of concern to the cancer society, but it did encourage solicitations directly to Dr. Farber's operation, which is as it should be.

DEVRA: Has that offended some people at the ACS?

CAMERON: I don't think it did, because his stature was such that they could not really find any grounds to be offended. They knew that he was a highly ethical guy and that he wasn't going to kick the traces like Andrew Ivy and do something bad.

But they were always a little bit defensive when it came to Rhoads because Rhoads was much more of a real hard-sell guy. They were afraid Memorial might take some of the steam away from the ACS. I think that was the reason they were reluctant to having Rhoads or any of his ilk at Memorial closely involved with the Board. Dr. Adair was the only one.

DEVRA: That's fascinating. If they felt strong enough, they would have surrounded him. Adair was the only one from Memorial who was on the Board?

CAMERON: That's right. Howard Taylor had been on the staff at Memorial, but that was before he had become professor, head and chairman at Columbia.

DEVRA: While we're on this business of competing organizations, can you tell me a little about three. You've mentioned the Jimmy Fund--that had credibility, respectability because Sidney Farber was a respected scientist. What about the emergence of the Leukemia Society? How was that viewed?

(Competing Organization)

CAMERON: There was an anecdote. I once knew the name of these people who founded it. It was a French name. (d'Villiers?) They had a son

who died, and they thought that there wasn't enough being done. The Cancer Society wasn't especially interested in leukemia; it was considered just one of the cancerous diseases. That was the refuge the Society had when the time came to try to either put this organization down or contain it. They said leukemia is another malignant disease, we're supporting research in malignant diseases, and we don't label any aspect of our program on leukemia research. Now that was about all that they did. There was nothing covert in the way of an effort to "get" this organization. When the Eleanor Roosevelt Foundation came into being, then they saw a real threat, and ACS did make a concerted and effective effort to absorb it, I understand.

DEVRA: I don't even remember that one.

CAMERON: Well, there are fellowships that bear her name.

DEVRA: How did the board review the Damon Runyan Memorial Fund?

CAMERON: I think, they first thought it was a flash in the pan and that it would die an early death. The second thing was that they thought the auspices were so shallow that it would never raise any amount of money; and thirdly, that if it did, it would not be from sources which we would normally attract, such as race track money.

DEVRA: Dirty money?

CAMERON: Dirty money, exactly. "Look down the nose," at this thing. Then, there was this peculiar character named Jack Teeter; Jack was sort of the secretary of the research program. He had a great deal of energy.

DEVRA: Was he a physician?

CAMERON: No, and far from it. He was a foxy type, a "cracker" from somewhere, but a lot of good judgment. Anyhow, this Jack Teeter was friendly not because of the underworld, but unscientific connections with Walter Winchell and Mr. Leonard Lyons who was Walter Winchell's very close friend. So it was natural that the Cancer Society would use him as the liaison go-between, and indeed that's the way it worked. I think Winchell was quite grateful to the Cancer Society for the judicious way in which it handled the Damon Runyon funds. Winchell was satisfied that his special obligations were respected.

DEVRA: In the beginning, as an independent organization, did they come to you or did you go to them and say we think we can help you?

CAMERON: If you had to pin it down, we went to them. Teeter was on the job for the ACS, and we wanted to have the Runyon Fund under control. At least we wanted to be sure that it was being spent properly and so it was Teeter who really was put up as the go-between.

DEVRA: Were there any other such organizations that sprung up which might have been at all threatening?

CAMERON: Offhand, I don't really recall any.

DEVRA: Now let's go back to talking a little bit more about chemotherapy. You got into the business of supporting the federal initiative and responsibility. You were supporting some definitive research yourselves even before that.

(Chemotherapy)

CAMERON: On the whole, in the activities of the Cancer Society, during the Committee on Growth days and subsequently, I think the general feeling was that the magnitude of drug testing and drug synthesis and evaluation on a variety of systems--beginning with rather simple ones--was so great that we would leave this as a responsibility of the Cancer Institute primarily. In some ways, I would say that the American Cancer Society's role in the whole chemotherapy development is not one of its singular accomplishments. I think that with cancer chemotherapy it was getting down to the scientific level where the Society felt that it was improper to exercise judgments as to where the emphasis should be, that there was always some reluctance of the Society to influence or try to influence the direction of the research. As a matter of fact, that was a more or less policy of the National Cancer Institute also until recently. Now you have developments in both organizations. I would say more recently, there has been an interest in developing a field such as immunology, and you would have almost a preference for programs that were in this field.

DEVRA: Targetted research?

CAMERON: Yes, targetted research, exactly.

DEVRA: The ACS felt that they shouldn't get into the business of specifying even a proportion of their research money for. . .

(ACS Policy)

CAMERON: That's right. I don't think they ever went that far. Now they may have after I left, but I'm sure there were many people who felt that somebody ought to sit there and try to accomplish what "boss" Kettering wanted. The big blackboard it's called. To put down everything that's going on; then it will begin to form a pattern after awhile, and you'll know the directions that are appearing promising.

Maybe at a point that would have been a good thing to do, but I don't think we're there yet. Maybe we are at the threshold; with the developments in immunology and in viruses, maybe we're reaching that point. I happen to think now that there is an awful lot of fundamental work that has to be done before we're in a position to ask the questions that need to be answered before we are going to be able to put things together. I would say the Cancer Society stopped short of pointing in directions.

(Chemotherapy)

DEVRA: I'm curious whether there was any concern on the part of some people, staff or Board, that chemotherapy was awfully close to quackery and that that was another reason to stay out of it.

CAMERON: I think not. The thing that I think supported it was the fact that you had some very strong people who advocated chemotherapy early. I think Farber had more confidence with the people than Rhoads had. Rhoads was sort of tainted with salesmanship, but Farber had a very solid background at Harvard behind him. Then there was another guy who was well respected because of his earlier work in the field of nutrition. His name was Tom Spies. He got in on it particularly from the point of view of drug testing. But the overwhelming thing that sold it was the involvement of the National Cancer Institute and advent of the likes of Zubrod, the systematization of the whole testing program, then the whole thing was really rolling.

I think that, because of its nature, there may have been some reservation at the very outset, but the results that were published early were so convincing that you could not deny the disappearance of pulmonary metastases. Then, of course, we had had the experience of hormones, which I think dated from the late 30's with Huggins' hormone manipulations in prostatic cancer and the application of similar principles to breast cancer control. This went through a period of evaluation and settling down but the auspices were favorable and the results were unmistakable. The effectiveness of the hormone manipulation was run out rather quickly as a therapeutic development.

(Andrew Ivy)

DEVRA: Was Dr. Andrew Ivy every active in the American Cancer Society?

CAMERON: Yes, I can remember Ivy coming to Board meetings. I would say it was a rather brief period. He came and was elected as a delegate-at-large. He was just over his peak as a physiology investigator. I think that people were beginning to have some reservation about the reliability of some of his discoveries. I can remember that he described a machine he had devised for measuring early hypertension before it could be determined by blood pressure measurement.

(C.C. Little)

We have a scientist at our laboratory named Wilhelmina Dunning. Dunning has done a lot of work in developing tumor-bearing strains of rats, including one famous prostate tumor, which I think bears her name. But she worked with Dr. Little earlier in her career, and she told me some things about him which makes me think that he's never quite been given his due. And The American Society for the Control of Cancer never raised more, I heard, than $800,000 in any year before 1944, which was the year the reorganization began. And so his resources to work with

were very, very limited. When I came into the Cancer Program, the offices were considered to be temporary. They were in the Empire State Building, and we had very little staff. I used to open the mail, for example. I'd get there first, determined to get ahead, but in that first year after we had taken in that year $3,000,000 in campaign proceeds, we were able to hire staff.

I think the number one accomplishment was that I participated to an important degree in the development of the base of an organization which could subsequently grow into the management of a program of many millions of dollars. The elements of the program were identified also. I would consider these as major achievements, because, as I hinted, perhaps a little egocentrically a while ago, the major components of the program were devised in those early days.

DEVRA: And they have remained unchanged?

(Program Directions)

CAMERON: Basically, yes. We considered the primal function of the Society to be public education. It had done that as best it could over the years in the development of the seven warning signals. I must say parenthetically that I became disenchanted with the seven danger signals, because they are not early signs of cancer, they're too often late. And that's why I think they've never made much of an impression on the death rate. By the time a lump is felt by the woman herself accidentally, it is metastasized in over 60% of the cases. It's so with uterine bleeding. This is an ulcerated tumor and this is not biologically early.

But that was the idea that set us off on the detection notion. I'm not happy with the way the detection program turned out, not today. Because most doctors don't look for signs enthusiastically. This lack of enthusiasm communicates itself to the patient. Therefore, there is no great thrust in this direction. Once in awhile, as at the Strang Clinic (the Cancer Prevention and Detection Center of New York), you'll find a sustained enthusiasm over a period of years. When you can get statistics as Emerson Day got which show the improved results of deliberately looking for tumors, why, then, I think the principle becomes validated. Economically, it breaks down because of the cost--the cost of finding a cancer--but now that we have paramedics maybe the thing can be reexamined.

That's part of the future, isn't it?

The professional education program was a major achievement because up to that time there had been pretty general acceptance that this was a layman's organization, and they had no business dabbling in professional areas, particularly trying to educate doctors as to the importance of cancer, what it looked like, etc.

(Organized Medicine, Cancer Clinics Opposition)

DEVRA: Did you get any backlash from the AMA or other societies...?

CAMERON: No. Not from organized groups, no. What we did would be to have messages sent to us maybe from the AMA, maybe from the American College of Surgeons. I think quite definitely from the College of Surgeons saying, "Don't let the ACS get too far into the business of stimulating the development of cancer programs in hospitals" including what they called the cancer clinics. The American College of Surgeons devised guidelines for the establishment of cancer programs and shut us out quite effectively. I think, in one sense, we were told to go easy in this field; on the other side, we did as well as we could.

We persisted in the production of material--motion picture films for doctors to be shown at staff meetings, medical societies, and so on. Second: Publications, monographs coming out at the rate of two a year on a particular subject such as thyroid or bone or urology. Third: The production of the magazine for general practitioners called CA (it was probably misnamed a Bulletin of Cancer Progress). As Dr. Lehman from the University of Virginia used to say, "We're not promoting cancer progress, we're trying to stop cancer progress." But that little publication has continued. I think it is a rather spritely thing and was very well designed. The other end we noted is only the Journal of Cancer Research. This is sort of a heavy tune. We ought to have a journal that is clinically oriented to research. And that's why we got Cancer started. Fourth, of course, we promoted the National Cancer Conferences.

DEVRA: Your own scientific meetings?

(Achievements)

CAMERON: Our own scientific meetings, yes, and the specific programs we've touched on, those devoted particularly toward the Pap smear or toward the smoking.

There was public education which continued and was augmented considerably. There was professional education, which was a new program. We never got very much into rehabilitation because we couldn't see our role in it. We encouraged it and said, "like motherhood it's great," but we didn't see what we could do. And let me say that education, research, service were always promoted as the basic program. The service idea faltered, though. I was constantly trying to promote it, because I said this is the thing which people who know about a case of cancer look to us to do. They're at the end of a rope. There must be some way we can help these poor people.

We started out by having the usual referral services. Cancer Society local offices would know where just everything was, clinics, self-help, homemakers, etc. Then, I said, "Really, this isn't enough. We're shoving it off on other agencies. There must be something we can do." Well, the dressings program was an obvious thing. That was an old activity in the Cancer Society, making dressings and distributing them; when you need them, by God, you're grateful for it. We got the units into a program of providing drugs, because these tended to be expensive and one of the reasons why people ran out of money.

DEVRA: Not experimental drugs? These are patented medicines.

CAMERON: No, these were the standard things which were used. It might be just analgesics and narcotics in one instance, might be hormones in another. Now they're heavily into purchase of chemotherapeutic drugs. Transportation of cancer patients to and from the hospital, to and from clinics for treatment. This proved to be a rather needed program. Oh yes, and the provision of sickroom equipment. These are called the loan closets. Hospital beds on down to basins and dressings. These were the elements of a cancer service program.

I was pleased to know--a lady called me up the other day here and said, "I have a friend who has breast cancer which has metastasized to the lungs. She had great difficulty breathing, and they cannot afford to buy commercial oxygen any more. Do you think you can get the Cancer Society to do something?" Well, I called them up, and they said, "Sure." They took her name, and they were able to keep her reasonably comfortable until the end by simply doing this. Here are these homely things to be sure--they don't control cancer, but they just make the misery a little lighter--I guess is the best you could say about it. But we did try to stimulate these services and get the divisions to explore as many different avenues as they could, how they could be helpful at the local level. Those were the achievements.

The great crowning achievement of this period was nothing I had anything to do with, or at least I have indicated to you, that it was at arm's length, and that was the Research Program. Dr. Little could put very little up for research. But when they began to take in $10,000,000, 25 percent of that amounted to $2,500,000 a year. And so extramural research became a substantial part of the activity. It is the thing that I think the reorganizers are happiest about and would point to as their crowning achievement. As a corollary, since we were out ahead and at one point putting more money into research than the NCI, we turned around and through citizen's testimony persuaded the Congressional Appropriations Committees to do better.

DEVRA: When you used to think of an ACS volunteer, what kind of person did you think of?

(Volunteer Concept)

CAMERON: Well I thought it was, pardon the expression, the little woman who wanted to do something but she didn't have the means to contribute money. There were little things she could do: She could serve as a member of the local board, she could solicit during the months of April and May, she could distribute pamphlets from door to door or whatever, and she could shake the canister at the railroad stations during campaign times and other times. It used to get down to a very chummy level, where you acted as a kind of missionary, you tried to press into the hands of your neighbors in the bridge club these leaflets of the seven dangers signals. This was my concept of a volunteer.

When I got into the Cancer Society, I enlarged that view somewhat, because I could see that there were women who were doing their damnedest as best they could to make this Cancer Society achieve its potential. There was this tremendous saving of life that was possible. They were volunteers, but they were in administrative positions, trying to lead other volunteers, recruitment. We thought that the volunteers were in the best position to do this kind of thing. And, of course, all of our Board members, from local units to National Board, are volunteers.

The growth of the Cancer Society's corpus, I think, was through such humble devices as the volunteer. (I told you about the Field Army and what an exalted position it occupied at one time.) Actually, I think, in a very practical sense, the Women's Field Army provided the bridge from the old organization, which was almost exclusively volunteers, including a lot of people who worked for the National Headquarters, over to this newer organization which was now so strongly professional in the best sense of the word.

DEVRA: To the extent that you've remained active, and I can see that you have, what role do you see for the American Cancer Society in the next quarter of a century, given the fact that we do have this enormous Federal investment in cancer control?

CAMERON: Well, you know, we used to say the Cancer Society will go out of business when a cure for cancer is found. (I wasn't the one who originated the phrase, I guess it was Mefford Runyon. By the way, have you talked to him? I get notes from him at Christmas. He used to come into New York a couple times a week.) I think the Cancer Society is here to stay, for the reason that it has reached that critical momentum of activity where you can't stop it; it's going to continue to grow, I suspect. I was skeptical about this when I went to an annual meeting two years ago and learned that the Cancer Society annual fund-raising had exceeded $90,000,000. I said "this is it. Now we're plateauing." One of the reasons it does reach such heights is due to the growth of the legacy program. People will not know of a specific cancer research institute that they think is worth supporting. Now, attorneys have been well informed, trust officers of banks, too. The Cancer Society supports research, it's got the best scientific advisors available, etc., and it's all true.

(Legacy Program)

DEVRA: Did this legacy program start during your period?

CAMERON: Yes.

DEVRA: Can you tell me a little bit about how that emerged? Because obviously it has had an enormous impact on your resources.

CAMERON: We would receive, adventitiously, bequests. So we said, there must be a way of encouraging this. We prepared the usual legacy forms which were printed in much of the literature--I hereby bequeath to...etc.

That wasn't enough and so we got a program started. How it really got formalized, I don't know, but we encouraged the divisions to get their best doctors and scientists to make a pitch at meetings of trust officers. In Philadelphia, for example, we would get (I did this while I was Dean of the Medical School) trust officers of the ten largest banks. We'd get somebody sympathetic enough to the cause to host a luncheon. This was usually at the Philadelphia National Bank or the First Pennsylvania Company. They served very good food.

DEVRA: At the ACS's initiative?

CAMERON: Right. And then we would go and make the pitch about the importance of the cancer movement and the program which had been evolved and the care which was exercised in the evaluation of projects, etc. Through this word we hoped it would spread to trust officers universally. It evidently has done so, because we have now this steady flow of bequests coming in. It varies, of course, from division by division, year by year, but nevertheless it is achieving something which is almost predictable.

DEVRA: You can even budget with that?

CAMERON: Yes, you can.

DEVRA: Do you think in general the Cancer Society in your tenure and subsequently has spent its money wisely?

(Management)

CAMERON: You understand that I feel this loyalty because I've known other voluntary agencies--not nearly as well as this one--But I've been a member of the National Society of Fund-Raisers and things like that and you get to know other organizations, particularly in a community. I don't know of any which is as well managed, which has had the degree of excellence and the competency of its guidance as the American Cancer Society has had. I think it has become an example of the voluntary agency movement, such as we have not had before.

I used to think that Basil O'Connor ran the best agency (March of Dimes), and that may have been because they raised the most money. But, no, they achieved what they set out to do. Now, the Cancer Society has a much more diffuse demand on its resources. We're not looking at the cause of a disease, we're looking at something terribly complicated. It's going to take a long time. At the same time we have the burden of the present-day problem on our backs. How are you going to divide the money between research to find the cause tomorrow and eliminate it and the needs of today, to save lives that can be saved under the terms of the earliest possible diagnosis and the best possible treatment. I have said a number of times that I don't know of any agency which has had a broader array of demands: I think this is still true today. I think the leadership has been absolutely superb, and it has been altruistic. I think Lane Adams (bless his Mormon soul) has chosen to do this because of a deep religious or spiritual feeling or desire for doing something good--a mission. Mefford Runyon was the same way. He was retired from the CS, he didn't need to work. And so it's been.

DEVRA: You've really got dedicated people?

CAMERON: Yes. I think it's remarkable.

DEVRA: If you were czar of cancer control, what would you do? In other words, if the National Cancer Program should lose some of its glamor, as some people are predicting it will and certainly some of its resources or maybe it would plateau, how do you think the ACS should change to accommodate this diminished Federal interest?

CAMERON: Well if I have heard correctly the ACS now owns its building. You doubt it?

DEVRA: I don't know. The rest of the building has Grey Advertising. Maybe somebody game them the present space, maybe the Champion Paper people gave...

CAMERON: Oh, that's how they got it I guess. Okay.

What I was leading up to was this. I think, first of all, I was saying that Rod Heller was a good administrator for his time. This guy Rauscher is a whiz kid, and his like has not been seen in the national cancer scene heretofore. I think he is absolutely a genius for the job. He is scientifically trained, a virologist, I believe, and here he is leading this tremendous social movement, which is the most organized professional attack on cancer that has been devised until now.

In a small country like Sweden, with the thing beautifully systematized, patients come in to these outlying stations and are referred to the center. That is what we are after. That's what Rauscher's after, I believe. We've got to go slowly and cautiously because we've got an awful lot of people who are still concerned that the integrity of relationship between the physician and the patient has to be preserved, above all else. Just let competency step aside for the moment. Now then, I think that this is going to materialize. I think it is also possible that the government is going to be constrained to support other forms of research than cancer, which has been getting a lion's share a long time. There is some feeling by the "retarded children" people and others that they ought to spread the money around a little. Maybe in this sense some of the glamor will wear off. Maybe we won't influence the death rate for another 5 years or 10, and people will say, "Oh well, so what else is new?"

But should that happen, and the federal support falter, I think the Cancer Society ought to pull out all its stops, and say, "We're going to step into the breach. We're going to use ACS money to serve the centers. And we're going to put the radiation therapy equipment in and we're going to have beta accelerators, here, there, and there, like the pins on my map of old, and we're going to have plenty of surgeons who are trained specially to do cancer work and medical oncologists aplenty."

There are going to be strategically located centers, with these new doctor-scientists, and they are going to have, of course, the added attraction of cancer chemotherapy (medical oncology), which I think now is a respectable specialty in its own right. We're going to have people who've been trained at chemotherapy centers such as Boston and Bethesda and Houston, and they are going to go out like the neurosurgeons went out from Dr. Dandy's clinic in bulk, when there were no neurosurgeons.

DEVRA: That's great emphasis on treatment and to some degree on diagnosis, now what about the other traditional activities in which the ACS has really pioneered. Some interest in prevention, I'd say less than perhaps would have been desirable, definitely in early detection, and now some reviving interest in continuing care and rehabilitation. Do you think that the ACS should abandon some of its traditional interests in public and professional education in order to invest in centers of excellence.

(Goals)

CAMERON: Yes. I'll tell you why I do. I think public education is vital. Polls seem to have indicated this. I'm terribly discouraged with all that's been done and the ACS even incurring the displeasure of some people who call it a propanganda machine, etc., stirring up cancerphobia in this country as we do nowhere else in the world, etc. Our record isn't all that good. Moreover a discouraging percentage of people have no knowledge of more than two danger signals. They know that a lump in the breast is ominous and maybe a sore that doesn't heal, but it's pretty spotty beyond that. I don't know if you can stir the people up enough to take the initiative to really let his education "sink" in to the point where it will ring a bell when the signal turns up.

The doctors--I think we can rely on the medical schools now to carry the ball. Maybe they still need a little prodding. I like the practicalities of it. I don't think a student should receive the M.D. degree until he has performed ten proctoscopies on his own under guidance. And there are practical things that could be done. But the medical curriculum is so crowded today that it is hard to give cancer the place that we think that it ought to have. Nevertheless, I think, I hope, we are achieving a common understanding of what a minimal cancer teaching program is.

I think the National Cancer Institute is the one to carry that job forward until it's a little more finished than it is. They did it originally when they established the cancer teaching program in the medical schools, schools of osteopathy and dental schools. Now, I think that the American Cancer Society could ease up on some programs which are not particularly productive in changing the death rate. You've got to keep your eye on something.

DEVRA: What are the kinds of things they could loosen up on?

(Rehabilitation)

CAMERON: Rehabilitation. I think this is important, but I thought at the time we got too stirred up over laryngectomies. If a guy wants to learn to speak, he's going to find out how to do it. Maybe we were right in those days in encouraging the speech centers and the enphasis on speech as a form of rehabilitation. But now it's there. I think it's there. Again, colostomy training. Well, you don't have to be very bright in order to evolve your own system. Everybody does. And so these things to me do not loom as important. When you get down to an individual pathetic case, oh, it will wring your heart, but that's taking your attention away from the important thing.

(Oversight)

Professional education: Well, I hope that the impact has been made and that professional organizations now are sufficiently conscious of the cancer problem and its impact in order to carry the ball. I think we can ease up there.

I guess we've got to keep going on with public education. But that can be done without too great an expenditure of energy and time. The lines are filled. We know the only things there are to do with this. I don't think it's so effective. So I think I would relegate it to a place of, say, secondary importance so that, if worse came to worst, and we had cancer programs evolving, we could put the emphasis on Centers, because they are where the clinical advances are apt to be made.

A geographic area that has been designated, recognized and accepted now is the State of Florida with Zubrod at the head. Suppose the government pulls out his underpinnings. He has said to Dr. Schultz that he may have to turn to the public in order to carry the program forward. Now this would be a serious threat to the ACS. And this was recognized by Lane Adams at the time. I was at the meeting of the House of Delegates when this was considered. Shall we encourage our divisions to cooperate with these comprehensive cancer centers? And the answer was yes. Let's do it with our eyes open. For the moment, it's the only decent course we can take.

If it's going to achieve something, let's help it. If it proves to be a dud, well we will have done our best in good faith. Now it is turning out that it may be a competitor. I wouldn't want that to happen, but the reason why I don't think it will seriously hamper the Cancer Society is that the people who can give substantial money to the Cancer Society--and I'll allow, I haven't seen a breakdown, but I would imagine that a very substantial part of the Cancer Society's money comes from large gifts, $1,000 or more--people who can give that are apt to be people who are congenitally or by experience mistrustful of big government moves. I think they are always going to look at the Cancer Society as "my voluntary agency." They pay taxes by force. But they will feel good about giving if they give it to the voluntary agency.

I N T E R V I E W

Interviewee: John Dunn, M.D.

Interviewer: Devra M. Breslow

Location: Berkeley, California

Date: April 1, 1976

Interview with Dr. John Dunn, Cancer Epidemiologist, California State Department of Public Health, April 1, 1976. The interview was held by Devra Breslow in Dr. Dunn's office in Berkeley.

DEVRA: Why don't you give me your biography and your association with cancer control?

DUNN: I graduated from Washington University Medical School (St. Louis) in 1931; then I came out here to San Francisco and interned at Letterman; from there I went to St. Luke's for a couple of years. From there I went into the U. S. Public Health Service. I was based at the San Francisco Presidio and was in clinical work. In 1936, when the Social Security Act became law, the Public Health Service found itself with very few people having much experience in public health, but the Service had a big responsibility under the Social Security Act. So I, unfortunately I thought, was tagged by the Service to move from clinical work into public health.

DEVRA: You didn't feel that that was your choice?

DUNN: No, like most medical students I had a practice-oriented medical career in mind. I hadn't really even considered public health as an interest of my own. But the regional office for the Service was here in San Francisco; I was assigned there. They sent me to Imperial County and San Diego and Santa Barbara to expose me to what the local health departments looked like and what they did and so forth.

DEVRA: Mainly on the scientific side or on the administrative side?

DUNN: Administrative primarily, going out with the various people in the various departments, seeing what they did, how the health officer dealt with some of his problems. It was an interesting experience, entirely new to me. Then I was sent to New York State to be part of their epidemiology training program which they ran for their own people and the Service had people sent there.

DEVRA: That wasn't just cancer-oriented?

DUNN: No, that was all public health. We rotated through the different district offices. At that time New York State was set up on State district offices staffed by State people.

Then, in the spring of 1937 we had the big Ohio - Missouri River flood. I got tagged, along with 6 or 8 other Public Health Service officers to be distributed along the rivers to look for any kind of epidemic that might occur and so forth. That was, of course, in Ohio. I was assigned to the health department in Siota County, I guess it was. That was an experience too, because here was a part-time local health officer in a State department which at that time was pretty much involved in some of the political hanky-panky that was going on at that time, 10% clubs and that sort of thing. The health officer was in there half a day and I think he started drinking when he arrived. There were a lot of interesting people there, a lot of hills along the valley there, and the eastern Kentucky migrants who lived in these "hollers", as they called them.

DEVRA: This was the depression?

DUNN: This was the depression, right.

DEVRA: Is this coal-mining country?

DUNN: There is some mining, but most of these people, I don't know how they lived, they lived along these little streams, and did a little gardening. There was an iron foundry in Ironton, which was a pretty good size operation, and so forth. But anyway, this went on until the fall of 1937, when I was told I was going to the School of Public Health, but nobody told me where, until October...

DEVRA: Had you expressed interest in that or was that one activity that the Service guaranteed?

DUNN: The Service felt that I should do this. I was sent to Michigan and then following this I went to Washington, D. C., and started doing health department surveys.

DEVRA: When you went to Michigan, did you concentrate on Epidemiology or in what?

DUNN: Just the whole general field. Michigan was unique at that time. It wasn't really a School of Public Health; it was a department. We were sent to different departments, for exposure to certain things, like newspaper writing, for example. We were in a class which was directed to newspaper writing and public speaking, so they took advantage of all of the departments and not have everything locked up in a school.

DEVRA: Was it a one year or two year program?

DUNN: I think it was a one year. You got an MSPH. Had to write a thesis. From there I went back to Washington and was assigned to do health department surveys for the next year or so, like Evansville, Indiana; and a county in Illinois right across from St. Louis that I can't remember the name of; Tulsa, Oklahoma. These were general surveys to look at what they were doing and make recommendations on their shortcomings, on how they could improve their program.

DEVRA: In those days, were most of the health departments staffed by only part-time health officers?

DUNN: Not in Tulsa. I think this Illinois county had a full-time health officer. Evansville, I just can't remember.

Then the Service was asked to do a survey of the Health Departments in San Mateo County and in Los Angeles. This was a group effort, about five or six of us took part in it. I came out here for a month and I stayed six months. It was a very enjoyable experience. It was back in the days when San Mateo County had a lot of rural area, and Los Angeles still had fruit trees and had a lot of parking spaces for 10¢ an hour and Knott's Berry Farm was out "in the country." But in Los Angeles, they had both city and county health departments at that time. Mostly survey requests were not really what was to be objective evaluation. They really were folks getting enough ammunition so they could fire somebody.

DEVRA: Was the pattern that the county in question would request to the Public Health Service that the survey be done by professional Public Health Service trainees?

DUNN: Yes. What came out of the Los Angeles experience was that we became different kinds of specialists in different places. I think here I was involved in the vital statistics and that part of it.

DEVRA: Is this before Dr. Chope came to San Mateo?

DUNN: Yes, at that time I think there was a retired army officer, who was health officer. San Mateo County had a charter form of government, they had a stipulated amount of money for the salary of the health officer. $4500.00, if I can remember back to those days. I wasn't even aware of the depression, because I didn't have any money in the first place. But now that I look back, I realize more how things were: WPA and so forth...

DEVRA: How much did you earn as a Public Health Service Commissioned Officer let's say in the 30s?

DUNN: I think I got married on $128.00 a month.

DEVRA: They provided your uniforms?

DUNN: No.

DEVRA: Housing?

DUNN: No.

DEVRA: Even when you went out on these surveys, then you went on a Federal per diem, or something like that...

DUNN: That was a strange kind of arrangement. The Service at that time, as far as commissioned officers were concerned, paid mileage and that was it.

DEVRA: They didn't pay housing?

DUNN: No, and then, if you worked on a project, you had to arrange to go back to Washington for a conference or something like this, just to keep yourself alive, to get this money. Then you had to work with federal civil service people who are on a regular per diem, they were always anxious to make the thing drag out. They would do better if they stayed longer. We were always anxious to get in and get out because we were going to go broke.

DEVRA: When you stayed in San Mateo County or Los Angeles, did you live with a family?

DUNN: No. We were in San Mateo for a couple of months and about three months or so in Los Angeles.

DEVRA: Were your families with you?

DUNN: We had our families, none of us had children I guess at that time, but we all had wives.

DEVRA: ...Who couldn't get jobs, because this was the depression.

DUNN: Right. So we lived in Menlo Park in a motor court, in L.A. in an apartment building and so forth. Somehow we made out, I don't remember just how the finances were handled, unless there was some addition to the mileage bit.

I went back to Washington in '39, to write the balance of my survey for L.A., Joe Mountin, whom I was working under at that time, said, "Let's get through and get out of here, because I don't want somebody to find out that you are going to be assigned to something else, and decide that they have something else that they want you to do. I want you to go to the Harvard School of Public Health for a year or two, because the cancer people are interested in expanding that program and we would like to see whether the School of Public Health is the proper place for a person going into the cancer field to go. Will you get enough out of that atmosphere?" Nobody really had a very good idea of what the role of Public Health really was in cancer.

DEVRA: Were you the first person selected to do this?

DUNN: As far as I know, I was the first to go to a School of Public Health. Scheele may have spent sometime at Memorial Hospital, getting exposure to the clinical end of it. But I was the first one sent to the School of Public Health to see whether or not this was a proper beginning for somebody going into the cancer field.

DEVRA: You had by then made some kind of decision that you wanted to go into the cancer field?

DUNN: Yes, that was interesting to me, because there were some clinical aspects in all that would be involved. But then again, in the Service you are not a free agent, by any means. You went where the Service sent you, whether you wanted to go or not.

DEVRA: Let me refresh my memory, because I didn't have your CV in front of me. You did your internship also, ah, where did you do your internship?

DUNN: At Letterman Hospital.

DEVRA: Did you do a residency?

DUNN: That was St. Luke's, assistant resident.

DEVRA: In what field?

DUNN: I was a rotating resident to all different fields.

DEVRA: So you hadn't concentrated in either medicine, or surgery or radiology...

DUNN: No. I went to the Marine Hospital. I was in surgery there, orthopedics.

About the time I was writing my part of the L.A. survey in Washington, there was the possibility of war being imminent, so the Coast Guard decided they should increase their number of people in the Coast Guard. All of a sudden, overnight, I was sent to the Portland, Maine, Marine Hospital.

DEVRA: This was before you went to Harvard?

DUNN: Yes, just barely before. 1939. Before September, before the war broke out. Actually, with the depression after all, they thought there would be an influx of recruits, which there didn't turn out to be. I was up there, and examined three or four men a day. The Marine Hospital was pretty happy to see me, because I could help out in the ODs. In fact, they offered to let me have a room in the hospital, if I wanted to do OD permanently, which I didn't.

DEVRA: OD means?

DUNN: Officer of the day. Officer of the night, really.

In the meantime, I was opening up all the communications that I could in terms of... there really wasn't all that much to do there, and I wanted to go to Harvard. I finally got freed to do that.

DEVRA: When did you actually enter Harvard School of Public Health?

DUNN: Well, it was October, I suppose, it must have been since classes had already started; October, 1939.

DEVRA: And it was a School of Public Health. Dr. Lombard was then head of...

DUNN: ...Chronic disease in the State Health Department.

DEVRA: And was Dr. Snegeriff there?

DUNN: No, I think that was before his time. I don't remember him until later years.

DEVRA: They actually had a Department of Chronic Diseases in the School?

DUNN: No. I went in to epidemiology classes and statistical classes and child health and then I did spend time with Lombard. When I got there, Lombard wasn't there. He was at the APHA meeting or somewhere. John Gordon was left in charge of epidemiology, so he kind of took me under his wing in deciding what I really ought to be equipping myself to do. He felt for a long time that epidemiology had applications with the non-infectious diseases. You can use the same line of reasoning.

When Lombard got back, he was very upset, because I had made so many commitments, decided by other people. He was an interesting guy, quite an intense person and quite secluded. At that time the MacDonald girls were with him, they had a little suite where they did their statistical stuff and so forth.

DEVRA: This was the Massachusetts Tumor Registry, is that right?

DUNN: No, they had a series of clinics over the State which were developed by the Lombard unit and where they had profesisonal people coming, mostly from Harvard, to talk to the local physicians about different aspects of cancer, what was being done in treatment and this sort of thing, largely clinically. Then there was Pondville, which was the state cancer hospital.

which was the state cancer hospital, which had some of the case material from the clinics fed into it.

DEVRA: What kind of data would the MacDonald girls be working on?

DUNN: Well, Lombard had case-control studies. He was looking at the mortality of immigrant groups and so forth, the differences in the Italian segments of the population. I have a copy somewhere, it was written in _____, showing the differences from past experience of some of the ethnic groups.

DEVRA: He had already made a career in cancer epidemiology?

DUNN: Yes, he was a good statistician.

DEVRA: Was he a statistician or was he an epidemiologist?

DUNN: He was a good statistician and epidemiologist. Some of the early cervical cancer studies were done by him.

DEVRA: In the 30s you mean?

DUNN: In fact they had one...

DEVRA: Incidence and prevalence studies?

DUNN: Case-control studies. They had one boo-boo. One of the things that they were interested in at that time was trauma of the cervix and the impact of douche chemicals and that sort of thing. One of their studies dealing with cervical cancer asked a question whether the women had a cervical laceration. I am sure that women don't know this. They know whether they have had a perineal tear or an episiotomy, but they don't know what happened to the cervix. So there was a biased effect that you get when you ask questions about the area that's the site of the cancer.

DEVRA: They are actually asking this of the women, not of the doctors examining them?

DUNN: Right. So there was a possible relationship between cervical cancer tears for the cancer group as compared to control, as you would expect. They are talking about something else, but they are talking about an area where these women now had a cancer.

The same thing about trauma of the breast. You can always get a positive association if you pursue it. Because women who have cancer of the breast are thinking about all the things that might have happened. When asked about trauma, they remember having bumped their breasts.

But Lombard did some very good work. As a punishment for my failure to get started on the right foot, I had to go to Lexington and set up one of these cancer clinics, one of these setups where you contact all the local people and get a public meeting organized, and explain the whole cancer program, the need for having clinics accessible to people.

DEVRA: Yesterday when I talked with Gene Miller, who also studied with Lombard later, in 1958-59, he said that, at least here in California, the use of the term Clinic really meant Tumor Board, that is, a multidisciplinary consultation at a specified time and week. Is that what you were working on in Massachusetts or was this actually a clinical experience where patients would come in and be examined?

DUNN: They came in and were examined.

DEVRA: By local physicians?

DUNN: Yes.

DEVRA: At a certain time of the week, in a hospital setting or a health department or what?

DUNN: It must have been in a hospital. How these operate I am not too clear. I don't think I ever knew. I went with speakers who went to these areas to talk, but I don't remember seeing the operation functioning as such.

DEVRA: The speakers were there mainly to speak to laymen or to physicians?

DUNN: To the physicians.

DEVRA: Were they provided by the Harvard School of Public Health, the Medical Society?

DUNN: They went out, I think, for Lombard. People who were working at Pondville, which was a State institution and largely staffed by Harvard physicians and very competent people like Ira Nathanson, and so forth. They were very knowledgeable.

DEVRA: Was part of your punishment also to go to Pondville for a while?

DUNN: Well, I went there because I wanted to.

DEVRA: And you spent some time there actually working?

DUNN: I didn't work. I visited clinics and watched what was going on there and at the Huntington Memorial Hospital, too. (I haven't been to Harvard for so long I don't exactly know what's happened) But Huntington was a little clinic building close to the School of Public Health, only for cancer patients. Nathanson had a clinic there and Simons, who was another prominent clinician, and Taylor. They had rounds there and I went with them on those rounds to soak up what I could on the clinical aspects of cancer.

DEVRA: How long did you actually stay with Dr. Lombard and the School of Public Health?

DUNN: Well, until September, 1940. I had my Master's degree already so I wanted to qualify for a DPH which I did, so far as the selection of a thesis subject. My subject was to be cancer control activities and the Service was interested at that point, too. In fact, (Dunn hands Breslow an unauthored statement which he, it

appears, had compiled.)

DEVRA: Were you going to limit it to cancer control activities in Massachusetts?

DUNN: No, in the whole country.

DEVRA: You could have done this history for us.

DUNN: That would need a lot of updating I think by now.

DEVRA: Had you actually started drafting some of this?

DUNN: I had an outline.

DEVRA: This is on State control and cancer laws, resume, cancer program, dated February, 1940.

DUNN: I had this letter from Jim Crabtiee, in which he was outlining the content of study and kind that was supposed to be done.

DEVRA: Crabtree was then at Harvard?

DUNN: No, he was in the Public Health Service. He was an epidemiologist, initially. He and Scheele were the people outside of research that were in the Cancer Institute at that time. I was subjected to a certain amount of pulling and hauling between Scheele and Crabtree as to what I would do when I finished my time at Harvard. Crabtree was the one I corresponded with about this possible study of cancer control, nationally.

DEVRA: Was he interested in that?

DUNN: Yes, he was; I would say he was primarily an epidemiologist. The Service set up a big epidemiology study. It must have been in the late 30s, early 40s, where they had people stationed in hospitals in New York, Chicago, New Orleans. This was one of the real shotgun affairs, because I think he (Crabtree) had a part in this, too. They went and asked all the clinicians they could get hold of to suggest any questions that seemed to be appropriate for the possible association of cancer. They compiled a huge document, like half an inch thick, of questions that were collected on these patients. Crabtree, as I remember, was trying to get something out of this material at that time.

DEVRA: What kind of title did Dr. Crabtree have in those days?

DUNN: Surgeon.

DEVRA: As opposed to surgeon general, but did he become a Surgeon-General?

DUNN: No, he went to Pittsburgh to be the dean of its School of Public Health. He was there with the Surgeon-General, Parran.

DEVRA: Were you the only trainee at that time?

DUNN: Yes, the Cancer Act was passed in 1937, and what they had in the way of cancer at that time was in Massachusetts.

DEVRA: What about New York State?

DUNN: Well, I am talking about the Service activities.

DEVRA: The Service actually had some programs?

DUNN: Yes, research programs, animal and so forth. Then the Cancer Act and the Wilsons donated the land for the Cancer Institute. When I came there in '38, it was under construction. Somehow they got the thing expanded to include all of the NIH. The Cancer Institute really hadn't gotten set up until about '39 or '40.

DEVRA: And you were the only fellow or trainee who was, in a sense, being groomed to come back to the Institute. Were they also doing this on an experimental basis, thought that they would try it with you and if it worked, maybe they could find some money for two or three more?

DUNN: Yes, but my thing got interrupted. In September '40, I was supposed to return to the Institute. I corresponded with a number of people about this other activity (my thesis). If I were going to do the DPH, I could probably stay in Boston for some more time to get the New England states.

DEVRA: This was to write your dissertation on cancer control at that time.

DUNN: Yes. But Dr. Thompson was head of NIH at that time. When it first came up to him as to where I would be assigned, he said, "Well, we can't very well justify a commissioned medical officer being assigned to cancer control at this point."

DEVRA: Was that considered frivolous?

DUNN: Yes, I think so.

DEVRA: Some kind of civilian, low priority activity.

DUNN: And the Service, of course, was kind of semi-military, you know, with the Coast Guard Medical Service. In time of war we would get assigned to the Army and Navy and so forth. This happened during the war. It was very confusing, the Public Health Officers were appearing in Army-type uniforms at one time and Navy-type another time; they had everybody confused.

DEVRA: Depending upon where they were stationed at the time.

DUNN: Yes, whether they were working with the Army or the Navy. And the uniform for the Service for the boarding officers was an Army type, khaki, and reminded you of the English in its design.

DEVRA: The uniform that was worn until the Service (i.e., commissioned corps) was more or less obliterated looked more like a Navy uniform. Dark navy and gold braid.

DUNN: Yes, that was the uniform worn in the hospitals, associated with the Merchant

Marine. PHS hospitals were oriented more towards seafaring. But the boarding officers who had to identify themselves in something different wore these khaki uniforms.

DEVRA: So you were called back from Harvard, and you were told that you weren't going to be able to complete your dissertation right then, is that correct?

DUNN: Right, I was being assigned at that time to Dr. Louis Schwartz who was in industrial dermatology. He was by himself, he and another young guy who was not in the Service but working with him.

DEVRA: What did that have to do with the war effort?

DUNN: You know, when the munitions plants started being set up, there was a lot of sensitization that took place with explosives. The people who were trained to operate some of these activities would get sensitized, would get a dermatologic reaction. We could treat the rash, but as far as removing their sensitization, no. They couldn't protect themselves very completely. There wasn't much that we could do about it.

DEVRA: So you were concerned about things like occupational protection? Going into plants and teaching them about gloves and masks?

DUNN: We would do patch testing on recognized cases, to establish whether or not they were sensitized to different explosives.

DEVRA: What about radioactive materials? Had that emerged at that point?

DUNN: No, of course there was that business in the first World War. But nothing of that sort that we were involved with anyway. Almost all was in munitions and in factories where they were introducing the chemicals.

DEVRA: Looking back on that, do you think any of those materials were carcinogenic in the degree of exposure in which some of the people were exposed?

DUNN: I don't know. The only thing that has come out of this is mustard gas. It's been identified as a carcinogenic factor.

DEVRA: That's interesting, because mustard gas in another form was used in the treatment of Hodgkin's disease (nitrogen mustard)...

DUNN: It is now. Well, you know, a lot of therapeutic agents are probably carcinogenic.

DEVRA: In and of themselves?

DUNN: But you've got to cut out the cancer that you are dealing with. You can't worry about what might happen 10 years from now, because you aren't going to be around unless it's effective now.

DEVRA: Was mustard gas being developed in the second World War or was it the first World War?

DUNN: It was the first World War, but they were getting ready to use it again.

DEVRA: And there were several plants here in the U. S. where this was being produced?

DUNN: Yes, some of the early observations on those populations didn't seem to show anything. It's kind of hazy now, what exactly the studies were in this country or somewhere else. But since then, there has been follow-up on the population of workers exposed at that time, in case there was a hazard.

DEVRA: Mainly a cancer-producing hazard or other kinds of diseases?

DUNN: Cancer, primarily.

DEVRA: So you went into dermatology and you were dispatched to industry. How long did this go on?

DUNN: When I got into this, I got interested in the whole subject of sensitization to simple compounds. Landsteiner, who was a famous old immunologist, had done a lot of work with this. I got interested in poison ivy, in fact I went with Schwartz---he was a very interesting guy, you know---very intense and hasn't the foggiest notion about research. So when I first went with him, he had an ointment developed which had an oxidizing agent in it, the hyrouacil which is activated in poison ivy, and can be oxidized and neutralized. He wanted to have some pictures taken demonstrating the use of the ointment; the other guy working with him wasn't interested. I had poison ivy as a kid, but I didn't think I was terribly sensitive, so I went out and put this ointment on and then picked poison ivy. We put it on our arms and went back to the lab later, and used some wetting agents so these were removed. That night, my wife and I were doing the dishes. All of a sudden, I began to feel something itching, burning in my arm, and all of a sudden, I realized that I was probably in for some trouble, which I was. I had the worst case I had ever had. Dr. Schwartz's idea of developing something was to do something that was rational, but don't worry about proving it, experimentally.

DEVRA: And you decided to take the bull by the horns and put your arms in it. When you went back in and said, "Look, I have poison ivy," what did he say?

DUNN: I don't remember precisely what he said. He said the oxidizing agent possibly deteriorated or something. Of course, he wasn't very sensitive, and he got by allright without more than a little evidence of dermatitis. But, I got interested at that point. We used a simple compound which can produce a sensitization reaction and all these other things that cause contact dermatitis. Then I started to work to unravel how these became sensitizers. I left Dr. Schwartz and went with the Industrial Hygiene Lab. I think they gave me the title of skin physiologist. That must have been in 1944, the war was still going on.

DEVRA: You stayed in the U. S. through the whole war?

DUNN: Yes, they were having trouble at that time with some of the vehicles used for DDT. There was a residue from producing aviation gasoline, which was a good solvent, but I had a lot of trouble with exposure to it, because it turned

out to be a photosensitizer. We did a lot of work with DDT as to its toxicity and as a sensitizing agent. These vehicles were used as solvents. You get that on you and you get into the sun and you get a dermatitis.

DEVRA: Did you subject yourself to some more personal experimentation?

DUNN: No, in fact we were using a carbon arc light, initially, to see if it could induce photosensitization. Kids went out to play ball. They would come in, saying, "Hey, you know, I got a rash here," and it turned out that our carbon arc was not equivalent to sunlight, so we had to use sunlight from then on. We had people sitting on a hill with exposed skin areas, which they didn't mind doing in the sun to test them. That went on until 1946. The war had just come to an end and the Service was responsible for examining immigrants. I went to Munich, Germany for nine months or so, to examine the first immigrants.

DEVRA: Who were going to come from Germany or Eastern Europe?

DUNN: Well, all over, most of them from concentration camps.

DEVRA: And going to come into the U. S.?

DUNN: We had to do physical exams. TB was the thing that would be restricting.

DEVRA: If you found TB in such a person, you could not allow them to come into the country until it was arrested. Were there provisions made, hospitalization and drugs?

DUNN: Well, not by the State Department, because we were just having to identify things that were restrictive. There was quite an organization to look after these people.

DEVRA: Was this through UNRA? You said it was through the State Department; I guess it was through Immigration.

DUNN: I was working for the State Department. But UNRA were looking after these people and of course we had problems arranging for what they needed.

DEVRA: Then you went back in 1946-48 into skin physiology at NIH?

DUNN: When I first came back I went into that, but by this time the laboratory was gone and the people who had been working with me and my guinea pigs had been dispensed. I had to do some inbreeding, which was an interesting point too.

Dr. Pap back in the early 20s worked with guinea pigs. I was trying to inbreed guinea pigs. I happened to see his papers. The name was so strange. Later, when I heard it again, I said, "Hey, I know that name," trying to remember when and where I had seen it. He was using cytology on guinea pigs to determine estrus, to determine when you can mate them. One of his papers I read to determine when guinea pigs were breedable for laboratory use.

DEVRA: Was Dr. Pap, let's say in that period, say in the 40s, before he and Traut published their paper which I think was 1943-44....?

DUNN: Well, Dr. Pap's first paper indicating that he could distinguish cancer cells was 1928.

DEVRA: At the same time Babes published his paper in Rumania. I don't know whether you recalled this, but there was a man named Babes, I think he was Rumanian or Bulgarian. He also had published perhaps in a European journal, but not an English-language journal, similar kinds of observations to Pap's. Apparently both of them were ignored. Later, I want to get back to this.

DUNN: They weren't too much interested in me continuing what I was doing within the Institute. At that time, Norman Topping was Assistant Director, and he thought I should be assigned.

DEVRA: How much control, let's say, would a Commissioned Officer have over where he was assigned? Could you express some interest?

DUNN: Yes, you could, but the Service needs somebody here and your Commission indicates that you are willing to serve wherever the Service decides that they need somebody.

DEVRA: You entered the Service in 1934, is that right? Roughly how many Commissioned Officers were there in 1934? Do you have a rough idea? Was it in the thousands or the hundreds?

DUNN: I would say hundreds, but most of them were in the hospitals. There weren't very many people, in fact, none of the Service people, Service Officers as such (outside of some epidemiologists), who had done work in infectious diseases, but as far as dealing with health departments as such, they were all, almost all federal civil service people, and they were just a handful. I remember when I first came, Dr. Ford was over here, in San Francisco. A big program at that time was pit-privies. They had developed a concrete slab with a concrete raiser which was an improvement over what people had had in the past and this was a WPA project.

DEVRA: These were for the workers, is that right?

DUNN: For people who were interested in having a new form of sanitation. I remember over here somewhere in El Cerrito---it was open country then---we came over and saw them making these risers. In fact, they had a big celebration, I remember reading in TIME, in West Virginia, when they produced their 100,000. You know people actually were so pleased with this kind of a thing, that they would invite the neighbors in to see.

DEVRA: Indoor toilets were a new thing in the 30s.

DUNN: Outdoor toilets.

DEVRA: These are outdoor, still.

DUNN: This concrete slab was shielding it from flies and it was much easier to maintain.

DEVRA: So after the war, well, it was several years after the war, in 1948, that they decided, with you presumably, that you had to do something else.

DUNN: As I say, the Cancer Institute was just getting itself mobilized, and they had set up a Cancer Control Unit...

DEVRA: It was called Field Studies in those days, Austin Diebert, is that right?

DUNN: Austin Diebert was head and Ray Kaiser was deputy. Kaiser and Diebert were the only ones there. They were feeling the need for more staff. So they said to me, "Whatever you would like to do, if you can find something that we would find agreeable to us, come aboard."

I thought I would be interested in epidemiology. That's what I went over there to do. There was this mass of data from a 1940 time period, that had a lot of data that had never been looked at. It had been collected under Scheele, probably by interviewing patients in New York, I suppose Memorial, and Hines Hospital in Chicago and Charity Hospital in New Orleans.

DEVRA: And they interviewed cancer patients?

DUNN: And they had an interviewer stationed at each of these places to do this extensive interview.

DEVRA: Were they concerned mainly with cause, trying to find etiological clues?

DUNN: They had asked clinicians, "What do you think is important for this or that site of cancer?" They had this tremendous shotgun questionnaire that was used by Sandy Gilliam later. It was one of these things in which you tried to do everything. When you get on to a specific, what you really want is not right there. I remember seeing some of the correspondence of people who were doing this survey, writing to whomever it might have been, Crabtree, maybe, saying that they had this impression about lung cancer and cigarette smoking.

DEVRA: Even as early as the 40s, some of the clinicians were saying this?

DUNN: No, this was some of the people who were out doing the interviewing in these hospitals.

DEVRA: The interviewers were saying this.

DUNN: I remember one letter I saw, this was mentioned as something they thought was more frequent than you might expect. But that's all that ever happened with it.

DEVRA: Was Dr. Crabtree a cigarette smoker?

DUNN: I would guess that he was.

DEVRA: What about Austin Diebert and Ray Kaiser?

DUNN: Yes, I am not sure about Ray, I think he did but I am not sure.

DEVRA: In those days, just about everybody did. Did you smoke cigarettes?

DUNN: Yes, I smoked cigarettes and Sandy smoked cigarettes. You know, we said later, "This is something that we shouldn't have done, but we did it, and the damage is done, so nothing worse can happen to you."

DEVRA: When did you stop smoking?

DUNN: Well, I quit several times. Once, in medical school, I started driving out here by myself and being sleepy driving, you know I would get a pack of cigarettes and light up one, it just helped to keep me awake. By the time I got out to San Francisco, I was smoking like I did before I stopped. Then I stopped when I was at Harvard. I went to a pipe. During the war cigarettes were hard to get. Working in a munitions plant, you came out, had maybe a few minutes, but never enough time for a cigar or pipe, and cigarettes were possible, so I got back into it that way. But I quit 10-15 years ago. I am with a pipe again.

DEVRA: So you went to Field Studies. They had this massive data from at least three different hospitals, and you were going to attempt to analyse these data.

DUNN: Scheele was Surgeon-General about this time and Sandy Gilliam was in Michigan, and he was needing to be reassigned somewhere. Scheele talked with him about coming to the Institute and looking at cancer as an epidemiologic subject. He was recruited by the Surgeon-General. I was deciding myself that that's what I'd like. The assignment was obvious. Sandy was going to be doing the epidemiology, although we worked right across the hall from each other.

DEVRA: Were you both doing epidemiology then?

DUNN: No. There were several things being promoted, cancer detection clinics and breast self-examination were being talked about and a little later the Service and the ACS developed a film and diagnostic tests were a big deal at that time.

DEVRA: Was this the Penn test era?

DUNN: Yes. Is Dr. Penn still around?

DEVRA: I don't know, I picked up something from Gene Miller yesterday about the Penn test, and, of course, at UCLA, there are some people that remember that with horror. Dr. Warren was deep into it. They did a Penn test study at UCLA before the medical school was really off the ground.

DUNN: They were just in these quandaries, I heard. Andy Dowdy was a classmate of mine.

DEVRA: At St. Louis, is that right?

DUNN: Yes, and you know they were desperate to do things. Penn was developing his test and they got interested in it, and they were using Sawtelle and the lab over there to look into the test.

DEVRA: Was that the only place in the country where they were working on the Penn test, where it was being looked at critically?

DUNN: At that moment, but you see there was a whole rash of tests at that time. Huggins had a test. The ACS really sacrificed him one of those fund-raising years, by saying this is the greatest thing since the wheel, you know, and there was another gastroenterologist in Connecticut who had a blood drop test...

DEVRA: These were supposed to be some kind of single index that you had cancer?

DUNN: Yes, there was a whole rash of these and obviously as they were being worked on initially, with well-established cancer patients as one group and young well people as controls. That really didn't tell you very much about how the tests might perform. So the question was, what kind of test would we want if we could get one?

DEVRA: And the field study people were interested in that?

DUNN: Yes. When all these tests were coming out, we really had no way of evaluating them. So I recommended that we should make a decision as to what kind of test we were looking for and try to evaluate it on that basis. Sam Greenhouse was a statistician and he thought that was an interesting thing, too. Dorn, of course, was a very capable person and he recruited a stable of statisticians, Mantell, Greenhouse, Schneiderman, and Cornfield, and they were all in one room a little bigger than this (10 x 10) just looking for something to do. Most of the tests were not unique. They gave you a value on a continuum: the normals in one area, the cancer in another. The question was how well they separated these two groups.

I used to go down the hall and talk to Sam. Then Mantell would be looking over his shoulder and he would tell Sam, well, if you want to do that, do it this way, the statistics of it.

DEVRA: So you actually published this Principles and Criteria for the Development and the Evaluation of Cancer Diagnostic Tests, Public Health Service Publication #9, in 1950?

DUNN: Yes, that's no way to publish anything. It gets lost.

DEVRA: Did it get lost, or did it circulate?

DUNN: Well, but I mean as far as the reference people looking for material, they just don't find or have access to it.

DEVRA: What kind of impact did this report have?

DUNN: Well, there was this conference in Portsmouth, with the Cancer Society, in 1949, the Conference on Cancer Detection.

DEVRA: Was this material on evaluating the diagnostic tests presented?

DUNN: Yes. I didn't present it, but I got into some of the discussion. At that time, the criteria we set required that if a test was going to be useful it would have to be negative in at least 95% of non-cancer patients and pick up 90% of cancer patients.
When we did this and got positive records, most of them would not have cancer because the cancer population is small and your non-cancer population is big, so even with that type of performance criteria, it would be questionable whether or not you had a good test. You could say, within this positive group, there are 1 out of 10 that really has cancer; the rest of them don't. Then you have to find which of these had the cancer, which don't.

DEVRA: Very respectable people were getting interested in this whole concept in finding a test for cancer. How long did this movement go on?

DUNN: Well, we set this up and then we said we had to have some laboratories that can look at these things objectively. So Freddy Homberger became one at Tufts; and then Lippincott in Seattle became one; B.M. Stowell at University of Kansas and Doug Sprunt in Tennessee and we had a man down in Alabama.

DEVRA: These are Pathology Laboratories?

DUNN: Three of them were pathologists, the one in Alabama and Albany was a chemist.

Sprunt, talking about the Penn test, was the lab that got involved in trying to confirm Penn's results. This almost led to a legal battle between UCLA and the University of Tennessee as to rights and so forth for the test, if it proved to be successful. And Sprunt made some modifications trying to sharpen it up. Penn had a precipitin test, and the antigen came out of livers of animals, similar to serology tests for syphilis.

DEVRA: Were any of these people considered quacks or were they all considered well-meaning, respectable?

DUNN: Yes, they were all enthusiastic and very dedicated. They were getting these differences because they were dealing with cancer patients who had a lot of physiologic incapacity and they are not specific. Many of them are related to the changes in the serum protein levels that happen to any sick person; and other sick people would get false positives too; pregnancy is another one.

DEVRA: So how many years did this enthusiasm, even at the Federal level (and I can see by the ACS) go on? I would say this is from 1947 to 1952-3.

DUNN: I left in 1953. I guess by that time we had gone through a number of tests. They were so obviously inadequate that enthusiasm for the possibility waned. Sprunt I saw frequently because I went to Hot Springs which is the old VD treatment center where we were doing cytology on the people admitted there for treatment. (In those days they were using Salvarsan, so they had them there for weeks.

DEVRA: Salvarsan. What was the procedure?

DUNN: They were treating them there with heavy metals.

DEVRA: This was before we had antibiotics.

DUNN: Cytology was something that we were getting interested in. We were doing cytology on the females brought in for treatment and that was the first cytology ever, I guess, done by the Service.

Well, Black, who is a pathologist in New York, had a Black Test; and there was a Huggins Test and Penn's Test. Huggins had a good chemist with him, Jensen. The Huggins test looked promising, but it was not specific for cancer.

DEVRA: Did the Federal government invest money in these tests or did they invest only in, for example, the development of this report on what the criteria should be?

DUNN: I think the only thing that they funded at all was the evaluation labs for whatever they were going to do.

DEVRA: These three labs...

DUNN: There were four. I can't remember the guy who was...

DEVRA: Well, I will ask tomorrow, because this was during Kaiser's era, and it was through his Field Studies and Investigations program. What other kinds of things were you doing during that time?

DUNN: The big thing at that same time was the cancer detection centers. I was completely turned off by this because it just didn't have any possibility as I could see it. We didn't have anything that specific that we could offer. Physicians were having to do all the examinations. It's just one of these things. You can't use physician time for screening, it's just too expensive and too boring for the physician eventually.

DEVRA: Is that what the pathologists were saying?

DUNN: That is what I was saying. This was right in the crest about that time in terms of promotion. The Strang Clinic at Memorial had an examination procedure. I guess they still have theirs...

DEVRA: Multiphasic...

DUNN: These were being promoted. The conference in Portland, Miami dealt with that. L. Henry Garland, who was a San Francisco radiologist, was there. I remember somebody from North Carolina was describing how they were running their programs. Garland got up and said, "It was the most flagrant example of state medicine that he had ever heard of." You know he was a real conservative. So he just tore into it for what it was worth.

Diagnostic tests came up. In the discussion, it was kind of a garbled discussion, we had Cuyler Hammond accusing me of being over-demanding in terms of performance characteristics of the tests.

DEVRA: This is the diagnostic test now? Not the detection tests...

DUNN: I didn't mention the University of Kansas, did I. That's the other, Bob Stowell, who is now at Davis. That was another place. I guess there were five places. Lippincott, Stowell particularly, and Sprunt with his Penn test did a fair amount of work with the different tests to the point where they would say, "Forget it, there is just no possibility."

DEVRA: Do you suppose people's expectations, laymen's expectations, were really being aroused by this, or was there not enough activity and publicity involved?

DUNN: There was a fair amount of publicity. The Huggins test, in one of the ACS fund-raising time periods, was being cited as a great development, with all

kinds of potential. They never were used in a real mainstream clinic. They were being tested all the time by people who were proposing them, but on the wrong kind of subject material.

DEVRA: With respect to cancer detection, your concern was the conventional cancer or any kind of disease detection was a physician time-consuming phenomenon. Therefore, it wasn't necessarily cost-effective or organized. Were there any notions at that time about how to organize detection services that could be more cost-effective?

DUNN: Well, people like Garland were looking at these things with horror, saying that the place for this to be done was in every physician's office. Every physician's office should be a cancer detection center. We just don't have the kind of examination procedures that made the center sound like anything that would be productive. They published some phenomenal things, out of the one in Minnesota, which I don't believe, in terms of breast cancer, etc.

DEVRA: About how much they were picking up?

DUNN: And the end results of cases that they picked up. What was needed was somebody looking at the data objectively to see just what the reasons for these phenomenal results were. The breast self-examination was being promoted with films. Of course, this is another example of a thing that so often happens. It sounds like a good idea and let's promote it and show women these films and tell them that they ought to be doing this, but nobody ever evaluated this thing.

DEVRA: Except with respect to the number of people who saw the film. I have seen data on that.

DUNN: Well, that part yes, what does that tell you. (Edward) Cohart, who I think was from Connecticut, wrote a paper on the follow-up of these showings to see what women did. He had some evidence showing that there were some that did continue...

DEVRA: They blitzed that film throughout the State of Iowa. They did a test in the State of Iowa and they estimated some staggering number of women had seen that film, but they didn't follow it up 15 or 20 years later to see how many still did breast self-examination.

DUNN: Or even the next year or the year after!

DEVRA: They didn't even do that.

DUNN: Cohart, I know, is the only person that attempted to see what happened in the immediate future after women had been exposed to this thing.

DEVRA: Did he do that all over the country, or just in Connecticut?

DUNN: Just in Connecticut. For the last 20 years, I have been trying to interest people in seeing what breast self-examination can do, because here is the simplest thing that we can possibly promote.

DEVRA: The ACS has been promoting this for years; they have had kits to teach people for years about this.

DUNN: When Cancer Control became part of the current cancer program, I did go out and look at a lot of charts to see what size tumors were coming in. They weren't small. They were four cm. and bigger. So, they were palpable long before they were operated on. One thought was, Let's see what size tumor women are coming in with. Then, the ACS could come in and do a really thorough program of education. Then we return, doing a survey of people to see whether or not the education "took," and we could look at the tumor size again to see if there was any difference. Eventually, we could say whether or not there has been any difference, with respect to survival. But, you know they are into mammography now. So that's old hat.

Hildebrand, who is in ACS Health Education, and myself had a number of meetings years ago to try and promote something like this. Dr. Fasal and I sent in a proposal to Cancer Control, when they were getting all this money.

DEVRA: Did you get any response?

DUNN: No, I didn't get a ripple. "We are doing mammography," I was told.

DEVRA: Yes, they are really hot on that now.

DUNN: The Mammography Committee that they had years ago to look at various things never met but once, I guess, at the time that HIP was being cranked up to do this evaluation. I got interested in how do you decide if you are doing any good or not with mammography? How do you evaluate those kind of data? I wrote one of my unpublished papers on this. You can't do it by looking at survival. You can, but it's complicated, and it will take a long time, because you are advancing time and your effect on survival is based on clinical disease, so you have to set this back some, an average duration, and plot it from there. It might take years before these two curves overlap and you finally decide that this is plateauing at a higher level than that one. In fact, I manufactured some data.

DEVRA: To test this out? Start that one again, Dr. Dunn. I think that is a fascinating concept.

DUNN: Well, you know, the epidemiological triad is an agent, the environment, and a host produces infectious disease. The host factor wasn't really all that important to them if they had an adequate exposure but didn't get the disease; this person had some kind of resistance. There were only a few of them so you didn't have to really worry so much about that role. In cancer where we have few agents, not too many, and we have a lot of difference in environment, and we have a host, which we treat pretty much as if they were all alike. But the host is a very important factor in the cancer process.

DEVRA: Mainly because of the host variations in the behavior of the host or typical makeup?

DUNN: No, clinical behavior, enzyme systems. You know, this has been working in cancer of the lung, this enzyme carbonhydroxilayse (AHH), where there is a difference between lung cancer patients and the distribution of high or low levels of this compared to normal people.

DEVRA: We were talking about your career from '48 to '53 when you were with Field Studies. There was a handful of people and a handful of rather defined program activities going on; they were supporting clinical traineeships, they were supporting monies for medical schools to have cancer coordinators, then to increase the amount of cancer education at the medical school level and dental schools and so on. But you and how many other people, and who were they, were involved really in the epidemiologic search and perhaps the search for cancer control mechanisms---cancer diagnosis, cancer detection, and other means and tools?

DUNN: Well, Sandy Gilliam was interested in epidemiology strictly. I was pretty much working by myself in this cancer diagnostic area, with consultation by administrative people. Cytology was being done at Hot Springs and we were beginning to see how we might use this as a more routine screening procedure.

DEVRA: Now, by this time however, Dr. Pap's paper has been published and there has been some interest in cervical cytology as a mass screening technique?

DUNN: Well, when Dr. Pap was doing this at Columbia, Traut's comment to his resident staff was "that crazy Greek who thought he could tell cancer by looking at the cell." Some of the resident staff were taking smears and giving them to Pap and he was taking them home and staining them and reporting back to them.

DEVRA: They were doing all this behind Traut's back?

DUNN: Yes, without Traut knowing about it.

DEVRA: This was in when? In the 40s?

DUNN: This must have been in the early 40s or late 30s. I have heard so many people talk who were working with Traut at that time, saying these things, and finally they showed some of these to Traut, saying, "Dr. Pap has something." Traut began to take some interest and pretty soon he became enthusiastic, of course. Then he came out here and he was a real promoter.

DEVRA: He came out here in the late 40s?

DUNN: He must have come out about the same time that I did (1953). I remember, maybe it was a little while before. I remember he was talking at the APHA meeting here in San Francisco, whenever that might have been.

DEVRA: Where did you go first?

DUNN: Right here?

DEVRA: Right here to the State Health Department. You were assigned here?

DUNN: Well, at that time Les was on the Presidential Commission. Nancy and I had been married out here and always loved this area. By this time, we were wishing that we could get back. I was getting pretty much fed up with Washington in terms of the concern of everybody jockeying for position and that sort of thing. The climate was nothing that I could get used to. My wife had ragweed sensitivity and she was getting asthma. We decided that we would certainly get back to California. So I saw Les at that time and told him that I would like very much to come back out. If he had any notion of wanting anybody assigned out here from the Service, Heller was a friend of mine.

DEVRA: Rod Heller was a friend of yours?

DUNN: Yes, we started out in the Marine Hospital together. He was Director of NCI. It got a little erratic. Every year he got requests to send people to California. He remembered that I was interested. I guess Les just wrote a letter asking him if I could be assigned. That is how I got out here.

DEVRA: So you came here in 1953, assigned by the Public Health Service Field Studies Program, you were lent by them, as it were. And what was it that you were going to do?

DUNN: Epidemiology. About the same time that we got interested in diagnostic tests, we also got into cytology. In going to Hot Springs, I went through Memphis and changed planes there. Sprunt had one of the labs for evaluating these diagnostic tests. Erickson, who was on his staff, had done some work in North Carolina on carcinoma in situ. On very careful examination of these lesions and their extent, and what they saw by doing dissections, for example Stoddard, he published some reports on their findings, the field effects, and how multiple lesions could be found breaking through, invading, if you look carefully. Erickson was interested in that particular lesion, had experience with it. We were talking about the possibility of setting up a screening program.

DEVRA: In that community?

DUNN: Well, I was talking with them about would they be interested in doing it. And here is something I wrote up in 1956.

DEVRA: This was the Proposed Cytologic Study of the Adult Female Population of Memphis and Shelby County, Tennessee for Genital Tract Cancer, a Case-Finding Study. Now this was the proposal for doing such a study. What happened and to whom did you propose this?

DUNN: Well, the Cancer Control Branch had its own Advisory Committee at that time. Then there was the Cancer Council (NACC). I wrote this for the Committee and the Council as a proposal for seeing whether we could do this, and also to get data to investigate what in situ was or is, because the pathologists were very reluctant to take an interest in carcinoma in situ. Cytology was something that they didn't want to have any part of.

DEVRA: Why?

DUNN: Because you can't diagnose cancer from the cells. You have got to have tissue. They would say you couldn't diagnose with just cells.

DEVRA: And everybody believed that.

DUNN: Right. Not having been trained in cytology themselves, they had a reason for not wanting to believe---because they didn't have the training to be able to function that way. I heard pathologists say, "You give a pathologist three months training, and at least he will know enough not to damage cytology, the technique."

The cytology commotion was not just by pathologists; Ayers was a gynecologist, Carter was a biologist at Duke who learned to do cytology and did a very creditable

job, and Neiberg was at the University of Georgia at Augusta.

DEVRA: Also a gynecologist?

DUNN: I don't know what he was. He was foreign trained, spent time in England, and then he got into it in Augusta. Pund was the pathologist there at that time whose interest was in cervical cancer. He had his own idea of its origin, the reserved cells, as they called them, dormant little cells lying underneath the epithelium and so forth. These are people who really promoted cytology.

DEVRA: This is in the late 40s, after Pap's and Traut's paper. Was it customary for staff to make proposals like this for review by the National Advisory Cancer Council and by your own Cancer Control Advisory Committee? Was that common?

DUNN: I don't know. There was a great restriction on NCI staff encouraging somebody putting in a proposal for something.

DEVRA: From the outside, that is. What about putting it in from the inside? For instance, did you write several other proposals while you you were there, or was this the only one?

DUNN: No. For diagnostic testing, it evolved as something that we were in to. I didn't write it up as a proposal that it was only the criteria that seemed reasonable for evaluation. But this was not only to propose a screening procedure but also to get data that would answer the question, what is carcinoma in situ?

DEVRA: You proposed this in 1950. Did you present this yourself to the National Advisory Cancer Council?

DUNN: I can't remember. I probably did to the Committee.

DEVRA: Do you remember much about the response?

DUNN: Well, it was favorable, as far as I can remember. We were talking to Sprunt and people that he had there about getting into it. Diebert had somebody who was in Seattle, Washington who was within the Service, who also expressed an interest, so at one time he was trying to move the project to Seattle, but by that time we were in too far already as far as I was concerned.

DEVRA: You wanted to do it only with Sprunt because you knew his work and their capabilities?

DUNN: He's a good organizer.

DEVRA: Is he well-respected in the organized medicine in Memphis?

DUNN: Yes.

DEVRA: Was he on the faculty at the medical school there, too?

DUNN: Well, he must be retired now. I haven't been in touch with him for years...

DEVRA: At that time, was he on the faculty of the medical school?

DUNN: Oh, yes. He was head of what they called a Cancer Institute. When I first went there, they were in an old rickety building. He got the money together to take over this 14-story building.

DEVRA: This was in Memphis, the University of Tennessee.

DUNN: Sprunt was a real brick-and-mortar man. He was real savvy politically. He knew who was on the various study groups, so he would gear it in such a way that that's where it would end up.

DEVRA: But did you consider him a reputable person to engage in this?

DUNN: Particularly for this kind of operation. Because this meant bringing something into the medical community and getting them to accept it.

DEVRA: Something that they may not have wanted?

DUNN: Right. Well, we had our problems.

DEVRA: Did you go with Sprunt, for example, when he would organize meetings?

DUNN: We went to the Medical Society and all pathology groups. You know he was real "smart." When they had a meeting and he had something coming up, he would see to it that all his staff got there, so he wouldn't get outvoted. All the other pathologists didn't come necessarily knowing what was going to happen.

DEVRA: Did he have that problem trying to promote this cytologic study?...

DUNN: Yes, some of the pathologists were getting to be very antagonistic.

DEVRA: Did some say that they wouldn't participate?

DUNN: Well, we didn't care whether they did or not. We were offering a cytology service to all physicians who wanted to take smears and have them looked at. They had to get into cytology just for self-protection.

DEVRA: The cost for examining the smears was going to be borne by this Federal grant. It was a grant, not a contract?

DUNN: Well, we actually hired the people. The staff of the cytology lab---their end of it was the pathology end of it---to supervise the laboratory. We put people there.

DEVRA: Did you train the people as cytotechnicians?

DUNN: Yes, not personally, but we had Ruth Graham when we started the Memphis project. We had certain restrictions we were going to run into. We didn't want to have physicians doing all this. We wanted technicians. We had had a nurse at Hot Springs who did all the cytology specimen taking, actually examined the cervix, actually learned when to call somebody to say, "Hey, there is something here that you need to look at."

DEVRA: Why didn't you want the physicians to do any of this? Because you knew that non-physicians could do it?

DUNN: They could do it. A lot of the population wouldn't have the means to go to a physician to have this done, so we had to have clinic facilities for those people.

DEVRA: Available for indigents and for private patients as well.

DUNN: Right. John Gaston Hospital was for the poor people. In cytology, the pool specimen is simpler to take.

DEVRA: In those days, was there a black hospital in Memphis?

DUNN: John Gaston, that was their hospital. It might have been both black and white, I can't remember.

DEVRA: And they participated in this project?

DUNN: Yes. You know, the girls, the people sitting on the benches waiting to go to clinics---we got them talking about coming in to have a smear taken. Sprunt would use the waiting room as a recruiting place to get volunteer women patients.

DEVRA: So, any woman that was willing could be screened? Is that right?

DUNN: Dr. Pap's technique was just a rubber bulb and a glass aspiration tube. That is what we were using, because we knew the physicians would not allow a nurse even, and certainly not a technician, to use a speculum. This is a medical procedure that they were not about to turn over to any non-medical people. That is how we got into the vaginal cancer specimen as a kind of specimen to use. Another interesting person was Ruth Graham, a technician working with Joe Meigs in Boston (a well-thought-of gynecologist). She read all of her specimens that she took. She was just a technician.

DEVRA: She also took vaginal pools?

DUNN: We had her come down and train the people we were recruiting. These were not biologically trained people. You have to remember that we had music majors, all kinds of people that were brought in to make up the cytology lab group; it's the kind of a person it is and the kind of intelligence she has that makes a good technician, not a lot of prior experience.

DEVRA: These were all women?

DUNN: Yes, I think they were all women. There was one man because he finally ended up at NIH.

DEVRA: It's interesting to me that the choice of taking a vaginal pool was really determined as a part of what we might call "organized medical politics and professionalism." Pathologists didn't want to have to do it themselves, it was time-consuming and boring. And yet the technology existed. So they had to try and figure out a way to harness that technology using cheaper help who wouldn't mind being bored. They weren't ready to relinquish what they considered a medical procedure, that is taking the smears, is that right, so the next best thing was to do

these vaginal pools. Now what about the quality of material that you get from the vaginal pool as compared to smears taken by a pathologist, even then. Was the quality of that material just as high?

DUNN: Well, it is a different kind of smear. Most labs that have not had any experience with it, don't like it. I mean the scraping is much better, more cells and so forth, but, as I said, Ruth Graham has done all her work with the pool. We were forced to do it because of the restriction of who took the smears and how. As there were fewer cells, they had to be looked at more carefully, When. . . there was an inadequate smear or something that was suspicious, they would ask for something from the cervix.

DEVRA: And they would get a doctor to come and do it? They would have to get the patient to come back?

DUNN: After a while, and this didn't happen too often, the nurse having done this a long time at Hot Springs would do some of these herself, without telling anybody.

DEVRA: Illegally?

DUNN: Yes, she couldn't get them . . . all the time either. So she finally taught some of her better technicians to also use the speculum.

DEVRA: Did the pathologist know that this was going on?

DUNN: No, and a frequent comment of the women was, "Gee, I had this by a doctor and it was always very uncomfortable, but you know the nurse and technicians were always very careful not to make it discomforting."

DEVRA: All this time the pathologists didn't realize that some of the nurses and technicians were doing this and getting better material presumably. It occurs to me that perhaps this study could never have been done unless you were able to figure out how to combine the technology, that is, doing cytology in some form, producing acceptable material, with catering to the mores and practice of the practicing pathology community. You had Sprunt and you had a good relationship with him on the basis of something else, on the basis of these cancer diagnostic tests. If you couldn't have done it with Sprunt, do you think you still would have been interested in doing this? Where else might you have done this study?

DUNN: I don't know of anywhere else. I had a lot of contact with the group there in Memphis.

DEVRA: They trusted you, obviously.

DUNN: Yes, but like I said, "There are three people that have always been involved in these things that we were doing, there was Sprunt and I and Jack Daniels." Because we ended up going back to his place at night and over Jack Daniels we would decide what to do next.

DEVRA: I see, so he was your comforter? But, it is interesting to me that you made an adaptation, and that technologically it was possible to make the adaptation. I was curious, did Pap come along with the bulb?

DUNN: Yes, it was shown in his '43 book.

DEVRA: That was the technique that they were using.

DUNN: Right, in fact, Pap's idea was that women could learn to take their own smears.

DEVRA: Yes, and there was a movement for a while in this country for it, there were kits actually made available for women to do this.

DUNN: Well, this one that Dr. Davies of Johns Hopkins developed, that was a fluid that you injected--I guess that is still being used by some.

DEVRA: The Cancer Control Program when Lew Robbins was there tested that kit.

DUNN: One thing I never felt was too objective about the testing of this, was because the labs that were doing this had always used the scraping. This was developed in Denmark at first. They also used the scraping technique. All was very comparable. When it was tested here, it was used by the labs that had no experience with the vaginal pool specimen, which is much more similar to the aspiration technique than it is to the scraping. They finally did do this in Memphis and I think they found it somewhat deficient.

DEVRA: How many years did the Memphis study go on?

DUNN: It was still going on in the early 60s. Let's see, when I came out here in 1953, the next year I guess I went to Colorado Plateau riding around in caravans examining uranium miners, which I felt a stupid thing to do...

DEVRA: So somebody took over your Memphis work when you left the NCI?

DUNN: Yes.

DEVRA: Would you say the NCI achieved its epidemiologic objectives in running that Shelby County study?

DUNN: Well, what I was saying was, that when I got out here, after this session in theColorado Plateau, they were trying to get me to go to Memphis, but if I was going to go to Memphis, I wouldn't have come here in the first place.

DEVRA: They wanted you to come to Memphis full-time, be assigned to Memphis.

DUNN: To work with the project there.

DEVRA: To analyse the data and that kind of thing?

DUNN: I was able to remain here, by having some correspondence arrive at Heller's desk saying that I had just arrived here and I wasn't planning on moving. Anyway, I managed to survive. Then I retired, in 1960, from the Public Health Service. Sprunt immediately called me and asked me to come to Memphis to look at the data. I went there over the next couple of years.

DEVRA: You actually lived there?

DUNN: No, I would ask what kind of data they thought we needed. Dr. Kashgarian was working the program at that time. He was a pretty good machine man, I mean

he had a fair working knowledge of data and so forth, getting it on cards. I was asking him to get certain data. Then I wrote up a report in 1962 (which I have copies of). It was used by Kashgarian as basis for a paper he wrote with me, shortly after that. But that data gave us some of the information that we were seeking when we set the thing up in the first place.

DEVRA: But it took all that time, from 1950 when this idea was introduced to the Advisory Cancer Council?

DUNN: We started cranking up about 1952.

DEVRA: Actually do you go to Shelby County to begin training people, organize the physicians, organize the techniques for picking up the patients in the first place? From 1952 to 1962, or at least until 1960, this program went on in its regular pace with NCI support presumably. Before that time, or after, did the programs develop in San Diego and other places in which you were also involved?

DUNN: Well, when I came out here I began to see something of the Gyn-Ob group in San Diego.

DEVRA: Dr. Martin is in this group?

DUNN: A group of gynecologists all practiced together. It's kind of a strange arrangement. The cytology lab was their lab, the one all of them used. They were very enthusiastic about cytology, so they were making it available to anybody else that wanted to send slides there.

DEVRA: Only cervical cytology, not lung...

DUNN: Yes, cervical. This was very upsetting, they were doing slides for $3.50. This low cost was really upsetting to the pathology community in the State.

DEVRA: And what did they want to be charged for that kind of a procedure?

DUNN: I think Dave Wood at that time was charging $10 for this and re-examining slides if anything was unsatisfactory. But none of the pathologists wanted to do any mass screening. They didn't want a big staff of technicians to supervise, but they didn't want anybody else to do it either.

DEVRA: How did you become familiar with Dr. Martin's work?

DUNN: I had seen some of his publications.

DEVRA: They were already published by the time that you got here in 1953?

DUNN: Yes, I became aware of them. One of the gynecologists (Dr. Slate) was a cytologist, he had worked in New Orleans before he came to San Diego. That was another bad aspect of it, as far as pathologists were concerned; he was a gynecologist passing judgment on slides. There was a lot of grumbling about that lab. They were getting slides from all over, Alaska, up and down the West Coast, and as far east I guess, as the Middle West.

DEVRA: They did it by informing gynecologists that they had this capability?

DUNN: I don't think they ever really promoted themselves. I think it was a word-of-mouth sort of thing, papers, exhibits, meetings.

DEVRA: So they became a major cytology center. This is in the 50s, middle 50s, (I have a set of questions that Leon Ellwein, who is working on our cytology chapter, asked me to ask you, and these all are beginning to fit in now.) He wanted to know whether you joined the San Diego program as what he calls the NCI interface? Were you, for example, reassigned down there?

DUNN: No, I was always from here.

DEVRA: You were always from here, Berkeley. You had been assigned here to the State Health Department as an epidemiologist and you went to San Diego as a consultant?

DUNN: Well, I went down first just to talk with them, to see what they were doing, where they were getting their material from, etc.

DEVRA: And you familiarized them with the fact that you had been at Shelby County?

DUNN: Yes, in fact Erickson came out there and talked to the San Diego Medical Society one time.

DEVRA: How did that go over?

DUNN: Very well. You know, Erickson was talking about the pathology end of things and since dysplasia really hadn't been "invented" yet at that time, the argument was "Is this carcinoma in situ or not?" If it wasn't carcinoma in situ, it's like saying it's cancer or it isn't cancer. Well, today, it may not be what you call cancer, but it is going to become a cancer. This is something that pathologists never really thought about in dynamic terms. They always think in terms of the moment in time. It is this, or it's not that. So there was a lot of discussion about the point that in situ is just a benign process and that, as Fred Stewart said one time at one of the National Cancer Conferences, cervical cancer comes from carcinoma in situ because where else could it come from? You've got normal epithelium today---tomorrow it may be cancer. It doesn't happen that way.

DEVRA: Did Martin meet with resistance from the medical community, not just from the San Diego pathologists, as you suggested, but those up and down the State?

DUNN: Well, he wasn't the cytologist.

DEVRA: He was the gynecologist.

DUNN: Everybody was a gynecologist in the group. He was the leader.

DEVRA: Did he meet with resistance locally and in the State? Getting this thing going?

DUNN: Well, Wayne Henderson was the business end of the group.

DEVRA: Also a gynecologist?

DUNN: He was really the administrative person, if there was such for the whole gynecological group. He kept them broke most of the time, buying buildings and property. You know, he should have been a businessman because he had a lot of that capability.

DEVRA: He would plow the monies back into real estate for them, even the $3.50 a slide? How early did they start being able to do this service at $3.50 a slide?

DUNN: Well, I think ever since I first knew about it.

DEVRA: 1952-53?

DUNN: Yes.

DEVRA: So you went down to see them shortly after you came here, made a connection with them...

DUNN: They had a chief cytology technician named Meritt. He worked in New Orleans with this other man whose name I can't remember at the moment. They were very experienced people. This man who was the chief cytologist was an excellent person. You know when they review slides they put together whether one said one thing and the other another. Meritt was right about as many times as the gynecologist.

DEVRA: And then did you promote the idea....for example, they didn't try to do the same thing going on at the Shelby Clinic.

DUNN: Yes, they had a lot of data.

DEVRA: Oh, they already collected a lot of data?

DUNN: Yes, so the first paper I wrote was on 30,000 women or something like this. That was all their data.

DEVRA: They had already examined 30,000 women by 1953?

DUNN: Well, 1954-55. Yes, they did a tremendous volume.

DEVRA: And all this time they had no NCI support?

DUNN: No, they never did have any direct support. What happened was, I got interested in what they were doing. They were interested in promoting it throughout the county. The question was how to provide the screening of slides that couldn't be paid for. So, I managed to get two technicians to put into the lab there.

DEVRA: State money or Federal money?

DUNN: Federal money.

DEVRA: Cancer control money?

DUNN: Yes, to examine indigent patients, the County Hospital population and so forth. Of course, that San Diego group is about as conservative as you can imagine. You couldn't even talk about Federal dollars. So I finally got these two technicians set up. The pathologist who was doing the County Hospital pathology decided, well, rather than putting this into that lab, he would do it himself. He wasn't doing it before then, but I mean we forced him into it. We did have the two technicians.

DEVRA: These are cytotechnicians trained by Traut here in San Francisco?

DUNN: No, they were trained by the group in San Diego.

DEVRA: Oh, they trained their own. They had been doing that. But you were going to pay for it now with Federal money?

DUNN: Right. So we had a little surplus of service at that time. A gynecologist in San Jouquin was very much interested in cytology. He had a lot of Mexican population. He heard about it, so rather than teaching him how to use this technique, we decided we would pick up some of the slides from San Jouquin Hospital and have them examined there in San Diego. As a result, we got the impression that the Mexican population had a much higher frequency of carcinoma in situ, which didn't prove to be the case later. We did some tests in Los Angeles, too. By that time, we got the San Diego County Medical Society to set up a cytology registry within the County Medical Society Building.

DEVRA: How did you persuade them to do that?

DUNN: Well, Martin and his people were very well respected among the gynecologists. Martin is the one that really got it set up within the county.

DEVRA: The pathologists were still bucking them a little bit.

DUNN: Yes, but vaguely.

DEVRA: And did you pay for staff out of Federal money to run that registry?

DUNN: It was either the NCI or the ACS.

DEVRA: And that supported this registry?

DUNN: Yes, this ran for three or four years and then it was abandoned. But we got enough additional data out of that to do another paper using San Diego data.

DEVRA: So at no time did the NCI or the Cancer Control Program within it put up money or even offer to put up money for data collection and data analysis or data management.

DUNN: In San Diego? My own services they were paying for.

DEVRA: Right. They were paying for your services, because you were assigned here and still being paid by Cancer Control.

DUNN: I am not sure from where this money came. We had about three people there in the registry. I have a feeling that it was ACS money.

DEVRA: But the initiative did not come from Washington; the initiative was yours, in a sense. You were familiar with this work, you were intrigued with it, you had the experience, so you took the initiative and went down and said, "I have had some experience and I would like to help you." You must have struck it off pretty well with Martin and his colleagues.

DUNN: Yes, I explained to them the statistical questions to be answered and how their data could be used in this way.

DEVRA: Did Martin have any statistical ability himself?

DUNN: No, not really.

DEVRA: And did you offer them any standardized forms similar or identical to the ones being used in Shelby County and possibly Toledo?

DUNN: No, their data came in as minimum information, really, race, age...

DEVRA: That's before you got involved. Now, after you got involved?

DUNN: Most of the data I looked at was collected before I got involved. It was accumulated over a period of years. We had a report form for the registry which didn't have anything in terms of other characteristics. . .

DEVRA: It wasn't similar to forms being used at Shelby County or Memphis or Toledo?

DUNN: No.

DEVRA: So you weren't really collecting the same information as from these three places?

DUNN: But they had data that I was interested in. The reason wasn't epidemiological. It was a prevalence and incidence in each distribution of specific rates that would be used to try to see what carcinoma in situ was. This is a paper I wrote before there was any data to describe how these data would be used to answer this question.

DEVRA: This is a 1953 reprint from Cancer, called The Relationship Between Carcinoma In Situ And Invasive Cervical Carcinoma. This is really the problem that you were interested in. (And, you are right, by then, it is called the Cancer Control Branch.) This was published just about the time that you left to come out here to California.

DUNN: Yes, in fact, I think I submitted it just before I left.

DEVRA: So your interest in working with the San Diego people was that they had all this marvelous data, maybe not so marvelous, but at least they had already examined 30,000 people on exactly the problem that you were interested in. So you must have devoted a great deal of your energies the first few years that you were here to working with the San Diego group.

DUNN: Yes, I worked with them quite closely and frequently.

DEVRA: You were based here, but you would go down there to work with them. Do you recall what the influence was of the ACS nationally, I would have to say, in furthering widespread application of cervical cancer screening? For example, did they develop, if you are familiar with this, possibilities for

cytotechnology training? Did they put up some money for that or did they arrange for it or promote it? Did ACS promote the concept of mass cervical cytology screening?

DUNN: Well, I think they promoted it, in a sense. This is a technique where you can pick up unsuspected cancer. The ACS is always fairly well dominated by older physicians who were conservative in how
they practiced medicine and how they regarded the physician role and the role of non-physician.

DEVRA: Have you been active in the ACS California Division?

DUNN: Well, I used to belong to their Educational Committee.

DEVRA: Did you find those characteristics that you just mentioned true here in the California Division, John Cline, Henry Garland, and such people. That they believed in themselves?

DUNN: Yes, the clinician's view of doing things like the sigmoidoscopic exam routinely, which I don't think could be promoted too well. Most doctors don't have an instrument. They would be better off if they didn't practice on somebody. So at the Cancer Society, we talked about this. "What are we going to talk about this year? Well, let's get something new."

My feeling was, we never really evaluated any of these things, what we should be doing and we don't do. Breast self-examination is a good example.

DEVRA: How important do you think the cytotechnology training programs were?

DUNN: I think they are very important. Cancer Control did most of that, supporting centers. There was one out here with Dave Wood and Traut. But it was an extremely important part in having the capability of launching this type of program, because organized medicine and pathologists were not willing to advocate mass screening of normal people.

DEVRA: We will try to keep covering this cervical cytology topic. You talked about the significance of the training programs. The next question is, "What do you think slowed the application of cervical cytology to mass screening and what do you think finally promoted it? What were these external forces?"

DUNN: Well, to do mass screening you have to use technicians. A lot of pathologists were not willing to accept the technician's evaluation of slides. There are still those that think a pathologist has to look at all of them. You can't do it that way. You can't have pathologists looking at every slide and make it cost-effective. So you have to have a lab, hopefully with a chief technician who is very confident and a staff of screen technicians who carry on most of the culling out of the slides that have to be evaluated. This is where the pathologist has to use his expertise. Of course, some of these very good cytologists, even with pathologists doing spot-checks, get to be very competent. But, the pathologist has to sign them out, so he has to look at some of them, you know. The whole need is to have a volume of material and use technicians for primary screening and have a good cytology supervisor technician. That was something that the pathologists as a whole were not interested in. It had a good deal to do with them not getting it promoted as far as the ACS was con-

cerned. But ACS did emphasize the use of this procedure in their cancer detection centers.

DEVRA: They promoted this as one of the tests to be done in such centers that were being supported. I gather cervical cytology was being conducted, but who was actually doing the work?

DUNN: Well, most of the centers were staffed by physicians. That was the other point. You needed a physician, not just a physician, but a gynecologist, because you had to do a pelvic examination at the same time. There's an interesting paper by Dr. Hulka, of the Pittsburgh, Allegheny County program. She was supervising a cytology screening program there and using gynecologists in all cases to do the examination and do the smears, mainly because of this insistence that somebody ought to be examining them physically as well as taking the smears. Well, out of all the cases they picked up in this program, none, not one cancer was found that wasn't found by cytology, as it were. And, you know, when you do a physical examination to decide whether you have a corpus cancer or ovary, it isn't early cancer anyway when you find it, because of the enlargement. Her idea of trying to do these other things along with cervical cytology, to me, didn't really have that much payoff.

It was the same way in Memphis when we first set up the program. You'd talk to physicians about it, they'd say, "You have women laying on their backs and you aren't even going to examine their breasts?" Well, we weren't going to examine their breasts because if we got into that, that would be even a bigger program than the cytology program and we would not get any answers to either one.

DEVRA: Would they have permitted nurses to examine the breasts, or technicians?

DUNN: Well, probably not, that would be another hassle. Now Martin trained a nurse to do pelvics down in the County Hospital in San Diego.

DEVRA: Only in the County Hospital, not in private practice.

DUNN: Right.

DEVRA: In the private practice, it had to be done by physicians?

DUNN: Right. She did a great job. Why shouldn't she? If somebody teaches her what you are supposed to feel. She's got fingers and sensation, she can learn when the pelvic examination is negative, or that there is something that somebody ought to look at besides herself. She knows when there is something wrong. In fact she picked up a rectal cancer one time, not from what she felt, but because she looked at the cervix. It was so anemic-looking, she knew the woman was bleeding somewhere but not in her genital ortans. So she got hold of the doctor and they started examining her further. It showed that she had a rectal cancer.

DEVRA: That is very interesting. In the private sector of course, it wasn't appropriate and of course you couldn't charge a fee for a nurse doing the examination. But it was allright at the County Hospital. (That leads to the next question.) "Why do you think the use of colposcopy in the U. S. has been so slow?" We are only now starting to do colposcopy. Can you relate that in part

to the cervical cancer mass screening history?

DUNN: Well, colposcopy was a European technology before it was used here. It is not a substitute for screening. You can teach a technician to use a colposcope too.

DEVRA: But not to do a cone biopsy?

DUNN: You can avoid doing a cone biopsy, because you can select your biopsy if you use the colposcope. You select the area that is most likely to be involved. Duane Townsend teaches technicians to use the colposcope.

DEVRA: In the County Hospital in Los Angeles. Do you think it was somewhat the same principle operating: It took a physician to do colposcopy, and the results from colposcopy might not be so remarkable, that it was desirable to do it on every patient or every fifth patient or only on suspicious patients. What other kinds of factors do you think led to the fact that it has been revived only recently?

DUNN: There weren't that many people trained in the technology. It was a matter of having to learn it if you are going to use it and then you need a fair amount of time to really evaluate what you have seen. It is not something that you would use as a screening procedure. It takes time...

DEVRA: It comes after screening though. Is it a natural follow-up to screening, in order to identify the positives?

DUNN: As I understand its capability, you can actually make a pretty close decision whether you have carcinoma in situ or not and pick the area of biopsy that is important to look at. It has a definite place. Duane Townsend is using it pretty much in this fashion.

DEVRA: If we were just now at the point where the use of cervical cytology as a mass screening device was at least professionally acceptable, what direction do you think we would be taking, we meaning the NCI and the ACS and physicians?

How do you think we would go about doing this now? It happened about 20 years ago.

DUNN: With the cancer program as it exists, there is an awful lot of money being pumped into Cancer Control; there would certainly be a lot more support for those who are trying to get something like this going. I think it would go faster.

DEVRA: Do you think we would have more demonstration projects than we have now?

DUNN: Where we would be as satisfied as we are now? We are still arguing whether in situ is a pre-cancer or not. You know, pathologists got to the point that they hated to say carcinoma in situ, because carcinoma has meant one thing to the surgeon---you take it out. The pathologist had a surgical specimen. He wasn't sure whether it was a cancer if it were an in situ lesion.

I say money is one thing that it takes to support things like this. Certainly, at this point in time, there is probably greater acceptance of cytology and what

it is capable of doing, than maybe even I am willing to accept yet. There is nothing we are ever going to do that will be any simpler than cytology. Here you have an area (cervix) that is easily exposed. The simple procedure of taking a smear and using technicians to screen, to sort out the ones that need attention. I can't think of anything that would be simpler and less costly. If we can't make this work, we can forget about anything else, as far as I am concerned.

DEVRA: Do you think we are making it work?

DUNN: Right now I am involved in Alameda County again. HPL did a survey back in '62 and then they did another survey in 1973. We got some more answers to the questions, "What about cytology? Have you ever had it? If so, how many times? When did you start and when was your last one?" Looking at those data, we see the Pap smear extensively used in the pre-menopausal group, mainly because of education, I suspect, and in older women even. But it all tapers off when you get to 60 years and older. There has been an awful lot of cytology done.

Look at the total cancer picture. Over 75% of all cervical cancers are in situ. But if you look at this age distribution, under age 50 is where the predominance of in situ is, as you would expect; over age 50, there are more invasive than in situ cases. And in the Memphis project, women who had been screened at least twice as negative, with this high prevalence in situ rate you have to screen women more than once just to get rid of the missed cases, because you won't know if you are picking up one missed or if it was a new one. So we are looking at those who are in their third or subsequent examinations. There is a proportion who are invasive at that time; in Memphis, this was maybe a fourth of the cases that we picked up. In San Diego it was something like 15%.

DEVRA: After how many exams?

DUNN: After two negatives. Then Dr. Martin and I talked about it a number of times over the years. He did a study not too long ago, looking at all the invasive cases in San Diego and Imperial County. He found that there are some women who had a negative cytology not too long before (a lot of these slides were not available to review, but at least they were called negative) who had invasive clinical disease within a short period of time. I am sure that this is the case, but what proportion of women are like that is the real question.

DEVRA: Are you still studying that?

DUNN: Yes. In Alameda County, now, they still have about 90 invasive cervical cancers a year and they have about 30 cervical cancer deaths a year. What I am interested in is how these invasive cases, cases that weren't seen cytologically or they were seen and they were negative, appear in the clinical records. Some of them are old women in their 80's who haven't seen a doctor in 15 years. This was an absence of cytology. But there are some who have had cytology in the chart about every year or two and it was negative but who had invasive disease. This is one of the things we neglect all of the time. The host (person) is a very important factor in cancer, not only in developing the disease, but how you handle it after you have it. It is not a uniform situation.

DEVRA: We need to study the host a lot more.

DUNN: Right. We have asked almost all the questions that we can ask, epidemiologically, about child-bearing, where you live, how tall you are, how much money you make, and other characteristics. But we haven't done anything yet to separate out people in terms of their physiologic makeup.

DEVRA: You probably have a captive population at Kaiser, people who go through multiphasic screening all the time. Have you thought about doing a study like that?

DUNN: Yes, right now we are trying to look at some Japanese women.

DEVRA: Do you remember Lester's paper, Race and Health? He found better health among the Japanese-Americans and certainly among the Japanese born in Japan, than among Caucasians and Blacks. There must be something different about the way they live. The only thing that he came up with was maybe it was education. But he hadn't looked at anything in their bodies.

DUNN: I gave this paper to a Nutritional Symposium. It's called Cancer Epidemiology in the Populations of the U.S., with Emphasis on Hawaii and California and Japan. That is, the cross-cultural populations. (This title was given to me) I have based it on our data here in the five Bay Area counties. But, the nisei are American-born Japanese women whose upper limit is now about age 60. They are developing nearly as much breast cancer as the Caucasian population.

DEVRA: Is that right? That really is a very challenging question. What's happened to them as hosts and what's happening to them with respect to environmental exposures and behavior practices, child-bearing practices, nursing practices and so on?

DUNN: Much of the nutritional change is conversion to the American diet. There is a gynecological research lab over at Oak Knoll Naval Hospital. Dr. Takaki provided us control patients. He is very interested in nutrition, has been doing some work with obesity, so he wanted to get into a study of the Japanese versus the American diet. They had a lot of naval dependents who were Japanese-born women who had married servicemen. I always felt that these women were instantly acculturating, but Takaki says it's not true; some women were going the other way, preserving the Japanese diet. So we have been trying to find the two ends of this scale of diet, to do some epidemiological studies.

DEVRA: Should we regard the 30 years, let's say between 1945 and 1975, (the beginning popularization of the Pap smear and today) as a long time, better than average, for a technical advance such as that?

DUNN: It was a long time. And they will always have the same procedures, as long as you invade private practice procedures. You know what happened with VD and this sort of thing. Anytime that private physicians feel that their bailiwick is being invaded, they are going to be resistant.

DEVRA: You think that's the principal factor in this delay?

DUNN: Yes, I think so. You know, we used to have women call in here and say, "I have heard about cytology and all, and I would like to have some, but I can't afford it. Can you tell me where I can go?" I have to tell them, "I don't know where you could go."

DEVRA: The county health departments weren't doing it?

DUNN: Alameda County has had one technician for many years, came out of Traut's training program, but she is swamped with material from the Highland Hospital (County). We didn't have any place we could tell people to go to get a Pap smear. You know, poor people have pride, too. Even a Black physician told me, "If they want to come to my office, I will take a smear for them and not charge." But the patient would have to go into his waiting room, which is full of people, and say, "There is nothing wrong with me, but I would like to have a cytology examination." You know she is not going to do that. She has enough pride that she is not going to go anyplace that she ordinarily wouldn't go to. You've got to take it to the community where she is and have her say something about how to organize it.

DEVRA: That really raises what I think is a very interesting issue. That is now, I think women especially, are becoming much more aware of their own bodies and their own health maintenance, things they can do about their health. What do you think that kind of lay awareness will do to accelerate the application of the other technological advances for cancer detection or even for other diseases. Do you think it has some kind of positive influence or could?

DUNN: Yes, I would think so. Here you have an old woman who had a fungating cancer in her vagina. She has not been seen by a doctor for 15 years. If she has any bleeding, she should have it looked at. I hope the oncoming generations are more aware of the things that can happen to them, how to identify them, what to do about them.

DEVRA: Can they be influential in even changing the practice of medicine so that the techniques like this could become more readily available, accessible and maybe not cost so much?

DUNN: Yes, medicine is coming around to the paramedical type unit. It has to be that. You know, medicine has found it perfectly allright to let a patient do self-diagnosis. "Doctor, I've got something wrong with my stomach, it may be an ulcer." The patient already has got some tentative diagnosis before he or she comes to see the doctor. That is allright. But many physicians believe that, for another lay person to be interested in this person to the point of being able to examine, is all wrong. I don't see this.

DEVRA: Do you find your medical colleagues who are clinicians beginning to come around to accept the concept of paraprofessionals being part of their team?

DUNN: Certain of them, but I am not that exposed to that much of acceptance. I don't think the public could make them aware of the fact that not everything that's done for them in terms of checking out their state of health could be done by non-physicians. Blood pressure---why can't anybody take that?

DEVRA: Let's go back to that question that I asked before, maybe now you could answer it a little differently. If we knew now that the Pap smear was good, everybody should have access to it, what do you think we ought to do about promoting it more rapidly? What would we do with organized medicine, with pathologists to get them more readily to recognizing their responsibility in not denying this service to women? What can we do besides holding out the money? (You mentioned that before.) Money was one incentive. But, one of the obstacles obviously

was just plain stiff-neckedness. The pathologists weren't going to yield this to less-trained people. Do you think now we would be able to do this more readily?

DUNN: Well, certainly more readily than 30 years ago. The pathologists we are involved with---like Rodney, a big one in Los Angeles...

DEVRA: He has found the entrepreneurial way to do cancer screening services...

DUNN: There are enough labs now that are interested in handling slides---most health departments can find a lab locally or not too far away where they can send smears for a reasonable price---so there isn't all that difficulty in finding labs that do this.

DEVRA: Let's go back to that question about epidemiological objectives. It is true you left the Cancer Control Program in Washington in '53 and came here. But you were involved in the Shelby County study, with San Diego, and obviously you are familiar with all those demonstrations that the NCI supported in cervical cytology. Do you think the NCI did achieve its epidemiological objectives by mounting these studies or do you think the NCI even had such objectives in the first place?

DUNN: I don't think anyone did but me, in terms of these data. I talked with Sandy Gilliam. I would say, "If you are looking at disease, at various points in development, you have to look at atypical rates in a rectangular (?) population." You give each age group a value of one and then if you add up the atypical rates of one stage of disease over another one, they should be equal. This was so foreign to his way of thinking. I would convince him of it. In another couple or three weeks I would talk with him again, and he would think the concept was screwy again.

DEVRA: You were the only one that really had these ideas. Now, if you were to introduce them to people like Ray Kaiser, who was the boss, how would he react to something like this? Did he think this was something worth pursuing, a respectable activity for Cancer Control to get into?

DUNN: I think so. Ray wasn't, is not, a statistician. He was mainly concerned with people and the administrative part of it.

DEVRA: For instance, when you introduced this proposal that you took to the Advisory Committee and the National Advisory Cancer Council, was he supportive of this? He had seen it, obviously.

DUNN: Yes, I think he was supportive of the idea. At one point he was wondering whether Memphis was the proper place, or Seattle, for example, but in terms of it being something that ought to be done, I think the Committee was in agreement on it. They understood what I was hoping to get out of the studies, but I am not sure if they understood all of it.

DEVRA: Was Mike Shimkin around in those days? Was he in your Branch at that time?

DUNN: No, he was in Biometry. He is one of these people that others react to either one way or the other.

DEVRA: How did he react?

DUNN: Well, at different times, he reacted different ways. I get along all right with him.

DEVRA: You both are roughly the same generation of Public Health Service Officers. You were both commissioned about the same time. You have not had parallel careers, but...

DUNN: Well, he started out in benchwork, then got pulled out for some Russian languate job during the war. I think maybe he promoted this thing at Laguna Honda, because the oncology center wasn't opened yet, and they had to get started recruiting people to put into the center eventually. I remember visiting out here when he was setting that up.

DEVRA: Was he interested in epidemiology in those days?

DUNN: No, I don't think he ever was. When they closed Laguna Honda down, it became a question of what to do with Mike Shimkin. Sandy Gilliam was one who didn't hit it off well with Mike Shimkin; he wasn't about to be in Epidemiology under Mike Shimkin. So they brought Shimkin in as Head of the Biometry Branch. Shimkin is not an epidemiologist in the sense of having done much in the field. He has a general feeling for it, but I think some of the ideas that he gets I'm not all that enthused with myself. I remember when we were involved in cancer diagnostic tests, I met him at a cocktail party over here at the Marine Hospital. I don't remember what the occasion was, but he was there, and I was telling him what I was into. He had a very negative feeling towards the NCI at that time. He felt that the diagnostic testing was a complete waste of time, which I would agree, but I was just trying to prove that it was.

DEVRA: You had to do it scientifically.

DUNN: Mike has really strong feelings one way or the other.

DEVRA: Was he always like that?

DUNN: Yes.

DEVRA: How did you get interested in the relationship between occupation and cancer risk?

DUNN: I began looking at occupations and the correlation of smoking as a factor. A couple of occupations looked suspicious, welding, for one. I guess the welder is the one that really stood out in the statistics. But, the problem that we had in the case control study was we had 80 occupations that were more prevalent in the case group than in the control group.

DEVRA: With respect to lung cancer?

DUNN: But then we have the level of statistical acceptance. Five percent is significant, if you are examining a large number of variables. You are entitled to have one out of twenty to come out as significant. That was part of the problem. But we did go ahead and do a prospective study.

DEVRA: How large a population did that involve?

DUNN: Well, it was 65,000 or so.

DEVRA: All men?

DUNN: Yes. We tried to get 5,000 in each occupational group if we could, because it would take that many to find a two-fold risk in, let's say, five years.

DEVRA: How long did you follow this group?

DUNN: We collected the first data in 1954-55, I think; we last looked at them in '62.

DEVRA: You were looking at mortality, cause of death, to see whether in fact they did have a two-fold or more risk of lung cancer among persons in these vulnerable occupations. What did you find?

DUNN: Well, we didn't find anything very striking the first time around. But, we were trying to get groups of 5,000 each with certain exposures. We had an asbestos union that had only 500 members, so we included plumbers who worked with asbestos, and some of the people who handle it as insulation occasionally. We didn't see anything very striking. But, when we pulled the 500 members of the asbestos union out separately, they did have something.

DEVRA: Was it a much greater risk?

DUNN: Well, some were three- or four-fold. It wasn't a large group, only 500. But they definitely had more lung cancer.

DEVRA: At the time, did you know anything about their cigarette smoking practices?

DUNN: Yes, we had cigarette information so we could adjust for that. In fact, we assigned cases in that group, as I remember, on a basis of their cigarette smoking practices; then the residual had to be explained some other way. That's how we identified there was an excess related to asbestos, not related to smoking, though. Some other groups had some increase, but something less than two-fold.

DEVRA: Was this strictly on the East Bay?

DUNN: No, Hoagie (Mr. Lemar Hoaglin) did most of the interviewing for that study. No, wait a minute, he did the interviewing for the ILWU study, but he also did most of the recruiting of this population. He contacted all of the unions. Maybe they were all Bay Area unions, the advantage being that the Bay Area unions are craft; down South (LA), they are industrial type, so you don't have any concentration of craft occupations in those unions. I think practically all of those we collected were in this area.

DEVRA: And, when you followed up on this observation, there was at least a three-fold increase among the asbestos workers. You knew something about their smoking practices. Did this lead to further studies on the correlation between cigarette smoking and disease?

DUNN: Yes, we looked at this population for other diseases as well. (We published a couple of papers with those results.) We had another similar occupational prospective study---no, not occupational study, planned. We needed less funding than for this one. Les (Dr. Breslow) was interested in air pollution, so they urged him to get a study going looking at LA smog, which we did. And we found there wasn't any excess of lung cancer in LA, at that time compared to SF.

DEVRA: This was in the 60s, because these papers came out in 1967, so you were looking at this from 1960-64...

DUNN: Well, that group came out of the American Legion membership. I think we collected them about '57 and followed them, I have forgotten whether it was '64 or something like that. Our thought there was, even though we didn't see any excess in LA, it could be the influx of new people might be diminishing smog's effect. So we decided to identify a long-term resident group. We picked a group of persons who had lived in LA from 1941, or before, and compared them with those living there a shorter period of time. Then SF and San Diego and the rest of the State were done separately. We didn't find any excess lung cancer at that time that we could attribute to smog in LA.

DEVRA: You have really devoted the last 15 years, at least, to epidemiological studies of at least three cancer sites. One is lung, one is breast, and the other is cervix. (And to a lesser extent, smoking correlated with oral cancer.)

What do you think we have learned from the kind of studies that you have engaged in (most of which have been prospective studies, some of which I realize have been retrospective)? What have we really learned about the formation of cancer and about how to intervene early enough to prevent it from causing excess deaths and premature disability?

DUNN: Well, we learned that smoking is one thing you can do without very well and be effective. Probably the most effective thing that can be done you know with the whole cancer problem is to eliminate smoking.

DEVRA: All smoking or just cigarettes?

DUNN: Cigarettes primarily. There is some cancer probably attributable to other methods of smoking, but nothing like cigarettes. And you know the effect is not just lung and bladder cancers, but on cardiovascular disease.

It would be a step forward if we could eliminate cigarette smoking. Many of the other things that we looked at haven't pointed too much to intervention possibilities. They're mostly oriented towards trying to find the kinds of people who are subject to greater risk for various reasons and we have asked, as I said before, almost all the questions that we could ask, without coming out with risk factors strong enough in my mind, to serve a purpose for identifying the highest group to screen. This factor has become very popular in the last few years.

When you talk about the high-risk group for breast cancer, you talk about the women who had late childbirths and who never married; yet, putting all of these together, you really don't have a very big chunk of the female population.

DEVRA: And we are seeing more and more breast cancer, despite this. There might be women who are a higher risk than some others. Could there be some who are a high risk and others who are a "medium" risk?

DUNN: The way I look at it, as a screening procedure, is that unless you base a screening procedure on morphology, you would be attempting to condense most of the cancer within a segment of the population. Then you could screen this population extensively and get at most of your cancer. Well, we don't have those kind of clear risk factors for breast, as we do with smoking and lung cancer.

DEVRA: How can we get at this, assuming that it was practicable, that it was cost-beneficial to get at some of these factors, in order to find a group that was really worth spending the money on screening? Would we have to have more information about their physiologic performance, is that it?

DUNN: Yes, if this is feasible at all, we need to start looking at host characteristics that seem to be associated with the disease. You know, I think all populations are more or less alike. The Japanese in Japan, they look like they have certain excesses and deficiencies. They come here and they look like us, epidemiologically; I think all populations are generally made up of the same degree of heterogeneity over a very broad spectrum.

DEVRA: You are talking about genetic heterogeneity?

DUNN: Yes. Give them the same environmental circumstances of living practices, and so forth, and they all start having cancer about the same rate.

DEVRA: And they start becoming more homogeneous as hosts?

DUNN: You know, you had all these hosts there (Japan) already. In the Japanese circumstances, there are some advantages as to the way they live, how they eat. Bring them here and they change; they adopt our cultural practices. So in these groups there must be a potential population segment that will get cancer. We don't all get certain cancers. You know, a two-pack-a-day smoker is more apt not to get lung cancer than he is to get lung cancer. So there is something about him that is different. We need to look and see if we can identify these differences.

DEVRA: How can we do this?

DUNN: Well, this enzyme thing that we were talking about a little while ago, where you can apparently look at lymphocytes...

DEVRA: This is AHH...

DUNN: Yes. If you had a technology, and technology can be streamlined if people put their mind to it, if you could take young people and identify those that fall into different categories and say, "Look at you, you are this kind of a person, and for you to smoke is extremely hazardous; this other group has a hazard too, but nothing like what you have." Things of that sort, or women who have certain endocrine patterns. For instance, in this Japanese group, if we took a Japanese woman who has been on a Japanese diet and had an endocrine profile and then switched over to an American diet, how long does it take her to readjust her whole endocrine pattern to reflect this new diet? Is this something that is very labile?

DEVRA: Does it take 2 years, 10 years?...

DUNN: It looks as if it must be fairly labile, because women coming from Japan don't develop breast cancer as much as their daughters, but they still have more than they did in Japan. Those who would have escaped in Japan are starting to get cancer here, because of the circumstances that they are now living under.

DEVRA: You are putting a lot of stock, at least for some types of cancers---and we haven't even talked about GI or GU cancers---you are putting a lot of

stock on diet as being perhaps one of the factors which may predispose some people to be at a higher risk of susceptibility to breast cancer, possibly.

DUNN: Well, breast, ovary,corpus in the Japanese women are going up and prostate in the men. Now Japanese men have almost as much occult cancer as we do, in the prostate. And Stemmerman has found, looking at the Japanese in Hawaii, that they have this but they have more activity in these occult lesions than you see in Japan. We see almost as much clinical cancer appearing in Japanese males now here, as in the rest of the population. At least it is going in that direction and fairly rapidly.

DEVRA: Diet is one possibility, sexual practices are another possibility. Could studies be designed which took into account all these variables affecting hosts and then somehow sorted out which ones are really operating to make people more predisposed to certain cancers?

DUNN: Well, you would certainly want to consider all these things that might be factors, changes that take place, adapting to a new cultural pattern, etc., but I suspect that the endocrine pattern is probably influenced a great deal by diet. These three cancers in women are very important. If they are affected by whatever the content of the diet might be that makes them susceptible---again, it isn't everybody---we have to define what kind of a person is identified with that kind of a diet and endocrine pictures that make them susceptible.

DEVRA: Are there other people you know, other epidemiologists who subscribe to this same thesis that you have been advancing to me?

DUNN: I have read here and there some suggestions that we are certain to see in the clinical picture where they are looking for host characteristics
they find that there are cellular differences in terms of adapting to certain kinds of exposures and enzyme systems that emerge to deal with this. I am not sure what all these things are, but it seems to me that we have to start thinking of differences between people in terms of hosts, and not just somebody to look at their height, weight, and anything else. I remember reading a long time ago a book by Roger Williams on enzymatic patterns; he points out that the enzyme pattern is just as distinctive for an individual as his fingerprints.

DEVRA: Is that right?

DUNN: It's so complex that it's just impossible to unravel. We certainly need to try to begin to find out why certain people develop cancer, certain cancers that they didn't develop in another culture.

DEVRA: The last paper that appears on your bibliography, although I suspect that you have had several published since then, was entitled, The Effect of Smoking on the Survival of Patients with Lung Cancer. That's a paper you did with George Linden. I don't have a copy of the paper in front of me, but can you perhaps recall for me what effect smoking practices had with respect to survival?

DUNN: There wasn't any evidence whether the person had been a smoker or not affected survival.

DEVRA: It didn't affect the histologic type?

DUNN: Well, it did that, I am sure. Again, this was actually George's paper, so I shouldn't speak too much on what the findings were. Since we had the data and

the mortality we thought we might as well look and see whether there was any relationship between smoking and disease survival.

DEVRA: Did you look at the treatment also, whether they were treated surgically or with radiation, with both, or with chemotherapy?

DUNN: I don't remember now, I don't think so. I think it was possibly staged. Chemotherapy has really only come into its own fairly recently.

DEVRA: With respect to lung cancer at least. In your estimation, where do you think cancer control efforts should be concentrated in the next quarter of a century? You have been talking about what we have done in the last 30 to 40 years. Where do you think we should be concentrating our efforts in the next quarter of a century?

DUNN: Well, one thing that I think we should do is to evaluate the screening procedures we have adequately. Just what do they do? Then, maybe we can start changing the outcome of the disease.

The HIP study was a good study. There was some evidence that there was some advantage with women 50 years and older. That's where mammography works the best. If that's true, then the next thing is, how are you going to use this technique? You have to get to the point where you accept the idea that technicians can do most of these procedures. They can examine the breast, they can take the x-rays, they can look at the x-ray films, they can pick out those that the radiologists need to make a decision about.

DEVRA: You said something very interesting to me. It reminds me of what the Director of the Division of Cancer Control and Rehabilitation explained to me, the time frame in which that Division is operating. They are doing a lot of "one shot, get in, demonstrate, get out, projects," to recycle the money. Now, if we are really concerned about evaluating whether these screening techniques have any enduring value, and really can have an impact on early detection, early intervention, survival after intervention, then we are obviously going to have to look at people for much longer than, say, three years after you intervened with something like Pap smears or mammography. To your way of thinking, we ought to be putting some of our resources much more into long-term evaluation.

DUNN: HIP---what did they do? Three to five years; you know, that should have gone on continuously. They should have pushed that for at least 10 years. There is an apparent advantage that isn't quite as big as it really looks, because you have this beginning of a prevalent, slow-growing disease. These are the ones that you can salvage. They would be in greater proportion in this initial screening period. Then you can continue on and get the incidence, there is where the real answer is. If the screening procedure is going to be used to find cancer, you have got to use it periodically with some regular frequencies. The decision as to what the frequency should be is---well, there is no way of knowing this. With breast cancer, if you say once a year, you are going to lose 10 percent anyway---those who were negative will have a mass before they get their next examination. So they have to use breast self-examination along with mammography, so that when they do find these suspicious lumps, they are trained before they get around to the mass examination to know that this is it and to have it evaluated.

DEVRA: That has an awful lot to do with the time frame in which the disease mushrooms to the point of being perceptible, detectible. Many cancers have a long, dormant period. Now with cervix cancer, of course, it's a little different story.

DUNN: Well, we think it is.

DEVRA: You have been in this work now for 20 years, and you are still questioning.

DUNN: Well, the average duration, looking at the in situ incidence and fatal disease incidence is about 9 or 10 years, but we don't know that, all we know is what the average is, and what the distribution is---well, we don't know. It's like they might have something like this (draws). We have a little group here who don't have a long in situ period. That is what we are so concerned about now.

DEVRA: Have you looked at Elizabeth Stern's data on dysplasia? You know she is following this group of women with dysplasia or who may develop dysplasia. She has some of the same concepts that you have, I have gathered.

DUNN: In our first screening, we came up with carcinoma in situ like this; it fell off pretty rapidly; then we had invasive prevalence that came in like this...

DEVRA: Starting at a later time. This is what---five years, four years.

DUNN: Yes. What this means is, in situ is coming in rapidly before age 30-35. The incidence is high. One of two things is happening. There is not very much new developing, or what is developing is counteracted by the disappearance of some that are going into invasive.

Then it comes to this period and drops off rapidly. That means that a lot of this is going into invasive. This looks like a time period, around menopause, where a lot of this conversion is taking place.

DEVRA: That would suggest what kind of interactions taking place? Hormonal changes, and, again, enzyme changes?

DUNN: Yes, I would think something that might be interfering with immunologic competence or one thing or another. Then, when we got incidence of carcinoma in situ, we should have known that this is the way it was going to be. It hits its peak about late 20s, then it drops off, so this is what it produces. You've got this rapid increase in incidence which builds up its prevalence; this is dropping off, and this thing kind of plateaus.

Then you have this rapid increase in pre-clinical invasive, which is picking up all these in situs which are converting. We have another small group of cases, I can't remember, I think it's fairly evenly distributed over age, which is invasive when you first find them.

DEVRA: Do they ever blossom into full-blown cancers?

DUNN: Yes, but we didn't see them as having in situ phase.

DEVRA: They just seemed to appear first as invasive.

DUNN: Actually this is the beginning of in situ and the end of in situ; here, the beginning of pre-clinical; and here, is clinical; then dysplasia starts here.

If this is all one disease, take the incidence rates and add them up, this and this should be equal. If this exceeds this, then something has regressed. Dysplasia is in with the in situ cases. At the beginning of the disease, then it is excessive, something has regressed, as dysplasia does.

But, the question is, Do the in situs either go on or stay dormant indefinitely? The data at Memphis would say that this group of invasive that come in here apparently without a long in situ phase and these together would equal this. (Blackboard demonstration) So everything is there. We didn't know very much about dysplasia then, Erickson called them "possible" and "probable" and "basal cell hyperactivity" and all these terms. I think Regan at Cleveland first introduced the use of the word dysplasia.

It's the same thing I was telling you about, with Dr. Hulka in Allegheny County. She had some of the best data that anybody could have, because she had prevalence and incidence of dysplasia and if you looked, everybody has been zeroing in on what dysplasia is; carcinoma in situ has to begin as dysplasia. You have to have a beginning at the bottom where the cells are growing, and then gradually displace the cells above them until it's full thickness and then it is in situ. So there is no question but what dysplasia is the precursor in in situ cancer. But having an excess, what is the rate at which dysplasia appears and disappears. In San Diego, they didn't use the word dysplasia.

DEVRA: What did they use?

DUNN: They used just in situ or nothing.

DEVRA: So they may have been missing some?

DUNN: Well, yes, although one man's serious dysplasias are another man's in situ. It's not all that clearcut when you get very extensive involvement. But they would say, with respect to their class 2 slide which was supposed to be probably due to infection, they had about made up their minds that they really shouldn't be saying this because the physician didn't know what to do with it. So we looked at their women who were class 2 at one screening; the next time around, 10 times as many of them went on to being in situ as came out of the negative and 80% of them regressed. Well, the same thing with Dr. Hulka's data. It looked like the dysplasia was either going to go on fairly soon within two years or was going to regress. I think that dysplasia is the first step in in situ, but frequently it does regress.

DEVRA: And it may come back. It can have an undulating cycle?

DUNN: Right, that's what we don't know.

DEVRA: And maybe the third or fourth time around it really moves into pre-clinical stages.

I wanted you to think back on your remarkable career---you have had a career that spanned over 40 years in which I would say, you really have been engaged

almost entirely (except for that brief time when they made you a dermatologist) in what we would consider classic cancer control. Not necessarily the active part of going out and organizing and running a program, but the epidemiological framework and the scientific inquiry.

DUNN: Yes, I have always been concerned with the possibilities of putting together this picture. It couldn't be done any other way.

DEVRA: That is, the natural history.

DUNN: Right. Now a clinician, his idea of doing this was to take a tissue specimen and if in situ shows carcinoma, then leave it alone and see what happens to it. Well, first of all, it took some tissue to get a slide and say it was carcinoma in situ, so that's no longer there; then the pathologists said, "Well, I am not going to say what it is unless you give me the whole cone."

DEVRA: You really have devoted almost thirty years to trying to trace the natural history of cervical cancer. Is that a true statement? Do you think we are closer to having the natural history defined in a way that can help society cope with this, as a condition, not just having tools like the Pap smear, obviously, which has helped to define it a great deal? But do you think we know a great deal more now about the natural history of this disease?

DUNN: Yes, I do, and lots of the statements made by other people in terms of the frequency of in situ, I don't know where they get their data. I have seen a lot of misinformation, but certainly dysplasia is something that we were not aware of originally as a precursor of an active lesion which could go in more than one direction. How long it took us to do this.

DEVRA: What was the turning point in suspecting that dysplasia behaved this way? Is there any definitive paper or procedure?

DUNN: Dr. Elizabeth Stern used to be at a cancer control clinic. She had some support from NCI and I went down to visit her, a site visit. She was doing some nice work, and she got interested in dysplasia, and found that many of them go on to become in situs.

DEVRA: Is she alone in this work? Or are there other people in this country now that are doing similar things?

DUNN: Well, Richart in New York developed models and he has looked at dysplasia. There hasn't been too much in terms of trying to decide this on some of the grounds other than the individual patient and her lesion, and when it becomes something else and when it disappears. That's the difficulty with any of these other methodologies where you identify a lesion and leave it alone.

You know, then you come into a morality and a legal question. Now, Nieberg, when he was down at Augusta, was running a study over in Milledgeville, a mental institution near Atlanta. What this study was going to do was, he could see these women patients when he wanted to, pick up some carcinoma in situ, and leave them alone and see what happened.

DEVRA: Leave them alone? How could he legally leave them alone?

DUNN: Well, he would have to do cytology fairly frequently to see whether or not they were converting...

DEVRA: If they were, then would he...?

DUNN: That's what happened to some Norwegians, too. Some Scandinavian countries have done this sort of thing. They'd say, "These cells were a little too dangerous to leave alone but we'll have to do something with that one, but this one looks a little less aggressive." They'd take the whole cross section. You limited yourself to the more benign-looking things and when it starts going bad, you have got to do something, because they can't afford to let it get out of hand. There is just no way to answer this question in that way.

DEVRA: Nieberg was an epidemiologist?

DUNN: No, he was a cytologist.

DEVRA: At Emory?

DUNN: No, at the Medical College of Georgia. He was the one who was heading up this symposium. He had a little study in Floyd County in Georgia that was doing some general screening. He has his own airplane. Everytime I was going to see him, he wanted me to fly over to Floyd County with him and look at his project. I had heard about some of his escapades and narrow escapes from previous flying, so I always managed to avoid it. Driving in the car with him was enough for me.

DEVRA: Is he your age?

DUNN: About that, I suspect. I think he is a little younger.

DEVRA: Well, do you have any other thoughts on cancer control that you would like to share with us now?

DUNN: As I say, we jumped into these things and start using them before we really know.

The 27 breast cancer centers---I don't think you are really going to find many answers. I suspect there will be enough differences. They weren't really set up in the first place to do evaluation, they are service-oriented. They will have a group of women that have been looked at more than once and they will tell us whatever will come out of it. They don't really turn anybody away. You know they had some suspicion that there might be something wrong. You see some of the rates and it had to be this way.

DEVRA: How long are they going to follow these women?

DUNN: Five years, I think, and then they are going to continue another five years of just follow-up communication.

DEVRA: But not offer them mammography or examinations? Of course they may get those through other sources.

DUNN: Yes, as far as support that they are getting now, I think that was only to be over a five-year period. What we are trying to get them to do here (Oakland)

is to give us the population that's being screened, let us run against our incidence system; we can pick up cases that they either didn't get or developed in the time before their next examination.

DEVRA: The women are from all five Bay Area counties, is that it?

DUNN: All those that have been through their system, they will have 10,000 women who have been screened. Some of them will come back, some of them won't. They will follow those. They already know that there are some cases that have appeared with a mass before their next screening. I think three cases, so far, of women who were negative in screening and turned up a palpable mass before the next one.

DEVRA: And you can then compare that kind of experience with the tumor registry data, is that correct?

DUNN: We can look at survival for these people. This is not a good way to evaluate mammography, but at least we can provide this kind of information. We can provide information on the total population that stayed in the area, who might have developed cancer sometime after the screening.

DEVRA: Any kind of cancer?

DUNN: Yes.

DEVRA: Especially if they were hospitalized at one of these hospitals which participate in the Alameda County Tumor Registry? All five counties participate in the incidence base?

DUNN: We will run all the incidence cases against the State death file. For years we have used the cumbersome way to follow up cancer patients, having to go to the hospitals and say, "Well, you have a patient here that we don't know about, do you know anything about them, will you find out and let us know?" The end-result that we are looking for is whether death has occurred or not. And we can do that by running against the death file and not asking anybody anything.

DEVRA: And that you don't need any permission to run.

DUNN: Right. And we show them that the only thing you can miss is the out-of-state deaths, which are a relatively few. You have a bias in the other direction with a positive follow-up because you lost a patient; and the patient dies, and you list her as a death and so you say, well, we completed follow-up on that patient.

But there is another group of lost cases that you know nothing about. That case came out of this lost group. These persons don't go into the denominator; you put the patients in the numerator, so you have more cases in the numerator than the denominator has provided. You penalize your survival figures by having more deaths.

DEVRA: If you were czar of cancer control, what would you do?

DUNN: I would resign.

DEVRA: Would you really? Do you think it's a hopeless job?

DUNN: Well, I think they have gotten into an awful lot of things that are not going to show them anything.

DEVRA: Is it because they haven't developed systems of measurement at the same time, or they are not concerned very much with measuring what they are doing?

DUNN: Well, I think some of that is true. For example, I know of a good medical center where they see about 2000 cancer patients and the State produces about 20,000. They have a State registry, but it doesn't function. So they will never know what impact they are having on the total cancer picture. They are only going to know about those 2000 who reached them, and this really won't tell them too much.

The NCCP (Northern California Cancer Program) has a system. We have done this already with incidence data. We can look at all the cancer patients occurring, know what hospital the person went to, and if he went to another hospital, whether he was referred. We can look at certain sites like Hodgkin's disease, for example. Stanford has a big reputation for treating it. We see some cases getting into the small hospitals and some of them get referred into medical centers or Stanford. We can see what kind of evaluations they offer in terms of disease stage, compared to the candidates for the treatment that Stanford could offer.

I think we can do a lot in terms of knowing more of how cancer patients are being handled. Some of them are not getting the benefit of what is available, this type of thing.

DEVRA: Do you think as some of the people in the NCI have said, as a policy, that if you got cancer you really should get yourself to either one of these comprehensive cancer centers or to a specialized cancer treatment center because the expertise is there? Do you think that is something that you should be selling as public policy? What will this do to the private doctors who are in the front line. They are the ones that are seeing all these cancer patients. They don't want their cancer patients stolen from them. In the end, they usually get them back, after the surgeon and the radiologists have had their play; and they have to see them through death. How can we help the practicing physician do his job better with respect to cancer care, really deliver comprehensive cancer care, and at the same time insure that people will have an opportunity to get the best that science does offer? Do you have any thoughts on what we can do?

DUNN: Well, I think some provision should be made to have the sort of clinical studies done that need to be done, to decide what the patient is eligible for.

DEVRA: These cooperative clinical studies, you mean?

DUNN: Yes, like with this Hodgkin's case. Did anybody really know whether this study experience was limited to one or more nodes, and, if so, should the person have gone to Stanford or UC for intensive radiotherapy. If they had more extensive disease than that, maybe it wasn't necessary for them to be routed that way. Doctors still should have known what should be done for patients at that point. But, to put everybody in centers probably is not necessary either.

There needs to be a decision made early in the patient's history where they know what the extent of the disease is, whatever can be known about the kind of disease, and what kind of therapy options there are. If there is more than one, let the patient make some decisions.

DEVRA: Do you go out into the clinical community once in a while, do you still go to medical society meetings here in Alameda County and things like that? (This is an interesting comprehensive cancer center because it's not in one place. It's a group of hospitals plus this Bay Area Cancer Epidemiology Resource. Is it two States now? Western Nevada, Northern California, Stanford, UC, University of Nevada Medical School, Davis, but capability in all of these places varies. For example, Stanford has this marvelous capability with the linear accelerator and radiation therapy for a number of cancer sites. There may be medical oncologists at UC-SF who know more about chemotherapy than the people at Stanford.) How does the doctor out in the community advise his patient, his stage two breast cancer patient, what is the best therapy? Where should that patient go to pick up the best therapy? What do you think he should tell his patient?

DUNN: Well, I think it would be an advantage if all the decisions could be made about the patient as early in this line of referral as possible, rather than taking the patient and loading the center with some that are no more capable of initial treatment or sending back treated patients to physicians who can handle it in the local area. I suppose it comes to a matter of almost tumor board decision-making about most every case of cancer.

DEVRA: Now in small hospitals, are tumor boards beginning to catch on? Are there incentives for having them? There is a rumor, of course, that in some States, in Massachusetts, they have actually done this, that to insure a high quality of care where the State or the Federal government is going to pay the bill or part of the bill, cancer care has to be rendered in American College of Surgeons' approved cancer program facilities, and the services have to be performed by board-certified specialists. They have done this by law now in Massachusetts (later determined law didn't pass.) I am not sure how widespread this is going to be. There is some talk that, for reimbursement by the Federal government, similar kinds of standards will be required under PSRO. In the long run, do you think this type of thing would be good for cancer control in this country? Tumor boards everywhere, and chains of communication?

DUNN: Well, it has to be set up and formalized. The patient should have the opportunity to be seen by these different specialties. Clear back when I was starting out in this whole thing, it was considered that there should be three people looking at every cancer patient: a surgeon, a radiologist, and a pathologist. Those three should be making decisions jointly.

DEVRA: Was that the triad that the American College of Surgeons promoted?

DUNN: Yes, I am pretty sure that that must have been where this idea originated. Still, the surgeon was the dominant figure, always.

DEVRA: Is that right?

DUNN: Especially if he could remove the tumor. The radiologist gets the case if the surgeon can't quite handle the whole thing or something recurs. There were not really that many options. Now we are getting to the point where there are options. Certainly in breast cancer. McWhirter came over. . . .

here and talked about simple mastectomy and radiation and that really horrified the surgical population of this country. I remember McWhirtercame here, gave a lecture at the Naval Hospital. All the surgeons were saying, "If I am going to treat a breast cancer, it's going to be a radical mastectomy or nothing." Well, I think if women had the idea that there are some options, that it isn't always necessary to do radicals---and the British had a very logical explanation for what they were doing---they said, "If you do a radical and you have lymph node involvement, then your survival rate is way down. Most of the time it has gone beyond. If the nodes were negative you didn't have to take them out. Leave them in there and we will radiate and we can do just as well as you, in terms of end-results."

DEVRA: And did they actually do a study that way, comparing radical versus modified?

DUNN: Using their own data. You know, people went over from here questioning whether or not these cancers were like those seen here. I think Garland went over there one time.

DEVRA: Well, this goes back about 20 years.

DUNN: Oh yes. They had a session at one of the cancer conferences which was pretty bad. There was an epidemiologist from Canada, McKinnon, who was questioning whether or not the treatment of cancer does very much anyway.

DEVRA: Any cancers?

DUNN: Well, he was talking about breast particularly. And a couple of pathologists from England and a statistician over here at the medical center. A large woman, Dr. Esch Lucia (I don't know how she got involved with this) was there. They had this session in the evening in a nightclub-type setting. Everybody sitting around. It looked like an inquisition.

DEVRA: And the question that night was what?

DUNN: It was not an objective scientific discussion about the merits of these two points of view, but trying to discredit this modification that was contrary to verything we had been doing and thinking for the last 30-40 years.

DEVRA: The Haagensen radical...

DUNN: There was a super-radical that got started one time only because the internal mammary nodes involved particularly for lesions in the inner half of the breast, which got to be very extensive, affecting all of the nodes. Chemotherapy seems to be quite effective. . .

DEVRA: In lieu of surgery?

DUNN: Right. Or along with local tumor removal.

DEVRA: That would be after you have done a biopsy at least, is that right? When you have staged it?

DUNN: So I think physicians as well as people ought to know that there are options. The patient should have an opportunity to think what his or her preference is, and whatever the preference is should be available. The

patient should be given the opportunity to know what else is possible and to make a choice.

DEVRA: Well, do you have any other wisdom for us? (I think you have had about 40 years of wonderful wisdom and you are one of the rarest persons I have spoken to, or listened to.) We've met only a few persons who have spent their careers in cancer control. Dr. Robbins was in the Health Department when, in the 40s?

DUNN: In the late 30s...

DEVRA: He hasn't spent his whole career in cancer control, although I think during the era when he was the Cancer Control Branch Director he was extremely effective, at least it appears that he was. He mixed it with Health Hazards Appraisal. He was one of the risk factor proponents. Gene Miller, actually--- almost all of his professional career has been in cancer control.

DUNN: Yes.

DEVRA: For example, you were a Public Health Service Commissioned Officer until you retired in 1960. Among your coterie of officers what kind of reputation did people have who worked in cancer control?

DUNN: Within the Service itself?

DEVRA: Yes, or even any control program.

DUNN: I think it was an honorable, acceptable activity. I was mostly interested, let's say, in the methodological ways of making decisions about the value of doing something. An awful lot of times, a thing seems reasonable and we start doing it. You don't really know whether it is anything or not. This I don't like. I like to see something that has potential and explore to find out what it really is.

DEVRA: And measure it to see if there really is something.

DUNN: Let's not kid ourselves. Let's say a surgeon was into mammography. I showed you this plot of survival. They would never think of throwing away the first group of cases because this is going to be the biggest group. And to convince them (surgeons) that they can't do something with it, they would say that they got a marvelous improvement in survival. Only because it's a different time frame in terms of when the disease was beginning and a d fferent mix in terms of the kind of cases.

DEVRA: They really don't intellectualize about it, do they?

DUNN: No.

DEVRA: They don't treat these as intellectual problems. You really have been very concerned with the methodology and evaluation, knowing whether or not you've got anything.

DUNN: Yes. We still have questions to answer about cervical cytology.

DEVRA: What kind of questions do we still have to answer with respect to cytology?

DUNN: Well, mainly, is there any component of women that don't maintain an in situ phase?

DEVRA: Who go directly into invasive?

DUNN: Or almost simultaneously. You get full involvement, but you get a penetration almost immediately and there may be something about these women immunologically.

DEVRA: Have you done anything so far to give you some hints about this group?

DUNN: No, with the Memphis and San Diego data, we have women who seem to be going directly into an invasive even though they have been negative twice before and didn't have a long time period between. Now we are looking at Alameda, to see if we can sharpen this up any. It will be very important to know whether this is 5%, 10% or 20%.

DEVRA: You were never interested in the administrative aspects of running demonstration projects?

DUNN: Not too much. I got enough of that in Memphis.

DEVRA: Were you the main administrator on that project?

DUNN: From the Institute, I was running the show. We had young men we brought in there too; in fact, several of them got their pathology boards out of it that way. But as far as direct responsibility, that was mine. What I was trying to back, most people didn't understand anyway, with respect to the data and what I was going to do with it.

DEVRA: Did people think that you were kind of selfish trying to get that stuff, or did they think well, that's alright, it's kind of a curiosity?

DUNN: I don't think most of them understood enough of what I was getting after.

DEVRA: Sprunt must have understood.

DUNN: Yes, in a general way.

DEVRA: So, he was cooperative.

DUNN: Oh yes, he was interested, this must have some real advantage if they go through this in situs, this is the time to identify and treat them, and this should be a curable disease in that case. I was very enthusiastic, because here was one cancer that we should be able to do something about. Because if there is this amount of individual immunologic variation, how many are there? That is what we need to know.

DEVRA: Have you ever started to think about looking at people who don't develop cancer. Looking at people who are 70 or older who have never developed cancer.

DUNN: Smoke a pack of cigarettes every day of their lives, drink a quart of whiskey....

DEVRA: What gave them or what is giving them protection? How would we go about looking at something like that?

DUNN: Again we would have to go about looking at them physiologically and biochemically.

DEVRA: You never really looked at that. I think of our 95-year old friend Mr. Arnstein in San Francisco---the worst thing that has ever happened to him is a little heart trouble. No cancer in his whole life. His 90-year old wife. How come they don't have cancer? Good genes or something?

Back to the diagnostic test evaluation, tell me about your contact with Cuyler Hammond?

DUNN: This was in this conference, the conference of cancer detection in 1949 and Dr. Stowell was the moderator. We got into the discussion of the criteria I was insisting upon. Hammond was thinking that I was being too stringent. Stowell ended the session by saying, "If you are optimistic, go talk to Cuyler Hammond; if you are pessimistic, go talk with Dr. Dunn."

DEVRA: How many people came to talk to you as a result of that?

DUNN: Well, the people that had these reference labs, they had a great deal of skepticism themselves after they tried to deal with a couple of tests. They never got around to the point of having to use our statistical method of deciding, because they were never good enough to get to that point.

DEVRA: The director of the ACS in the 40s, Dr. Charles Cameron, he succeeded Dr. C. C. Little after the big shake-up in 1945.

DUNN: He was a member of this Advisory Committee of Cancer Control. He was a very dynamic person, forceful...

DEVRA: Was he very supportive of this idea of finding a single diagnostic test?

DUNN: No, I don't think so. I remember his making the point that somebody in New York who read signatures thought he could tell whether a person was a candidate for cancer or not, or had cancer.

DEVRA: You know, I still get people on the telephone at UCLA who call up and ask me if there is such a thing as a cancer-prone person, and if you can tell it by a handwriting test. There are still people doing this, or selling this idea. But Dr. Cameron was much more sympathetic to the idea of cancer detection and the methods of cancer detection, I gather. By then, the ACS was beginning to invest some money to train the cytotechnologists or to run demonstration projects.

DUNN: Yes, they supported a lot of cancer detection clinics.

I N T E R V I E W

Interviewee: Dr. Margaret Edwards

Interviewer: Devra Breslow

Location: Bethesda, Maryland

Date: November 3, 1975

DEVRA: Could you give me your assessment of the factors that influenced the pace at which control measures, such as the Pap smear and mammography have been applied in broad screening efforts in communities?

EDWARDS: I think the pace at which anything takes place in this country is a reflection of our culture and how we deal with innovations. The pace will be rapid if the innovation is acceptable, and it will be torturously slow if the innovation is unacceptable for some reason.

I think we are a very independent people. It's very hard to motivate us to do anything. We have to have some kind of rewards for what we do and feel that it's the "in" thing to do. That goes right up and down the total social scale, I think; it isn't just limited to people in a certain class. People have to feel that this is worth something to them.

I think your question can be answered better, perhaps,by sociologists and behavioral scientists than by those of us who trying to promote these innovations. All we know is that they are good to do, but how to get them accepted is very difficult.

DEVRA: Is acceptability a factor of not only consumers but really a factor of the so-called providers, physicians and other technologists and the institutions? If they don't find it acceptable or feasible or practical, is that part of the resistance?

EDWARDS: I don't know. I think we all believe in the things we think will decrease morbidity and mortality in all diseases. I think if a procedure is unacceptable to people, applicability is harder. Perhaps that's why physicians have avoided the rectal examinations for instance, and the Pap smear. Not that they don't believe that they are important, but they are a nuisance, they are time-consuming. Sometimes it is difficult to convince people that these are as important they are, unless the person has symptoms. Then, there is the economic factor. You are going to have to charge additional fees, let's say, for a Pap smear, and that sometimes will either deter physicians or pose some kind of a barrier in dealing with patients.

I remember when the polio vaccine was finally about to be released in this country. There was a large meeting of the county medical society to which I belonged at that time. I went to the meeting, even though I probably wasn't going to use the vaccine, because I didn't deal with children, but I was interested to hear the discussion. The discussion was strictly limited to what the doctor could charge. It had nothing to do with who should be first to receive the vaccine. As it finally turned out, I think, pregnant women and certain other classes of persons got this vaccine, which was in scarce supply at the onset, in addition to children. The whole discussion had nothing to do with that. It was all about what should be charged, what should be the standard fee.

DEVRA: Rather than how to organize distribution in some systematic way...

EDWARDS: You really can't get away from the economics, even though in our sophisticated way, we think of it as rather a crass item, but it's very basic to much of health care.

DEVRA: Maybe some of that is also linked to the fact that people are very "symptom-oriented." I wonder whether the origin of that is behavioral or

professional. Doctors have taught us, even the American Cancer Society has taught us to be "symptom-oriented." Don't worry about prevention as much. We have down-played prevention.

EDWARDS: I think it's human nature, when you are feeling okay. Our whole orientation as human beings, not just in this culture but in any culture, is to conceive as yourself as okay, physically fit. Any threat is pushed back until it is unavoidable. Consequently you shouldn't have to do things to stay fit. You are already that way. I think as human beings, we can't conceive of ourselves as unfit until it is obvious or we are threatened by illness. You wouldn't walk under a boulder that was falling down a hill toward you. When you perceive a threat you avoid it. Otherwise, you feel that you are fit to walk anywhere.

DEVRA: Do you think that's unique to Western Culture or to all cultures?

EDWARDS: I think it's just a part of the human condition, of being human. I don't think it's unique to Western Culture at all.

DEVRA: That leads naturally into the second question. What is the potential then for human beings being more responsible by practicing various self-examinations? You mentioned in your letter, the real possibility of exploiting self-examination, getting people more interested in their own bodies and their own bodily systems. What do you think the potential is for this?

EDWARDS: I think it's very good. I think people do this anyway. No one ever passes a mirror without looking at himself. We are our own keepers. We are more concerned about our appearance as well as our well-being more than anyone else is for us. I think, that since we are always looking at ourselves anyway, we might as well look constructively. I think breast self-examination is a great thing. I think women have found it very acceptable. I think that we should extend this; we certainly can look at our mouths and throats more intelligently than we do, and look under our tongues and places like that for possible redness or what might look abnormal. Our skin is certainly available to us, and there are many other things I think could be done in the way of self-examination, that really haven't been exploited at all.

DEVRA: Do you think the time is right now? People do seem to have a lot more interest in their own well-being. There really should be some opportunities to develop strategies to encourage more.

EDWARDS: I think that's one thing that could be pushed as far as is realistic. Any procedure that can cause embarrassment will tend to be more difficult to conduct routinely in screening or routine examinations.

DEVRA: What is your assessment of other cancer control measures which rely on individual knowledge and initiation, rather than only what doctors and other professionals and institutions can advance?

EDWARDS: I think that physicians, who are supposedly most knowledgeable people about what should be done, by and large take pretty good care of themselves and get preventive things done and have managed to stop smoking, more so than other professions, in greater proportions, demonstrate that when you have all the facts, you are more likely to perform more rationally, then when you don't.

EDWARDS: I don't think you can tell people too much. I think the more they know, the better. I think it's hard, because there is a lot to tell and some people don't want to listen to it all. But I think the more the public is informed, then the greater their knowledge. The less excuses they will be able to find. They will be doing what we all know we should.

DEVRA: What do you do about professional resistance to people knowing more about medicine and their bodily functions? (I recently recommended we put our Cancer Bulletin in patient waiting rooms, to which our super specialist oncologist replied, "People have too much misinformation already and it makes my life complicated.") How do we bridge that barrier between the physician reluctance to have patients informed or misinformed, and the need for people to know and be more active in their own health maintenance?

EDWARDS: I suspect this attitude is age-related and will gradually fade away, because the younger physicians are being educated in a freer and more open society then those of us who are in a position of authority now. I think openness is age-related. It will gradually diminish, even if we do nothing about it. If something were to be done, I think we should concentrate on older people, the people who have been in practice longer, who have these attitudes. I don't think they are shared to the same extent by the younger people.

DEVRA: Do you see contributions the cancer center could make in better informing their constituent populations, both professionals and laymen?

EDWARDS: Yes, and I think they do too. In fact, I believe it is part of their guidelines. The latest version of the guidelines for Cancer Centers includes a commitment to relate more directly to professional education, not just practitioners within their institution, but those in their area as well as the public.

The comprehensive centers are establishing offices of communication, to deal with the public and with patients, and with health professionals in in that area. I think it is all to the good.

I think there should be some standards as to how this is done, both for convience among the centers and so that the information is not overwhelming from one center and very skimpy from another.

DEVRA: What types of health and communications specialists working together and using what types of strategy, do you think could create a better informed populous?

EDWARDS: I am really not very knowledgable along those lines. There is the profession called health education. I think maybe those people haven't been brought in as much as they might. But then, I think even more important, are those who know the advertising market. Those types of people know how to call attention to things, and they have studied this to a much finer degree then the health professionals have. They know how to reach certain types of individuals, with various convincing messages, or we all wouldn't be buying the things that we do. I think they could make real contributions, as could people in the public media who know how to attract attention to certain programs, how to appeal to people more directly with certain types of messages.

DEVRA: In the final paragraph in your letter, you talk about the disadvantage of the average lay person in relying on health practitioners, who are relatively less qualified, than other practitioners to deal with cancer-family practitioners, internists and others who are really the front line. What do you see as the particular obligations of the better-trained oncology specialists in relating to these practitioners?

EDWARDS: I feel two ways about that problem. I think that the better-trained specialists have an obligation. Most of them realize it and consultation services are being established. I think that is fine. But you have to have, not only a willing consultant, but a client or applicant, someone who wants the consultation and who realizes he needs it. He has a question that he knows he can't answer. There, I think is even a greater problem. I think there are more willing consultants then there are prospective clients.

What I would like to see, and to some extent this is under way, are some kind of standards established for what people should be obligated to do and what limitations they should be aware of in their practices. So that the general practitioners have a set of things that they are obligated to do relative to cancer and another set of procedures they shouldn't attempt at all They should recognize those circumstances and their obligations to refer patients to other specialists and know what kind of specialists. Within the specialities there should also be limits set as to what they do relative to cancer. Pediatricians, for example, should be involved primarily in diagnosis but not necessarily in management of children with cancer.

DEVRA: How do we enforce such standards?

EDWARDS: You can't enforce them. They have to come from the professional groups themselves. In this society, we can't enforce such standards by law. Maybe the PAS regulations that are developing around the country can make a contribution to that.

I think the American Academy of Family Medicine is doing something along these lines. They are trying to educate their members and set up some kind of guidance for them. What I would like to see incorporated in every professional training program in the specialties and the so-called general medicine residencies, would be guidelines as to what should be the expectations concerning knowledge and skills relative to cancer upon completing training.

The American Dental Association, under contract with Cancer Control, has just finished a set of curricular guidelines for dental schools relative to cancer. Now, I think there are some limitations to that, but it is a great start. Medicine should do the same and all its specialty training programs might profit by doing something along those lines.

Then the maintenance of competence after training is another big factor. Here, in our society at least, we haven't quite yet reached the point where we can control this, as is done in other countries. Continuing education institutes are established just for that purpose. Refresher courses are given for practitioners.

DEVRA: They are actually incorporated in a recertification process?

EDWARDS: Recertification is coming. There is still no control over what type, it's left to the option of the individual as to what type of courses he takes. It may not be those he needs, as much as those he has an interest in. Perhaps he would need to take something else that he might overlook.

DEVRA: To what degree can training program which you direct from this office, (the Clinical Cancer Education Program) contribute to relationships between oncologists in the centers and practitioners in the communities? Are there incentives in that program for outreach continuing education or are they mainly for undergraduate health science students?

EDWARDS: It depends on the applicant. These programs provide support for undergraduate education. They also influence the education of people at the post-residency level who are interested in going in to cancer specialities such as medical oncology, pediatric oncology, etc. And they stress doing something in continuing education. Because the latter is the hardest, it's the one with the least proportion of effort in these grants. Primarily the applicants support activities carried out at the institution itself, like courses and seminars and lecture series, that are open to practitioners.

But there are some innovative things too. One of the dental schools send out a dentalhygienist with equipment to project audio-tapes, and a laryngeal model on which she can teach dentists to do indirect largyngoscopies in their offices. This has been very successful. It sounded a little weird at the outset but they selected the right type of person, they got the agreement of the various dental societies to do this as a test of methodology for such demonstrations, and it is now being expanded.

DEVRA: Is this in one specific area?

EDWARDS: Yes, it's in Lexington, Kentucky, going into the rural counties. Dentists are given a pre and a post-test and they have responded very favorably.

These grants do support a variety of activities, some of which have to do with continuing education, all of which have something to do with undergraduate education. There are more institutions that we can provide support for. This is the limitation of this program. Its efforts, are as far as they go, have a favorable influence, but they don't begin to cover all that is needed.

DEVRA: One thing I have observed is the tendency for centers to encourage people, such as other practitioners, dentists, to come to the center, where the resources are available. Have you had any experience, or have you learned as you visit some of thse programs about techniques of taking information and demonstrations out to where the practioner is, taking it right into a group office, for example?

EDWARDS: Yes, I think that is always more effective. It is also more expensive and more time-consuming, in both time and people. There are just not enough people to do that, and still do the work back at the center. It's the costliest way of doing it, but it is more effective.

DEVRA: I wonder if it would have to be done necessarily by peers, doctors relating to doctors. You described the dental hygienist. I was wondering, for example, could health educators, or specially trained cancer control people accomplish this, people who are not necessarily as expensive as physicians?

EDWARDS: I suppose to some extent that doctors wouldn't pay too much attention to such people. It is very difficult to reach physicians in any other way other then with their peers. If someone is coming, they want an important person, not just an associate or a resident.

But those who have taken the time to go out and do "circuit-riding" have found it very rewarding and have been able to establish satellite centers. Another way you can do this is to bring a person from a community hospital or the community into the center for an extended period of time, up to several months.

DEVRA: Perhaps by sending a resident in his place out to take care of his practice?

EDWARDS: And give that person a degree of expertise so he could serve as a mini-expert in his community. Where this has been done, it has worked very nicely. These people then maintain a continuing relationship with the center and become sort of a "community X's" man in the center. He goes back maybe once a month for a conference so he can keep himself informed and one place. In a city in Florida, the University gave these people clinical appointments and that enhanced their prestige in their own committee greatly. I think that is perhaps one of the more attractive ways of doing this. It's true, it's time consuming, but I think the payoff is much greater. When you go out to a community, all you have left behind is a memory. You have to keep going again to reinforce that. But when you train somebody and then assign him where he is a continuing source of expertise, the effect is much better. I think more of that should be done around the country.

DEVRA: Do you see that happening?

EDWARDS: We have encouraged it in a few places through our grants, but no, it is not happening, to a large extent, that I know of.

DEVRA: Do you think there should be more oncology training throughout the preparation for a health profession such as medicine or dentistry?

EDWARDS: I don't know if it needs to be more, I think it should be more structured, more organized and conform to what I said earlier, some sort of a standard minimum. I think the amount of time that is available in the health professional schools is in such demand, that you can't do it by just adding more hours. It's the way that you provide the teaching, I think that should be looked at more carefully and made more attractive and more lasting.

DEVRA: What types of on-going educational strategies appear to be having some impact at the undergraduate level? Have some of these centers come up with ways of improving the quality of the oncology training students receive?

EDWARDS: You mean the techniques?

DEVRA: The techniques, the actual practical experience, even the didactic techniques, or using things like computer-assisted instruction.

EDWARDS: I think self-instruction, however it is carried out, has been an improvement over the standard textbook and journals methods. I think nothing replaces good clinical exposures. If these are structured and designed with a

EDWARDS: definite objective in mind, then I think that's the ideal teaching technique. Some of these mannequins and things that are used in teaching are also very good. They spare the patient endless examinations.

For instance, there is something called a tudorendoscope, on which you can learn how to insert a sigmoidoscope. It could also be used to teach bronchoscopy and some of the other more endoscopic techniques where you don't have to use a live patient to teach. Mannequins can be used to teach the dentist how to do indirect largyngoscopies.

But it is basically a matter of making it interesting, and appealing, and the teacher who can do that has won half the game. In talking with students around the country, I still find that they learn more from good teachers then they do from poor teachers. And particularly with a subject like cancer, which is bad news, no matter how you try to gussy it up. It's still an unpleasant disease to deal with, and teaching doctors to deal with it objectively and to try to get satisfaction out of dealing with it, influences their future behavior.

Teachers who have that attitude themselves are the best teachers about cancer. Teachers who have that attitude and are also the best in their field are able not only to teach what needs to be done about cancer but to teach the proper attitude towards doing those things. That, I think, is perhaps more important with cancer than any other disease I can think of.

But cancer is unattractive; you have to have a different point of view to deal with it successfully and appropriately than you do with other diseases. If this attitude can be communicated in the health professionals schools, then I think we have come a long way. Good teachers can do that, whereas no self-instructional method can convey that quality. The attitudinal component can only come from live bodies.

DEVRA: Well, another group of live bodies, or maybe dying, are cancer patients. To what degree can they especially those who have been cured, interact in the educational process?

EDWARDS: Oh, I think they can be most effective. I am so happy to see this coming along. To some extent this has been done always, but its only recently, again with more openness of our society, that this has really been done in a big way. Of course, the wives of our president and vice-president having had cancer and being so open about it, Senator Kennedy's son and all that, has helped. I think even the people who are not cured, can make a contribution.

And they are doing so, Joseph Alsop, that sort of reporting, I think is very effective. It may be a little dismal to some people, but you can get a lot of insight. I think that the cancer patient can make a tremendous contribution. The American Cancer Society has a woman who had breast cancer and she goes around talking about it. She is just great. I think more of that needs to be done, particularly in the hard-to-reach groups, people in those population groups whom you really need to get to the most and who are least responsive to screening and that sort of thing.

DEVRA: Are you familiar with any organized approach to harness the committment of people who have been screened, and who are high-risk individuals to promote early detection? Have you heard of any organized programs for this?

EDWARDS: Well, I imagine the American Cancer Society may be doing things along those lines, using volunteers.

DEVRA: The NCI, to your knowledge?

EDWARDS: Not that I know of, but then I don't know what all Cancer Control may be doing at the moment, or planning.

DEVRA: Getting back to educational strategies for the health professions, what kind of initiative might come from the professional speciality boards? We need to identify the role of professional speciality boards not just in continuing education but perhaps in undergraduate health science. Do they have a role?

EDWARDS: Yes, I think they have a role. But I think it would be hard to expect any organized activity on their part. It must be something they want to do, rather than something that the government would demand of them. In fact, each of the major specialities that impinges on cancer has strong cancer committees that are already doing things that have been responsible, for example, setting up sub-specialities in oncology, as we now have in internal medicine, gynecology and pediatrics. I think the American College of Surgeons is doing something along those lines, at least recognizing the problem regarding surgical oncology. With these sub-specialities, there are standards for the qualifications of such individuals.

They're doing as much as they think they can. Probably they would like to do more, but I think it's hard to formalize efforts of this type. They have to come willingly, and I think just the interest that cancer has had in the last few years, particularly since our legislation has placed such emphasis on it, has made all of these groups more conscious of their own oblications and responsibilities.

The Association of American Medical Colleges plays a role in this, too. They set the standards for medical education. But they do not set categorical standards, and they don't want to. They feel this is something that they shouldn't be getting into and I tend to agree with them. But individual professional organizations, I think, are undertaking these responsibilities pretty well.

DEVRA: What role do you see Cancer Centers playing in surveillance of the educational content within their own institutions?

EDWARDS: I think they all will do that. The American Association of Cancer Institutes acts as a self-disciplining board for those that belong to it (I think it includes most of the comprehensive centers This is one of their tasks. They are going about it, I think, trying to standardize, set minimum standards for the various training programs carried out, at least within their own institutions.

DEVRA: Do you see different educational strategies being required in Schools of Public Health and in training for the allied health professions which inpinge upon cancer care?

EDWARDS: I think the allied health professions are awakening to their own responsibilities relative to cancer, particularly nursing. I am very gratified by their interest in recent years regarding the nurse's role in cancer. There are several nursing schools now setting up degree programs for nurse oncologists. I think this is great. They have always had a great interest in cancer. This

EDWARDS: goes back to the early days of the Cancer Control Program as I knew it in the 50's and 60's. But there was never much that they could do and I guess they never really felt too welcome in taking any authoritative role in cancer care, other than following orders. They have always had a great interest in it, because they are with it far more than doctors. They see what happens to patients. They have a greater realization than, I think, the medical profession does. Some of the psycho-social aspects we are only now beginning to address directly. So nurses have taken a good approach to this. There is a new Society of Nurse Oncologists, and I think it's all for the good.

The other health professions are, (and I know less about them so I really shouldn't even comment) taking similar looks at their own responsibilities. The social worker who specializes primarily in cancer, for example.

DEVRA: Through your educational grants, is there some encouragement of team teaching or team management?

EDWARDS: Now, this is the basis on which this grant program was originally developed: to provide and coordinate an approach to cancer teaching. Teamwork is the theme that we go around talking about and insisting upon, although I must say that it is difficult to bring about.

DEVRA: And is it even more difficult to evaluate?

EDWARDS: Yes.

DEVRA: Are you finding a lot of resistance?

EDWARDS: It isn't so much resistance. It is the way medical schools and dental schools are structured, along departmental lines. Even the systems-approach and some of the other changes that have come about in health professional education just recently, haven't had a great impact on the departmental structure.

It's difficult to carry out. You have to get people together. In order to do that you have to plan things in a formal way. People have to make commitments to go on certain rounds every time they are scheduled. It's a matter of timing and re-structuring things. Like anything else, when you are committed, you do it. In places where this commitment is high, it comes off very well. In places where the commitment is low and a tendency to status quo prevails, it's more difficult.

DEVRA: You might find in some institutions also an attitude that the same patients are being used or exploited over and over by different students, medical students, social workers, nurses all in training. For example, in our institution, we want to train patient associates who are essentially trying to deal with death and dying. One reaction we got was "the patient simply can't stand being used anymore." Have you run across this problem?

EDWARDS: No, but I dare say it happens. Again I think it's a matter of proper scheduling, so that you don't have a dozen people tripping about and doing essentially the same thing. If you can have them all there at once, the patient can say it once and that will be it. So I think it's a matter of mis-management.

DEVRA: Could community practitioners be even involved in team teaching?

EDWARDS: They should be and sometimes are.

DEVRA: Joining in rounds and so forth. In your estimation, which three to five individuals have contributed most to cancer control historically, I would say, all the way back to 1910.

EDWARDS: Well, I think you would have to mention Papanicolaou, because without that technique we would still be blundering around, unless we discovered one that was better. That certainly seems to be one of the most valuable tools that we have in cancer detection. Dr. Elsie L'Esperance. I think you have to give that person credit for setting up a scheme of examining "well people" for cancer.

Sidney Farber, for pioneering the approach to chemotherapy that lead to the development of really effective drugs in several diseases. We must give credit, although I am sure there were others before him.

I think in responding to a single screening program, there was a woman at what used to be called Womens Medical College in Pennsylvania. She was a surgeon. Her name was Catherine McFarland and she promoted Pap smear screening on a wide scale in the city of Philadelphia. She went into industries and got the women in various factories to agree to come for screening. She incorporated it into the standard routine in the GYN clinics and all the other clinics, long before it was being done as widely as it is now.

I think Ernst Wynder and the others who came up with convincing evidence that yes, smoking is, there is no doubt about it, smoking is a factor in the etiology of lung cancer, and particularly Daniel Horn, who promoted so much the exchange of information; he did many studies as to why people smoke and why they have trouble stopping. I think that was a major contribution. There was a woman in Philadelphia also by the name of Ingleby, a pathologist who worked with Gershon-Cohen. She kept insisting that you could see early breast tumors in X-rays if you looked carefully and corrolated it with the pathology. She didn't get the credit for it, Gershon-Cohen did, but she certainly was the one who was persistent in keeping that work going and proving that you can use this technique not only to detect existing cancers but potentially early ones. Her name has sort of dropped by the wayside.

DEVRA: Now you talk about Dr. L'Esperance, and it occurs to me that her work was done, if I remember correctly, during the first World War and then in the twenties. The whole concept though, the screening of apparently well people, has never really been very popularized. Do you have any idea why? What are the factors?

EDWARDS: Well, I think it could be organization, financing, professional disinterest. But as I said earlier, I think it's part of human nature. It has to be learned that it isn't consistent with out human nature to feel you might be threatened by something. I guess that is why we stay alive. If we succumb to all the threats that there are, we would go into despair and kill ourselves. It's that aspect of human nature that keeps us on an even keel, most of us at any rate, and I think it also influences our behavior.

DEVRA: Well, do you think also medical education has played a role in perpetuating the complaint-response system, rather than the health maintenance concept?

EDWARDS: When you think of the history of medicine, I don't think the idea of really preventing anything came into existence until maybe the last 100 years or so. Consider how little we had to offer people with diseases. Now I am not talking about injuries, but diseases.

DEVRA: Especially communicable diseases.

EDWARDS: Yes, I don't think you could expect medicine to do otherwise. Because the whole way back to the witch doctors, it was more response-to-symptoms rather than thought of preventing things. If you did think about what you might do, all you could think of was prayer. And people did pray for relief of plagues and so forth. There was no other recourse. It's only with the understanding of disease etiology, I think, that we have come at last to think in terms of prevention. This in teaching has lagged behind what we know to do.

I don't think you can expect medical education suddenly to turn itself inside out, when its whole tradition and development has been otherwise. I think it gradually will, and especially in some countries where they have perhaps a little more insight and more practicality about their medical care then we do. Think of England and the socialized systems of medicine, the USSR, where their emphasis is on preventive medicine. They have really made it pay off, because their society is such that they can control it. That makes a big difference too, when you can withhold pay, for instance, of people's salaries until they get their health examinations. Imagine doing that in this country.

DEVRA: It would be a wonderful idea.

EDWARDS: It would be wonderful, but we could never get away with it. But there are incentives that I think you can offer people: reduction in their health premiums and this sort of thing...

DEVRA: Well, just sort of a random thought I had, with the concentration on complaint-response, we also have cancer quackery in this country. To some degree these may go hand in hand, but if we had the opportunity and the incentives with more emphasis on prevention, perhaps we would have less anxiety about the consequences of cancer, which in turn really spawned this whole market of cancer quackery. It seems to be worse in that field than perhaps any other disease known to man.

EDWARDS: Arthritis, probably more then cancer, because there are probably more people with arthritis that don't die. They live on and on with arthritis and go from quack to quack. That's probably the biggest market for quackery. But cancer is, I guess, the next biggest.

DEVRA: In your estimation, where should cancer control efforts be concentrated in the next quarter of a century?

EDWARDS: Prevention, but I still put detection ahead of that. Detection and early diagnosis, because I believe very strongly that early diagnosis leads to better results then late diagnosis, in most forms of cancer.

I think that if we could control what people do about diagnosis we could save many,many,many lives.

I think that research should go into prevention, but I think that control should be directed towards detection, hopefully to be followed with the appropriate therapy.

DEVRA: What factors and relationships are essential to direct cancer control efforts in the next quarter of a century? What kind of resources do you think we need? What kinds of alliances between the public and private sectors, between the biomedical community and the cancer control community, for example?

EDWARDS: I don't know, but I would say some kind of standards might be set in the medical profession and the dental profession of appropriateness of certain procedures and inappropriateness of others. I can't say how that can be done, or who should do it.

DEVRA: We still have gynecologists who practice surgery even though they are not fully trained to do this. Is that what you mean?

EDWARDS: What I am talking about is what kind of surgery you should do at what stage, let alone who does it, or whoever is best qualified to do it or who can do it appropriately. I don't care if it's a general practitioner, if he knows how to do it and he can do it well. The likelihood that a specially trained surgeon can do it better is of course greater, but that's not what I am so concerned about, as that the right thing is done at the right time.

I think that there should be criteria for what the right thing is in many forms of cancer. We have to do more cooperative studies to perfect our techniques. Certainly, it should be possible to form a consensus on what the right thing to do at the right time is for a variety of cancers at various stages. It seems to be a pity that this has not been done yet. I'm not thinking about a "cook book" that everyone can go out and use, but minimum criteria. It seems to me that it could be developed.

DEVRA: Are there any currently cooperating cancer control efforts with which you are familiar with that should be eliminated totally, or at least be redirected?

EDWARDS: Possibly there are. I think there are some projects that have been started that perhaps will fail when their support runs out, for instance, because there has not been adequate planning, not necessarily by the people who initiated them, but by the people who have carried them out, to continue them, and that always seems a pity, when support is withdrawn and things just fade away. But, I don't think I can give a very useful answer.

DEVRA: What about that whole concept of support? Things are generated because for example, the Federal support is there, with the stipulation that the program should be continued at the local level after a certain period of time. These are "start-up" funds. Do you think the wave of cancer support that we have now, when it subsides, if it subsides, is then going to have its impact on cancer management and cancer detection?

EDWARDS: Yes it will, if those things really turn out to have been worth having done. I am thinking of some of the things the old cancer program started, which continued, despite lack of support: the training of cytology technicians for instance, was supported at one point by the Cancer Control Program. When that support phased out, some of the schools closed, but most of them didn't.

DEVRA: Do you feel that in your own program, for example? These are seed moneys. Do you think these moneys are in perpetuity, and medical schools and dental schools will always be dependent upon these funds, or will they do these things anyway?

EDWARDS: The money in this program is designed primarily to improve the quality and scope and expand activities. When support disappears, if those activities have been effective, they will be continued anyway. We used to support tumor registries at the outset of this program. Then we realized that we could spend an awful lot of money supporting tumor registries, so we began drawing back. The tumor registries for the most part found other sources of support.

I think it depends on the quality and the characteristics of the activities themselves, that these, or any grant supports. If they are really useful and effective, and of good quality, for the most part, means will be found to keep them going.

What will also happen is that the people who have special opportunities may have fewer of them. The student and the trainees that I am thinking of, because that does take extra money.

DEVRA: Do you have any other wisdom for us as we proceed in this chronicle?

EDWARDS: No. All I can say is "good luck."

I N T E R V I E W

Interviewee: Dr. Kenneth Endicott

Interviewer: Devra M. Breslow

Location: Bethesda, Maryland

Date: May 19, 1976

(Impact of CC Separation)

DEVRA: During your tenure as NCI Director, what mechanisms were implemented or sustained to relate research findings and resources to the Cancer Control Branch, which was then physically and programmatically separated from the National Cancer Institute?

ENDICOTT: Well it's hard to recall back then. Don't think anything very active was done. I did try to maintain contact. Robbins, I think, was in charge of the program. He would meet regularly with the advisory council.

DEVRA: He had his own advisory committee as well?

ENDICOTT: They were pretty much hell bent to go their own way. I tried always to keep an open door to them but there wasn't anything on the horizon to get excited about.

DEVRA: How did the people at NCI feel about that, that they were going their own way?

ENDICOTT: I'm not sure. I'm pretty sure of one thing, that to have them separate is a bad idea. But I don't really have an answer for that question.

DEVRA: When you came in, it had already happened?

ENDICOTT: Yes.

DEVRA: It had happened in the 50s. Did you ever have any discussions either with Dr. Robbins or, let's say working down or working up, with Dr. Shannon about the possible reintegration of cancer control?

ENDICOTT: Shannon was responsible for splitting it and there was just not much point to it.

DEVRA: How did the Council feel about all of this?

ENDICOTT: I think they were pretty indifferent about it, really. It was a period of time when cancer control efforts were sort of eclipsed, wasn't much on the horizon to get excited about. I don't know if it would have made much difference if it had still been part of NCI. In the applied and development research programs, and there were several, the tendency was to carry them right on through, by the NCI.

DEVRA: Even to the point of field testing?

ENDICOTT: Yes.

DEVRA: And would they get money to do this so that you almost had two different competing bodies within the Public Health Service that were doing field testing?

ENDICOTT: Well the major field testing was in the therapy area. We had an enormous program. It would not have made any sense to have discontinuity.

DEVRA: How about in diagnosis?

ENDICOTT: In diagnosis, I set up a special contract effort in the diagnostic area under a man named Eli Nadel. And then subsequently Nat Berlin took it over. There again we didn't really have anything much. There weren't many leads.
DEVRA: Nothing was really coming out of the laboratory that had much pertinence to mass application? So the things that Robbins was doing--he was promoting the Pap smear and smoking and eventually mammography and some subventions to states--they could be safely separated out from the NCI regular activities to go their own way?

ENDICOTT: Well I'm pretty sure that the things that were already well established probably were sustained with more enthusiasm with the separated group than they would have been with a primarily research-oriented group.

DEVRA: Do you think they would have been swallowed up by the research group? Eventually they were bigger in the old days, in the 40s, but as the scientific aspect of the NCI grew the force in what was called cancer control by comparison was diminished. And then pieces of it would split off, one piece being epidemiology, another piece being biometry. It sort of was the "mother," and before you knew it the children were all out there and there wasn't anything else for mother to do.

ENDICOTT: I elected to make epidemiology and biometry a strong part of the research program.

DEVRA: You needed them there?

ENDICOTT: They had the potential to make a great contribution through strengthening of the capability team to do research in the etiology area.

DEVRA: Now that was at the NCI? The stimulus had come from cancer control experience. But we had to do more in etiology?

ENDICOTT: Well I'll tell you, I took a look at the whole cancer program when I arrived on the scene and decided that it was poorly balanced and that we had done more than enough in mouse cancer and it was time we really addressed the human condition and cast about for ways to do that. Now there were certain glaring inadequacies in the program.

DEVRA: Such as?

(Carcinogenesis & Etiology)

ENDICOTT: The level of inquiry in the area of chemical carcinogenesis was shameful, especially with clear history that there was "gold in them thar' hills." It seemed to me that also that there was undesirable fragmentation of effort.

DEVRA: Throughout the Institute?

ENDICOTT: Well, in the etiology area again. By virtue of the fact that the radiology component was split off and mostly being addressed by AEC, the virologists and the chemical carcinogenecists were not communicating. If we're going to addresse the problem in that, we had to have a very strong epidemiological program, backed up with in-depth laboratory work. So I think one of the major policy decisions was to pull the whole area of epidemiology/biometry/virology/chemical carcinogenesis together under one man.

DEVRA: Who was that?

ENDICOTT: Mike Shimkin and then Paul Kotin.

DEVRA: And give them enough budget to do something?

ENDICOTT: Really it was Paul Kotin. Mike was responsible for pulling epidemiology and biometry together but it was when he left actually that I recruited Paul Kotin and finished the job. There was one fairly weak organization which I abolished and the that was the Field Investigations and Demonstrations Branch. They had a bunch of soft programs _____ _____. That was really the beginning step, to liquidate that, and then begin to build the other. At the time I went to the Cancer Institute they were strong in applied and developmental research only in cancer chemotherapy. And I had set that thing up before and knew that it was good. Then I decided to use the same basic management techniques to push along the other areas.

DEVRA: Did it work?

ENDICOTT: Yes I think that it did. There were several elements in this: One was to make contract money available at that point for large-scale efforts both in chemical carcinogenesis and viral oncology. Another was to join hands with the AEC and get interdenominational effort under way and bridge that. I don't know what the current situation is. We always had a lot of turf problems, though.

DEVRA: What they (AEC) should be doing and what you (NCI) should be doing?

ENDICOTT: About who was in charge. As long as you had Paul Kotin at one end and Alex Hollander at the other, there were bound to be these problems.

DEVRA: He was the AEC man?

ENDICOTT: Yes. I know they have a strong problem there now. I just don't know what the relationship is. Another thing that occurred which I think was of major importance was the establishment of IARC in France.

(IARC)

DEVRA: That was during your tenure? What particular stimulation did you give to this or did your office?

DEVRA: And did you influence for example the mechanisms they developed for the studies that they did?

ENDICOTT: Yes. I took a very active role.

DEVRA: And the advisory committee set up and in the appointment of John Higginson?

ENDICOTT: Yes, I think I talked John into going. I played a very active role on the Board of Governors and so on. And tried to support him as best I could with NCI resources.

DEVRA: In fact, in the beginning was a large part of their U.S. money NCI money?

ENDICOTT: At the outset the basic contribution was the State Department. But we constantly supplemented that with contractual money and people.

DEVRA: Is that still the pattern?

ENDICOTT: I don't know.

DEVRA: You're obviously very proud of that. Just about as proud probably as you are of the chemotherapy and national chemotherapy service center.

(Carcinogenesis Regulation & Management)

ENDICOTT: Anyhow, I think it was the thing that needed happening and has a good purpose. I think as far as I know they are doing a good job. I kind of agree with "Satch" Page, the baseball player, "you should never look back, they might be gaining on you." So when I cut my ties, I really cut them.

Now there was an unfortunate thing that happened later. . . . And the tragedy hasn't played itself out yet. And that was that the decision to split off the National Institute of Environmental Health Sciences. Shannon had appealed to me personally to help the thing off the ground, and I gave them a big hunk of NCI, including Paul Kotin, and a lot of people.

DEVRA: Are they the people who went to the Research Triangle? Left the area? Were strictly attitudinally separated?

ENDICOTT: Yes. Now the tragedy is that our toxicology capabilities through the PHS were fragmented. It's even more fragmented now because another mistake was made, that was to launch the FDA as an independent competitor. Now we have three. NCI, NIEHS, and the thing down in Arkansas.

DEVRA: That's a piece of the FDA down there?

ENDICOTT: Yes.

DEVRA: Is it conceivable that there is constant duplication of effort?

ENDICOTT: It's almost certain.

DEVRA: What about protection of the public as a result? Since one of these, at least, is a regulatory agency.

ENDICOTT: There has been recognition of the problem. Under Ted Cooper's policy board there is a Toxicology Coordinating Committee that tries to coordinate their activities. But it's a mess. Somebody else will have to straighten that out one of these days.

DEVRA: What do you think the consequences have been for the public at large?

ENDICOTT: I think basically wasteful. Now then the task of defining the chemical risks in the environment is so enormous that I don't think the great nations can afford to squander their resources in this area by having a myriad independent enterprises. I think this is one area in which some degree of international planning and sharing of tasks is in order. For us to have it fragmented, the way it is, and the U.S. to be alone in this?

DEVRA: Are we considered the leaders in toxicology research, internationally?

ENDICOTT: I think probably we are.

DEVRA: But when it comes to management, we are not setting a very good example?

ENDICOTT: We can do a lot better. Unfortunately that's the way it is. but with the spinning off of EPA, the health component of environmental concerns has been substantially weakened. Well, it serves us right. I don't think we were doing an adequate job in the Public Health Service. It was a

half-rate effort. I think we missed the boat in several areas. I don't think we did too brilliantly in the area of personal health services either. I guess it began when we lost "water", and then the "air" followed after that, and it wasn't too long 'til there was an EPA.

DEVRA: What do you think contributed to that sort of demise of authority, demise of responsibility?

ENDICOTT: I don't know. I've often thought that it was petty squabbling between the engineers and the doctors.

DEVRA: That's very depressing. What about the level of competence, in general, of the PHS to handle these major pollution and environmental problems?

ENDICOTT: I don't think that was ever really tested because it wasn't until after it was split off that any decent regulatory legislation was written.

DEVRA: In that sense then, it was a boon that it was split off?

ENDICOTT: Probably.

DEVRA: What about the level of technical and medical competence now within that agency?

ENDICOTT: I really shouldn't comment on that.

DEVRA: Do you think that if it had remained more integrally a part of the PHS, regulatory powers would have come or not?

ENDICOTT: Yes I think it would have. I think there is essentially concern over health issues that would have brought the regulatory power. I think it was our concern over pollution and that kind of thing.

DEVRA: Were there people in Congress, specific Congressmen, Senators, who were in this fight or has it been a slow fight?

ENDICOTT: No, I think it was largely or as far as I can recall, decisions made in the executive branch.

DEVRA: I was wondering how much influence people like Ralph Nader and others may have had on this?

ENDICOTT: Really Nader came along later.

DEVRA: Well some of this is Rachel Carson's period, too.

ENDICOTT: Yes, I suppose if you had to pick one thing that happened, her book might have been the most important one single thing. I suspect that it was.

DEVRA: Were there any other major policy achievements or program achievements before we talk about the resource achievements? There all sort of mixed in.

ENDICOTT: They really are hard to separate.

(Etiology vs Therapy)

DEVRA: Maybe we could talk about the promotive influences of some of these activities that you've referred to and some of the obstacles. For example, in addition to your own initiative, what really promoted the strengthening of the biometry/epidemiology, the whole etiologic approach to cancer control? What did you have going for you?

ENDICOTT: I think I was fortunate in picking a handful of key people.

DEVRA: So your judgment was obviously...

ENDICOTT: Well we always had substantial representation of basic scientists on the Council and I think really with the exception a virologists or two and Phil Shubick, we never really did succeed in getting people on the Council who were really strongly motivated in this other area. And I had to conclude at that time at any rate there was very little stomach anywhere in the scientific community for the kinds of things that needed to be done. It is just not an area where you are apt to get a two-way ticket to Stockholm.

DEVRA: There was a lot of that going on? Did you have a couple of Nobel Laureates on the Council in those days?

ENDICOTT: Yes we did.

DEVRA: Wendell Stanley?

ENDICOTT: Wendell was one. He sure as heck supported me. He was very outspoken in getting the cancer virology program launched.

DEVRA: Helped to persuade some of the others that this was vital?

ENDICOTT: He did better than that. He had a number of inspirational talks with Lister Hill?

(Carcinogenesis)

DEVRA: You mentioned Phil Shubick earlier. What kind of a role did he have on the Council in stimulating interest in chemical carcinogenesis as a priority for the NCI?

ENDICOTT: Well, he's an ardent advocate of research in that area. You know Phil, of course, he's oftentimes his own worst enemy. But he can certainly be counted upon to support the area and to bring pretty good critical judgment there on technical issues.

DEVRA: Can you think of some of the other promotive influences and some of the obstacles?

(Smoking)

ENDICOTT: I think probably one of the promotive things we did was to finally convince Luther Terry that he had to do something about smoking and health. He certainly was a reluctant dragon, for a long time.

DEVRA: Let me ask you something. Lester saw Lee Burney yesterday, and said to him, "I have a question of you. I'd like to know why your statement of 1958, your Surgeon-General Statement, had so little impact?" I haven't seen his reply, but it is rather discouraging as we look at this history to realize that it just takes forever and forever for some things to sink in. Now you say Terry was a reluctant dragon, and the pressure was coming from where to push him to do something more than what obviously wasn't happening already?

ENDICOTT: Pressure was coming from the ACS, American Heart Association, American Public Health Association, and I remember at the meeting of the Board of Directors of the ACS, I just barely headed off a vote of censure, promising that if they lay off, I'd get back down to Washington and persuade them to do something.

DEVRA: They had been appealing through conventional routes--lobbyists, friends, and so on--and he wasn't responding?

ENDICOTT: No he wasn't.

DEVRA: Was he a member of the Board himself?

ENDICOTT: No, I was.

DEVRA: This was maybe in 1963, 62? So you brought the message back to him that things weren't good in the Cancer Society?

ENDICOTT: And convinced him that he ought to set up the committee or commission.

DEVRA: Did you staf that commission? People from your office?

ENDICOTT: Not entirely, but we supplied the bulk of the staff.

DEVRA: Now, that's a very interesting vignette as a matter of fact. He wasn't ready really to take a public position stronger than what had been taken say in 1958?

ENDICOTT: That's right.

DEVRA: That was a way to gain time. And during that time what else would happen? Deals with the tobacco companies?

ENDICOTT: No, not really. I don't really think that the commission came up with one iota of evidence that wasn't already at hand.

DEVRA: But the times were different.

ENDICOTT: A little. A blue ribbon commission, backing up the pronouncements I think was probably useful, but the British had already done that.

DEVRA: We have plenty of research here. Research paid for by the NCI, by the ACS.

ENDICOTT: But somewhere along in the middle of that Terry became a convert. I think one has to remember, of course, that the Government itself was in an awkward position.

DEVRA: Why, because they were subsidizing the tobacco farmers?

ENDICOTT: Yeah. Tobacco is still a major industry, and there are some states that are practically dependent on it. We get a hell lot of revenue out of it.

DEVRA: Federal Governments, State Governments.

ENDICOTT: And tobacco industry was well organized. It raised noticeable lumps on your head every time you...

DEVRA: Did they attempt to do that to you?

(Tobacco Lobby)

ENDICOTT: Oh yeah. They'd always be troublesome at the time of appropriations. There were two Congressman on the appropriations subcommittee in the House who always came to the Hearings loaded with questions prepared by the tobacco industry. And we had plenty of trouble with interstate and foreign commerce committees. Quite hostile hearings.

DEVRA: There are several Southern Senators on that committee?

ENDICOTT: Oh sure. And it really was a drawn battle to get the first warning label on the cigarette pack.

DEVRA: Do you remember any specific events that really led to that decision finally? Any decisive events?

ENDICOTT: I don't really recall whose idea it was to do that. There were certainly some fascinating hearings on it, though. On the cigarette smoking, lung cancer issue, I think one of the interesting things we got going during the 60's was the first time a program with the tobacco industry aimed at developing a less hazardous cigarette.

DEVRA: Whose initiative was that?

ENDICOTT: That was mine.

DEVRA: That this was the way to go. The other things might work but

ENDICOTT: It could be a long, long time before tobacco smoking disappeared, and we shouldn't put all our chips on an exhortation. It would be a good idea to see what we could do to make it less hazardous.

DEVRA: How much money did the NCI put in it originally, because I gather the industry was putting money into it also?

ENDICOTT: You're going to see Carl Baker, aren't you?

DEVRA: I did and I'm going back to see him again. Would he know about the dollars? What about occupationally induced cancers? What rol did the NCI play in stimulating greater attention to bladder cancer among certain workers or lung cancer among others?

(Occupational Protection)

ENDICOTT: I think the most direct thing that we did was done under Paul Koten. And it's probably spun off to the NIEHS, I really don't know now. The two of us discussed this a lot about how to tackle a problem. There still is, I'm sure, a tendency on the part of the industry to deliberately ignore occupational hazards for economic reasons. We felt that there were excellent research opportunities in selected occupational settings if we could just get access.

DEVRA: So you tried to put out some "carrots?"

ENDICOTT: Yes. And my feeling was too that if you could really get big outfits like the steel industry and the automobile industry and so on interested in defining their hazards, you've taken a first importance step in getting them to do something about this.

DEVRA: Did you offer them incentives such as grants or contracts and technical assistance?

ENDICOTT: It was primarily technical assistance and this was one of Paul Kotin's great talents--getting into a place and working out with them how to maintain surveillance and potential high-risk occupational groups.

DEVRA: Did he succeed with a number of major employers?

ENDICOTT: He did. Steel industry, automobile industry, and some of the aerospace industries, even Proctor and Gamble. He knew how to talk to them.

DEVRA: Larry Agran, whose writing this section, has talked to him but after reviewing these notes, he might want to see him again. Dr. Hueper had left the NCI when you came in. He was still around? What was he doing?

(Hueper)

DEVRA: You brought Paul Kotin in above him, certainly?

ENDICOTT: It's very interesting. Hueper was approaching mandatory retirement. I went to him and said who is the best man in carcinogenesis in the U.S.? And he thought about a while and said Paul Kotin. I then asked him if he'd help me, and he said he would. And he did. I think he sort of regarded Paul as a protege.

DEVRA: Had he trained in his laboratory?

ENDICOTT: I don't think so.

DEVRA: But he did respect him?

ENDICOTT: Oh yes, very much. Once Paul arrived, I think Hueper was a little chagrined. He's a very proud man. But he was instrumental in getting Paul there.

DEVRA: It never occurred to Hueper to provide technical assistance to these industries, did he?

ENDICOTT: Well, Hueper was much more inclined to a laboratory study which he thought was conclusive and a lot of people wouldn't agree with that. And then going after them with a meat ax, he'd go after industry.

DEVRA: So that alienated him from the places where replications of this research would have to go on?

ENDICOTT: You see, he was ready to "lower the boom" on them before others would concede that the evidence was there.

DEVRA: Of course the NCI didn't have any regulatory powers?

ENDICOTT: No, it didn't.

(Regulatory Agency)

DEVRA: I've always been intriqued why a man who was as much of an activist as he appeared, certainly in his writings, was content to stay in a non-regulatory agency. Of course, I'm probably naive, maybe there was no other regulatory agency at that time.

ENDICOTT: What little regulatory power the government had was so diffuse, that it wasn't really very much. A good example would be the agricultural chemical area. There was a little piece of it in the FDA, had and a little piece of it that the Department of Agriculture had, would be hard to pick a place to be where you really had much leverage. But Heuper was a genius when it comes to ferreting out carcinogenenic agents. He really had a genius for that.

DEVRA: So he did make a contribution?

ENDICOTT: Oh yes. Unquestionably. He's a great man.

DEVRA: How about the food additive carcinogenetic link? People talking about that even in the 60s, getting suspicious?

ENDICOTT: I suppose that major things occurred in the 60s. Cranberries. Aflatoxins and cyclamates.

DEVRA: What position did the NCI take in these? Assistive, probing?

ENDICOTT: Probably the one that I'd pull out, the place where we played the most active role, was in the area of aflatoxin.

DEVRA: That's when we found it in the diet of the fish, with liver tumors, fish in California?

(Cyclamates)

ENDICOTT: Abbott carried the ball with cyclamates. I really thought that was heroic. Miller didn't make any attempt to suppress the information. Because they really brought the evidence forward themselves. Miller of Wisconsin. I know he came to me and laid the evidence before me.

DEVRA: Were they distressed since they were producing this?

ENDICOTT: Oh yes. They were distressed but so far as I could see they did not make any effort to suppress the evidence. In fact, I'm sure that he and Abbott took the initiative and carried the evidence to the FDA and laid it on a desk. He came and asked me what he ought to do, and that's what I advised him to do.

DEVRA: Of course the battle is still going on.

ENDICOTT: The research was his, and the credit is all there.

(Aflatoxin)

DEVRA: What about the aflatoxin one? What did your people do?

ENDICOTT: There again the best one to talk to would be Paul Kotin.

DEVRA: That happened while he was there?

ENDICOTT: Yes. He provided quite substantial resources

DEVRA: ...to the fish meal people?

ENDICOTT: Yes. Ran the thing down, pinned it down as I remember it to peanut meal.

Paul Kotin was in full charge of that. He kept me informed, but I wouldn't want to take any of the luster away from him.

DEVRA: One thing I remember is that happened fast?

ENDICOTT: It did, indeed. He can move fast and did.

DEVRA: Faster in some ways certainly than some of these others that have been diddling around for the last 5 or 6 years.

ENDICOTT: I really hated to see Paul leave.

(Prevention)

DEVRA: What role did the NCI during your leadership play in stimulating greater attention to mobilizing forces focused on prevention of cancer generally. We've talked a little about etiology, but beyond that, in the development of integrated, etiologic resources, what happened?

ENDICOTT: From a resources standpoint, the main thing we did, I think, was internal and it really amounted to putting dollars and space and slots into the area. I can't really point to anything.

DEVRA: Did you attract any good people at that time?

ENDICOTT: Yes.

DEVRA: Just as you were able to attract Paul Kotin, were you able to entice anybody else to really get interested philosophically and practically in the whole concept of cancer prevention?

ENDICOTT: Paul Kotin recruited many excellent people.

DEVRA: I realize that there was an enormous amount of emphasis then on what was happening with treatment because chemotherapy really was making substantial inroads.

ENDICOTT: Other than what I've already talked about, I really can't point to anything very much.

DEVRA: What role did the NCI play in promoting the development of early detection procedures or propagating those such as the Pap smear?

ENDICOTT: I did set up a special program under Eli Nadel. He left and passed it on to Nat Berlin. I really don't know what has come of that whether its fizzled out or whether they still have something going.

(Early Detection)

DEVRA: Was that considered a major emphasis of fundamental research at the NCI? The search for early detection mechanisms?

ENDICOTT: Well the approach really was more applied and development rather than basic. We set up an organization and gave it resources to really concentrate on evaluating and promoting whatever was in sight.

DEVRA: Being developed elsewhere? In industry, anywhere?

ENDICOTT: Earlier efforts that were set up under Rod Heller were, I thought, principally directed toward discrediting phony diagnostic tests. And some worthwhile work was done in that area. But the attitude was, as I would characterize it, as "here we are, we will evaluate what you have. Prove to us that it is not phony."

DEVRA: Rather than putting out some kind of seed money to develop, for instance, a better fibreoptic instrument to detect colon cancer or lung cancer?

(Automated Cytoscanner)

ENDICOTT: Yes. I undertook to turn this thing around and see what the heck we could do in a positive direction. For example, there had been a lot of money put into an automated scanning device for screening Pap smears and so on. I asked them to do whatever was necessary to find out if this thing is a feasible approach or wasn't?. If it is, let's wind it up. If it isn't, let's quit. As a result, they brought in evidence that what it

was counting was clusters of leukocytes. It really wasn't needed. The thing that it did was to find little hunks of pus. Wasn't really detecting cancer cells at all.

DEVRA: Was there any interest in such things as fiberoptic or other sigmoidoscopes?

ENDICOTT: That was an area pushed by Robbins. Nadal was to survey the state of the art in diagnosis and develop leads.

DEVRA: To evaluate what was already being developed or to stimulate development?

ENDICOTT: To pursue leads and to see what he could do with it.

DEVRA: What was his division called at that time?

ENDICOTT: I've forgotten. Maybe Carl would remember. But it was a discrete organizational entity.

DEVRA: And it may have come in part out of what was left over from the Diagnostic Aides Branch.

ENDICOTT: It did.

DEVRA: Ray Kaiser's program. Which was sort of the end of Ray Kaiser in the Federal establishment. That's what he was left with. So it was a reorientation of that program. Instead of being negative, it was trying to be more positive. (See insert on page 15a.)

ENDICOTT: Well, in mammography we laid out a major study with HIP in New York.

DEVRA: How did that come about? Did they come to you?

(Mammography - HIP)

ENDICOTT: No, we went to them. That was Mike Shimkin's doing. He convinced me that before this was sawed off on the public, we better find out if it really was any good. Did it really do anything about morbidity and mortality? Or was it just another gadget? We took the initiative. I think that much of the experimental design was Mike's and it was certainly his decision to put major resources into, there were many bucks he put into that thing. "Mega-bucks."

DEVRA: You believed in this idea though? How about the Council? Did they go for this?

ENDICOTT: We used contract funds which did not require council approval.

ENDICOTT: When Jim Shannon decided to transfer "cancer control" out of NIC and NIH, Rod Heller - Director of NCI - was strongly opposed to the move. He developed an organizational entity - The Field Investigations and Demonstrations Branch - to continue "cancer control" activities in NCI under the leadership of Ray Kaiser.

When I succeeded Rod Heller, one of my first official acts was to abolish Ray Kaiser's Branch. I did so in order to get the activities back into the mainstream where more rigorous scientific standards could be assured and where the activities could feed out of the more generously financed "pots" of research and research training.

The Branch contained a powerful intramural group in epidemiology and Biometry whose skills were urgently needed to develop better and larger efforts in research on etiology and prevention.

Having had a favorable experience in the field of chemotherapy by combining intramural research with a big contract program, I decided to try the same thing in etiology and prevention first under Mike Shimkin and then under Paul Kotin.

DEVRA: So here we are a good example. Did you issue an RFP for this mammography study? Or how did this wind up going to the HIP group in New York?

ENDICOTT: It's so long ago, I really don't recall precisely. The formalities of RFPs hadn't really evolved much then. It was not a competitive kind of thing.

DEVRA: Going around and see who had a big enough population, who had the interests, and maybe the technical competence?

ENDICOTT: As I recall it, Shimkin simply decided that HIP was the best place and to do it. You'd have to ask him about details

DEVRA: And Sam Shapiro, right. But you were proud of that. Felt this was something that was either going to make or break this as an early detection device for cancer.

DEVRA: Why?

ENDICOTT: Because they believed in it. They really did believe in it.

DEVRA: R. Lee Clark had been convinced by Bob Egan that it was really going to save lives? And Lew Robbins was doing these reproducibility studies to find out if local radiologists could do these mammograms. Was this really a battle between the M. D. Anderson people saying we've got something here that we think we own?

(Technology Assessment)

ENDICOTT: No, it wasn't that. They had real missionary zeal about it. It was sincere and honest. To my mind, what Shimkin did was really a pioneering effort in technology evaluation, which is something that is still something that is largely untouched. And it's haunting PHS right now. And Ted Cooper and Don Frederickson and I are spending a lot of time, I suspect, in the privacy of our own bedrooms wondering what the hell to do about it.

DEVRA: The whole idea of how do you evaluate technology?

ENDICOTT: Yeah. Which is pressing on me particularly hard right now, because of the responsibilities we have under the new planning legislation and providing national guidelines. This is powerful charge to the Council and therefore to us in the area of technology assessment in connection with it. And I am also responsible for the National Center for Health Services

ENDICOTT: So somehow between NIH, FDA, and to some extent CDC and HRA are going to tackle this thing. Because there is just no way I can wiggle out from under it. And I don't have the technical competence. We have virtually no scientific competence in the biomedical area in HRA.

DEVRA: But you're being charged now by law with evaluating the effectiveness of certain technologies.

ENDICOTT: I have a political scientist in charge of health planning and resource development. A great guy, Harry Cain, is first rate. An economist in charge of NCHSR. I think Harold Margulies and I and a little handful of guys over at the Bureau of Health Manpower represent our whole competence in the biomedical area and that's not saying very much. So we have to draw somehow on the really tremendous power of NIH.

DEVRA: Are they cooperative? Are they willing to share some resources with you?

ENDICOTT: Well, Don Frederickson is properly motivated, but by and large, the attitude within NIH is, ho hum. "We does our thing and we publishes our papers and the world ought to be God damn glad we're here." And I can't help saying the world should be glad they're here, but they don't have any enthusiasm for technology assessment.

DEVRA: This sounds like a broken record. This is the same story with cancer control. It's all right, let them go downtown because you know, they're applied, and they're kind of fooling around and trying to get things into private practice. And you know, we're lab men. We're the pure scientists.

(Current Cancer Control Program)

ENDICOTT: I did what I could and that was quite a lot to get the Cancer Institute really concerned about solving the cancer problem. Honest to God. I still don't think they know quite what the heck they're doing with the cancer control program. At least that's the impression that I have. Carl Baker, I don't think he wanted a cancer control program.

DEVRA: That's correct.

ENDICOTT: Probably for the next few decades the most important thing they are doing in my view is developing better triage of cancer patients to get them into first-rate places for care.

DEVRA: Well that's not something the NCI is doing on its own. That's something that's come out of the center's mandate in a sense. The system of the centers with outreach.

ENDICOTT: But we already had that. We had a bunch of cancer centers before.

DEVRA: Right. We had Roswell Park and Ellis Fischel and M. D. Anderson, Sloan-Kettering.

ENDICOTT: And McCardle in Wisconsin, and Kaplan and Company at Stanford.

DEVRA: But they were not multidisciplinary in all of them, especially the ones in the West. They were site-selected or they were therapy-selected. If you have Hodgkin's disease, it's fine to go to Stanford. I'm sure if you have several other things, it's fine to go to Stanford, too, but they didn't have equal kinds of competence in all the diagnostic and therapeutic modalities. The movement for centers--that whole concept of centers was already present, do you think, when you were the director?

ENDICOTT: Yes.

DEVRA: It wasn't reinvented in 1971?

ENDICOTT: Had some expansion or something? I can't take credit for it, because they were already there when I got there. During my tenure we launched an organization, I'm sure it still exists, but I don't know what the title of it is now, it used to be The Association of Cancer Institute Directors. Really responsible for getting that off that ground.

(ACS)

DEVRA: How did your office relate to the American Cancer Society?

ENDICOTT: Very closely.

DEVRA: You were obviously on their Board? Some of them were on the Council?

ENDICOTT: We had a very good working relationship.

DEVRA: With the Washington staff.

ENDICOTT: Not the Washington staff. But we worked very closely with the headquarters people in New York.

DEVRA: Were there tradeoffs?

ENDICOTT: Oh yeah. There were many things we did jointly.

DEVRA: Not just conferences and funding of research activities?

ENDICOTT: No. Let's see, what would one be, there were so many. I think probably the most significant had to do with lobbying.

DEVRA: Of your budget?

ENDICOTT: Yes. Now, Rod Heller and I both took a very active role in the American Cancer Society, as best we could in helping them raise funds. An effective working relationship between NCI and ACS in advocating appropriations, I think, clearly was the most significant.

DEVRA: It helped you immeasurably.

ENDICOTT: The other principal area of extensive collaboration other than joint sponsorship of meetings, which we did constantly, was pretty effective blending of our staff resources in the general area of public education.

DEVRA: That's kind of interesting because NCI and NIH generally really never had much of a mandate and didn't really seem to have much of a budget to do public education. Some professional education obviously. Quite a bit of that.

(Public Education)

ENDICOTT: Both NCI and ACS plowed that field pretty thoroughly but it struck me that the ACS because of its enormous volunteer organization really had tremendous power in this area and so my feeling was let them take "center stage" and put our resources behind them. One of the things which you have to understand to work effectively with a voluntary organization is that they have to have a lot of visibility if they are going to get money. And I think a secret of really effective working relationships between the Society and NCI is to let them take the lion's share of the credit and don't get uptight about it. Because they reciprocate by pushing for a big budget for NCI. Okay, let them get the headlines, what the hell. I used to have an awful time with that because they continually annoy our public relations people.

DEVRA: Because they were getting so much...

ENDICOTT: Yeah, we'd jointly sponsor something. ACS would be prominent, way down here, in tiny print would be the NCI. We'd be putting in 90% of the budget. I just laughed about it.

DEVRA: They have to go out and raise money, too. Do you think there were any people on the ACS Board who particularly thought the government, NCI, even in their public education mission was slow, on the cigarette issue, early detection, and so here they were in there doing all the work in a sense, or they were doing what they felt was the government's work.

ENDICOTT: No, I don't think so. Clearly the example I cited to you of the ACS Board of Directors almost censuring the Surgeon General is a good

example of their impatience. I think they really felt this was their bag. There were some pretty shrewd, level-headed people there. I think that the ACS was sort of uneasy about the giant that the NCI was in terms of the resources it had for research. Clearly, they were never going to raise that kind of money.

DEVRA: They say in a lot of the annual reports that of course when a cure for cancer is found, they'll go out of business. Do you think they really believe that? Either part of that statement? One, that there will be a cure for this whole battery of diseases, and two, that they'll go out of business. Or will they get like the T.B. Association?

ENDICOTT: _____ Anyway, I don't think it's a cause for immediate concern because it's not inminent. I really think that the relationship between the NCI and the ACS is sort of a model.

DEVRA: There isn't anything like it that I've found. I don't know whether the Heart Institute behaves the same way with the American Heart Association.

ENDICOTT: I don't think so. The groundwork, I can't take credit for. The groundwork was really laid by Rod Heller. I just came along and played a very active role.

DEVRA: Did he give you some points on how he had done it and how to keep it up?

ENDICOTT: No, I don't think we ever really discussed it.

DEVRA: His predecessors really didn't pay much attention to the Cancer Society? Spencer, Scheele.

ENDICOTT: Rod was primarily a "public healther." He was thoroughly at home in public health, was sort of uneasy in a research environment. But in a public health arena he was thoroughly at home and a real pro. I doubt if I'd have had enough common sense to really stroke the relationship, if he hadn't gotten it started. It was really easy for me.

(American College of Surgeons)

DEVRA: How did your office relate to the American College of Surgeons?

ENDICOTT: Well, we had very good relationships with the American College of Surgeons. More than any other one thing, I attribute this to a close personal friendship I had with Ravdin.

DEVRA: IA. Was he the president in those days?

ENDICOTT: No, I think he had already gone through that stage and was on the Board of Regents. I went to Ravdin and persuaded him to become chairman of the clinical panel in the chemotherapy area. Out of that grew all sorts of joint effort things we did with the American College of Surgeons.

DEVRA: Did the College testify for regular appropriations?

ENDICOTT: No, I don't think. Ravdin did, but it wasn't in that capacity. Things that we worked on together were to reinforce their efforts in upgrading registries and establish criteria for various levels of sophistication on the part of hospitals. This carried over and blended into RMP. A number of those enterprises were underway before RMP was spawned.

(College of American Pathologists)

DEVRA: What about the College of American Pathologists?

ENDICOTT: That was I think sort of minimal. Probably Lew Robbins and his cytology staff worked with them more closely than we did.

DEVRA: Did they bother you?

ENDICOTT: Oh no. The door was always wide open. _____ But really I can't put my finger on anything much. We did support the field through various enterprises with the AFIP, that was funded through the National Research Council. Tumor registries and all that kind of stuff.

DEVRA: Did you have any big fights with them?

ENDICOTT: Oh, no.

DEVRA: The big fight had already taken place over what to do about the Pap smear.

ENDICOTT: I can't recall we had any hassles with them. Matter of fact, I think we've had a very warm relationship so far as I can recall. I can't think of any animosity. I can't on the other hand put my hand on anything very positive. (Endicott was originally a pathologist.)

(American College of Radiology)

DEVRA: How about the American College of Radiology? Because you mentioned that you got the HIP study going.

ENDICOTT: I personally had very active relationships with the whole radiological community. Not all, but mostly. Henry Kaplan convinced me that we really had to do something about therapeutic radiology and we did. We established a major training effort in therapeutic radiology.

It did create kind of a running battle with Dick Chamberlain and that element of the radiology community that was convinced that they ought to stick with general radiology.

DEVRA: Diagnostic radiology?

ENDICOTT: The general radiologists got very little training in therapy and the rest of it was diagnostic. Even after all these years, I still will meet with the radiologists now and then, and we will mutually hold hands. This is an area in which Paul Kotin's wife, Pauly Stephan, played a very active liaison role with the radiology community. We undertook a variety of things. Not always the American

College. You know that's kind of a fragmented area. We put resources and efforts into second generation hardware, into defining what a center really ought to be, what kind of hardware and staff you need, what kind of a population it could serve. There were things bubbling and boiling all the time. That was a very active area.

In international cancer control area, I think that is about the size of it really.

(UICC)

DEVRA: Do you still maintain some involvement with the IRAC. No. What about the UICC? Were you involved with that too?

ENDICOTT: Tangentially. There were a couple of key people in the Institute who really devoted a lot of energy there. I managed to give it a lot of support by making it possible for Murray Shear to devote essentially full-time to it. My motives were mixed. (I had to get him out of the laboratory for one thing), but we did put a lot of effort especially through Murray. He and Harold Dorn were prime movers in this. And I always encouraged it, but I didn't take an active part myself. The Cancer Institute though, when I left there, was still a major source of support for the International Union.

DEVRA: I haven't asked you at all about the RMP period. I have a question I want to ask Carl about. I guess maybe I'm a little mixed in my own understanding of where you people were. Your tenure really ended at the time

ENDICOTT: 1969.

DEVRA: 1969? Well RMP had already been created? Had cancer control already been sort of slipped in to RMP. Bill Ross's program that was downtown.

ENDICOTT: Yeah, that occurred during my tenure.

DEVRA: Did you have any feelings about that or were you even asked to express them?

ENDICOTT: No. The RMP didn't really work very effectively with the Institute.

DEVRA: Even when it was within the NIH?

ENDICOTT: Even when it was in NIH. I was never very keen about RMP.

DEVRA: How did you perceive RMP in relation to this sort of familiar established program priorities?

(RMP)

ENDICOTT: I thought it was kind of an abortion from the beginning. I made a very strong plea to the DeBakey commission to support the idea of regional cancer centers.

DEVRA: How did that go over?

ENDICOTT: Well, it was before the Cancer Panel that Sidney Farber was chairman of. It went over with a bang.

DEVRA: As long as it had Regional Heart Disease, Stroke, and Cancer Centers?

ENDICOTT: That was the beginning of the end of the whole thing I think. Because they began to mix apples and oranges. And they are really three different areas, and it just didn't make sense.

DEVRA: You really felt there would be more integrity if the categorization could be maintained? That they were different diseases, treated by different people?

ENDICOTT: You know you can move a cancer patient quite aways, but a coronary is another kind of an animal. And the stroke victim still another kind of problem. Well by the time all the cooks in that broth got through with it, by the time it emerged from the Congress, it was a "swoose." It was not swan and it was not goose, it was a "swoose." It was a "swoose" to its dying day, which hasn't quite arrived yet, but its imminent.

DEVRA: Did it siphon resources away that might have gone into NIH, into other things?

ENDICOTT: No, I don't think so. Matter of fact, probably got resources that wouldn't have otherwise ever been created.

DEVRA: So it did some good perhaps.

ENDICOTT: I was afraid that it might siphon away funds at first. But it became obvious that it wouldn't. It was a discouragement to me that it really had so little come out of it that was relevant to the cancer field. It seemed to have been primarily focused on cardiovascular disease.

DEVRA: Then RMP got into the health services area, which of course really overlapped what you were involved in (health manpower), and that made life rather unpleasant, I am sure--confused if not unpleasant.

ENDICOTT: I must say that in spite of all the bad marks that people have given RMP, I have found that they were putting their fine Italian hands in some pretty splendid enterprises. Because its mission was vague, though, and suffered from constant change of leadership or lack of it.

DEVRA: Do you think it was a victim of all this reorganization and something that maybe it was innovative? We always scream about innovation. We don't even see it when it hits us sometimes or we don't know how to handle it.

ENDICOTT: I think it was a born target for a Nixon administration. One that tried to diminish the Federal load and look around for weaklings and knock them off.

(Cancer Control Progress)

DEVRA: Would you like to evaluate the progress of cancer control during your tenure or indicate your recommendations for the future?

ENDICOTT: I don't think we got very far.

DEVRA: What about the cooperative clinical trials?

ENDICOTT: I think we've made some major progress in developing organized target research in the cancer field during my tenure, . . .
but I really can't really. . .
claim that I did anything for what is normally identified as cancer control.

DEVRA: Or what we call it now. What about the environment in those days, within the Institute though. You brought in good people. You stimulated good people. The philosophy may not have been what we call classic cancer control.

ENDICOTT: I think we laid a good framework on which you can build on for the future.

DEVRA: And you certainly did something in the environmental health area.

ENDICOTT: But I'm basically a research guy. My contribution is in the kinds of things that a research guy would be broadly interested in. I don't think by that time I had any hang ups about the virtue of basic research. I did at the outset. I was just as biased a guy as anybody you'd find at first. But I had gotten over that pretty much by 1960.

DEVRA: But the cooperative clinical trials and the chemotherapy program in a sense, were applied research. That, you considered a natural progression. But demonstrations of early detection techniques or strategies to inform people that smoking was hazardous to their health, how did you perceive those, apart from the fact that they weren't in NCI, did you ignore them entirely?

ENDICOTT: No. I didn't feel they could be ignored. I wanted them to prosper. But basically I don't think I really had anything too much to contribute in that direction.

DEVRA: What about other members of your staff or other resources that you had? If Robbins came to you, and I don't know if he ever did--he did?, what kinds of things would he ask for?

ENDICOTT: I think he really was on the alert for something you could give. But I didn't have anything much for him.

DEVRA: Was he discouraged do you think?

(Robbins)

ENDICOTT: No, no. If he was I wasn't aware of it. I think he was disturbed by some of the organizational shuffle through, that sort of thing. He was a stout hearted fellow. He didn't give up easily.

DEVRA: One of the things he got working on toward the very end of his tenure, about 1964, was to get somebody to work on a 6 inch fibreoptic Sigmoidoscope, to use in lower colorectal cancer diagnosis. Was that idea brought into the Institute as something that maybe the Institute should work on, or was it taken out into industry generally?

ENDICOTT: I don't really remember. I kind of think maybe he persuaded Eli Nadel to put some dough into it, but I don't remember for sure. But I do remember his keen interest in it.

DEVRA: And here we are, 11 years later, and what do we have? Well, we got a colonoscope finally and we got a heminocult, hemotholte, that may or may not pan out. There hasn't been very much glamor or enthusiasm for people to work in early detection or in prevention. I don't think everybody is looking for Nobel prizes, but somehow that has just not become something very appealing.

ENDICOTT: No it hasn't. I thought on the plane this morning about what my recommendations for future directions are. I really don't have any.

DEVRA: If you were czar of cancer control today?

ENDICOTT: I would never had taken the job back then unless I had some pretty clear cut ideas about what I was going to do about it. But I really don't have any red hot ideas in this area today.

DEVRA: You have enough integrity to admit it. That's very healthy.

ENDICOTT: On the other hand, in this job, I can see lots of things.

DEVRA: Health resources administration. When you first came here though, it must have been a big puzzle?

ENDICOTT: Oh, what a can of worms. Still a can of worms. But you know it takes a while to launch an agency out of pieces of other agencies and get really working together. We've got a long ways to go yet. But we have some interesting resources. We've got awfully fine people, including a gal I stole from the Social Security Administration. And we've got a good bunch of people in Health and Manpower. Still have a lot of things to be done and finally winding up a bunch of previous reorganizations and disposing of all the leftover parts.

DEVRA: You going to be reorganized again? We're going to have a national election this fall.

ENDICOTT: I don't think there is any end to reorganization. If I had to guess where the next big reorganization is going to come, I'd say it's in the interface between Public Health Service and the Social Security Administration. That's long overdue. Just how it's going to come out, I don't know. But I don't think that HEW can afford the luxury of forever of having things the way they are.

I N T E R V I E W

Interviewee: Dr. Wilhelm C. Hueper, M.D.

Interviewer: Larry Agran

Location: Bethesda, Maryland

Date: December, 1975

Larry: When you wrote your book, published in 1942, Occupational Cancer and Allied Diseases, you worked on that I believe you told me for four years, is that right?

Hueper: In 1938 I started writing the book and collecting the materials, after I was separated from DuPont.

Larry: What were the conditions of your separation from DuPont?

Hueper: One nice day I was called into the office of Dr. Gehrman,* the medical director and his assistant medical director, I don't recall his name but he was there too as a witness, and I was told I was dismissed for economic reasons. Now, at that time, we still had a sort of depression (an economic depression) and that was at least an apparent excuse for getting me out, :

He had about a half-year before I was informed by one of my co-workers who had personal connections with the editor of the local newspaper in Wilmington, Delaware, received information from Dr. Gehrman that he and Dr. Von Etting, the director of the Haskell Laboratory had succeeded in producing cancers of the bladder in dogs and that thereby the causation of the bladder cancers in dye workers had been solved. Now, this man had worked with me as my informant and he knew exactly what I had done. Yet, Gehrman had not the slightest idea of what I was doing. He had hired me for that purpose because I was told that when I came in November 1934 that I was specially hired to solve, for the DuPont Company, the problem of the bladder cancers in dye workers. Naturally, that had somewhat of a preceeding history. Two years before I had written to Mr. E. I. DuPont, whom I knew personally at that time, that from my observations after visiting the dye works, I had come to the conclusion that their workers would have the same cancer hazards to the bladder as similar workers in European plants, especially Germany, Switzerland and England and that an investigation would show that these men have an increased incidence of bladder cancer. I didn't get

* Spelling not confirmed.

any personal answers to this. My boss at the cancer research laboratories at the University of Pennsylvania told me several months later that they had come to the conclusion that they had no cancers among their workers. "Well," I said, "that may be, but they would get them." Then about four months later, suddenly Gehrman and the research director of the research station came to us in Philadelphia and said, "We have some now." I said, "How many?" and he said, "We now have 26." I said, "You will have more. This is a going concern now." They were very apprehensive, especially the chemical research director because his brother had been the manager of a big dye work operation and he had contact and it would take a while before he developed cancer. At that time I had already figured out that it would take about 15 years. I told them that men who are getting cancer now are those who your company employed in 1917 when they created the dye work operation.

Larry: What do you suspect was the real reason for your termination?

Hueper: I insisted that such observations be published. My philosophy of controlling cancer hazards in industry was fundamentally different from that of the DuPont Company. The management at that time took the view that such observations were strictly the business of the managment and didn't even need to be directly communicated in all their tragic implications to the workers. My viewpoint was that as soon as the management became aware that a possible cancer hazard might exist in any of the operations, the workers should be informed why control measures were being taken so that they could get the full cooperation of the men. The interesting part was that in 1928, some communication on cancer hazards in American industries were published and it had referred to possible bladder cancer hazards and had come to the conclusion that the average American workers was by far too intelligent and curious to stand for any health hazards related to occupational exposures.

Larry: I'd like to bring you back to the conditions of your termination.

Hueper: As I said, I was called in and was told that I was dismissed towards the end of 1937. So, I said that they should giveme two months' time so that I could get a new position, which they graciously consented,

and I stayed with the DuPont Company until the first of February in 1938. Sometime in late spring of 1939, I got indications from the American Director of the International Union Against Cancer that there was a meeting taking place, in 1939, in Atlantic City and I was asked to present my findings on bladder cancer before the International Union. I said I would gladly do that, and in the meantime the findings (on my work on bladder cancer at DuPont) had been published. I was astonished.

Larry: Published by whom?

Hueper: By the Journal of Industrial Hygiene and Toxicology.

Larry: Why were you astonished that they published it?

Hueper: The DuPont Company was financing parts of the journal.

Larry: That is astonishing. And they didn't kill it?

Hueper: No. I expected they would kill it because I had sent in the paper before I was fired. That was published in 1938 and then early 1939 I got a letter from Dr. Gehrman in which he wrote that I had promised in my contract not to publish anything that I had seen or worked on at the DuPont Company, and if I failed to comply, they would take the proper legal methods to enforce that. I went to my director at the Warner Institute for Therapeutic Research and showed him the letter and said, "What shall I do? I would like to show them my teeth, but I don't know what it would be worth right now." He said that I'd better not do that because I couldn't fight DuPont because they can buy any witness. I went back and talked with the director of the International Union and I told him that I regretted very much, but I had to withdraw my paper. I asked him if he had had any contact with Dr. Gehrman and he said no, that they had no way of finding out unless he was on the program. He was one of the clean-minded scientists.

Then I had worked with them on the sudden deaths of dynamite workers in American dynamite plants including the DuPont Company and there was the Hercules, and Atlas dynamite companies. In all three of them, they had sudden deaths on summer days with hot weather. We had a conference with

the German dynamite workers and we presented our evidence. These men died suddenly, like a coronary death, either on Sunday afternoon or Monday morning when they entered the plant and that was due to their contact with an ingredient in safety dynamite which produces a lowering, as all nitrides do, of the blood pressure. I had gotten the tissues of some of the dynamite workers who had died at other plants and I had found that these men, who were usually middle-aged, had severe coronary sclerotic changes of the vascular system of the brain. They were buried with the diagnosis of arteriosclerosis and hypertension. These findings were never published. The DuPont Company, at that time, sent shiploads of explosives to Japan because the Japanese had invaded China and needed a hell of a lot of explosives. Some of these men had symptoms of heart irregularities and abnormalities of the electrocardiograph. Sometimes they were taken out of such nitroglycerin operations but when the demand for explosives was too high, some of the men were sent back to help out. In one case I was asked by Dr. Gehrman whether I would approve of this, and I said no I wouldn't because the men had diseases of the vascular system and when they get into the old environment they would die, and sure enough about three weeks later the men were dead.

Larry: Do you think that they were fearful that you would implicate them?

Hueper: Well, they probably didn't like the idea that somebody who did not approve had that knowledge. Then I was asked not to take along any slides or records I had prepared during my work, which I did do because they were my copies; they were not their copies.

When I was sent to the Warner Company, I had worked with organic nitrides which the Warner Company had developed and was somewhat in the general character similar to a nitride which had developed in rabbits and rats and showed similar effects. In the production of that paper, I said that the dynamite workers who had developed chronic nitride poisoning may die under such circumstances with that methodology.

One man whom I had worked with at the Haskell Laboratory later told me that there had been a good laugh session of the executives of that particular company regarding the aspects of my publication, in order to find out whether they should prosecute me; they came to the conclusion that that might not pay off. It surely couldn't have paid off because the publicity would have been too great and so that phase faded out.

Larry: Let me get into this, you worked on your book from 1938 through 1941 I take it?

Hueper: That's right, yes.

Larry: Did you have employment during that time?

Hueper: Oh yes. I was the assistant director of the Warner Institute for Therapeutic Research.

Larry: When would you work on the book, evenings?

Hueper: Oh no. I could work on it during my regular work hours. The Warner Company did not expect me to work on any commercial products.

Larry: Which library did you work at to get your materials for the book?

Hueper: While I was still in Pennsylvania, I went to Philadelphia and used the Library of Physicians. They have a very good library.

Larry: You were able to get almost all of the materials you needed?

Hueper: Yes, I got a lot of it. It was very helpful and when I gave the library the listing of all the things I needed, the next day they had everything already lined up for me. Later on, while I was in New York, I went to the library of the New York Academy of Medicine.

Larry: Your book came out in January of '42, is that right?

Hueper: That's right.

Larry: Did you harbor any expectations that the book, because of its depth, would turn things around in relation to the impending cancer hazards that you wrote of?

Hueper: That was during the war so the sale of the book was not impressive

at all. It was a difficult time to try to interest people in the loss of life. The importance of the book, as it was finally termed a classical book, appeared only after the war was over for several years. None of my books had good sales.

Larry: Were you disappointed at that?

Hueper: Yes, I was. Naturally, anybody hopes that their books will sell. After a while, I later published books with the realization that they would draw only a limited amount of sales. I wrote them in order to put the evidence into print so that no medical director of an industrial company, or attorneys, could confront the courts with the claim that it was not published.

Larry: Let me bring you to another publication. In 1964, the World Health Organization published in its Technical Report Series a pamphlet entitled Prevention Of Cancer: Report of a WHO Expert Committee. You were of course one of the committee members.

Hueper: It was not "of course", I had trouble in getting there.

Larry: You did? Why don't you tell me about that, who was trying to prevent you?

Hueper: I haven't found out. It was not government money. WHO paid for that. It was no money question at all. I have experienced that several times.

Larry: Let me get into the substance of this a little bit. One of the concluding statements here, actually the outset, is that, "The types of cancer that are thus influenced directly or indirectly by extrinsic factors collectively account for more than three-quarters of human cancer. It would seem therefore that the majority of human cancer is potentially preventable." As this was being written, was this regarded as a major statement? A new kind of turning point in the whole question of carcinogenesis?

Hueper: I had said that for years.

Larry: You had, but what about the others?

Hueper: No, they did not, because they did not recognize what kind of a problem we had.

Larry: Was there much dispute among the six or seven of you?

Hueper: No, there was not much dispute. The only interesting part to me was when a lady from Canada, who was meeting with us in the United Nations Building, said to me, "Dr. Hueper you testified against your own country." and I said, "When this conference was opened, the chairman said that we were not representatives testifying for our own countries, but that we were testifying on the scientific public health problem and this I did. For your information I could give about the same type of incriminating evidence for Canada, but I won't do that because I am leaving that up to you Canadians."

Larry: What was the response?

Hueper: She was silent.

Larry: When this report was written, was there a feeling among you that this was a very significant statement?

Hueper: Yes it was.

Larry: So you and the committee concluded its work there with the feeling that in fact this was a very significant statement?

Hueper: Yes it is significant. I quote that many times in my book. When the language was adopted by the committee, Dr. Sabad of the Soviet Union said to me, "Isn't it a great source of satisfaction to you that the committee has come to this conclusion?" I said, "Yes, it is indeed."

Larry: I note that you met November 19-22, 1963. Was your work interrupted by the news of the assasination of the President?

Hueper: No, not at all. The only thing we paused for was to give a brief memorial. They were all very shocked. I was not shocked.

Larry: Why weren't you very shocked?

Hueper: I had a very poor opinion of Kennedy. (Criticism of Bay of Pigs followed).

Larry: The question of bioassays for carcinogenic properties remains. At what point do you feel the techniques became sophisticated enough so that bioassays for carcinogencity should have been incorporated as a part of public policy? In other words pre-testing as a part of public policy? Were the techniques at hand, for example in 1950?

Hueper: Oh yes, I think they were at hand in 1942. When I wrote the book, there was already several years of intensive work on chemical carcinogenesis and experimental carcinogenesis on hand. They had not extended that to the various substances which were suspected. In 1942 we knew relatively little about occupational cancer and that was the reason why I could write the book.

Larry: Perhaps the highest expression of the usefulness of this approach came in this country by way of the Delaney Clause in 1958, yet if you look at public policy since 1958, for example in the pesticides area or in the occupational health area, the legislative expression of the Delaney Clause was not translated or transferred to these other areas; so you have from 1958 to at least the present a situation where the Delaney Clause has not been transferred to other areas. How would you explain that?

Hueper: One reason is that the Food and Drug Administration never approved of the Delaney Clause and it still doesn't. The other outfit which doesn't support the Delaney Clause was a special committee of the National Academy of Sciences.

Larry: So the F.D.A. did not?

Hueper: The F.D.A. did not.

Larry: How about the National Cancer Institute? Were they vigorous supporters of it or not?

Hueper: Up to 1952 they were totally against it. After 1952 they let me testify. They never took an official position. They left that up to the persons who were invited to testify. After 1956 I testified again with the consent of the National Cancer Institute and Public Health Service, which I did not do in 1952. In 1952 I testified as a private citizen.

Larry: Who granted the permission from NCI later when you testified?

Hueper: Well, that went through channels from the head of the National Cancer Institute.

Larry: From what you've told me and from what I have been able to gather, there has not been enthusiastic support from the National Cancer Institute for your work?

Hueper: No there wasn't.

Larry: Why was that do you think?

Hueper: My work led to political difficulties. It is easy to work on genetics or on viruses or on bio-chemistry. There are no implications whatever. And there are no political difficulties. My work directly confronted them with the problems and that was one of the reasons why the Surgeon General kicked me out of the epidemiological study and restricted me only to experimental work.

Larry: Was that Scheele at the time?

Hueper: Yes. Dr._ .later on told me repeatedly that as I continued to work on this that nobody would recognize me. Dr._ _was Associate Director of NIH and later on the Associate Director at NCI.

Larry: How about your relationship with Dr. Scheele?

Hueper: Well, I scarcely knew him. I met him soon after I came, but then he became Surgeon General.

Larry: How about your relationship with Dr. Heller?

Hueper: That was quite good. Heller was a man who was quite flexible. He was nice and gracious. He had a peculiar intelligence. He could write a paper on cancer without really knowing anything about cancer. He was approachable.

Larry: Was he supportive of your work? Did he feel it was important?

Hueper: The officers, by inclination or necessity are pragmatists and they would only accept an approach as long as it did not interfere with their own prospects for promotion to positions.

Larry: When you were at NCI did you find that the budgetary support for your work was adequate or inadequate?

Hueper: It was inadequate. I asked several times for the creation of a special division for environmental cancer in a special building outside of the reservation because we cannot work with carcinogenic materials in such a big clinical center.

Larry: What was their response?

Hueper: No response. I had the minimum support they could give. My budget was $120,000 and I had a staff of 14 people. After I left, they developed a budget of over $2 million dollars.

Larry: On page three of your most recent letter to me, you've written that at one point you called for the creation of a separate environmental cancer institute.

Hueper: I wanted ultimately to have all of the environmental cancer problems dealt with in this organization. The main reason for that was that

it takes experts and facilities to do a real job, and such organizations could turn over such problems to one central place where they could be dealt with most effectively and reliably, and probably most economically.

Larry: Do you think that it is still a sensible idea?

Hueper: Yes. I think they are doing apparently nothing. For example, the National Cancer Institute does nothing on occupational cancer. I cannot tell you my source of information, but I have that information. The National Institute of Occupational Health and Safety cannot do anything because they have no staff or facilities for that. All they can do is epidemiology and chemistry of the chemical environment in industry.

Larry: Again, in your most recent letter, you wrote that you continued your epidemiologic work on lung cancer among chromate workers by joining with Dr. Mancuso. Then you went on to say that during this early period you also began your successful demonstration of carcinogenic properties of pure metallic nickel. The first steps were taken in creating three areas with special industrial cancer hazards from petroleum products in Southern California and so forth. Were all of these activities going on after you were relieved of your responsibilities in 1952?

Hueper: Well, I had started part of that already before. I was a consultant to the chromate industry when I went to the Cancer Institute and I brought that connection.

Larry: Was there, in fact after 1952, something in the way of a barren period, a desert in the whole area of carcinogenesis because of your being relieved of your responsibility?

Hueper: Yes, because my work in the area of carcinogenesis was separate from my epidemiologic work.

Larry: Why do you believe it took roughly up until 1950 for the scientific community and the policy makers to recognize the value of testing animals first as predictors, rather than using animal tests simply to confirm what

had already been tragic toll taken among human beings? Why did it take so long?

Hueper: Well, I think part of it was ignorance of the conditions of the experimental work and then lack of the appreciation for the practical importance of that type of work. In part, I think there was an attempt to simply throw the blanket over it.

Larry: You are probably aware of the pending Toxic Substances Control legislation.

Hueper: Not very well.

Larry: Well, there is a pending bill which would, if enacted, require pre-testing on most commercially significant chemicals. It has had an extra-ordinarily difficult time gaining acceptance and final passage, and it is in doubt now as to whether it will be finally passed. One thing that has occured to me is that one of the reasons that this kind of thing is so difficult to get passed is that people in positions of authority recognize the implications of something like this. For example, if you have a bill like this which becomes law and then you routinely run tests and you begin to discover things causing cancer, which have never even been suspected as being cancer causers, then suddenly you are confronted with the major decision and now something has to be done with it. You either have to sweep it under the rug or you have to make the hard decision to do something or do nothing about it. It makes bureaucrats very uncomfortable.

Hueper: It is my belief that the government will only do what it is forced to do and so does industry. And, in some instances, if they can avoid the law, they do that cheerfully.

Larry: Who do you regard as having been the person in NCI most supportive of your work?

Hueper: Red Stewart.

Larry: In what way?

Hueper: Red Stewart saw to it that I was invited to meetings and he was also involved in pointing out to the Public Health Service how important my work was.

Larry: What was Red Stewart's position?

Hueper: He was the chief of the Pathology Branch and the Pathological Laboratory of the Clinical Center.

Larry: What might you do if you were designated the cancer control czar of this country and you were given authority to do whatever you desired by way of presidential or executive order? How would you proceed? In other words, if somebody gave you full command?

Hueper: I would, as the first step, attack the control of all those environmental and occupational cancer hazards which have already proved of human significance. I would try to eliminate or restrict the human contact to the carcinogenic agents as much as possible, not "practically". "Practical" is a word which permits practically anything. Some of the environmental agents which we know of as of human significance will continue to exist in the human environment. We simply cannot eliminate them because they are natural products. For instance, radiation, ultra-violet radiation is here. Naturally, we cannot put human beings in glass houses but we have to consider the realistic conditions under which we exist here on earth. Then, of course, there are other products such as nickel or arsenic which we can reduce the contact of to a considerable degree; I would take that step-by-step. In the meantime, I would try to develop an effective bioassay system for demonstration of the carcinogenic properties. I think we are on the way of developing that already.

Larry: Are you optimistic about the future?

Hueper: If I did not retain a certain optimism, I almost could say I'd shoot myself.

I N T E R V I E W

Interviewee: Raymond Kaiser, M. D.

Interviewer: Devra M. Breslow

Location: San Francisco, California

Date: April 2, 1976

Interview with Raymond Kaiser, M. D. -- April 2, 1976. Held at Dr. Kaiser's Home, San Francisco, California. Interviewer: Devra M. Breslow.

Dr. Kaiser was the Director of the Cancer Control Branch of the National Cancer Institute in the late 40s and early 50s.

KAISER: I think we had about 12 million to start with, just for the Cancer Control activities, back in the early days.

DEVRA: 1947-48.

KAISER: Yes, that would be about right.

DEVRA: In those days that represented as much as a third of the budget for the whole NCI.

KAISER: Oh, that was most of the budget for the NCI. When the Cancer Institute began in 1937 there was an NCI Act, the basis for the Cancer Control program. The Act itself says something like, I can't remember the words exactly ...."Investigate the causes, prevention, control and eradication of cancer," something like that, which really was the basic groundwork for Cancer Control efforts in this country. They had established the National Advisory Cancer Council at the time this Act came into being. Several people scattered around the country were instrumental in bringing about the establishment of the Cancer Institute and they were on the Advisory Council.

DEVRA: People like James Ewing...

KAISER: That's right. That's how this thing began. James Ewing, and Frank Adair was a surgeon in New York City, Dr. Dudley Jackson, down in San Antonio, Texas.

DEVRA: Was C. C. Little on the first board?

KAISER: Yes, he was on one of the early boards, anyway, if not the first one. The man from Massachusetts, one of the early ones, was Herb Lombard, and from Connecticut Dr. Matthew Griswold, and from New York Ewing, and...

DEVRA: Rhoades?

KAISER: No, he was later. Louis Dublin.

DEVRA: From the Metropolitan Life Insurance Company.

KAISER: Right. From Roswell Park, which was one of the original cancer institutes, Dr. Kress, Dr. Louie Kress, Dr. Sugarbaker, out in Missouri, at the Ellis Fischel Hospital. Dr. Spencer was the Director of the Cancer Institute at that time. Now, they were established with the Cancer Institute in 1937. It was only a couple of years after that, like four, when Pearl Harbor occurred, so everything that related to the possibility of our studying cancer control was put on the back burner.

DEVRA: Were you already in the Public Health Service at that time?

KAISER: Yes.

DEVRA: And what were you doing?

KAISER: I was in internship, I graduated medical school in 1937.

DEVRA: Here, in San Francisco?

KAISER: No, at the University of Colorado Medical School. I went into the Public Health Service, encouraged by a Dr. Amesse, who was one of the key pediatricians in Denver at that time. He had been in the Public Health Service earlier. So I interned at our Public Health Service Hospital in Seattle, and continued in the Service.

Now, at that time, one of the major philosophies of the PHS was to get a new officer acquainted with all of the activities of the Service, which included quarantine service, mental health, medical care for the Coast Guard and so on, so they transferred us all around. (I was married after a year in Seattle) Then, I was placed in the VD control activities.

DEVRA: Was Rod Heller your Chief in those days?

KAISER: No, Dr. Vondelehr, then Dr. Heller came later.

DEVRA: We are still talking about the late 30s now.

KAISER: Dr. Parran came along. Then I was assigned to Hattiesburg, Mississippi, to do VD control activites around Camp Shelby, which was one of the major camps preparing for World War II, I think it was called a limited emergency, when Roosevelt was the President. I took care of soldiers and U.S. Army officers from Ohio, Illinois and Indiana, 60,000 among others, put in this camp. Hattiesburg (the town) must have been a couple thousand people in size, at the time so had all the problems of VD control.

DEVRA: You must have had plenty of activity.

KAISER: Oh, yes, plenty. These gals came over from New Orleans in trailers, parked outside of the camp. I continued in that for a while, then I got accepted to Harvard School of Public Health to get my MPH.

DEVRA: When did you go?

KAISER: That was in the fall of 1941.

DEVRA: Just before the war broke out?

KAISER: I was there when the war was declared.

DEVRA: And you were sent there to get an MPH in something particular, health administration, or epidemiology?

KAISER: Yes, because I took some courses in that, hospital administration as well, that was at Peter Bent Brigham Hospital. But before the year was over, the PHS asked me to leave there.

DEVRA: So you did not finish your MPH?

KAISER: Yes, I finished it.

DEVRA: Did you work with Dr. Lombard at all?

KAISER: Yes, and that is where I first got the exposure to Cancer Control. I was exposed to their cancer hospital, and Dr. Lombard's activities.

DEVRA: What kind of an impression did it make on you at the time?

KAISER: Well, I was interested. I had an idea that maybe we could take some of the VD control activities and efforts and apply them to cancer, heart disease, eventually most chronic diseases, which is where I ended up after I was at the Cancer Institute.

That concept turned out to be a mistake. Actually, there was very little that we could apply to Cancer Control because we didn't have cancer detection procedures and methods that would really work. So, the PHS brought me to California. That was in 1942. I served in the San Francisco regional office; we called them district offices at that time.

DEVRA: Down on 50 Fulton Street, was it there at that time?

KAISER: No, wait a minute, yes it was there. It was in the Federal Building there; later we were moved into 450 Sansome Street, San Francisco.

DEVRA: Across from the Immigration Building.

KAISER: The Immigration was eventually placed in 450 Sansome.

DEVRA: And what were you doing here in San Francisco at the Regional Office? Were you the Director of this Office?

KAISER: No, Dr. Walter T. Harrison was the Director. I was sent to San Francisco to be the Public Health Regional Medical Consultant which I became later, but the immediate activity I was involved with was the evacuation of the Japanese-Americans from the Western Defense Command. (Japanese-American Evacuation)

DEVRA: Oh, would you like to tell me a little bit about that?

KAISER: I am not very proud of it, because it was something that should never really have happened.

DEVRA: But you were ordered to do this?

KAISER: I was on order to do this. To begin the evacuation, General DeWitt ordered the Western Defense Command area evacuated. The Japanese-Americans were to be first placed in assembly centers. The order would come out, and "within 24

hours all the Japanese-Americans within this particular area will meet at such and such a place and be prepared to be evacuated within 24 hours after this initial meeting." We would provide all of the medical exams and rule them as safe or not to travel.

DEVRA: Prior to their being moved?

KAISER: Yes.

DEVRA: You mean you went up to, say, Tule Lake and Manzanar, inspected them when they got there and maintained health services?

KAISER: No, I inspected them in their own community areas. I had nurses and physicians. We moved them to assembly centers, not relocation centers. We moved them to assembly centers, as we called them, like Santa Anita Race Track. We put them in God-awful places. Why we didn't have really an epidemic break out, I'll never know. Those that could not be moved, we put into hospitals in California, here and there, and moved them when we could. We were responsible for all of these assembly centers.

DEVRA: How many of you were involved with this?

KAISER: Of Public Health Service Officers, I would say about seven or eight medical officers and a handful of nurses.

DEVRA: How many thousands of people were you responsible for moving?

KAISER: Oh, we moved about 250,000 individuals.

DEVRA: Over a period of six months?

KAISER: Yes, and the Western Defense Command went all the way up into Western Oregon and Western Washington.

DEVRA: So you were responsible from the Canadian border to the Mexican border?

KAISER: Right, and then I used some of the men from our Hospital in Seattle. Then we had sort of a dispensary outpatient activity in Portland, Oregon, so I used one person from there, Dr. Drescher, who was assigned to the State (Oregon) Health Department VD activities.

DEVRA: What were the health problems that you did discover in this population, or were there any during this process of trying to move all of these people?

KAISER: Oh, there weren't many major problems.

DEVRA: A little TB?

KAISER: Some TB was discovered, but by and large state agencies, California agencies for example, knew about these individuals. There were Japanese physicians who had taken care of these people, who had things pretty well under control.

DEVRA: You couldn't harness the Japanese physicians to work in this program, could you, or could you get them involved?

KAISER: I got them involved at the assembly centers and eventually they were involved in what we called relocation centers, places like Topaz, Utah, and Rowher, Arkansas and Tule Lake, Calif. We got them back beyond this mileage limitation, I can't recall now how many miles it was. Back away from the coast, anyway. The bad part about it was that there would be people walking around outside the wire fence, and the Japanese inside weren't allowed to leave the relocation center. Eventually they could, but that was a year and a half later. They were not allowed to come back into the Western Defense Command geographic area until after the war was over. They couldn't return to California.

DEVRA: So this process of movement went on soon after Pearl Harbor, within a month?

KAISER: May 1942.

DEVRA: And it went, let's say, on to Christmas, 1942. By that time, there were no more Japanese here on the West Coast, or something like that.

KAISER: We got them all into these assembly centers, and then they passed a law for relocation authority and we began relocating them, I guess most of the year. They were all out of California, with the exception of this Tule Lake arrangement.

DEVRA: Until late 1945?

KAISER: Until after VJ Day.

DEVRA: You obviously had some mixed emotions about it at the time.

KAISER: Oh, sure.

DEVRA: Almost everybody did, I guess.

KAISER: These were the orders, we were in uniform and we had to do what we did. I was assigned to this general's office out here in the Western Defense Command. Then somebody got the idea that we ought to do this to the Japanese American people living in Hawaii.

DEVRA: The Japanese living in Hawaii, evacuate them to the mainland?

KAISER: Yes, we had two major shiploads of women and children; the men weren't brought along. General Delos Ammons was in charge at that time, and he ordered the evacuation of these folks, women and children first, so we disrupted families. You were talking about the things that happened, this kind of thing, psychologic things happened frequently.

DEVRA: Now that was what I was going to ask you about. You didn't find very much physical illness apparently, not even in old people.

KAISER: Not a great deal, no.

DEVRA: What about the psychological impact on families and individuals, and what could you do about it?

KAISER: It was horrendous, actually. There wasn't a great deal we could do about it. We did bring them in to psychiatric centers, those who were actually in extreme anxiety, mental states, etc. But then somebody, somewhere, got the bright idea that this was a lot of damn foolishness as far as Hawaii was concerned, because they had been out there for years, were inter-married, and obviously had nothing to do with the (Pearl Harbor) attack.

DEVRA: Of course they weren't a state then either, just a territory.

KAISER: Just a territory and that complicated matters, too.

DEVRA: But in the meantime, two shiploads of women and children were brought to San Francisco; you put them through a process in San Francisco and then where were they relocated?

KAISER: To inland areas.

DEVRA: Did they remain through the war or were they released earlier?

KAISER: They stayed there. I don't know what happened to the male members of the family, but out of this came the brilliant Japanese-American troops who served overseas in World War II.

DEVRA: The ones that went to Italy. Did you follow up these people once they were moved inland?

KAISER: No, we had no responsibility, the so-called civilian agency, the Relocation Authority, took over. Dr. G. D. Carlyle Thompson, you may have heard of him, I think he is somewhere in California now, down in Riverside, Calif.

DEVRA: It was his responsibility?

KAISER: He was the medical authority, in the War Relocation Authority.

DEVRA: Was this in the Public Health Service? No.

KAISER: I guess that was entirely a civilian organization, but he was the Medical Director for that.

DEVRA: So you had this big push that went on in 1942, in which you were the responsible person.

KAISER: The military asked us to do this, arrange for all the evacuation trains. We had medical service physicians, nurse coverage on all of the trains.

DEVRA: Did you have to more or less employ on a short-term basis local physicians in communities up and down the coast?

KAISER: Yes, they gave me all kinds of money to hire them on a contract, on a per diem basis.

DEVRA: So, they would go on the trains, let's say to Utah, Arkansas, all along the way you would have medical coverage. Did you have supplies on these trains? A little dispensary set up on each train? Each train might hold how many thousands of people?

KAISER: Oh, it was in the hundreds actually. I guess it was a couple of hundred per train.

DEVRA: Were there sleepers?

KAISER: Yes.

DEVRA: They had food?

KAISER: They had dining cars on all of them.

DEVRA: Serving American food, not Japanese food?

KAISER: Oh, yes.

DEVRA: So the culture shock was absolutely terrific. You probably had people that didn't eat, who had never seen some of these foods before.

KAISER: Mentioning eating, we had dentists involved in this thing, also.

DEVRA: On the trains, or once they arrived at the relocation centers?

KAISER: Prior to that, those who needed dental correction, cavities filled, dental emergencies, were taken care of.

DEVRA: Before they went out on these trains.

KAISER: Yes, we put them in the assembly centers, and they were there until the military decided that they could commandeer a trainload. We would arrange for all the medical coverage. The medical staff would go with them to the relocation centers. They would be returned at public expense and then they might take another train out.

DEVRA: The dentists were community dentists or were they Public Health Service dentists?

KAISER: Community.

DEVRA: You had money to buy them too.

KAISER: It was at a tremendous cost, billions of dollars, really. In the millions, I am sure, but I think it got into billions, because everything was provided even if it was American food. In these assembly centers we had to set up all the kitchens.

DEVRA: Tent kitchens?

KAISER: Yes, where they had buildings. We used the grandstands, where they could be converted, we used grandstands and race tracks at a lot of places.

DEVRA: I have a recollection that at Tanforan (a track in San Bruno, California) the horses were gone, and they used the stalls for living, is that correct? Families---how many families could live in a stall? They didn't put floors down, it was just the raw earth. Did they bring mattresses in for them to sleep on?

KAISER: Oh yes, it was overwhelming, but anyhow, we got a long way from where we started to talk.

DEVRA: It's a long way away, but it's an interesting piece of history.

KAISER: Nothing has been written by me at least.

DEVRA: You never wrote about that. Did you want to?

KAISER: Oh, I resented it at the time, this goes a long ways back.

DEVRA: You probably felt ashamed.

KAISER: This really should be off the record, what I am going to tell you now.

DEVRA: All right, then I am going to take it off the record.

(PAUSE)

DEVRA: Well, then you really had much more empathy and obviously conflict. You had a public responsibility to do this. Here you are in the Public Health Service, you fulfilled it, but you must have had a lot of personal anguish.

KAISER: Oh, very definitely.

DEVRA: You were involved with this activity until, say, the first of January, 1943. How long did this exercise go on?

KAISER: I don't know. I would say, until the end of the next year when we were totally out of it as far as the Public Health Service and the military was concerned.

DEVRA: Did you remain in San Francisco through the Second World War?

KAISER: Yes.

DEVRA: In the Regional Office. You never went overseas?

KAISER: No. I went into the Alaska defense area.

DEVRA: Was that concerned mainly with VD?

KAISER: Public health work, generally.

DEVRA: For civilians or the military?

KAISER: Well, it was both actually. In association with the military camps, which takes me back to the Mississippi experience, and I related to all the VD problems in the military camps.

DEVRA: In Alaska?

KAISER: Also in several states in the region at that time. We had eleven Western states.

DEVRA: Plus Hawaii as a territory.

KAISER: And Alaska as a territory.

DEVRA: Did we have Guam then?

KAISER: We had it as a territory, but I didn't have to get there.

DEVRA: So you had 11 Western states and 2 territories. In those days there were only 5 or 6 Public Health Service Regions. This was Region 5. So you had really...

KAISER: All the VD problems in all of those states.

DEVRA: Related to the military camps. So those were your responsibilities. Right? That kind of surveillance throughout the war.

KAISER: And any other public health problems associated with it. I had contact with all the State Health Departments through the VD program.

DEVRA: Public health during the war was mainly concerned with infectious diseases rather than chronic diseases.

KAISER: That's exactly right.

DEVRA: Now, what happened. How did you get into the cancer control?

KAISER: After World War II was over, I had itchy feet like quite a few of the officers did. I hadn't had an opportunity to get exposed to the private practice of medicine, so I thought that I wanted to get out, leave the Public Health Service for a while. When I did go out, a brief time...

DEVRA: You practiced medicine?

KAISER: I got exposed. I was at the County Hospital.

DEVRA: Doing what?

KAISER: Just general medical service. The more I stuck with it, the less I thought of it, because my vast experiences were in the public health area. So I changed my mind after a few months and came back into the Public Health Service.

At that time, one Dr. Austin Deibert was trying to get a cancer control program launched. He got in touch with me and wanted to know whether I wanted to be in the Cancer Control Program.

DEVRA: Had you ever met him before?

KAISER: Never. He was just a telephone voice to me. He said that he heard that I had changed my mind and wanted to come back into the Public Health Service, so they really never charged me with being out.

DEVRA: Did you have other opportunities or did you think, well why not?

KAISER: Well, there were other opportunities, but this seemed to be the way I was leaning. So, I actually re-entered the PHS, after never really having been fully out of it.

DEVRA: Then where did you go? Did you have to go back to Washington?

KAISER: I went to Washington. At that time it was decided, that I would go into several of the cancer activities in the States and get some training in cancer.

DEVRA: You would go to the States that already had active programs?

KAISER: Right. New York, Massachusetts, and Connecticut, and I went to Buffalo, New York--to Roswell Park--and was involved in clinical medicine as well as administrative public health medicine.

DEVRA: How long were you there?

KAISER: Oh, a half dozen months. Then I came down to New York City, Sloan-Kettering, spent time there.

DEVRA: Also mainly on the clinical side?

KAISER: Yes, and some time in Connecticut. Most of that time interval was in training and exposure to what we'll call cancer control programs in those states.

DEVRA: This was in late '46 and '47? Did you already have the title, Assistant Chief?

KAISER: No, when I actually came back to Washington after this training period, then I became Assistant Chief.

DEVRA: In Massachusetts, what kind of training did you get?

KAISER: That was with Lombard in relation to their cancer control activities.

DEVRA: At that time did any of them have what we now call epidemiological investigations?

KAISER: Dr. Lombard was involved in some.

DEVRA: And what role did you play in that kind of activity?

KAISER: More observer than anything else.

DEVRA: Did you develop some kind of enthusiasm or sensitivity about epidemiology?

KAISER: Yes, I think I developed enthusiasm at that time, and it stayed with me for a number of years as far as cancer epidemiology is concerned.

Then, I think I began to realize that cancer epidemiology would have to be a lot different than VD epidemiology. When we talked about prevention of cancer we talked about prevention of deaths from cancer. This got us into primary prevention and secondary prevention (which I think Les Breslow had a lot to do with, not too many years after that), in relation to chronic diseases.

It was then that I realized that primary prevention might be something that we couldn't accomplish in cancer epidemiology, but we could think in terms of secondary prevention, in terms of deaths, prevention of the spread of the disease, and so forth. That's got a whale of a lot to do with a lot of thoughts ahead, subsequent to that time. Then we go back to the Act that created the NCI, which had in it "research investigations and epidemiology" written right into the Act, and cancer control and prevention and eradication. Also, in there it had authority to carry out educational procedures in relation to cancer. Money had been set aside (during World War II), which in essence was called "No year" funds. In other words it was not restricted to use in any particular fiscal year. Those funds I learned about.

DEVRA: Had they accumulated during those years?

KAISER: Yes, because during the war years there were not scientific research investigators or epidemiologic investigators available to work on the cancer program. The Cancer Institute scientists were more or less kept at the ready, those who actually didn't go to war and become part of the military establishment. As I said earlier, Dr. Spencer was put in charge of the NCI while the rest of the folks went out, those that were younger, and fought battles in various senses of the word.

So when I arrived at the Cancer Institute, these funds were available. Some of the early thoughts that I had were that when the war was over, we needed to train the oncoming generation of physicians in cancer. I undertook and established what we called the cancer teaching programs, both for students and graduate physicians, in medical school. I suspect that you have a whole...

DEVRA: Well, I have read your papers on that...

KAISER: ...history about that, because at that time, my thoughts were that the average physician perhaps wouldn't be seeing more than one or two cases of cancer in his practice during a year's time. I am talking about the general practitioner, the internist. He needed to have some idea of how he could recognize cancer early. So we began to get individuals in medical school teaching the oncoming generation something about cancer, just fundamentals and early recognition, at least.

DEVRA: So you had this idea. Now, let me ask you something.

KAISER: Now I had the money available because it was this "no fiscal year funds," as we called them at that point.

DEVRA: You were essentially the Deputy Director. Tell me something about your relationship with Deibert. Tell me something about him as a person. Because Dr. Deibert has died, I will not have the opportunity to interview him. I haven't talked to anybody who knew him very well. I am curious to know about him as a person and a leader and how you divided up the planning and program implementation of responsibility.

KAISER: He was a marvelous "front man," as we called him.

DEVRA: Was he a good politician?

KAISER: His background had been entirely in the VD program. He ran the VD Center at Hot Springs, Arkansas, for quite a number of years. Actually he wasn't recognized for what he. . . . accomplished in the VD program. He did a tremendous amount for the VD program. He trained a lot of the individuals who had gone through the VD Control Program. They were all trained by this man. He certainly was a political type.

DEVRA: How did he get into Cancer Control? Just natural progression within the Public Health Service?

KAISER: To tell you the truth, I don't know. I never did know.

DEVRA: In 1946, there he was, he was the head of this Cancer Control Program. You are coming in. You got your training out in the field. Now, how did you work with each other?

KAISER: Essentially, he enjoyed establishing many of the relationships with the ACS and I think he established the relationship---I know he did for a fact---with Roswell Park, the training activity for individuals whom he was trying to recruit. He did a good job of recruiting people within the Public Health Service as well as individuals from outside to come in and join the Public Health Service. He did that during all the years that we were associated, really.

DEVRA: Did he help to deal with the AMA for example, and some of the professional bodies, the pathologists, etc.?

KAISER: Yes, he established the initial relationships, and then we divided it up, and from that point forward, I took care of those. The AMA, Pathology Board, American College of Surgeons, all of those were my activities entirely. Those continuing relationships, once he made the opening overtures; then I came along. He would go out and stir up things, really. I was the guy who had to come along and put oil on the waters and calm everybody down.

DEVRA: Mainly among physicians, or did he do this among Congressmen?

KAISER: He was restricted in his relationship with Congressmen, but he did some of it there. But he did this with the public generally and physicians. Physicians would get so God damn mad at us, it wasn't even funny.

DEVRA: Give me an illustration of this kind of interaction.

KAISER: Well, there was a Dr. David Wood, and he would just get riled. David Wood had a few reasons for getting riled himself. He had a lot of difficulties and conflicts, but in the early days when I first began thinking of cytology, cervical cancer cytology---

DEVRA: This is after Pap and Traut had published the paper?

KAISER: He and Dr. Deibert would come to blows, practically.

DEVRA: Dr. Wood? Was Dr. Wood on the Advisory Council in those days? Or was he on the Cancer Control Advisory Committee? How would he get involved in this?

KAISER: The College of American Pathologists.

DEVRA: Oh, he was high up in that?

KAISER: Oh, yes, he was one of the officers for quite a number of years.

DEVRA: Dr. Deibert was all for doing what?

KAISER: Well, he thought in terms of us really establishing a cervical cytology investigation program and trying to bring it around to where it could be practically used by practicing physicians. Dr. David Wood had resistance to this.

DEVRA: He was a typical pathologist, and he didn't want to spend his time looking at slides.

KAISER: Right, and he didn't want any of the other pathologists to get involved to this extent, because he was sure that we were going to be taking money away from the pathologists. But he never expressed that in words.

DEVRA: I learned yesterday that Sprunt was able to do this in Shelby County. He was able to do cervical cytology for $3.50. But Dr. Wood was charging $10.

KAISER: He never was able to do a low price smear, in California.

DEVRA: Right and...

KAISER: Have you heard about Olive Gates? She ran his laboratory in his home... cervical cytology lab...

DEVRA: Right in the basement of his home, where the files are now? He has got a copy of every slide he ever read.

KAISER: Oh, I am sure that that's true. Well, anyhow, that is an example.

DEVRA: Did that go on for several years,that kind of tension?

KAISER: Oh, yes; eventually I was able to get to the College of Pathology members. It was sort of a "divide and conquer" situation, getting the cervical cytology program launched. Dr. Sprunt, that you mentioned a few minutes ago...

DEVRA: A member of the College of Pathologists...

KAISER: Dr. E. Von Hamm, at Ohio State Medical School; Dr. John Graham, in Massachusetts, and a leader at Roswell Park and his wife.

DEVRA: These were all pathologists?

KAISER: Dr. Stowell in Kansas Medical School and Dr. Stoval in Madison, Wisconsin, University of Wisconsin Medical School, Dr. William Russell in Houston, Texas; and golly, I can't think of some of the other names. Dr. Purvis Martin in San Diego.

DEVRA: And there was a man in Toledo, I think.

KAISER: Yes, there was also a man in Chicago, I can't think of his name now.

DEVRA: That's not Huggins?

KAISER: No, Charlie Huggins was closely associated with him at the University of Chicago Medical School. Huggins was a surgeon, cancer surgeon and clinical investigator.

DEVRA: I am curious, how did you approach these pathologists to try to promote the idea of screening?

KAISER: Essentially through the meetings of the College of Surgeons to which I was regularly invited, not as a pathologist but as a sort of an interested person, a person interested in tissue examination and so forth. It really ties into the American College of Surgeons; they had a program of investigating and establishing cancer clinics, which I underwrote, and I was a member of the College. . .

DEVRA: You underwrote? Meaning the Cancer Control Branch?

KAISER: Yes, and later we got the ACS to also contribute money because it got to be very expensive. This was encouraging the establishment of cancer clinics. Then the cancer clinics had to have pathologists involved in what we called tumor boards which we got to eventually. These are all ideas that came pretty fast.

So that, really, I got to the pathologists through the cancer clinics of the American College of Surgeons. I guess I really, essentially, approached them directly as individuals.

DEVRA: Would you go to see them or talk with them over the phone individually?

KAISER: I would go to see them.

DEVRA: Did you have any carrots to offer them, money?

KAISER: Yes, lots of carrots.

DEVRA: What were you going to pay them for?

KAISER: Actually for establishing cervical cytology screening programs.

DEVRA: At that time were there trained cytotechnicians?

KAISER: No. We began by training high school students as cytotechnologists first at Memphis (University of Tennessee). Early in the activity we learned that this was a pretty boring problem, looking at slides hour after hour, thousands of slides before a positive slide came along.

DEVRA: Who came along with the idea that it would be acceptable for people who were not medically trained and who were usually not men to do this?

KAISER: I am afraid that I am responsible for that idea. It just brought me into conflict with the College of Pathology, which insisted that these people be more overtrained than undertrained, be individuals with "background." So we developed--I don't know if they were ever thoroughly persuaded--we developed a large operation at Shelby County in Memphis.

DEVRA: That was the first major program for training cytotechnologists?

KAISER: Yes, we had a training program there, so at every screening center that we established after that, we had a training component. The one at Ohio State, for example, which incidentally was a combination grant program and program of our own employees. We would hire those people, which we did at Memphis, a lot of those people were on her payroll. A lot at Ohio State and a lot at Houston, Texas.

DEVRA: What about Cornell when Traut was still there? Was that one of yours, too?

KAISER: Yes, but we didn't have any civil service employees on the payroll at that time.

DEVRA: Recall for me how you decided to go to Shelby County, and why Sprunt somehow and the medical community in Shelby County were receptive?

Is Sprunt still alive?

KAISER: I don't know whether he is or not. So there was personal animosity there, and that was. . .

DEVRA: But Sprunt must have had the respect of his peers in Memphis, in order to be able to do this.

KAISER: Oh, by all means. I think he had with him an excellent second man, Cy Erickson.

DEVRA: Also a pathologist ?

KAISER: Yes, who really enjoyed tremendous nationwide acceptance.

DEVRA: And the gynecologists in that town must have been sympathetic?

KAISER: They were.

DEVRA: Do you remember when they trained the cytotechnologists in Memphis, did they train black women or only white women?

KAISER: I don't think that we had established a color line. Of course, Memphis had a lot of colored people and...

DEVRA: ...And they had a separate hospital.

KAISER: Right, right across the street from our lab there.

DEVRA: You mean as employees or as patients...

KAISER: Both. I can't remember. We didn't exclude anyone who was interested. It was confined to girls mostly, women I should say, but we did have a few males involved. But the salary they were able to command wasn't necessarily high enough to support a family, so we had problems in that regard.

DEVRA: Did you used to go down to see Sprunt yourself?

KAISER: Regularly.

DEVRA: You did? How often?

KAISER: I would say monthly visits.

DEVRA: And Jack Dunn was stationed there, was that it?

KAISER: Jack Dunn was a more frequent visitor than I was. He was specifically in charge of the epidemiologic study related to cervical cytology at Memphis.

DEVRA: Now, at the time that the Shelby County study was developed, apparently it was presented to the Advisory Committee for Cancer Control, maybe more than once, I don't know how many times it took. Do you remember whether it took several tries before they would give project monies for this?

KAISER: Yes, I think it took a couple of tries.

DEVRA: And you were the one that proposed it and Dunn proposed it?

KAISER: I was the one who related to the Cancer Control Advisory Committee all through my time in Bethesda.

DEVRA: Even when Deibert was the Chief, the Committee was your responsibility? How often would that Committee meet?

KAISER: Quarterly.

DEVRA: They were all physicians?

KAISER: No.

DEVRA: You had some laymen there?

KAISER: I am just trying to think. The movement towards lay people came down from the National Advisory Cancer Council. It became politically astute to have laymen members on the Advisory Council. Cancer Control Advisory Committee reviewed the grant applications and advised on programs and activities and so forth. Then it would go to the Advisory Council, who had the final word. As

I say, it became politically important to have lay people. Mr. Goldblatt, from the department stores of Chicago, for example, and a Democratic National Committeewoman from someplace in Colorado. That kind of sifted down to our Advisory Council, I mean our Cancer Control Advisory Committee. That Committee was quite a lot later in getting lay members. We were essentially physicians and scientific investigators, Ph.D's, until the later years of the existence of that Committee.

(Policy plus Congressional oversight)

DEVRA: This raises the question, how were policies determined during the period that you were associated with the Cancer Control Program? What was the process by which you actually set a policy and perhaps set a position on the allocation of funds? Were they staff initiated or...?

KAISER: They were staff initiated. I would get an idea, for example, and we would take it before the Advisory Cancer Control Committee and discuss it thoroughly. We actually met for a couple of days, sometimes we would meet specifically to discuss policy and where we should be going and so forth, without any consideration of grant requests.

DEVRA: You only had the granting mechanism and the formula grants. Did you have contracts?

KAISER: I had contracts, I had all types of financial support mechanisms.

DEVRA: What mechanisms were involved in setting priorities for your fund allocations, for what were called special projects. Not the formula grants and not the money that went to the university medical schools, but these other special projects, like cytology screening---how would you set the priorities? How long would this sometimes take?

KAISER: All the applications for this type of thing were reviewed individually, on the basis of merit and their relationship to possible success.

DEVRA: For example, did you have a 1-year plan or a 5-year plan or anything like that?

KAISER: No, as funds became available, we asked for our funds each year and a certain sum was allocated for special projects, for example, by Congress. I would have to say, although we recognized that some of these activities should be long range, we still got our money only on an annual basis, so we used our money as we got it, really. Even the continuing programs. Once they knew your money for education was used up, we had to get money each year for that, and it would become harder and harder to convince the Congressmen that we ought to have money for education. There were a couple of other reasons for this.

(Clinical Traineeships)

DEVRA: Explain to me why that became a difficult thing.

KAISER: Well, initially in the Cancer Institute legislation, there was a provision for providing clinical traineeships. That was written into the original Cancer Institute Act. Administering this program was one of my responsibilities---all the educational activities. That was to train individuals

in the specialties of radiology, surgery, pathology and internal medicine.

DEVRA: You had funds available for clinical traineeships and really began to strengthen our capabilities, our national capabilities for specialty cancer care.

KAISER: That's right.

DEVRA: These are really beyond an internship or beyond a residency.

KAISER: Oh, yes, very definitely. As time went on, Congressmen began to ask the question, "Why should I provide funds to train somebody who is going to charge me more money to take care of my cancer as a specialist?" That spilled over into the attitude for providing funds for medical schools and dental schools for specialized training in oral cancer...

DEVRA: Do you remember the Congressmen who were particularly aggravated by this?

KAISER: No, but I am sure that if you wanted to read the Congressional Records through the years there, you would find that several names kept coming up frequently.

DEVRA: People who had a lot of interest in health, but their concerns were that public monies were being spent for the clinical training of people who were going to go back into the private sector and make money...

KAISER: Greater sums of money as a result of this specialized training.

DEVRA: So how would you defend this every year?

KAISER: It got to where it was impossible to defend it.

DEVRA: At what point did this clinical traineeship go under?

KAISER: Not while I was there...

DEVRA: I talked with Margaret Edwards, and there is sort of a curve of when it dropped down, it's being called something else now. You know, it's back up...

KAISER: It has gone back up? Well, it never was eliminated while I was there, but it kept getting harder and harder to defend each year. That was a tool I used so I could train some of these young graduates in pathology. So, around those we would organize a cytologic screening program, in the center where the individual was being trained.

DEVRA: Because you had already given them some carrots?

KAISER: Right. One of them that I think of right off hand is Dr. Vaeth, who is now, or was, the radiologist over at Mt. Zion Hospital here in San Francisco.

DEVRA: Jerry Vaeth?

KAISER: Yes, he was a Cancer Institute trainee.

DEVRA: He got his clinical training...

KAISER: I trained him, I provided his clinical traineeship.

DEVRA: In radiology, in radiation therapy? Was he trained here in San Francisco or...?

KAISER: No, he was trained elsewhere, I forget where right now, but he was one of the last trainees whom I supported...

DEVRA: This would be in the late 50s.

(Cancer Control Program Training)

KAISER: Yes, when I was at the Cancer Institute, late 50s, early 60s. So that was another way. (Oh, let's see, there was something that occurred to me when I was talking about education.) Oh, I wanted to go back and talk about....there was a statement in the Cancer Institute Act that made it possible to provide money for State Health Departments for cancer control activities, and one of the earliest things we thought of doing was training cancer control directors in and for State Health Departments.

DEVRA: Where would you train them?

KAISER: That was a problem. There was no specific place to train them. So we began by investing funds in the training activity at Harvard, Yale, California eventually, Smith... Schools of Public Health ...

DEVRA: These were Schools or Departments of Public Health, academic institutions.

KAISER: Right. Eventually, I got to all of the 10 Schools of Public Health that there were at that time. There were 9 or 10.

DEVRA: They all had access to these funds? Can you think of some of the people who were trained on those programs?

KAISER: Yes, one of the men that was trained, Edward Cohart, at Yale, he was one of the men whom we originally trained. He was on my staff, we sent him there for training and we assigned him to a state. In fact, Dunn...

DEVRA: He's another one, he was sent to Harvard, he told me. He thinks that he was the first, he is not sure.

KAISER: I think he probably was the first.

DEVRA: And Cohart, when he finished his training at Yale, did he go to the Connecticut State Health Department?

KAISER: No, he was assigned to Massachusetts. Then, after some time in Massachusetts, he decided that he liked Connecticut better and then he came down and joined the school there as one of the teachers.

DEVRA: To some degree your purpose was aborted because, although he was going to teach chronic disease control and epidemiology, he wasn't in the State Health Department as the Cancer Control Director.

KAISER: Another man we had, a man named Callabrese, who I think we assigned directly to Dr. Griswold in the Connecticut Health Department, and then I think he left the field entirely sometime later.

DEVRA: Did any of these people stick besides Jack Dunn, in a sense, stay with Cancer Control, even if they weren't leaders?

KAISER: We trained some individual staff members for District offices, what we call regional offices now, and, let's see, the man that we sent to Atlanta was a Dr. Harold Graning, who is still in the Public Health Service. He has been in the construction business and...

DEVRA: And "power" business. He was trained as a Cancer Control coordinator, as it were, for the State Health Department.

KAISER: Right.

DEVRA: Did he actually do that in Georgia?

KAISER: He did that for a while and then we put him in the district office, as they were called then. Then he went from there to being the regional director in Chicago. We trained a man for California, by the name of Behling.

DEVRA: What happened to him?

KAISER: He was sent out here and ended up in the Regional Office. He is now a dermatologist. He went on to get training in dermatology and he is in private practice down in the Peninsula, I think.

DEVRA: How would you evaluate the effectiveness of that effort in accomplishing what you were aiming to do?
(State Health Department Programs)

KAISER: I would say that we didn't come close to accomplishing anything, not because of the procedures of training, but because the environment didn't seem to be right for state health activities, for State Health Departments to get intensely involved in Cancer Control activities.

DEVRA: Was there anything to seduce them?

KAISER: Yes, we had state allotments, and these allotments kept going up for a number of years. We fought desperately for them. I have staff members in Washington, the last one I had was a Dr. Hon, who related directly to the state programs. I had a nurse or two who did likewise. Who were concerned primarily with dealing with State Health Departments and Regional Offices on cancer control activity. Somehow or other, that has never caught fire.

DEVRA: You think it would have caught fire if you had more money to give to the State Health Departments or that they needed more recognition of chronic disease control, and cancer control was a reasonable priority for them?

KAISER: I think the latter. I think the lesson they needed then was more recognition of the priority of cancer and chronic disease programs as such.

DEVRA: How would you cultivate the State Health Departments in the first place? Would you hold regional conferences to inform them about this?

KAISER: Yes, we got them into Washington, as a matter of fact.

DEVRA: The State Health Directors?

KAISER: Yes, and the men that they wanted to be in charge of the cancer control activities. Dr. Ed Zimmer, for example, from Iowa, had a program going for a long time out there, and several other people were active in the business. We also established, and this was one of Dr. Deibert's ideas, a National Cancer Control Committee, which was made up of State Health Department people, essentially, and ACS represented us.

DEVRA: Was this the nucleus of what became the Public Health Cancer Association?

KAISER: That's it.

DEVRA: Used to meet at the same time APHA met?

KAISER: Right.

DEVRA: Most of these people were public health people?

KAISER: That's right and we specifically designed it that way. It was Dr. Deibert's idea.

DEVRA: Did you give them some support?

KAISER: We gave them support. He and I were officers and Les was an officer at one time, I think.

DEVRA: Did you support the staff at one time, or was there no staff?

KAISER: There wasn't much of a staff.

DEVRA: Did they have specific activities to do?

KAISER: No, I think it was more of a "let's get together and see what's happening" kind of thing.

DEVRA: Sort of talking to yourselves.

KAISER: Talking to each other/ and other APHA members through program presentations. That was specifically designed to get states more interested, because they would be coming to me at the APHA meeting anyway, hopefully, and we could get more and more of them. We tried to build it into a force that would bring more attention to cancer control and the need for it, and also more appropriations for the public health segment of it. Remember, I am already giving money for the schools of public health for training activity.

DEVRA: Right, and you are giving money to the State Health Departments...

KAISER: And they are getting it. Here then was an official organization that never became very large, I guess I am still a permanent member of it, but, these were things that we were trying to do in relation to cancer control within the states.

DEVRA: There was a lot of money for cancer control within the states with a lot of flexibility. Yesterday, for example, I looked at the ledger books in the California State Health Department that were kept, fortunately, although they don't think they have them before '61. But I looked from '61 to '68 to see what they did with the formula money. They didn't keep the dollars in-house; there were applications filed from all over the state. They gave some to the Palo Alto Foundation, the ACS, various county health departments. They were small grants, $5000, $10,000, very flexible money.

KAISER: We tried to keep it that way for a long time.

DEVRA: I am really curious to know why, even though this money went to training, what were the obstacles that prevented state health departments, especially in the major states, from really taking advantage of this and building up cancer control programs?

KAISER: First, I don't think the public had been educated to the point where they would demand services. Second, we didn't have really diagnostic tests of any sort that would tell whether a person really had cancer in his body some-place.

DEVRA: That might have been a function for a Public Health cancer control officer, to develop programs administering such tests. Yesterday, I learned a little about one of your special projects. You did, in fact, support the development of that book on criteria, that Jack Dunn and Sam Greenhouse developed, the Criteria for Cancer Diagnostic Testing. There seemed to be a great wave of passion then for a single diagnostic test. Maybe you can tell me what stimulated that and what role...?

KAISER: I was one of the initial thought-provokers in that area, I guess. I think once we had established the reliability, I guess you could say, of cervical cytology...

DEVRA: As a detection device...

KAISER: It occurred to me that we might also have cancer of the lung shedding cells, so I tried to develop pulmonary cytology projects of a research epidemiologic investigative nature, or whatever term you want to use.

DEVRA: And put money into it?

KAISER: I had a staff operating it at the M. D. Anderson Hospital in Houston, Texas. Specifically confining their efforts to pulmonary cytology. We found from a research standpoint, that this got to be real complex and difficult. Next to impossible to accomplish and pinpoint.

DEVRA: Was that because we didn't have a fiberoptic bronchoscope in those days?

KAISER: That's right, that came later.

DEVRA: Where do you get those fresh cells out of the lung?

KAISER: Sputum concentration and coughing up. You know, people don't learn how to cough up sputum very easily.

DEVRA: Was this the only pulmonary cytology project that you had?

KAISER: We tried a little of it at Ohio State, but we never really got that one off the ground. There were two, and one in Chicago. That was at the suggestion of Dr. Charlie Huggins, who was on our Advisory Council, the one in Chicago. Those three, three seemed to be enough...

DEVRA: Now that was the application of the principle of exfoliative cytology to another site. That's, again, early detection, and...

KAISER: I think part of our problem was that we didn't have preventive measures. We were trying to find something that in a sense wouldn't necessarily substitute for it, but would get us a little closer to diagnosing the thing. Then, by happenstance, one of the staff members, I believe what's his name, Pope Lawrence, ... developed...

DEVRA: Was he a physician?

KAISER: No, he was not a physician. He developed a contact in Hagerstown, Maryland, by happenstance, more than anything else. We had done a study of cancer occurring in the residents of Hagerstown, Maryland, and we had previously done cancer studies there, epidemiologic studies, largely because the population there was static. There was not much change in the population prior to World War II or until many years after World War II. We found an abnormal amount of cancers occurring, lung cancers and other kinds of cancers, in this population.

DEVRA: Are there any clues as to why?

KAISER: We wondered about why. Pope Lawrence was sort of an engineering type, a radiation engineer, so we sent him up there to investigate. He eventually developed a study and found that there was radiation in many of the homes and buildings in Hagerstown, in the bricks from which the homes were constructed. They were prepared, made locally, and there was radiation in the soil.

DEVRA: Did they ever find out what the source was?

KAISER: We found out that a lot of the buildings were radioactive in the low level sense. And then with those residents living in there all those years...

DEVRA: What kind of cancers were they developing?

KAISER: So Mr. Lawrence eventually developed a contract with a man who had lost a wife from lung cancer.

DEVRA: Was that the one that was most prevalent among these people?

KAISER: Yes. And this man was interested enough that he wanted to contribute some money to the cancer program. He also turned out to be the local undertaker. So we developed a laboratory; he provided the money, and I provided

the staffing and funds for equipping it. We had a going operation to investigate this occurrence, abnormal occurrence of lung cancer, in a defined population. We got that nicely started, he made the contribution of this laboratory to the NCI, and we had a research director of NIH by the name of Shannon who went through the roof.

DEVRA: It wasn't accepted?

KAISER: Oh yes, it was accepted, but from that point forward, I wasn't very popular with Shannon, because he had nothing to do with bringing this epidemiologic investigative research laboratory into existence.

DEVRA: When was this?
(Field Investigations...late 50s)

KAISER: I don't know, along about the middle to late 50s, about the time that he decided that he didn't need the Cancer Control Branch in the NCI, or NIH I should say, and we became the Field Investigation and Demonstration Branch at that time, then a little later we became the Diagnostic Testing Branch...

DEVRA: Is that what you were called? The Diagnostic Testing Branch?

KAISER: No, I don't remember what it was after that.

DEVRA: You sent an epidemiologist up to this lab, Mr. Lawrence?

KAISER: Yes, and he was put in charge of it. Dr. Gilliam and, another, Dr. Milmore...

DEVRA: Yes, Benno Milmore...Sandy Gilliam and Benno Milmore.

KAISER: And Pope Lawrence. They weren't actually in residence there all the time, but this was an epidemiologic laboratory. At about that time (this gets me back to the diagnostic business), I got the happy idea of a blood test for cancer, cancer in the circulating blood.
(Diagnostic Testing Branch)

DEVRA: Had you been reading anything that really gave you this idea?

KAISER: Oh yes, I had been looking at the literature and had checked with some of our pathologists in the Institute, Dr. Harold (Red) Stewart, in particular, and wondering if with cervical cancer, one way of its spreading was by circulating blood; the other way would be just growing into the surrounding tissue and the lymph channels. I thought that possibly there might be some way of finding cancer in the circulating blood. I never found anything in the literature to encourage me in particular, but it was just a thought, so I got a pathologist on my immediate staff, a young pathologist, and I put him in Hagerstown. Dr. Pruitt was his name.

DEVRA: He was placed in the Hagerstown lab...to work on the possibility of a circulating blood diagnostic test for cancer?

KAISER: Yes, at the same time, we were also developing, there and in New York, an electronics laboratory, a cytoanalyser, cytoscanner machine (which brings us way

back to the training of cytotechnicians), hoping that we could develop an engineering tool that would shorten the scanning time for screening and analysing cervical specimens.

DEVRA: Did you ever think, or did you get any encouragement from the engineers that the whole thing could be automated, or would it still take a human being to look at the smear?

KAISER: We constantly kept getting "good vibes," as they call it nowadays, that this was possible from an engineering standpoint. Unfortunately, it never did materialize.

DEVRA: How much did you invest in something like that?

KAISER: Tens of thousands.

DEVRA: Several years' special project money...and you had a couple of engineering firms working on this?

KAISER: That I know I have something written on, but it never got to the point where it was discriminating enough to make the accuracy reliable.

DEVRA: What kind of cooperation did you get from pathologists in the field about this development?

KAISER: Surprisingly good, and we used the young man that I mentioned, Dr. Pruitt, immediately on this and Dr. Albert Hilberg, who was another pathologist on my Washington staff.

DEVRA: They had credibility with the College of American Pathologists?

KAISER: Yes. Dr. Hilberg had been in the research side of the Cancer Institute for quite a long time anyway, and he worked directly with Dr. Pruitt.

DEVRA: Do you suppose the pathologists thought that, if we could get an automated system, they would control it and we wouldn't need cytotechnicians, we wouldn't have to employ all these people and so on...do you suppose that some of them thought that this would be a nice, almost profit-making endeavor?

KAISER: They were hopeful that it would be.

DEVRA: And that they would control it?

KAISER: That's right. You said the word there that's the most vital thing that I could mention. Control was one of the big barriers, control by pathologists. Not being a pathologist, you could see how this could raise a lot of hostile feelings.

DEVRA: They wanted to control this entire thing?

KAISER: Pathologists wanted to control all Cancer Control Activities. They knew that no one could do without them. I mean, you can't diagnose cancer without a pathologist reading the tissue.

DEVRA: How many pathologists did you have on your Cancer Control Advisory Committee at any given time?

KAISER: Oh, I would say not more than two, possibly three.

DEVRA: Out of a total of 18.

KAISER: Roughly, and we always had State people. We never had an Advisory Committee that didn't have one or two State people...

DEVRA: You mean State Health Department people?

KAISER: State health people, or public health people like Gaylord Anderson, Lombard, Griswold. They weren't necessarily on simultaneously, but I mean, they were there.

DEVRA: Did they sense that this is what was going on on the part of the pathologists?

KAISER: Yes, they did, as far as the pathologists were concerned.

DEVRA: But they outnumbered them, numerically. Can you remember really any big fights between the public health side and the pathology side?

KAISER: No, it was just sort of an underground feeling of resistance. It was my idea that if we could get them to join us we might be able to get some reconciliation, a little softening of the attitude, but as long as Dave Wood and that type were in charge and were the top officers of the College of Pathology, this was something that you could really feel. Now Dave Wood practiced the same technique. He became very high on the councils of the American Cancer Society, the national level, just as I was. And he figured this was one way that he could control activity in Cancer Control through the ACS, and also through the College, but once he passed beyond the College of Pathology officers' roles, then things began to loosen up a little bit.

DEVRA: The pathologists were in Hagerstown and this was in the late 50s. John C. Pruitt, MD and Albert W. Hilberg, MD and papers that describe this search for the diagnostic testing of peripheral blood. One paper they wrote was Identification and Isolation of Cancer Cells in Peripheral Blood in the May '59 issue of the Journal of Abdominal Surgery; another was Spray Technique for Preparation for Cytologic Specimens for Automatic Scanning Machines, that really dealt with the search for an automated cytologic scanning machine that could in fact replace human beings; and another paper, The Field Trial of the Cytoanalyser, by William B. Courtney, MD, Albert Hilberg and others, was in the Journal of the NCI, October, 1960, Volume 24. Another paper, Modified _____ (?) Technique, Preparation of Smears for Automated Electronic Scanning, by William B. Courtney, MD, appeared in Volume 25 of the NCI Journal, 1960. Now these were efforts on the part of the NCI itself.

These are really two problems. One was to find an automated cytologic scanning technique which could cut down on person-power and make the efficiency and the cost of doing Pap smears and perhaps other kinds of cytologic tests more efficient. The second, really, was a search for a single diagnostic test, using the circulating blood as the medium.

At the same time, there were people outside the Cancer Institute who were beginning to come up with the possible tests. The Penn test, I heard of a couple of others yesterday. Do you remember what role your Branch played in either stimulating these people by giving them money or suggesting that there had to be some kind of a uniform way of examining and evaluating the quality criteria of these tests to see whether any of these could be effective?

KAISER: We funded the New York firm that was helping us on the automatic scanner, a contract arrangement, for possibly five years.

DEVRA: Now the Institute and your Branch were obviously very interested in all the diagnostic tests. Do you remember, for example, the Penn Test. Did your Branch play any role in underwriting some kind of a test for the Penn Test?

KAISER: Oh yes, we had a contract arrangement as well as a grant arrangement.

DEVRA: With Stafford Warren and Andy Dowdy at UCLA? Is that the only place in the United States that the Penn Test was being tested?

KAISER: Well, it was the only one in which we had any participation. I am sure that it was being checked in our other laboratories.

DEVRA: Can you remember something about the climate of enthusiasm and aspiration for a single cancer diagnostic test, whether this came out of the lay public, from physicians. Where did all of this momentum come from?

KAISER: Well, I must admit that a fair share of the enthusiasm came from me personally. Although many of our staff members shared that enthusiasm. We were all operating as a team essentially.

DEVRA: Who besides yourself?

KAISER: Oh, I think Dr. Deibert underneath it all had a desire for this kind of thing, and John Dunn, and later, I think Dr. Gilliam had a similar desire.

DEVRA: Did you have any staff people who were chemists, pathologists, clinicians?

KAISER: A chemist, I can't think of his name at the moment, who was in the NCI proper, research side...

DEVRA: Well, you can think of that. When you presented this idea to your Advisory Committee, how did they respond? That some of the money should be allocated for evaluations of cancer diagnostic tests, both by your own staff and Hagerstown and by people in the field to whom you would give contracts or grants?

KAISER: They were in favor of it. As a matter of fact, some of the funds were made available on a grant basis which had to have the final approval of the Cancer Council and the Committee...the Committee first and then the Council. So they enthusiastically supported it.

DEVRA: There was no resistance on the part of anyone that you remember? No skepticism?

KAISER: I think there was skepticism on the part of the pathologists represented, but they were of the opinion that it ought to be given a fair trial. Which was sort of an open-minded acceptance of the study.

DEVRA: Right, but for several years you gave money through grants and through contracts to test three, four, five different possible diagnostic testings mainly concentrating on the circulating blood as the medium, is that right?

KAISER: That, and we spent a lot of money on the Penn Test.

DEVRA: You did. Was that the most promising at that time?

KAISER: At the time it seemed to be, yes. Of course, the largest segment of money was put into cervical cytology, exfoliative cytology, I should say...

DEVRA: Which was really early detection as opposed to, say, a single diagnostic test to find cancer anywhere in the body. Okay, now as the results began to come in from these various evaluations of a single cancer diagnostic test, and the evidence became clearer that there wasn't one definitive test, how long did it take before you, as the leader, and other members of your staff and even your Advisory Committee felt that the effort should slow up, and money should no longer be poured into a search for a single diagnostic test?

KAISER: Oh, I would say probably six years. Somewhere in that range.

DEVRA: By the early 50s or the middle 50s you weren't going to spend any more money on this kind of thing. Do you remember if the ACS was putting money into this at the time?

KAISER: At the time, no.
(Dr. James Shannon)

DEVRA: How did Shannon react to this. Was he enthusiastic or not?

KAISER: I would say that he was lukewarm about it. Dr. Shannon was very much a purist in terms of research activity. I think "applied research" or "clinical research", as such, were not part of his area of interests. Although he had come to the NIH from a drug firm.

DEVRA: He came sort of in the middle of your tenure, as the Chief of Cancer Control. Did you meet with him very often or ever?

KAISER: Infrequently.

DEVRA: At his request or yours?

KAISER: Mostly at his request, although I made a number of requests to meet with him, but he would fail to keep the appointment that kind of thing. I asked him repeatedly.

DEVRA: He didn't have much enthusiasm or interest for cancer control, or heart disease control, for that matter, either. How did he manifest this ultimately? Besides ignoring you?

KAISER: He just ignored us, largely, and he accused me of carrying on program activities with a captive. . .

committee, the Cancer Control Advisory Committee.

DEVRA: How did he accuse you, actually verbally he said this to you? More than once?

KAISER: Yes, a couple of times.

DEVRA: Did you do anything about this?

KAISER: No, I wasn't in a position to do anything about it at that point. I continued the committee. I did submit the selection of proposed members names to him all the way through.

DEVRA: Of course the terms of these people rotated all the way through. I was going to ask you, what role did you play and other members of your staff in selecting members for the Advisory Committee of Cancer Control?

KAISER: Initially I think we selected all of them. At that time, the final clearance on members was left to our staff. Eventually in time, it became necessary for all advisory listings and proposed members to be approved by the head of the National Institutes of Health. Also, at that time, subsequent to Dr. Shannon's arrival at the NIH, the lay representation of membership became important.

DEVRA: Would he play a particularly significant role in assuring that appropriate members were on each committee?

KAISER: That's right.

DEVRA: Did he think he was going to get signals from Congress?

KAISER: Also, on the Study Sections.

DEVRA: Lay members on the councils of NIH.

KAISER: Yes.

DEVRA: All right, now when you and your staff were recommending appointments to the Advisory Committee, what criteria did you use and what pressures were you subjected to, by either organized medicine or Congress or other bodies?

KAISER: Well, initially we had no pressures at all. Eventually I suppose that the pressure really came from the Director of NIH, in terms of what was politically important as far as testimony for hearings and so forth.

DEVRA: Did you ever actually have a congressman call you up and say he would like to have so and so put on your committee?

KAISER: No.

DEVRA: Would a staff man for a congressman ever say that?

KAISER: No.

DEVRA: Did you ever pick people who were on the board of the ACS, and pick them deliberately?

KAISER: Yes, in fact, we picked the scientific medical director, Dr. Charles C. Cameron. He was on for a number of years.

DEVRA: Was it a kind of a gentlemen's agreement that he would be on your board, and then you, in turn, were on his?

KAISER: I was on several of the ACS boards.

DEVRA: High-ranking decision-making bodies?

KAISER: Yes. The training committee, professional education, lay education, three or four of them.

DEVRA: Were you on the Executive Committee?

KAISER: No.

DEVRA: Was that considered a conflict of interests?

KAISER: No. At that time, conflict of interest wasn't of any great importance to anyone. Supposedly we were working as a team with the private sector and volunteer groups---and we were actually, so that sort of thing did not arise.

DEVRA: I wonder if we could talk a little bit about how the Cancer Control Branch did relate to the ACS during your tenure.

KAISER: Well, we had sort of this interlocking directorship or membership purposely, and brought into existence by the staff at NCI rather than first by the ACS. In other words, we approached them and said, let's get together on this. We also jointly sponsored support for various projects like the American College of Surgeons' program; we would take a request for funding through our channels and they would likewise take it through the ACS. And it would be a complementary kind of thing.

DEVRA: This was, for instance, support for the American College of Surgeons cancer clinic approval program. Somehow they never generated enough money to do it themselves; they don't charge hospitals to have it done; they have always had the money from the NCI and/or other private public health services and from the ACS.

KAISER: Initially, it was entirely by the NCI. Then we tried to broaden that base so we could use some of our funds for other things. In the professional education area, we produced, in conjunction with the National ACS headquarters, 7-8 diagnostic films, professional education films for physicians only. I was involved in script writing and the whole gamut and served as a consultant for them. Part of the reason this came into being was that a non-governmental organization could produce films, whereas we had certain limitations and restrictions as far as the Cancer Institute was concerned.

DEVRA: Was that also true with respect to things like conferences? That Conference on Detection in Portsmouth?

KAISER: That's right. We began that sort of activity in conjunction with ACS. We also established a series of National Cancer Conferences.

DEVRA: That was during your tenure?

KAISER: Yes, these alternated with the four year meetings of the International Cancer Congress. These were held on the interim two year dates. They were actually four years apart. But there were two years in between them and the D. C. Congresses.

DEVRA: Who had the initiative for these conferences?

KAISER: I think Dr. Deibert deserves the credit for the National Cancer Conferences.

DEVRA: And he took this idea to the ACS in the 40s after they had reorganized?

KAISER: That's right, and out of those came---they must be up to a dozen or so now... (9/76 will be #8)

DEVRA: Yes, they are up to 8, only this year they are changing it. They are going to call it the National Clinical Management Conference. It is the same confederation, though.

KAISER: We had close working arrangements with ACS. We would select part of the speakers, nominate individuals, and see that certain subjects got presented.

DEVRA: You had a very intimate working relationship with Dr. Cameron?

KAISER: Oh yes, very definitely.

DEVRA: Did you get on the phone a couple of times a week and talk with Charlie Cameron?

KAISER: Oh yes, more frequently than that.

DEVRA: He would call you?

KAISER: Yes, and we'd confer with other members of his staff...names that I can't recall right now. We spent certainly as much time at their place as they spent at ours. I mean it was sort of a reciprocal working arrangement. We had a very healthy and favorable one. I think we were benefiting both organizations.

DEVRA: For instance, if you were in a tight situation about something that you really wanted to try, but you couldn't do it in the framework of government and certainly not in the framework of NIH, which had certain regulatory restrictions, could you go to the ACS and say, would you bring it up at your Board and see if they'll float the money?

KAISER: That's right, and if it was something that we could legally get into eventually. There are many restrictions in relation to this. Oh, breast self-examination....

DEVRA: Why couldn't the Federal government promote that?

KAISER: In relation to pamphlets, handouts and going to the radio stations and that sort of thing. We couldn't do this as a government group, but ACS could. And there are announcements even today about breast self-examination.

But the Federal Government doesn't allow this sort of, shall we say, promotion of itself.

That was an example that we went with to ACS. Can they do public education? Of course, it is one of their main responsibilities, and they said they would do this, and see that this concept gets advertised in films, pamphlets, etc. Health information for the public about cancer couldn't seemingly carry the endorsement of the NCI, of the Federal Government. So that in all these areas, we got together with the National Headquarters of the ACS, and helped them. We hired writers.

DEVRA: You hired the writers, on your payroll?

KAISER: That's right, to work with ACS, who could produce and arrange for showing times, air time.

DEVRA: ACS would be responsible for the publication and dissemination...

KAISER: Right. One time, we hired the writers for the American Society for Clinical Pathologists.

DEVRA: You did. Did they come to you with that idea or did you sell that to them?

KAISER: We sold it to them.

DEVRA: And what did you want the person to write?

KAISER: Promotion of cytology and all of cancer control activities.

DEVRA: So that writer, that was your writer, essentially, since you donated this writer to the American Society of Clinical Pathologists, for the professional pathology community.

KAISER: And lay as well...

DEVRA: Published it in scientific journals?

KAISER: No.

DEVRA: In the public media?

KAISER: Yes.

(LUNCH BREAK)

DEVRA: You were telling me just now at lunch that there were two other people, both pathologists, whom we shouldn't neglect. One of them was Sidney Farber.

KAISER: Sidney Farber of the Childrens Hospital in Boston, was one of the pathologists who encouraged me in all matters related to cancer control activities.

DEVRA: Why do you think he was so sympathetic?

KAISER: Well, I didn't know it, initially, but it developed that he had a colostomy and had survived successfully for many, many years and believed in cancer control activities. One of the concepts that he had, however, which even to this day hasn't come to operation was that a number of specialized cancer hospitals should be organized throughout the US on a regional basis so that the cancer patient could get excellent care and treatment. I feel that he believed in this so strongly that he attempted to influence legislation on a number of occasions, Federal legislation that is, leading towards this possible end.

DEVRA: Did he ever ask you to assist him in this effort? Prepare testimony or get data?

KAISER: Oh yes, and that happened more than once. As a matter of fact, in relation to cancer control activities I prepared materials almost routinely for Dr. Farber and Mary Lasker, who was quite influential in the ACS national program.

DEVRA: Was either one of them on your Cancer Control Advisory Committee?

KAISER: Both, and they were promoted from that activity to the National Advisory Cancer Council, subsequent to terms of service on the Committee. This kind of arrangement seemed to further the efforts of the Cancer Control program, and enhance the activities of the Cancer Control program.

Dr. Robert Moore is an especially close friend, who was Dean of the College of Medicine, Washington University in St. Louis, and a member of our Cancer Control Committee initially. He was very instrumental in softening the possible hostility in the College of Pathology and certainly was a strong supporter of the cancer teaching programs in medical schools and dental schools. In fact, he has published papers related to the general effects of that program and has evaluated it on two or three occasions. I suspect that you can find it in the literature someplace.

DEVRA: Getting back to Dr. Farber for a moment...

KAISER: Dr. Farber made me a Special Consultant to the Childrens Hospital in Boston.

DEVRA: While you were the Cancer Control Branch Director?

KAISER: Yes, and I spent some time in clinical activities there, and at a later time appointed a full-time staff member who served with Dr. Farber for a number of years.

DEVRA: You appointed this person, did you say, or you were appointed?

KAISER: I was appointed as a full-time staff person, and I also assigned a full-time staff person...

DEVRA: And paid for that person?

KAISER: Yes, to work with leukemia in relation to diagnostic possibilities, earlier diagnosis, etc. There was a woman physician, who spent full-time ex-

ploring diagnostic procedures, tests, if you wish to call them that, which might relate to the earlier detection of Hodgkin's disease, solid tumor diseases and leukemia. She was still at the Childrens Hospital in Boston when I left the Institute in 1961.

DEVRA: During the time that you knew Dr. Farber so well, in the late 50s and early 60s, I may get the title wrong, but the National Chemotherapy Service Center was established. Farber was one of the principal scientific forces behind its creation. He was by then already on the National Cancer Advisory Council.

KAISER: Yes, and so was Mrs. Lasker at that time. Dr. Farber worked very closely with Dr. Kenneth Endicott in establishing the Chemotherapy program.

DEVRA: Did you maintain a strong relationship with him after the creation of this Chemotherapy Service Center?

KAISER: Yes, but it was established as a separate organizational unit within the Cancer Institute, outside of the confines of the Cancer Control Program.

DEVRA: Was it ever considered that it should have been within the Cancer Control Program?

KAISER: Since it was a new undertaking, it was felt by the Director of the NCI that it should be operated as a separate entity. It was the first time that this type of thing had ever been attempted. And so it was maintained outside the Cancer Control Branch. We did have through the years a program related to cancer, occupational cancers.

DEVRA: Tell me about that.
(Hueper and Carcinogenesis)

KAISER: Which antedates much of the present publicity. We had on our staff a Dr. Wilhelm Hueper, who was much interested in carcinogenesis as it related to industrial products, and many, many years ago maintained that the asbestos industry was contributing many cancers---lung cancers---to the overall cancer problem. He did a lot of studies. We maintained him in a specific lab and also supported him with adequate staff.

DEVRA: He reported to you?

KAISER: He reported directly to me, as Director of the Cancer Control Program.

DEVRA: Now you inherited him, since he had already been there?

KAISER: Yes.

DEVRA: What was he like?

KAISER: He was of Germanic extraction, and somewhat abrasive. He created some problems in his public relation activities, especially as they concerned industry. He was right, and felt that he was right, and no one else had any real justification for doubting his results.

DEVRA: Did you doubt him?

KAISER: Oh, on occasion, yes. It turned out that he wasn't always absolutely correct in his specific accusations. However, he did have a number of findings that have later been substantiated, and only today are some of these products being withdrawn from the market. For example, this Red Dye #4. This was one that Dr. Hueper had investigated in the lab many, many years ago, and published on it. It is only within 1976 that such a dye product is being withdrawn from the market.

DEVRA: Now he would come up with some findings. Then what administrative process went on to either validate this or to make recommendations for regulation?

KAISER: As far as validation was concerned, we would undertake to establish a replicate study in another lab outside of Federal Government and support it through grants from the Cancer Control Branch.

DEVRA: Was he successful in competing for these kinds of monies for these replication studies?

KAISER: Yes, I would say so. I would say that in this area we had all of the financial backing that we could efficiently utilize.

DEVRA: Did he ever appear before the Cancer Control Advisory Committee and make presentations?

KAISER: Yes, he did on a number of occasions. As I mentioned earlier, they would find him a little bit aggressive and uncompromising as far as his end-results were concerned. However, the Advisory Committee and Council more or less tolerated his presentations.

DEVRA: Would they punish him by not allocating funds for these replication studies?

KAISER: They never did.

DEVRA: And what about regulation?
(Radium Loan, regulatory functions)

KAISER: The NCI Act had no enforcement or regulatory provisions excepting for one item, and that related to the use of radium. When I came to the NCI they had established a radium loan program, by which radium was loaned out to qualified radiologists in the various institutions and hospitals throughout the country for treatment of cancer patients. At that time, it was felt that radium was one of the best therapeutic methods for certain types of cancer. That program I inherited as part of the Cancer Control Branch activities. I believe that it is continued to this day. There were certain enforcement, regulatory provisions in this radium loan program, so that it was possible to withdraw the radium loan from any area where its use was being abused or harm was being done possibly to patients. But this was the only area where we had enforcement powers related to cancer activities.

DEVRA: Okay, but the Branch is an investigative body in a way, because you are doing research, and certainly Hueper's work must be called research.

KAISER: Absolutely.

DEVRA: He is coming out with observations about occupational cancer, and there are other regulatory agencies that have the power to control working conditions in industry. How and in what manner were his findings transmitted to those agencies that have such regulatory powers?

KAISER: Through scientific publications and also by personal and written communications with the Occupational Health Division of the Public Health Service and other entities that had an interest in industrial environments. However, particularly the Occupational Health Division was never adequately funded, so that this was a bar to actually carrying out any enforcement in relation to cessation of industrial activities.

DEVRA: This must have been very frustrating to Dr. Hueper...

KAISER: Extremely so.

DEVRA: Would this just antagonize him more and enrage him more?

KAISER: This did exactly that on more than one occasion. However, it did not dissuade him from continuing this type of investigative pursuit. When uranium first came into being, it appeared to me that possibly there might be a higher incidence of lung cancer among uranium miners. I developed a study conducted by staff of the Cancer Institute in the Field Investigations Branch, it was called by that time. We looked at people working in the uranium mines of Utah and Colorado, particularly in Utah. Dr. Hueper was our medical advisor; incidentally, he was a pathologist also.

DEVRA: He was. You were really surrounded by pathologists.

KAISER: I guess that was my first line of defense. Not, really, but I knew that it was necessary to get pathologists clearly on our side without bludgeoning them into acceptance. I would say that we made special efforts in relation to pathologists. In relation to the uranium study, as I indicated, Dr. Hueper was our advisor and Mr. Pope Lawrence was the field manager of this study. He had worked closely with the Atomic Energy Commission. This study was carried out over a number of years. I can't remember when we published on it, but it was published eventually, and it did confirm in fact that the uranium miners had a higher incidence of lung cancer than other types of miners.

DEVRA: What kind of action followed this revelation?

KAISER: This was brought to the attention of the Atomic Energy Commission, the National Academy of Sciences and published in scientific journals, and I think, possibly in a small way, influenced the mining of uranium, but particularly influenced the atomic energy plants that were to be, nuclear power plants that were to be constructed, for peacetime uses. I think that there is a little bit of credit that we can take in relation to this epidemiologic investigative study having contributed to some eventual enforcement.

DEVRA: Let's talk about the role of the Cancer Control Branch in relation to epidemiology of lung cancer as possibly caused by cigarette smoking. What

happened within the Branch itself? Did you have studies going on to investigate whether or not this was in fact a real association? As early as 1957, the Surgeon General issued a rather timid statement on the association. By 1959, Dr. Burney issued a rather vigorous statement about the possible correlation between cigarette smoking and lung cancer. What role was the Branch playing? (Lung Cancer, Cigarettes)

KAISER: The Branch had a number of epidemiologic studies underway through Branch support and contracts throughout the country in relation to lung cancer and cigarette smoking.

DEVRA: One must have been this one here in California.

KAISER: Yes, it was. We supported that one.

DEVRA: Did you support the one in Hammond's shop?

KAISER: Cuyler Hammond of the ACS received his initial support from the Cancer Control Branch of the Cancer Institute, and his investigation of the relationship of cancer (Let's see, we had someone assigned here in California, I remember in San Diego, a lung cancer study, no, that's something else.) At any rate, we did have a number of studies that we were investigating and, of course, in the Institute itself animal experimentation in relation to cigarette smoking was being conducted. Almost from the time I arrived at the Institute. Dr. Hilberg, who was on my staff at a later time, was associated with the animal studies.

DEVRA: What about the epidemiologic studies within your own unit---was that considered to be a germane activity for you to be doing?

KAISER: Yes, it was. But at that time we had only a limited number of individuals who were capable of conducting epidemiologic studies. Gilliam and Milmore were there after Dr. Dunn left. So that they, in time, eventually got around to where it was possible for them to engage in some studies, Dr. Milmore particularly. Dr. Gilliam and I did studies in relation to lung cancer and epidemiology generally. I think there is a publication on that somewhere.

DEVRA: Did you find some correlation between lung cancer and cigarette smoking practices?

KAISER: We did, and attempted to bring these to the attention of the Director of the NIH.

DEVRA: Who was that at that time?

KAISER: A nutritionist, the man before Shannon, Dr. Henry Sebrell. . .

DEVRA: Was he a cigarette smoker?

KAISER: Yes.

DEVRA: What did he say when you presented him with this?

KAISER: Well, he was a little bit doubtful about the results, and it took many studies and considerable investigation in later years to actually come to the present day conclusion about cigarette smoking and lung cancer.

DEVRA: One of the things that has intrigued me about the process of that particular revelation was the fact that either investigators in the field were initiating an interest, applying for and receiving the monies to do these retrospective studies, and also prospective studies. As long as the work was being done outside of your Branch, you were paying for it, but it was being done in California, New York and all over the country, that was all right. I am guessing. I don't want to put words in your mouth...

KAISER: That was acceptable.

DEVRA: Is that right? As long as you didn't do it in house and you didn't publish it as originating in your branch?

KAISER: That's right.

DEVRA: Was that more or less the policy throughout your entire tenure there?

KAISER: No, I believe that tended to change over a period of time. As a matter of fact, I am sure that it did change. We eventually came down to the point that we got Mike Shimkin onto this study. I don't know where he is today.

DEVRA: He is in San Diego.

KAISER: (But he left the NCI and I guess lives in San Diego today.) At one time when there was a change in the organization of the Cancer Institute and in all of NIH, as a matter of fact, he was placed in epidemiology, after Sandy Gilliam.

DEVRA: Was Gilliam a cigarette smoker?

KAISER: Yes, he was, as I recall.

DEVRA: I know Jack Dunn told me that he had been a cigarette smoker several times, is now a pipe smoker and has been for several years.

KAISER: Dr. Hueper, going back to him for a moment, had done some studies on cigarette smoking and lung cancer. His feeling was, that with the arsenic or something used in the curing or in the paper, an increased incidence of lung cancer occurred. All of these investigations were published in scientific journals, but I think it remained for the Cancer Society to bring this to the attention of the public in general. At least the Cancer Control Branch was not successful at bringing it about. We weren't necessarily restricted, in the sense that we were told not to do these things, not to publicize the fact, it was just that there didn't seem to be anybody in favor of doing it.

DEVRA: People on your Advisory Committee?

KAISER: Yes, I would say that they weren't all that concerned about the incidence of lung cancer. Of course a number of them smoked, but I really don't think that they resisted any publication on this basis.

An area that I have not mentioned up to now, Dr. Harold Dorn carried out a number of mortality studies. It seems to me that we did these at about 5-year intervals

and not at 10-year intervals. Retrospective studies of all the cancer cases throughout the country at the various cancer clinics. Eventually he became my representative on the College, The American College of Surgeons Cancer Clinic Committee. He did considerable work in relation to lung cancer. However, I believe, although he was a non-smoker, I think that beneath it all he was a little skeptical about the relationship between cigarette smoking and lung cancer. I believe that he felt that tobacco generally had some relationship, but he was reluctant to say that cigarette smoking was the cause of it. He also became the advisor for the Branch with Dr. Cuyler Hammond, who was a statistician. Dr. Dorn was a PhD, not an MD, and he related to Dr. Cuyler Hammond. I think eventually that Dr. Dorn came around to accepting the findings that Dr. Hammond had. It was after that Dr. Hammond went to Yale or wherever he went from the American Cancer Society.

DEVRA: He has stayed there (ACS) all these years.

KAISER: Is he still there? I guess he had been a teacher at Yale. I was a teacher at Yale, too.

DEVRA: In the School of Public Health? Is that right? Now what would you teach?

KAISER: I tried to talk about cancer control...

DEVRA: And how often would you do that?

KAISER: Oh, perhaps a couple of times a year.

DEVRA: What level students were you teaching?

KAISER: They were all graduate students, master's candidates.

DEVRA: Were they skeptical about this as a career opportunity? Or something that society should care about?

KAISER: I think there was a certain amount of skepticism involved. They were accustomed to thinking in terms of something a little more tangible and practical in the way of preventive measures which I couldn't offer them, naturally.

DEVRA: But that didn't faze you.

KAISER: No, it didn't faze me. Each year they would ask me back. I would go back and Dr. Cohart insisted that I come back two or three times a year.

I was requested to go to Harvard on numerous occasions, but I think that relationship was probably through Dr. Farber rather than Lombard. Lombard was on our Advisory Committee and so was Farber at a later time. We had a number of Boston representatives on our Advisory Committee, Dr. John Spellman, who was the brother of Cardinal Spellman. He was later promoted to the Advisory Council. We got involved in this in order that we might do a study of cancer, an epidemiologic study of cancer in nuns...

DEVRA: Did you accomplish that?

KAISER: We accomplished that. A number of promises had to be made in relation to publication of this epidemiologic data, and I don't know to this day whether this has actually been published or not.

DEVRA: This isn't the study in New Orleans?

KAISER: No.

DEVRA: This is the nuns in New England or Massachusetts?

KAISER: In the New England area generally.

DEVRA: A number of orders?

KAISER: Yes.

DEVRA: You were looking for cervical and breast cancer risks?

KAISER: Mostly.

DEVRA: Actually the whole cancer experience of nuns, was that the idea?

KAISER: That was the idea entirely.

DEVRA: Somebody got a grant to do this?

KAISER: Yes.

DEVRA: At Harvard?

KAISER: One of the New England institutions, I believe it was Harvard. Anyhow, our epidemiologists were involved with the grant. This was in the days when conflict of interest was not too prominent a feature of everyday activities. So, in a number of these grant situations, it was possible for a member of our staff to be an advisor to the study and obtain results of the study through direct contact this way. But, to this day I can't tell you honestly whether we ever got around to seeing that nun study published. Perhaps Dr. Dunn mentioned it to you.

DEVRA: The only one I know about is, Lester used to go to New Orleans to visit a study of cancer in nuns that a Dr. Nix had, but he got his money from the ACS.

KAISER: That does ring a bell. New Orleans does ring a bell to me on the study of lung cancer, and Dr. Ochsner got support from the Cancer Control Branch through a grant mechanism for study of lung cancer and its relationship to smoking.

DEVRA: Did he ever sit on your Advisory Committee?
(ACS-NCI)

KAISER: I believe that we asked him to, but he was so involved with the ACS at that time that he didn't have a free moment that he could call his own, plus his regular surgery and practice and so forth. We were instrumental in getting

him to a number of those National Cancer Congresses, however.

DEVRA: You would underwrite the transportation for a number of your scientists to go?

KAISER: We would try to select individuals who we thought could bring a pertinent message or findings to the group.

DEVRA: These are programmed speakers...

KAISER: Yes, and those people we would underwrite. We would do this in conjunction with Dr. Cameron and his staff of the ACS; those they selected, they would underwrite, in cases where we couldn't and they could; they would underwrite me, for example.

DEVRA: That's right, because you wouldn't be able to go otherwise. So the ACS would pay for you to go to the National Cancer Congress.

KAISER: That's right. Dr. Deibert while he was there; until he left.

DEVRA: And maybe even the Director of the National Cancer Institute?

KAISER: That's exactly right, and some of the scientists around, the Cancer Institute, Stewart and a few of the others that we used to want to be there.

DEVRA: Well, in the middle 50s things started to change. There were several reorganizations, the beginning of what I call the rumbles in the Public Health Service. There were a whole series of rumbles, and they went on not all the time, but in the middle 50s there was obviously some reorganization.

KAISER: I think that we had half a dozen reorganizations in the Public Health Service. As a matter of fact, the first reorganization wasn't finished, before our second one was underway. This kept up for a number of years, even up to today.

DEVRA: And in the middle 50s, the Cancer Control Branch changed its name and to some degree even its function. It became the Field Investigations and Demonstration Branch, is that correct?

KAISER: Field Investigations and Demonstration Branch.

DEVRA: You were its chief?

KAISER: That's right and at that time I suppose...

DEVRA: And Dr. Shannon had already become the Director of NIH...

KAISER: He had become the Director of NIH, yes; he was there prior to that time, actually. Dr. Van Slyke, Shannon's deputy, who suggested that we change the name and do a little reorganizing. . .

DEVRA: And what did that entail?

KAISER: Perhaps reduce the amount of support we were providing to cervical cytology screening.

DEVRA: Why was that?

KAISER: Because he felt that by this time we had proven the efficacy of the Pap smear cytologic technique, and it was up to another type of organization, perhaps a public organization, such as the ACS, to get this adopted by the practicing physician. This was not necessarily the function of a research investigative organization.

DEVRA: As long as the Field Investigations and Demonstration Branch remained in the NCI, your concern in part had to be research?

KAISER: Yes. This was when we perhaps began to look into the diagnostic test possibilities a little more deeply.

DEVRA: And a great deal more of your effort was going into that at that time.

KAISER: Yes, I would say so. Certainly more of our resources.

DEVRA: Did you maintain the State formula grants or was that transferred when the Cancer Control Program was transferred?

KAISER: It was not transferred at exactly that time, at the time of this re-organization and name change, but it was subsequently. Perhaps within a couple of years, the State program was transferred to --- I don't know what it was called then --- whether it was the Bureau of State Services or not...

DEVRA: And they brought Lew Robbins in.

KAISER: Yes, that is right. Whatever that date was...

DEVRA: That was around '57-'58.
(1957-59 shift from NCI)

KAISER: That's when the State programs were transferred, the funding for only the State programs.

DEVRA: In '58, some of which we now call Cancer Control was essentially pulled out of the NCI and transferred downtown to the Bureau of State Services, under a man named Lew Robbins. Can you tell me something about the background of the decision to split off what seemed to be control from what had been control, now called Field Investigations? Who precipitated this and what the form of this split really was?

KAISER: I don't think I am able to tell you the background; it was presented to me as a "you will."

DEVRA: This is Dr. Shannon speaking?

KAISER: Or Dr. Van Slyke, his deputy. You will no longer have a relationship with the regional offices and the State Health Departments as far as Cancer Control

activities are concerned. By this time, heart disease control activities were having difficulties, perhaps part of the background was that mental health, heart disease control, and cancer control activities were placed in the same basket, and a decision, an overall general decision, was made which resulted in the old Cancer Control Branch having to divest itself of State public health cancer control activities.

DEVRA: How did you feel about this?

KAISER: Not good. I thought it was a mistake to actually break up the combined team approach that we had working at least somewhat successfully through the years, and even though there was no discussion of it, or I was not asked whether I would like to have this done, I would follow through with the decision that had been made. It was during that time that I asked to talk with Dr. Shannon repeatedly. I think he granted me one audience out of 15 or 20 requests.

DEVRA: How long had you been in the directorship, maybe a year or so?

KAISER: I think a lot longer than that, but I just can't tell you exactly how long it was.

DEVRA: Were there hints for a year or so that this was going to happen?

KAISER: No, it was more or less a surprise happening, and certainly having Dr. Van Slyke, the Deputy Director, advise us was totally unexpected, since he had been Director of the heart disease program in the Heart Institute just prior to joining Dr. Shannon in the NIH Director's office.

I suppose, looking back, I might have had some premonition about it, had I been paying a little more attention to what was happening in the field of mental health. The State mental health program was being talked about with as far a separation from the Institute of NIH as possible. That actually began about a year before it happened to us. Dr. Felix at that time was a little more resistant then; by this time, Dr. (J. R.) Heller was Director of the Cancer Institute.

DEVRA: What did he do and how did he react to this obvious schism?

KAISER: Well, Dr. Heller was the great compromiser, he was a very nice person and I have a tremendous amount of respect and admiration for him.

DEVRA: Oh yes, I have interviewed him. I hope to see him again. But he was a great compromiser...?

KAISER: Yes, he was, he was not one to rock the boat.

DEVRA: So if you went to him and asked, "What the hell are you doing to my job?" ...

KAISER: He would sympathize with me, but he wouldn't take any overt action to save us, no.

DEVRA: What about Mike Shimkin?

KAISER: He eventually told us that this is what was going to happen. He had been told by Dr. Van Slyke to tell us. So it was really handed down from God. I never had the chance to talk to Shannon about it, not to this day, not about this, no.

DEVRA: What did your Advisory Committee say when they were confronted with this---here, a rather substantial piece of your program is peeled away...

KAISER: I think that what had happened in recent years, at least at that time anyway, in recent years was that "research" had become the magic word. A tremendous amount of dollars were being poured into the pure research effort. So that if we were to survive at all as an organizational unit, I think the Council felt that we had to get more research oriented, call this applied research or clinical research, anything but cancer control. Even though the (enabling) Act specifically spelled out "cancer control" as the definite entity that the Cancer Institute should engage in.

DEVRA: So the substance of this applied research was, then, the search for a single diagnostic test? Also epidemiological studies. Was Harold Stewart with you then, no, Harold Dorn?

KAISER: Yes, he was on my immediate staff.

DEVRA: You had several other statisticians, Sid Cutler, Haenszel, Geller; you had the core of what has become the Biometry Branch. They came out of your group, right?

KAISER: Milmore was in that group, too.

DEVRA: Milmore was in that group as an epidemiologist. Dr. Gilliam was still living then?

KAISER: Yes.

DEVRA: So the Epidemiology Branch really came out of this enterprise. Dr. Hueper was still around while all this was going on? (Hueper)

KAISER: Yes, he was still there when that happened. He retired for age reasons sometime after this. He might have added some fuel to the fire, I don't know. That's one time I did discuss with Shannon. It was about Hueper sounding off in the public press about one of his findings. What study it was I can't even remember now, but...

DEVRA: How did Shannon react to that?

KAISER: He thought it was distasteful and practically forbade Dr. Hueper talking to any news reporter from that day forward. Oh, of course, this upset Dr. Hueper tremendously. And he--complied, I should say, but not without considerable conversation, sounding off and talking. I think probably a real rift developed between him and Shannon. Although he really retired because of years

of service, he could have continued beyond 65, he just made up his mind that when he got to be 65 he was going to get out of Shannon's clutches.

DEVRA: How did Dr. Hueper get along with people like Harold Dorn and Haenszel? Did he have anything to do with them?

KAISER: Yes, they interrelated, interreacted. I think probably Dorn got along with him a little better than Haenszel, but he was later, of course. They thought enough of Dorn in the front office to take him away from me and maintained him over there. Then we got Tom Dublin, the son of Lou Dublin. He came to the Cancer Institute and was on my staff for a while in the statistical and epidemiologic area. He was an MD, however. And then when they got this real breaking-up business, they took away Dorn. I kept Dublin on for a little while; then all of a sudden he disappeared "by extraction," withdrawal, rather than any consultation about it at all. That was about the time that Mike Shimkin came on the scene. This was getting pretty close to '60 or '61.

DEVRA: What was Dr. Shimkin's role?

KAISER: He became head of what was called the Biometry Section out at NCI. He had been in the research lab prior to that.

DEVRA: Well, now was that a parallel section or was he under you? Was he under you at this point?

KAISER: At that point he was working with me, yes. Then we had the great reshuffling. Mike didn't disappear right away, but he did within a year's time. He decided that he had had enough of this mixed-up outfit.

DEVRA: And he went to Temple. But when he was in your Field Investigations group, he was the head of what was called Biometry and he had, then he got Haenszel, Cutler, Schneiderman. They were sort of moved over under him and became more of a service unit rather than an investigatory research unit?

KAISER: Yes, and I think that's what caused us this split. They were trying to separate the service elements from the national investigative or research elements.

DEVRA: The Epidemiology Branch was left with you or not?

KAISER: They were temporarily, they were still there when I left, come to think about it. Milmore was still there.

DEVRA: Why did you decide to leave and what did you do?

KAISER: What I did was nothing. What happened was the Director of Chronic Disease...he is now in private practice out here...

DEVRA: In the Public Health Service?....Dr. Leslie Knott

KAISER: Yes. He called and asked me if I wanted to come to the West Coast. I took all of five minutes to say yes. By that time the Field Investigations and Demonstration Branch had been reorganized again.

DEVRA: Not by you, though.

KAISER: No, and we had become the Diagnostic Research Branch, which further restricted the scope of our activities.

DEVRA: And your budget?

KAISER: Right.

DEVRA: And your relationship to public health objectives? You were no longer in any sense a public health enterprise, you were pure research, as it were.

KAISER: And almost entirely restricted to diagnostic procedure investigations.

DEVRA: Concentrating on what sites, do you think?

KAISER: We had pulmonary, cervix, breast. We did some breast secretion studies. Part of my crazy idea was that since some of these cancers, carcinomas exfoliated, why didn't all of them? Well, there are plenty of pathological reasons why they don't. I had to find out the hard way, I guess. And, gastric cancer, colon cancer...

DEVRA: Most of this was by contracts and grants to outside groups, or was this also work that was going on right there in your own program?

KAISER: With the exception of Hagerstown Laboratory, almost all of the other activities were on grants and contracts, all over the country.

DEVRA: But your field of activity was being shriveled up...

KAISER: Very definitely, being narrowed down to this.

DEVRA: Had Biometry been pulled out by then?

KAISER: Actually, that's when they became part of Dr. Shimkin's staff. They were really elsewhere. Haenszel was Head of that Biometric and Epidemiological Section. Shimkin fancied himself as an epidemiologist. Most of the epidemiologists I had on my staff didn't think that he was, and they gradually drifted away from Shimkin and he gradually left the Institute. That's when in essence he became more of a Field Investigations and Demonstration Branch and we became the diagnostic research branch.

DEVRA: During this whole period, prior to the phone call offering you a chance to come to California, did Lew Robbins ever come out to learn what you had been doing before he took this operation downtown?
(Lew Robbins)

KAISER: We got a call from him one day. He said he was coming out over that weekend to pick up the files related to the State program. I must say that Lew Robbins was one of the men whom we had trained way back when to be a regional health consultant in cancer control. He was one of my original cancer control consultants.

DEVRA: Is that right? Now where did you send him?

KAISER: I don't remember where we had him, whether he was out in this direction or in the midwest. I think maybe in the midwest.

DEVRA: Maybe he was one of the few that took it in, in a sense.

KAISER: I think it must have been the midwest, because there was an office in Chicago at that time; there were only five across the whole country and they were called district offices.

DEVRA: He was trained as a cancer control officer to go into a regional office.

KAISER: Right. I believe he was in the midwest and Dr. Graning was in Atlanta. I don't remember whether Cohart was up in New York or in Boston. Behling was out here. I forgot who the fifth one was. He must have been across the bottom states. (William Ross)

Anyhow, Lew Robbins had been trained in cancer control. I knew him. And what he had done in this interval, I haven't the slightest idea, but I said, well, I have been told to give them to you, so come on out and we will see that you get only that portion of it. Dr. Hon, who was really a bird dog when Robbins came out, came over that weekend and saw that he got only the files that related to the state grants program. It was after that, about one half dozen years, that they set up this smoking business. I think once he got those down there, I rarely talked to him on the phone, maybe once a month or that sort of thing. (Advisors)

DEVRA: Now, you still had an Advisory Committee, but it must have been a different kind of Advisory Committee.

KAISER: Yes, it changed.

DEVRA: Because the focus had changed now to these diagnostic devices so you had maybe more clinicians, fewer laymen.

KAISER: Yes.

DEVRA: Were you still at this point privy to making some recommendations about the people who would serve on that Advisory Committee?

KAISER: Yes, as a matter of fact the Secretary established, as a ground rule somewhere along those intervening years, we couldn't blow our nose without getting approval from the secretary of HEW. He couldn't do anything about it either. If we sent a name through. We were permitted, and that's the right word, to send up a bunch of names, send them on through, for so-called clearance. (As a matter of fact, I remember putting Les's name on there repeatedly. We never got through to him. I have no way of knowing why to this very day, but we never did make it.)

But the procedure was such that we could put a lot of names on a list. Shannon wouldn't necessarily send them all down, but the Secretary's office would not necessarily look at the ones that we did send down. He had his own ideas about

who he wanted on, and then, we had so many Secretaries there for a while. One of the Secretaries decided, all of a sudden, that we had too many Advisory Committees and too many Councils, and the word came back down that we had to eliminate some of them. Actually, I didn't eliminate my committee while I was in residence. I think somebody else did after I left.

I think Robbins had a Cancer Control Advisory Committee. But somewhere in that interval, I don't remember whether it was Folsom or another Secretary said---and then that voice got louder and louder and more restrictions were placed on the selection of people---and it just got harder to select anybody. This all happened subsequent to the time that Shannon told me I had a captive committee and I could control them any way that I wanted to. That is not true. They had independent lives.

DEVRA: How many times did Shannon say that to you, that you had a captive committee?

KAISER: Only on two occasions that I can remember. He probably said that to me on the basis of his getting instructions from the Secretary's office about who could be put on advisory committees. I am real vague about the timing in there, but I think he was getting pressure from above that they weren't selecting people that he had listed as selections. So I thought, well, I might as well pass the buck and share the wealth with the rest of these peons down here. They can send me all they want and I'll send them down, they just won't get selected. He wasn't joking about it. He was serious. It was an accusation, really. I said, well, you're all wrong about that, and to prove it why don't you come over and sit in, the next time around. He did come over and sit in. After the meeting, after the second time that he sat in, Shannon said to me, "I still think you have a committee that you control any way that you want." "If that didn't prove it to you," I told him, "I don't know any other way I can prove it." So we let it go at that.

DEVRA: You really had a cold war with him. Of course, you weren't the only person...

KAISER: No, no, by no means was I the only person. Lots of them had. He was not the most popular Director of the NIH. I think I inherited part of the difficulty, this final difficulty, because the NCI was the first Institute of the NIH. At that point, I don't think anybody really envisioned a large Institute; I know that they didn't envision the clinical center. They didn't envision that being practically as big as the Public Health Service was moneywise prior to that time. I think there was considerable resistance, because it was the first Institute, and it had all its freedom.

I guess the next one was the Heart Institute. What did they do? They came along and asked me, "Well, what about the Cancer Institute and this organization?" I said, "I can tell you all about it, I wasn't here when it came into being, but I can tell you all about it because I started with this thing as my bible." I just took the Act. I said as long as the Act says I can do it, by God, I am going to give it a try, and that was my attitude. If it had any hope for the cancer patient at all---and I can save a few lives---and it says I can do it in here, I can loan radium and I can do this and I can do that, by God, I am going to get some money and do it. Lo and behold, to continue

that little story, the Heart Institute got a copy of the NCI Act and copied it word for word and came into being. Of course this is all pre-Shannon.

Then the third one was mental health, and then we began to proliferate like this. You scratch your head and you have an Institute. Some Congressman was bound to have arthritis or diabetes, or this, that, or the other thing. But we never did get a Chronic Disease Institute.

DEVRA: Did you work for that?
(Chronic Disease Institute)

KAISER: Yes, I tried to, but there were some of the older scientists on staff who had what they called the Hygienic Laboratory, which was down at Henrick's Brewery downtown, next door to the Public Health Service headquarters, a little building on Constitution Avenue. There were some of those---God rest their souls---who were pure scientists. They really were honest-to-God researchers, and this was their field. By golly, if you reared this way or that, they would have no part of it. They, I am only talking about the Cancer Institute piece now, because that was all they had, that was the big Hygienic Laboratory. That became the Cancer Institute because that was a going operation. A PhD, Dr. Voegtlin, from the Chicago area, came in as the Director, and he was there for many, many years. Spencer was really just a token Director.

What I was trying to say was, that they didn't understand that there were other areas that they could spill over into. You did research for research's sake, pure research, and if you happened to stumble across something that applied to the patient, well, so much the better, but let's don't get practical to the point where we want to do that.

That was Shannon's philosophy--pure research, pure and simple. Now when I got there, Henry Sebrell was in charge of NIH; the one just before him, Rolla Dyer, was there when I first came to the Institute. He had been on some of these epidemiologic studies way back in his early career, in the Public Health Service. He had gone out to privies and saw the connection between this, that, and the other, he was with some of those old pioneers in the Public Health Building. He knew what it was to get something practical in the way of research epidemiology done, so he welcomed me with open arms, he wasn't at all resistant. Sebrell was the same way. He was opting for something that isn't even popular today--nutritional research--and he is still, if he is alive, with the Institute in New York on nutrition. He could understand what the score was.

But Shannon comes in right straight out of the lab and says, pure research is the only thing. Then he got into administration. Coming from this background, he was no administrator, pure and simple. You can ask any of 6,000 people who worked back there, he ruled it with an iron hand. No compromises. We had him over to the house socially--he was a right good Joe--but Monday morning, he would be the same old guy all over again.

DEVRA: Was he really a tyrant?

KAISER: Yes, that's my opinion.

DEVRA: It is interesting that his wife was a public health trained person.

KAISER: Public health trained, oriented and operating. She operated in the National Health survey all the time that he was at NIH.

DEVRA: Somehow it didn't have much influence on him with respect to cancer control.

KAISER: She tried to defend the heart disease control program to him. I know this personally, because I heard it going on over a little cocktail party we had with all the people at the "station," as we called it.

DEVRA: You were friendly with him socially. He lived on the "compound," the "reservation," excuse me, you lived on the "reservation?"

KAISER: Yes, he lived two houses away from me.

DEVRA: Oh, was he neighborly?

KAISER: Not very friendly, but he wasn't to anybody.

(State Health Departments)

KAISER: .....I can't blame it all on Shannon. I don't, because during the same period, the Service was having all of these reorganizations. The morale of the senior officers was going down hill and it just kept going down. Nobody did anything to discourage it. The togetherness disappeared. I think this was just a symptom of the times, just as the State Health Departments changed their mentality. Their concept of what they should be doing was changing. They didn't have as many reorganizations as the Public Health Service, but the Service didn't know in what way it wanted to go. There was no way of getting anybody together. Prior to that everybody looked to the Surgeon General. He had a group of advisors immediately around him. Incidentally, the Surgeon General was the Chairman of the National Advisory Cancer Council, as he was supposed to be of all of the Councils. But they got so numerous and he had many other things to do, so that eventually he ran out of time. While I was there, before Dr. Scheele became Surgeon General, I was helping Scheele run the Institute, the total Cancer Institute.

DEVRA: Is that right?

KAISER: Yes, for a while.

DEVRA: You were more or less his unspoken deputy?

KAISER: Yes, I was in his immediate office.

DEVRA: When did you get involved in helping plan the building of the clinical center at NIH?
(Clinical Center planning)

KAISER: Oh, that was in its very early stages. The 50s, it must have been, I can't remember the exact year, yes it was in the early 50s.

DEVRA: And you worked with, who was the other person?

KAISER: Dr. Allan Eschenbrenner, who was from the Pathology Section of the NCI. He was a young man, as I was in those days. I think that this evolved on us because Dr. Scheele, having been Director of the NCI, and going downtown to become the Surgeon General, had some influence. He had the idea of a clinical facility, where clinical research could be done directly. It had to evolve through the Cancer Council. Perhaps Dr. Farber had a lot to do with this. I know definitely Dr. Dudley Jackson, a private practioner in San Antonio, Texas, had a lot to do with it. They were also looking for something for Kaiser to do at that point, and Scheele would say, "He is sort of available, he is in between engagements, so to speak. Why don't we just pull him off of whatever he is doing and put him over there." I got to play around with a lot of the plans for the Clinical Center.

DEVRA: Did you pick most of your staff except for Dr. Hueper, who was already there when you arrived?

KAISER: Most of my staff, yes.

DEVRA: You brought in Haenszel?

KAISER: No, that was by Shimkin.

DEVRA: You brought in Sandy Gilliam?

KAISER: And Jack Dunn and...

DEVRA: Marvin Schneiderman, did you bring him in?

KAISER: No, I think that was also Shimkin. That was at the tail end of my stay back there. That is how the Biometry Section got established.

DEVRA: Well, we have covered the whole spectrum, temporarily, of your experience as the Chief of what was the Federal Cancer Control Program---and also the contraction of your program first to the Field Investigations and then to the Diagnostic Research Program--and the splitting off of what was the activist program with the States and public health aspects of it. That was in the late 50s, 1958 another reorganization, at least the field application of cancer control measures was pulled out of the Institute and transferred to the Bureau of State Services, under the direction of Dr. Lewis Robbins. You got a phone call offering you an opportunity to come to California as what?

KAISER: As the Regional Chronic Disease Consultant. I actually came out here in 1961.

DEVRA: And you remained here indefinitely in that capacity until you retired in 1974.

KAISER: Well, I became Assistant Regional Director, and have a record for being the longest Acting Regional Health Director in the Public Health Service. I was Acting Director for almost four years. I never made it to full-time Regional Health Director. During this period, the PHS went through at least four reorganizations.

DEVRA: And the office of the Surgeon General was abolished. The regional offices, then (1973), became sort of secondary foci of importance.

KAISER: But, I carried the chronic disease consultancy for three or four of those years. One time, I was Deputy Regional Health Director, Chronic Disease Consultant, and Health Manpower Director. I had all three of them at one time. Then Dr. David Brand, who was Head of the Heart Disease Program in Washington--- I recruited him to the Chronic Disease Program out here---became our Chronic Disease Regional Consultant. Then I was Associate Regional Health Director for Manpower and Deputy Director at that time.

DEVRA: And your title when you retired in 1974 was what?

KAISER: Director of Health Manpower Education.

DEVRA: Reporting back to Ken Endicott.

KAISER: That's right. He was in the Cancer Institute for a while. They got into a lot of arguments about reorganization. He was split off, and he became sort of a nonentity sticking out there all by himself, and he had been a pure bench research worker too, a pathologist.

DEVRA: I'll tell you. I have three more questions, if you have enough stamina.

Looking back on your career in Cancer Control, how would you evaluate the progress achieved during your leadership of the National Cancer Control effort at the Federal level? For example, what pathways would you have liked to have pursued that were not realized?

KAISER: Well, I would certainly hope that the public health aspects of the Cancer Control Program would have had more significant accomplishments to their credit. I don't think that the clinicians ever really felt that this cancer business, as they like to talk about it, was really a function of the public health agencies. I don't think that they were ever convinced that this ought to have been in the realm of public health activities.

DEVRA: Do you think they did things to prevent them?

KAISER: They said well, you don't catch it from each other, it doesn't have an infectious element or a contagious element, and you have no means of applying it to the mass population. True, you can accomplish something by education of the public and of the medical profession and other professions, nurses and so forth, but that really, in their thinking, is not a function of the health department. It's okay for them to keep statistical records and maintain their tumor clinic records and that sort of thing, deaths and so forth, and maybe do some epidemiological studies, but that was usually thrown in as an afterthought. I don't think we ever accomplished full acceptance in those terms and we certainly didn't get a fire built under the epidemiologic approach that should have been built there and had been so slow in coming about. Now, they are reluctantly accepting some of the findings that were made years and years earlier, but what's happened in this interval, 20 years time, a lot of lives have been lost.

DEVRA: Years ago, one of the recommendations of the National Advisory Cancer Council, I think this was as early as 1953, was we needed to train more epidemiologists and biostatisticians. Whatever happened to that resolution--the impact that it had on your program?

KAISER: One thing that happened to it was that I supported an epidemiologic training program at the CDC, Atlanta, Georgia. I paid for teachers and stipends of students that might be interested in cancer control.

DEVRA: Was this a short course in epidemiology of cancer? A year to train some cancer epidemiologists?

KAISER: I supported it for four years.

DEVRA: How many trainees came out of it?

KAISER: Cancer epidemiologists? None.

DEVRA: Why?

KAISER: I don't know the answer to that. I guess there wasn't that much of a germ of interest stimulated in cancer epidemiology. Oh, I do know. Cancer epidemiology is real hard going. Very tough, you have to work at it like crazy. And over a long period of time. And I think that most of the people that we had at CDC in training, that they recruited, mind you, and tried to graft on to them some cancer or chronic disease epidemiologic knowledge, were young people who couldn't wait that long, couldn't be that deeply interested in cancer as an entity.

DEVRA: Was this the one that Alex Langmuir had organized? You gave his branch the money to do this, pick these people, train them for four years?

KAISER: For four years. Then, we decided that we weren't getting anything out of it and we stopped supporting that particular training program.

DEVRA: Are there any other pathways that you would have liked pursued that were not during your tenure?

KAISER: Not beyond what I have mentioned, I guess. I really had a free hand and I got to do a lot of things that the average Service officer didn't have a chance to do. I had an opportunity to do research on my own. I had my own lab to do clinical activities as well as administer programs on grants and contracts, fellowships, traineeships. I had my cake and I was able to eat it too. I really enjoyed it.

DEVRA: You did. Would you say of your whole career this was the most significant period?

KAISER: I think so. I had the most fun then, I am sure of that. In a sense I was my own boss, once I was in charge of the total program. We had a lot of enthusiastic employees, it would have been no fun if they weren't enthusiastic. We worked as a team essentially, all the way through.

DEVRA: Even at the end when it got pretty rocky.

KAISER: Even at the end, we were supporting each other as team members.

DEVRA: Did you all leave at about the same time?

KAISER: Roughly so, I would say over a year's time.

DEVRA: Everything just split up. During that period, let's say, 1946 to 1961, what efforts or pursuits do you think might have been overly emphasized or maybe too much money was plowed into them?

KAISER: As far as the Cancer Control Program was concerned, I don't think any. I don't think we had an excess of money at any time. I think our program progress kept pace with the money as it was appropriated by Congress. Initially, when I arrived here, I was a very popular fellow, because I had a whole bagful of money that the research side could tap. Such a simple thing as getting automobiles, government autos, nobody except Cancer Control Branch had the authority to buy automobiles. So I was real popular, just by law. It was written into the National Cancer Institute Act, the framers of that Act were really all-inclusive thinking fellows apparently, because they covered all avenues as far as the entity of cancer was concerned.

DEVRA: They really envisioned that you were going to get into automobiles and take this "stuff" out to the people?

KAISER: Yes, that's right.

DEVRA: Did you do that? Do you feel that you did that?

KAISER: I think that is probably one of the areas where we were the least successful. I think we came to a compromise on this with the ACS. We felt that the public education aspect was heavily their realm. Even though we did produce public information, that's the right word for it, in relation to breast self-examination; we actually started the first Cancer Control Bulletin in conjunction with the M.D. Anderson Hospital in Houston, Texas.

DEVRA: Did you give them the money to start that?

KAISER: Yes, and we were on their Advisory and Editorial Boards. I helped write some of the articles. I guess I was a reporter in a sense. I would get plenty of experience in the field. I had some of Dr. Hueper's findings published in there, thinking that we might be hitting a different audience, and getting a little more acceptance, rather than just having it published in a scientific journal. We did breast self-examination through that media. We had one special issue devoted to breast self-examination as performed by the woman herself.

Incidentally, that came into being because I was fiddling around with a bunch of statistics that came out of the cancer clinics operated around the country. I found that in a very high percentage of cases the woman first noticed the lump in her breast, maybe in 85 or 90% of the cases. So I figured, if we can get

women looking for them, unusual masses and so forth in their breasts, maybe we can really find cancer of the breast earlier and thereby get surgery or radiation or whatever the choice of therapy is, to this individual to save her life. We never did prove that that actual stage of the disease, that earlier identification, really related to changing the breast cancer mortality that much.

DEVRA: That's what Jack Dunn thinks. He says that all these years we have been telling women to do it and we never once have evaluated whether it's had any effect on mortality.

KAISER: We did know for that specific individual, we may have done a great deal.

DEVRA: But we don't know if we have done it for the masses.

KAISER: For mass application, no, I can't answer that either. And just persuading that many women and that many physicians to do something about breast cancer self-examination, in spite of what you may have heard, got the first little nudging of mammography underway and we did that, with Dr. Gilbert Fletcher at M.D. Anderson Hospital.

DEVRA: Was this before Egan?

KAISER: Yes, Egan did his work down there, after Fletcher. Gilbert was the original radiologist and I think Egan came to his staff or something. Wasn't he at Houston too?

DEVRA: Yes.

KAISER: Gilbert Fletcher, we just sat down and were talking about it one day. If these masses are large enough, and you can get some examination that will differentiate the normal tissue from the tumor tissue, if there is a change in the density in the tumorous tissue, could we bring that out on an x-ray?

DEVRA: Did you support Fletcher in some of this?

KAISER: Yes, very definitely. We also supported Egan when he had his larger study and went into the community examination.

DEVRA: The reproducibility study. Now, let me understand some of that. Some of the money for that reproducibility study and some of the moral support came from your Branch and some came from downtown when Lew Robbins was there.

KAISER: I think he picked it up after this reorganization and all of it came from downtown.

DEVRA: The initiation came from your Branch. Did you actually go down to see Egan's work?

KAISER: Yes, and I also saw Fletcher's original work.

DEVRA: And were you impressed?

KAISER: I was impressed. I think it does accomplish what Egan claims it does. We weren't in at the stage where it became common knowledge for radiologists, though.

DEVRA: What did Fletcher show you that gave you some confidence this might be a useful screening or detection mechanism?

KAISER: He had actually run a series of cases on admissions to the M.D. Anderson Hospital where they had known breast cancer, and then he ran some where there was no finding of breast cancer; so-called normal cases, I guess you would have to say; at least they didn't have breast cancer.

DEVRA: How did you hear about him?

KAISER: I think I was just kind of browsing through the literature originally.

DEVRA: You were interested. Did you go down to see him? Is that what happened?

KAISER: Yes, that is exactly what happened.

DEVRA: So you saw what he was doing and what did you suggest to him?

KAISER: Oh, I suggested that he do more of it and enlarge the size of the study and get some true unknowns, you know, people who weren't being admitted to the cancer hospital, which was what M.D. Anderson was.

DEVRA: Asymptomatic women.

KAISER: That is exactly right.

DEVRA: Did you give him the money to do it?

KAISER: Yes. We underwrote it through a grant arrangement.

DEVRA: That was before Egan.

KAISER: Yes. Then when Egan came along, well, we also supported him in his early work. Then the reorganization took place. I also tried to get duplicate studies run at Sloan-Kettering Institute in New York.

DEVRA: Following Fletcher's experience or the Egan experience?

KAISER: Following Fletcher's.

DEVRA: What happened when you took this idea to New York?

KAISER: They were not too receptive to the idea, Dr. Rose with whom I had personal contact, as well as with Dr. Purdy, Stout and Haagensen, the surgeon who did most of the breast surgery up there. He was interested in breast secretions at that point. I had done some breast secretion work, that is we had supported some.

DEVRA: As a better index perhaps?

KAISER: Yes.

DEVRA: We are coming back to that again, ductal cytology.

KAISER: Yes, and so he had this mouse colony that he was milking. That's a whole story unto itself, but that's how I got there. They were not all that much interested. Then, in fact, a couple of radiologists whom I had on my Cancer Committee from Philadelphia, Edward Chamberlain and who was the other one?...

DEVRA: Gershon-Cohen was never on your committee, was he?

KAISER: No. We underwrote a study of Cohen's, though.

DEVRA: On his radiologic studies of the breast.

KAISER: That's right. That was one of our special projects. These names keep popping back faster than I can tell them to you. Oh, what was the other radiologist's name? Anyhow, he was a bigwig in the College of Radiology and I sent him down to talk with Fletcher at M.D. Anderson. His name was Eugene Pendergrass.

DEVRA: Was he impressed with what he saw?

KAISER: He was more impressed than Dr. Chamberlain was. So, when the time came for the special project to be reviewed and the grant request came through, while he told the whole Committee and the Council, incidentally, because on occasion I would take one of my Committee members in with me to talk about a project before the entire Advisory Cancer Council---of course, in some instances that influenced the final action and sometimes it didn't. But the whole point is that I did take him down there. He talked to them. Then, the Director of the M.D. Anderson Hospital, R. Lee Clark, was on my Advisory Committee. (You were asking a while back how we got some of these nominees. We started out picking the people who had been in the cancer hospitals to form a nucleus and we built from there. That is how we got R. Lee Clark on the Committee one time.) He tried too, he talked about Fletcher. This is where I first got wind of it. Then I went to the scientific literature and read about it in there. And then I went down to see Fletcher.

DEVRA: In the meantime you already had been supporting Gershon-Cohen. Why was it that Cohen's work never caught on? What was he doing that didn't work?

KAISER: I am not able to answer that positively, but I do know that the Cancer Council cooled off on support for him.

DEVRA: But he did get several years of support from you?

KAISER: Yes, he did. Whatever happened to terminate it, I don't recall. I can't recall that much of it now. You see, I was intimately involved in all these projects. I went in and I felt compelled to tell the Council exactly what was going on, what we hoped to accomplish in addition

to what the applicant had written on paper, because, you know, there are a lot of people who can write grant requests that are as glowing as all heck, but if you read between the lines, they aren't saying anything. Fine piles of paper....

DEVRA: Would you site-visit any of these potential projects?

KAISER: We always site-visited.

DEVRA: You had enough budget to site-visit all these potential applicants for special projects?

KAISER: I correct that. We site-visited all the approved special projects.

DEVRA: Oh, but that's after they are going? But you didn't site-visit them in advance.

KAISER: Some of them, yes. I was correcting the statement that I made that we site-visited ALL of them. All of these applications would come in and we wouldn't even know they were coming until they arrived on our desks, unless we postponed those for an indefinite period. The cycle was such that if you didn't get them in and get them reproduced and reviewed relatively soon, it might be two years before the applicant would have a chance to get his request before the Council. Some of them that we knew about we did visit in advance, some of them we went out and stimulated purposely and site-visited on those occasions. Then after they were approved, we went out and site-visited the projects to see how they were going along. We would take members of the Committee or the Council along with us on some of those visits.

If they were debatable items or debatable projects, I should say, that they couldn't make up their mind about when the Council met, we would delay that purposely and not take action until a site visit could be made. We would do that.

DEVRA: Did you site-visit Shelby County before you gave them money for the cytology program?

KAISER: I believe that we did.

DEVRA: Because that was a fairly large project.

KAISER: Yes, it was. Yes, we did. That thing kept growing by leaps and bounds. Between the two of us, Dr. Sprunt was a nice guy. I don't know what Dr. Dunn told you about him, but he had delusions of grandeur. He was going to have a second Cancer Institute down there.

The only difficulty about that study was that while it took a certain amount of endeavor to initiate it, it took ten times that amount of endeavor to get it stopped. After we had been supporting it for all those years, from about the 8th, 9th year on we were trying to get a termination point. I tell you, he moved everything except Congress down there. He had the Senate, he had that reporter, I have forgotten his name, the clerk of the Senate, no, the clerk of the House.

DEVRA: From Tennessee?

KAISER: Yes.

He had a great big name on the Washington scene, you would recall it. He was clerk of the House, let's say late 50s, early 60s. He would get the legislation together. That guy did come from Tennessee, I take it back, and Sprunt knew him. Sprunt would get together with this guy and they would talk about this thing and he'd have him come down to visit.

DEVRA: Was it a contract or a grant?

KAISER: Well, part of it was our own staff. It was a combined arrangement.

DEVRA: But some of it must have been a grant which he had to re-apply for.

KAISER: That's right.

DEVRA: It wasn't Kefauver?

KAISER: No, he was a Senator. This is a staff person in the Senator's office.

DEVRA: Jack Dunn might remember, as a matter of fact. He said that three people worked on that study. Sprunt, himself, and Jack Daniels. (He told me they used to come back every night and talk it over with some Jack Daniels.)

KAISER: That's right.

DEVRA: They knocked off a lot of that.

KAISER: He got really perturbed with Sprunt more than once...

DEVRA: Jack Dunn? Oh yes, I am sure.

KAISER: Complained about Sprunt to me like crazy. I would go down and say, "Look, we can't have this going on. You have all these people." I don't remember how many we had---75 or 80 people on my payroll there---plus the grant funds. It was a big operation. I had some 700 employees at one time. Cytology labs scattered all over the country.

DEVRA: What lessons do you think the current and future Cancer Control Programs can learn from the experience that you and your colleagues had, first at relating to the NCI, since that is where the Cancer Control Program has now been returned?

KAISER: I didn't know that.

DEVRA: What would you teach your successors?

KAISER: I suppose so much water has gone under the dam since then, that to really give an intelligent answer, you would have to have a kind of restructuring of our preventive service activities, at the state and city levels. I would hesitate to call it a reorganization. But something would have to be done to the mentality of the subscriber to services as well as the people who

would be providing them. Education is a slow, slow process. You have to keep doing it over and over again. Like so many of these things that have been done before, people suddenly think that they have discovered something new while in effect they have actually rediscovered something. They let up on the educational efforts for a while and people go back to their old ways of thinking. I am reminded of this in relation to breast self-examination.

When we got that film out initially, we couldn't keep women away from seeing it. In fact, I had one nurse full-time, going around the Washington area, you know how many people there are there, showing that film and talking about breast self-exam, how to do it. Full-time, for more than a year. It was hotter than a firecracker. I would say a year or two at the most. Then I guess that generation either continued to do it, or figured that it wasn't worthwhile, and it sort of dropped off. We didn't get any requests for it later.

DEVRA: You never evaluated whether people kept it up, did you?

KAISER: Hundreds of dozens of copies of the thing were made. We sent them all to regional offices. Initially, they were all gung-ho and they got them out. They were used a little, then no call for them at all after about a year and a half.

DEVRA: These kits?

KAISER: Yes, so I don't know what the educational process is there. How to sustain educational interests, that's the issue.

DEVRA: So you think we have to do more to understand how people learn?

KAISER: My daughter doesn't do breast self-examination. I am sure that she has been exposed to it and heard the story about it. My wife, Alice, did. She also gets her Pap test from time to time. We discovered a lump on her, in addition to her present problem, 17 years ago. She had surgery.

I have to tell you this story about myself. I don't have a thyroid. I haven't had one since 1956; I have been taking supplemental thyroid. We are great boosters of routine physical examinations for a long time, in the Cancer Control Program. This was true before they found what I am about to tell you. I went over to our hospital for a routine physican examination. We had to have them every year. One of the young residents there was feeling around down in here and he felt a lump on my thyroid in the part that connects the two lobes (the isthmus). At surgery it proved to be malignant.
We finally had to conclude that, from the standpoint of finding cancer in any form, that the amount of effort and expenditure didn't support our findings. we didn't find enough cancer by this means.

DEVRA: To make a routine physical exam an effective cancer screening mechanism, you mean?

KAISER: That's exactly right. After all these tens of thousands of exams, that was the conclusion we had to reach. We included the Pap test and breast secretion and those that would submit to a colon or rectal exam.

DEVRA: This was supporting groups like the Strang Clinic, cancer clinics through the Public Health hospitals, and where else?

KAISER: I gave some money to Roswell Park. I can't remember if I ever got any to Ellis Fischel Hospital in Columbia, Missouri, or not, but I did at the Swedish Hospital in Seattle, Washington.

DEVRA: But apparently these were asymptomatic people who were being examined.

KAISER: People right off the street. They had no symptoms at all, presumably. We talked them into having a complete physical exam. As far as they were concerned, it was for free. But, of course, it wasn't, I mean your tax money was paying for it.

DEVRA: What else can we learn?

KAISER: We learned, if you are just existing on a dollars and cents basis, you can't do mass screening of asymptomatic people as an economical cancer screening device.

DEVRA: You wouldn't deny that it may be educational, but it's not economical. What other kinds of things did you learn?

KAISER: I don't think that had to be repeated. As far as somebody starting out in Cancer Control nowadays, I don't think we have to do all that over again. I don't think we have to plow the cervical cytology area anymore. True enough, we don't have all the evaluation that we need, but I think in the hands of a physician, practitioner and a good lab review source, this is worth doing on women anywhere in the child-bearing age, more so as they get further in age.

DEVRA: Did you learn any special lessons about relationships with the ACS?

KAISER: It is a volunteer organization. They have frequent changes in their membership and staffing. It gets pretty fragile. Sometimes it's difficult to get continuity of relationships with people there. I don't know enough about the organization at the moment to know if they have a medical and scientific director or not...

DEVRA: Dr. Arthur Holleb.

KAISER: Oh, I know Art Holleb. He was one of the junior men when I was there. I believe that he was over at the Memorial Hospital; yes, I know him. Well, if you can get some continuity of the full-time staff members, I think that would be an advantage in having a close relationship. I think you ought to have a close relationship.

DEVRA: Do you think it ought to be reciprocal? For example, do you think the Cancer Control Director should sit on various committees of the ACS, maybe even on the Executive Committee?

KAISER: I definitely do.

DEVRA: You never sat on the ACS Executive Board?

KAISER: No, but the Director of the Cancer Institute, Dr. Heller, did. It was a long time before we got him into that position where he was on the Executive Board. That was probably Mary Lasker's doing, because she was highly instrumental in the early organization of the national headquarters, with her husband, of course.

DEVRA: But you think this symbiotic relationship between these two organizations, in the long run, assists cancer control?

KAISER: I think that would be beneficial to both groups. Until we got into that sort of involvement, none of the public ever heard of the NCI. I don't know if they are too sure about it now. But, I think that it is an important relationship that should be strengthened and continued, and made as near stable as you can, even with all of these changes.

DEVRA: What if we had no voluntary cancer control body? What if there was no ACS? What would happen?

KAISER: I think the public would not be informed at all. It's just like I just mentioned, no one ever heard of the NCI, before we sort of joined hands with the ACS.

DEVRA: They really became your spokesmen, your publicists...

KAISER: We couldn't make films, so we went to them. Joint names appeared on the films. But, we couldn't produce them as a government group.

DEVRA: What do you think you learned with respect to the State Health Departments?

KAISER: Well, I am afraid that I have a feeling that the State Health Departments have been on a decline for some time. This didn't all happen at once. People don't look to the State Health Departments to do the things they did before.

DEVRA: They don't think of the State Health Departments as leaders?

KAISER: No, and that's the decline that I am talking about. There has been a trend toward...well, you don't have any notable State Health officers or City Health officers, as far as that goes. You used to hear of individuals like Dr. Bundeson. Even though they weren't necessarily good health officers, you at least heard of them once in a while. I can't remember seeing the name of a State Health Department director. As a matter of fact, I don't even know the name of the one here in California.

DEVRA: His name is Jerome Lackner. He was Caesar Chavez's personal physician.

KAISER: That's what's happened to State Health Departments. That, I regret; I wish it hadn't happened and I don't know how to correct it.

DEVRA: But is it still important for the Cancer Control Program at the Federal level to relate to State Health Departments?

KAISER: I think so, because one of the provinces of the Health Departments is public education. They, along with maybe local chapters of the ACS can, if we ever get a vaccine for some types of cancer, the Health Departments would be the logical place.

Because, as I mentioned earlier, physicians in general practice and private practice don't see more than a couple of cancer cases a year, if that many. They wouldn't be really all that much concerned about provision of a vaccine or something that was really preventive.

DEVRA: In your estimation, what three to five individuals have probably contributed the most to cancer control as we know it today? Looking back over all the people and all the scientists...and don't be modest.

KAISER: Well, I like to think that I contributed a little bit, but I don't know whether it's the most. I think that I really am responsible for saving a few lives in the country with some of the things that I did while I was back there. I know I did in relation to my own family. Beyond that, I am sure we got a few cancers early as a result of what I have done.

Ah, that is a difficult question to answer. Charlie Cameron, who was at the ACS at that time, I think, was genuinely interested. Really got shook up when some members of his staff developed cancer and died. I think during that period that he was a real contributor. Some people didn't care for him. Dr. Deibert didn't care for him particularly, but we managed to get the three of us to go along together most of the time.

DEVRA: He was on your Advisory Committee and on the Council?

KAISER: I think Dr. Sidney Farber would be number one as far as I am concerned. He took an entity in the area of medical pathology that wasn't at all popular; and he never gave up hope in the area of leukemia. He did more, in terms of the current treatment of leukemia, than any other person. He was not a writer, God knows; everybody including me and lots of more important people would say to him, "Write this up, put it somewhere." The man died and he hadn't written up anything compared to what he had done in the way of studies...research, treatment, influence, nothing descriptive.

DEVRA: That's really tragic.

KAISER: Now I think of some of the senior men like Dr. Ewing. I think Dr. Ewing did a tremendous amount for cancer and cancer control, too. Some of his assistants, too.

DEVRA: Did you have anything to do with Dr. Pap, by the way?

KAISER: Yes.

DEVRA: Do you think that he made a contribution?

KAISER: I would say that he made a contribution, unknowingly. When he developed the smear business, that wasn't what he was looking for at all.

DEVRA: What was he really after?

KAISER: He was concerned about the menstrual cycle in women. This got buried in the scientific literature. He must have been fiddling around and reporting on that in 1920. It took all of that time, up until '47 or '48 before anybody really got around to looking at this and saying, "My goodness, this might mean something."

DEVRA: Do you think that was a long time?

KAISER: Not by ordinary standards. That's probably the usual incubation time. I am concerned that this kind of information can get buried in scientific literature and nobody does anything about it. I don't know, since there have been so many publications and they are going up hill like this, maybe this is the place for the computer. Maybe either putting some of this stuff into a computer, I don't know how you would do it.

DEVRA: So you could differentiate things that really were worth pursuing and worth demonstrating?

KAISER: Right. I did it in my own case. I went to the literature and looked up things that I thought were relevant just by title.

DEVRA: You did this when you were a leader, active in cancer control. This gave you hints of where to put your programs and where to put your money.

KAISER: I think the answer to cancer may already be in the literature. (I don't want to be quoted on that.) When I was at the Cancer Institute, to survive you had to publish or perish. That was the ground-rule. People were so busy doing that to hold on to their jobs over on the research side, in a sense continuity was important too, that they just put in a potboiler from time to time, as they called it, to keep their name on the good-boy list over here. They were doing general research. We can cure cancer in some animals.

DEVRA: Some types of cancer. The mouse people have been saying that for quite some time.

KAISER: I think maybe some of this can be carried over to the human problem. I think also a complete review in depth of literature would benefit the public in a solution to the cancer problem.

DEVRA: Do you think some of the major cancers are preventable, by things that people do for themselves?

KAISER: I think in the area of environmental cancer, if you choose that term, yes, very definitely.

DEVRA: Do you think the government should be more aggressive about environmental control of possible carcinogenic agents? That we are putting enough of our resources into that aspect of prevention?

KAISER: Carcinogenesis has been a broad field of investigation by the NCI for many years. They get up to a certain point and then they don't go further.

They don't carry it over to enforcement or anything else. You would need a new agency that really had some teeth in it to get beyond that to do something about it.

DEVRA: Even when you were there, they were doing environmental studies, not just Hueper's work. Did you sense that there was always a sort of frustration that the NCI did not have powers of enforcement?

KAISER: From my standpoint, yes.

DEVRA: In the environmental area, you felt that, too. Your people were coming up with environmental clues...

KAISER: I couldn't say. Well, you are right, or you may be right, but what can you do about it. We didn't have the enabling legislation to get into enforcement And if we did, there's somebody else in the Service area that should be doing it.

DEVRA: Not research. In your estimation, where do you think cancer control efforts should be concentrated next?

KAISER: Well, I suppose it just depends/on whether we can afford environmental corrections. I don't know whether we can afford socialized medicine or whatever you want to call "medicine for everybody." God knows, we can't afford it now on an individual personal basis.

DEVRA: Would that help cancer control?

KAISER: I think that might help cancer control.

DEVRA: If we organized around a national health insurance scheme providing universal coverage, financial incentives...

KAISER: And then don't let up on the possible preventive sides or looking for means of prevention. Viruses are very intriguing, they have a relationship to some types of cancer. We did it with polio. I think that some day there will be a possible vaccine developed in relation to some viral types of cancer.

DEVRA: But the viral exploration is really not control. That's more research; but what about prevention, good old-fashioned public health?

KAISER: I don't think that has been exploited sufficiently.

DEVRA: You tried to show the way, using public health principles.

KAISER: I tried. I got people who were in the field. That was a disappointment, really. We couldn't move them "off the dime," so to speak.

DEVRA: Who were they that you couldn't move?

KAISER: I guess the public generally and certainly Congressmen became disenchanted with State Health agencies. Then something happened after World War II. Everybody got to thinking, well, if we developed an atomic bomb, then we can do all

this research and cure cancer. They even said it---the Congressmen. They did not know what they were talking about. Let's put all our dough in the research basket and we'll solve all of our problems. I don't think that we solved any so far with this concept. But the basis of this, the appropriations for NCI generally went through the roof. It got to the point that we were supporting more than half of the research that was being done in the country. It didn't relate to application as such. I would say from 55% it dropped off some, and the appropriations have dropped back. God knows, we don't need any more institutes to pursue categorical diseases. They are pursuing general research.

I N T E R V I E W

Interviewee: Dr. Leopold Koss

Interviewer: Leon Ellwein

Location: _____

Date: March 5, 1976

Interview with Dr. L. Koss by L. Ellwein, March 5, 1976

LEON: Let me give you just a brief introduction on the project. The project is funded by the Cancer Control Program at NCI. Lester Breslow, who you know, is the principal investigator. We are looking within the time period, primarily, 1945-1970. The purpose of the project is to look at history, to see if history can give us any guidance or suggestions in recognizing the problems that perhaps we made in the past and how we can do better in the future. Our approach in this has been on the one hand to do a general survey, a complete literature survey of index-medicus along with other bibliographic abstract publications, for this entire time period. We have a list of some thousand items that are key things, primarily talking about cancer programs, cancer control programs. Obviously we can get all kinds of research items, that is not the intent here. In addition to that, as I mentioned, we are picking up on certain scientific and technological advances that have been identified as being important. For these things we are developing a history of how they came about; what are the personalities involved; what are the difficulties involved; how long did it take for widespread use; if wide spread use took quite a long time, what are some lessons about the future.

KOSS: Let me ask you this, do you want my comments to concentrate on anything specific? Any specific organ system?

LEON: Yes. For the purpose of this interview maybe we can leave the broad things for last in a summary. Most important for this interview, and you are the first person of maybe one of two people that will probably be interviewed in this area -- we haven't really identified the second person yet -- the purpose of this interview is to talk about cervical cytology.

KOSS: I would be delighted.

LEON: So we might start with you just saying whatever you want. Maybe we want to take it chronologically. I have done a literature review and I am aware of some of the early work going back to the 19th century and so forth. Maybe a good starting point is the work leading up to Dr. Papanicolaou.

KOSS: Let me begin with what I believe is the beginning, and that has to do with the discovery of the sequence of events in human carcinogenesis. Interestingly enough, this was done first for the uterine cervix. You must realize that until the early days of this century, the concept of where cancer came from was a very vague one. There were all kinds of embryologic theories that cancer were aberrant tissues that under the impact of unknown factors, progressed to malignant tumors.

Actually the idea that cancer was really a misdirected development or misdirected evolution of the normal cell, is a very recent one. It goes back only to the early days of this centruy. The recognition that cancer was not an overwhelming disease from the very beginning but it was a disease that progressed in stages is relatively new. And for carcinoma of the uterine cervix, which is the subject matter in which you are mainly interested, a key contribution was by a man whose name was Schauenstein, who in 1908 published a key paper as a part of his doctoral thesis. He was from the city of Gratz. Oddly enough I just learned this this past October. I had no idea of where he

really came from. And this chap Schauenstein, whose photograph incidently I have if you want to have a look at it, wrote a modest paper that was part of his doctoral thesis on the development of carcinoma of the uterine cervix. In this paper he pointed out that prior to invasion, let alone metastases, there was an identifiable state of cervix cancer which was confined to the epithelium of origin. This paper, for the first time, introduced the idea that the carcinoma that is the cancer of the epithlium originated from abnormal epithelium. That was the first time that this matter had been so clearly stated. It was of such interest that subsequently a number of people, whose names may not mean very much, worked on this topic. One of them was a gynecologist from Mt. Sinai Hospital in New York. His name was Rubin. Rubin wrote a paper in 1910 in which he confirmed Schauenstein's observations on early stages of carcinoma of the uterine cervix. There was also a major book published, I believe in 1917 or 1918, by Schottlaender and Kermauner, again in Germany, in which another important point was made and that was the separation of cancer of the cervix from cancer of the endometrium. These two diseases were previously often confused under the single heading of carcinoma of the uterus. Kermauner and Schofflaender separated the two and again spoke of carcinoma in situ and, in fact, to the best of my knowledge, were the first ones to introduce the term carcinoma in situ into the literature. Prior to that, people were talking about superficial cancer, intraepithelial cancer or whatever other modifications of the German equivalents you may find. The German word is Oberflöschen carcinoma, meaning surface cancer.

LEON: Wasn't there somebody by the name of Broders?

KOSS: Broders came on the scene much later. He did use the term carcinoma in situ but this was at a much later date.

LEON: It seems to me that he has been identified as the person credited with that term.

KOSS: Broders was very active in classifying and grading human cancers. He wrote a very interesting paper on bladder cancer in 1921 or 1922 in which he graded carcinoma of the bladder and pointed out that according to grade, the clinical prognosis sis may be different. I don't believe that Broders was the first one to introduce the term carcinoma in situ. Although, he may have been the first one to use it in the American literature.

LEON: Okay.

KOSS: He did it perhaps in 1928-29.

LEON: Someone has quoted him saying the term carcinoma in situ was given to this lesion in 1932 by Broders.

KOSS: That's correct, but my reference goes back to 1917-18. That was Schottlaender and Kermauner. In fact I am certain of it, because I have been very interested in the past history of this disease. Broders, as you will see, my information is correct, came out with this in 1932, and that was in reference to, I think, carcinoma of the larynx or some such lesion for which he used the term. As far as the survey system is concerned, the subsequent history is a very interesting one. When the World War I intervened, it disrupted a lot of thinking and rather good effort. The idea of an early cancer of the uterine cervix came up again in the early 1920's and that was in a different context. There was a man named Hinselmann who thought that the

human eye was perhaps an inadequate way of inspecting the uterine cervix. As a consequence, he developed an instrument which is the colposcope and he started looking at the uterine cervix at the magnification of anywhere from 4 to 20 times. Hinselmann was not a pathologist. Yet he started seeing things on the uterine cervix that you couldn't pick up with the naked eye, that he thought were abnormal. He started taking biopsies of these various lesions and, of course, he asked his fellow pathologists, or the pathologists working with him, to interpret these biopsies. They were totally unconvinced that there was such a thing as carcinoma in situ. Therefore, Hinselmann, fearing the ridicule of the pathologists decided not to class his lesions according to histologic findings but developed a series of classifications which he called rubrics or columns or classes if you will. He started publishing papers according to lesions which he put in rubrics one, two, three or rubrics four. And what was interesting was that in Europe the colposcopy became acceptable too, at least in some University centers. There was a number of people who followed Hinselmann's example and did colposcopy. That was true for Germany, Switzerland, and France to some extent. The colposcope did not, prior to the 1960's, penetrate into America for a variety of reasons. One was that the American gynecologists were always in a hurry and you can not do colposcopy in such a tremendous hurry. You have to have your patient rest comfortable for a period anywhere from three to fifteen minutes, depending on what is found, and the American gynecologists were quite convinced that no American female would be willing to sit in a gynecologic posture for such a lengthy period of time. I rather suspect that they didn't think they had the time to devote.

LEON: What about the training required to use this technique.

KOSS: Oh there is no doubt about it.

LEON: Maybe that was it.

KOSS: There was also no interest in this whole business. The colposcopy angle gave some very interesting results in Europe and there were a number of books published on the effectiveness of colposcopy as a means of discovery of the early stages of cervical cancer which were quite interesting. Some very interesting things came out of it but this never really became a mass means of screening. The next stage in the development of the cervix cancer detection business is Papanicolaou, who in 1924 was working on the cellular manifestations of endocrinology of women, and didn't know beans about cancer. As a matter of fact, since I knew him rather well, I can tell you that I don't think he knew anything about cancer until the end of his days. That's quite
besides the point. He came across some cells in smears
which he couldn't interpret. They were odd looking cells so, to the best of my knowledge he consulted the then leading cancer pathologist in the world, Dr. James Ewing. Ewing, was then professor of pathology at Cornell and also the pathologist at the Memorial Hospital for Cancer. Ewing apparently told him that he thought that these might be cancer cells.

LEON: Was this still in the middle or late 20's?

KOSS: 1924-25. At about the same time, and not knowing anything about Dr. Papanicolaou's work, there was a Romanian Pathologist named Aureli Babes, who, using not a smear the way Papanicolaou did, but using a microbiologic wireloop, was scraping surfaces of cervices to see what the cells looked like that came off the cervix. By then, needless to say, there had been a fairly substantial number of papers going back to 1850 that documented that cancer cells were morphologically

quite different from normal cells. I am not going into this sideline.

LEON: Was he scraping cells off cervix's of normal women?

KOSS: No. He was taking cells from specifically abnormal women.

LEON: He was looking at this in contrast to Dr. Papanicolaou.

KOSS: In contrast to Dr. Papanicolaou who came across these cells, looking for a cell cycle and not for cancer. Babes was, of the two, vastly more successful, because according to what we know today about him he published his findings, or presented his findings for the first time, to some Rumanian Society as early as 1926-27. Then in 1928, April, he came out with a major article in the French publication Presse Medicale with the specific title, The Diagnosis of Cervix Cancer by Smear.

LEON: We have got the article, we discussed it a couple months ago.

KOSS: Oh yes, and Papanicolaou on the otherhand, also in April I believe, perhaps March or April 1928, also presented his first paper which was in the setting of an absolutely unrelated conference, which was a conference on the betterment of the human race in Battlecreek Michigan in the Kellog Foundation's facilities.

LEON: What was that third conference. It was a Race Betterment Conference, in what way race betterment?

KOSS: I think it was just sort of a mixture of things.

LEON: A genetic kind of a thing with a social slant.

KOSS: No, it was sort of a pseudo-scientific slant. It really had nothing to do with the betterment of the human race. It had something to do with the makeup of the people. I have reviewed the proceedings of this conference which are published and Papanicolaou's article is about a page and one half or two of the rather massive volume that has to do with a tremendous variety of topics. I think there is something on alcoholism there, something on child care and something on some entomologic study, and I think you might do better if you do not accept this for granted but go back and inspect it because I am not quite sure exactly how to summarize that. I think it was a very confusing mixture of things.

LEON: Does anyone know of the events leading to his being invited to give a paper, or reason for giving it, to this group?

KOSS: I am afraid that I don't know, but maybe Mrs. Papanicolaou might know. She is the guardian of his archives. She may have a notion as to what may have happened. She would be a good person to consult. She would be thrilled to speak about him.

LEON: One might speculate that he didn't recognize the significance of it.

KOSS: No, not even afterwards.

LEON: Maybe he would have aimed more towards -

KOSS: He had absolutely no concept as to what this thing meant. As a matter of

fact, I hold it from the former Dean Hinsey of Cornell who told me this story many years ago that Papanicolaou didn't pay the slightest attention to this paper. He thought it was just a little something that he contributed. Cornell is where he was, until nearly the end of his life, as you know, and not very well regarded, needless to say, and not particularly highly appreciated until the end of his days .

LEON: What about the people whose interests were in the endocrinology associated with what he was doing?

KOSS: I am going to come to this now because I think its a rather interesting story. He returned to Cornell to do work in endocrinology and he was still after the ovulatory cycle in the human female, and the cancer work was really of tertiary importance to him until Hinsey became the Chairman of the Dept. of Anatomy of which Papanicolaou was never a full member, always a clinical member. You may not know that.

LEON: He had a clinical appointment?

KOSS: Yes he had a clinical appointment, but he never had a full appointment. He remained a clinical associate professor of anatomy until the day he retired and when they retired him, they made him Clinical Professor of Anatomy Emeritus. Although Cornell today is bragging endlessly about what Dr. Papanicolaou did there, they really gave him a very hard time. They never treated him right. Let this be on the record.

LEON: They didn't see the contribution -

KOSS: Not even after he became world famous did they make the effort. You know he was a foreigner.

Of course, I am a foreigner myself, so I might look a little less critically than the people at Cornell, in any event, he never really got the recognition. There is no doubt about it but that is beside the point. Hinsey, when he became the new chairman of anatomy, thought that Papanicolaou ought to, perhaps, spend his time in a little more useful way then just looking at the cell cycle. He sort of ran out of ideas, Papanicolaou, in those days. So Hinsey came across his little cancer paper and asked Papanicolaou about this. And Papanicolaou said, well, I would be interested in it but the gynecologists that I am coming to with this problem would just laugh at me. So Hinsey got hold of Herbert Trout,who was then associate professor of Gynecology or something like that at Cornell, and Trout agreed to cooperate with Papanicolaou and started giving him vaginal pool smears, not scrapes of the cervix, but vaginal pool smears. Papanicolaou dutifully started identifying cancer cells in these smears and developed his stain which is now known as the Pap stain, a polychrome stain. I don't recall exactly the date of when this stain was discovered, but I am sure that you can come across this very easily. As a matter of fact, if you want to hold it for a minute, I can give you some of the dates.

KOSS: This is a German reference and which has to do with the stain. Yes, you see, he published this stain in 1941.

LEON: Right, so that was tied in or relating to cancer oriented.

KOSS: Then he had the first publication with Trout on the same subject in 1941

and then there was the monograph in 1942.

LEON: 1942 or was that 1943?

KOSS: No, 1943 you are right.

LEON: Okay, thank you.

KOSS: Now, there was also a third person who again in a parallel but unrelated way played a major role in this, and that was an Italian named Viana. A former student of mine, Larry Douglas, published a translation of one of Viana's contribution.

LEON: I got that out.

KOSS: You have already got that. I don't recall exactly the dates of Viana's contributions, but he was about parallel with Babes and the early Papanicolaou. In any event, the questions you must ask yourself and which I have been pondering over many times is why was Dr. Papanicolaou successful after several others where at least his contemporaries and probably predecessors in terms of sequence of events and failed. I think it has something to do with the post war climate of the U.S., where people were flushed with victory and were looking towards a perpetually better life. The social make up of the society had undergone a very drastic and radical change, and, very obviously, social progress was needed in more ways then one. If I may just offer a philosophical comment for whatever it is worth, I don't think that there has been in the history of any nation, so far, and that includes the Soviet Union, a period of change, equally dramatic as in the life of America from 1945-1975. I think there was an unbelievable change.

LEON: You mentioned that he succeded where others failed. In a sense Babes didn't fail either. He succeeded.

KOSS: He didn't succeed on an international scale. He may have succeeded within his clinic. I don't even think that he - I mean by success at this point is the introduction of the method on a mass scale.

LEON: So, his results really weren't taken -

KOSS: His results were not even known when Papanicolaou published his results. And then there is a very interesting point as to whether Papanicolaou knew about about Babes' work because he did not quote Babes in his work. There is some debate about this and my very good friend Dr. Barney Naylor, who is Professor of Pathology at the University of Michigan in Ann Arbor, thinks that he knew it.

LEON: Babes' work was in a medical publication, but it didn't have any impact either, so really the first real impact did take place in this country in the early 40's.

KOSS: That is correct, and actually the first outside of Cornell. Now things become somewhat confused as to who did what first, but possibly as so many other things in human life, things began to develop in a parallel fashion. In New York City there was another innovation which began approximately 1942 or 1943 and that was the opening of two cancer prevention clinics which were called Strang Cancer Prevetion Clinic. They were opened by a women pathologist named Elise Strang L'Esperance, who was a colleague and friend of James Ewing and who had personal,

major interests in cancer of the uterine cervix. She opened, with I believe her sister, May Strang, two cancer prevention clinics which were called Strang Cancer Prevention clinics. One was at the Memorial Hospital in New York and the other was at the New York Infirmary, a Hospital downtown, still in existence, 17th and Second avenue, which is essentially a women's hospital, run by women, for women. Some day I will tell you the story of the Memorial Hosptial and how this ties in with cervix cancer, but I think it would lead us, at this moment, too far astray but it is a facinating story. Elise Strang L'Esperance, who I said was a pathologist and was the first director of the clinic, that she herself had opened, introduced the vaginal pool smear as a part of the cancer prevention examination.

LEON: From the early 40's?

KOSS: In 1943.

LEON: Really, then using one of the first. . .

KOSS: She was probably the first to introduce what became known later as the Pap smear, or variant thereof, into general medical use.

LEON: Now, Papanicolaou, I think, in some of his writings refers to the fortunate association with women's hospital.

KOSS: That's the Strang Clinic.

LEON: So he was at that time -

KOSS: Oh he was very much involved in that, oh yes. He was, in fact, since there was no laboratory of cytology anywhere except at Cornell, which Papanicolaou was running, the recipient of the material from both Strang clinics, both the uptown and downtown, And the material from these smears was to a large extent, and he gives credit for that, the basis for the material that appeared in his atlas which was his first really major cytologic publication. At the same time, perhaps somewhat later, interest in the Papanicolaou method had been also developing in Boston at the Free Hospital for Women, at that time. And I believe it was the late Dr. Joe Meigs, who was a world famous gynecologist, who together with a technician Dr. Ruth Graham, she is now a doctor, honorary of some sort, but she is really one of the first and very meritorious cytotechnologists, started taking some smears. Ruth Graham came into this field from some other field, became perhaps the first true cytotechnologist.

LEON: I think Meigs presented some of his evidence in a paper that I ran across, in 1943. How did he get interested in it?

KOSS: I couldn't tell you. I don't know.

LEON: You don't know if he was communicating with Papanicolaou.

KOSS: I am sure, everybody was communicating with Papanicolaou. He may have gone through Traut. Don't forget that before Papanicolaou and Traut published their book Traut, who was a gynecologist who later became Chairman of Gynecology in San Francisco, presented a number of papers at various scientific meetings so that I believe that Dr. Meigs may have gotten his ideas by listening to Traut at one of the meetings. But I am not sure. Ruth Graham remained in Boston for a good

while, married at sometime, I am not sure at what point in life, a gynecologist whose photograph I just found, John Graham, and he unfortunately committed suicide some maybe eight or nine years ago, I don't know exactly why. And Ruth Graham is still well and alive, living, working, actually operating a private cytology lab somewhere near Buffalo. And I understand that she may have another Laboratory in Boston. She would be an interesting person to see and I think she is deserving an enormous amount of credit. Just slightly later, just at the end of the war, in perhaps in 1946-47, another person appeared on the horizon, who also never really received the credit that he deserved, who recently died, and that was J. Ernest Ayre. Now Ayre was a Canadian gynecologist. He, I think, is deserving of the credit for introducing the idea that if the cells fall off the cervix and can be plucked out of the vaginal pool, then if you went directly to the target organ and scraped it, that you can perhaps get a better sampling. He introduced what was then subsequently known as Ayre's spatula or Ayre's scraper. Everything else, technically speaking, after Ayre was just variations on the theme. I don't think anyone has made a meaningful discovery, technical discovery, since that time.

LEON: The cotton swab -

KOSS: The cotton swab was really a variant on the same theme. The cotton swab may have been introduced at the Strang clinic, because when I joined Memorial Hospital in 1952 the cotton swab was being extensively used and presumably still being used today. So with these people you have the background, Dr. Papanicolaou, Graham, Ayre, and perhaps one more person who was a chap who became the director of the first formal cytology laboratory outside of Cornell. His name was Scapier. He was my predecessor at Memorial Hospital. He died unfortunately of a malignant melanoma.

LEON: How is that spelled, I have not run across that?

KOSS: It was spelled, Scapier, Joseph. He published a few things. He was a man of a rather uncertain personal background. He spoke French, but I was sure of one thing, that he was not a native Frenchman. I don't know where he came from. I have no idea and I never really inquired. He was married to a very lovely lady, and by the time that I met him, which was before 52, he already had metastatic malignant meanoma and he was dying. It was a miserable thing to watch him die. I think the stage has now been set for a tremendous expansion of the cervix cancer prevention program and the American Cancer Society which existed for many years, prior to that, of course, but under a different name. It used to be called the Cancer Prevention Society or something like that. They needed a cause absolutely desperately, you know. They were a Society without a cause which was very different from the March of Dimes, with whom they were actively competing and there was a society with a major cause, polio. They came across the Papanicolaou business and I am not quite sure at this point who, in the American Cancer Society hierarchy or board of scientific advisors or who ever it was, was instrumental or had the stroke of genius idea to say, the thing to do for the American Cancer Society is to propagate the cervix smear, or the vaginal smear as it was called in those days.

LEON: So they were saying that it was ready for widespread use. Were the early studies, those first clinics, set up with the idea that it works, it's demonstrated, we're going to apply it, or were they also research oriented and saying, while we apply it we're going to measure how effective it is?

KOSS: The studies of effectiveness of the Pap smear have not really been properly conducted until today. At the time it was amazing to the people that you could take a smear of what appeared to be a perfectly normal cervix and find out from the smear and subsequent confirmatory biopsy that the patient in fact had an early stage of cancer. That was the key. How effective Papanicolaou's smear was, the question has never really been properly asked.

LEON: And it wasn't asked at the time.

KOSS: No, the only measure of effectiveness that was publicized and properly perhaps, was not whether the smear could miss lesions, but that in anywhere from three to twenty patients per 1000 screened, you would pick up a variety of precancerous lesions. That was the measure of effectiveness. But let me just talk about the effectiveness later. I want to talk about it at some length because it is a very important point. But I don't want to break up -

LEON: Yes, but my question was, at that time, were they thinking of demonstrating effectiveness?

KOSS: Well, they were demonstrating the fact that you could find precancerous lesions of the uterine cervix in completely asymptomatic women whose cervix appeared if not completely normal, did not appear cancerous in the classical sense. Then of course the great debate started. What were we discovering, because the figures that people were receiving in those days, anywhere from three to twenty per 1000 prevalent cases, didn't jibe with the known figure of mortality or morbidity from invasive cancer of the cervix. They were much higher figures. And I don't think that for a long time, in fact until today in some quarters, people understood the difference between prevalence and incidence. People were looking at prevalence thinking it was incidence. Then, another debate started, on the following topic. Now, we do discover all these abnormalities of the epithelium, can we tell from looking at these abnormalities whether or not the patient is going to develop invasive cancer of the uterine cervix. A number of people claimed that they could on the strength of the histologic or cytologic appearance of the smear or of the biopsy. And they said, well if the lesion shows maturation on the surface, it should not be called carcinoma in situ, it should be called dysplasia. If the lesion does not show this and its made up of cancer cells or abnormal cells, it should be called carcinoma in situ. And the carcinoma in situ was the bad lesion and the dysplasia was the good lesion, and this notion still prevails in some quarters, until today. However, let me just share with you my personal point of view, which is now supported by an increasing body of scientific evidence, that prognostication of these precancerous lesions on the strength of the cytologic or histologic appearance is not possible and not warranted. I myself was instrumental in publishing a very long-term study which appeared in 1963, which clearly indicated that when you observe the lesions known as carcinoma in situ and lesions known as dysplasia, under identical conditions of observations, the results were about the same. And subsequently, within the recent years, additional evidence from several sources is coming to the fore to suggest, or to indicate, that in fact what we lack today is the means of predicting the future of any epithelial lesion in the cervix and, parenthetically, if I might say so, in the bladder as well. Although our good mutual friend is very enthusiastic about being able to unravel the mysteries of cancer, preferably within the next few months, I would, based on my personal experience, it has been a quarter of a century since I have been dabbling in this, I dare say that he will fail.

But we are still failing with the uterine cervix. Which brings me to the next stage of development. Perhaps its important to emphasize one more thing. For many many years after the American Cancer Society had been pushing the cervix

smear, the vaginal smear, the pathologists were extremely resistant to this idea and it's the gynecologists who forced the pathologists to perform cytology, with a very few exceptions. There were a few among us pathologists who, a quarter of a century ago, felt that there was something to it that was just not totally obvious. And Im delighted to be able to single out Dr. James Regan, a colleague and friend of many years, whose career in cytopathology began at about the same time as mine, perhaps a year or two before.

LEON: I think that the influence of the American Cancer Society is probably noted here. What I have got here is a little excerpt from a paper by Lombard in 1948. He comments that the use of the method, he is talking about vaginal smears has been limited owing in large part to difficulties in interpretation of smears and the time and training required for the confidence of diagnosis. He goes on to say that the pressure of public demand resulting from recent publicity of the vaginal smear technique has prematurely forced an answer to the question of its practicality both as a routine diagnostic method and screening test. So there was some influence, already,of the ACS's activities. Then I have one other question. In 1957, almost ten years later, a fellow by the name of King makes the same sort of statement.

KOSS: Now, which one of the Kings was it?

LEON: Unfortunately I don't have, these are just brief notes, I don't even have his full name.

KOSS: From the circumstances I could probably identify him.

LEON: He makes this point. Cytologic diagnosis of uterine cancer has recently gained the attention of many be lay magazines and newspaper editors resulting in public demand for something that the medical profession is not ready to provide. Then he refers to the 1948 ACS sponsored National Conference on Exfoliative Cytology in Boston. The question is, over this ten year period, they were both saying that there is pressure for something that we were not yet ready to deliver. That was ten years of pressure or more and yet no progress.

KOSS: Well, that is not completely correct that there was no progress. By 1948 I think there were perhaps only two labs or three labs of cytology in the country and perhaps in the world. One was Dr. Papanicolaou's lab, one was the Memorial Hospital, the Strang clinic lab, and the third one was the lab in Boston. By 1958, the number of labs of cytology, certainly in the United States, was many many times more then in 1948. I couldn't give you a figure for 1958, but I would say that based on the minutes of the various cytology meetings as they were being held, and it may be of interest to you to go to Dr. Warren Lang who is the Secretary of the American Society of Cytology to ask him for attendence figures from the initiation of the Society in 1951-52 until today, the meetings were being attended by an increasing number of people. There is no doubt about that and I would say that by 1958 there must have been at least three or four hundred people coming to these meetings. Today, of course, we have up to two thousand.

LEON: I think that if one looked at early information on what percentage of women had Pap smears, it was still quite low.

KOSS: That's true but we still, even today, are far from having reached the optimum numbers, let alone the population that most requires it. This is jumping the gun a little bit, but let me just pursue this. In any case, in 1958, Dr. King, whoever he might have been, reflected the feelings of many pathologists who felt totally overwhelmed by the demands. Cytology requires specialized training and experience, which these people, especially people in practice, didn't have the time to take. They also felt, because of the extreme tediousness of this work, there is enormous tedium in screening routine cytology, it was impossible for the practicing pathologist to do a decent job screening, and at that time, I think, we had only two schools of cytotechnology, one of which was under my aegis at the Memorial Hospital at that time, and another one under aegis of Drs. Papincolaou and Seabolt at Cornell. And I believe that James Regan may have had one in Cleveland, that is entirely possible. All the people who came on board, they are just a bunch of newcomers. However, there was, if I may be just completely candid about this, what I think swayed the pathologists in the direction of cytology, because some of them began to realize that enormous profits could be made by doing screening. Actually the stimulus came from, like so many things in American life, the fiscal incentive. And the fiscal incentive became very very substantial.

LEON: So some of the concerns that the smear was just an adjunct to a biopsy had been eliminated and now they were saying for mass screening lets use cytology.

KOSS: That is correct. I think I must single three major population surveys that I think are very important, for the history of it. One was in Memphis, Tennessee under the guidance of a man named Cyrus Erickson, Dr. Cy Erickson. He used only the vaginal smear, and he showed that with the first screening you get so many and so many cancers. The second screening the thing drops. The second survey which was important and perhaps is still the most important population survey in the world is the one that began at British Columbia under the direction of Dr. Boyes and Dr. Fidler. The third, although much the late comer, was in Louisville, Kentucky. That was Dr. Christopherson and his friends. All three surveys had one thing in common. They showed, that cytology is capable of discovering a very substantial number of unsuspected precancerous or early cancerous lesions of the cervix. None of them, until fairly recently, was able to show that this method of detection in fact reduced the mortality from the disease. And that is a very important point. Although the Vancouver survey, which is perhaps the most important survey, finally after about 20 years, showed that there was an impact on the mortality from cancer of the cervix and a substantial impact at that. They still have not reduced the death rate of cancer of the cerivx to zero. Far from it. They showed of course significant differences between the screened population and the unscreened population, but the zero level which was anticipated has not been reached today, after 25 years.

LEON: Were there any particular events that stimulated the Louisville study or the Memphis study?

KOSS: I think it was only a matter of money. Dr. Christopherson received very substantial funds from the cancer control program from Dr. Bill Ross.

LEON: So maybe the cancer control program was really an important stimulus for these studies. Without that -

KOSS: That was only true for the Louisville study. I think the Memphis study was supported, I believe, by the American Cancer Society. The Canadian study was

not supported by any American agency, that I know. Now, subsequently, minor projects have been sponsored here, but essentially the Government never had a key impact in the U.S. on the performance, interpretation and after-care of the Pap smear, because they never had really a meaningful long-range program with the target population that would be sufficiently well kept under control to really document the usefulness of that thing. Let me say why, in my judgment, the programs have failed to achieve the zero. There are several reasons for it. One is that the population that is most apt to develop cancer of the cervix, that's the economically underprivileged, promiscuous, poor genital hygiene women, are the least apt to get into a screening program. Even if they do get into a screening program, the follow-up becomes very difficult. They are essentially scared of any organized program that they think may have some impact on their personal lives. Secondly, and perhaps scientifically most important, the significance of the lesions known as dysplasia have been neglected for a very long time. Many patients with this diagnosis have been let go. They were coming back, five or ten years later with invasive cancer of the cervix. There is no doubt about it whatsoever. This is now coming to the fore, 25 to 30 years later. I think these two reasons are sufficient to explain the failure. And now let's go to the third reason which I think is something that you already brought up. How accurate is the Smear? Is a patient who has a single Pap smear, which is completely free of abnormal cells, is she free of cancer? I would say to you sir, today, that she has about a 40% chance that she has got something that is not detected in the smear. We still don't have a really meaningful survey which would combine cytology and colposcopy as two parallel independent procedures to prove or disprove the validity of the smear. I only know, what I know is anecdotal if you will. It has been my experience repeatedly during my professional life that patients with very important lesions of the cervix had completely negative smears. That anyone who believes that a smear is 100% is just a damn fool. How inaccrate the smear really is, we still don't know. I am using the top of my head figure of approximately 25%. Furthermore, if as has been done by Sedlis and published in the August 1974 issue of Acta Cytologica on two smears he could clearly show,

he pointed out there had been a significant number of lesions which were seen either on smear #1 or on smear #2 and that the overlap of the two smears was remarkably small.

LEON: Collecting an adequate sample?

KOSS: I am not even talking about that, because this isn't contrary to what many people say, what is adequate, what is an inadequate sample? People are not really always very honest about what they have, and I will say to you regardless how good a sample, there is a very high percentage of negative smears in the presence of important lesions, especially if these are second or third smears. In other words, what I really think is extremely important that if you have a lesion and you try to follow it by smears and you get negative results it is totally meaningless. Because, unless you follow this patient, you have to follow the patient for five years, seven or ten, because she will come back with something else. That's why colposcopy, which has finally been introduced into this country, is beginning to play such an important role. Because the only way you should do colposcopy is on all women who had some abnormalities in the smear, regardless of the degree of abnormality. And people are now screaming the same thing about colposcopy, that they were screaming about the smear, in 1958. They say we have no facilities and no know how. Sure we have the facilities and the know how.

LEON: Colposcopy could be used as a screening method.

KOSS: No, it should be used at this point as the second step of the cancer detection process. But, if we ever want to have a truly meaningful study of how accurate cytology really is, we have to take a very large number of women and subject them to competent cytologic sampling and colposcopy at the same time. This way we will find out how many lesions were missed by cytology that are picked up by colposcopy and, the other way around, how many lesions were picked up by colposcopy and missed by cytology. The number of such lesions will be substantial.

LEON: In terms of cytology there can be the scrape or the aspiration of the vaginal pool, or, thirdly, a self-adminstered sample. Could you comment on that.

KOSS: Again, you see, what people are trying to do is to say it's adequate or it's inadequate, what compared with what. You know, you are comparing two unknowns to each other when people say, all right the standard method is the cervical scrape, therefore, self administered smears should be at least as good as the cervical scrape, right? And it's been proven that it is not quite as good. The question is, now, how adequate is the cervical scrape. I would say to you today that the cervical scrape is not really as adequate as people claim. In fact, my current views are that a minimum of three smears should be obtained from each women at intervals of approximately six months before she can be dismissed as having no lesions. Maybe with the passage of time I will say she needs five smears.

LEON: Both smears should be done, a scrape or a swab and the other from the vaginal pool?

KOSS: It should be the best that people could do.

LEON: But both, the cell scraping as well as the vaginal fluid?

KOSS: This becomes a matter of personal preference and training. I personally think that the vaginal pool smear is a very good smear for a number of other reasons. You can pick up endometrial carcinoma, ovarian carcinoma, all kinds of things. This is my personal point of view. I have always liked the best possible sample.

LEON: A couple other questions that I have noted. This deals with now some recent statistics indicate roughly three fourths of all female women have gotten at least one Pap smear, so it seems to be going up pretty sharply in recent years. In fact, in the 20 to 40 year age group it is almost 90%. So maybe we can start seeing some results in the future.

KOSS: But we still need some meaningful surveys.

LEON: But the question that I am getting to is the obvious one that mortality from cervical cancer began to decline before, and maybe it's accelerating a bit more now, but -

KOSS: State statistics point out that mortality from cancer of the uterine cervix began to decline in 1945, which is way before there could have been any impact of mass screening.

LEON: There is some data that show it declining even before that, back in the 30's.

KOSS: Maybe, I just don't know about that. But I will say to you this. One of the possible reasons for this is the popularity of total hysterectomies which is a more difficult and much more effective operation. The prior operation was just to cut out the body of the uterus and leave the cervix inside. The more modern operation, as it evolved in the late 30's, perhaps, was the so called total hysterectomy which then removed the cervix at the same time. This might be a factor. The other factor may be a social factor. This is also the time when the standard of living of the great American populous began to climb. And as you know, I already mentioned the epidemiological factors which are unquestionably involved, and incidently, there is a marvelous paper, Italian paper, going back to 1841, by Ringoni Stern. Do you know this paper?

LEON: I don't believe I do.

KOSS: Ah, it's an absolutely marvelous account based on the vital statistics for the city of Verona in the 18th century in which this chap pointed out that widows and married women had infinitely more cancer of the cervix then nuns who, of course, were sexually, at least theoretically, inactive. Whereas cancer of the breast was just the opposite. The married women and widows had fewer cases of cancer of the breasts then the nuns. That is the first epidemiologic paper on cancer of the cervix, showing that sexual exposure, or whatever it is, was an important factor. It's a marvelous paper. Do you read Italian?

LEON: I think I remember a reference to it in a book: Cancer Mortality, 1915.

KOSS: Marvelous reference. In any event, it is also very possible that what happened in the 30's and the 40's is that we began to reduce to some extent the prime target population of cervix cancer, poverty and ignorance. I think is has something to do with the social movement. We were just emerging from the great depression.

I think the social change was written in the fabric of American Society between the late 30's and the 40's it's just absolutely unbelievable.

LEON: Moving to a bit more general topic, you mentioned earlier something to the effect, in terms of Dr. Papanicolaou, of what some of his colleagues thought about his work or had an influence on how enthsiastic or not he was about it. I guess that is, in a sense, a general deterrent to any kind of advancement. One is always concerned with sticking your neck out and advocating a particular thing that one is convinced of and I guess that general peer pressure, in some regards, will slow advancement.

KOSS: I would agree with you with the following reservation. The reservation has to do with the intellectual climate in which we operate. I think, if you just read the daily newspapers today, you are aware of the fact that people come up sometimes with the most incredible notions and scientific ideas, mainly in reference to cancer. These things get written up in the newspaper, although the following week or the following year they may be proven just completely false. You also know that we live today in an era where the pressure is on the other foot, if you

don't produce new ideas regardless of how outrageous or stupid they might be, your granting agency will look at you. If you just fiddle around with something that is promising, perhaps, but not sensational, you may be sunk. This has something to do with the availability of funding. Today there is an enormous competition for the research dollar. There are many more people making a living out of cancer then dying from the disease. That's one of my favorite sayings. People just have to come up with an idea. In the 40's the opposite was true. There was virtually no money. No pressure to produce and in fact the society, the scientific society, was so extraordinarily conservative that it took Rous forty years after he had documented that the virus may cause papaloma in the rabbit to be even spoken to or recognized let alone receive the Nobel Prize, which he got at the age of nearly 90. So I think it has something to do with the make up of the society. We have an extremely conservative society.

LEON: The climate may be better today for advancement.

KOSS: You know there are always good points and bad points in everything. You also build up the hopes of the society today with all types of sloganeering and statements. We say we can lick cancer tomorrow and then it will be in the next week or the following months and things like that. I think it's building up hope and, you know, you come to your Watergate, finally, in terms of public response. You say, what the hell, you make all these nonsensical claims that you don't deliver. The Congress which reflects the people and which appropriates the money, they are just saying, well, we have spent five billion dollars for cancer, where are the results, my grandmother just died of cancer, it should have not been. What was she supposed to die of, old age, senility, or shot by a jealous woman? Death is inevitable. People forget it.

LEON: How would you then summarize the lessons learned in cervical cytology, in terms of the points that we should keep in mind when we look to the future and the future directions of cancer control. Is there anything from the cervical cytology that we should remember?

KOSS: Yes, I think that there are a great many lessons that are coming from this. First of all that we always ought to look at new ideas. We ought to give a chance to the new investigators that come up with some notions, regardless with how ludicrous they might be, we really ought to give them a chance. I think that is lesson number one. Lesson number two is that we should not embark on a huge popular campaign before we have our facts well under control. I would say to you that in the case of cervix cancer, we still have more misunderstandings than actual facts under control and we have spread many misinformations about this. And in many instances, the comprehension of the public and of the physician of what's really adequate and what's inadequate, what is 100% and what is only 80%, is still not good. Thirdly, I think that when the Government establishes its priorities in terms of cancer, it has been my contention for many years that by meaningful, well conducted, and well supported programs of cancer detection, merely applying the existing knowledge to a number of fields of cancer where this knowledge can be applied, we would gain more from it than many of the basic research programs that are now in progress. I don't really think that cancer is due to a single mechanism or that it is a single disease. Each organ has its own cancers and, more importantly perhaps, each individual, each person, has his own cancer. All. . . other things being equal, the person, the host, may have a very different response to what seems to be an identical setting in the next person, for reasons that we quite ignore. I would say that this is a target area, as to why this happens. That is a target area of major interest and it will remain so for many

years to come, because there are so many things to look at. I am not sure that the rather unsophisticated immunologic things that are going on today are really going to give us an answer because there are many more parameters. Finally, I am rather distressed that people believe that because money is being poured into the field of cancer, that this is necessarily going to bring the answers closer or faster. The progression of progress of mankind has nearly always been made in a very unexpected way. You take some of the capital discoveries that are governing our lives today, they usually reduce themselves to Newton's Apple. In some way keep your eyes open and just observe the unexpected. No money can buy that.

LEON: Regarding what seems to be almost a thirty year lag between cervical cytology in the 40's when it was really was out on the table and just today when it seems to be having widespread use that 30 year period should not to be looked at as a unnecessary delay? That should be expected?

KOSS: Oh I should say, this was done faster and probably better then, mass application of any new discovery. Don't forget, this was the first mass cancer prevention program in the history of mankind. There has never been anything like it before. Regardless of how imperfect it might be, and I certainly told you all my reservations about some of the aspects of it, this is the first successful cancer prevention program, ever. All the other cancer prevention programs which derived, you know they have lung cancer prevention programs, maybe bladder in special situations, endometrium and breast, of course, and all that, they are just little pickaninnies, that derived from the big fat mother program which was the cervix cancer prevention program. It was a milestone in the history of mankind.

LEON: Then in your opinion, is it correct to say that cervical cancer cytology is the most important technological scientific advance in cancer control?

KOSS: So far, I don't know of anything better.

LEON: Consumerism, or the ACS? Comparing it to -

KOSS: I think it was by far the most significant thing. There may be other things in the future. And you might want to compare sometime in the next 20 or 30 years from today mass mammography, mass chest x-ray or mass sputim cytology. How did they fare and compare? It had a
marvelous target organ, it had a cancer that was curable, reachable, and the cure was not worse then the disease. In many other cancers, the cure might prove to be worse then the disease because, for instance, with the lung cancer detection program, what is happening today is that they are performing huge operations for miniature cancers. They are shooting big canons at little birds.

LEON: Well, but a little bird that will grow to a big bird and eventually -

KOSS: But it does not mean that these operations are really preventing other little birds from hatching. You see that this has already become evident, after you have removed one lobe or one lung, these unfortunate patients who respond to their cancer differently then their colleagues or friends are already developing secondary or tertiary carcinomas, in the opposite lung. So what are you going to do? This is well known in the renal pathology, people who develop cancer of one renal pelvis have a very high probability of developing it in the opposite thing. In the kidney you can remove both kidneys, put people on dialysis and ultimately give them a transplant. But the lung? We have a huge one million dollar a year

lung transplant program here in this building one floor above us. We have four human lung transplants that survived. One lived three days, one lived one week, one lived two weeks, and one lived sixty days. Not much we can do to make people breathe, unless we can develop an apparatus for oxygenation. So I think it's a very interesting thing. Now if we could have an equally successful - if you want me to make, just as a final point, what organs in my judgment could be amemable to an equally successful drive. Now, in the women, certainly the breast, we ought to look at it. It's being looked at very very carefully. Also, in the women, carcinoma of the endometrium, which is not being looked at as carefully as it could be or should be. Then, in men, there are certain organs where you can look for whatever you want and you won't be able to cure it. Carcinoma of the prostate is one example, where there isn't a damn thing that you can do about it. It cannot be cured very well. But you can live with it for many years. Certain organs in both sexes are eminently suited for cancer detection, that's the gastrointestinal tract, and in my judgment, and at least to a large extent, the urinary bladder. That's a very good one.

LEON: On the colonoscopes, the different types of flexible fiber optic scopes, can we be thinking about carrying out definitive studies on improving mortality or will we get to a point ten years from now where it's not ethical to study it, like it's not ethical, some claim, to study cervical cancer?

KOSS: You can no longer do that. You can no longer do meaningful follow up studies as it was possible twenty or five years ago. In those days you didn't have to tell the patient.

LEON: But, can we do it now -

KOSS: I think it is too late for any kind of follow up study - screening we can do.

LEON: But could we test, in a definitive type of study, the effectiveness of colonoscopy using the fiberoptic instrumentation.

KOSS: Yes, but it's not exactly a very pleasant thing you know. If you have ever had a sigmoidoscopy, you can really appreciate the discomfort that comes with it. It's not a very pleasant instrument to have shoved up your rectum.

LEON: Yes, so as a screen it has its limitations.

KOSS: By discomfort yes.

LEON: I think we have concluded unless you have any final comments.

KOSS: I thank you very much.

I N T E R V I E W

Interviewee: Herbert Lombard, M. D.

Interviewer: Lester & Devra Breslow

Location: Newton Center, Massachusetts

Date: June 18, 1976

HERBERT LOMBARD and OLIVE LOMBARD, his daughter. Conducted jointly by Dr. Lester Breslow and Devra Breslow at the home of Dr. Lombard in Newton Center, Massachusetts, June 18, 1976.

LESTER: We are doing a history of cancer control. That is why we wanted to talk with you. As you know, they are starting up again. This is the third or fourth time I guess in the last three or four decades that cancer control is in the National Cancer Institute. Because there is a whole new group of people there, for the most part new people around the country, it seemed like it might be helpful to talk to some of those who have been involved in cancer control back in the 30s, 40s, 50s to determine what their views were.

We've also been studying the various documents of the cancer control programs. The way we are approaching it is to also have conversations with people. Devra, my wife, is doing most of the interviews; she has interviewed some 60 people around the country in the Cancer Society, in the States, and the Federal government, research laboratories, cancer institutes, and the like.)

The first question we like to ask is what you regard as the three or four or five most important contributions to cancer control? What really pushed cancer control along, especially since 1946. We are concentrating on that period, but if there are things that occur to you before that time, you might mention them also. When we say, what were the contributions to cancer control, we include scientific and technological achievements, also organizational or other achievements. What things have come along that favored cancer control?

HERBERT: It is difficult for me because I don't remember ... but I can probably say the Pap smear is one of the big things that's happened. The reorganization of the Cancer Society is another. They reorganized about that time. And, as far as my State has gone, we have gone downhill since then. And that is one of the sad things about this. _____ My chronic disease program division is completely gone. Why they couldn't continue with our project, I don't know. They did for a few years after I left (1959).

DEVRA: This is the chronic diseases unit of the State Health Department?

HERBERT: Yes. That whole outfit is gone.

DEVRA: Why do you think it is gone?

HERBERT: I don't know. Ask Al Frechette. It's awful to work all your life to build the thing up, I began in 1926 on this cancer program _____, working it out, and I was hoping that after I got through that someone would carry it on and increase it, but they let it drop. So I wonder how good it was, if they felt that they had to let it go that way.

LESTER: Well, Herb, I don't want to break in here, but I must, to tell you that it may have stopped in Massachusetts or been slowed down, but the things you started--at least the statistical services--have now spread, well you know as well or better than I, to other States and across the country.

HERBERT: States have done it. But not here.

LESTER: So, they picked it up in other places, in California now we have cancer registries going for the whole population. In the whole Bay area,

HERBERT: Of course that's awfully good.

LESTER: But then next decade, you know, it will be some place else. These things have a habit of moving around. I wouldn't feel too bad. So, you've mentioned the Pap smear and the reorganization of the ACS. What other big events. Do you think that these statistical services are worthy?

HERBERT: Well, of course, the ACS is very important. I began moving from one thing to one thing, then I changed my mind entirely before we get through.

LESTER: What was your original idea?

HERBERT: I didn't believe it.

LESTER: Cigarette smoking?

HERBERT: I didn't think these programs were worth it. Then I found out about men and I still don't know about women, why they acted that way, because they get lung cancer without the cigarette. I am very confused about. I know it is a bad habit. I was doing it four packs a day.

LESTER: Four packs a day? When did you quit?

HERBERT: '42.

LESTER: In '42 you quit?

HERBERT: Yes, I was going blind because of it. I imagine in a year's time I wouldn't be able to see anything. But I quit because I needed my eyesight for my job, so I quit.

DEVRA: Just like that?

HERBERT: It was the hardest thing I ever did in my life. Four packs a day to nothing.

OLIVE: He just put the cigarette out, went into the office, and never picked one up again.

LESTER: Is that right?

HERBERT: I was nasty. I was awful. I smoked sweet fern. I chewed gum. I got a pipe and smoked tea. That was an unusually satisfactory thing. Then Dr. Warren told me I was getting too much tea in my system and I had to stop that.

LESTER: I remember the law, you mentioned '27 or '26, and I remember the law in Massachusetts that started the cancer program. Didn't that law say something about, "with or without the cooperation of the State Medical Society"?

DEVRA: No...of physicians, agencies, or others.

LESTER: Why did they put that in the law?

HERBERT: They wanted to be sure it went through. A bunch of men were very interested in the thing, and some of them didn't have much use for the Medical Society, so they put that through. I never liked that thing. I had nothing to do with the writing of that. But it went through.

LESTER: Was there some objection on the part of the medical societies to that law?

HERBERT: Not too much. There is always some, but not too much. They thought there might be.

OLIVE: From hearing about it, they didn't object because you kept them from objecting. Instead of going and giving them orders, "you've got to do this," he would go to the doctors and talk to them and get them to think it was their idea to do this for the clinics and things. Diplomatically, talk them around, until they didn't realize that he was the one that was doing it. So they accepted it because of that. But they objected to some of the other directors and some of the other divisions of the State department. It was simply Dad's way of handling the people, handling the doctors all over the State.

LESTER: How important do you think the State clinics and hospitals were in the Massachusetts program? Were they very important or not so important?

HERBERT: As an educational thing, I think they were good. I don't know how much they were otherwise.

LESTER: Educational for the public or for doctors?

HERBERT: Largely for doctors. Also for the public somewhat. But the doctors were on their toes a lot more because of the clinics.

LESTER: And Pondville.

HERBERT: And Pondville.

DEVRA: Did you used to go out to Pondville and collect records to do studies?

HERBERT: Oh, yes.

DEVRA: It was a good place to get records. I visited there yesterday. I saw the biggest record file room I've ever seen in my life. It is enormous. They have never put one piece of paper on film. They have the records all the way back to the first patient. They have about seven statistical clerks still working in that room and a statistician.

LESTER: Thinking of each of these contributions now separately for a minute, what do you think really favored them, brought them into being, and what held them back? Let's begin with the clinics and hospitals in Massachusetts. How did it happen that they came into being in Massachusetts? What favored it here, particularly?

HERBERT: There was a Catholic priest, Monsignor Ambrose Roche. He was a priest one time over in Watertown, and as I remember it, he got very involved with people with cancer. He got very much upset over it. And so he stood up to the Daughters of Isabella, that's an organization of the Catholic church, and they had an awful lot to do with getting the legislation started. And he had it planned right there in the legislature. Every time a question came up, they'd tell him he had a student in the night time type up the answers and next morning on every desk in the legislature with the answer to that problem. But, I didn't know it all until years afterward. He kept himself very much in the background, but he was working very hard on the thing. I think he was more responsible to the Massachusetts program than anyone else.

LESTER: Did you know him at all while this was going on?

HERBERT: Later.

LESTER: You never even met him till later.

HERBERT: Didn't know of him til later. Then when we were looking up on history of the program, we ran into him. I was in his house one day, and he was looking a little seedy. He died a week later. But before that, he had me drop down on my knees, and I did, and he said, I want you to carry on the Massachusetts program," and within a week he was dead. So that gave me an awful job.

LESTER: What year was that?

HERBERT: I don't remember.

DEVRA: It's in his biography.

LESTER: But there must have been some receptivity on the part of the legislators.

HERBERT: Oh, one of them was very strong. The others heard him say that, one out of every 6 of you are going to have cancer, and there is no place for you to go. That, of course, brought on Pondville. The legislature was all for us in those days. We could get money from them easily.

LESTER: What forces held it back if any? What resistance did you meet?

HERBERT: Not too much.

LESTER: Not even the Medical Association?

HERBERT: Well, I practiced 5 years before I went into public health, and I know how to handle them. At least I thought I did. By letting them think it is their idea. I go over their heads when I wanted something. Discrepancy _____ what do you think of this? And before they got through, they wanted it. They were behind me almost to a man. My biggest problem was the commissioners. I worked under six different ones: Three of them were with me and three of them were against me over the years. So I broke even.

LESTER: That's fair enough. Taking the Papanicolaou smear and its evolution and use over the years, what were the major factors that favored that and what held it back?

HERBERT: There weren't enough people to read them, they had to train people to do that. That was our problem there. The cost was another thing. You couldn't get it done. We didn't feel we could offer it as a free service without reason. We were always for it, but money didn't come.

DEVRA: I went to see your friend, Shields Warren, the other day. And he told me, actually, that at least on a demonstration basis you did have a program here of Pap smear screening, and you wrote a paper with him about that. You're the senior author, in fact. I guess you got the money from the Cancer Society to do this screening program in the beginning?

HERBERT: Most of the extra things we got from the Federal government and Harvard University. Harvard furnished us with a combination of things. We get quite a lot of money from the Federal government that way that we couldn't get straight (from the State). And they gave it to Harvard, so we could hire people. We got good people we couldn't hire on account of Civil Service, so I got much better people that way.

LESTER: You mentioned also the reorganization of the ACS. What were the major factors that contributed to that? Why did that come about?

HERBERT: Oh, it's a long story. The ACS ran long on a 2 by 2 basis for years and then they wanted more money. And Pete Little decided he was going to try what they call the Women's Field Army. And that raised a small amount of money, much more than they would have had otherwise, but it just didn't raise enough. So I think maybe Mary Lasker was as important as anybody. She got some of her husband's prime good people to get thinking about it and I was at a meeting there in which one --I don't know the man's name, but he was one of the Lasker group--and they thought of going into a big scale, so they could raise a lot of money. And I was dumbfounded with the results. But, of course, they do raise enormous amounts compared to what they did with the "little stuff" before then.

OLIVE: They weren't doing much in education either, were they? In the early days? The Cancer Society before they changed over?

HERBERT: Oh yes. They were trying to do a lot. On a small scale.

OLIVE: ...not widespread.

LESTER: If you had to pick four or five individual people who had contributed the most to cancer control over the past 30 years, who would you pick?

HERBERT: On your list, it said in this century. And I spent more time on that question than on all the rest put together. And I finally came to the conclusion that I would put Joseph Bloodgood, Jim Ewing, Bob Greenough Frederick Hoffman, and I'd add Mary Lasker?

DEVRA: That's a good list.

LESTER: That's a very interesting list. That's the best list we've got.
DEVRA: Absolutely. It's an interesting list because it has a real sense of history.

HERBERT: I spent a lot of time on that list. There are a great many other people that should be included, but I tried to get those who I thought something about. A lot of them, like my boss, Bigelow, ought to be included, but I don't think he was quite fitted into that list?

LESTER: What about people in the Public Health Service, Cancer Institute? Would you include any of them?

HERBERT: Well, I wanted to put in Parran and Scheele. Leonard Scheele was a very close friend of mine, and he had a lot to do with it but I didn't quite see it. I mean may be wrong, you know. That's my view.

LESTER: What would you say are the major contributions of each of those, say Greenough.

HERBERT: He was Head of the American College of Surgeons. He preached cancer control all the time, and he had a lot to do with the first clinics that came out. That's why.

LESTER: Was he responsible for the American College of Surgeons' sponsorship and standardization? He was the one, you would say?

HERBERT: I think so.

LESTER: What about Ewing. You mentioned him. What was his major contribution?

HERBERT: He was a darn good pathologist. I met him at the Mohonk Conference in 1926 in which he talks about the prevention of cancer and impressed me. Then he was running weekly conferences there at Memorial. Perhaps he didn't belong there, but at the same time. All of these connected with the early Cancer Society (American Society for Control of Cancer). They were working hard. Hoffman, of course, was a good statistician. He made the biggest collections of figures, a very Olympic man.

DEVRA: How about Dr. Bloodgood?

HERBERT: Oh, Bloodgood was a grand person. He was all over the lot. He'd come up here to Boston any time I'd ask him to to give a speech. He invited me down to his house one time. I'd stay three or four days running around with him when I was in the early stage. They were all in the early part of the century, when they were putting together...

OLIVE: Bloodgood was traveling all over the country, wasn't he, lecturing on cancer...

HERBERT: He and I went to St. Louis to lecture once.

LESTER: Looking back at the past, especially the whole complex of the State Health Departments, the Federal government, the Cancer Society, the medical

centers and so on, what do you think the major lessons should be drawn from that, for the people at the present time--from these organizational relationships?

HERBERT: Mustn't be jealous of each other. I think that was the trouble with a great many of these groups. I think it had little feeling. I know I was a little upset with the Cancer Society. I thought they were encroaching on me.

LESTER: When you were in the State Health Department?

HERBERT: Yes. I think there is a tendency that way. I had the feeling the federal government was a little jealous of some of the work being done in the State, but I may be wrong.

DEVRA: Some people have said that those good State programs really withered (less influential) when the Federal government got into the act around 1946 and they had a lot more money to give away?

HERBERT: Well I didn't notice that. I think the point is the people you've got working for you. If you've got really sincere people. George Bigelow used to talk about how Sodom and Gomorrah could be chained in one Christian person. But one good man would change the cancer program.

And then in the towns where we worked. We had one man up in Lowell. He got an incurable disease and had to give up his practice. But he kept up on the clinic as long as he lived. He was that devoted. It was men of that caliber. You can go to town. But if you don't have them, you can't.

One of our Commissioners said you've done all you can do in cancer, now you better turn on to some other disease. Huh, I got mad. Well, I didn't pay any attention to him. But he never liked me afterward, because I wouldn't do what he told me to. But he tried to stop me from working on cancer. A lot of the Commissioners want to make a showing in something new for themselves.

And of course Bigelow was the one that started the cancer program here in Massachusetts. And he gets the credit for it. Some of the later men wanted to go on and get credit something else. What I tried to do was play along with them, help them in their line, but not give up on cancer. I got away with it until I retired.

DEVRA: When did you actually retire?

HERBERT: When I was 70 years old.

LESTER: That's 16 years ago, 1960?

HERBERT: The last day of 1959.

LESTER: If you had to list the one or two most significant accomplishments of your own in cancer control, what would you say?

HERBERT: I'd say my studies, my statistical service. Of course I'm prejudiced. I like statistics, and I'm prejudiced. The gynecologists give me credit on

the relationship of early marriage and cancer of the cervix; of course I wasn't the first one to come out on lung cancer and cigarette smoking. The Graham-Wynder crowd did that. But I supported them.

LESTER: With respect to standards and quality of care that has been so important, what the American College of Surgeons did and what the States and you did in Pondville and clinics here, what are the most important lessons do you think from that period in regard to maintaining the quality of care now? Let me elaborate that a little bit. Now, there is some concern with advances --technological advances in diagnosis and treatment of cancer--that these are not somehow being spread into the community as rapidly as they could. Do you think that's true? What are the important things to maintain quality of care?

HERBERT: I don't know. I'm out of it at the present time. You would get more ideas from talking to some of the others, like Shields Warren, or some of the others.

LESTER: We are asking several people that question.

HERBERT: I wouldn't want to answer that.

LESTER: How about the development of personnel? What lessons would you draw from your experience with developing people who would work in cancer control? Are universities important, schools of public health, or medical schools, or agencies? What's important in developing personnel?

HERBERT: To find the right person, and that's difficult. I started with 45 at my peak, and I tell you there might have been 15 of those that I thought were really good, and the rest of them were of Civil Service were just mediocre, something like that. I don't like the Civil Service system in picking them, because you can't use Civil Service. But you get around it anyway. So I had a lot of dead wood. I could have saved the State money, had I picked just exactly what I wanted.

LESTER: Apart from Olive, who would you say were the most important people you worked with over the years that you developed and felt they had done a great deal?

HERBERT: Evelyn Potter Huyck, Eleanor Macdonald. Those are the two most outstanding ones. There are others that were really pretty good. Eleanor Kelly was good.

LESTER: Eleanor Macdonald is in Texas now? I lost track of Evelyn Huyck a long time ago.

HERBERT: She's retired and living in Florida, Sarasota.

OLIVE: I have her residence if you'd like to write to her?

HERBERT: I had one girl who wrote me a letter after she got through. And she said, "I never liked you." She said, "I hated you, but you did me an awful lot of good." She got a job down in New Haven afterward. That was praise in a back-handed way.

LESTER: That's the best kind of praise.

DEVRA: Olive, are you still working?

OLIVE: Yes and no. I've written a book on statistics. I've had to give up most work. I do work with Shields Warren and Olive Gates as a consultant on statistics. Most any paper that they write, they ask me to look at and help with the figuring. But, I had to give up most of my work, when my mother became ill, to run the house and take care of Dad.

HERBERT: I can't be around by myself. She has to do so much. I was pleased when they asked her to write this book on statistics.

DEVRA: That's a very interesting parallel. Because I heard, I guess it was Dr. Warren said to me, or maybe it was somebody else, "We never published a paper until you went over the statistics. It was a matter of policy, because we knew were making all kinds of mistakes. And now Olive has the same reputation and regard. That's really quite wonderful.

HERBERT: Of course, Bigelow made a rule that any paper coming out of the Department I would go over. He made a lot of enemies by doing it because...

OLIVE: That included all the men at Pondville and all the doctors. All of them had to because they were tied up at the State hospital, Dad had to okay them regardless.

HERBERT: I turned down a paper written by _____ Grantley Taylor. He was mad as he could be about it. I turned him down because he made a statement on a too small sample. I said, "You can state your opinion such and such and so, but you can't say that it's absolute proof." Before he got through, he was coming round to me himself, in writing some of his other papers.

LESTER: You've mentioned several people that were influential in the development of the program, Bigelow, the Catholic priest, some of the legislatures, and others. With respect to Pondville, the State hospital, the State clinics, who were the most important people? Same ones or a different bunch?

HERBERT: Same ones.

LESTER: They're responsible for Pondville and the clinics?

HERBERT: Yes. Of course, it was put into the department and then the department broke it down. The law said it shouldn't be a hospital and a clinic?

DEVRA: Were you very friendly with Dr. Daland?

HERBERT: Oh yes, I know him very well. He was a grand guy. We grew old together because he was at Pondville from the very beginning.

DEVRA: Do you ever see him now?

HERBERT: No, he moved down on the Cape. I used to see him. We both were Directors of the American Cancer Society. We used to meet quite often. I'm still an Honorary Director, but I don't go.

LESTER: We thought we might stop by if we had time to see him. I don't remember him. I never met him.

HERBERT: Daland...He's one of the nicest men you'll ever meet.

DEVRA: How did you and Shields Warren and Daland all sort of get together?

HERBERT: Warren and I met at the Lake Mohonk Conference in 1926. We found ourselves seated at the same table the first night we were there. I never had heard of Shields Warren, and he never heard of me, I guess. We got acquainted and we used to walk around the Lake Mohonk between talks. We got acquainted and have been very close friends ever since. As close as I've gotten to anybody, I suppose. Daland's awful close, too.

That's the beauty of this kind of a job. You meet so many nice people.

LESTER: They really are wonderful people, aren't they?

HERBERT: Yes, I think so. And people down at Washington. I liked Len Scheele. He was a wonderful guy. Parran and I first met down in the First World War; he was Second Lt. or something down at Sheffield, Alabama, Muscle Shoals. He was down there with the Public Health Service. I was there with the Army. And that's we ran into Parran, at first.

LESTER: Was that before he became Commissioner in New York State?

HERBERT: Way back. He was nobody there. He was just one like the rest of us. But he always remembered me, up until he passed on. I've met some great people.

My father couldn't understand why I didn't want to continue doing general practice the way he did. That was one of the sad things that bothered me. He wanted me to continue. I just took it for about 4 years. But then I got fed up. Perhaps I'm a slob. But I did like to meet all the wonderful people that I've run up against over the years.

LESTER: You talk about not liking general practice. What induced you to leave that and go into public health work?

HERBERT: Just the desire to do it. I got a job. My father was in politics, as well as medicine. I got a job up in Maine as the District Health Officer. So for 4 years I tried that. Then I thought, I didn't know enough. I'd better go to school. So I took a scholarship from the Rockefellers to go to Harvard for a year. And then I had to go back to Maine for one more year. In the meantime I got a cancer on my shoulder. So I got interested. Doctor told me I'd only live a year. I probably wouldn't. That was a cheerful thought.

LESTER: What kind of cancer was that?

HERBERT: Sarcoma. So they operated on me twice. The first time it came right back. The second time they cut wide and it didn't come back. They put radium on. But for years, I was afraid it was going to come back.

But, that's why I got interested in cancer. So they offered me this job. A lot of people didn't want it, didn't think it was Public Health, didn't want to go into it. But I was perfectly willing to for that reason.

LESTER: Who offered you that job, Bigelow?

HERBERT: Kelly. The first one was Kelly. Bigelow on the second. I did a study on the cancer situation under Kelly. Then after the bill was passed, I was under Bigelow.

LESTER: So you began before the bill was passed with Kelly at State Health?

HERBERT: I studied the situation for the State. I travelled all over the State, asking doctors and Boards of Health and then I and made a report, which came out as a legislative document related to Pondville's establishment.

LESTER: Who were some of the other people around the country in those days in State health work who were interested in cancer. Say before 1950. Who were the 20s, 30s and 40s, people who were most important around the country in State.

HERBERT: Mort Levin, of course, in New York. I can't remember all their names. We had a great many people who'd come to see us the same as you did in those early years. Funniest one was a Chinaman. This man came in and he bowed low. He said, "Is this Dr. Lombard?" and I said yes. He said "When I arrived in this country I wanted to know where I could go to see about cancer. They said there is this fellow in Massachusetts named Lombard", they told him that over in San Francisco. He said, "when I got to Chicago, I asked them where I was to go, they said "a man in Massachusetts named Dr. Lombard." Then he went to Washington. "They told me the same thing." Then he said "here I am." He bowed way down. That is probably the greatest compliment I ever received.

LESTER: What year was that?

HERBERT: I don't remember. My memory is pretty...

LESTER: No, your're remarkable.

DEVRA: You know something. He can't remember things, and he's a little younger.

OLIVE: Dad remembers so much. I'll say who was that, and he'll know immediately.

DEVRA: Do you remember Ray Kaiser?

HERBERT: Yes, I do very much. He was in Washington for a while and then he went out West afterward.

DEVRA: Now, when he was the Chief of the Cancer Control Branch after Dr. Deibert, they started giving monies to the State Health Departments. Do you remember what you did with the money? Probably wasn't a lot. Maybe $15-20,000 a year?

HERBERT: I don't remember which. I've had so many different grants. We started in. We gave money to the clinics. Then, we cycled it through Harvard, so I could hire certain people that were good. Then along came in '55, I think, they had us down in Washington and, all I can remember was they were giving money for this new registry they were starting. Shimkin was there.

By the way, we got a letter from Shimkin the other day, Olive did. He wanted my picture.

DEVRA: He's writing a book. He's written one book that's already published, that's called the History of Cancer Research through the Ages and now he is writing another book called, "The History of the National Cancer Institute." But I bet I know why he wants your picture. He is the picture editor of Cancer Research, the journal.

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March 9, 1977

Dr. Lester Breslow,
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Dear Les:

Thank you for the chance to add a few lines to the transcript on the History of Cancer Control project. The transcript left out some things that I thought were important. It also lacked what we thought was a unity in our struggle in cancer control. I'll be brief.

INTRODUCTION:

Our job in the Cancer Control Program was to help the practitioner of medicine. In the local community he tries to provide what is called "good practice" for that community, and for that time in history. In cancer control we were trying to stay one step ahead of him. What should he know today that will be good practice tomorrow?

A. USPHS ROLES:

Organizations are like individuals. They develop patterns of behavior that do not change very much. We tried to set up the Cancer Control Program (it was set up from scratch) as being responsive to the needs of the practitioner. This is reflected in the programs which were set up for the four top cancer sites: lung, cervix, colo-rectal, and breast.

A1: CANCER CONTROL AS AN ENTITY:

This report concerns my own personal experience with cancer control. My training in cancer control was six months with the Roswell Park Memorial Institute, and with Herbert Lombard in Massachusetts. This was followed by a year in the Chicago Regional Office. Ten years later (after an international assignment and a heart demonstration program) John Heller gave me training at the National Cancer Institute in what was then known about cancer,totaling nine months. Cancer Control means both prevention and treatment of cancer.

A2: THE CANCER INSTITUTE: It's Legislation and it's control program in 1946.

Dr. Dudley Jackson can be credited with the concept that led directly to the National Cancer Institute. In 1920 he began writing to medical and government leadership proposing a research organization within the U. S. Government. His nephew, Maury Maverick the Congressman who coined the term "gobbledegook", wrote the legislation with the help of the old Hygienic Laboratories, in 1937. After World War II, Dr. Leonard Sheele, then Director of the National Cancer Institute, set up in the Institute the first cancer control program. It was headed by Dr. Raymond Kaiser, who had to follow the guidelines of a research program, which is hardly conducive to exploiting research breakthroughs in practice.

A3: REGIONAL CANCER CONFERENCES:

How does one start a national cancer program? This one began with a series of regional cancer conferences, to which practitioners and state health officers were invited. These conferences were greatly successful, for they gave us a knowledge of the leadership in each region and an idea of what programs were ready to be implemented. We soon learned not to discuss roles, but to ask what problems had been found in cancer control. Permissive discussions with about 15 in each group, generally with two groups, provided considerable insight in the two days of the conference.

A4: CHRONIC DISEASE DIVISION.

The harsh reality of research is that one must "publish or perish". And the publication must be in terms of protocol which spells out how new knowledge of diseases like cancer can be learned. The researcher does not need to spend long hours in meetings to find better ways to get physicians to accept new practices. And yet to the USPHS there is a responsibility to anticipate the needs of the practitioner and help him to make those changes which provide his patients with better chances of recovery. It was for this reason that cancer control was taken out of the Cancer Institute and placed in the Chronic Disease Division. To the Chronic Disease Division and the newly established Cancer Control Program, the investigator and the medical educator existed for just one reason, to help make the practitioner a more effective provider of medical service. Research and teaching are exercises in futility if they do not result in these changes which provide the patients with a better care.

A5: ADVISORY COMMITTEE TO THE CANCER CONTROL PROGRAM:

The Cancer Control Program needed an advisory committee. This advisory committee would give the new Cancer Control Program a working relationship with the practice of cancer control. The regional cancer conferences showed us who should be on the advisory committee, and so about 15 physicians with insight into practice were selected to help with the business of cancer control. They reviewed the project grants, and made recommendations concerning policies. The Chief of Cancer Control was the Chairman of the Committee, and a member of the committee was selected to be the co-chairman and project review chairman. The writer was fortunate in obtaining Dr. David Wood to serve as Chairman of the Review Committee. He had selected him from the vantage of the regional conferences, and Dr. Wood appeared to be the best man possible. This proved to be the case in the work of the Cancer Control Program, and much of the success of the cancer control program can be laid to the efforts of Dr. Wood.

A6: CANCER CONTROL PROGRAM AND ITS STAFF.

The Cancer Control Program had from 5 million dollars a year in 1958 to 15 million in 1965, as operating budget. Staff members who had demonstrated ability in other programs, were selected to help with this development. Staff meetings were not held, but task forces were set up in the staff, and these constituted the business of the staff. Communication was achieved by distributing to staff a daily log of the Director. This was given as Xerox copies to Dr. Breslow, approximately 2000 pages of daily log. The Program was set up to control the four major cancer sites, cancer of the lung, cervix, breast and colon-rectum. (See below) A dentist was added to explore the possibility of mounting detection programs by dentists of the oral cavity. This was expanded to a program directed to cancer of the head and neck.

As the work of Cancer Control developed, certain members of the Advisory Committee became key figures in the growth of the program. Besides Dr. Wood, Murray Copeland gave program opportunities to the Program from his rich insights into the working of the various disciplines in Cancer Control. Warren Cole, John Cline, Lee Clark and Thomas Carlyle made important contributions. Amos Johnson as past president of the American Academy of General Practice, and leader in the Office Detected Cervical Cancer Project, was of particular significance to development of the Program.

B. NON-PHS ROLES IN THE CANCER CONTROL PROGRAM'S WORK.

The Cancer Control Program drew on many organizations to further the practice of cancer control. Chief among these was the American Cancer Society, but many other organizations contributed to cancer control.

B1: AMERICAN CANCER SOCIETY.

Government is by nature conservative in its approach to change. Not so the American Cancer Society. They were able to quickly mount a prospective study against lung cancer, and when this was confirmatory, to launch a program against the cigarette. Cancer Control Program worked closely with the ACS so that synergisms between the two agencies could be discovered.

B2: AMERICAN COLLEGE OF SURGEONS

The Cancer Committee of the American College of Surgeons has served to develop a liaison between professions and agencies concerned with cancer. The Chief of the Cancer Control Program was invited to sit in on these meetings, and many opportunities were made available to perform multi-agency programs because of these relationships. There was an undercurrent of competition in these groups, as in all groups that provide life saving measures. The Surgeons tended to dominate, or attempt to dominate all the work of cancer control. In the early 1930s the American College of Surgeons fostered the development of the Cancer Diagnostic Clinics, which for many years were administered for the College by Bowman Crowell, so they had earned the right to compete for leadership of Cancer Control.

B3: COLLEGE OF AMERICAN PHATHOLOGISTS.

The surgeon or radiologist points to suspected cancers, but it is the pathologist who tells what they are pointing at. Pathologists were at the cross-roads of medicine and wanted to have decision making, or at least a veto of what was done by the U. S. Government in Cancer Control. The fact that our co-chairman of the advisory committee was a pathologist did much to ally fears. Performance of Pap smears by technicians rather than pathologists was extended far beyond what it had been.

B4: COLLEGE OF RADIOLOGISTS.

The College is composed of radiologists who restrict their practice to diagnostic radiology, and also those who restrict their practice to radiation therapy. In this they are schizoid,

because the two functions are so different. The development of mammography during my tenure made the diagnostic radiologist a close ally.

B5: AMERICAN ACADEMY OF GENERAL PRACTICE.

It is often said that the life of the cancer patient is in the hands of the first doctor that he sees. This "first doctor" is usually a primary doctor. Our first two regional conferences were held without participation of the American Academy of General Practice. After this there were always several participants. Our health hazard appraisal program was built on the interest of family doctors and especially of the physician assigned to Cancer Control Program by the Academy, their Chairman of the Commission on Education, Dr. John Paul Lindsay. Five years of demonstrations of Health Hazard Appraisal prior to 1965 probably influenced the concept of comprehensive and continuing care by a generalist physician, as proposed by the Millis, Willard, Ravdin and Witten committees.

B6: COMMISSION ON CANCER STAGING AND END RESULTS.

Hippocrates admonition to "prognose" may have founded medicine, but two steps were necessary before a prognosis could be made: the physician must make a diagnosis and he must stage, or give the degree of severity of the disease. Then a prognosis is possible. A joint commission between top professions and agencies, that develops criteria for diagnosis, staging, prognosis and treatment, operates to make the practice of cancer control more precise. Their proposals for staging are usually disregarded by practitioners, as being too complex, but they do influence future practice. The Cancer Control Program worked with the Commission and provided funds and moral support.

B7: INTERNATIONAL UNION AGAINST CANCER.

The UICC asked me to be a member of their prevention and detection committee. In this capacity I presented a paper on the mammography reproducibility study in 1962 at Moscow, and in 1966 chaired a panel discussion on cancer detection evaluation in Tokyo.

B8: PUBLIC HEALTH CANCER ASSOCIATION.

The PHCA was an organization composed of cancer people in voluntary and official health agencies, generally non-practitioner or clinical members. This agency could move more easily into promising areas with recommendations. For instance, this agency was the first to take a stand against the cigarette. The writer served as secretary of this organization for five years, and President for one.

C. TECHNICAL APPROACH TO CANCER, GENERAL

The control of cancer follows the orthodix approach by medicine to diagnose, stage, prognose and treat established disease. But there is much to be done before the patient has any idea that something is wrong. The genesis of cancer does not take place overnight, but follows long established insults to tissue. This calls for a more precise means of defining the problem when prevention and early detection is desired. Here are the considerations:

C1: THE INDIVIDUAL PATIENT AND THE ONTOGENETIC MARCH OF TIME.

Medicine treats patients as "cases" and the case begins with the chief or legitimate complaint. This ignores the fact that each individual is totally unique and his cancer begins many years before. The term "ontogenetic" was chosen to dramatize the fact that the individual has tendencies to repeat the problems of mankind.

"Ontogeny recapitulates phylogeny" is said of the way the individual repeats the evolutionary development of man in embryo. But the individual tends to repeat catastrophies in the maturation of mankind after birth, in terms of probabilities of death. The forty five year old man has a high risk of death from lung cancer when he smokes. The individual is an endangered individual because he will eventually die. A recognition of this danger will provide him with risk reduction procedures as he matures. How may this be applied to cancer control?

C2: IDENTIFICATION OF MEANS TO GAIN SURVIVAL ADVANTAGE.

When cancer is present medicine makes the diagnosis so that appropriate treatment may be made. The stage is determined so that a prognosis may be made. Prognosis is determined so that a better alternative in reduced risk of death can be selected. The treatment given provides the individual with a reduction in risk of death. This can be called survival advantage. This use of probability recognizes that medicine is a mystery, even though a perceived mystery, and each physician must accept this inability to provide an absolute cure. He cannot tell the patient that he can be cured. He tells him that he can be given a better chance of survival, a survival advantage. Early in the program, in 1959, discussions were initiated to give this survival advantage to well people, through the diagnosis of those prognostic characteristics that placed people at higher risk of death from cancer. For lung cancer there was cigarette smoking. For cancer of the cervix there was dysplasia. For colon-rectum cancer there was history of polyp of the colon or rectum. And for breast cancer

there was an image on x-ray that in a high number of cases was associated with early cancer--a stippling that appeared like detritus. These high risk characteristics were called risk factors, or better, precursors. These precursors could be diagnosed, and staged, (degree of severity) and prognosed (prospective studies) and given survival advantage by an intervention.

C3. PRUDENCE VERSUS RESEARCH: THE INVESTIGATOR'S GOAL CAN INTERFERE. There are many times in the practice of medicine, when the clinician exercises judgment as to what is best for his patient, that the investigator might say with honesty and sincerity, "That method of treatment has not been proven." Clinicians, and especially the consultants in the several specialties, can say just as honestly, "I'll be the judge of what is best for my patient." The practitioner's training has been as good as the investigator, and he cannot always wait for the proof that the investigator would deem optimum for practice. This dilemma is appearing in preventive medicine. How much proof must we have before advising our patients not to smoke, to not drink so much, to choose to be active and not sedentary, to be slim rather than obese? When is mammography a better alternative for the patient than no mammogram? Our cancer control program was forced to make these decisions as an aid to the practitioner. The investigator can be so sensitized to "valid data" that he will ignore prudence, but not so the practitioner. The investigator's allergy is expressed in the old tired phrase, "First of all, do no harm." If this were followed, no drug could be given for they all can produce side effects, and no surgery would be performed, for there are hazards whenever the body is anesthetized, or placed in surgical shock. In the development of information about a phenomenon, there comes a time when prevention possibilities are interesting, then as more is available, it may be prudent to practice, and finally after adequate confirmatory information the method may be proven. The Cancer Control Program judged several of the breakthroughs given below as being in the category of prudent.

C4. MOTIVATION IN CANCER CONTROL

Motivation in cancer control was pursued vigorously with many behavioral scientists. The Florida ADC project was an attempt to see how many ADC recipients could be brought in for Pap smears. This resulted in a small group discussion approach that more than doubled the women who asked for Pap smears. Shop Talk was a physician education approach in which films and sound were used

(color slides) to "pump prime" family doctors to talk about their cancer problems.

C5. HEALTH HAZARD APPRAISAL

In 1959 the Cancer Control Program asked Dr. John Hanlon to set up a program to test the possibility of estimating for the individual a total personal risk. This would be initiated on the average risk as a base, and adjusted by a credit-debit system for the prognostic characteristics for the several causes of death. Today this credit-debit system is called the Geller-Gesner Tables. With this system one may diagnose precursors, and then follow with a staging (degree of severity) and a prognosis (chances in the next ten years) and a treatment (intervention that gives a better prognosis.)

C6. NATURAL HISTORY OF DISEASE, BY PRECURSOR.

It is a natural part of our "reality principle" to want to know where we are. In health Satchel Paige may have said it best with his, "Don't look behind you, somethin' may be gainin' on you!" The cancer control program developed a chart that shows the progression in the natural history of disease with a sequence that went from, "No risk, vulnerable, precursor present, signs, symptoms, disability" and in some cases on to death. As an approach to the health hazard appraisal, it is well to know where one is, to have a fix in the natural history. This calls for both specialists in precursors and generalists in putting all of the risks of the patient in a package that provides total personal risk.

C7. PROBABILITY TABLES FOR DEATHS BY AGE, SEX, RACE.

Harvey Geller put together tables that to the extent one was average, gave chances of death in the next ten years. These deaths were given in deaths per hundred thousand. It has served as the base for the HHA program that developed.

C8. SURVIVAL ADVANTAGE, EXCESS DEATHS, BENEFIT TO RISK RATIO.

A part of the expression of the health problem of the individual is his total personal risk. Another part is his chance of death from one of the top causes. Still another problem is the extent this is raised by possession of a precursor. Still another problem is the degree that this risk can be reduced. To the practitioner, only that part of the risk that can be reduced is his problem. This reducible portion can be called survival advantage, excess deaths. It can also be expressed as a benefit to risk ratio.

C9. KEY ROLES AND DECISIONS.

Who has the responsible roles in the practice of cancer control? The Cancer Control Program attempted to demonstrate a continuing responsibility in decisions relating to the control of cancer. When ad hoc committees were formed, the Cancer Control Program reviewed them before introducing them as program. Ad hoc groups have their place, but they are never present when evaluation sessions are held, for they have moved off in many directions as individuals. Ad hocery was discussed and its disadvantages pointed out, as well as the advantages.

D. CANCER OF THE LUNG

Early in the 1960s cancer of the lung passed colon-rectum as the greatest cause of cancer deaths of any site. It was then wiping out all of our gains in reduction of the death rate from cancer.

D1. SURGEON GENERAL'S REPORT IN JAMA ON CIGARETTES AND LUNG CANCER.

The Surgeon General, Lee Burney, assigns the task of preparing a report on cigarettes and lung cancer to the Cancer Control Program. Forty-two drafts later, and 18 months later, the paper is published. This November 1959 report caused the stock market to fall--temporarily.

D2. PHILADELPHIA STUDY OF EARLY DIAGNOSIS.

Dr. Katherine Bukow and a group of Philadelphians conducted a study to see if early diagnosis using routine 6 months chest x-rays would make any difference in the death rate from cancer of the lung. The results after screening 5,000 men were that the 5 year survival moved from 5% to 7%. Early treatment was hardly the way to gain survival advantage.

D3. BEHAVIORAL STUDIES IN CIGARETTE SMOKING.

The Cancer Control Program was fortunate in obtaining Dr. Dan Horn who with Hammond, had conducted the prospective study of cigarettes and lung cancer for the American Cancer Society. Many behavioral studies were conducted of habits of children and adults.

D4. DR. DAN HORN AND THE DEVELOPMENT OF THE CLEARING HOUSE.

In 1965 the cigarette smoking and lung cancer program was moved out of the Cancer Control Program, and a new branch, The Clearing House on Smoking and Health was established.

D5: SAN DIEGO SATURATION STUDY

One community was selected for a study of what could be done if every agency and every medium were exploited to achieve reduction in cigarette smoking.

D6: INTER-AGENCY COUNCIL ON SMOKING AND HEALTH

At one of the meetings of the American Cancer Society the writer was asked if the PHS could help to set up an inter-agency council on smoking and health. The council was established with all the key agencies with the exceptionof the American Medical Association which at the time was fearful of political effects of a position on cigarettes with certain health legislation coming up. The group chose your writer to be the organizing chairman.

E. CANCER OF THE BREAST.

For a time in the life cycle of the female, cancer of the breast is the greatest cause of death: our Geller Tables place this at age 30 to 45, for the 1974 data. As every cancer site is different, cancer of the breast is different, but in a multitude of cell types and grades.

E1: IAN MACDONALD AND GERSHON-COHEN

During the fifties, two men profoundly influenced the diagnosis and treatment of cancer. Ian MacDonald of Los Angeles claimed that most of cancer of the breast was so stubborn that treatment would not affect it, that there was a biological predeterminism in the growth. This affected attitudes toward the practice of breast cancer control. About the same time, Gershon-Cohen was working with mammography and was able to demonstrate to Isadore Ravdin, later on the Cancer Council of NCI, that he could find cancers with mammography that were only 4 or 5 millimeters in size. He kept alive the hope that something might be done about cancer of the breast.

E2: ROBERT EGAN AND THE M.D. ANDERSON STUDY

When Robert Egan began to demonstrate a reproducible method of breast cancer detection with mammography, those of us who had been sensitized by Gershon-Cohen immediately began to look for signs of viability and validity in the practice of mammography. A reproducibility study was conducted involving 25 institutions, by the Cancer Control Program and M. D. Anderson Hospital.

Radiologists previously unfamiliar with mammography were asked to perform mammograms preoperatively on consecutive cases coming to biopsy. These radiologists were able to detect about 80% of the cancers that were present on biopsy. This was after a week's training in mammography. It was so promising that we developed a training course with Egan, operated by Bill Melton, and trained at my last count in 1965 about 1300 radiologists to do mammograms. In the American Cancer Society we pooled our talents to determine what to do about screening, and decided to wait for more results. We know, though, that screening would be coming some day. At the time women under age 45 were looked on as not being worth screening because of the difficulty of getting an adequate image on the X-ray. The glandular tissue was too dense.

E2: MAMMOGRAPHY REPRODUCIBILITY STUDY.

An interesting phenomenon developed among the twenty five radiologists in the study. At our first annual meeting in 1961 the radiologists were fearful and pessimistic. Missing one breast cancer, after Egan's results was disconcerting. They stuck with it but with much "cognitive dissonance". A year later when they met they were an exhuberant, enthusiastic group. They had all had experiences of confounding the clinicians with their discoveries of cancer, small, often in the other breast to what was palpated. They were now sure of themselves in that "perceived mystery" which all medicine must face, dealing with probability and not absolutes.

E3. THE TEACHING SET AND THE TRAINING OF RADIOLOGISTS.

The work of Bill Melton was most gratifying. He was able to work long and productive hours in setting up training programs, and getting just the right kind of copies of the film. With Egan he put together many training sets for radiologists that changed the policy of the American College of Radiology from "no training of radiologists" to many sponsored training programs.

E4. EMORY AND THE CENTER FOR MAMMOGRAPHY.

Robert Egan moved out of M. D. Anderson to take advantage of a doubling of his salary, at the Methodist Hospital in Indianapolis. His move did not affect the reproducibility study which was continued at the M. D. Anderson. He soon tired however of the "service role" and Emory University took advantage of this desire by Egan to set up a research program at Atlanta. Here was a case where the control program sponsored research, in exchange for assistance in the development of training for radiologists in the management of breast disease. Screening was still a function set up by NCI and Michael Shimkin at the HIP.

E5. THE FOURTEEN ANNUAL MEETINGS ON BREAST CANCER

The shared excitement by radiologists in the performance of mammography was the motivation to continue the annual meetings. These gradually became "breast cancer" meetings, and were sponsored by both ACS and the Cancer Control Program. About three years ago I participated in the fourteenth annual meeting, and told the history of development.

F. CANCER OF THE CERVIX.

Cervical cancer in unique in its accessibility. Colposcopy takes advantage of this accessibility to examine the squamo-columnar junction for changes. The Pap smear makes control appear simple, but the contrary is true, for in early detection there are fourteen steps that the clinician must take to find and treat cancer of the cervix with the Pap smear, and there is a loss of cases at each step, so that only repeated smears will reduce the loss at the several steps. The greatest controversy over cancer of the cervix developed while the pathologists were hold outs, saying that cancer could not be diagnosed by examination of a single cell. The gynecologists who had tooled up to read Pap smears themselves noted that dysplasia was a very shaky state that could become malignant at any time. Serial sections often showed microinvasion. Severe dysplasia began to be called "carcinoma in situ". Your writer heard one notable argument at Roswell Park Memorial Institute between the pathologist and the gynecologist, in 1946. The gynecologist urged that severe dysplasia be called, even in the absence of any microinvasion, "in situ" because "the women wouldn't do anything about it if it were called dysplasia." The gynecologists finally pursuaded the pathologists to accept the term in situ, and then also pursuaded them to read the exfoliative cytology slides, as well as the biopsies. Their concern for the term carcinoma in situ stemmed from their certain knowledge that preventive medicine wasn't as commanding as the treatment of cancer. They knew that the in situ lesions were treacherous and became invasive without warning.

F1. THE WASHINGTON CYTOLOGY UNIT AND PAP SMEARS ON EMPLOYEES.

The Cancer Institute under Dr. Ray Kaiser as head of control programs, had set up the Washington Cytology Unit. This unit of a pathologist and about twelve cytologists had been established to do Pap smears on women employees in the Washington area. I inherited this operation, and continued it, doing contract work for various groups including the Air Force. It was expensive and controversial in that it performed Pap smears for some but not for all, at no charge. It was discontinued with regrets.

F2 FLORIDA ADC PROJECT AND GROUP DISCUSSION.

Early in the program Dr. Heller and I discussed the need for a large study of women at high risk of cancer of the cervix. What could we learn from such a group? For one thing, we wanted to know the prevalence of cancer of the cervix in an untested population of women at high risk: Multiparous, low income, promiscuous. Dr. David Wood confirmed by a review of biopsy sections that 2% of the women had cancer of the cervix, in situ or invasive, at the time of the examination. We also learned that this group of women when given the opportunity to discuss the availability of Pap smears in samll group discussion would ask for a Pap smear in over 90% of the cases.

F3. FOURTEEN STEP PROGRAMS AND THE TWENTY-FIVE CENTERS

It was mentioned above of the great loss of cervical cancer to follow up. This occurred, we observed, at fourteen different steps. Grants were offered to institutions that would perform Pap smears with attention to those steps where loos of cervical cancer could be expected. This led to many demonstratons which showed the kind of multi-discipline approach necessary to a high quality screening for cancer of the cervix.

F4. THE AMERICAN ACADEMY OF GENERAL PRACTICE AND THE OFFICE DETECTED CERVICAL CANCER PROGRAM.

The general practitioner in 1964 was at a low ebb. It appeared that he would drop out entirely. Many concerns were expressed about a medicine without any primary physicians. As a pilot study of Health Hazard Appraisal, and a test of the interest of the general practitioner in a preventive program, Drs. John Heller, Amos Johnson and Ulrich Bryner and the writer deveoped a program directed to detection of cancer of the cervix in the office of the generalist. Only members of the AAGP were included. These were organized in groups of about 12 physicians each, and arrangements were made to keep records on all patients examined, including follow up to biopsy results. The study was terminated in the reorganization of control programs in about 1969. But at very low cost to government, 1.7 million Pap smears were taken by physicians and 4,000 cancers discovered. The AAGP was so well satisfied with the program that they set up a permanent cancer committee.

F5. SOS (SELF OBTAINED SMEAR), THE PRO AND THE CON.

Much interest was expressed in lay magazines in the performance by women, on their own, of Pap smears, in the SOS package. The fact that these services often or usually were conducted outside of the usual routine of practice, excluded them from the careful follow up necessary for a high quality program. Their demise could have been anticipated.

G. CANCER OF THE COLON RECTUM

Of the four cancer sites, this proved to be the most difficult to organize a control program for. The reason probably lies in the fact that the ACS and the Cancer Control Program were committed to a proctosigmoidoscopy, when the average individual did not tolerate a proctosig, and could not be taught to tolerate them. They were painful. Today the hemoccult test seems to be meeting the need very well as a screening device. But at the time, Howard Gowen a member of the staff proposed that the fiber optics be adapted to provide an endoscope for replacing the inflexible proctosig. The original intent was to develop a 50 centimeter instrument, which would have been able to discover at least 80% of the cancers or polyps, and could be placed at perhaps a cost of six or seven hundred dollars in the hands of the family doctor. The write- did not follow the development of the instrument after 1965 and today the instrument is about six feet long, goes all the way to the cecum and has been so successful in the management of colo-rectal disease that it has seemed to me that every city over 100,000 has a specialist in the use of this "Colonoscope".

H. HEALTH HAZARD APPRAISAL.

The study of HHA by Dr. John Hanlon led to the development of a more comprehensive program by Dr. Joseph Sadusk, Jr. He saw the need for a multi-discipline approach to assessment of risk, and each of the consultants at George Washington University School of Medicine who participated, brought important risk factors to the evaluation of chances of death from each of the top causes. This work, involving 103 patients in the out patient department, was published, along with an amended health hazard appraisal chart, in the Journal of the American Medical Association in 1968 (JAMA 203: 1108-1112).

Let me know if I can provide any more information.

Sincerely,

Lewis C. Robbins M.D.

Lewis C. Robbins, M. D.
Consultant, Health Hazard
Appraisal

LCR/kw

I N T E R V I E W

Interviewee: Dr. Lewis Robbins

Interviewer: Lester & Devra Breslow

Location: Indianapolis, Indiana

Date: November 20, 1975

Interview with Lewis Robbins, M.D. -- November 20, 1975. Held at Dr. Robbins' home, Indianapolis, Indiana. Interviewer: Dr. Lester Breslow and Devra Breslow.

ROBBINS: The Health Hazard Appraisal had been a part of Cancer Control from the start. I brought all the files to the Methodist Hospital. I was working for the Methodist Hospital until a year ago last July, (July, 1974). I took my personal files and Cancer Control files. They are down in the basement.

I didn't have the time to do this job like I would have liked to, but let's tell you where we are. Now, in here is my log. If we take just the '57 log, (this is where Rod Heller brought me in May, and planned a program from May through December) that would give me everything that I needed to know, in '46 and '47. But here I am now, in May of 1957, and I have been working for a year to try and get out of the International program. He (Heller) finally got me into the Cancer Control Program, while it was still at the Institute. But our plans were to take downtown the Institute's program in Cancer Control which was very little, most of it was research. So, I found myself in the position of looking the whole thing over, to take what I wanted, and take it "downtown." In the latter part of November, I picked up the files. There were about three file cases and I took them all downtown. I put them in the Cancer Control Program office there and we were off.

DEVRA: I am puzzled about something. Was it a deliberate move to take Cancer Control programs from the NCI, which was research oriented, and move it to the Bureau of State Services of The Health Service? Were there forces operating in the NCI that were antagonistic or unsympathetic having Cancer Control in the Institute?

ROBBINS: I don't think the NCI ever wanted to give anything up. The Surgeon General said, "you are not doing cancer control." The Cancer Institute will never give anything up. They will keep everything that they can. Investigators have an allergy. When an investigator finds something, he doesn't want to see it operating until he's got another piece of the money. Investigator's allergy, I call it.

Have you ever heard of Willy Sutton?

DEVRA: Yes.

ROBBINS: He said he robbed banks because "That's where the money is." So, there are roles that we play. I think of Cancer Control as having an allergy. The allergy is the cancer investigator. But, at times it's very, well you know, it's critical. What would you do without them? How can we live with them?

LESTER: Would you start back, as you did a few minutes ago, Robbie, and tell us a little about 1946, how you came into Cancer Control at that time, and what you did briefly in the intervals before you came back in May 1957.

ROBBINS: In, from '41 to '46, I was in a local health department. I was a city health officer. First a county-city health officer, in Wichita Falls, Texas, from '41 to '43. Then from '44 to '46, I was a San Antonio City Health Officer. In these two assignments I was carrying out what was considered "for the good of the Service." That term "for the good of the Service" was deeply imbued in all of us. If you didn't affect the death rate at some phases in your program, you wouldn't get anywhere. So the "good of the Service" called for me to be at a local health department and know what was going on in our local health department. Now, even before that I had one local assignment, in Bloomington, Indiana; and before that I had had a year and one-half as director of local health administration, in the Indiana State Health Department.

ROBBINS: Before that, I was in the Johns Hopkins and before that I had an internship and residency in Medicine at the Methodist hospital here. That goes as far back as we need to go.

LESTER: In April of 1946, what happened?

ROBBINS: In April of 1946, Len Scheele was looking for somebody to go into Cancer Control. I don't mean somebody. He was looking for officers. And I was asked if I would like to take an assignment in Cancer Control. Well, I had had this previous experience. So, in 1946, they sent me immediately to Roswell Park, and I spent about six months at Roswell Park, and then selected assignments with Herb Lombard and various others.

LESTER: How did Scheele happen to pick you?

ROBBINS: I think it was because I was available.

LESTER: You were willing? You were a public health officer.

ROBBINS: And I said yes.

LESTER: And after you had this period of indoctrination at Roswell Park and with Lombard and others, what did you do?

ROBBINS: Then they sent me to Chicago. In Chicago, I was given an office in the Regional offices of the Public Health Service. We had five or six states. We had Wisconsin, so I met Stovall. We had Ohio, so I met Bowman Crowell of the Cancer Clinics. He was in the American College of Surgery and he started the Cancer Clinics. He had the very first one which was in Columbus, Ohio. It later became a part of the Ohio State University.

Very shortly after that, exhortation may be rewarding to the exhorter but only demonstration brings change. If you demonstrate, people have to do it, they just have to. So from Columbus, he went to Cleveland, Toledo, Cincinnati, we had 35 cancer clinics in Ohio! That first demonstration was powerful, because it was limited to one thing and that was the diagnosis of cancer.

LESTER: In the region, you developed the rudiments of a cancer control program?

ROBBINS: I assumed that my job was to get cytology started. So, I was beefing it up where I found it in Wisconsin. I started it in Indiana and Ohio. Illinois I talked about it, but I don't remember getting anywhere. Kentucky was in and out, but I did try to get it started there.

LESTER: Who was at that time in charge of, or what there anyone in charge of Cancer Control for the whole service? You were the Regional Chicago office.

ROBBINS: Austin Diebert. He could have come in December of 1945, maybe even early in January '46.

LESTER: How long did he stay in that position?

ROBBINS: He was there the entire time I was there, which was until July 1947. I think he stayed two to three years more before Ray Kaiser took it over.

LESTER: So the sequence was Austin Diebert started it, in December 1945. That was prior to availability of Federal funds or for the states. Were there other regional officers at that time?

ROBBINS: I think I was the only regional officer. Diebert and me.

LESTER: Then Kaiser came in about 1950 succeeding Diebert. At that time you were in Heart Disease.

ROBBINS: Summer of 1947 they assigned me to Temple and the University of Pennsylvania, and I began tooling up at Temple in heart disease. By November, we were in Boston, I had a chance to select where we would go, and I said there isn't any question. Paul Dudley White is in Boston and we should go to Boston.

LESTER: What did you accomplish during that year or in '46 and '47 when you were with the Chicago Regional office?

ROBBINS: All that I could point to was getting for myself an acquaintance with cancer clinics. Taking cytology to every state and getting cytologists started in two states. A familiarity with cancer registries.

DEVRA: I think when we get back to this period we ought to follow an outline. This one fits in pretty well.

ROBBINS: This outline in here I can always talk to it. But it isn't the way I worked. I worked by cancer sites. This cancer control entity.

Now, my first acquaintance with something called cancer control was in 1940 down in Bloomington, Indiana, when the old Women's Field Army had a militant, feminine, delightful woman named President, Florence Franzen. It was her job to pull everybody together in the county and work on cancer. She was doing it. She wouldn't let anybody tell her no. I was telling her about how busy I was with maternal and child health and communicable disease and all that, and she said cancer is important, too. I said it isn't a public health problem, and she said well it's my problem and why can't you help me.

Then, my next feeling for cancer was when I met Bowman Crowell and started visting these cancer clinics and found myself looking at a disease that killed many, but if they had treatment they would save a lot. And suddenly those cancer clinics told you something. I think they were powerful.

In the early days, they demonstrated patients with cancer. I will go more into it, but to me the "march of time" and cancer of some sites like colon-rectum, it is relentless. To me Cancer Control is limited to one individual. Unless you know where that individual is, in his natural history of developing cancer or the disease itself, why you are really not concerned with cancer control anyway. When the investigator doesn't keep that in mind, when the teacher, the medical school teacher doesn't keep it in mind, the professional organization doesn't keep it in mind, when the health agency doesn't keep it in mind, when the practitioner doesn't keep it in mind, when the patient doesn't keep it in mind, whey they get involved in all kinds of games, you know. There should be an effort to learn more about the natural history of cancer--in practice.

I have to tell you a story. I had a very close friend named Jim Young; he helped us to develop our Health Hazard Appraisal program. He told me about a woman who came in with her daughter. This woman was about 65 and she had a cold, so he treated her cold. Six months later her daughter brought her back in and there was something wrong with her breast. Jim Young felt her breast and there was this tremendous mass. Unquestionably cancer, Jim Young said: "Why didn't you tell me about this before?" She said, "Oh, I did. I came in six months ago, but you talked to me about colds." Her problem was cancer when she came in before, but now she had somebody to blame cancer on. She didn't have the courage to face it herself. People play games with each other when they're not taught appropriate roles.

We can figure out all kinds of ways to solve problems to make problems bigger or smaller. Here is where we have to start. With the individual. You can have a great success with the cervical cancer program that finds carcinomas in situ, and you know that if you had 100 in situ, 40 of them would go on to invasive cancer, 25 of them would regress and not even be cancer. The rest of them would just stay right where they were. Carcinoma in situ for the rest of their lives, as far as we know. But those 5 cancers that you discover, what did you do for the 100 women, or for how many you screened? What else did you do? What have you done today for them?

Austin Diebert and his program came at the right time to explore the work of Dr. Papanicolaou. The program that I developed was built around that. The cancer registry today is of great importance, but the early registries weren't important except for certain demonstrations: Massachusetts, Connecticut and California.

DEVRA: Did people have much confidence in registries? Showing the way at that time in the late 40's and middle 50's?

ROBBINS: They did in Massachusetts where Herb Lombard was developing his cancer registries, and he had more data then he could write up. Many stories. They never got written. Margaret (Mrs. Robbins) helped Herb Lombard on the retrospective study of breast cancer.

I got assigned to Roswell Park. Dr. Kress was a dynamic extrovert, very opinionated but really good for that job, because he started putting this cancer hospital together.

When I went there I wanted to get my finger on as much cancer as I could. Clinically, and at autopsy, I got my fingers on it. I went through cards and reports and files and I got a feel for the cancer that they were looking at. That New York State hospital had some clinicians.

Now Rod (John R. Heller) - I told you a little about him. When the Surgeon-General told us we must take Cancer Control out of the Institute, here was Rod, who was an old public health man and it must have hurt him very much to lose it.

LESTER: Burney was the Surgeon-General and Heller was the NCI director at that time?

ROBBINS: Correct. Burney was depending on Rod to choose a man that would take the job. I know Rod, he oriented me into the public health service. He had looked over the available people and I was available. I can't tell you how frightening it was to be responsible for the entire country's Cancer Control, especially with all the antagonism and hostility that I would encounter out at the Institute. I had to spend a year almost a year at the Institute with all that hostility and seeing me take something away from them that they wanted so desperately. I could document the desire on the part of the Institute to keep cancer control. (Pointing to the log 1957-1965)

ROBBINS: I ought to let you thumb through that log and see what they were giving me, and I felt that if I couldn't write it in my own words, I didn't learn it.

LESTER: In 1946 and '47, there was the beginning of Cancer Control with Austin Diebert and his interest in cytology, funds for the states, and the embryonic tumor registries in Massachusetts and Connecticut. . .what do you think was at that time promoting Cancer Control? What were the forces that made it happen in 1946-47? (Had there been anything previous?) What were the forces that kept it back, it didn't go very far in '46, '47.

ROBBINS: The forces that put us to work in the public health service. Back in 1920 there was a man named Dudley Jackson. I had been communicating with him since 1921. I was reading about his treatment of snake bites and teaching it in first aid. But he was a gynecologist. He had a very great personality, Dudley Jackson, was concerned with cancer in the 1920s. He began writing letters for ten years. He wrote everybody that he could think of and somewhere those letters are still existing, but I have a couple of pages in my log that summarized as much as I could get out of the letters. But, he said "all I did was to sensitize myself to cancer in those ten years." About 1932, his cousin, Maury Maverick said, "I am going to run for Congress and I want you to vote for him." And Dudley Jackson said, come to my cancer clinic. After they spent the day in the cancer clinics, Dudley Jackson said, "Now I want you to write a bill when you get to Congress on an Institute for cancer research. We are not co-ordinated. We have to have a bill on cancer research." So, Maury Maverick became the legislator, and when he got to Washington he goes to the Hygienic Institute and they assigned him to a guy named Verhultz. He and Verhultz spent almost one year writing a bill. Verhultz had it in the drawer. One day Senator Bone came to see Verhultz. He said, "I am interested in something on cancer. I want to get a program started on cancer. What can you do?" Verhultz pulled it out and showed it to him. He took a look at it and said, "This is what I want." He turned around and walked out the door, and in one week he had ninety signatures of Senators.

LESTER: That was senator who?

ROBBINS: Bone of Washington. So, when it came over to the House of Representatives with all those signatures, Maury Maverick saw that this was his bill. He had all these signatures. He didn't know how it happened. He didn't object. He was over-joyed. He had Dudley Jackson off his back and he didn't ever try to make a claim, well I don't know, that I know of for any historical background on it. But of course, it went through the House of Representatives like magic and so in 1937 we had some legislation. That legislation began putting people on cancer.

The work of Dudley Jackson was directed to one thing--getting a federal research program going in cancer. He didn't think anyone else had the money, clout and continuity. I knew him from having been a local health officer in San Antonio, and returning later to tell him I was to be the Chief of the Cancer Control Program. He gave me two or three days to tell me what he could about his dreams and the size of the job as he saw it. Since I had been reading his work from 1922 on,* I was tremendously impressed with his contributions and his dreams.

Now you have people and money and the talk has to go to cancer control, so our reluctant public health service found themselves in a position where they had to do something.

LESTER: When did Cancer Control then become an entity within the Public Health Service?

*Dudley Jackson began writing about the treatment of snake bite before 1921.

ROBBINS: It didn't become an entity in the Public Health Service until Rod Heller was told that we had to have a full-time program out of the Institute. He was told that, but, he decided to comply with it.

LESTER: That was in 1956? Then you wouldn't count then what happened in 1946 and 1947 as Cancer Control?

ROBBINS: I wouldn't call anything Cancer Control until we had been in the job and then some things happened. The most important thing that happened and it happened without my "thinking it up", was when my chief Al Chapman (whom you know) said "I want you to put together a regional conference on Cancer." From the time he said that, I was trying to get the Institute to help me on cancer control. Now the first three months in 1958, I tried to get a little work started.

DEVRA: What do you think wrecked cancer control, Dr. Robbins?

ROBBINS: The thing that wrecked Cancer Control was when, in about 1969, they decided to close up Cancer Control and Chronic Disease and they did away with the programs.

When an officer in the PHS was assigned from a public health program to a research program, he tried to do the best he could do to carry it out. The individual in government has to perform or leave. My concern is with the difference between research, teaching and practice. If you money comes from research and you are assigned to research, there are rules like "publish or perish" to guide you. If practice of public health is your assignment, then "reduce the death rate" is the responsibility. Your decisions have to be for that year and what you can do with available knowledge. If teaching is your role, then you can't specialize on any particular knowledge, as a rule, but to prepare practitioners to practice with whatever new knowledge is available. The teacher and the researcher make good consultants and advisors to control programs, but they cannot and will not assume a continuing responsibility to practice. The control program chief has a lonely job of taking what is known for sure and what is prudent, (can't default in its practice because the evidence is so strong) and developing programs for that year. Now here's the problem. In the Institute that is supported by funds for research, one gains no seniority and leadership by assuming and discharging control responsibilities. As long as the Institute has both a research and control role, the research will be beautiful, and the control will be slighted. Before the PHS was dominated by the appropriating and administrative people, there was a division of responsibility between research and practice.

My reference to "investigator allergy" is a reference to the responsibility a researcher has if he's working for a research agency. And the conflict of interest he has if he's doing control. For the research minded are not looking for the practice of research results, but the determination of that performance which will produce certain research results. Once their reference group (other researchers) accept the findings, they're off after protocol that will interest their colleagues. This is much more simply stated if one will ask who's paying the salary.

Now, here is one program that was going on. The office detected cervical cancer project. They now have 1,700,000 Pap smears taken in doctors' offices. This was having a tremendous impact, and it was difficult to get up to the point where the Academy of General Practice agreed to have a program. Five thousand doctors had now done 1,700,000 Pap Smears, and what did PHS do? Boom, cut it off. And every other Cancer Control activity in CCP cut off. Why? Because the investigators were in charge, and that investigator allergy was at work. They couldn't see for anything

ROBBINS: the application of what is now all known to the individual. They couldn't see that.

Here are 200 million people in the U.S. all at risk of having cancer and they cut the service program because we don't know enough, God only knows why. But the program was in the hands of the investigators and they wanted to take over.

LESTER: This was in what year?

ROBBINS: 1969.

LESTER: Why in 1946, when you say there was a cancer clinic movement, when the Pap smear was available, when there was a Women's Field Army, when there was some professional interest, when there was a National Cancer Act, when there was money for and from the states for Cancer Control, why didn't Cancer Control get really started then? You say it only really began in 1957. Why didn't it start in 1946?

ROBBINS: Well, I think it's because the program was dominated, if not completely, by the Cancer Institute, and the Cancer Institute developed research programs. We have to know more before we can apply. Well, you know, that depends on whether you are an investigator or a practitioner. If you are a practitioner of preventive medicine, you always settle for what we know today, isn't that right?

LESTER: So you would have attributed this entirely or largely to the investigators. What about professional resistance by elements of the medical profession that didn't want government participation in a program like cancer control? Would you attribute any of it to that?

ROBBINS: Well, let me talk with you personally rather then talk to the world. To you personally, I would have to say, the man who is rich in alternatives, even if he is working for government, will always find a way to operate. Whether you are in government or a gynecologist, you are always going to be fighting with people. To me the answer lies in having a man who is rich in alternatives. Then you will find a way.

LESTER: So, the locale of the Cancer Control element of the Federal government in the Cancer Institute, oppressed by investigators' point-of-view, you think kept it from growing in '46 and '47. Then it emerged again in 1957 when Rod Heller was head of the Cancer Institute acting under the direction of Burney, the Surgeon-General who brought you in to develop the program.

What were the forces that led Burney to re-activate Cancer Control? To put it out of the Institute into the Bureau of State Services?

ROBBINS: I think they were pressured to do something about Cancer Control. There are always people with this equipment you know. They are always thinking. There are legislators that think. It doesn't make a difference what job you are in somebody is always thinking. We have checks and balances in government. So they began to ask questions and what are you doing about cancer. Here is this man in Congress that died of cancer. What are you doing about it? They weren't happy with what Ray Kaiser was able to do about cancer in NCI.

LESTER: He was at that time, from the early 50's to the middle 50's in charge of Cancer Control for the Public Health Service?

ROBBINS: Until late 1957, he was in charge. He was doing a good research job. A lot of it was operations research. But there comes a time when you have to get out and do some programs. Even when we had the chance, of course, we had to scratch to look.

But about 1961, I had the most wonderful advisory committee. The most wonderful staff. Dan Zwick and Cecelia Conrath and Dan Horn, Harvey Geller, the statistician. I had a tremendous staff.

DEVRA: Did you pick this staff?

ROBBINS: Yes, I picked them myself. Hand-picked. We were in a position to do anything we wanted to. It was something like Camelot. We knew it couldn't last, but boy while we had it.

We must not overlook Dave Wood, because he was giving us a climate. We were able to work with doctors and work through all of their different organizations. As long as you were helping them to do the thing that they knew they had to do, why they worked fine. It was only when we talked about roles that we had problems. They didn't always accept my role. Well I'd find some alternative. If you didn't do what you wanted to do in Cancer Control you produced the best registry and the best way of measuring success in Cancer Control. So we had an evaluation.

DEVRA: What did you mean here when you said the problem against role?

ROBBINS: When 1958 came and we were struggling for something to do in cancer and I was trying to get some cytology going, but I couldn't get any help from the Institute, and there just wasn't an opening anywhere. My chief, Al Chapman said, we've got to have regional conferences. So, suddenly it came together like a clap of thunder, this was the direction. In the march of those cancer conferences, (you were at the seminar conference, Les, because we involved you in the planning) and you told us who you wanted there and when we finally got to running these conferences, we found that every time somebody would say, what is the agenda, somebody else would say, what is your role? That would shut off the discussion. But, then we would say, what is the problem of cancer?

It was important to point to the feature that gave us a cancer control program. We used the principle of group discussion. We brought together the cancer specialists and consultants who were doing cancer diagnosis and treatment, with the public health people like myself and state health people. As long as we talked about cancer sites, and detection and etiology and diagnosis, staging, prognosis and treatment, we found great interest and great exchange. When we permitted questions like, "What role should I play," the discussion stopped and everyone started jockeying for position. We just stayed out of role and talked about cancer and what we knew that would control it. It gave us our base from which to operate for we had all the states represented in ten regional conferences. It gave us our advisory committee.

LESTER: Do you mean what are the sites?

ROBBINS: Yeah, what are the sites. Do you mean how many deaths there are? Yeah, that's right. They would all talk about a mile a minute, why, because they had never been asked before about anything so authoritative. So, we kept a stick for the State Health Department people who would start talking about roles, and we would just be ready to crack them. So, each of these regional conferences lasted about two days and we just talked steady, had no agenda except "what do we know about cancer".

DEVRA: Had the formula grants to states begun? Were they available?

ROBBINS: They were out. They were not only available but they were being spent the way the state wanted them to.

DEVRA: That occurred even before the Cancer Control program was moved into the Bureau of State Services?

ROBBINS: It must have started. . .

LESTER: I can tell you when it started. It started in 1946-47.

DEVRA: Formula grants for Cancer Control?

LESTER: Correct.

ROBBINS: Some states spent it for cancer and some had subterfuge and spent it for different things.

DEVRA: One of the thrusts of the Regional Cancer conferences which involved State Health Departments and the voluntary sector, I suppose and other health professionals, surgeons, radiologists and so on, was perhaps to influence the use of those formula grants as well as to generate other ideas for cancer control?

ROBBINS: I knew that we could hardly affect the State Health departments. They are so hard-nosed. Well, their program was fixed and they resented anything that the Cancer Control program would tell them. What did we know about cancer in their states.

So, in order to get aboard and find something that we could do, we had to listen. That is the best thing we could have done, because I not only learned about problems, but I learned about where the leadership was. You could see leadership and boy, it was so obvious. What was the name of the radiologist who was such a. . .

DEVRA: Henry Garland.

ROBBINS: He gave me such a hard time. He started working me over on the annual physical. I wasn't satisfied with the annual physical. I thought I would do something better. But, he was using that to beat me. But, Dave Wood spoke up for me. Tom Carlisle spoke up, then John Cline and suddenly Henry Garland had to shut up.

LESTER: They are all West Coast people, that is interesting.

ROBBINS: They were. That was the regional conference--California, Oregon, Washington, and Nevada.

LESTER: In the use of the formula grants, could anything have been done by the Service, in spite of the resistance of the State Health departments to get more productivity out of those grants for the states.

ROBBINS: I neglected in the first four years but then we involved the states. By 1962,3,4,5, I began trying to get them to spend money for cancer control. (In your outline, on Definition of Cancer Control, you had the most wonderful concept, dividing between research and control; research finds the means; control identifies the means to us, evaluates and promotes it.) Now to me, that word "means" has a package; what if a means develops to stage cancer or to diagnose cancer better or to propose cancer better.

The Pap was one obstruction, that was a diagnostic test. But it really, it has to be interpreted, and you know why, because it identifies a person who might have carcinoma in situ. The Pap Smear isn't diagnosing cancer, it's diagnosing a precurser. Why do they call it cancer then? I'll tell you why. Because nobody would pay any attention to it when they would call it a dysplasia. They won't do anything with it. But, if you call it carcinoma in situ, then they will examine it, do something with it.

I remember a battle royal at Roswell Park in 1946. Three men got together, then five then six right in the hall and--the pathologist was saying that the Pap Smear is no good. You can't diagnose with it. But, Jack, I can't think of his name right now, but he was there and he said carcinoma in situ is not a cancer but we have to call it cancer and the pathologist said we can't call it cancer if it doesn't metastisize. Or if it hasn't already metastisized. Well, they did.

LESTER: Who invented the term carcinoma in situ?

ROBBINS: I don't know, but I sort of think it was Papanicolaou. . .or Traut.

LESTER: That term was important in getting the thing used.

DEVRA: Before we get into specific topics, could you tell us how the priorities are set in your program. To what degree did your advisory committee influence the priorities regarding how you were going to spend you money, where you would spend your effort?

ROBBINS: The selection of the advisory committee will tell you part of it. I asked Rod Heller who the best men were. I took his suggestions and I thought about them. Some of them I didn't even follow. One of them was Joe Cunningham, an influential cancer man but so modest that you probably won't hear much about him. But Cunningham, in Birmingham, Alabama, was a very effective guy. I told him that I wanted to get Dave Wood to be the Chairman and I was afraid that Dave would turn me down, so I began to get him to help me lay a trap for me. Naturally, I didn't ask Dave until he got real enthusiastic.

But every step from the time that I started working on the advisory committee until Dave said yes was designed to make him the chairman. I knew he could give us leadership like nobody else would, and he was communicative. John Cline wouldn't talk to anybody, but Dave was afraid of no one, so we created a climate through that advisory committee, and Dave Wood that allowed us to work with people. Now, Dave has his weaknesses. Who hasn't? Everybody has got them, but for that job he was just right.

DEVRA: Was he influential with others on the committee and also with your staff in helping you to set priorities for action?

ROBBINS: I told him early that the staff was my job. The advisory committee was his. Dave didn't want, at least I don't think, to try to influence my staff.

The priorities were set by me and I had been working for years on them. I didn't want anybody to tell me. I had the responsibility. I selected cancers of the lung, breast, cervix, colon, rectum. We had to take cancer of the head and neck.

DEVRA: Was that a political decision?

ROBBINS: More or less. The big program that we got into was more political than anything else. It was the dentists that worked.

ROBBINS: Cancer of the mouth and pharynx was assigned to a dentist, because we wanted to be able to get dentists to practice cancer control. Unfortunately, we didn't find an effective tool for dentists. The Pap Smear was too highly technical, too uncertain. But to my dentist chief, cancer was a fascinating field, and there was always the chance that we would find an effective cancer control program for the mouth and pharynx. We enlarged the field to the head and neck, hoping that there could be more cancer control in the enlarged area. I don't remember finding important support for this concept. In my discussion I agreed that it was political. It was political in that all things are political, that one has to find an approach that attracted others because of its soundness.

DEVRA: Oral cancer detection?

ROBBINS: I can't help you in this area, because I didn't work very hard on cancer of the head and neck.

LESTER: On what basis did you select the other sites that you mentioned?

ROBBINS: I wanted the top causes of death. Almost half of all cancer deaths are in those four sites. If I couldn't control those four, I wouldn't be doing anything about cancer control. You see you take something like chorocarcinoma. It might make a big splash, 500 a year. But we had to do something about cancer.

DEVRA: About the ones that were killing people.

LESTER: In regards to those main sites, what were the major external forces that were promotive of cancer control and what were the deterrents?

ROBBINS: Well, let's take cancer of the lung. The promoters were very small. There weren't enough people really. The Public Health Service officers - so many of them smoking, they gave me no help. There were a few people in the Public Health Service who admitted that cancer of the lung was important, it was involving a lot of deaths. But except for Mike Shimkin and Rod Heller and one or two others, I got little support. The tobacco industry, those people who were in some business affected by the tobacco industries, it was just one tremendous obstacle.

The tobacco industry and Tobacco Industry Research Committee did look for other causes of lung cancer besides cigarettes. They financed one program in California on air pollution and the automobile. This to me was like a red herring. There are more cancer deaths (lung cancer) in the city per capita among cigarette smokers, but I don't think it can be importantly pinned on the automobile as opposed to other air pollutants.

This is an illustration. He saw the opportunity to get some money and promote his professional continuing education program. So, he went to the tobacco industry and they gave it to him. How did they give it to him? What did he have to call it? Well, he had to call it air pollution. So, they developed quite a case for air pollution causing lung cancer.

DEVRA: To counteract the myth, as it were, that cigarette smoking was really the cause of lung cancer?

ROBBINS: The tobacco industry research is so well known to both of you that you know exactly. But there was C.C. Little always in there looking for any chance to do-- I call it "a chance to get a doubt" every three months.

If they could find some reason that would make headlines, then the cigarette smoker would be kept off balance. His cognitive dissonance would mount--and then the doubt would come and it would drop. There is no cognitive dissonance now. Then they would build up again and they would have to hit him again. So, here was the tobacco industry with their techniques.

LESTER: What about the 1959 report on lung cancer?

DEVRA: From Lee Burney, then Surgeon-General. Was that influenced by your Cancer Control program? Your people were the staff for that Surgeon-General's Report.

ROBBINS: I did it.

DEVRA: You did it.

ROBBINS: But, on February 4, 1958 there was a meeting and Dr. Heller presided.

DEVRA: Was the meeting of the advisory committee?

LESTER: Let me read the paragraph from Robbins' log. "Dr. Heller presides at meeting to discuss a physician statement of the Surgeon-General on smoking and cancer. The following were present. Dr. Shimkin, Dorin, Dublin, Watt, Zukel and Zessin. Shimkin presents an outline of the statement which makes the point that smoking is one of the causes of lung cancer and recommends that school children be taught this fact. Dr. Watt stressed the point that a program of education of school children would look like a missionary activity. He felt that people would react strongly against a missionary attitude on the part of the federal health agency. He said the climate was not right for such a program. Dr. Heller planned to give a physician statement to the Surgeon-General."

ROBBINS: It reminds me of a story. The little boy went into the library and said that he wanted to learn about penguins. Adn the librarian said, "here are some books on penguins (gives him 5-6)." So he goes and browses around. He found a stack of books about this tall. The little boy in five minutes was walking out. The librarian said, "Just a minute little boy, didn't you find what you wanted about penguins?" He said, "Yeah, but that was more than I wanted to know about penguins."

LESTER: Could you tell us in your own words about that 1959 statement.

ROBBINS: On D-day, June 6, 1959, we had a big meeting and it will be in there. On that day, the Surgeon-General said, Robbins, (this is Lee Burney) I want you to write a paper for my signature to go to JAMA. Let's tell them about cigarettes and lung cancer. So I started writing. We had ten drafts by the end of the summer. These drafts would go around the PHS and each person would correct them. Forty-two drafts in 18 months after D-day. We finally had the paper that went to JAMA. It appeared in November 1959. JAMA was tickled to death to have it.

I had a task force that wrote it personally for a while, then I got involved in other things and I wasn't as good as a chairman of this task force named Sam Kirkwood. Sam Kirkwood is a real good writer, so he wrote the drafts. They were never mailed, they were hand carried everywhere we went. We didn't wait that 18 months, you know. Just as fast as we could get them out, we did. I would hand-carry them to Mike Shimkin at the Institute. Mike was the most talented guy. (You know Mike very well.) He was unafraid. Some people think that he is stupid, he is so unafraid. But I think he is one of the big cancer people.

DEVRA: Why was the decision made to put it into JAMA? Because that was the most popular medical journal read by practitioners?

ROBBINS: That was the most authoritative place for a statement that could reach physicians.

DEVRA: Once it was sent, or hand-carried to the publishing offices at JAMA, how long did they hold it before they agreed to publish it? Or did they publish it immediately?

ROBBINS: Devra asked about how long the Cigarettes and Lung Cancer Paper was held up in the office of the JAMA. Not at all, beyond the editorial functions. They were very good about getting it out. They gave us a high priority. But the American Medical Association was dragging its feet on cigarettes and lung cancer. As you know the big research program that is still operating in the AMA was funded originally by the Tobacco Industry.

We tried to get the AMA to take a stand: Give us a representative to the Interagency Committee on Smoking and Health. Pass a resolution in their House of Delegates on smoking. I remember being at a meeting of one of the State Medical Societies and hearing an AMA representative say, "With the Medicare legislation coming up, we can't afford to antagonize the Southern tobacco states." But the AMA does not control the policies of the JAMA. I found them often in conflict.

Well, the stock market came down on tobacco the next day; then it gradually came back up. It had a tremendous impact.

LESTER: What was the relationship between that statement and the subsequent report of the Advisory Committee to the Surgeon-General on Tobacco and Health in 1964? What was the sequence?

ROBBINS: An authoritative position had been stated in 1959 on cigarettes and lung cancer. That paper was the official position of the Public Health Service on cancer of the lung until 1964. For a four year period, that statement was our official position.

I remember Lee Burney came to me and said, "I have been taking an awful beating on your statement that there is no proof the filter will reduce the risk of lung cancer. That is giving me more trouble then anything else." Well, 1964 came along and here was the Surgeon-General's Committee. The Surgeon-General was on this rostrum and some reporters asked, "Dr. Terry, can you tell me, what effect does the filter have on lung cancer?" Terry turns to the committee who says there is no proof the filter has any effect on lung cancer. Well, that's all he can say.

Lee Burney said that first position had more scrutiny than any paper since the Bible.

LESTER: Did you have any role in the Surgeon-General's Advisory Committee Development?

ROBBINS: No, very studiedly they kept me out.

LESTER: Why did they keep you out?

ROBBINS: By then I was so convinced, it would have been impossible. They wanted a committee that had "an open mind."

LESTER: Who would know the story about how the Surgeon-General's Advisory Committee really came about? What were the forces that led to it?

ROBBINS: There is one man. He was in the Surgeon General's office, commissioned officer. He worked in California for a while.

(I'll think of his name after a while). He was in the background. He put it together. (I can't think of his name.)

LESTER: At that time you were already identified as having an advocacy role with the regard of the control of cigarette smoking, so they couldn't let you participate.

ROBBINS: That's right.

LESTER: What would you say, during this whole period, were the turning points in applying the knowledge of what we knew to advance cancer control? What were the major turning points? Would you say, for example, one was the Surgeon-General's JAMA statement? What other points would you pick out as being critical during the time you were in the cancer control program, 1957 to 1965?

ROBBINS: If you take cancer of the lung, I would have to say that when Dan Horn agreed to come to the Public Health Service, to me that was the turning point, because it had a tremendous effect from then on.

Another turning point was when Katherine Boucot asked us for help, funds to put together or to report a study that she had developed. They got mired down due to a lack of funds. It was that study of 3,000 veterans and doing chest X-rays every six months, to see if it would affect the death rate from cancer of the lung. They improved the death rate by 2 percent I think. They went from five percent to seven percent in that study. So, I think when that came out, that was a turning point. The San Diego Saturation Study, also.

DEVRA: That's Dan Horn's first study? Was that done with Cancer Control program funds?

ROBBINS: It came out of Cancer Control. The most important time was when Harold Diehl in the committee meeting on lung cancer came in and tapped me on the shoulder and said, "Come here," he said, "look we want to develop an Inter-Agency Council, the National Interagency Council on Smoking and Health. What do you think about it? We think it would come better out of the Public Health Service than out of the Cancer Society or any other organization." I was so excited, I almost dropped my teeth. I said I thought it was a great idea. The Surgeon-General immediately said fine, so we started to work, building an Interagency Council. I was the Acting Chairman. Well, from the moment the Service accepted that, it was inevitable. I think that was a turning point. The Surgeon-General's report had something to do with it. But it isn't exhortation, it's demonstration that makes the change.

DEVRA: Did you think the Interagency Council had the means as well as the influence to demonstrate that stopping smoking would have an impact on mortality and morbidity?

ROBBINS: The Interagency Council was a means to strengthen and fortify the efforts of each agency through an exchange. One agency's work could be shared by all. One agency's efforts could be enhanced by being widely used. This worked very well on the National level. It permitted the cooperating agencies to share the heat we were getting from the Tobacco Industry and other tobacco interests. In the early days, early and mid-60s, there was a lot of indirect pressure.

Cancer of the breast. Now, you would like a turning point there. On March 24, 1961, we had a meeting at M. D. Anderson Hospital. R. Lee Clark presided.

LESTER: (Reads from Robbins' daily log.) Dr. Lee Clark and I discussed mammmography for cancer of the breast. I voice my concern for acceptance of our proposed studies of asymptomatic cases. Our anticipated 5 cancersin 2000 does not seem to have the ability to be persuasive in this area. Dr. Clark and I discussed acceptance of radiologists today in this process. We discussed the need for radiologists to demonstrate their ability to learn the technique of mammography and then perform diagnoses on cases of cancer of the breast. Dr. Clark and I discussed the possibility of changing the study to demonstrate reproducibility by several radiologists. Dr. Clark asked if I will meet with some of the people including Dr. Dowdy and Egan to re-discuss this possibility.

ROBBINS: That to me was the turning point. It was a just a march of events from then on to the completion of the reproducibility study to find that 80% of suspected cancer, if existing could be diagnosed by a mammogram and palpation. From there we went into training of radiologists, developing teaching sets, and our first meeting of our reproducibility study, known as the first annual meeting on breast cancer. (They had the 14th in Puerto Rico, this February. At that meeting, I presented a 10-minute speech on the history of the development of mammography.) And that's the paper. I'll be happy to have it typed for you so it's readable. They thought they would like to hear just how it was developed.

LESTER: I think better than getting it re-typed, Robbie, a Xerox of this would be better yet.

DEVRA: That's a good companion to Bob Egan's Wendell Scott Lecture.

ROBBINS: Mine preceded his. Oh, maybe mine followed. Anyway, they were at the same session.

LESTER: How did you happen to go to Texas and have this discussion with Lee Clark and that group?

ROBBINS: Beautiful question. On February 25, 1958, I attended a National Cancer Institute meeting. They were reviewing cancer research projects. At that meeting, I.S. Ravdin, the Philadelphia surgeon, said he would like favorable consideration to request a study of the possibilities of the diagnostic x-ray in early detection in mammary cancer. It has been found that mammary cancer can be detected on x-rays when it is as small as one centimeter. Such tumors are entirely unsuspected. This method of course is a screening device. There is considerable prejudice among radiologists against this study. Radiologists say that this is impossible and it cannot be done. Dr. Ravdin said he knew it was better than palpation, because recently he had seen several cases which had developed and which were predicted by x-rays but on palpation the surgeon said no cancer is present. Surgery disclosed that cancer was present and it was proved on pathological examination. The changes on x-ray to be associated with a calcification of the tissue in and about the tumor.

ROBBINS: Dr. Gershon-Cohen had applied for about $15,000 to continue his studies on mammography. He had been doing mammograms for about 10 years, almost alone in his interest in mammography. If Ravdin hadn't spoken up about the two occult cancers that could be seen by Gershon-Cohen, but not palpated by Ravdin, the research by Gershon-Cohen might have stopped right there.

Dr. King of the Navy made the point that this was not diagnostic, it was only screening. Dr. Kaplin objected to support of this study saying that a false sense of security would be raised by a negative x-ray. He said nothing will beat a physical examination and intuition on the part of the clinician examining the patient. Dr. Rigler urged that we check for unnecessary exposure in this study.

The Radiologists Section has already turned this study down.

(See Log for February 25, 1958)

But, ya know, here was Gershon-Cohen working for 10 years. He had already been working for 10 years. He was crying to get people to help him. How much did he want? Some pidily sum. As I remember it was about $15,000. But a very small amount, and they had already turned him down.

DEVRA: They? Meaning whom?

ROBBINS: The National Cancer Institute Radiologists Section had turned him down. Now it was being considered by the Council and Ravdin said, "I really just had to say something at that point. I have to tell you something. That man brought me an x-ray and said that he wanted me to examine this woman's breast. I did, I took the tissue out. It was negative on pathology. When I told him it was negative he came back and he said the cancer is there. Go back and find the cancer." Ravdin said, "I told him no way will I go back. I'm not going to disturb that woman any more. Two weeks later, I finally said yes, I will go back. He helped me find where to take the specimen, we examined it under a microscope and it was cancerous and I did a radical mastectomy." But his point was that Gershon-Cohen could see something, nobody else could. Gershon-Cohen would hold it under a bright light and take a glass and look and others couldn't find anything, but Gershon-Cohen said, it's there. Don't you see it? Nobody saw it. Radiologists couldn't see it. Nobody believed him. They thought it was--I don't know what they thought it was. That didn't move me. But when I heard that Robert Egan had a technique that was finding it, and was already publishing a study. . .

LESTER: He was at that time at the M. D. Anderson Hospital? His 1959 paper?

ROBBINS: When I heard that I went down to Houston. I was not impressed on the first visit. I went back on the second visit, March 8, 1961. It was February 25, 1961 and March 8 , 1961.

LESTER: This is the March 8, 1961 daily log of Dr. Robbins: "Dr. Morgan, Mr. Geller and I discussed the M. D. Anderson mammography study. We agreed that probably it would not be warranted at present to do a case-finding study among apparently well people from the general population. However, we talk about the high incidence that probably could be expected among certain women in the population. If 5 cases could be expected per 1000 of the general population, there might be a group that would have as high as 15 or 20 per 1000 women. These women would be those that had at

LESTER: least two relatives with cancer of the breast, who had a history of chronic cystic mastitis, who were over 40 years of age and nulliparous, and those women who had had one primary cancer of the breast. One mammographic x-ray is supposed to pick up 90% of those that can be found with x-ray. One x-ray will probably cost about $5.00 to take. Just as the ADC projects select a high incidence group, a project such as this might also select a high incidence group. For $1,500, or 3,000 x-rays, perhaps 40 to 60 cancers could be anticipated. We discussed the possibility of setting this up so that if it were justified, a project grant might be prepared for complications.

ROBBINS: Just preliminary thinking. But when I was with Egan at M. D. Anderson, I met a man who had paid his own way from Ravenna, Ohio, just outside of Cleveland. He had spent a week with Egan. I saw him on Friday of that week. I asked him, "Can you see any cancer in there?" He said, "Oh, yes, I'm finding cancer Egan couldn't find even on his own cases. He showed me how to improve the image." I asked him, "Do you think you could do them back home?" "Oh yes," he said, "I will have no trouble."

Suddenly, I could see radiologists all over the country learning this within a short period of time and doing it. I came back and I asked five men to go to Houston, I would pay their way. They were Jim Cooney, Radiologist and Medical Director of the Cancer Society, Ted Hilbish, Cancer Institute Radiologist, Tom Carlisle and two other men, top men in the country, Eugene Pendegrass and Wendell Scott. When the five of them came back, Scotty told me, "I had looked at mammography before, but what I saw down there is a quality I have never seen before. I don't konw what it will do, but I saw a quality film I have never seen."

So, then we had a battle with Endicott. He wanted to do the reproducibility study and I didn't trust him.

Here was a contest between the Director of the National Cancer Institute and the Chief of the Cancer Control Program. We wanted to perform a reproducibility study to see if other radiologists could learn to do mammography. We being the Cancer Control Program and the M. D. Anderson Hospital. This study we had planned would tell us that along with, since we were doing consecutive cases, the specificity of the mammography (did we get all the cancers) and also the sensitivity (were we getting false positives). Was mammography practice ready? How big a job did we have on our hands in teaching radiologists to do mammography. I had alerted Mike Shimkin at the Institute to the development by Egan. He had begun discussions as to what kind of study would be required to answer the biggie, "Does mammography save lives?" He began his HIP study, and we trained his radiologists as a part of our reproducibility study.

The contest came out as a compromise. Cancer Control would do the reproducibility study and the Institute would do the long-term research study.

My comment, "I didn't trust him (Endicott)" still goes today. The Institute has a primary responsibility and a continuing one: to do research. If their control programs aren't effective, that isn't their primary responsibility. Researchers can advise and consult on practice, but they have no continuing responsibility toward practice.

I found myself reviewing mammography with the American Cancer Society. What should we do about the question of screening. We decided that we were years away from the use of mammography in screening, in 1964. But we'd better work toward it, because mammography looked very promising. Meanwhile mammography could be used for the

ROBBINS:
management of breast disease, as a means of pre-biopsy review of the breast. So we would teach radiologists to do mammography, and by the time I left in 1965 we had trained about a thousand. (Postscript added in 1/1977)

There's an issue here. Les's committee that reviewed mammography for the American Cancer Society was reporting on danger of radiation. They could not accept a continuing responsibility because they were an ad hoc committee. They were advisors and consultants to those who did have a continuing responsibility. So far so good. But why was it necessary to take this advice and consultation to the public before it could be reviewed by those having a continuing responsibility to the practice of mammography. The ACS was heavily committed to a practice program. The fears that were generated among women under and over 50 may never be corrected. Here's a friendly comment in the context of my answers about the past and the present. How do we see the practice decisions which should be made in 1977, made in 1977? By insisting that the people with a continuing responsibility in practice make them. And that they have the advice and consultation of experts, and they can add this to their decisions, but that practice decisions be made by those with a continuing responsibility to practice.

DEVRA: Dr. Endicott wanted to do the reproducibility studies in the Institute?

ROBBINS: Right.

DEVRA: Because it was research?

ROBBINS: It was research and they milked it for everything it was worth. They started that HIP Study and it took years to get it out. But, meanwhile, Lee Clark asked, "What about that study of Robbins?" Endicott asked him what we thought we ought to do. I've forgotten just how it was said, but Lee Clark let him know he was real anxious to do the study we had planned. We had spent a lot of time putting it together. So, it got started with the sufferance of the Cancer Institute.

I love those guys, you know, Ken Endicott is great, but he is at the Research Institute.

LESTER: Now the sequence was. . .First, you were acquainted with Gershon-Cohen and Ravdin pushing his work; secondly, you visited Egan because you had heard about his paper. . .that was in 1959 or 60?

ROBBINS: My first visit was in early 1960.

LESTER: Your second visit was March 24, 1961.

ROBBINS: No, my second visit was within a month of my first visit.

LESTER: So they both occurred in 1961. At the second visit, you met the visiting radiologist from Ohio who gave you the notion that a radiologist could learn this in a week and he could even improve upon the interpretation that Egan was giving to his films.

Then you decided to invite down the group of Pendergrass, Carlisle, Scott and others to have an independent look. Late Spring of 1961, they came back and said, "That's the thing to do."

At that point, then, to develop the reproducibility study, there was a contest between the Institute and yourself, with the upshot that both did it. The Institute started the HIP study and you started what?

ROBBINS: I started the Reproducibility study, which called for 24 institutions to examine consecutive cancers that have been sent in for biopsy - if they were coming in for biopsy, every one of them had to have a mammogram first. So the consecutive cases were examined to determine how many that were found to be cancerous the radiologist was able to identify in advance, independently. And even he didn't know where the cancer must be, just he knew there was something there.

LESTER: So there were 24 centers that participated in that study? What did that study find?

ROBBINS: It showed that 80% of the positives were true positives. There were 20% false negatives.

LESTER: Was 80% of the cases in which the mammography was positive subsequent events showed that the patient did have cancer. 20% subsequent events showed that the patient did not have a cancer.

ROBBINS: No, that the mammography had missed the cancer.

LESTER: So mammography then detected 80% of the total of the cases that were being diagnosed by the best diagnostic techniques being applied at these 24 centers.

ROBBINS: Mammography would have picked up 80%. Now we had to decide, "What is that worth?" When a surgeon finds a lump, does that mean that you do a mammogram, and if the mammogram is negative you don't do a biopsy? No, you have got to go ahead on the suspicion. . .

LESTER: On the clinical grounds, because the mammography is falsely negative in 20% of those cases. How long did it take you to do that study?

ROBBINS: About one year and three months.

LESTER: Where was that published?

ROBBINS: A surgical journal.

DEVRA: What kind of participation or resistance came from radiologists during the course of the reproducibility study? Were they enthusiastic, supportive, willing or was official radiology very skeptical and disinterested?

ROBBINS: We would go into an institution. Lee Clark would already have called ahead of time so the surgeon would ask the radiologist if he would do him a favor and participate in this study. He was already sensitized by the guy he was working with. Getting paid, too. When the radiologist was asked to do this reproducibility study, why, he said yes. The only one that turned me down was Wangensteen at University of Minnesota. He couldn't see what it was worth.

LESTER: In this outline that you have given us, you have the names of Ian MacDonald and Gershon-Cohen. What did they do?

ROBBINS: Ian MacDonald and Gershon-Cohen were contemporaries. Ian MacDonald was saying, "Hey, you know there is a biological predeterminism that acts in about 40% of these cancers. They are going to go on to death no matter what you do. Twenty-five percent of them will respond, the rest of them it's touch and go." He was saying, "You really don't know enough about breast cancer to affect it." That was being

interpreted by a lot of people as a reason not to teach self-examination, not to look for it, just let it happen. Don't make an effort to find cancer of the breast.

But, at the same time, here was Gershon-Cohen, telling us something else.

DEVRA: Two poles.

ROBBINS: Right. So, we had a plateau, in breast cancer control, we were not affecting cancer of the breast. I knew that we had to resolve it some way. When I heard that guy from Ohio tell me that you could - "I can do it, I learned it in one week" - this made a profound impression on me.

LESTER: As demonstration, not exhortation.

ROBBINS: Right.

LESTER: What was the teaching set for training radiologists at Emory?

ROBBINS: After the reproducibility study we asked what we ought to do. By that time, I had gotten a young hospital administrator by the name of Bill Melton. Now, Bill did for breast cancer what Dan Horn did for smoking and lung cancer. Of course, Dan Horn was much more sophisticated, his methods were made more sophisticated. But Bill Melton was full-time.

We had this breakthrough and I couldn't follow up on it. After six months, Bill Melton took the breast cancer program over. All I had to do was to say "go," and if anybody threatened like my administrator, (I had an administrator that kept wanting to dip his fingers in, it wasn't Dan Zwick, it was the guy that followed) I would say, put money in that mammography program because that's paying off.

DEVRA: The concept was then to teach radiologists as fast as possible.

ROBBINS: We had about 1,000 radiologists trained in mammography before I left. I wanted to get them up to the point where we could then have a screening program. I knew there would be some tools developed. But so long as I was there, I had to hold back, because we neither had radiologists to do mammography nor did we have the acceptance of the medical profession as to the interpretation--the validity. We knew it would be a while, but we had demonstrations and training going on.

LESTER: How did Emory enter into it?

ROBBINS: One tie Bob Egan came to me and said, "I am going to leave the M.D. Anderson." We were right in the middle of the reproducibility studies. He said, "I am going to take a job at the Methodist Hospital in Indianapolis." "What are you doing that for?" I asked, "Well," he said "they are going to pay me $40,000. I'm only making $25,000 here at Houston." So I said, "You can't do that. You can't leave this study. You can't take this program with you to the Methodists." Well, he thought he could. I told him that he couldn't--that I controlled the money. Finally, we worked it out that he would return to Houston on the weekends to read films, so we could complete it at the M.D. Anderson Hospital.

LESTER: He would commute from here?

ROBBINS: But finally at the Methodist he sought to leave. Being an investigator he had become tired of practicing. Now, he would take a much less remunerative job, if we could find a place for him. We thought the Radiology Department at Emory was very

ROBBINS: useful. We arranged for him to have a grant down there. That center had become very important.

Let me give you a little bit of homely public health philosophy. If you have your efforts scattered all over, so that you have got to use a lot of traveling (like Les Breslow is doing right now in coming to Indianapolis) how much are you going to learn? To me you got to have a center where the "thermals" are concentrated, then you can fly. But if your "thermals" are all over the country and they are hard to find, they are hard to catch, then you don't fly. It's like using a sailplane. You have got to find a way to build your "thermals" and this is what we were doing at Emory. Emory had an excellent breast cancer program.

DEVRA: You negotiated with Emory to get them to bring Bob Egan there to set up one national center for mammography?

LESTER: What about these 14 annual meetings on breast cancer?

ROBBINS: Well, the first one was the "anxiety complex" meeting. The 24 radiologists that had been trained by Egan came. They were the most anxious guys you ever saw. Egan was supposed to find 98% of breast lesions with his technique, but they were missing a lot of them.

I think Egan was finding 98% of them in the older women. You can find them easily in older women.

He didn't lie about it, he jsut selected his population. These radiologists at the first meeting gave us all of their problems and we fed back to them a story. This story was published in the M. D. Anderson's Cancer Bulletin. At our first meeting, we were tyring to stuff their shirts, trying to build their hopes up. (The surgeons had also come with them.) All this effort to get them tooled up was put into the Cancer Bulletin article. (November-December, 1962.) The second year, they all came back. I have never seen such a happy occasion in all my life. They were all finding cancer. They knew each other had problems, and they were a happy bunch. The third meeting was a natural and because it seemed productive we just marched in to an annual meeting, not on mammography but on breast cancer. Thousands attend these meetings now.

LESTER: Are the original 24 radiologists listed in this Bulletin?

ROBBINS: Yes.

LESTER: Now, Robbie, there are several points here that I am puzzled about. Firstly, there was Gershon-Cohen, the radiologist who taught Ravdin, the surgeon that breast lesions could be seen on film. Other radiologists poo-pooed it, but Ravdin knew it could be done, because he was a clinician and Gershon-Cohen showed him.

You looked at it, but you were skeptical. Then you read about Egan at M.D. Anderson. You went there. The first time you were still skeptical. You went back a month later, thinking well maybe there is something there, and that time you met the visiting radiologist from Ohio. He convinced you, because he had gone through the kind of a week that was necessary to make a radiologist effective with this tool. He was a community radiologist, and that's what really turned you on.

Then you organized the reproducibility study of the 24 radiologists, at the same time contesting with Endicott and the NCI about a more research-oriented study epidemiologically based on the general population (the HIP study) and that went ahead. This all

LESTER: took place in the early 60s.

You were training the radiologists. By the end of 1965, you trained 1,300 radiologists to do this technique.

Now, it's 1975, and we have the American Cancer Society and the National Cancer Institute joined in a demonstration program in 27 centers around the country. We also have a member of the NCI staff, John Bailar, publishing a paper in which he points out that there is a radiation hazard from periodic mammography, and as a matter of fact there may be no gain, no net gain in early detection of breast cancer. More cancer is caused then discovered and cured. He bases his conclusion on the HIP study which was started in the early 60s.

This debate is going on now. It goes back to the statement of 1958, the earliest meeting when Leo Rigler pointed out that there was a hazard from radiation that had to be considered. Why does it take 17 years before we get to the point of where we are now?

ROBBINS: I just read a statement by the American Cancer Society. They have fallen back from Bailar's statement. They have regrouped and restated their position stronger then ever, that today it appears if we are prudent we will be doing mammography. Instead of limiting it to women just over the age of 40 or 50, we probably should be doing it even earlier. Now, they have (ACS) descended on Bailar. They have disected him, and they see what was eating this young guy. It's that "investigator allergy" again. He wants more money for research, and this is the way to get it. Just throw these doubts out and then people have to respond. They have to listen to you.

LESTER: You really think, Robbie, that's the motivation here?

ROBBINS: Well, I think it's a subconscious motivation by Bailar. We have been fighting the matter of radiation. It comes up at every meeting. We discuss how much, what evidence we have. There was never a session when we didn't discuss radiation hazard.

LESTER: Was it ever studied really carefully, systematically?

ROBBINS: By many different people. The literature is full of it. There were lots of studies of the radiation hazard going back to 1961. It's reported in that Bulletin. We have had a lot of money to test the radiation hazard, and it's been spent for that.

LESTER: What are the best studies about it?

ROBBINS: Check with Egan and Melton.

LESTER: Well, if there was one kind of resistance, then what else has required 17 years to get to the present point - and we're still not at the promised land.

ROBBINS: We've been at this since 1958, when Gershon-Cohen was talking to Ravdin about it. I feel that this has moved about as fast as any public health program I ever saw, to be as complicated as it is and as controversial. Cancer, takes a lot of demonstrating before you know anything about it. I would say, for a cancer project we moved pretty fast.

LESTER: Why did it take 17 instead of 7 years?

DEVRA: Resistance on the part of physicians and public apathy. People weren't scared enough.

LESTER: What were the most important things in your mind? You put your finger on this "allergy of the investigator" as the key element.

ROBBINS: Yes, when he isn't allergizing you know, he can do a lot of good work, but the minute they try to keep you from applying what they had discovered, you got to suspect them. Let me give a little secret. I call it the means to advance patient advantage. What is a patient advantage? Well, you got to put a whole kit together. That kit goes from the means. It's got to start with a diagnosis. If you got the diagnosis you can make a staging. Unless you know where you are at in that disease, you can't make a prognosis.

This is what our fiends in the Commission on Cancer Staging are up to.

From staging, you can go to make a prognosis. But once you know what the patient's chances are, without treatment, then you can say, how much can we effect this prognosis? Is there an alternative to this horrible prognosis? It might be 100% or it might only be 20%. In cancer it's 100%, but in pneumonia it might obly be 30%.

The difference here is what Thomas Chalmers, Mount Sinai University School of Medicine, calls the "Benefit to Risk Ratio." We call it Survival Advantage. It is risk reduction for the patient with cancer. Means is designed to advance patient advantage. But, by concentrating on it, by identifying with that word means, you can break means down.

LESTER: Can we go on to cancer of the cervix?

ROBBINS: I inherited the Washington Cytology unit.

DEVRA: Is that in Washington, D.C.?

ROBBINS: He had eight or ten cyto-technicians. They were in my department. They did Pap Smears. We did them for the Army, we did them for a group in Columbus, Ohio. We did Pap Smears for employees. It was a way for us to get acquainted with cancer of the cervix.

It was in our own shop, so we had a wonderful time. I would like to have phased that out. But, we got well acquanited with it; we also installed a laboratory that could examine the cilia of the bronchial epithelium. We could demonstrate what happened when cigarette smoke hit the cilia.

We could have demonstrated this in public schools. But, that didn't get done, because it was just too difficult administratively. The laboratory director, Weaver, had a drinking problem, so I got involved in all kinds of personality problems with him. Finally, I had to give up my idea of making a laboratory. But it can be done today, you know.

LESTER: He was the director of the Washington Cytology unit. right? And you wanted to go on from cervix to bronchial epithelium. You were held up because the administrator of the unit had a personality difficulty and drinking problem?

ROBBINS: Right.

LESTER: It was a "personnel accident" that it didn't get done back in 1957-58?

ROBBINS: Through '65. Here is why it would be good. Demonstration is better than exhortation. Here we are doing all this exhorting about lung cancer, but where are the demonstrations?

LESTER: Actually what would you do? Would you take epithelium through a bronchoscope?

ROBBINS: I would get calf bronchi and set up a model demonstration on how tobacco smoke affects our bronchial epithelium. It lives you know for 36 hours after it is taken out of a calf. So you can demonstrate what the cilia are doing.

LESTER: Why wasn't that done as a project or contract?

ROBBINS: Well, we had lots of dreams back there, but they (PHS directors) didn't think I had limitations. I couldn't do everything that I dreamed up. Now I planned it with several people, you know.

But once I was out of that, the Public Health Service teaches us to let go. I let go of cancer, I got out of it. I could have allergies of my own.

LESTER: What about the rest of the cervix?

ROBBINS: Rod Heller and I talked about the demonstration of Cancer Control. Where would we do a cervical cancer demonstration project? And, I heard a lecture by Tom McNealy on the ADC recipient. (Aid to Dependent Children) How effective the smear was. They (ADC recipients) couldn't be used. We speculated on their cervical cancer rates. They probably had (he didn't know at the time) about 2% carcinoma in situ. So we decided to do a Florida-wide ADC project. Involve the Florida State Health Department. Involve their cancer clinics. Involve everybody who saw ADC. In the first year we worked just in Dade country; we came up with 10% out of 5,500. We got 550 Pap Smears. In the second year, we got up to 40% participation. We went from Dade to Bower, to Palm Beach and all that. Dr. Derryberry, the health educator, finally said, "Why don't we use the proven method of behavioral science?" I said, "What have you been waiting for?" So we sent a team, a health educator and a nurse and the staff in Florida that were doing the smears.

We recommended that they bring together small groups and offer them tea and sandwiches and then give them a low-key presentation to cancer of the cervix. Everybody has had a sandwich and is feeling pretty good, and one asks another, "Why didn't you have one, Ziggy?" "Well, I was afraid that if I had my uterus out, I couldn't have my man." Somebody else said, "I had my uterus out and I'm having my man." They talked it over and we said that everytime someone would raise an objection, there would be an answer in that group. Participation went from 40% to 98% in some counties. But we couldn't always have a quality approach to group discussion.

The Florida ADC study taught us, besides the fact that 2% of them had cervical cancer, that group discussion is a very effective tool to get people to change. We published a book, and we tried to demonstrate in other states. But, it came too late in my administration.

It should, even today, be looked at to see if we really have been doing Pap Smears on all ADC recipients

We didn't know why cytology wasn't working, but it wasn't. We weren't getting people screened, it wasn't a good case-finding program. After a long series of false starts, I came up with 14 steps, each one of which called for a demonstration but each one could miss the cancer. For example, if you do the scraping, you may not get a good scraping. If you take a bit with a biopsy, you may not get the cancer. The cytologists might miss the cells. The pathologists might miss the place where there is cancer. By the time you finished, you had lost a hell of a lot of cancer. Our 14-step program was "a demonstration of the places where you lost cancer." We had 24 centers demonstrating it. . .perfecting screening, really.

DEVRA: You would indoctrinate them into the possibility of these 14 steps, or did you tell them about them?

ROBBINS: We outlined the 14 steps. Then we asked them to tell us what their experiences were.

DEVRA: Retrospectively?

ROBBINS: Both, prospectively and retrospectively.

DEVRA: You gave them grants or contracts to do this?

ROBBINS: There was a lot of money in it. It's my memory that we did 14 million Pap Smears. I could be wrong.

LESTER: What about the office program?

ROBBINS: Now, this is the most important thing we did in all of cancer control. The family doctors said, "You guys, you government people, are liars. There isn't a problem of family doctors going these. We do them on our patients."

So I said, what percent of the women in America do you suppose have had Pap Smears? Well, 40-50% they thought, but we were able to show them that only 4-5% had had smears by 1963. So, when the GP's heard that from the President of the College of American Pathologists, they said, "We'll help you." We had a goal of 100,000 Pap Smears. After they got their first 100,000, they wanted to go on. When they finished, they had 1,700,000 of them. That's when the program was cut by the government in 1969.

DEVRA: Was the cut in part because they felt that the technology had been demonstrated and it was now in wide usage, that government money was no longer needed, or were there other forces?

ROBBINS: It was very small at cost. The doctors didn't want anything for it.

I think it was because they wanted to cut Cancer Control. Put it in the Institute. And the Institute didn't want Cancer Control to continue. Now, you can see how biased I am. I don't know what their thinking was, because they were getting more for their money than anything else they were doing.

The SOS project: We began to get efforts to develop SOS programs (Self-Obtained Smears). I went along with them for a while. But cancer is so serious, that when you take it out of the judgment for a clinician, you will make all kinds of mistakes. A physician can take the responsibility for mistakes, but laymen can't. So I found SOS was not effective.

LESTER: You thought that that wasn't practical because the women weren't responsible in taking the smears?

ROBBINS: That's right.

DEVRA: There was too much chance of error, that they wouldn't actually get to the proper tissue?

ROBBINS: It appears that the errors are tremendous.

DEVRA: But you did demonstrate it?

ROBBINS: Yes, there were several studies that attempted to demonstrate it, Jack Frost at Johns Hopkins had one and there was another at Hopkins that was responsible for a lot of them. I don't think it's been answered today. I got my feeling, but...

LESTER: You mentioned Frost. My bit of history that Frost was one of the very young physicians for whom we provided (in California in 1947) training in cytology. He was just starting out his career.

We were trying to use that Cancer Control grant (state subvention) for constructive purposes. One of the things that we did was to train some pathologists and a few other physicians in cytology, because we were encountering so much resistance in the profession, that we couldn't make any headway.

We couldn't train technicians, because the pathologists said, "No, that's a medical diagnostic procedure and we can't let it out of our hands." The gynecologists were all for moving ahead. There was a group in San Diego that began in the late 40's, the gyn-ob clinic, which trained technicians to work under their direction. They didn't need pathologists. They were also physicians. We thought to overcome the resistance, the pathologists will take some of their bright young promising prople and give them training. Frost was one of those people.

ROBBINS: Isn't that interesting. He has turned out to be one of the leaders.

LESTER: We should put that in the history. In your mind, why did it take so long? From 1946, when Papanicolaou and Traut demonstrated what could be accomplished with the cytologic technique for cervical cancer, training programs were initiated at that time, then you came along in the late 1950s and early 60s and found that a relatively small percentage of the women in America had benefited from this technique, even though physicians felt, especially leaders of medical organizations, felt that they were doing pretty well. You tried some demonstration programs, carried some out in Florida and elsewhere, different approaches.

Here we are in 1975, about 30 years later, and we have the job pretty well along now, but by no means complete. Why does it take so long?

ROBBINS: Alright. Here I am without any restrictions.

I think it's conceited for us to say that a categorical program can mount an effort that will do people good. When we go out to the public with a cancer program, and don't listen to people with their heart problems and their cirrhosis problems, this is a conceited approach. It's an approach that has built-in opposition.

We ought to be going to people with a total problem. We should be concerned about their life expectancy. When you send a person to the doctor and the doctor has trained a girl at the front desk to listen very carefully for that legitimate complaint, the patient is sent away. The person might be told, "If you would like to come back in four or five months, we'll give you a physical." That patient is put in a category - the "non-sick" patient." We have two categories of people - the sick and non-sick or apparently well. My concern is that we have a bunch of concerted efforts - professions and services - that will say, "You only have one problem, and this is cancer." That's like saying, you only have one problem: either that you are sick or you are not sick. We've got to put these phases together.

People don't understand. They won't unless we tell them "we are interested in you, no matter what you have," that's a different approach. Doctors need to know about

ROBBINS: a person and what makes him vulnerable. How else will a physician know the natural history of his conditions in order to make a prognosis? Without a prognosis, he can't prescribe treatment and intervention.

We have to advance "patient advantage." Are we treating disease or are we only looking for a stage of the precursor disease? We still use advancing patient advantage. The word "means" can be applied to sick people as well as well people.

DEVRA: Did you develop this philosophy at the time you were directing the Cancer Control Program? Did that help you to formulate your interest in health hazard appraisal and then prospective medicine. Which embodies this philosophy?

ROBBINS: Going back to Les' question about why it takes so long to demonstrate something simple like the Pap Smear, I'd say because we are a nation of specialists. But when it comes to applying this specialty knowledge to the individual, this may happen or it may not. It is up to chance encounters. The generalist is the individual, the patient, the client. He must find some way to have this benefit of the specialties in preventive medicine. Some specialists are of such great importance that they must be applied at every level of involvement. The patient must be trained. He must be educated at an early age. We spend too much time in health education on making health education available to people but not helping them to apply it to themselves. Our program of HHA will help people to learn where the big risks are and where the biggest pieces of survival advantage can be found.

Now I've got some words to explain it.

In 1949, I was in Boston. We were fighting David Rutstein and fighting everybody you know on coronary heart disease. I have already been in cancer. I had already been in public health. I had seen us lick disease after disease after disease. I started writing a paper called "Between Visits, Whose Responsibility?".

Here is the doctor who looks at the patient and his life cycle. He records one episode; maybe 3 or 4 years later, he will see him during another episode. Is he wondering what's going on between times, in fact what's going on when that patient comes back to that doctor?

Years ago, a 20-year-old man went to a well trained family physician here in Broad Ripple, Indiana. He had a lesion on his face removed. After he left, he was later found to have high blood pressure, 190 (I haven't heard the end of that story.) Twenty years old with high blood pressure of 190! And his blood pressure was not taken in this office of a well trained family doctor.

How does all of medicine get to the individual? I think we are conceited, maybe it's stupid, to think that we can work out a person's whole health with a "cancer program," or a "heart program," or whatever. Let me show you the natural history of a disease going from birth to death. Somewhere, there is the onset of disease. There could be signs present that only the doctor can see. At that point, symptoms begin. You can have early and late symptoms. Farther in life, disability sets in. At each phase, we can sometimes reverse it. Not always. That's the bad part. But does that mean there is nothing going on back here? Why, of course not.

We have got a precursor or risk factor in almost every disease. That precursor has stages, like blood pressure, progressing from 140 to 160 to 180 to 200. Even before the precursor, there is a time of vulnerability to a precursor -- or this high blood pressure.

ROBBINS: If you are in a cigarette smoking society, you are vulnerable; if you are riding in automobiles, you are vulnerable- if you are old enough, you are vulnerable to coronary heart disease, although you may not have a precursor. Even before, there is a time when there is just no risk at all.

I think children under six are at no risk of smoking. The minute they get to be 6 or 8, they are in the vulnerable population. Now, where does Les Breslow's means get applied here? It can be applied anywhere and along the life course.

LESTER: Exactly. But it ought to be today. We ought to, if the investigator said, just give me ten more years and I'll have a study. Especially with what we know today, because that individual might not be able to wait ten years. Everything is, ah, well. Would you discuss prudence?

ROBBINS: Yes, this is a different approach. We identify a diagnostic means, for example, the Pap Smear. It's the means that leads to the diagnosis of cancer. In 1957, I took what we knew about the Pap Smear and I visited 30 practictioners who had been on my advisory committees as a local health officer. I asked each one what he thought of the Pap Smear. Well, there was a great gamut of answers. Some of them weren't doing Pap Smears, some were. One man even asked one, "What is a Pap Smear?" This was in 1957.

If you ask yourself how did each of these men arrive at what he does? It's "teaching quality." Somebody had to teach him. You know what we do about teaching quality, if only a precursor is present? We do very little.

(I am going to make this wild statement.) You can go into almost any place in Indiana, and you won't find anyone (physicians) who can answer questions about cigarette smoking and lung cancer. We just don't have anybody that knows the data. We don't have people who know diagnosis, staging, prognosis and treatment of the precursor, cigarette smoking and lung cancer. Do they know about diagnosis of lung cancer itself? Of course. We can go to the Methodist Hospital, and doctors can give you information about lung cancer until you don't want anymore. But where do you find it?

The American Cancer Society--all of their oncologists--their consciences were clear when they made the statement associating cigarette smoking and lung cancer. The ACS had put people in the community that know this well enough. So when a doctors says, what do we really know about cigarette smoking and lung cancer, the ACS has the data.

We have great errors today in medical schools and medical practices, and in doctors' families, complete ignorance about preventive medicine.

LESTER: Incidently, I would like to have a copy of the last ten minutes of the tape for use in a course I am organizing in preventive medicine for medical students.

ROBBINS: It's really only a draft. I have been writing about this for about three years. That's the present effort to answer your question.

LESTER: You mentioned your own career. You got into Cancer Control first in 1946, when the Public Health Service first had some interest and the Congress was pushing, some funds were made available for Cancer Control. You entered the PHS for a brief period as a regional consultant, a Cancer Control Program Organizer. Then you came back in 1957, when as you said you "saluted and took the assignment". You were "just a public health service officer" doing it for the good of the service? Lee Burney was Surgeon-General. Let's move on to discuss what you did about cancer of the colon and we'll come back. . .What happened at the end of '65? How did your part in the Cancer Control Program come to an end? What were the circumstances?

ROBBINS: When I took Cancer Control, it was only a means of getting round to the Health Hazard Appraisal concept. I had money. I could start mounting this effort. The Surgeon-General came in and asked me, "How are you going to integrate Cancer Control into medical practice?"

LESTER: We began to talk about the circumstances that lead to 1965 when you left the program. You said you took on Cancer Control because it was a simple way of getting into the whole issue of health hazard appraisal, advancing the advantage of the patient. When you were in Cancer Control you had money and you could spend it to demonstrate some of these things.

ROBBINS: I could finally see that I wasn't really getting anywhere. Toward this goal of health hazard appraisal. I was, to me, wasting time, categorically.

Beginning early in 1964, I began asking to be relieved. I said, "have you got somebody, because I want to get out of here, because I want to start working on Health Hazard Appraisal."

By then I had enough seniority that I didn't have to salute everybody. I could pick out those that would agree with me.

They gave to me a chance to be a special consultant on Health Hazard Appraisal. They set me up with an office. I had a secretary, I started a program. They (PHS) would have let me do both, Cancer Control and Health Hazards. But that is not the way to be a health officer. You have to be a full-time health officer. I knew what a full-time Cancer Control man could do in this thing that I wanted to do. So now (1965), I am full-time.

When I was in the Local Health Service Division of the State Health Department, I found that the part-time health officer seldom accomplished much in the way of program development. He would handle problems as they came up, but mounting a program that took training and logistics wasn't possible. He would always have to run off to see a sick patient. And the sick patient always had priority over the preventive program.

Here, again, is that matter of responsibility. To make public health practice decisions of any quality at all, we needed to have health officers who had continuing responsibilities. After they had made a decision, hold them accountable for the follow-up. Preventive medicine needs to have roles with continuing responsibilities. And our target can no longer be the population group, but must be directed to the individual in the population. Not only deaths in the population as a problem, but risks in the individual as a problem.

We had already started working with George Washington University. We have a program going in Health Hazard Appraisal. It hadn't been written up, but it was going. Then I began working on Jefferson Medical College and the Methodist Hospital. And because Jack Hall, when I came out to talk with Bob Egan, had asked me what we were doing, I told him. I took off from that Cancer Control work and began finding some people here in Indianapolis who were interested.

LESTER: So, you left the Cancer Control effort because you were anxious to move into something broader, not categorical health hazard appraisals but general. What happened to Cancer Control? Who succeeded you?

ROBBINS: We had a year before then to pick the guy. Bill Ross was available. He was certainly a well-trained officer. So he was brought in from Chicago PHS Office. I had overlapped with him. But he had a period of time when he was able to begin learning about Health Hazard Appraisal. He had about six months.

DEVRA: Did you pick him or was he picked by others?

ROBBINS: He was picked by Gene Guthrie, Chief of the Division of Chronic Diseases.

LESTER: This was in the Bureau of State Services? Then what happened?

ROBBINS: When I stepped out, I just told them I was through with Cancer Control, and let go.

LESTER: In your opinion, what happened. You weren't in it directly but you must have been aware of what was happening. What did happen?

ROBBINS: Well, I never saw a guy so mixed-up as Bill Ross turned out to be. His life was a succession of problems (personal). He wouldn't spend full-time on the job. I have to say, he was a great disappointment.

LESTER: Why did the superior officers keep him there and allow this to continue?

ROBBINS: You will have to ask them, because I wasn't talking to anybody about cancer. That was their job. I had something that took all I had and then some. Look how slowly it's moved.

DEVRA: Had you really been thinking about Health Hazard Appraisal and this whole philosophy even before you were picked to do the Cancer Control Program?

ROBBINS: Since 1949.

LESTER: Those were the days when Chapman and some of the rest of us were into some multiphasic screening.

ROBBINS: I had a little experience of multiphasic screening in 1948. I knew that that was not the direction. Now, if it's combined with a program that provides people with information on their high risks and how to reduce them, that's worthwhile. If you are just doing multiphasic screening to find existing disease when there are no symptoms, it is a waste of money.

LESTER: From 1949 to '65. What would you say were the truly key events, if you were to pick three to five or so? Contributions scientifically, program developments, or personnel assignments or whatever?

ROBBINS: Getting Rod Heller to come to the National Cancer Institute was one.

LESTER: He had been in VD work before that time.

DEVRA: He was a public health man?

ROBBINS: He had been in VD. That was his most important assignment. He had been in VD for a long time. Then, second, I say this from the tremendous feeling that I had when I heard that Ravenna, Ohio, physician after I had been sensitized to the problem of breast cancer. Hearing that Ravenna physician tell me that he could do mammography as well as Bob Egan, well that gave me the biggest charge that I ever got in cancer.

The third event was when the family doctors (Academy of General Practice) agreed to take this program and do Pap Smears in their own offices. This was putting us in touch. We had 42 states that were doing Pap Smears in doctor's offices, 5,000 doctors.

DEVRA: You didn't have to go to a special center to get it. You could get it from your family physician?

ROBBINS: Yes, Devra.

LESTER: What were the budgeting problems and processes that favored the program? What didn't favor it? What was the attitude of the Congress? Of the Administration of those immediately above you in the program?

ROBBINS: I felt that they were giving us much money as we could spend wisely. We had all that we needed, then.

DEVRA: Do you have any rough estimate in your own mind as to how much money you had; let's say, in 1958 compared to what you had requested, and how much money you could have spent in 1965?

ROBBINS: We always start poor, you know. I didn't have enough money until about February of 1958 to be sure that I had a secretary, I would get little dabs of secretaries but then I got a secretary, then an administrative assistant. By the end of that year, I had, let's say, $500,000. It went right on up, not counting the $3,000,000 going to state health officers. (State Health Departments) In 1965, I had $12,000,000. You know, that's about all that I could have spent well. If they gave me more, I could have spent it, but I don't know how wisely.

LESTER: What about your relationship with the State, during that period?

ROBBINS: Being an old State Health man myself, I was able to relate to them very easily, but it just looked as if the State Health Department had lost his mission. He didn't see how he could really gain great successes with cancer like he had before. He couldn't see why it was worth the effort. He is always looking for those tremendous successes, or he was at that time. But tremendous successes we had in the 30s, 40s and 50s.

DEVRA: But not with chronic diseases?

ROBBINS: It didn't come with chronic disease. They weren't willing to adjust their thinking. Take one problem at a time. One precursor doesn't give you much help on understanding one another. You got to know one precursor's will. This has happened to me in my public health career 30 times on the national level and 30 times on a local level.

There were others that taught us, but I remember the scientists especially fighting us on Mammography. They fought Mike Shimkin in that HIP study.

LESTER: Why did they fight it?

ROBBINS: Well, I guess maybe they felt it was their job to be his majesty's loyal opposition.

DEVRA: They were researchers. Do you think they wanted to control the bucks that went, not just for the original research but all the way through pilot testing, field testing, widespread community testing, which was something they had absolutely no knowledge about how to do? They wanted control's budget?

ROBBINS: I am sure this is it. Yes, it takes a lot of people to provide the checks and balances.

DEVRA: One of the things that is very intrigueing now, of course, that the Cancer Control Program is back in the National Cancer Institute. . .

LESTER: When did it go back?

DEVRA: It went back by the mandate by Congress in 1971 that it was to be a component of the National Cancer Program.

LESTER: In 1946, it was a part of the National Cancer Institute and remained there until 1957-58, when you came into the program. At that time, the decision was made by the Surgeon-General to move it from the Cancer Institute into the Bureau of State Services. You kept it there until 1965 and it remained there after you left for five or six years. . .

DEVRA: No. It was in limbo in the late 60s, because when Regional Medical Programs was first a part of NIH, Cancer Control was put back into RMP.

ROBBINS: What year was it?

DEVRA: 1967-68. Then RMP was moved to HSMAH, and Cancer Control or whatever was left of it went with RMP.

LESTER: Then in 1971, with the National Cancer Act of 1971, Cancer Control was re-established as. . .

DEVRA: A responsibility of the National Cancer Institute.

LESTER: That's where it is at the present time.

DEVRA: I am struck by something else you commented on, that so much of the effectiveness of the Cancer Control Program during your period and questionable effectiveness during the subsequent period, really had to do with the personalities and convictions of the people running the program.

ROBBINS: Oh, always. But if you happened to be an honest man, like Shimkin, and you are in the National Cancer Institute, they will let you do research, but they won't let you do control. Mike is such a complex character. He moves easily from control to investigation. It's very difficult to do that.

DEVRA: Was it his responsibility in the period 1946?

ROBBINS: No.

DEVRA: Earlier?

ROBBINS: No. He had one of the divisions in the Cancer Institute. It was Ray Kaiser.

DEVRA: For field investigations, is that what it was called?

ROBBINS: He has been very supportive of Cancer Control, but he is still an "investigator."

LESTER: He (Shimkin) is one of the few investigators that has seen the light of Cancer Control. They still call on him to come to the Cancer Control conferences because he carries the prestige of an investigator.

ROBBINS: Right.

LESTER: Although he understands Cancer Control.

DEVRA: So many of the investigators that are so clinically oriented, or basic science oriented, and don't have a public health outlook, don't really grasp how you can take a technological development and spread it rapidly to masses of people. For example, immunotherapy is the current example. That is still being controlled and I think perhaps in the foreseeable future will be controlled by research investigators, because it is still in the frame-work of research. But aren't following people treated with immunotherapy long enough yet to know whether it is going to have some value for masses of people. It occurs to that mass application was one of the problems that Dr. Papanicolaou was having. Were there times during your period as chief of the Cancer Control program when you would get really discouraged? What were some of the elements of discouragement?

ROBBINS: Between Hayakawa, and the public health service, and experience in a number of disease control programs, I know you can't win every game. Some do move faster than others. So I can move from something that is frustrating to something that isn't.

If there is any wealth in this world, it's the wealth of the alternatives. If you got alternatives you got everything. The Public Health Service has arranged to give you alternatives, and that old Commission Corps, boy, when that went down the drain, it really cost us.

DEVRA: Well, your departure from the Cancer Control Program was about at the same time as another reshuffling of the Public Health Service. Was that disorganization or constant change in organization having an impact on the viability of the Cancer Control Program?

ROBBINS: You know, I believe that if we had somebody other than Bill Ross, that wasn't so mixed up, or if he had the drive to continue it. I think I would have gone along with it.

DEVRA: Regardless of what was happening the Public Health Service?

ROBBINS: When you see somebody stumble, fumble and fall like Bill Ross, it weakens everybody in the staff. Bill Ross was well-meaning, amiable, and well trained. I'm so sorry.

DEVRA: Well, he has managed to stay in government service until about three months ago.

ROBBINS: What happened?

DEVRA: He went through a whole series of cancer-related staff positions, mainly in the National Cancer Institute. I don't have all the titles memorized. But he staffed a bladder cancer project and then he was involved in something called, special projects in the extramural division which sounds like a place where they put people out to pasture. Then I understand he was asked to leave by Dr. Rauscher. He retired earlier this year. He supposedly still lives in Bethesda. It's sad to think that as significant as cancer control even though it is categorical has some limitations, could in fact not survive the vagaries or individuals running it. It indicates something to us about the history of a program like this.

LESTER: Really quite senseless until you get it more institutionalized. A place like Roswell Park for example, has gone on through changes in governors, administrators, leaders and staff, but the Federal Cancer Control program has had one goal in 57 to 65, it had some little twinges prior to that time, and then it went into a demise and was absorbed into the Regional Medical Programs. (RMP did quite a bit in Cancer Control. It wasn't called that specifically.) Then, the National Cancer Act has revived it in a somewhat different form. Skipping the Ross and the RMP periods, do you have any comments on the current phase of cancer control? 1972 forward - Not so much the personalities, but whatever comments you would like to relate about the program situation, hopes, etc.

ROBBINS: I am not close enough to it to have a good opinion.

LESTER: Well, for one thing, it is based in the National Cancer Institute. What would you expect of that?

ROBBINS: Everything I have said earlier about the Cancer Institute could apply today, because they are still "investigation-oriented." If you don't believe it, just listen to any group discussion, any staff discussion. What are they talking about? What are their hopes?

LESTER: Would you take the Cancer Control program out of the National Cancer Institute?

ROBBINS: I don't think it ever should have gone into it. I don't think it will ever--now understand, I am not speaking from knowledge--I am just saying that that poor girl, Diane Fink, anything that she gets is hard won. She has earned it. Anything she gets is hard-won, and the program will never really take off until they carefully plan to put in a control agency.

DEVRA: But, we don't have the stamina of the Public Health Service anymore, so that's not a viable alternative. Are there other control agencies?

LESTER: Within HEW? Well, there are programs that are aimed at disease control, but not in the National Cancer Institute. There are various methods to deal with this problem. In mental health, of course, was removed from the National Institutes of Health. A new kind of an approach was created, which would, some people thought, combine the investigation and the control aspects in a single agency, the National Institute of Mental Health. They did try to bridge the gap between research and control elements. I am not sure how that was worked. I haven't been close ot it, but your views on the matter are then quite clear, Robbie.

How about a person like Rod Heller, whom you admired so much. He was the director of the Cancer Institute when you took the program out of there.

ROBBINS: When Rod brought me into the staff, he would let me discuss and then he would throw in ideas here and there. He was a tremendous leader. That leadership makes a man fight, you know. But he had the problem that they have today in the Institute. Which one of these research programs shall I promote? Which one of these means shall I push.

If you push one, you make some enemies, if you push another you have made others. You just can't win. Because there are all kinds of special interests, like Zubrod, Berlin, who will fight you. They're thinking something different, not control, you know. They are not trying to get present knowledge to people. They are thinking about what do we really know. Well, there's prudence. If I am a practitioner, I got to apply what we know today. I can't say, "Go away come back in ten years."

LESTER: For a man who's been involved in setting up in these 27 breast cancer detection programs (Berlin). . .

DEVRA: And its precursor, because they grew out of something called the Breast Cancer Task Force.

LESTER: You have talked with thim. How do you interpret this, Devra?

DEVRA: I think actually you are correct in your assessment. Berlin (I can't speak for Gordon Zubrod because I haven't interviewed him yet) and Jim Peters, and Palmer Saunders were apparently the chiefs of the four divisions which were going to be involved when the National Cancer Plan mandated Cancer Control be brought back into the Institute. They were somewhat dumbfounded, obviously, because they knew it was not research. They also knew their philosophy of being able to control the research dollars all the way through field testing was going to be intimidated. They were going to lose some dollars somewhere on that spectrum. I don't know, really how much of a personal dilemma it was for each one of them.

In Dr. Berlin's case, I think it was one of the agents that made him decide to leave the Cancer Institute. He is now the Director of the Cancer Center at Northwestern University, Chicago. He has been there for about four months. Zubrod also left; Saunders has retired.

These may just have happened to be coincidental, but I think those men may have had a mixed loyalty problem. They were very loyal to Dr. Rauscher and wanted to see him do the best job, given the constraints Congress had mandated. They knew they weren't going to get those money assigned to control back into their bailiwicks. What could they do as an alternative? What some of them did was to leave. That was a personal decision I think for some of them.

ROBBINS: Well, we all have skills that don't fit. I think a guy has to fall back and re-group every night. What is true today will change tomorrow. What's true for me today and for Margaret (Mrs. Robbins) today, may change for her tomorrow, but not for me, but our decision will be based on both of us. So you just have to continually fall back and re-group.

Dr. Hayakawa (semanticist) tells you how to speak so that you are understood by yourself and others. I know how biased I am. But I will fall back at night and I'll write down what happened. That thing will give you alternatives the next day.

DEVRA: Do you still maintain logs?

ROBBINS: Oh yes, I write them everyday.

LESTER: Do you write them in long-hand or do you dictate them?

ROBBINS: Right now I am typing them myself.

LESTER: But in those days when you kept these books (logs), how did you do it?

ROBBINS: I dictated them and my secretary typed them. If I were on a trip, I took the typewriter and I typed them.

LESTER: Would it be possible . . .We have the days of some critical events, but would it be possible, it would be terribly important for our archives if you would be willing, we would pay for making a Xerox of the whole set. We would ask you, just to see whatever you could do here in Indianapolis. I don't mean you should send them. Just, you take them and you get them copied and get a bill and send us the Xerox copy and the bill or shipping and everything.

ROBBINS: In a log I was no more frank than I would be sharing ideas with my immediate staff. Often I would have to protect a member of the staff, but I would still say it in a log. I found a way to put every significant development in the log.

LESTER: You just wouldn't identify the person?

ROBBINS: That's right.

DEVRA: Did the staff have copies of these logs?

ROBBINS: I gave copies to them, each member. If Joseph Delapointe had several people, I would give him one copy and he could share it.

DEVRA: Did they get log reports daily?

ROBBINS: Daily. (You talk with Bill Melton and he'll tell you.)

DEVRA: Did Judy Silsbee type them?

ROBBINS: No, she wasn't my secretary. She had a very special position. That girl is a brilliant girl. She wrote these reports that we presented to the National Advisory Cancer Council. She wrote them. Now I corrected them, I gave her the material to write them, but they were very well written.

LESTER: Can we get back to one of these paragraphs that we skipped over I am afraid. Let's run through quickly because we don't want to take too much more time, this second paragraph, headed non-public health service roles. Can you spend a minute or two on each of those. The Voluntary and Professional Sectors.

ROBBINS: The American Cancer Society, I was on their board for a while. That is such an important agency. When I came they had already been trying very hard to get the Surgeon-General to do something about cigarette smoking and lung cancer. He wasn't doing anything. The Cancer Society was working with the Heart Association and the TB Association and the National Health Council to do something about smoking, and the Surgeon-General was being quiet.

When I came in as chief of Cancer Control, I found that I couldn't get the Public Health Service to go as far as the voluntary agency was going. Now the ACS is working hard and they continue to work hard. But it is categorical, they just worry about cancer. That leaves in nicks for fragmentation and fragmentation causes problems.

The great man was Dr. Harold Diehl. He was a tremendous leader. I don't know anybody else that's really affected the Cancer Society like he has. They are a bunch of guys whose money comes from the United States but from sources where they can get it. They will go to Victor Weingarten and ask him to collect money and give him a part of it. If they go to their own staff for money, like here in Indiana, why a man's tenure depends on his increasing each year the amount that he collects. If he only collects 10% he is fired. If he collects 15% or more each year, they will keep him. Now, if he collects 20-25% more than he did the year before, he's on his way up in the organizations.

ROBBINS: Here is a problem of all voluntary agencies. How do you keep the program solvent? The field representatives of the ACS are fund-raisers, and promote the work of ACS in research and education along with the fund raising. That money for research comes from the efforts of paid staff. I have no fault to find.

Now what does he know about cigarettes and lung cancer and the means? Nothing. Who does? Nobody. Not in this state. Go ask the State Health Department. They don't know about the precursor.

What does the Cancer Society do? Well, to me some of the work they do is great, and some of it is of benefit to the salaried.

LESTER: How about the American College of Surgeons?

ROBBINS: Ah, the College of Surgeons would like to fight the College of Radiology for position. They want to run Cancer Control. They have the cancer committee of the American College of Surgeons and they have been doing great work. Bowman Crowell back in 1935-45, who ran the cancer clinics was working in the office of the College of Surgeons and paid by them. The College of Surgeons and their cancer committee are very friendly to cancer control. How much it affects the treatment I can't guarantee, because sometimes I think that they would use their position to gain advantage in the control of cancer.

The American College of Radiology: Their schizophrenia is whether they are diagnostic or therapeutic. These two things are pulling against each other all the time. They took that mammography teaching kit that we developed, and they took Bill Melton, and they took off on a teaching program in many areas of radiology. Which was good.

The character of that organization changed. They are moving very rapidly. They are now able to challenge the College of Surgeons in a lot of problems.

The American Academy of General Practice. Now, I saw them go from a bunch of boys that were led by Cahill to really sophisticated men that could run their own organization. They were very naive when Cahill took them over. He could do anything he wanted to. One time I saw him run a meeting in which the room was darkened, a flood light was shown on an American flag, and Cahill gave the kind of corny pitch that would exhalt the Academy. I had never seen anything so blatant and overt. They loved it. [The College of Surgeons was going to run him out of town.]

I want to say one more thing about the American Academy of General Practice. They developed a lot of skills in group discussion, in verbalizing their decisions. They used some behavioral scientists that we helped them with. We had a series of films called "Shop Talk." Our idea was to get them talking about Cancer Control in the office. This would reveal their problems. I saw Alabama go from 2% Pap Smear to 4% in one year. Purely, I thought, on the basis on their group discussion.

The College of American Pathology. Well, this bunch of guys used to be at the cross-roads. They are the smartest guys. They just don't have anything to do except sit around and think. So, Dallas Johnson worked for them for a long time, and they fired her. I was surprised that she could work with them as long as she did. They are very able people, but they are motivated by the dollar. It is so important to them, it's their way of staying in business, to attract membership, to get residencies. Everything depends on that dollar.

ROBBINS: When I first went to Roswell Park, I asked Dr. Tibideaux, the pathologist, what he thought of the Pap Smear. "No good, you can't make a diagnosis on one cell. That's impossible." So, the gynecologists bought microscopes and got their own training and they started diagnosing cancer of the cervix and they began making money at it. The pathologists, one by one, came to them and said, "Hey, that's pathology," and the gynecologists, one by one, would say "By my guest." They were glad to get rid of the job. So the College of American Pathology moved in to cytology. They started moving in about 1949, had board exams in 1952. They weren't really smart. It took them too long to get into it.

DEVRA: What about the Commission for Cancer Staging and End Results, I think that's what they now call the American Joint Committee?

ROBBINS: The American Joint Committee for Cancer Staging and End Results - they are a bunch of theorists who love nothing better than to come up with the perfect staging. They are trying to get perfect staging, but they'll never get it. They need a program that is not acceptable to the people who diagnose and stage cancer.

One of the reasons I wouldn't take a full-time job with the Committee was that they don't recognize anything unless it is metastisizing cancer. Except for cancer of the cervix. They will accept in situ phase. Now, there are pressures to make them except carcinoma in situ of the breast.

DEVRA: So they are not precursor-conscious. They are only biopsy-forward conscious.

ROBBINS: Yes. They are getting so much money. I don't know what you do about it.

DEVRA: You said you were offered the opportunity to be the secretary for the American Joint Committee.

ROBBINS: Right.

DEVRA: How long ago was that?

ROBBINS: About two years ago. I was fascinated. I wanted to combine it with my efforts in Health Hazard Appraisal. When I learned that they didn't care about the precursor, why I didn't care about them.

LESTER: How about the UICC?

ROBBINS: I was chairman of their cancer detection committee for several years. I took the team to Tokyo and we had a panel discussion on cancer detection. Before that I presented the first report on mammography and at the Moscow conference. We got good coverage on that.

I was able to get to the Florence Congress (1975) with Margaret. Who should start talking but the National Cancer Institute director of Canada, Miller. Miller was telling us that he questioned very seriously whether we ought to do Pap Smears. Here they had been so well established. I couldn't understand why, now, they were abandoning it in Canada. Well, it's lack of prudence again. If it's your own life, yes, but if it's a matter of dollars what direction to spend your dollars? They are afraid that it costs too much. Why, I see lots of special interests in this program. (The Public Health Cancer Association is something you (Les) got an award from not once but several times. We tried several times to revive that group.)

DEVRA: When did it get killed off the last time?

ROBBINS: After I left cancer control (after '65). Mike Shimkin was the last guy I heard who was trying to revive it. I think it has a very important place. The name might not be right, but what name is right? Is there a right name for that organization. I think it ought to be revived.

LESTER: Now, let's talk about your work on cancer of the colon and rectum.

ROBBINS: When I came in 1957, everybody was recommending procto-sigmoidoscopy to find cancers of the colon and rectum. But after you have had one procto-sigmoid exam, the chances of getting the second one are poor [about as good as the speed that you can run.] So, we had a hiatus there. Howard Gowen came up with the suggestion, why don't we get a fiberoptic manufacturer to make a flexible procto-sig.

LESTER: This was when?

ROBBINS: This was 1958. Then Howard Gowen left in 1960. But his idea was so sound. I began pulling in the fiberoptic companies. I got hold of Marvin Pollard. He agreed to take a young service officer, (Bergein F. Overholt (we were able to defer this guy from the draft), and keep him in Pollard's office while he was working on it. We wanted a 25 to maybe 50 centimer instrument that any family doctor could use. Well, it didn't go in that direction. The one he developed went to six feet, not 25 inches, but 6 feet; not 11 inches, but 6 times as far. Pollard and Overholt. Today there is a colonoscopist in every large city. I even found two in Idaho, in small communities, and they are busy. They are as busy as they want to be. First, before surgery, you do a colonoscopy.

So, this is established, but I have followed this so I think I can give you a recommendation. Today we ought to say, you can have a procto-sig if you like, but we recommend a hemoccult test. You'll go on a no-meat diet for 2 days then take three consecutive stools. If your hemoccult test is negative on three stools, you don't worry. If it is positive, and it is positive in about 10%, then you got a problem. I would begin with a procto-sig, not colonoscopy, because it costs you $150-$200.

LESTER: Why can't they use the fiberoptics for the 11-inch examination, rather than six feet?

ROBBINS: Well, that's what I wondered. It went in the direction that would give the greatest aid to the profession, the gastroenterologist and the surgeon now. Because of the use of electrocoagulation, others frown on its use by gastroenterologists. You ought to be based in electrocoagulation, so you don't make a lot of stupid mistakes. It's a skill.

DEVRA: Could we draw some parallel, then? Could we be training people? More physicians in the proper use of this early detection mechanism. Just the way you did with mammography? You developed teaching sets and teaching programs and held annual conferences and used a lot of other mechanisms.

ROBBINS: I see them going on without any encouragement.

LESTER: Correct me if I'm wrong. Because of the unacceptability of the people over repeated procto-sigmoidoscopy (I wouldn't have one unless I had a a diagnostic problem where it was exceptional) for screening, you initiated with the fiberoptic people the development of another device that would be flexible and not have the pain and trauma connnected with procto-sigmoidoscopy. You thought it could be

LESTER: developed as a screening instrument for colon and rectum cancer in the lower end of the bowel, the first foot or so. But instead, under the stimulus of the profession, the development of the device went in the direction of a diagnostic instrument for the entire colon down to the cecum, a half dozen feet rather than a dozen inches. This could obviously not be applied as a screening technique. Nobody is going to have this thing treaded through the whole colon except for a diagnostic purpose. Therefore, the original intent of a screening instrument was lost because of the professional preoccupation with diagnosis rather than with screening. That the opportunity still exists, you would say, in developing a shorter instrument with a range of about 12 inches rather than six feet, for those cancer that can be screened and found early and then something done about them.

ROBBINS: The standard proctosigmoidoscope which goes to about 25 centimers or 11 inches or so, only finds about 60-65 percent of the colon-rectum cancers. If we could go to 50 centimeters, the take might get up to 80-85%. We were hoping to develop a 50-centimeter proctosig, which would have been more desirable from an early detection standpoint. This would take one thing: money and the willingness of a program oriented to control. With a continuing responsibility to control.

That's right.

LESTER: Most colon-rectum cancers you can do something about are in that first foot. You cannot justify putting a fiberoptic instrument into everybody's cecum, periodically, but you could justify doing the first foot periodically, even in the hands of a family doctor. That is relatively simple. The yield is much greater, so you can use it for screening. The other one you cannot use for screening, it's just not feasible.

DEVRA: By the time you do use it for diagnosis it's already too late?

LESTER: That's right. By the time you justify using the diagnostic instrument, you have lost a great deal. So the practical outcome is screening in the first foot.

(FINALE)

LESTER: Even though you have been out of Cancer Control for ten years, you have obviously kept your interest in it. You have a historical perspective. If you were to advise the Congress or the Surgeon-General or the Secretary of HEW or whoever, as to the directions Cancer Control should take in the next 5, 10 or 25 years, and the directions it should avoid, what would you say?

ROBBINS: I would say that it is possible to put a Cancer Control Program in operation, but it would have to be out of the Institute. It would be the kind that would work on a level that took "what means we had at hand and applied them today."

The battle of what is prudent is being taken care of by every health agency. The American Cancer Society will tell us what's prudent about cigarettes and lung cancer. The Heart Association will tell us what's prudent about cholesterol and when it is prudent. The Rosenman-Freedman appraoch to measure stress. (But not yet. Can you trust doctors and people in Indianapolis to tell a man that he is in type A behavior and that he has got to do this or that, change his life. Of course not. It's way too soon. But it will come. The Heart Association someday will take a position on stress.)

DEVRA: You seem quite convinced that Cancer Control isn't going to reach people broadly if it's based in the Institute. Do you think it belongs in a public health agency?

ROBBINS: There are a lot of public health people that can be tuned up to do it.

DEVRA: What about the credibility with practitioners? Do you think that it can be sustained best by a public health agency? Especially now that the Public Health Service, and even county health departments have lost a great deal of prestige. I think it would be, even now, a super-human effort, might kill Cancer Control totally, if, for example, it were extricated from where the Institute and put into an unfamiliar agency. I think that would be the only apprehension I would have. If we had a revived Public Health Service of the type we had operating in the 40s and 50s, maybe it would be feasible.

ROBBINS: That wasn't brick and mortar, that was a commissioned corps. It was built up over years and years. I knew the kind of performance I had to provide in order to stay or to get advancement. I had to have all kinds of training first and so my deferred gratifications were very great.

But it had developed in '57. I was really ready for that program. I think I was at the right age. Everything was just right for me. To me it was a great break to get that job. Even if I was scared to death to take it. One of the turning points was after I had spent three weeks with these old friends in the medical profession in Wichita Falls, Texas, and in San Antonio and Boston, Massachusetts, and Newton and Bloomington, Indiana, I came back and talked to Dr. Heller and a little group that had heard about my findings. That was the turning point. Because they recognized instantly that I had been there before and now I knew it again. I knew the way to go. They would permit me to work there. That was really the first time I knew I had it made.

DEVRA: Beyond the logs which we are now going to arrange to have copies and sent to us, can you tell us where annual reports of your program to your advisory committee might be? Do you know where we might be able to get annual reports prior to 1962? Do you think Dave Wood would have them in his file?

ROBBINS: He would be very likely to have them. I presented them to NACC, National Advisory Cancer Council. Maybe they have them. Then Judy Silsbee wrote them. so have you talked with Judy yet?

DEVRA: Not yet, we wanted to talk with you first. Everyone we talked to in your period, like Bill Melton said I don't want to talk with you until you have talked with Robby. Judy is still around. She stuck it out through RMP, I'll talk with her. The other kinds of things that we were looking for are the following: Is it possible that there were any program plans? Of course, we may find them in the logs, which were proposed, but either never evolved into programs or they were radically changed. Is that possible?

ROBBINS: They will be in my log. You heard me wrestling with that little idea of having a demonstration program on the cilia of the bronchioepithelium. That's very traumatic, you know those cilia are waving away all the time, 10 times a second, moving the junk out. When you put in tobacco smoke, well, they go fump, the cilia are stunted, they are not working as fast.

DEVRA: That was proposed and never got off the ground?

ROBBINS: Well, I have to blame myself.

DEVRA: Do you or Judy have copies of testimony you may have given for congressional committees?

ROBBINS: I didn't do my testifying. That was done by the divisional chiefs, Chapman and Guthrie.

LESTER: In thinking about your career, particularly from that period of '57 to '65, would you say that the major contributions were these, your personal contributions?

1) One was to establish and maintain for that period an excellent staff from which came the Cancer Control Program that pushed things along in the late 50s and early 60s.

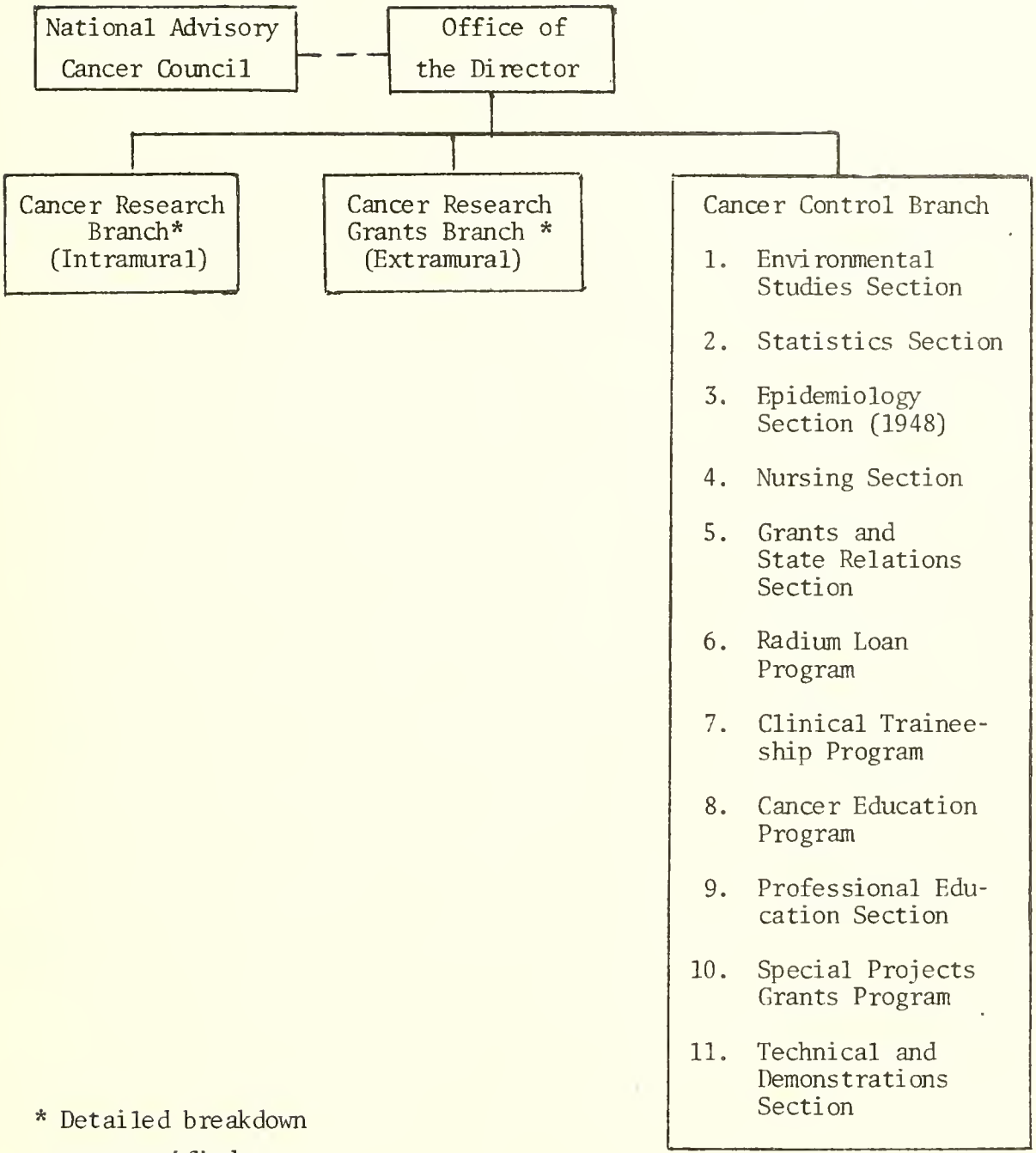
2) Second, the mammography reproducibility study.

3) Surgeon-General's statement on cigarette smoking in JAMA in 1959. Were there others that you would classify with these three?

ROBBINS: The working relationship with the medical profession, especially that Academy of General Practice project. It got to 1,700,000 Pap Smears. The Florida ADC project, because that used behavioral science. For example, during WW I, we wanted to get women to use liver. How did they do it? They got them into group talking about how nutritious liver was, they started answering questions, and the women started using liver. It was so well established it went into AA, Weight Watchers, and others. Yet behavioral sciences won't even claim it today.

National Cancer Institute

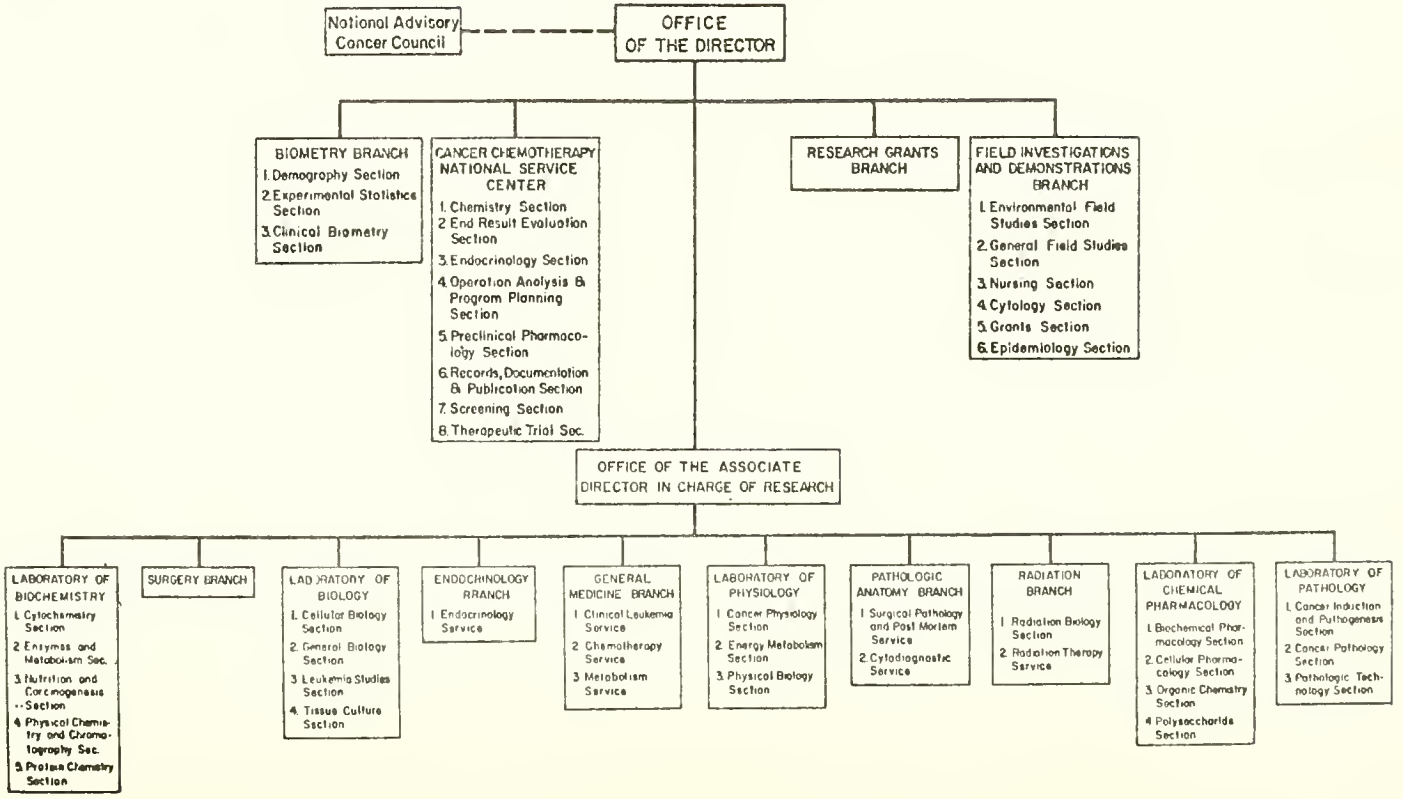
1947



* Detailed breakdown not specified

NATIONAL CANCER INSTITUTE

1957



APPENDIX 15

HEALTH POLICY ANALYSIS AND ACCOUNTABILITY NETWORK, INC.
1104 North Cole Road • Boise, Idaho 83704 • 208/376-9900

February 2, 1977

Lester Breslow, M.D., and
Mr. Larry Agran
School of Public Health
University of California
at Los Angeles
Los Angeles, CA 90024

Dear Dean Breslow and Mr. Agran:

Pursuant to the subcontract from the University of California, Los Angeles, the enclosed document represents the completion of the specified work of the subcontract.

As you know, the subcontract was let for the enhancement and amplification of the general data base available on your History of Cancer Control Project. Specifically, the expanded information related to an overview and assessment of the Regional Medical Program (RMP) activities in cancer control. The enclosed document is intended to serve as the basis for your use in the History and, per our discussion, as a stand-alone document we shall use. The "lessons learned" in the document emphasize the issues in creating favorable environment for community-based cancer control programs, and, it is hoped, will be useful to general readers as well as to the NCI.

In preparing this report, more than 800 documents generated by or concerning various RMP cancer control efforts were reviewed. Information was drawn from many of these documents for inclusion in this report. In addition, interviews were held with persons who were involved in key decision-making processes as they related to the RMP involvement in cancer control. Again, information contained in these interviews were included in the report. A transcript of the interviews is available at your request. Information concerning the RMP projects was also provided to HPAAN by the Bureau of Health Planning and Resource Development, HRA, DHEW. Obvious inaccuracies in the computer printout received were corrected, and where appropriate information was added based on records of individual RMPs. The analysis of the funding history of cancer control projects by the RMPs rests on these sets of information.

In total, the report represents a considerable amount of library and interview research and analysis on the part of Dr. Alfred M. Popma,

and other HPAAN staff. The initial draft was reviewed by a number of persons at UCLA. Members of the HPAAN Board of Directors and the PAR Group assisted in reviewing the conclusions section. A former DRMP staff person also informally reviewed the initial draft and contributed new and corrective information. Finally, a Washington-based individual familiar with the RMP legislation and the American Cancer Society provided valuable insight and detailed reviews. Revisions were made on the basis of the various review comments, but the authors assume responsibility for any error of fact or interpretation.

In addition to the enclosed report, 36 documents were ultimately selected from the HPAAN library and were formatted using your procedures for possible inclusion in the Data Bank for Cancer Control. These documents were selected on the basis of criteria specified by UCLA and in consultation with a UCLA data bank representative. As we understand it, there were insufficient funds in the History project to actually include these documents in the data bank. Other related materials were forwarded to UCLA at an earlier date and are also ready for inclusion in the data bank. As you know, funds to support specific costs of computer inputting were not part of the subcontract budget. Unfortunately, HPAAN efforts have already resulted in a cost overrun on this subcontract, therefore, subcontract funds are not available to cover any keypunching and other inputting costs.

Speaking for Dr. Popma, Dr. Smith and other members of the HPAAN staff who assisted us, I would like to say that we have both enjoyed and learned from working with the UCLA personnel to complete this review of RMP cancer control. We have found the History of Cancer Control staff to be extremely cooperative and helpful. We sincerely appreciate their suggestions concerning draft materials. The detailed, thorough, insightful criticisms and comments by Mrs. Devra Breslow and Mr. Agran were particularly useful and were conveyed in a manner that any "authorly pride" was in no way offended. Finally, Dean Breslow's efforts both during and prior to the subcontract award are appreciated and valued.

We look forward to an opportunity to be able to work with you again sometime, and hope you might consider us as an external evaluator for other projects or grants and in other appropriate roles where we might be of service.

Sincerely,

Jerome Selby

Jerome M. Selby, Director
Division of Research and Evaluation

JMS:ckg
Enclosures (2)
cc: Mrs. Devra Breslow
Alfred M. Popma, M.D.
C. E. Smith, Ph.D.

AN OVERVIEW OF CANCER CONTROL IN THE REGIONAL MEDICAL PROGRAMS

ALFRED M. POPMA, M.D.
JEROME SELBY
C. E. SMITH, PH.D.

HEALTH POLICY ANALYSIS AND
ACCOUNTABILITY NETWORK, INC.
1104 North Cole Road
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208/376-9900

December 31, 1976

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CREDITS AND ACKNOWLEDGEMENTS

The following report reviews cancer control program issues, experience and lessons in a recent national program aimed partially at regionalization of health services. It was prepared as part of an expanded data base for Lester Breslow, M.D., in his role as principal investigator in a National Cancer Institute supported study of the history of cancer control activities and programs in the United States since the mid-1940s. Dr. Breslow and his staff (Ms. Devra Breslow and Mr. Larry Agran) provided HPAAN with helpful assistance and guidance far exceeding the expectations that could have been reasonably set upon them.

Alfred M. Popma, M.D. assumed primary responsibility for drafting and rewriting tasks. Mr. Jerome Selby supervised the completion of the contract and conducted the National expenditures and project analyses; he and C. E. Smith, Ph.D. each provided editorial and drafting services. Mr. Nathaniel Polster and Ms. Phoebe A. Lindsey contributed major editorial assistance. In addition, Mr. Roger J. Warner contributed editorial suggestions and some draft material. J. Gordon Barrow, M.D., John R. F. Ingall, M.D. and Charles H. White, Ph.D. each provided valuable review comments. Ms. Tara Burt fulfilled library research and copy editing assignments and was assisted in library research by Ms. Marsha Morris.

While occasional consultation was made with others involved in the subject of the report, individuals thus consulted are too numerous to list. Major, invaluable assistance was given by those persons interviewed in depth: Robert Q. Marston, M.D.; Edward Morrissey; Stanley W. Olson, M.D.; Samuel R. Sherman, M.D.; Margaret H. Sloan, M.D.; Julie Sorenson; Paul Ward; and Charles H. White, Ph.D.

The contents of this report are, however, solely the responsibility of HPAAN and do not necessarily reflect the views of the persons listed nor of the University of California at Los Angeles which, through the National Cancer Institute, provided contract funds to support this study.

C. E. Smith, Ph.D.
President, HPAAN

SECTION I: LEGISLATIVE AND ADMINISTRATIVE HISTORY

ABSTRACT

The Regional Medical Programs were a ten-year federal experiment whose original mission included direction to foster partial regionalization and systems improvements for local cancer control services. Some fifty relatively autonomous local organizations were created across the nation. Federal resources were provided for professional staff support and for the development and implementation of local demonstration projects aimed primarily at meeting a high priority services development need as viewed by key local provider and knowledgeable consumer interests. Demonstration projects typically were funded for one-to three-year periods with widely varying amounts of local professional staff involvement and dollar awards. Demonstration projects were awarded subject to the approval of Regional Advisory Groups, a local governing board. In the early years of the program, federal approval of specific projects had been required. Due to the relatively autonomous nature of the local organizations and the particular circumstances of legislation, federal administration and significant fluid changes in federal priorities, the Regional Medical Program cancer control efforts constituted a relatively small portion of the total RMP program. A number of lessons in this regard, as well as in the areas of the administration and development of local services, are applicable to current public and future programs.

GENESIS

The history of Regional Medical Programs spans a decade which saw enormous changes in the nation's health care system. Between the time of the publication of the recommendations written by the President's Commission on Heart Disease, Cancer and Stroke (DeBakey Commission) in 1964 until the passage of legislation reorganizing health planning and development in late 1974, approximately forty laws were enacted which directly affected the nation's health care system.

Two efforts toward controlling cancer and heart disease had been made at the federal level prior to the appointment of the DeBakey Commission. The first efforts came over a number of years in the form of marked increases in congressional appropriations for biomedical research.

The second effort was the appointment of a blue ribbon committee by President Kennedy to prepare a plan for a massive control program in heart disease and cancer. The committee on Heart Disease and Cancer was mandated by the President to create a plan in approximately one month. A report was prepared and was in the process of being delivered to the President on the day of the Bay of Pigs invasion of Cuba (April 17, 1961). So urgent were the military and international diplomatic problems, that the report was never made to President Kennedy or the public. (1)

Nearly three years later, the direct impetus for the Regional Medical Program (RMP) was in President Lyndon B. Johnson's Special Health Message to Congress in February 1964. The President stated: "Cancer, heart disease and stroke stubbornly remain leading causes of deaths in the United States. They now afflict 15 million Americans. Two-thirds of all Americans now living will ultimately suffer or die from one of them." (2)

The President pointed out that about 50 percent of cancer victims are under age 65 and that cancer kills more children under age 15 than any other disease. He mentioned that over a quarter billion dollars was being spent annually by the Public Health Service to combat heart disease, cancer and stroke in addition to large investments by other organizations, both public and private. The President felt that new discoveries, new drugs, and new technology provided an impressive and hopeful basis for a major campaign against these diseases.

The President stated that, "Much remains to be learned. But the American people are not receiving the full benefits of what medical research has already accomplished. In part, this is because of shortages of professional health workers and medical facilities. It is also due to

a lack of the public's awareness of recent developments and techniques on prevention and treatment." (3)

He then announced that he was appointing a Commission on Heart Disease, Cancer and Stroke which would recommend steps to be taken to help reduce the incidence of these diseases through the application of new knowledge and more complete utilization of the knowledge available.

The Commission on Heart Disease, Cancer and Stroke

On March 7, 1964, President Johnson named a group of distinguished physicians, scientists, and informed citizens to the Commission (see Appendix A). Staff was selected and the Commission held its first meeting on April 7, 1964, at the White House. This meeting was addressed by President Johnson, who charged the Commission with the following mandates:

1. Measure the full impact of these diseases upon the nation.
2. Evaluate resources that are already available for acquiring new knowledge.
3. Identify obstacles which stand in the way of advancing knowledge and provide guidelines on overcoming these obstacles.
4. Put the country's great resources to work to overcome these diseases. (4)

The Commission organized itself into eight subcommittees with the chairpeople of these subcommittees constituting an executive committee. A subcommittee was established for each of the subject areas of heart disease, cancer, stroke, research, manpower, communication, facilities and rehabilitation. (5)

Letters were sent to 59 professional organizations and several voluntary health agencies, soliciting written statements which would set forth the views of those organizations relevant and pertinent to the work of the

Commission. Each subcommittee held hearings which obtained the opinions and recommendations of groups, agencies, and individuals appearing before them. A total of 45 such meetings were held, with more than 166 experts giving testimony. Over 7,500 pages of testimony were accumulated from these hearings. All subcommittee reports were reviewed by the Executive Committee of the Commission which met, as a whole, on six occasions.

Commission Findings and Recommendations

The full report of the Commission, containing its findings and thirty-five specific recommendations for implementation, (6) was presented to President Johnson in December of 1964. The general concept of the Commission's recommendations embraced a regional approach to research and improvement of health care concerning the categorical diseases. (7) While such a concept was not new, the Commission viewed it as a practical means of rapidly implementing its recommendations.

An analysis and review of regionalization efforts in the Regional Medical Program is contained in a recent study by the Health Policy Analysis and Accountability Network. (8) However, the concept of regionalization was broached during the early 1930s by Assistant Surgeon General Joseph W. Mountin. (9) In 1932 the National Commission on the Costs of Medical Care focused its attention upon the potential benefits of a regional approach to the delivery of health care services. Also in 1932, the Bingham Associates Fund began the first comprehensive regional effort to improve patient care by linking continuing education of physicians in the state of Maine with the University Medical Center in Boston. A decade later the Commission on Hospital Care and the Hill-Burton Act further advanced the philosophy of regionalization for health care services improvement. (10)

Little nationwide influence was exerted to implement regionalization concepts in the twenty year period of the 1940s and 1950s. By 1965, several factors began to have a direct effect upon this concept.

One of these factors was the creation of a massive national biomedical research effort, the first such activity in this country or the world. Between 1941, when the total biomedical research expenditure was $45 million, and 1947, biomedical research funding almost doubled to $87 million. However, by 1967 the total had reached approximately $2.3 billion, a 5,000 percent increase in 27 years. The most significant result was the tremendous outpouring of new knowledge in the medical sciences with a trend toward increased numbers of discoveries each year. Cures were found for diseases which had been thought incurable and newer techniques and modalities of detection, diagnosis and treatment were developed. (11)

A second factor which affected the concept of regionalization was that the increasing public awareness of these advances in care began to create a demand for more rapid dissemination and application of this new knowledge. The Commission quickly became aware of the lag between discovery, field-testing and widespread application of new knowledge. Addressing themselves to this need, the Commission studied the broad aspects of these categorical diseases, including the need for additional research, development of necessary manpower, improvement and development of communications, construction of necessary new facilities and the rehabilitation of patients.

The Commission's recommendations were based on the following principles:

1. The federal government should share the responsibility for assuring that persons suffering from or threatened by the categorical diseases have ready access to the benefits of the best in medical services based upon the products of scientific research;

2. The federal government should assume a major responsibility for strengthening and broadening the support for research which will generate new knowledge for the control of the categorical diseases;

3. The federal government should have a major responsibility for direct and diversified support of medical education and other programs designed to produce the manpower upon which the control of the categorical diseases depends; and

4. The nation can well afford, and the people will enthusiastically support substantially increased expenditures to save lives today and produce more lifesaving knowledge for tomorrow. (12)

The Commission recommended that:

1. A national network of 25 Regional Cancer Centers be established for clinical investigation, teaching and patient care, in universities, hospitals and research institutes and other institutions across the country;

2. A national network of 150 diagnostic and treatment stations be established in communities across the nation, to bring the highest medical skills in cancer within the reach of every citizen;

3. A broad and flexible program of grant support be undertaken to stimulate the formation of medical complexes whereby university medical schools, hospitals and other health care and research agencies and institutions would work in concert; and

4. A program of developmental grants to medical schools be developed to enable them to improve their total capacity for both academic and research programs for the ultimate purpose of creating a greatly increased number of true "centers of excellence" in medical education and research. (13)

To stimulate the participation of communities, the Commission recommended that a special program of incentive grants to communities be established in the Public Health Service for the development of community planning and coordination of health activities. Recommendations were also made to increase the size and scope of the Public Health Service programs for community health research, and to provide assistance in establishing and maintaining coordinated laboratory facilities. (14)

A national program for the early detection of cervical cancer and a similar national program for continuing education of health professionals, as well as public education, were among the high priority programming recommendations of the Commission.

The Commission recognized the importance of the National Institutes of Health and urged expansion of their programs with increased financial support. Similar recommendations were made concerning the expansion of the Public Health Service activities. (15)

Programs were urged to increase (1) support of medical and nursing schools, (2) recruitment of young people into the health professions, and (3) grant support to young investigators desiring research training. Clinical fellowships, lifetime career awards and paramedical training programs were deemed necessary to develop adequate health manpower. (16)

Similarly, the expansion of patient care facilities under the Hill-Burton Act would require greater availability of funds. The Veterans Administration would need additional funding as well to improve and expand their care of cancer patients.

An improved system for data collection was deemed essential to understanding the problems of cancer control and expansion of the National Library of Medicine and National Audio Visual Center program were considered absolutely essential.

The development of programs designed to train and support laboratories and personnel for animal research was urged. In addition, it was recommended that a National Drug Information Clearinghouse be established in association with the National Library of Medicine. Lastly, the Commission recommended cooperative research efforts between American and foreign laboratories when such programs would be in the national interest. (17)

Federal Legislation

Early in 1965, legislation related to Commission recommendations was developed and introduced into Congress as House Resolution 3140 amending

the Public Health Service Act (42 U.S.C. ch. 6A, entitled Sec. 2, Title IX, Education, Research, Training and Demonstrations in the Fields of Heart Disease, Cancer, Stroke and Related Diseases). This legislation was referred to the House Committee on Interstate and Foreign Commerce and the Senate Committee on Labor and Public Welfare.

Hearings on the proposed legislation were held by both the House and the Senate Committee. These were generally regarded by Capitol Hill veterans as important hearings related to legislative proposals of broad impact. There was controversy and there were displays of political strength by a number of health organizations.

When the debate reached a peak, with no viable consensus among contending parties, a conference was called by President Johnson and included John Gardner, Secretary of the Department of Health, Education and Welfare (DHEW) and several American Medical Association (AMA) leaders during August of 1965. President Johnson was deeply involved in pushing through Great Society legislation, including Medicare. It was widely known that he considered Medicare a very important part of his legislative program. In this context AMA, which was opposing enactment of the proposed RMP bill in its original form, told the President that, "the proposed legislation was jeopardizing AMA's attempt to work with the Secretary of DHEW relating to Medicare law." (18)

Some observers felt that the AMA saw, in the legislative proposal to create 25 regional medical centers, the threat of 25 large medical centers or clinics siphoning off a significant portion of their practices and thereby damaging the private physician's ability to earn income.

The Administration accepted some 20 amendments to the bill. (19) No new centers were authorized under the new bill and one of the most important amendments provided that the legislation would in no way interfere with the

existing patterns of patient care, professional practice, or methods of financing care. As enacted by the Congress the legislation promoted "regional cooperative arrangements" among existing health institutions.

On the face of it, the legislation appeared to be self-contradictory. Its premise was that research results were not reaching the patient in a timely fashion under contemporary patterns of care, but the goals of the law could not be achieved unless something significant happened to the pattern as they stood.

In this circumstance, the only viable interpretation of the statute was that RMP administrators could do whatever was needed to be done in changing patterns of patient care so long as the change did not seem threatening to AMA. As a matter of fact, as is demonstrated later in this paper, many important changes in health care delivery were introduced by local RMP groups which had the ability not only to avoid conflict with AMA, but also to involve many medical societies and private physicians in changing the medical care delivery patterns.

The enacted law (Public Law 89-239) placed less emphasis on proposed and existing centers and more emphasis on peripheral institutions. (20) Marston perceived the law as placing unusual emphasis on voluntary local initiative rather than on mandatory federal direction. (21) The act established a grant-supported system through which representatives of health resources development could identify and meet local needs within the area of categorical diseases. Recognition of geographical and societal diversities within the United States was one of the main reasons for this deviation from the Commission's recommendations. Testimony from spokesmen for the nation's health resources strengthened the case for local initiative.

The grants authorized by the act would encourage and assist the establishment of regional cooperative arrangements among medical schools, research institutions, hospitals and other health related agencies to develop programs of research, education and patient care, aimed at making available to the public the latest and best knowledge to combat the heart disease, cancer, stroke and related categorical diseases. The intent of the act was also to generally improve the health manpower and facilities of the nation.

The rewritten bill passed the Congress and was signed by President Johnson on October 6, 1965 and became Public Law 89-239. The stage was set for a new and intensified attack on cancer. The legislation put the program in the National Institutes of Health where the division of Regional Medical Programs (DRMP) was created. The DRMP was designed to operate under a National Advisory Council in the NIH pattern. The Council reviewed and recommended funding levels for each region. The Surgeon General signed off on each grant in the usual NIH fashion. Each grantee, whether a medical school, medical society, or free standing non-profit corporation was to operate under the advice of its own Regional Advisory Group. The Regional Advisory Group (RAG) advised the prospective grantee on formulation of its plan for development. After a grant was awarded the Regional Advisory Group continued to advise on the implementation of the plan.

Inherent basic principles in the act required the establishment of regions, each composed of a geographic area containing the necessary prescribed elements for development of a program. (22) Within the broad national objectives, the design of each Region's program was to be locally determined. This approach was a significant departure from the usual practice of directing programs from a national level to be uniformly conducted at the local level. The

organizational structure and funding process were both quite complicated, as indicated in Appendix B, Regional Medical Program Organizational Structure.

PROGRAM BEGINNINGS: THE NIH PERIOD

At the first session of the National Advisory Council for Regional Medical Programs on December 21, 1965, Dr. William H. Stewart, Surgeon General who was presiding, delineated the functions of the Council. He stressed particularly the emphasis made during the House Committee hearings on local initiative. The importance of the local advisory group was pointed out. A second point, emphasized by the law, and discussed in depth by the House Committee at the hearings, was the flexibility of the act, with no hard and fast rules being developed initially by NIH, in order that a variety of programs might be permitted to emerge. (23)

Dr. Kenneth Endicott, director of the National Cancer Institute (NCI), appeared before the Council and discussed the major programs of NCI. He stated that the NCI expected a very close relationship with RMP in the development of cancer programs. The NCI was attempting to identify promising locations for the development of cancer centers and felt that, through a close liaison with RMP, much could be accomplished to develop strengths presently not in existence. The development of resources by RMP through its planning could become extremely valuable to NCI. (24)

At its early meetings, the National Advisory Council addressed the many problems of procedure for this new program. Dr. Robert Q. Marston was appointed Associate Director of NIH and Director of the Division of Regional Medical Programs (DRMP) on February 1, 1966. The Advisory Council decided to follow the NIH format of processing grant requests by referral to ad hoc

study sections and review committees prior to evaluation by the Council. It was deemed essential by the Council that original applications should be limited to planning for the organization and development of a framework for subsequent specific programs which then would be considered for operational grants.

However, in planning, each region was to collect and analyze data concerning the numbers and distribution of health care personnel and institutions, the quality and location of equipment and other factors affecting the delivery of health services in their regions. Based on that analysis, regional programs were to establish their own local goals and objectives within the scope of the Regional Medical Program effort.

A central point was that RMPs were expected to accomplish their mission by developing and funding short-term (about three years) demonstration projects which would be continued by appropriate federal and local agencies after RMP funds were no longer available.

Battistella has stated that grass roots planning gave the local communities, for the first time, an opportunity to study their regions and, on the basis of their unique needs, define cancer control programs. (25) With most of the regions eager to engage in definitive programs, there appeared to be a widespread desire to deal with the problems of acute coronary heart disease. (26) The rush to start heart disease programs was partly due to the fact that such programs did not require an in-depth study of community needs as did cancer control and partly because hospital coronary care units (CCUs) were the newest major development available for technology transfer. Nothing quite as spectacular was ready in the field of cancer. The prototype coronary care units were easy to copy and adapt to each region. (27)

It was soon possible to demonstrate statistically the life extension that these units provided. Heart disease was something that could be attacked immediately.

This was one of the great achievements of RMP. As could be anticipated in the light of testimony given to Congress, and in the way the law was ultimately rewritten before signing, the medical profession was quite apprehensive about the introduction of these units in many parts of the country. Cardiologists saw their activities being taken over by nurses, for instance. The excellence of the technology to be transferred and the community organization skills of the RMP staff involved, however, were able to change the pattern of medical care distinctly here without violating the law and without causing conflict in the medical community.

This success enabled the RMPs to address the need for training with regard to these units. The need was so enormous that many RMPs focused on training necessary to staff coronary care units. (28)

As a result of the programming opportunity in coronary care, a vast majority of early planning was aimed toward providing early, adequate care at community levels for patients having acute coronary problems. Cancer control programs, seemingly requiring far more study and planning, were relegated to lower priority. Only those regions where there had already been some previous planning for long-range cancer control were in an optimal position to develop RMP cancer programs rapidly.

Regional Boundaries

With the burden of identifying regional limits, many applications were submitted for a wide variety of geographic areas, ranging from a single community hospital in New York State to the massive area of the states of

Washington and Alaska. Since the legislation made no requirements for population of the region, each regional area was free to define its own population service area and geographical limits. In many areas there were geographical overlaps, some of which were easily and quickly adjusted through regional cooperation, while others required several years of debate and negotiation.

Medical schools were quick to recognize the importance of the program and many applications for grantee responsibility were entered from such institutions. Of the 56 regions, 37 were initiated with medical schools as their grantee organization (see Appendix C for a listing of the RMPs). However, educational institutions were generally reluctant to put into effect the community orientation philosophy of the RMPs, believing that too great a financial burden would be placed upon the medical schools which would have to dilute their own resources to expand into community programs. There was considerable apprehension on the part of the practicing medical profession regarding the domination of medical schools over the program. As a result, a number of state medical societies became involved in diffusing program control and domination by medical schools by endorsing more representative governing bodies. Such diversified control was formed through the establishment of a more balanced regional advisory group (RAG) containing many nonmedical school representatives. A broad-based RAG of this nature allowed greater opportunities for the development of cooperative arrangements.

It soon became apparent that appropriately qualified staff pe sonnel throughout the country were scarce. Many regions found it difficult to find competent, qualified people to direct organizational and planning efforts. About a year prior to the enactment of PL 89-239, the NCI had allocated to each medical school an additional $25,000 to double the annual amount for

developing cancer teaching programs. Many schools used these funds to do preliminary planning for RMP programs when they knew that the legislation was destined to become law. Such institutions were in the forefront in submitting applications and many of these grant requests reflected the expertise of skilled administrators. Other newly formed regions had great difficulty in finding the needed personnel. A number of regions were severely hindered in their efforts because of lack of "grantsmanship" and therefore were delayed in development of regional planning. Assistance was given by DRMP staff to such regions and, eventually, acceptable programs were developed, applications approved and funds granted for programs covering the entire United States, each approved by a local regional advisory group and implemented under its supervision.

A number of systems development organizations in the country which had gained expertise in planning for the Department of Defense and the National Aeronautics and Space Administration (NASA) seized the opportunity to assist in planning RMP programs. A number of regions thought they needed this type of planning help and entered into subcontracts with these groups. Nineteen regions applied to DRMP for this type of assistance, but the companies selected tended to submit mass-produced systems plans without sufficient or insightful effort to study, define and try to meet the unique resources development needs of each region. Almost all of these "sophisticated" applications were disapproved and several regions were required to start over and do the planning themselves. (29)

Applications for operational grants were submitted early in the course of planning from many regions. A number of projects reflected excellent planning, but others failed to meet DRMP guidelines and were returned for submission or, in some instances, were completely rejected. Site visit

teams comprised of National Advisory Council members, Review Committee members and DRMP staff assisted many regions in producing satisfactory operational projects which were acceptable for funding. The first planning grants for RMPs were approved by the National Advisory Council in April 1966, and the first operational grants were awarded in February 1967.

Control of National Program Content

The autonomy of the individual regions to develop their own program led to one frequently voiced criticism of the Regional Medical Programs: the wide variety of activities did not produce a significant national program impact in any particular area. Insofar as it goes, this criticism contains some degree of accuracy; however, it reflects one more aspect of the misunderstanding in the "curious odyssey of the Regional Medical Programs."

The enabling legislation never intended that there be uniform program content nationally. In effect, federal administrators had only three mechanisms available to them to control the program content of the individual Regional Medical Programs. The first mechanism was through informal techniques such as fostering communications among individuals in various parts of the country interested in similar projects, sharing of project information through a "National Information and Data" newsletter, and providing unofficial guidance by federal employees interested in particular disease categories. Another general mechanism used by federal administrators to influence the content of Regional Medical Program activities was the strategy of "earmarking" funds. "Earmarked" funds involved setting aside a particular amount of the annual appropriation to be used only for specific kinds of projects. However, such "earmarks" assumed fund requests for such projects would be submitted by the regions.

National program staff also influenced program content of the regions' projects by holding various national conferences, letting it be known informally that the federal staff would take a stand for or against particular kinds of projects at the National Advisory Council. Significant influence was exerted in some instances by use of these mechanisms. However, national "control" of RMP program content did not exist. This organizational control characteristic was both a strength and weakness of the RMP program and was a major factor in the subsequent demise of the program. (30)

Nationally Initiated Projects

National DRMP staff purposely carried out or contracted some projects themselves under section 907 of PL 89-239. In the RMP legislation of 1965, section 907 required that the Surgeon General of the Public Health Services (later the Secretary of the Department of Health, Education and Welfare) establish and maintain a list or lists of medical facilities in the United States which were staffed and equipped to deliver the latest advances in the diagnosis and treatment of patients with heart disease, cancer and stroke (end-stage kidney disease was added in 1968) and which could provide training in relation to these diseases. In carrying out this responsibility, the Surgeon General/Secretary was expected to utilize the assistance of national, professional and voluntary health organizations.

When the National Advisory Council for Regional Medical Programs considered this section of the legislation, its members noted that although the Surgeon General was supposed to be responsible for establishing and maintaining a list or lists, he was urged to utilize the help of national professional organizations in this endeavor. The Council believed that such organizations should be given the responsibility for advising the Surgeon General/Secretary on the lists and in developing the criteria on which such lists should be based.

The first effort launched under section 907 was in the field of cancer. The contract was negotiated with the American College of Surgeons because, through its Commission on Cancer, it already had an organization representative of the various disciplines necessary for a comprehensive approach to the problem of cancer. Furthermore, the College already had a voluntary hospital inspection program providing approval of cancer services according to certain criteria. At one point the criteria had been limited to the presence of a cancer registry. Over time (and spurred on by the section 907 contract) the criteria were expanded and strengthened to include a hospital cancer committee which would be responsible for the quality of cancer care, supervise the registry and arrange educational cancer conferences for the staff. (Medical audits of the quality of care provided to cancer patients have recently been added to the ongoing program.)

The American College of Surgeons had been concerned with the care of the cancer patient since its organization in 1913 and had begun in 1921 what may have been the first national tumor registry when it collected reports of bone sarcoma. The College's Commission on Cancer was established in 1922 and in 1933 published the first list of institutions with approved cancer programs. The RMP Cancer Guidelines Committee named by the College included outstanding specialists in the field of cancer from within the College as well as other specialty groups. This group felt remarkably uninhibited by federal pressure of any kind. (31) Dr. Warren Cole, a respected ormer professor and head of the Department of Surgery at the University of Illinois College of Medicine and a past president of the American Cancer Society, was named chairman of the Committee to conduct the study.

Following several years of study, including site visits by the Committee to numerous representative institutions, the Committee prepared a report

which was submitted to the RMP National Advisory Council and the NCI. (32) It did not list each individual facility by name. Instead, the Committee devised a program of recommended guidelines against which an institution could measure whether satisfactory management of the cancer patient was being provided. Guidelines were determined for the care of such patients, both in the physician's office as well as in the hospital. Guidelines included advice about the inauguration and development of a tumor registry and the functions of a good tumor registry were spelled out. An interdisciplinary approach was emphasized in the guidelines and the importance of support services such as rehabilitation, nursing and social services was stressed.

The "Cole Report," published in 1970, gave the RMP National Advisory Council an excellent guide for considering applications for planning grants from regions, as well as a guide to evaluate the potentialities of RMP operational programs in cancer control. At that time, Dr. Verne Wilson, Administrator of HSMHA agreed that DRMP should utilize these guideline materials through a contract with the Joint Commission on Accreditation of Hospitals (JCAH) and endeavor to find out how the hospitals of the United States measured up. A detailed questionnaire was prepared covering all four fields and was sent to all short-term hospitals in the country through the cooperation of the JCAH, the American Hospital Association and the National Center for Health Statistics. The response was remarkable and ultimately yielded data on 92 percent of short-term hospital beds in the United States. Results of that questionnaire were published by the Government Printing Office in a seven volume set entitled Hospital Services for Selected Chronic Disease Patients - 1972 covering data collected in 1971. (33)

The guidelines were then summarized and rearranged to cover three categories of hospitals and special disease centers (such as cancer centers) and published in the Journal of the American Medical Association (JAMA) in late 1973 and early 1974. The cancer guidelines entitled "Optimal Criteria for Care of the Patient with Cancer" were published in the January 7, 1974, issue of JAMA, (Volume 227, No. 1) and reviews and comments were solicited. Based on careful consideration of the numerous responses revised guidelines were published in late 1974 by the JCAH in a booklet entitled "Hospital Categorization Guidelines."

Although no official central listing of medical facilities was ever prepared before the phaseout of the RMPs, State Health Departments, Comprehensive Health Planning Agencies, and their successor Health System Agencies have been able to match the data for their regions in the inventory volumes against the guidelines and make their own identification of hospitals meeting these criteria. The Bureau of Health Resources Planning and Development, Health Resources Administration has also used material to map health resources in every state and county and has made this information available to the Governors of the states and to the State and Teritorial Health Officers.

The cooperative efforts with national professional organizations carried out under section 907 of the RMP legislation represented one of the most productive and rewarding efforts of this interesting experiment i American medicine. The time and effort invested by hundreds of experts in the development of guidelines for quality of care in these four disease category fields cannot be adequately acknowledged. A real sense of satisfaction and pride was created on the part of the physicians who were setting the goals for their own specialties with government assistance but no government interference.

Planning applications from many RMPs indicated that there was concern by health professionals in many regions that little judicious planning had gone into the development of radiation facilities for cancer treatment. Cobalt 60 units were being installed in physician offices and hospitals with little thought about the likely volume of use. In many instances, facilities were duplicated, and highly sophisticated and expensive equipment installed merely to satisfy the whims of hospital boards, wealthy donors or physicians insistent in having such facilities "in their hospitals for their patients." Much of this had occurred because of a lack of guidelines or information to assist in planning radiation oncology facilities.

Several years prior to the inauguration of RMPs, the American College of Radiology had recognized this hiatus and had established a "Commission on Cancer." An independent Committee on Radiation Therapy Studies was formed about the same time under grant support from NCI. The first chairman was Gilbert Fletcher, M.D. of M. D. Anderson Hospital and Tumor Institute. In 1966, the Committee on Radiation Therapy Studies formed a "Subcommittee on Regional Medical Programs." That Subcommittee produced a much used "Blue Book" entitled, "The Prospect for Radiation Therapy in the United States." (34) DRMP entered into a contract with the American College of Radiology to use the "Blue Book" and later the revised version entitled, 'The Role of Radiation Oncology' as a working basis for providing consultation to various RMPs and to other organizations in the nation on the most appropriate development and organization of radiation therapy facilities. (35) The specific purpose of the Blue Book and its revision was to serve as a guide to the evaluation of the need for radiation therapy facilities and as a basis for recommending possible means of filling gaps in service in the region being reviewed. Eventually, 22 radiation oncology programs were

established to assume the provision of expert radiation on a regional basis including calibration of the equipment by radiation physicist, computerized dosimetry and consultation by radiation therapists.

Under the chairmanship of Robert D. Moreton, M.D., a systematic plan was devised to obtain information. Radiologists in each state were contracted by regional chairmen in nine College of Radiology designated regions to obtain pertinent information concerning the establishment of radiation therapy facilities in communities which varied in size from densely populated metropolitan areas to sparsely settled rural communities. Efforts were made to determine the necessary factors which could be used to avoid duplication and feedback to communities of the necessary information to develop oncology treatment facilities with adequate personnel and equipment to provide the benefits of quality care to cancer patients without leaving their home environment.

This new Blue Book was widely disseminated by the American College of Radiology. It led to a subsequent workshop in radiation oncology facility planning in 1974, following which additional recommendations were made. These were incorporated into a widely disseminated volume, A Planning Guide for Community Radiation Oncology Facilities. (36)

During the "NIH Period" most of the regions were well advanced toward the operational phase. The first two years of RMP saw a growing spirit of cooperation between organized medicine and RMPs. What had begun with a spirit of distrust on the part of medical associations gradually changed from grudging tolerance to definite acceptance and cooperation. The growing leadership in RMPs gradually involved physicians, dentists, nurses, allied health personnel and hospital administrators as well as interested and knowledgeable consumers. These groups functioned best in planning specific organized innovations to meet the needs of patients at the local level.

In October 1968, after two years of planning, 49 regions had advanced, moving at different paces, toward operational grants. By this time 36 regions had full-time directors or coordinators with the remainder having assistant or part-time coordinators. These regions had assmebled approximately 1,200 staff members. Regional Advisory Groups had been expanded to include knowledgeable consumers and allied health personnel in addition to physicians, nurses and medical school representatives. Because of the progress displayed by the various regions, there was a general feeling of well being, both on the part of staff and the National Advisory Council.

Even though less than five percent of the funds appropriated were awarded for cancer projects, the availability of funds for this type of program apparently was no problem during the NIH period. In fact, more cancer projects could have been approved if the regions had submitted more requests. (38) At this time, the effort required for planning cancer projects and/or the lack of interest by individual RMP coordinators and their Regional Advisory Groups and the waning support of nationally influential cancer interests were probably the biggest obstacles to the development of cancer projects.

After significant effort in planning and organization, the RMP program was abruptly moved, administratively, to the newly created Health Services and Mental Health Administration (HSMHA) within DHEW. This shift occurred partly as a function of significant forces in the National Institutes of Health which, in protecting biomedical research and general medical education support philosophy and budgets, were apt to divest themselves of programs which dealt with complex and costly health delivery systems changes. The effects were drastic on the ability of the RMPs to be a significant and stable mechanism for transferring new technologies of cancer control. (39, 40, 41, 42, 43)

THE HSMHA PERIOD

By October of 1968, over half of the regions were involved in operational programs in heart disease and continuing education activities or had developed programs for these categories. Cancer programs were conspicuously lacking, however. They accounted for only about nine percent of the total RMP funds for fiscal year 1968. However, 1968 witnessed several important events which had significant effects on RMP. First, in early 1968, there began a process of reorganization of federal agencies within the health fields. The upper echelons of HEW staff devised a new structure to be called the Health Services and Mental Health Administration (HSMHA). Dr. Marston was slated to become the director of this new agency. With Surgeon General William Stewart exerting his influence and Dr. James Shannon, Director of NIH, consenting, RMP was moved from NIH to HSMHA. Dr. Marston, who had become the new director, felt that he could still wield an important influence over RMP. In May of 1968, Dr. Marston assumed leadership of HSMHA and Dr. Stanley W. Olson was appointed Director of the Division of Regional Medical Programs.

Olson came with a background of 14 years as dean at Baylor University Medical School and two years as coordinator of the RMP at Vanderbilt. Unaware of the move of RMP from NIH to HSMHA, Olson on his arrival in August 1968 found that his original assignment to associate director in NIH and director of RMP had been shifted to HSMHA, which in his words, "was a real shock to me." (44)

The organizational move was also significant since, as a division of NIH, RMP enjoyed the protective relationship afforded to NIH by Congress. (45) HSMHA never enjoyed this protective relationship and, in fact, disappeared after only four years. Being removed from NIH caused a loss of the close

relationship with NCI in developing and augmenting cancer programs. (46) RMP did, however, assume responsibility for many of the cancer programs such as training fellowships and other programs formerly conducted by other units of DHEW (Chronic Disease Control programs in the Bureau of State Services. (47)

In March 1968, companion bills were introduced in Congress to extend Regional Medical Programs. They were the result of the Report on Regional Medical Programs prepared by the Surgeon General of the Public Health Service, submitted to the President through the Secretary of HEW in compliance with the original act (PL 89-239). The Report concluded that "the initial progress provides solid evidence for continuing the program without modification of its essential nature and purposes" and recommended a five-year extension so that Regional Medical Programs could "attract the long-term commitment of the kind and quality of people, and the full participation of all affected institutions which are essential to the program's success." (48) Although Congress subsequently passed the extension legislation (PL 90-574) in October 1968, the extension was for only two years.

In May 1968, a revised set of guidelines for RMPs was issued by the Department of Health, Education and Welfare. Although the guidelines restated the categorical disease approach for RMPs, they suggested that activities that had a more general impact beyond the categorical diseases may be supported because they are essential to achieving the purposes of RMP. (49)

In November of 1968, Richard Nixon was elected President of the United States. Dr. Stanley Olson reflected on the change in administration as follows:

> During his two years as director of DRMP, Dr. Marston was mostly preoccupied in getting planning grants started. About the time 75% of the regions had been funded for a planning grant, the change in administration took place. Then, instead of funding increasing to the projects which could have, everything leveled

> off. By that time it was a matter of trying to get something done that would provide a quick feedback because the program was planned from one budget session to the next. 'What have you got to report? We gave you this money! What have you done with it?' I think, in part, that was one reason why some of the planning grants came out the way they did and that's, in part, why everybody was looking for a quick return. Almost from the time we were in HSMHA, we were stymied by two factors: (1) the Nixon administration really had no enthusiasm for this Lyndon Johnson program, and (2) the identification with NIH and its orientation to getting research benefits out to the bedside was extremely modified. (50)

It soon became apparent that program priorities were to be reassigned throughout the administration. No longer was cancer to be one of the important diseases in the triad authorized and directed by the Congress for a major control effort. Tumor registries, an important tool in cancer control, were frowned upon through the simple process of discouraging requests for funds to the registries. Over the months, cancer programs were displaced by emphasis on problems in primary care. End-stage renal disease was embraced as a program priority by the earmarked-funds mechanism. Vast amounts of funds were also earmarked in numerous, non-cancer program areas such as arthritis and emergency medical services.

At the local RMP level, long-range cancer planning, which had required several years of intensive and costly study and planning, were shelved because of administrative directives to change program emphasis. Excellent cancer programs evolved at local levels were phased out or markedly reduced in scope for lack of funding. (51, 52) The frustrations of continuing upper administrative priority changes produced an intolerable situation for Dr. Olson. (53)

Many people, including Dr. Marston, feel that the Office of Management and Budget (OMB) was one of the primary forces in the decline of both RMP and cancer activities. According to Dr. Marston:

> It was a strange period under Mr. Nixon--not to blame it on a single person. There was a change in the attitude of OMB. While I was director of RMP, I worked closely with capable and helpful individuals with amazingly broad ranges of significant responsibilities. The role of the individual in OMB then was to be helpful and to understand as much about the programs as possible, and to be available to agency heads to tighten and justify their programs. Later, OMB moved into a different management style which was essentially one of great suspicion of the expert. The assumption was that the self-interest of the agencies would throw a bias into any decisions which had to be countered. Since you couldn't depend on the Cabinet officers to be other than advocates of their programs, then OMB--I think at cost to the country--began taking much more an adversarial role and began to say that it was their responsibility to make broad decisions and to implement them without being troubled by having too much specific knowledge about the programs. (54)

Two of the major forces which resulted in the disappearance of major cancer activities after fiscal 1970 were (1) the lack of program stability, administrative support and the consequent need for short-term impact information to justify short-term programs, and (2) the combination of reduced funding levels and lack of program stability which, in turn, motivated major cancer interests nationally and regionally to seek other means of federal support for large-scale cancer control efforts which resulted in the National Cancer Act of 1971. The impact of a cancer control program can usually only be studied over a long-term period, partly because survival rates of cancer patients are usually measured in terms of a five-year period. This is in contrast to heart disease efforts where in some cases results are more quickly achieved. In addition, effective cancer control programs are not an inexpensive undertaking, and level or reduced funding of RMPs meant funds were not available to start cancer control projects, even if program stability and support were necessary for continuation. (55)

Early in 1970, deliberations began once again on the issue of extending Regional Medical Programs. The new enabling legislation, passed in October

1970, incorporated the changing concepts regarding RMP that had evolved since Title IX of the Public Health Service Act. The program was changed in several significant ways. To the categorical diseases of heart, cancer and stroke was added kidney disease. In addition, the purpose of the Act was expanded "to promote and foster linkages among health care institutions and providers so as to strengthen care." (56) Also added to the purposes of the act was the improvement of health services for persons residing in areas with limited health services. There was an inclusion of functions of prevention and rehabilitation, as well as previously stressed training, diagnosis, and treatment. Finally, provision was made for new construction of facilities for demonstrations, research, and training where necessary to carry out regional programs. One of these rare RMP construction grants, in the amount of approximately $5 million, was awarded to develop the Fred Hutchinson Memorial Cancer Center in Seattle, Washington. These new concerns with primary care, increased health services to the medically disadvantaged, relationships to comprehensive health planning agencies, and community orientation, indicated a new emphasis which was, at least partly, a result of the replacement of the Surgeon General with the Secretary of DHEW as the person ultimately responsible for the operation of RMP. A new set of recommended national priorities for health was issued in the spring of 1970 by DHEW.

Many experts felt that the expansion of the RMP mission was a fatal mistake, for example:

> It is just too bad that RMP went the way it did. It (RMPs) was always much opposed to changing from the categorical diseases into all these programs. I felt that we had no business getting into emergency care. But I felt we should have stuck with our original premise of heart disease, cancer and stroke. (57)

The result of these changes in RMP mission was pointed out in the Congressional Record on December 13, 1974 by Representative Paul Rogers, chairman

of the House of Representatives' Subcommittee on Health, who summarized the effect of these changes on the RMP mission:

> In fiscal year 1970, over 80 percent of RMP projects were disease-focused, categorical in nature, and only 17 percent of the projects were of a comprehensive or multi-categorical nature. Two years later, in 1972, one-half--45 percent--were of a comprehensive or multi-categorical nature and the percentage of projects focused on a single categorical disease had dropped correspondingly--83 percent to 55 percent. (58)

At about this time, the federal administration first began the practice of "forced carryover" of funds (59) and began using the RMP budget in various noncategorical ways. (60)

"Forced carryover" is jargon meaning that the OMB or other fiscal control agencies do not release a part of the money Congress has appropriated for a program and then the "savings" are carried over to the following year, usually for the purpose of reducing the next year's appropriation. The technique is sometimes a means of whipping a program into line--of warning it to revamp its behavior and purposes, or perish. It is a sure way to throw consternation, confusion, destruction and depression into the working ranks of a program. There is no surer way to reduce the productivity and momentum of any program if that is the intent. (61)

The handwriting was on the wall. Under a new administration, and without the protective shielding of NIH, RMP became an administrative target for destruction. Funds in the President's budget for RMP were cut drastically. Continuing education to improve cancer patient care, development of radiation dosimetry centers, and training programs of cancer fellowships met the same fate as had tumor registries. Planning for improved cancer control programs at local levels became virtually nonexistent. The only funds provided by RMP for cancer control were limited continuation funds for projects previously initiated.

The RMP became a handy vehicle for the launching of any new programs the Congress wanted to implement. Programs such as end-stage kidney disease, emergency medical care, arthritis and area health education programs were assigned with earmarked funds for RMP to implement. Cancer, as well as heart disease and stroke, lost out progressively in the competition for funds. (62) The final action, which practically ended the initiation of major cancer control activities by the RMPs, was the passage by Congress of the National Cancer Act of 1971. The law placed the responsibility for cancer control programs in NCI. As a result, RMP personnel at both the local and national level were uncertain about continued initiation of new cancer programs by RMP. Coupled with the administrative change in the RMP mission statement, which eliminated cancer as a priority area, cancer projects that were funded by the RMPs were few after 1971. The small amounts of RMP funds awarded for cancer control from 1971 to 1975 were mostly for continuation of projects started prior to 1971 and for some small-scale, local coordination efforts aimed at effective preparation of local communities to apply for and use NCI project grants. (63)

In the fiscal 1972 budget recommendation, which was introduced in 1971, the Administration began publicly to criticize RMPs and suggested yet another shift in emphasis to delivery of primary care, emergency medical services, health manpower development and cost containment, with categorical and continuing education program activities to be held at a minimum. Even with the acceptance of these shifts of emphasis, OMB had recommended only $52.4 million in new money--about half of what had been available in 1971. The presidential budget message to Congress carried the assumption that, with the carryover of $34.5 million of unexpended 1970 and 1971 funds, RMPs would have $86.9 million for 1972 activities. (64)

During this period of RMP history, it had become increasingly difficult to persuade the DHEW budget managers and later OMB to release carryover funds. Early in 1971, for example, the Administration essentially reduced funding to RMPs and it became probable that the $34.5 million allocated for 1971 would not be awarded, but would be carried over to FY 1972.

Representatives of RMP met with DHEW Secretary Elliott Richardson in an attempt to have the carryover funds released. Secretary Richardson showed renewed interest in the program and in the spring, RMPs "were charged to help define 'health maintenance,' to set criteria for quality in health maintenance organizations and to develop and set in motion quality control activities." (65)

A $10 million supplemental appropriation in fiscal 1971 was approved by Congress to restore momentum lost through Administrative cutbacks. The Administration then adopted a concept of level funding for RMPs in fiscal 1972. Program stability seemed finally to have arrived, even if tenuously.

Casper W. Weinberger moved from the Office of Management and Budget to become Secretary of Health, Education and Welfare toward the end of 1972. Because of his public statements, it was widely assumed that health budgets would be cut. This became a fact in early 1973 when the President's health budget for fiscal 1974 slated Regional Medical Programs for phase-out by June 1973. The Administration charged, among other criticisms, that RMPs were too closely linked to categorical disease programs. Further, RMP projects "have not been carried out according to any consistent theme or set of authorities."

As Director of the OMB, Mr. Weinberger declared that (1) "It is not an appropriate use of federal funds to finance continuing education for

professionals generally capable of financing their own education to improve professional competence;" (2) "Originally established to upgrade health care for persons threatened by heart disease, cancer, stroke, kidney disease and related diseases, the RMPs in recent years sought more to improve access to and generally strengthen the health care delivery system;" and (3) "Dismantling the superstructure of the RMPs will also reduce the competition of improving the health service system in the U. S." Weinburger added that, after an expenditure of nearly $500 million during the life of the program, "there is little evidence that, on a nationwide basis, the RMPs have materially affected the health care delivery system." (66)

The disparity of views about the RMP program increased. For example, RMP programs involving continuing education to practicing physicians were cited as being contrary to the public good since relatively affluent physicians should pay for their own skill advancement, particularly if that advancement led to additional income. The RMPs replied that transferring new medical care techniques was part of their legislative mandate.

Before the Congress could act on extension legislation, the Administration notified all RMPs on February 1, 1973, that plans for phasing out operations by midyear must be submitted by March 15, 1973. In addition, the Administration began impounding funds for a wide range of programs, including RMPs. RMP leaders and key members of Congress agreed with the general idea that provisions of the Public Health Service Act needed revision. The Congress overwhelmingly voted to renew the Act for one year in order to allow sufficient time for revamping the programs. In the face of an apparently sure veto override, Mr. Nixon signed the extension legislation into effect in late June 1973.

Additional confusions and uncertainties continued through the year of extension as various levels of the Administration argued that funds could not or would not be released before June 30, 1973. For example, some $6.9 million was released to the regions on the last day of the fiscal year with the stipulation that the funds could not be spent. Because the Administration anticipated the end of RMPs, no mission statement was prepared for fiscal year 1974, despite the release of funds. A mission statement was issued in September 1973, and five program areas to which RMPs were to be "restricted" were announced: (1) quality care assurance, (2) emergency medical services, (3) hypertension, (4) kidney disease, and (5) development of new and more effective manpower utilization and training programs. In addition the RMPs were to provide assistance to areawide and state Comprehensive Health Planning (CHP) agencies in carrying out the provision of section 1122 of the Social Security Amendments of 1972. The original categorical diseases emphasis was noticeably missing in the new mission statement. (67) During this period, DHEW reorganized and the HSMHA was replaced by three bureaucratic units. Health Resources Administration, headed by Kenneth Endicott, M.D., became the overall umbrella agency for the RMPs. A succession of federal directors and reorganization of the RMP program overlapped the HSMHA and HRA periods and continued until its end with the line of succession moving from Stanley Olson, M.D. to Harold Margulies, M.D. to Herbert Pahl, Ph.D. to Mr. Cleveland Chambliss to Mr. Gerald Gardell and finally to Mr. Kenneth Baum.

THE HRA PERIOD AND TERMINATION

Although Congress had appropriated $90 million for RMP for fiscal year 1974, the Administration released only $17,100,000 to the Regional Medical

Programs in September of 1973 to cover operations from July 1 to December 31, 1973. In midyear, DHEW released another $44,900,000 for Regional Medical Program operation for the remainder of fiscal year 1974, again requiring each Regional Medical Program to submit a complete funding application on short notice.

However, in September 1973, an attorney advised RMPs that the Administration was in violation of section 601 of the Public Health Service Act which stated it was unlawful to impound funds appropriated by the Congress. The National Association of Regional Medical Programs (NARMP) was formed and filed suit in the United States District Court for the District of Columbia on behalf of the RMPs against the Administration for release of the appropriated funds and relief from program restrictions. At this time, the total fiscal 1973 RMP impoundment was about $101.5 million. During the first part of fiscal year 1974, additional funds were impounded, bringing the total amount to $126 million.

On February 7, 1974, the court ordered the Administration to pay the $126 million of impounded fiscal 1973 and 1974 funds to the RMPs. The suit also relieved the mandatory termination date of June 30, 1974. In addition to these two points, the court also lifted the restrictions imposed by the Secretary of DHEW limiting RMPs to the five areas identified in the September 1973 mission statement. The order of February 7 became effective immediately and the court ordered the government to pay the costs of the suit. DHEW was to assist the RMPs to find ways to make the program as effective as possible in a manner consistent with congressional legislation.

Theoretically, the RMPs could now return to the development of programs in the area of categorical diseases. However, there was insufficient time

to plan major efforts. Credibility had been seriously eroded, local communities were less interested in working with a program that changed direction on an annual, or more frequent, basis; and a large number of valuable staff members and advisors had begun leaving the RMPs. Ultimately, Congress passed PL 93-641, the National Health Planning and Resources Development Act of 1974, a controversial and little understood act. This act mandated a transition of RMP, CHP and Hill-Burton programs to new local structures to be responsible for planning, development and regulation in local health care systems. Some residual RMP activities remained and Congress included in a fiscal year 1975 supplemental appropriation $10 million for "crucial RMP projects" to provide stability of funding until other federal or local programs could assimilate them. A chronology of the significant events in the RMP efforts in cancer content is presented in Appendix D.

In the study of crucial projects conducted by the Public Accountability Reporting (PAR) Group in the spring of 1976, only seven cancer control projects were identified. Each crucial activity had to meet one of two criteria: (1) no continuation support or specific mechanisms had yet been identified for orderly transition from old to new federal programs; and (2) where orderly completion or new funding support were thought to be reasonably possible, neither were likely to occur until after current resources had expired. (68)

Subsequent to withdrawal or discontinuation of RMP funds, many cancer control projects have continued to function, have been largely self-supporting or have been merged into activities of the National Cancer Institute. (69) While the RMP program fell far short of original hopes and expectations as a vehicle for cancer control activities, several strategies and specific projects emerged and are deserving of further discussion.

SECTION II: PROGRAM EXPENDITURES AND OVERALL EVALUATION

FUNDING HISTORY

RMP funding and expenditures analysis is complicated by varying length grant periods, the numerous administrative changes, assorted funding methods, and the lack of availability of complete records concerning project funding. Additionally, consistent program costing techniques were not used by local regions except in special studies conducted by the Public Accountability Reporting Group. Nonetheless, sufficient information is available to estimate reasonably the amount of total RMP dollar investment in cancer activities during its life. The patterns of investment reflect the administrative shifts in mission at the federal level.

Although the RMP authorizing legislation was passed and limited funds were made available in federal fiscal year 1966, the first RMP projects were not funded until April of 1967. At that time, only three of the fifty-six regions funded their first projects, few of which were cancer control projects. Funds expended prior to April of 1967 were used primarily by the RMPs for planning, organizing and employing local staffs.

Although cancer was one of the three diseases originally targeted by the legislation, cancer projects never received a large share of RMP funds. Between July 1, 1965 and June 30, 1975, more than one-half billion dollars ($618.4 million) were made available to the program. Of that amount, (based on as complete federal records as are available) the best estimate is that about $34.5 million (5.6 percent) was awarded to local projects whose single-purpose target disease was cancer. However, numerous other RMP projects devoted some portion of their activities to cancer control. For example, in early RMP projects large-scale efforts to provide general continuing medical education often provided workshops on techniques in cancer care. (70)

In 1974, a national study of RMP efforts in cancer control for the time period of July 1, 1971 to June 30, 1974, revealed the following:

1. $10.4 million was awarded to projects whose only target disease was cancer;
2. An additional RMP investment of $1.3 million was spent in cancer control activities by the RMP program staff and projects which had some cancer control effort, such as continuing education projects; and
3. A total of $16.0 million of cost sharing was provided for RMP cancer control projects by outside funding sources. (71)

An analysis by year of the RMP funding for single-purpose cancer control projects only, yields the information contained in Table I below.

TABLE I

RMP CANCER PROJECTS
FUNDING HISTORY (72, 73)
July 1, 1965 - June 30, 1975
(in millions of dollars)

FISCAL YEAR	TOTAL RMP AWARDS	CANCER CONTROL PROJECTS	% OF TOTAL RMP AWARD
1966	$ 2.1	$ 0	0
1967	27.9	.4	1.5
1968	43.6	4.0	9.3
1969	72.4	7.3	10.1
1970	78.2	6.5	8.4
1971	69.7	5.7	8.2
1972	140.0	3.9	2.9
1973	54.5	3.1	5.7
1974	80.0	2.5	3.2
1975	50.0	1.1	2.2
TOTAL	$618.4	$34.5	5.6%*

*This represents the total percent of RMP award dollars which went to cancer control and not a sum of the Percent of Total RMP Award column.

The Administrative changes which took place in 1968 were not reflected in the fiscal 1969 budget for cancer control activities. In fact, the largest effort in cancer control projects occurred in fiscal year 1969 both with respect to total dollar volume ($7.3 million) and as a percentage of the total RMP funds available (10.1%). Impact of federal administrative changes did not begin to appear until fiscal 1970 and is reflective of review and approval time lags. Generally, a year is needed to implement program changes before any change is reflected in expenditures information. Fiscal 1970, however, did show some significant changes in RMP funding for cancer control activities. While the total RMP award was almost the same, cancer activities received almost a one million dollar cut and the proportion of the RMP funds dropped to 8.4%. This trend continued and the dollar volume for cancer control declined every year after fiscal 1970, a reflection of growing administrative changes in mission, growing disenchantment with RMPs as a vehicle with sufficient stability and funds for cancer control and initiation of the new NCI control program. The majority of the funds for cancer control programs in the latter years of RMP was usually continuation funds for projects initiated prior to 1973. (74)

An analysis of RMP funding by three administrative eras which are defined by the changes in Directors of DRMP, yields the information contained in Table II, "Total RMP Cancer Control Project Investments." For purposes of project funding analysis, the administrative eras have been extended one year for Dr. Marston and Dr. Olson since awards for the following year were approved prior to the change in the administrative agency. (75) Estimates of the non-RMP dollars invested in RMP cancer control projects are given and are based on a previous PAR study. (76) The ratio of local cost sharing funds, through no

TABLE II

TOTAL RMP CANCER CONTROL PROJECT INVESTMENTS (77)

July 1, 1965 - June 30, 1975

(in millions of dollars)

	TOTAL RMP AWARD	RMP CANCER CONTROL AWARDS*	PERCENT OF TOTAL RMP AWARD	COST SHARING ESTIMATE	EST. TOTAL EXPENDITURES
Marston Era (NIH Period) February, 1966 to June, 1969	$ 73.6	$ 4.7	6.4%	$ 6.4	$ 11.1
Olson Era (Early HSMHA Period) July, 1969 to June, 1971	150.6	15.3	10.2%	20.9	36.2
Era of Frustration (Later HSMHA and HRA Periods) July, 1971 to June, 1975	394.2	18.6	4.7%	25.5	44.1
TOTAL	$ 618.4	$ 38.6	6.2%**	$ 52.8	$ 91.4

NOTE: During the Marston Era, a considerable portion of the total RMP award was used for planning grants by the developing RMPs. Since there was a time lag between approval and funding, many of the cancer control projects approved during the Marston Era received most of their funding during the Olson Era.

*The figures are best-estimates which include all known project awards to cancer control activities plus estimates made by the Regions of the dollar costs and of RMP program staff time plus projects which were only partially cancer control projects (such as general continuing education projects). (78)

**This represents the total percent of RMP award dollars which went to cancer control and not a sum of the Percent of Total RMP Award column.

matching funds were required by DRMP, was somewhat higher than one-for-one. Thus, the figures in Table II represent an estimate of the maximum amounts of all RMP resources plus local resources devoted to specifically identified cancer control projects.

The areas of project emphasis with NCI's cancer control intervention categories is summarized in the PAR Analysis (79) in Table III below.

TABLE III

RMP PROJECT EMPHASIS
BY NCI INTERVENTION CATEGORIES

July 1, 1971 - June 30, 1974
(in thousands of dollars)

AREA OF EMPHASIS	FY 72 $	% of FY 72 $	FY 73 $	% of FY 73 $	FY 74 $	% of FY 74 $	TOTAL $ 72-74	% of TOTAL $
PREVENTION	$ 83	1.3	$ 40	.9	$ 7	.6	$ 130	1.1
DETECTION	1,313	21.1	870	19.9	239	20.5	2,422	20.6
DIAGNOSIS AND TREATMENT	2,455	39.4	1,736	39.7	370	31.8	4,561	38.8
REHABILITATION	712	11.4	523	12.0	166	14.3	1,401	11.9
EDUCATION AND TRAINING	1,674	26.8	1,199	27.5	381	32.8	3,254	27.6
TOTAL	$6,237	100.0%	$4,368	100.0%	$1,163	100.0%	$11,768	100.0%

Table III indicates that the majority of RMP funds was expended in the traditional intervention areas of Diagnosis and Treatment, Education and Training, and Detection by descending magnitude of expenditures. Since the level of expenditure in each area was relatively stable over the three years

studied, it may be assumed that the pattern was relatively constant throughout the RMP experience.

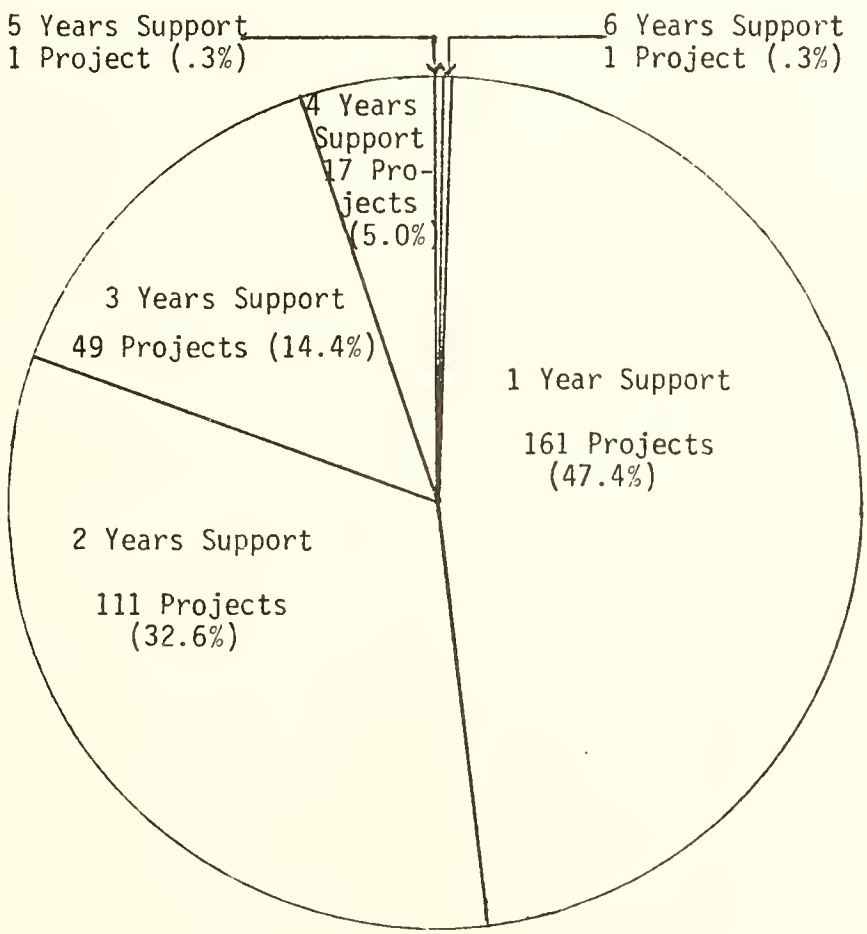
Of the 340 RMP cancer control projects specifically identified in this review, 321 (94.4%) projects received three years or less of support. Nearly one half of the projects (47.4%) received only one-year of funding. These funding patterns reflect the shifts of RMP mission.

The distribution of projects by the number of years of support are presented in Figure 1 below.

FIGURE 1

RMP CANCER CONTROL PROJECTS BY
NUMBER OF YEARS OF RMP FINANCIAL SUPPORT (80)

(340 Projects)



This review of the funding history of RMP cancer control projects indicates a modest amount of total resources dedicated specifically to cancer control, a declining trend over the ten-year life span, and a pattern primarily of short-term (less than three years) investment. Substantial community and other funds to accompany RMP project awards were attracted and a number of projects partially targeted at cancer control were conducted.

EVALUATION

While individual studies of numerous RMP cancer control projects were done, the funding and mission instability of the total RMP program frequently led to a lack of systematic application of evaluation research techniques to assess impact of the projects on the health care system and/or the targeted populations. Because of the great variability of the projects and the lack of specific national goals, evaluation research approaches were not used nor could they have been.

The Regional Medical Programs operated within a framework of changing objectives, unstated national goals and the inability to control major factors influencing local health care systems. Moreover, the purpose of seeking evaluation of Regional Medical Programs was not solely to justify or control but also to learn; i.e., to gain information which would allow for relatively quick alteration of behavior by both individuals and organizations engaged in systems improvements projects.

On a regional evaluation level, the common orientation in many RMPs to begin projects quickly strengthened the tendency to engage in very specific, limited objective projects rather than more complex and comprehensive regionalization programs since the former were easier to "evaluate" and "justify or control."

The Regional Medical Program experience reinforces a view that there is no single way to do evaluation of health resources development programs. In the RMPs, evaluation largely became regarded as a management technique rather than research into the value and outcome of community demonstration projects. Evaluation was seen as most useful when it occurred on an on-going basis and where there was provision for redirecting or otherwise managing projects based on findings. This view recognized, in a pragmatic way, that evaluation should not be viewed as an end in itself.

In a world similar to that inhabited by the Regional Medical Program, the end result of evaluation should probably focus also on learning; e.g., "Are the projects effectively accomplishing specifically stated objectives and, are they serving as an effective intervention in the fabric of community health systems improvement?" Learning of this nature not only provided RMPs with guidance for redirecting local projects, but also with summative evidence regarding project impact and outcome. Cancer control projects for which evaluation research designs were carried out aimed at assessing, for example, project associated changes in five-year survival rates. These project evaluations were, however, fewer in number than project evaluations in which the major purpose was management control and redirection of project activities.

There were, nonetheless, a considerable number of project-level evaluations regarding patient outcome in which the more competent local RMPs and project directors tended to develop and carry out relatively sophisticated strategies. A sample extract from the RMP evaluation of cervical cancer detection project conducted in Alabama between 1963 and 1969 illustrates that level of sophistication. The extract states that:

> Evaluation of the project's effectiveness in providing early diagnosis and treatment of cervical malignancies is made largely by comparing the changing pattern of incidence of invasive carcinomas with in situ carcinomas. For the five-year period prior to the inauguration of this project (1958-1962), records on indigent cancer patients admitted to the hospital showed in situ incidence of 33%, with 77% invasive. By 1967, after the project had been in progress five years, in situ incidence had increased to 57% and invasive had decreased 43%. Utilization of the program is also evaluated by comparison of the total number of patients screened with the total number of indigent female patients in the county. Effectiveness of follow-up is evaluated by comparison of the number of patients scheduled for return examinations with the number who keep their appointments. (81)

Projects which were specifically designed to bring direct impact upon population groups were few in number. The installation of a cobalt unit and new services in Alaska, for example, did provide a new treatment mechanism for a population whose cancer patients otherwise would have had to travel many thousands of miles to the nearest facility. No formal study of population impact was done, however. Similarly, RMP initiated provision of dosimetry radiation therapy services, individual medical consultation and specific assistance to peripheral hospitals having cancer diagnosis and treatment facilities undoubtedly made an impact upon cancer treatment and treatment resources in approximately 17 defined populations in the nation, but systems evaluation of these projects was not done at a sophisticated level comparable to the project activities themselves.

In the early years of the Regional Medical Programs, the st ess which was to come on an evaluation research orientation in project activities had not yet made itself felt. While this orientation and an associated local capability were to become an important aspect of the ᴰ qional Medical Programs during their middle years, there were at that time, few cancer control projects.

A number of projects by the various RMPs were undertaken in a relatively quick response to community demands and/or to subjectively observed or "felt" needs. Additionally, since very little adequate baseline data existed in most of the nation (patient flow patterns, services utilization, adequate tumor registry information), a number of RMP projects themselves aimed at creating concurrently these kinds of data, particularly in geographic areas with limited comprehensive health planning capability. In "quick-response" projects, insufficient attention was paid generally to specification of outcome-oriented objectives, and even less to development of formal evaluation research designs. Thus, as evaluation became more important to the Regional Medical Programs, attempts at more rigorous evaluation met with frustration due to inadequately designed or altogether missing objectives in the original proposal. Local organizations frequently discovered that little could be done in these situations to adequately evaluate activities from a service impact perspective.

As the emphasis on evaluation as a management tool increased, however, there were changes in evaluations of already established and ongoing project activities. Newly imposed evaluation demands brought about the formulation of more precise objectives for ongoing projects as they came up for renewal. By the early part of the HSMHA era, most local regional organizations had developed and implemented sophisticated project fiscal and progress monitoring systems. It then became typical practice to assess project progress regularly to redirect activities, to adjust budgets, and to terminate "unsuccessful" projects. This local management practice tended to force the development of more sharply defined objectives and evaluation procedures for new projects and, in turn, resulted in better controlled project activities in the later

years of the RMPs. Patient and population impact studies remained few, however, due to the lack of stability of funding and mission.

Aggregate Output Studies

The only regular attempts to document the aggregate performance of the RMPs, began in 1973 when a leadership coalition of RMP coordinators and evaluators formed the Public Accountability Reporting (PAR) Group, a cooperative project of all RMPs and the federal RMP program. The purpose of the PAR Group was to gather, analyze and report data concerning, on a national basis, the accomplishments of the RMPs.

While several studies of aggregate output of the RMPs were made, most did not occur until years after the larger investments in cancer control projects. (82, 83, 84) For analytic purposes, "projects" in these studies were defined as one-year segments of one-, two-, and three- or more-year projects. Results of the output studies point to impressive numbers of successful RMP initiated demonstrations which were incorporated into local health care systems, of significant numbers of medical and allied health persons trained, and of specific regionalization arrangements made.

The information presented in Table IV on the following page is a summary of the output data for all RMP projects for selected calendar years between 1970 and 1975.

TABLE IV

People Served and Health Manpower Trained by RMP Projects
(Selected Calendar Years) (85, 86)

	People Directly Served	Existing Health Manpower Trained	New Health Manpower Trained	Estimated People Served By Manpower Trained
Calendar 1970	6,000,000	86,000	7,500	20 million
Calendar 1972	10,000,000	107,000	14,000	30 million
Calendar 1973	7,000,000	114,000	12,000	14 million
Calendar 1974	6,000,000	109,000	20,000	13 million
Estimated 1975	Not computed	140,000	25,000	18 million

Note: In these studies, "People Directly Served" were patients who were provided direct care by RMP demonstration project personnel. "Existing Health Manpower Trained" were physicians, nurses and allied health personnel who were functioning in an established role. "New Health Manpower Trained" were practitioners who were trained in new roles, such as oncology nursing and cytologic technology. "Estimated People Served By Manpower Trained" were annualized estimate of patients seen by the providers during the year following training.

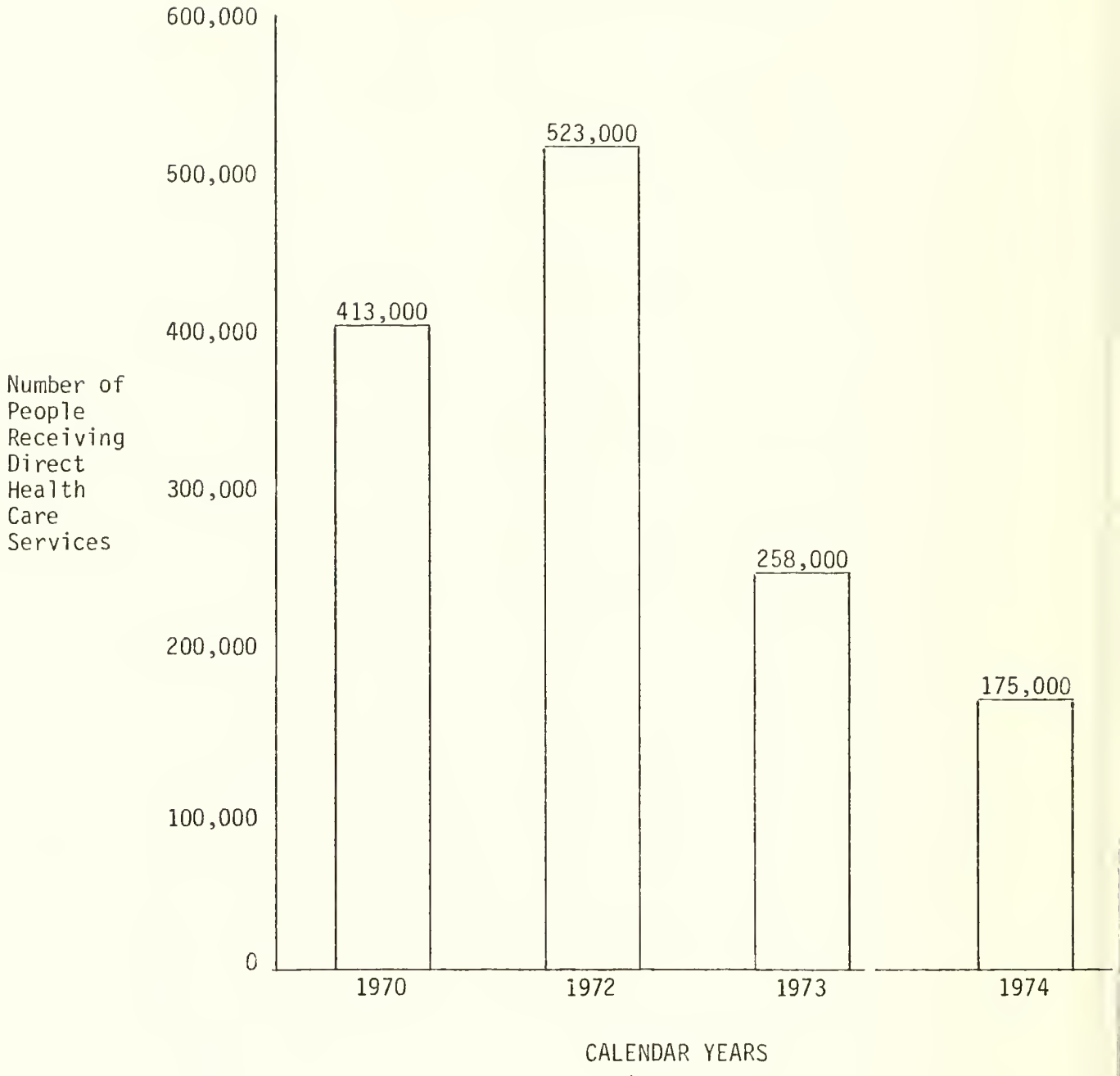
Within the category of "people directly served," information was available on cancer projects as a separate group. The number of people receiving direct health care services in RMP cancer control demonstration projects is provided in graphic form in Figure 2 on the following page.

As an indicator of aggregate achievement the overall output studies collected information reflecting activity in carrying out the local roles of convening and stimulating "cooperative arrangements." Between July 1971 and June 1975, for example, the local RMP professional staffs provided some 20,000 instances of "technical assistance" to other local health organizations, including cancer control projects, for the development of community health resources. "Technical assistance" was a term used to describe a variety of activities in the use of RMP staff and volunteer expertise among

FIGURE 2

People Receiving Direct Health Care Services
From RMP Cancer Control Demonstration Projects (87, 88)

(Selected Calendar Years)



various elements of local health care systems. In effect, this sharing took the form of staff assistance, for example, in project planning and management evaluation. Provision of services of this kind was a key part of the community-coordinating function the local RMP staffs and volunteers provided to the regions health care community. An end result of the projects and the community-coordination function was numerous formal "cooperative arrangements" among multiple health interests and/or new health organizations such as clinics, rural health stations, and health planning agencies. More than 1,000 instances of these results were reported in fiscal year 1975 by the RMPs. (89, 90)

With few exceptions, however, these aggregate output studies of the RMP program did not, unfortunately, separate cancer control related projects from general effort. As previously mentioned, the only comprehensive output-oriented study made of all RMP initiated cancer projects was carried out in 1974 through independent contractor to the NCI. In this study, (91) between July 1971 and December 1973, of some 300 RMP initiated cancer projects which were terminated for RMP funding, at least 116, about 40%, continued their activities using funds from other sources. Of the remaining 184 projects, most were completed while others were terminated by the local RMP because of insufficient progress. Most of the 116 projects which continued had multiple sponsors and funding sources. The 116 projects had a total of 254 funding sources and a total of 165 separate sponsors. Approximately $7.3 million of non-RMP funds annually were used to support continuation of these projects when the study was conducted. (92)

A number of the projects aimed partially at improving cancer care and can be generally classified as continuing professional education projects, where some of the project activity was concerned with teaching techniques in the management of cancer patients.

These educational projects varied widely in scope and relatedness to cancer control. Nationally, almost all RMP continuing professional education programs were under the aegis of medical schools. For some schools, large-scale continuing education was a new and somewhat hazardous undertaking. Never before had some become involved in community outreach programs in-depth, and a number of medical schools were fearful that their own out-of-pocket costs would rise, requiring a reduction in resources for basic curriculum and research. Paul Ward, California RMP Director, recalled, "There was a feeling reported from some schools that funds could be better spent in basic cancer research and medical education rather than in being heavily invested in community-level improvement of quality care." (84, 93) Many other schools, however, already involved in continuing education programs for the professions, were in a position to rapidly expand their programs. In the areas of cancer control projects, there were symposia, traveling lecture and consultation teams, audiovisual tape production, telephone consultations on specific cases, educational programs in dosimetry networks, short-term refresher courses and communication networks for information sharing.

It is reasonable to assume that cancer patients ultimately benefited through improved diagnosis and treatment disseminated through these widespread continuing education projects, which were in operation in almost all RMP regions. Adequate overall evaluations linking teaching to use of new techniques of such efforts were not done, however. To do so would have required an extensive, complicated effort at a time when the RMPs were in the midst of program mission shifts.

Comment

Other RMP initiated projects which are known to have led to individual systems changes in various locales were the tumor registry development efforts carried out in numerous regions. The tumor registries provided a community basis, in some cases, for the current Surveillance, Epidemiology and End Results (SEER) program of the NCI, a sophisticated data base which may permit better overall program evaluation.

The complexities, progress and problems of evaluation in RMPs have been addressed by several writers. Ginzberg, for example, stated that,

> RMPs were a federally funded set of locally designed and administered programs bounded by non-specific legislative goals, shifting administrative priorities and liberal guidelines displaying considerable diversity. If it was difficult to demonstrate a clear return from the mass of projects undertaken, it was impossible--surely in the short run--to demonstrate the value and potential of the process function. At its height in 1972, RMPs nationally involved close to 16,000 individuals serving in a voluntary capacity as advisors, consultants, and committee members; throughout the ten-year life of the program the number may have reached 40,000 to 50,000. From within this group a cadre of leaders at the state and local level emerged who were surely unavailable at the program's start. However, such a sensitization function, in the absence of more concrete benefits, gains no constituency, and cannot continue to attract the flow of resources required for its survival. (94)

Other complexities in the environment in which the RMPs existed worked against rational overall evaluation. For example, confusion was widespread both in communities and at various levels of federal administration because an adequate explanatory framework was never developed and/or promulgated regarding the differences between RMP planning for specific services development and public policy level comprehensive health planning. To cite lessons applicable to the future, retrospective judgment of the value of RMP approaches needed to be done on a project basis as well as an overall output or legislative/administrative basis.

Based on the above review of the status of evaluation of all the RMPs, it is appropriate to move to a closer look at specific RMPs as examples of the cancer control efforts. Such a presentation of examples should also clarify some of the problems inherent in attempting to evaluate the overall RMP experience.

SECTION III: EXAMPLES OF RMP CANCER CONTROL EFFORTS

EXAMPLE SELECTION

Since little overall evaluation of RMP cancer control activities was conducted, three regions were selected to provide a variety of specific project examples which illustrate cancer control activities at the individual region level.

While it would be desirable to assess all regions' efforts in cancer control, a lack of systematic evaluation prohibits comparative judgments of regions with respect to cancer control projects. The three regions were chosen for illustration on the basis of geographical distribution and the expenditure levels for cancer control activities.

The constructed specification and the regions chosen to exemplify that specification are as follows:

1. A region was sought which had a variety of projects and contained a large population base. California was selected as an example of a region which developed a plan for systematic and comprehensive cancer control program in one of its geographic subareas. Implementation of this plan was then delayed due to the lack of funds at the national level.
2. A region was sought which appeared to have invested a very small amount of its resources in cancer control programming. A review of federal expenditures records indicated that Connecticut RMP was a good example. In addition, the Connecticut RMP was also a region which initiated programming on the basis of a well-defined regionalization strategy.

3. A region was sought which had placed a comparatively large amount of its resources in a single cancer control project. Mountain States RMP was a region which invested almost one million dollars in a single project over a five-year period and was therefore selected.

The California Committee on Regional Medical Programs (CCRMP) example reviews the development of and problems caused by a unique approach to a statewide organization which established nine autonomous subregions. The example is then reviewed in the context of the nine subregions. Some of the regions engaged in the development of a comprehensive cancer control plan only to be thwarted before the RMP plan could be implemented.

The review of the Connecticut RMP provides an overview of a regionalization strategy and demonstrates how a small investment in cancer control fits within that strategy. The results of the small cancer control investment are of interest and serve as an example of the unrecorded accomplishments of RMP efforts.

The Mountain States RMP review provides some detail about a multistate organizational effort to develop a large-scale cancer treatment center in a small population-base area.

CALIFORNIA COMMITTEE ON REGIONAL MEDICAL PROGRAMS

California was one of the few states in the country which a ready had the components for the building of a highly sophisticated cancer control program at the time of the enactment of Regional Medical Program legislation. With a long-established, prestigious medical school in the southern portion of the state and one in the Bay area as well as two new schools of medicine being organized, there were many highly qualified people to assist in the development of such a program. In addition to the two degree-granting schools of public health and a respected cancer-oriented chronic disease division in the state department of health, there was available an abundance of experienced manpower highly knowledgeable in the cancer field.

California had been the recipient of many U. S. Public Health Service and American Cancer Society awards and grants for post-graduate training in specialties dealing with cancer. A training school for tumor registry technologists--the only one in the country--was developed at the University of California in San Francisco for the training of well-qualified individuals to supervise tumor registries. This program had become the "mecca" of the world for high-quality training of such personnel.

Many years prior to the initiation of RMP, the California Association of Pathologists had established a cancer tissue registry which became an outstanding consultative service to California physicians. It continues to the present time solely supported by the state pathologists.

The roster of the American Cancer Society contains names of three illustrious California physicians who served as President of the American Cancer Society - Drs. David Wood, John Cline and Justin Stein. These, in addition to a host of other physicians as well as lay persons, have served as outstanding leaders in many capacities on the National Board of Directors of the American Cancer Society or on its committees and subcommittees.

In December of 1966, California became the fifth RMP nationally to receive a planning grant. California was the largest single state Regional Medical Program with respect to population size. The state had a highly developed and complex health care delivery system richly endowed with health resources. The physician-to-population ratio was among the highest in the nation, and the eight (later nine) medical schools began to participate in RMP from the program's inception. The funding level of the California RMP was consistently higher than any other RMP's, exceeding $14 million (10 percent of total program appropriation) at its peak in 1973.

To begin planning for the establishment of an RMP in California, a meeting of the deans of the medical schools in California and the director of public health was called. They were joined by representatives of the California Medical Association and by some of the voluntary health associations. This composite group eventually became the nucleus of the California Committee on RMP. Both the California Hospital Association and the California Medical Association offered their services to act as grantee for the RMP grant. The CMA made $20,000 available as financial security to California Medical Education and Research Foundation (CMERF), and the research foundation of CMA became the recipient of the grant. The CMERF Board was expanded to include representatives not only of the original five medical schools but also three additional, newly established medical schools.

The relationship between the CCRMP and the CMA fluctuated with the annual change of officers in the CMA but was generally considered good. Strong relationships were established with the California Heart Association, each of the medical schools, and the American Cancer Society. (95)

Although the California RMP was technically a statewide program, it consisted of nine area programs each of which was located in and directed by a medical school. The Regional Advisory Group for each area program was an autonomous body which proposed projects and had specific priorities for its area.

It was expected that the Regional Advisory Group in each of the nine areas in California would draw upon the skills and knowledge of the American Cancer Society and other experts to rapidly form a well integrated statewide cancer control program. However, Regional Advisory Groups were not able to prevail against the forces of local autonomy to achieve this cohesion and thus a prime source for designing, developing and implementing such a cancer program was not utilized.

The presence of the medical schools was a significant determinant of both program emphasis and structure. The program emphasis closely followed changing national priorities and, initially, California's RMP program was heavily directed at categorical diseases. One of its principal areas of early operations was categorically-oriented continuing education, including advanced training in coronary care for physicians and nurses and the development of specialized team approaches to treatment of stroke victims.

Dr. Charles White, the CCRMP program director, indicated that if categorical disease support had continued to receive emphasis at the national level and if California had begun to develop a cancer program with a statewide committee, it would have been possible to develop a cohesive cancer program in the entire state by the early 1970s. (96) *Such a statewide program may have been possible because the CCRMP reorganized in 1973 and the nine areas were phased out and replaced with one statewide organization.*

Mr. Paul Ward, the CCRMP executive director, felt that during its ten-year life span, the CCRMP exerted tremendous effort toward bringing cohesiveness to health care programs, and had it been allowed to continue, the benefits would have been even more substantial. (97) *The CCRMP provided an organized leadership mechanism for California which included resources, funds and prestige. The relationship between the State Health Department in California and the CCRMP was always excellent. The State Health Department participated on the CCRMP committees, was cooperative and was helpful. The CCRMP developed a number of programs involving the State Health Department.*

It was Dr. Samuel Sherman's opinion (98) *that to establish a good statewide cancer control program in California one should follow the prototype that was established in Northern California and institute it on a statewide basis; i.e., mobilize local leadership and take the expertise from the medical centers out to the areas where it is needed.*

Although nearly every area had some cancer project, only Area I - the University of California, San Francisco; Area III - Stanford University; and Area VIII - the University of California, Irvine, had extensive cancer control projects. The programs in these areas are summarized below.

Area I - The University of California, San Francisco

Area I, based at the University of California, San Francisco, mounted an extremely extensive planning effort. They attempted to produce a comprehensive cancer program in that area.

The earliest of the cancer related projects was in San Francisco, where the Area I Regional Cancer Program intended to improve cancer care through consultative and educational services to supplement and improve existing resources.

Area I developed and received full funding for one major project (project 15 phase I) of their comprehensive cancer control program. The four major activities which comprised the project were:

1. *Clinical cancer consultation service provided consultant teams to participating hospitals. Teams were made up of specialists in radiation therapy, chemotherapy, surgery, and nuclear medicine. One hundred fifty physician visits to outlying areas were made, serving approximately 600 patients during the first two years of operation.*

2. *Radiological physics services assisted local facilities within Area I to use radiotherapy equipment in an optimal manner and involved 500 physicians and nurses.*

3. *A Computerized Data Retrieval Service was developed and tested. A process was devised for entering patients into the record system; typewriter and telephone linkages were established, and a central computer connected remote sites. Record forms were completed and five hospitals participated in the system with assurance that clinicians would cooperate in completing necessary forms on each patient.*

4. *Educational programs were offered to medical, paramedical and lay persons through one-day cancer workshops, telephone consultation and preceptorships. Participants numbered more than 1,200. In addition, 15 students were trained as radiotherapy technologists in an ongoing program established at the San Francisco City College.*

The second major project in Area I (Phase II, Project 44-Medical Oncology Program) was designed to supplement and enhance Phase I (Project 15). This project was approved by the Regional Advisory Group but was not implemented due to a lack of funds. Phase II had four components: (1) a cancer education program for physicians using centralized and decentralized university-level postgraduate programs; (2) continuing education for nurses that would be tested through systematic variation of learning approaches to determine the most effective ways of keeping all levels of nurses up-to-date; (3) a regional communications forum for cancer specialists to act as a continuing resource for feedback and evaluation of ongoing programs in cancer by RMP; and (4) establishment of a service to provide radioisotopes to hospitals in the area, leading to a nuclear medicine bank or central pharmaceutical service using computerized inventory control of isotopes and their half-lives leading to reduced costs and enhanced local delivery.

From February through May of 1972, Area I proposed two additional projects for Phase II; 94A - Regional Cancer Program - An Extended Care Concept and 94B - Radiation Therapy. Both projects were funded.

Project 94A was intended to improve cancer care through a variety of continuing education and training programs, including radiation therapy conferences and full implementation of the cancer data retrieval system. The project, over a 12-month period, held seven conferences and seminars with more than 600 health professionals in attendance.

A completely different intervention strategy was proposed for Project 94B, which established a radiation therapy physics support and consultation program as a self-supporting service in Northern California. Physicists, dosimetrists, and radiotherapists from center in San Francisco provided regularly scheduled visits to physicians operating radiation therapy facilities. Installation assistance, equipment calibration, dosimetry determinations, treatment planning, and individual instruction were provided. Telephone, tele-communicator and computers were also used for additional consultations.

The project ran from November 1, 1972 through June 30, 1973. In an effort to make seminars and workshops self-supporting, participant/registration fees were charged to offset costs. Specific output reported by two provider institutions (University of California, San Francisco and Mt. Zion Hospital) included: 26 radiotherapy consultation visits; 189 radiophysics consultation visits, 132 dosimetry consultation visits, 174 physics consultation visits and 625 treatment plans. These programs became self-supporting after June 30, 1973.

Area III - Stanford University

The Area III branch of California RMP at Stanford University spent two years in specific needs assessment and project planning before submitting the proposal in 1970 for a comprehensive cancer program.

This program (Project 73) proposed four interrelated activities in the first stage of implementation of the long-range plan. Activities proposed were radiotherapy services at three hospitals, a district tumor board in San Joaquin County, an oncology unit at San Jose Hospital, and a consultative-teaching service among these three activities. However, local Technical Review Panels approved only the tumor board component, which continued in existence from September 1, 1971 through December 31, 1973. General objectives of the project were to provide consultative services and continuing education resulting in improved care for patients with cancer or suspected malignancies.

Area VIII - The University of California, Irvine

The final area in California that produced a cancer control effort was Area VIII - the University of California, Irvine. Although Area VIII did not propose a comprehensive areawide program, they did, in 1968, propose several specific projects; i.e., public relations and health education; information centers dealing with a dynamic registry, chemotherapy, surgical therapy, radiation therapy and dentistry; research projects such as genetic counseling, exfoliative cytology and pancreatic and gastric cancer; and rehabilitation and hospital discharge planning. Through a series of reviews,

at the local and national level, all components of the proposal were denied with the exception of the Dental Program (recommended at a lesser level) and the Discharge Planning Component.

In 1970, Project 72 - Community Program in Radiation Physics was submitted by Area VIII. Major revisions were required by the local Technical Review Panel. The project was eventually funded in 1972 to provide comprehensive continuing education in clinical radiotherapy and radiation physics, to improve the quality of care and to attain greater uniformity in terminology, techniques and dosimetry. The project was scarcely begun when reduced budgets and federal phaseout instructions occurred.

After 1973, the CCRMP consolidated all of the Area Programs into one statewide program. By this time, however, there was no emphasis being placed on categorical diseases and no cancer control activities were initiated by the CCRMP after 1973.

CONNECTICUT REGIONAL MEDICAL PROGRAM

Strategy

In Connecticut, a committee appointed by the Governor agreed to assume active leadership in the development of an RMP soon after Public Law 89-239 was enacted. This committee was composed of representatives from the major public and private health interests of the state. As a result of the efforts of this committee, one of the earliest planning grant applications received by the Division of Regional Medical Programs in 1966 was from Yale University requesting that the State of Connecticut be established as a region. (99) *The application was well-designed and lauded by the National Advisory Council as being a "model application." The planning grant application proposed a study of the state's health care system as the basis of RMP program activity and determined that planning would be on a non-categorical basis, asserting that an effective attack on the categorical diseases designated in the legislation could be mounted only via improvement in the delivery system characterized by two main features: the division of the entire state into ten health service areas to stimulate local planning and problem solving, and the development of two university-community hospital networks for the eventual affiliation of each community hospital with one of the state's two university health centers (Yale and the University of Connecticut). A central vehicle for this affiliation was the joint appointment of full-time chiefs of medical service to key clinical services in the community hospital. The community hospitals were encouraged to expand their scope of activity to that of community health centers, to take leadership positions in local health area planning and to work closely with other general hospitals and community agencies and practitioners. Beyond involvement of the universities in local service delivery via the networks, the Connecticut Regional Medical Program (CRMP) aimed at the development of university outreach into the community planning process through Community Studies Units which would provide technical*

assistance to localities for the analysis of health problems and the design of programmatic solutions. (100)

Reflecting these central objectives, the largest proportion of the DRMP funding annually was awarded to university-community hospital partnership projects. Only a fraction of these projects involved the three basic categorical diseases. These partnerships established a framework for the development of a variety of specialty care programs and joint training and research projects. At the termination of the CRMP, this development of cooperative efforts represented the CRMP's most conspicuous success. All but one of the state's community hospitals were linked into a university network and there was a substantial increase in the presence of full-time chiefships, filled or authorized. Regional linkages between university based and community physicians within specific subspecialties had also been achieved, and some extension of the university's traditional educational and research activities into the community was developed. (101)

Although the formal University Community Studies Units had not been maximally developed, reflecting university ambivalence regarding its role in this area, the CRMP had nevertheless funded a considerable number of individual research projects for purposes of local health planning whose potential for implementation was enhanced by local involvement in the development of information. Another area of some success was that of health manpower training. After 1970, the CRMP discontinued funding for continuing education of physicians and dentists, concentrating on nurses and allied health occupations and developing a statewide training consortium.

Consistent with the commitment to subregionalization implicit in its establishment of health service areas, the CRMP viewed itself as a predecessor, functionally, to Comprehensive Health Planning (CHP) and was instrumental in the development of local ("B") agencies. (102)

Conducive to the success of the CRMP in the implementation of its primary regionalizing objective was the particular pattern of involvement of the universities. Although Yale was designated as grantee, the concentration of effective power was within the executive committee of the RAG, which included significant representation of non-university providers, particularly community hospitals. Thie structure won cooperation from this normally distant private sector elements and was achieved in the wake of prolonged negotiations to overcome the initial resistance of the Connecticut Medical Society to what it perceived as dominance by the university and other non-private sector interests. In the actual implementation of structural and programmatic changes within the community hospitals, the availability of reimbursement for the increased costs was a powerful adjuvant. Crucial to the universities' initial stimulus to activation of the regional affiliation program was the pre-existent sophisticated level of health care within the state, notably universal accreditation of the community hospitals and virtually universal hospital staff appointments for practicing physicians. The latter simultaneously induced physician support. (103)

The small total number of community hospitals, the limited geographic size of the state, and the presence of an efficient road system made the effort feasible. The preliminary exhaustive study of the state's medical care system in which the leadership--public, private, and academic--of the state's medical care system collaborated, and the conscious effort of the CRMP leadership to engage universities in a nondominating relationship with the community and the providers, were significant determinants of programmatic achievements. (104)

Cancer Control

Turning specifically to cancer control in a strategy context, one of the products of the CRMP planning grant period was a proposal to study and devise a relatively small, two-tiered system of radiation therapy which would consist of four regional centers in the state and two university centers. The proposal included interaction between the universities and the regional centers. However, the Connecticut State Medical Society regarded this effort as a most dramatic illustration of infringement upon medical practice. Discussions with the medical society led to the withdrawal of this proposal. (105)

The CRMP realized that the universities and practicing physicians eventually would have to come to grips with the problems associated with nonsystem radiation therapy. Therefore, in 1970, the CRMP funded an outside agency, the Connecticut Hospital Planning Commission, to identify possible solutions to the problem. (106)

The Commission studied each hospital in the state, with emphasis on those having radiation therapy equipment. The study carefully examined the cancer patients who were referred for radiation therapy and the equipment, personnel, supporting services, population served, educational and research activities, utilization and patient referral flow patterns of each hospital.

A steering committee of the Hospital Planning Commission was appointed and included representatives from 11 different health agencies. A final report was produced and was adopted by the Connecticut Hospital Planning Commission in January of 1972. Upon adoption, the report became the Commission's frame of reference for the following four years in reviewing any hospital programs in radiation therapy. The report also became the standard which the State Council of Hospitals utilized in the discharge of certificate of need responsibilities.

During the following two-year period, the CRMP was able to build and strengthen linkages between university and community hospitals. The linkages provided enough statewide leverage in upgrading radiation therapy that 15 community hospitals became formally affiliated with Yale University. An additional 18 community hospitals became affiliated with the University of Connecticut.

Subsequently, Yale has been designated as a comprehensive cancer center under the National Cancer Act of 1971. Prior to that designation, Yale and the University of Connecticut had developed joint administration of radiation therapy. The two organizations developed plans for outreach throughout the state to involve all community hospitals. Through joint university cooperation, a new division of epidemiology was established and assumed responsibility for reorganizing and improving the statewide tumor registry which has been in operation continuously since the 1920s.

According to Dr. Edward Morrissey, the former Associate Director of the CRMP, the planning phase of the outreach service to all community hospitals has been completed and the plan is being implemented. The plan provides for full-time salaried chiefs of staff in community hospitals with direct consultative resources in university departments of radiation therapy. In addition, Yale has assumed, under contract, direct staffing and administration responsibility for a radiation therapy department of one voluntary cancer hospital and a community hospital. (107)

Another mechanism which has developed as a result of the CRMP planning and other activities that have linked previously unassociated health institutions is the combining of two community hospitals to provide single administrative and professional auspices for radiation therapy. Yale University provided consultative services and professional direction.

Thus, the original CRMP concept of improving radiation therapy on a voluntary reform basis through upgrading existing capabilities, eliminating expensive duplicative programs and providing a continuing source of consultation and education, has come to fruition and actually has expanded far beyond the original concept. The activities presently provide an excellent system of quality radiation therapy care for cancer patients. The results have been accomplished through an initial grant of approximately $18,000 to the Hospital Planning Commission, a moderate amount of CRMP staff time, the credibility of CRMP in the state, and effective use of regionalization techniques and overall strategy. This success is evidence of a major breakthrough achieved after several ineffective starts.

A second important facet in the CRMP's cancer thrust, which did not occur until 1974, was an attempt to transplant and nurture the hospice program at St. Christopher's Hospital in London, England, to New Haven, Connecticut. (108) *Leadership for this effort came from the chaplain at Yale University, who had spent several months of a sabbatical at St. Christopher's. Upon his return, his contagious enthusiasm moved the CRMP Regional Advisory Group to provide an award of $50,000 for the first year of a feasibility study.*

During the first year, a medical trade area with a 500,000 population base corresponding to the boundaries of a future Health Service Area was demarcated. Hospitals, state government and various regulatory agencies were consulted and plans formulated. During the second year, several family foundations made contributions to supplement the CRMP investment. The program was further developed and endorsement of certificates of need

were obtained. At the beginning of the third year, a home service care program for cancer patients was in operation. Medical and nursing staffs were acquired, and affiliation agreements were made with the community hospitals and nursing homes in the area. A fund raising campaign in the community and the obtaining of several grants, including one from the National Cancer Institute, have ensued. Land has been obtained, and a facility for the terminal cancer patient will be constructed. Volunteers have been recruited and the entire community is enthusiastic. The township is presently considering becoming the responsible fiscal agent for construction with a lease-back arrangement with the hospice.

The hospice program is filling a definite regional service need for the terminal cancer patient, caring for him at home until the time that institutional care is needed. An open-staff policy allows the individual patient-physician relationships to continue and also provides a basis for necessary and desirable consultative services with the Yale Comprehensive Cancer Center. (109)

Although the CRMP never was heavily involved in supporting cancer control programs, it is an excellent example of what can sometimes be accomplished with relatively small amounts of seed money and time, given the presence of an effective, "neutral party" catalyst for the development of health resources.

MOUNTAIN STATES REGIONAL MEDICAL PROGRAM

The Mountain States RMP (MSRMP) was formed in 1966 to serve the four states of Idaho, Montana, Nevada and Wyoming. The geographic area to be served was approximately 656,000 square miles with a population of about 2,000,000 persons. The grantee organization was the Western Interstate Commission for Higher Education, a thirteen-state public instrumentality.

The first task undertaken by MSRMP was a comprehensive assessment of health resources and needs. Based on this assessment, MSRMP initiated its first operational project in 1968. This project, as in many regions, was a coronary care project. However, planning for a major cancer project was underway at the same time the coronary care project was being developed. One year later, MSRMP initiated the development of the Mountain States Tumor Institute (MSTI) which was to become a model project. (110)

The MSTI, a regional diagnosis and treatment center, was started in Boise, Idaho, where it could provide cancer care services in a medical trade area of approximately 100,000 square miles in Southwestern Idaho, Eastern Oregon, and Northern Nevada. The population in this area was approximately 400,000 people. Planning for the MSTI started with the appointment of a planning committee by the Idaho Foundation for Medicine and Biology. This committee was formed about the same time in 1966 that Alfred M. Popma, M.D., resigned as Chief of the Department of Radiology at St. Luke's Hospital in Boise to become Director of the MSRMP.

A cancer treatment program at St. Luke's Hospital had been started in 1938 and had served as a basis for the development of the MSTI. The program included radiation therapy, an active tumor board and cancer registry as well as a training program for radiation therapy technologists. During the planning process, the committee endorsed the concept of a regional cancer center with sufficient resources and staff to provide a full range of patient care services and, concurrently, intensive and continuous educational programs for medical, dental, and allied health personnel. Educational programs were also to be made available to patients, their families and the general public.

During the project planning process, MSRMP conducted a study of patient referral patterns and resources of the 20-county medical trade area to be served. Hospital administrators and radiologists in this area agreed to cooperate in the development of a cancer center, to discontinue orthovoltage treatment of cancer patients in their respective institutions, and to refer cancer patients to the cancer center in Boise for treatment. In addition, the St. Luke's Hospital Board of Directors agreed to cooperate in the planning and development of the cancer center and also offered to purchase a building site to construct a free-standing facility and to donate the radiotherapy equipment in use at that time. The St. Luke's medical records would also be made available. Extensive use was made of knowledgeable consultants from surrounding medical schools to devise organizational structure of the center. American Cancer Society cooperation in planning was also quite helpful.

With the planning completed and numerous agreements made, MSRMP submitted an application for review and funding by DRMP. A budget of $2.5 million was proposed to cover a period of five years. (111)

After a site visit and consultation by DRMP staff, the application was approved in March 1969 by the RMP National Advisory Council for a three-year period but with considerable reduction of funding. (Eventually, almost one million direct RMP award dollars were provided to MSTI in addition to significant amounts of RMP staff assistance, leadership and support.) The funding reduction caused considerable revision in planning which resulted in the elimination of some equipment purchases and education programs. Maurice Burkholder, M.D., an internist in Boise, was named director of the MSTI. Dr. Burkholder soon recruited a radiation therapist and made preliminary moves toward obtaining a highly competent chemotherapist.

The affiliation agreement with the MSRMP (WICHE) was completed, articles of incorporation were filed, and the Mountain States Tumor Institute became a wholly owned subsidiary of St. Luke's Hospital. The Hospital's Board of Directors became the Board for the MSTI, but appointed a Policy and Planning Council to which was delegated the responsibilities of general overall operation of the MSTI. This ten-member Council was representative of the geographical area served by the MSTI, with ex-officio members from St. Luke's Board and staff from the MSTI.

Plans were developed for a 14,000 square foot facility connected to St. Luke's Hospital by a tunnel. The design incorporated space for outpatient radiation and chemotherapy treatment, clinical research, and educational programs. A chief of radiation therapy and a radiation physicist

were recruited. Tumor boards and treatment planning conferences were begun and in 1970 a coordinator of cancer nursing care and education was added to the staff.

The new building was opened in May of 1971 and soon thereafter a counselor for patients was hired who initiated a unique program of emotional support for cancer paitents and their families. The patient oriented research approach at MSTI is ideal for patient care and for science. The cooperative clinical trials at MSTI are primarily based on National Cancer Institute protocols and the feedback to NCI is an important part of the research picture. At the same time, the patients are treated according to the best-known protocols.

In October of 1971, a director of clinical research was added to the staff and over 100 patients were placed in chemotherapy treatment as part of a cooperative effort with other western cancer centers.

In late 1972, a conference with the Governor led to preparation of a funding proposal for submission to Idaho's legislature. This legislation, recommended by the Governor, appropriated $200,000 and was enacted to provide partial support for patient care, training, and research with emphasis on service to the citizens of Idaho. Subsequent legislative appropriations have continued to provide the MSTI with similar financial support.

Educational activities conducted by the MSTI included treatment planning conferences, basic science lectures, cancer symposia for physicians and nurses, nursing workshops, fellowship programs in oncology, radiation therapy technologist training, emotional support programs and outreach consultative programs to other cities in Idaho.

In 1972, cancer nursing education programs were conducted at outlying hospitals. An emotional support film was produced and presented in 12 states. Dermatology, gastroscopy and consultative programs with the Boise Veterans Administration Hospital were begun. Increased usefulness of a statewide tumor registry which was initiated by St. Luke's Hospital in 1964, was provided when the MSRMP funded the effort in Idaho in 1970. The MSRMP funds allowed the registry to improve its efficiency on a statewide basis and to become associated with the Rocky Mountain States Tumor Registry (RMSTR) in Salt Lake City, Utah. The RMSTR program provided the basis for efforts later funded through the Surveillance, Epidemiology and End Results (SEER) program of the NCI under the new National Cancer Law.

The MSTI was expanded in 1975 and an 18 MEV linear accelerator was added in 1976. Additional physician and supportive personnel in radiation therapy, chemotherapy, nuclear medicine, cancer nursing and education provided expanded programs in all areas.

With a modest grant from an Idaho legislative appropriation, a pediatric oncology program was developed in 1976 which will bring an interdisciplinary approach to childhood cancer. This program was aimed at pediatric patients in Idaho, Western Montana, Eastern Oregon and Northern Nevada. Currently, it is generally anticipated in the area that there

will be substantial growth in this program and that it will serve as an important response to regional needs in the overall cancer control program of the MSTI. (112)

The MSTI has developed visibility and strong support through interaction with the utilization of organizations, institutions and people in the community. Outreach from the close ties with St. Luke's Hospital to Boise State University, Veterans Administration Hospital, University of Idaho, College of Idaho, and the American Cancer Society have continued to enhance growth and development. The awarding of a core support grant from the NCI for a cancer center and the awarding of an NCI/ACS contract for one of the nation's 27 Breast Cancer Detection Centers have greatly assisted the MSTI to expand its sphere of activities.

The MSTI's contributions to cancer control programs at the statewide level helped bring about a one-cent per carton tax on cigarettes. A sum of $55,000 is annually earmarked by the Idaho Legislature as support for the tumor registry.

A review of the MSTI's funding during the period form 1969 through 1975, indicates that a total of $887,465 was received from the MSRMP. In addition, $378,581 was received from community interest in the MSTI. During this same period, patient revenues climbed to a total of $1,608,309. During the last two years of this period, $912,092 was obtained in grants and contracts to enhance new projects, plan expansion and provide additional patient services. Land, original construction and expansion renovation accounted for more than $800,000 from St. Luke's Hospital for the Mountain States Tumor Institute.

The MSTI, while not entirely self-supporting, is viable and growing and providing to a widespread community a series of services to cancer patients and their families. Much of these services were previously unobtainable except in far distant centers. Continued close cooperation with physicians will spread and advance the interdisciplinary approach to cancer diagnosis and treatment. Planning for and inauguration of additional screening programs will increase its visibility. (113) *The uniqueness and strengths of the MSTI and its responsiveness to regional needs have attracted the NCI core support and made it a model for the NCI to use in the development of other similar small centers throughout the country.*

Thus, the MSRMP efforts in cancer control (primarily the MSTI and the tumor registry) have developed into successful, ongoing cancer co itrol activities. Although there have been no impact studies conducted, in terms of cancer survival in the area served by these two programs, both activities are filling a definite need for cancer control efforts in an area where cancer care would, otherwise, not be available.

OVERVIEW OF THE SAMPLE PROJECTS

As is apparent from the three sample activities, a variety of approaches were taken by the individual RMPs to carry out cancer control and resource

programs. However, several kinds of approaches and processes were common to the RMPs. Commonalities include the use of many experts and establishment of new linkages to strengthen existing systems. Each of the RMPs operated under the program direction of a local Regional Advisory Group (RAG) and many organized similar, multiple-interest committees to design and guide specific projects in cancer control as well as program areas.

Creative and flexible involvement of multiple interests was a key community process element of the most effective RMPs. Since a local RAG included leaders from the region's health care establishment, it provided a powerful lever which had not previously been available for conducting joint efforts among various health care interest groups within communities. Projects which were authorized for funding by the RAGs had, in general, been politically and professionally "predigested" prior to their inception. Consequently, such projects tended to be more easily assimilated into the fabric of local health care services systems. Since the RAG and project committee members themselves were frequently potent political forces within a region, they were often influential in gaining commitments for cost-sharing and continuation support in addition to obtaining needed cooperation from other health interests.

Overall, this RMP community-coordination approach served essentially as a catalyst to initiate action likely to be successful and as the continuing "glue" that held multiple interests together to bring about collective solutions to locally perceived problems and needs in cancer control. With varying degrees of effectiveness throughout the country, the combination of active voluntary advisory group members and an active staff provided a forum and action mechanism for carrying out leadership and support functions for activities ranging beyond single institution or individual interests.

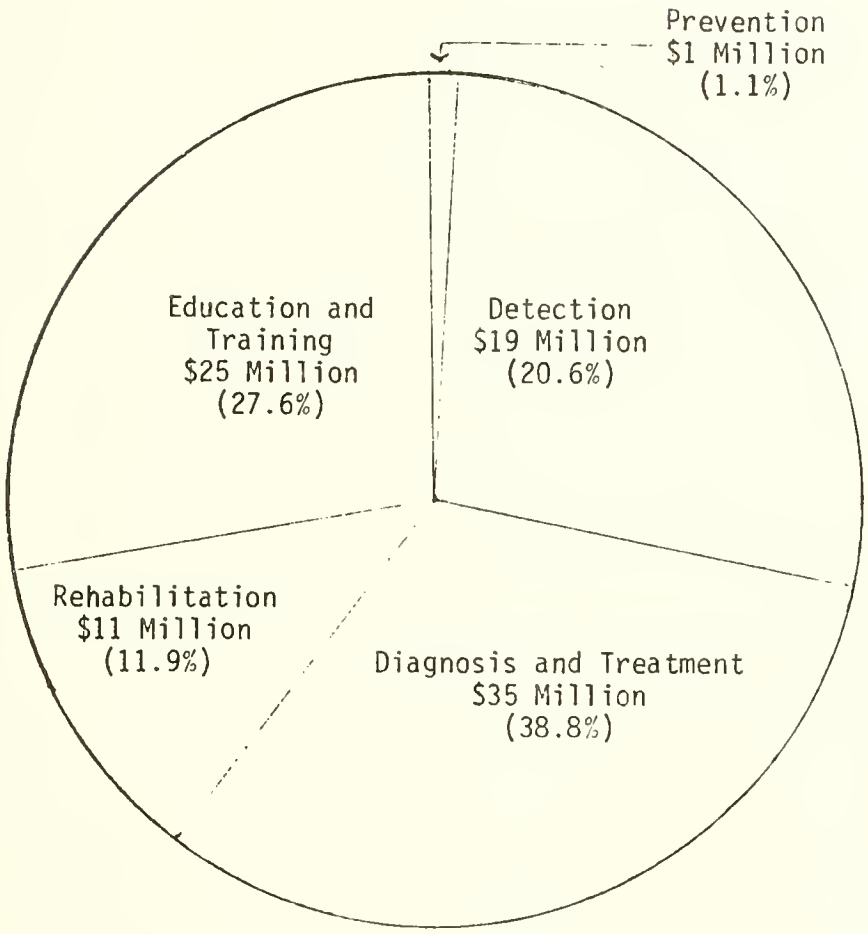
The RMPs which were not included in the examples presented varied from little or no investment in cancer control effort to a heavy investment. More than 30 RMPs funded cancer projects which specifically provided continuing professional education about cancer patient management. This figure does not include those previously mentioned general continuing education projects which included some cancer workshops as well as other health care workshops. Those types of projects were funded by almost every RMP. At least 20 RMPs provided some funds for tumor registries. More than 20 RMPs funded projects which included specific screening and detection activities, ranging from breast and cervical cancer screening clinics to the beginnings of stable cancer detection centers. At least 18 RMPs provided funds for medical therapy consultation networks. In addition, a wide variety of other projects were funded by the regions. (114)

The RMP investments for a three-year period are classified in accord with NCI intervention categories and were presented in Table III earlier in this paper. An extrapolation of that information, to include the entire RMP cancer control effort totaling approximately $91 million, is presented in Figure 3 on the following page. (115)

The information presented in Figure 3 indicated that the RMP cancer control activities were focused primarily on the Congressional mandate of transferring new technology from the research center to patients' bedside. Generally speaking, the major advances in cancer care technology between 1965 and 1974 were in the areas of detection, diagnosis and treatment. Both demonstration projects in these areas as well as projects which provide education and training are obvious modes of transferring new technology to health care providers. Thus, about 87% of the funds available to the RMP cancer control projects were expended directly on these areas.

Figure 3

Total Funds Expenditures by Areas of Emphasis
in RMP Cancer Control Projects
($91 Million)*



*This dollar amount includes known RMP fund awards to cancer projects, RMP program staff efforts, the cancer portion of partial cancer projects and the non-RMP funds provided. (116)

Included among the conclusions of the earlier study cited were,

1. The RMPs have maintained a significant though decreasing financial investment in cancer control activities since FY 1972. Information available from Regional Medical Programs Service (RMPS) indicates a FY 1971 investment total which is in accord with this trend.

2. Federal Administration changes in priorities and mission directives are closely associated with a significant decline in RMP investment in cancer control activities.

3. RMP cancer control projects operated with substantial financial investment from cosponsors. In addition, annual costs for continuing significant number of projects were assumed by non-RMP funding sources when RMP funds were withdrawn.

4. Within the RMPs, there is a large reservoir of valuable experience and information which could be useful for future cancer control programs with respect, for example, to planning and review, cooperative local implementation, cost analysis and evaluation procedures. (117)

The conclusions of the above study were generally found to be valid in this overall review of the RMP cancer experience. Additional detail, comments and conclusions from the historical review and analysis of lessons learned from the RMP experience in cancer control are provided in the following section.

SECTION IV: COMMENTS AND CONCLUSIONS

COMMENTS

In comparison to its total program, the Regional Medical Program (RMP) experience in cancer control was limited with respect to scope, stability and recognizable national impact. However, there is sufficient reason to believe that the basic RMP approach (that is, a community consortium charged with responsibility and provided with a modest amount of discretionary funds) can develop and nurture continuous service improvements and innovations within the voluntary systems reform framework. Sample policy issues in the RMP experience which have continued relevance for public cancer control programs may be summarized as follows:

1. In the current pluralistic health care system, there is need for a publicly funded, quasi-public "missing management element" which provides the coordinating leadership and support roles among fragmented federal and state health programs and among frequently competitive local interests.
2. For such a community consortium to work optimally, it must not be co-opted by a particular interest or interests. In the RMP experience, for example, early domination by medical schools sometimes led to a predictable bolstering of tertiary medical center capability, with little or no real attention beyond lip service to active outreach and community development activities. Toward the middle and last years of the RMP program the local organizations had generally evolved

into relatively well-functioning, balanced consortia of interests which, in many respects, acted as a quasi-public agency capable of rapid and effective program production in various high-priority areas for which federal dollars and support became available. In the main, however, the local organizations were not effectively directed and utilized by the federal administration.

In effect, the RMPs, in the middle and latter years particularly, had finally evolved into local organizations which could carry out the original legislative mission calling for leadership from the "grass roots" level in an appropriate manner. A unique and effective radical decentralization of authority for project grant awards had been made by the federal government. However, sufficient checks and balances were not developed organizationally, so that ensuing problems of moving efficiently toward national goals in the context of local control of operational authority were never adequately addressed conceptually or practically.

3. The widespread use by national and regional staffs of the technique of drawing together respected leaders to develop optimal guidelines, standards and criteria for the delivery of specific kinds of services is one which holds substantial promise in numerous areas. Guidelines without professionally adequate local translation and support efforts may tend to be somewhat sterile, however. In addition the regular production of

national "state-of-the-art" guidelines for optimal care would provide a specific basis for guiding, evaluating and redirecting regional and local disease control programs and others.

Other broad public policy issues raised by the RMP experience are relevant but are beyond the scope of this review. The reader is referred to the references. (118, 199, 120, 121)

In many instances, the Regional Medical Program served as a bridge between prior and current federal efforts in a number of program areas including cancer control. Prior to the establishment of the National Cancer Institute's Division of Cancer Control and Rehabilitation, the RMP had provided probably the largest scale federal mechanism for community-based cancer control programs. A lack of political support at the national level seemed to have developed for RMPs as a mechanism for cancer control and its cancer control efforts had generally declined prior to the NCI program establishment. As discussed earlier in the text, this lack of support was, in part, attributable to the view that the admixture of diseases and mission compromise in the final legislation did not fully capitalize on the nations readiness for a more structured regionalization of cancer control services.

Other issues in current cancer control programs center around issues addressed in the RMP experience. For example, differences in philosophy seem still to exist between community-oriented and consortial development forces on the one hand and medical school, comprehensive center forces on the other. These philosophies were at the heart of some conflicts and cancer control experiences of the early Regional Medical Program management.

Today's cancer control program problems, however, are further exacerbated by growing public recognition that the existence of technical capability to save lives or reduce suffering does not necessarily mean that capability

will be used. Specific programs must be mounted to transfer technology. It is increasingly realized, for example, that on a large group statistical basis, gains from the widespread application of some costly and highly sophisticated medical care technologies may be minimal in proportion to the relative costs to society. On the other hand, widespread applications of some techniques hold substantial promise of identifiable gains measured by changes in morbidity and mortality statistics and in other health status indicators. The failure to measure consistently and properly the human impact as well as the systems advances produced in RMP cancer control projects has resulted in a lack of convincing evidence (except for some relatively stable and well-evaluated projects) that the overall cancer control program of the RMP made such an impact.

Unless control programs are relatively stable and thereby capable of being evaluated with respect to changes in health status, questions of program value will remain largely unanswerable. Unless program value is clearly established, programs which stress innovation, improvement of quality and availability and access to services may give way unduely to programs which stress cost containment, facilities and services regulation and "gaining control of runaway inflation."

Careful review of specific projects and the overall Regional Medical Program experience does, however, suggest a unique, rich basis of experience for current and future disease control programs. These lessons may be most important in analyzing workable public mechanisms for making continuous services improvements locally.

CONCLUSIONS

While intertwined with general aspects of this large and complex program, a number of lessons from the RMP approach apply specifically to cancer

control and can be drawn from this review of its history. Additional lessons regarding both local and federal operations and policies have been studied in projects oriented to child health (122) and arthritis services. (123) A series of policy analysis documents and technical and administrative systems summaries are currently being produced in response to a Congressional mandate to study the program in depth. These analyses and systems manuals will constitute useful materials for future organizations which undertake health resources development functions. These documents and other current references (124) provide a wealth of further detail regarding aspects of RMP functioning.

Dr. Herbert Pahl, former federal director of the Regional Medical Program, identified a general achievement which underlies several major lessons specifically applicable to cancer control programs.

> If one major achievement of the RMPs were singled out, it could be the establishment and effective functioning of a flexible, vigorous forum for the identification, debate, resolution and implementation of plans to meet local community health needs. These forums were established with communities, states, and regions throughout the United States and addressed community needs which fell within the scope of the broad RMP legislative mandates. (125)

The creative development and use of an effective community forum is vital in local health services improvement efforts. Delicate and informed intervention, backed by flexible and capable organizational resources is essential when cooperative or collective action is required of a number of traditionally competing and insular forces. A number of these community organizations and other aspects of the RMP experience are summarized below as a basis for lessons for other cancer control programs.

Lesson I -- Community Forum

Perhaps the most important constructive lesson learned from the RMP experience was the creation and evolution of Regional Advisory Groups (RAG), the development, review and program governing board of the local regions. Many regions also formed project advisory councils which acted as subcommittees of the regional advisory group to serve the same functions for specific disease control or other programs. These groups were composed of various admixtures of physicians, educators, lawyers, nurses, engineers, ethnic minority representatives, and others, many of whom had no prior knowledge of health programs. They gradually fused themselves into highly responsible groups and became political forces within their regions. Working together at regional levels, these groups frequently became active leaders in their communities. They often supplied the motiviating forces as well as initial dollar resources to inaugurate desirable improvements and innovations in health care. With the final phaseout of RMP, a void of such leadership has been left at community levels. The leadership has not yet been fully reestablished and may require many years to reproduce. (126)

Lesson II -- Federal Support

For a large scale, complicated federal program to grow and to accomplish its stated objectives, there must be congressional and top-level executive department backing. During most of the last eight years of RMPs' life, there was spirited and extensive congressional backing with little or no executive support or effective direction. In spite of numerous attempts by local RMPs for mission stability and continued budgetary support, the shifting administrative priorities and the multitudinous ramifications of RMP programs at local levels made it impossible for the Congress and multiple constituencies to understand fully and completely the problems and potential of the program. Well-developed cancer control programs did not exist in great measure. Had such exemplary programs been visible beyond the project level, the nation might have been able to make better use of the RMP experience.

A functional national cancer program could probably have emerged through the local community forum approach had there been sufficient funds, national program mission stability and appropriate support. Drawing together the various fragmented federal programs into community-based consortia to act as regional resource development organizations such as the RMPs may be one of the better organizational mechanisms available for large-scale disease control programs.

Lesson III -- Timing

Well-designed cancer control programs require time, effort and professional skills to plan, implement and evaluate. Usually many months or even years of specific planning efforts are needed. Once a good regional program aimed at creating persisting changes is designed and funded, implementation begins slowly and there is a gradual crescendo in activities. Time, therefore,

is an important consideration. Supplementary funding must usually be obtained, facilities often constructed, personnel recruited, and community and professional acceptance established.

The multiplicity of federal changes in administrative direction and funding levels (with no consistent level of spending identified for cancer control programs) gave rise to a marked uncertainty, both at national and regional levels, as to the validity of any long-range programs in cancer control through the RMPs. A loss of credibility ensued in the regions, and few projects were developed in cancer control. Had priority levels been established and had there been administrative and Congressional support for cancer programs, it is very possible that many RMPs would have embarked on a serious attempt to provide excellent programs in their regions. Impatience for results is sometimes partially responsible for lack of results.

Lesson IV -- Regionalization

While the general subject of regionalization of medical care is not within the scope of this review, it is one aspect of Regional Medical Programs to which several writers have addressed themselves. (127) The development of some successful cancer control projects as well as the abject failure of others to achieve desired objectives may be attributed to varied use of regionalization philosophies and techniques. For example, a large number of physicians had been fearful of regionalization for the congressionally mandated categorical diseases, yet after the first few years of planning and operational programs many of them lost their fears and became advocates and participants. The laboriously constructed platform of effective local networks has now almost completely disappeared and probably cannot be rebuilt within the next several decades. (128) Experienced staff and volunteer personnel attracted to the health care field by RMP, have been lost in phase outs. Thus, the RMP experience and the Program's demise have frustrated many local physicians and RAG members who were ardent supporters and participants. Growing concern has been expressed with present efforts of DHEW to devise local health care innovation programs on the basis of ever increasing numbers of single disease and regulatory approaches.

The shifts in legislative authority, funding levels and administration priorities affected the RMPs and have implications for future programs. The following aspects of the RMP experience illustrate lessons related to these topics.

Lesson V -- Commission Report and Legislation

The enabling legislation (Public Law 89-239) emanating from the Congress largely emasculated the DeBakey Commission's report for a regionalization of services, partially due to testimony introduced at legislative hearings by organized medicine. The position was expressed that there would be too much

regimentation of the practice of medicine; fears existed that legislation might lead to federal domination over the private practice of medicine. In addition, the Congress had probably failed to evaluate adequately its own early mandate of August 5, 1937, when it established the National Cancer Institute, calling for "research into cancer diagnosis and treatment and the useful application of their results, with a view to the development and prompt widespread use of the most effective method of prevention, diagnosis and treatment of cancer."

For example, a simple amendment to authorize the extension of NCI activities into each state, with adequate funding and creation of community-based local organizations, might have accomplished far greater results in cancer control. However, the traditional resistance to large-scale community programs in the NIH and its medical school constituency would have remained a factor.

Lesson VI -- Legislative Concept

Study of the ten-year life span of RMP leads to the conclusion that a better approach could have been made by the DeBakey Commission than its recommendation of a package proposal to the President and Congress. Other than the fact that heart disease, cancer and stroke are the three largest killers among the chronic diseases, this triad of diseases has essentially little in common. While the Commission was cognizant of this fact, it apparently did not give adequate weight in devising the particulars of a long-term assault program on cancer, heart disease and stroke. Had the Commission specified priorities and specified three, separate, detailed approaches in response to their mandate, the proposed legislation might well have been written in a different form. This factor becomes even more apparent when the entire RMP is surveyed over its ten-year life span. The additions (by an administration apparently intent on holding down development of health resources) of numerous ad hoc responsibilities to the ever shifting RMP mission confused Congress, RMP administrators, and the provider community as well as the public. While the community mechanism is a desirable means of reducing fragmentation in federal programs, sufficient attention and support to specific systems functions or disease control programs must be given in a stable manner.

For example, it has been stated by Dr. Robert Q. Marston, that had the Congress passed separate legislation for cancer control, which in many ways was similar to tuberculosis control, a system of control and research programs could have been devised to produce a very effective nationwide program. Dr. Martson pointed out that cancer is a group of diseases largely diagnosed and treated by specialists, whereas heart disease and stroke are far more commonly seen and treated by professionals who are not specialists. It was Dr. Marston's feeling that the DeBakey Commission's original concept of RMPs could well have been carried out had the program been limited to cancer. (129)

The current health resources development functions of the federal government are in a state of evolution and intensive examination regarding role and

effectiveness mechanisms to assure equity if access to quality care for the American population. The large and ambitious Regional Medical Program experience provides a basis for retrospective analysis of numerous issues and problems likely to require several years trial and study before adequate solutions are institutionalized.

APPENDICES

APPENDIX A

Members of the Presidential Commission on Heart Disease, Cancer, and Stroke:

Dr. Michael E. DeBakey, Chairman

Dr. Samuel Bellet
Mr. Barry Bingham
Mr. John M. Carter
Dr. R. Lee Clark
Dr. Edward W. Dempsey
Dr. Sidney Farber
Dr. Marion S. Fay
Mr. Marion B. Folsom
Mr. Emerson Foote
General Alfred M. Gruenther
Dr. Phillip Handler
Mr. Arthur O. Hauish
Dr. Frank Horsfall, Jr.
Dr. J. Willis Hurst
Dr. Hugh Hussey
Mrs. Florence Mahoney
Dr. Charles W. Mays
Dr. John S. Meyer
Mr. James F. Oates
Dr. E. M. Papper
Dr. Howard A. Rusk
Dr. Paul W. Sanger
General David Sarnoff
Dr. Helen B. Taussig
Mrs. Harry S. Truman
Dr. Irving S. Wright
Dr. Jane C. Wright

COMMISSION STAFF

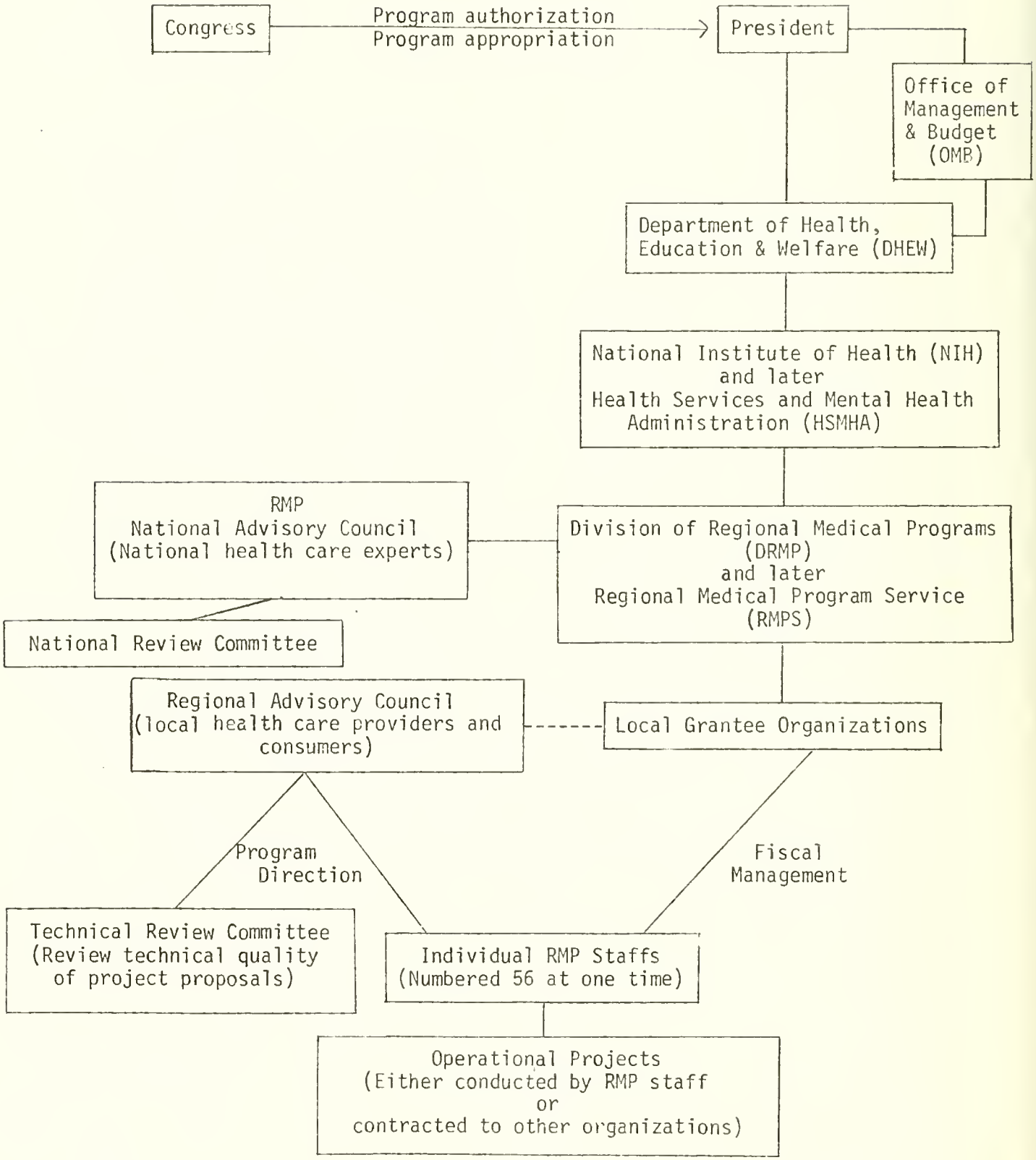
Dr. Abraham M. Lilienfield -- Staff Director

Mr. Stephen J. Ackerman -- Executive Secretary

(The staff included 26 specially qualified persons)

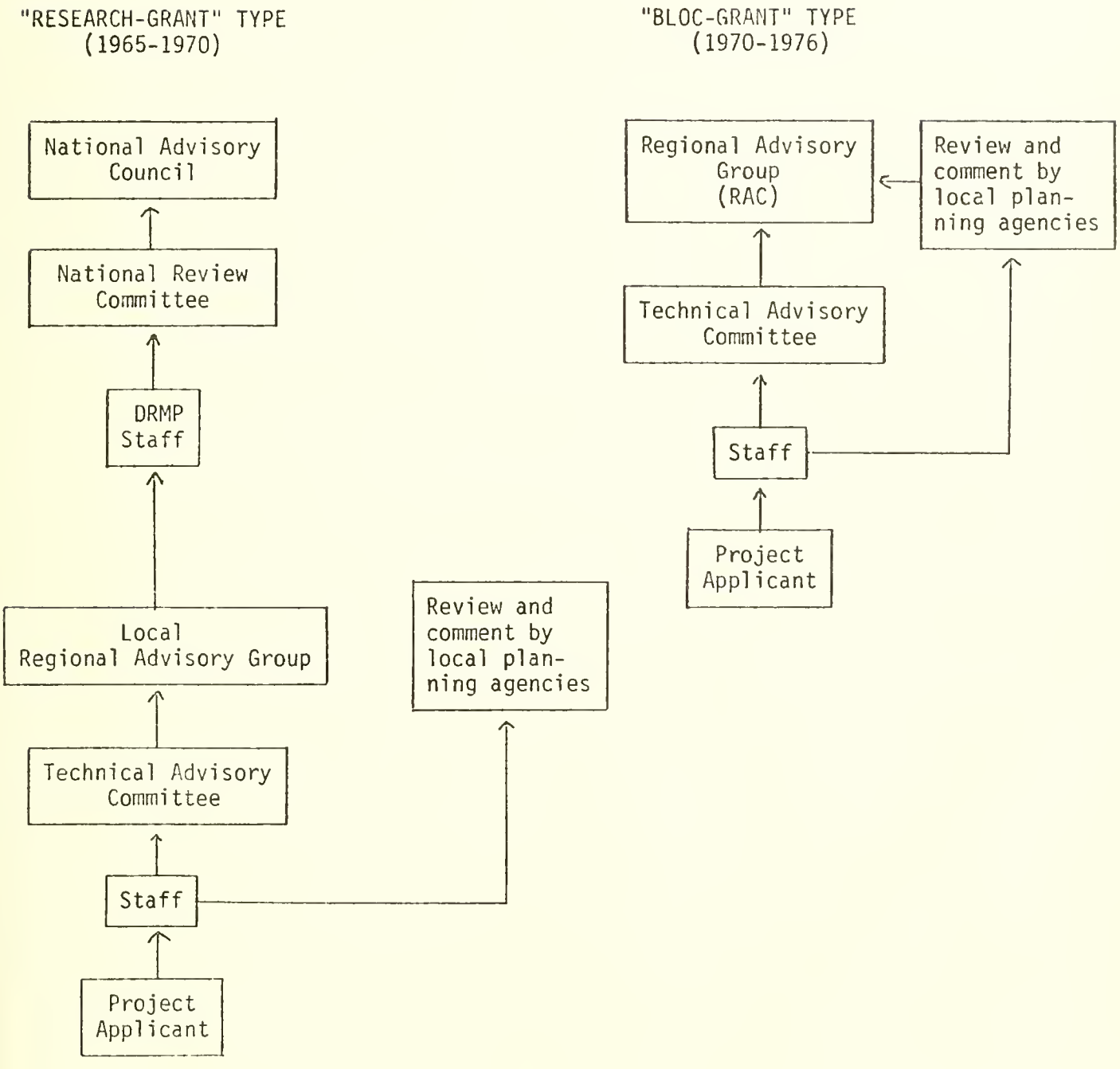
APPENDIX B-1

Regional Medical Program Organizational Structure



APPENDIX B-2

TYPES OF PROJECT REVIEW AND APPROVAL
FOR FUNDING REGIONAL MEDICAL PROGRAMS



APPENDIX C-1

List of Regional Medical Programs

Northeast Section

This section includes 10 states with a population of approximately 52,966,000 people in a geographical area of approximately 175,000 square miles.

Region Number	Region Name
04	Albany
50	Central New York (Syracuse)
08	Connecticut
26	Greater Delaware Valley (Philadelphia, DE)
13	Lakes Area (Buffalo)
54	Maine
66	Nassau-Suffolk (Long Island)
42	New Jersey
58	New York Metro
03	Northern New England (VT)
65	Puerto Rico
25	Rochester
59	Susquehanna Valley (Central PA)
62	Tri-State (MA, RI, NH)
41	Western Pennsylvania (Pittsburgh)

North Central Section

This Section includes 9 states with a population of approximately 39,833,000 people in a geographical area of approximately 572,000 square miles.

Region Number	Region Name
61	Illinois
37	Wisconsin
56	Bi-State (IL, MO)
27	Iowa
43	Indiana
60	North Dakota
67	South Dakota
68	Nebraska
53	Michigan
21	Northlands (MN)

Western Section

This Section includes 13 states with a population of approximately 36.567,000 people in a geographical area of approximately 1,781,000 square miles.

Region Number	Region Name
34	New Mexico
38	Washington/Alaska
15	Intermountain (UT, parts of ID, MT, WY, CO, NV)
40	Colorado-Wyoming
01	Hawaii
12	Oregon
32	Mountain States (ID, MT, NV, WY)
55	Arizona
19	California

Southeast Section

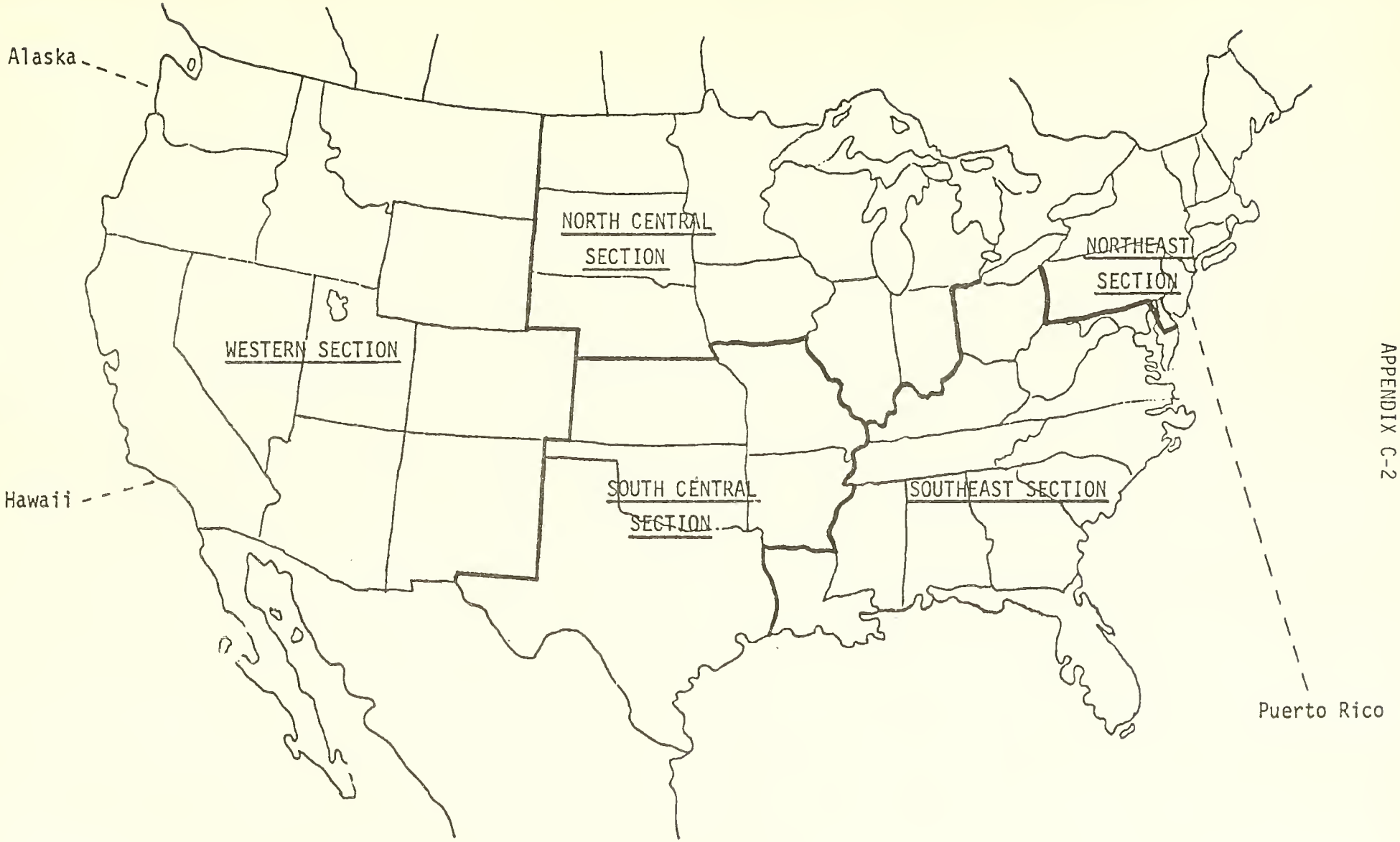
This Section includes 13 states and the District of Columbia with a population of approximately 59,667,000 people in a geographical area of approximately 458,000 square miles.

Region Number	Region Name
28	Alabama
24	Florida
46	Georgia
33	Louisiana
44	Maryland
51	Memphis
31	Metro Washington
57	Mississippi
06	North Carolina
48	Ohio Valley (Greater part of KY; SW Ohio)
35	South Carolina
18	Tennessee Mid-South (Nashville)
49	Virginia
45	West Virginia

South Central Section

This Section includes 5 states with a population of approximately 23,530,000 people in a geographical area of approximately 542,000 square miles.

Region Number	Region Name
52	Arkansas
02	Kansas
09	Missouri
23	Oklahoma
07	Texas


Alaska
NORTH CENTRAL SECTION
NORTHEAST SECTION
WESTERN SECTION
Hawaii
SOUTH CENTRAL SECTION
SOUTHEAST SECTION
Puerto Rico

PAR
12/5/75

APPENDIX D

Chronology of Significant Events
in Regional Medical Programs Mission

Date	Activity
February, 1964	President Johnson's Special Health Message to Congress proposes establishment of Commission on Heart Disease, Cancer and Stroke.
March, 1964	Commission on Heart Disease, Cancer and Stroke appointed (DeBakey Commission).
December, 1964	Commission report presented to President Johnson.
February, 1965	Legislation based on the Commission report introduced in Congress
October 6, 1965	President Johnson signs bill authorizing the establishment of Regional Medical Programs into law as Public Law 89-239.
December, 1965	First meeting of the RMP National Advisory Council.
April, 1966	First planning grants for individual regions approved and funded.
February, 1967	First operational grants to individual regions approved (funded in April, 1967).
March, 1968	RMP program extension legislation introduced in Congress.
Spring, 1968	RMP moved from NIH to HSMHA.
October, 1968	Extension legislation passed - becomes Public Law 90-574 (modified original purposes of the program).
January, 1969	Nixon Administration takes office.
March, 1970	RMP program extension legislation introduced in Congress.
October, 1970	Extension legislation passed - becomes Public Law 91-515 (significantly amends program purposes).
1971	The Office of Management and Budget initiates "forced carryover" (impoundment) practices.
December, 1971	Congress passes National Cancer Act of 1971 - becomes Public Law 92-218.
January, 1973	President's Budget for Fiscal Year 1974 submitted to Congress requesting no further funding of RMP.
February 1, 1973	Administration sends telegrams to RMPs requiring phase out plans

Date	Activity
June, 1973	RMP extension legislation overwhelmingly passed by Congress -- becomes Public Law 93-45
November, 1973	National Association of Regional Medical Programs files suit for release of impounded RMP funds
February, 1974	Federal court orders release of impounded RMP funds
December, 1974	Congress passes National Health Planning and Resources Development Act of 1974 -- becomes Public Law 93-641 (this law allegedly assigns RMP function to newly created Health Systems Agencies)
June, 1975	RMPs funded through June 30, 1976
June, 1976	$10 million appropriated by Congress for continuation of "exemplary RMP projects"
June 30, 1976	RMPs phase out

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(39) See Note (1).

(40) See Note (26).

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(42) Interview with Paul D. Ward, former executive director of California Committee on Regional Medical Programs by C. E. Smith, Ph.D., and A. M. Popma, M.D. of Health Policy Analysis and Accountability Network, Inc., October 27, 1976.

(43) Interview with Samuel R. Sherman, M.D., Director of Health Systems Management Corporation, by C. E. Smith, Ph.D. and A. M. Popma, M.D., of Health Policy Analysis and Accountability Network, Inc., October 27, 1976.

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(66) See Note (59).

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(128) See Note (42).

(129) See Note (41).

Surgeons General
United States Public Health Service

Thomas Parran, MD	1936-1948
Leonard Scheele, MD	1948-1956
Leroy E. Burney, MD, JD	1956-1961
Luther L. Terry, MD	1961-1965
William H. Stewart, MD	1965-1969
Jesse L. Steinfeld, MD	1969-1973

Directors of the
National Institute of Health

George W. McCoy, MD (authority on leprosy), 1936-1937

Lewis R. Thompson, MD (field investigator), 1937-1942

Rolla E. Dyer, MD (infectious diseases), 1942+-1950

William H. Sebrell, Jr., MD (authority on nutrition), 1950-1955

James A. Shannon, MD (physiologist, kidney & other medical research), 1955-1968

Robert Q. Marston, MD (research radiation effects), 1968-1973

Robert Stone, MD (pathologist), 1973-1976

Donald Fredrickson, MD, 1976-

+ Title changed to National Institutes of Health in 1943.

Directors of the National Cancer Institute

Carl Voegtlin, MD	1938-1943
Roscoe Spencer, MD	1943-1947
Leonard Scheele, MD	1947-1948
John R. Heller, MD	1948-1960
Kenneth Endicott, MD	1960-1968
Carl Baker, MD	1968-1972
Frank J. Rauscher, PhD	1972-1976
Arthur Upton, MD	1977-

Directors of the Federal Cancer Control Program

Leonard Scheele, MD (Public Health), 1939-1947

Austin W. Deibert, MD (Public Health - VD Control), 1947-1951

Raymond F. Kaiser, MD* (Public Health - VD Control), 1951**-1957

Lewis Robbins, MD (Public Health - Cancer Control), 1957-1965

William Ross, MD (Public Health - Cancer Control), 1965***-1972

John Bailar, III, MD (Epidemiology, NCI), 1972-1974

Diane Fink, MD (Medicine; Division of Treatment, NCI), 1974-

---

*Chief, Cancer Control Branch, 1951-1953
Chief, Field Investigations & Demonstrations Branch, 1953-1961
Chief, Diagnostic Research Branch, 1961-1962

**Program moved to Division of State Health Services, Bureau of State Services

***Portions of program transferred in 1968-1970 to Regional Medical Programs Service, HSMHA

Members of the Cancer Control Program Advisory Committee, 1948

Frank Adair, MD	Surgeon, Memorial Hospital, New York City
Charles Branch, MD	
Charles Cameron, MD	Medical Director, American Cancer Society, New York City
Dr. Edward Chamberlain	
Dr. Herman Hilleboe	
Dr. Herbert Lombard	Chief, Cancer Control Division, Massachusetts State Department of Public Health
Dr. Robert Moore	
Dr. Frank Queen	
Dr. Edmund Zimmerer	

Members of the Cancer Control Program Advisory Committee, November, 1962

Frederick J. Brady, MD	Director, Pima, Arizona, Health Department
Ulrich R. Bryner, MD	General Practitioner, Salt Lake City, Utah; former president, American Academy of General Practice
Bernard Bucove, MD	Director of Health, State of Washington
Thomas Carlile, MD	Radiologist, Mason Clinic, Seattle, Washington
John W. Cline, MD	Surgeon, San Francisco; past-president, American Medical Association
Warren H. Cole, MD	Professor & Head of Surgery Department, University of Illinois College of Medicine
Joseph A. Cunningham, MD	Professor of Pathology, University of Alabama School of Medicine

Members of the
Cancer Control Program Advisory Committee,
November, 1962 (continued)

Harold S. Diehl, MD	Senior Vice-President for Research & Medical Affairs, American Cancer Society; former Dean, University of Minnesota College of Medical Sciences
John P. Lindsay, MD	General Practitioner, Strang Clinic, New York City
Mack I. Shanholtz, MD	State Health Commissioner, Virginia
Charles E. Smith, MD	Dean, University of California-Berkeley School of Public Health
Leonid S. Snegireff, MD	Associate Professor of Chronic Disease Control, Harvard School of Public Health
David A. Wood, MD	Pathologist; Director, Cancer Research Institute, University of California-San Francisco Medical Center
Paul A. Younge, MD	Gynecologist; Associate Chief Surgeon, Massachusetts Free Hospital for Women; Secretary, American Society for Cytology

List of American Cancer Society Chairmen of Board of Directors

Prepared by ACS Medical Library*

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April 1942 - March 1944	John J. Morton, Jr., MD	(Surgery)
April 1944 - April 1945	Herman C. Pitts, MD	(Otolaryngology)
May 1945 - May 1946	Eric A. Johnston	(President of Motion Picture Association of America, Inc.)
June 1946 - March 1948	Eric A. Johnston (Honorary)	(See above)
June 1946 - 1947	Ted R. Gamble	(Chairman of the American Theater, Portland, Oregon)
1948 - 1949	Eric A. Johnston (Chairman)	(See above)
1949 - 1950	Eric A. Johnston (Honorary)	(See above)
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1955 - 1956	James S. Adams (Honorary Chairman)	(Businessman)
1959 - 1962	Rutherford B. Ellis	(Businessman, Lipscomb-Ellis & Co.)
1957 - to date	Mrs. Albert D. Lasker (Honorary Chairman)	(President, Albert & Mary Lasker Foundation)
1962 - 1966	Francis J. Wilcox	(Lawyer)

* New York, August, 1976

ACS Chairmen of Board of Directors (continued)

1966 - 1967	Travis T. Wallace	(Founder & Board Chairman, Great American Reserve Insurance Co.)
1967 - 1971	William B. Lewis	(Former Board Chairman, Kenyon & Eckhardt, Inc.)
1971 - 1973	Charles R. Ebersol	(Lawyer)
1973 - 1975	Winston Armin Willig	(Businessman, W. & R. Investments)
1975 - 1976	Thomas P. Ulmer	(President, Ulmer - Burgess, Inc.)

List of American Cancer Society Presidents

Prepared by ACS Medical Library*

1937 - 1938	Frederick F. Russell, MD	(Professor, Preventive Medicine & Epidemiology, Harvard University Medical School - Public Health)
1938 - 1942	John J. Morton, Jr., MD	(Surgery)
1942 - 1944	Herman C. Pitts, MD	(Surgery)
1944 - 1947	Frank Adair, MD	(Surgery)
1947 - 1948	Edwin P. Lehman, MD	(Surgery)
1948 - 1949	C. C. Nesselrode, MD	(Surgery)
1949 - 1950	Alton Ochsner, MD	(Surgery)
1950 - 1951	Guy Aud, MD	(Surgery)
1951 - 1952	Charles C. Lund, MD	(Surgery)
1952 - 1953	Harry M. Nelson, MD	(Obstetrics & Gynecology)
1953 - 1954	Alfred M. Popma, MD	(Radiology)
1954 - 1955	Howard C. Taylor, Jr., MD	(Obstetrics & Gynecology)
1955 - 1956	C. V. Brindley, MD	(Surgery)
1956 - 1957	David A. Wood, MD	(Pathology)
1957 - 1958	Lowell T. Coggeshall, MD	(Medical Education)
1958 - 1959	Eugene Pendergrass, MD	(Radiology)
1959 - 1960	Warren Cole, MD	(Surgery)
1960 - 1961	John W. Cline, MD	(Surgery)
1961 - 1962	Thomas Carlile, MD	(Radiology)
1962 - 1963	I. S. Ravdin, MD	(Surgery)
1963 - 1964	Wendell G. Scott, MD	(Radiology)

* New York, August, 1976

ACS Presidents (continued)

1964 - 1965	Murray M. Copeland, MD	(Oncologic Surgery)
1965 - 1966	Leonard W. Larson, MD	(Pathology)
1966 - 1967	Ashbel C. Williams, MD	(Oncologic & Thoracic Surgery)
1967 - 1968	Roger A. Harvey, MD	(Radiology)
1968 - 1969	Sidney A. Farber, MD	(Pathology)
1969 - 1970	Jonathan E. Rhoads, MD, D.Sc.	(Surgery)
1970 - 1971	H. Melvin Pollard, MD	(Internal Medicine)
1971 - 1972	A. Hamblin Letton, MD	(Oncologic Surgery)
1972 - 1973	Arthur G. James, MD	(Surgery)
1973 - 1974	Justin J. Stein, MD	(Radiotherapy)
1974 - 1975	George P. Rosemond, MD	(Surgery)
1975 - 1976	Benjamin F. Byrd, Jr., MD	(Surgery)
1976 - 1977	R(andolph) Lee Clark, MD	(Surgery)

List of American Cancer Society Executive Vice-Presidents

Prepared by ACS Medical Library*

1938 - June 1945	Clarence C(ook) Little, Sc.D. (Managing Director)	
Jan. 1944 - Sept. 1945	J. Louis Neff (Executive Director)	(Executive Secretary, Nassau Medical Soc.)
1945 - 1946	Rear Admiral Charles S. Stephenson (Acting Managing Director)	(Retired U.S. Navy)
Oct. 1945 - 1946	Edwin J. MacEwan (Administrative Director, Business Director)	(Executive Vice-President, New Haven Chamber of Commerce)
April 1946 - Oct. 1946	Col. Ashley W. Oughterson, MD (Executive Vice-President)	
Oct. 1946 - 1949	Douglas Poteat (Administrative Director)	(Lawyer)
July 1, 1949 - Oct. 1959	Mefford R. Runyon (Executive Vice-President)	(President of Family Counseling Service)
1959 - to date	Lane W. Adams (Executive Vice-President)	(Bank Executive)

* New York, August, 1976

List of American Cancer Society Medical and Scientific Directors

Prepared by ACS Medical Library*

1938 - April or June 1945	Clarence C(ook) Little, Sc.D.	(Managing Director)
June 1940 - May 1941	Samuel Binkley, MD	(Asst. Managing Director)
June 1941 - ?	Samuel Binkley, MD	(Medical Director)
June 1945 - Apr. 1946	V A C A N T	
June 1946 - 1947	Col. Ashley W. Oughterson, MD	(Medical & Scientific Director & Vice-President)
Mar. 1947 - 1948	Charles S. Cameron, MD	(Acting Director)
Apr. 1948 - 1955	Charles S. Cameron, MD (Appointed Feb. 28, 1948)	(Medical Director & Vice-President)
1956	W. Kenneth Clark, MD	(Acting Director)
1957 - 1958	W. Kenneth Clark, MD	(Vice-President for Medical Affairs)
1959	Roald N. Grant, MD	(Acting Vice-President for Medical Affairs)
1960 - 1967	James P. Cooney, MD (Major General, U.S. Army)	(Vice-President for Medical Affairs)
1957 - 1967	Harold S. Diehl, MD	(Senior Vice-President for Research & Medical Affairs, & Deputy Executive Vice-President)
1968 - 1969	Arthur I. Holleb, MD	(Senior Vice-President for Medical Affairs)
1970 - to date	Arthur I. Holleb, MD	(Senior Vice-President for Medical Affairs & Research)

* New York, August, 1976

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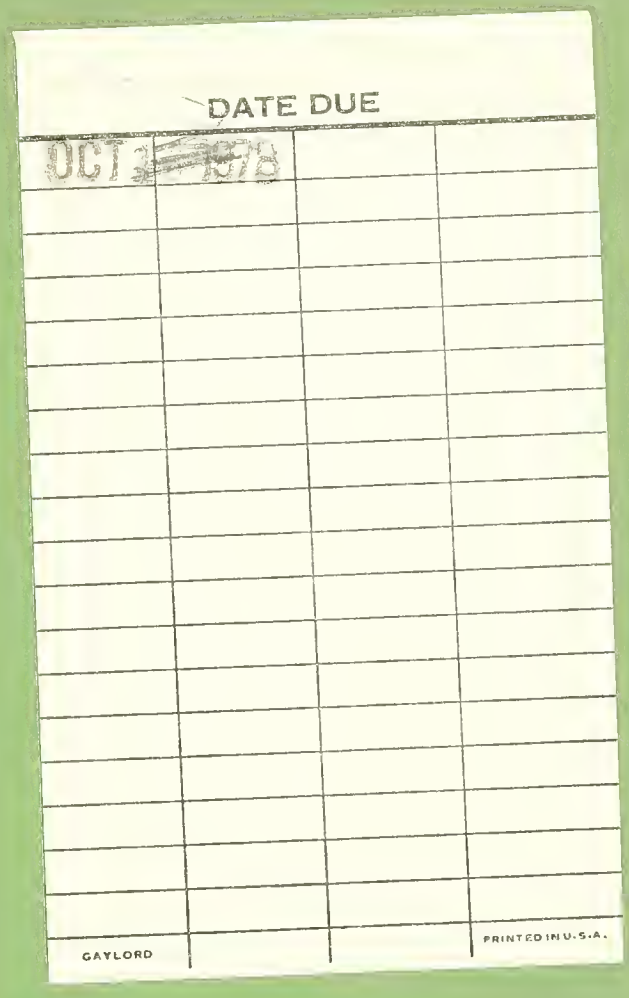

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