ADVISORY COMMITTEE ON HUMAN RADIATION EXPERIMENTS

MEETING

JUNE 21-23, 1995

The Madison Hotel
Executive Chambers
15th & M Street, NW
Washington, DC

VOLUME 15

READING ROADMAP: A GUIDE TO JUNE 21-23 BRIEFING BOOK (Vol. 15) AND PRELIMINARY CHAPTER DRAFTS

Below is a guide to the Briefing Book and Chapter Drafts for the June 21-23 meeting of the Advisory Committee on Human Radiation Experiments. This guide should help ease your reading load for the next meeting, given the short amount of time you have to review these materials. Items followed by an (*) will be sent to you by a subsequent mailing or fax or will be distributed to you at the meeting.

We have divided the reading into four categories: (1) Essential; (2) Encouraged; (3) At Your Leisure; and (4) For Your Reference (e.g., updated rosters, meeting timetables and locations.) "Essential" draft chapters are those chapters that are specifically identified as agenda items.

ESSENTIAL

Tab A-1: Agenda

Final Report Chapter Drafts:

- Part III, Chapter 15: Research Proposal Review Project*
- Part III, Chapter 16: Subject Interview Study
- Part III, Chapter 17: Contemporary Projects: An Ethical Analysis*
- Part IV, Chapter 18: Findings
- Part IV, Chapter 19: Recommendations

Envelope: Draft Minutes from May 8-10 Meeting

ENCOURAGED

Final Report Chapter Drafts:

- Introduction: The Atomic Century Begins*
- Part I, Chapter 4: Ethics Conclusions*
- Part II, Overview
- Part II, Chapter 5: Biodistribution*
- Part II, Chapter 6: Isotopes
- Part II, Chapter 7: Children*
- Part II, Chapter 8: Total-Body Irradiation
- Part II, Chapter 10: Atomic Bomb Tests*
- Part II, Chapter 11: Intentional Releases
- Part II, Chapter 12: Observational Studies
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AT YOUR LEISURE

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FOR REFERENCE

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ADVISORY COMMITTEE ON HUMAN RADIATION EXPERIMENTS
PUBLIC MEETING
JUNE 21-23, 1995

BRIEFING BOOK (VOLUME 15)

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A-1 Agenda

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ADVISORY COMMITTEE ON HUMAN RADIATION EXPERIMENTS

PUBLIC MEETING

JUNE 21-23, 1995

Madison Hotel ✥ Executive Chambers ✥ 15th & M Street, N.W. ✥ Washington, DC

TENTATIVE AGENDA

Wednesday, June 21, 1995

9:00 AM	Call to Order
	Opening Remarks *Ruth Faden, Chair*
9:05	Approval of Minutes of May 8-10, 1995 Meeting
9:10	Public Comment
10:15	Break
10:30	Committee Discussion: Findings (1944-74) [Part IV, Chapter 18]
12:00	Lunch
1:15	Committee Discussion: Findings (1944-74) [Part IV, Chapter 18] *(continued)*
2:15	Committee Discussion: Recommendations for Remedies for Former Subjects [Part IV, Chapter 19]
3:15	Break
3:30	Committee Discussion: Recommendations for Remedies for Former Subjects [Part IV, Chapter 19] *(continued)*
5:00	Meeting Adjourned

Thursday, June 22, 1995

9:00 AM	Committee Discussion: Subject Interview Study [Part III, Chapter 16]
10:00	Committee Discussion: Research Proposal Review Project [Part III, Chapter 15]
10:45	Break
11:00	Committee Discussion: Research Proposal Review Project *(continued)* [Part III, Chapter 15]
12:30 PM	Lunch
1:45	Committee Discussion: Contemporary Projects--An Ethieal Analysis [Part III, Chapter 17]
2:45	Committee Discussion: Recommendations for the Protection of the Rights and Interests of Human Subjects [Part IV, Chapter 19]
3:45	Break
4:00	Committee Discussion Concerning Strategy and Direction: Mechanisms for Sign Off of Final Report and Personal Statements
5:00	Meeting Adjourned

Friday, June 23, 1995

8:30 AM	Committee Discussion: Recommendations for the Protection of the Rights and Interests of Human Subjects [Part IV, Chapter 19] *(continued)*
10:15	Break
10:30	Committee Discussion: Findings--Contemporary Intentional Releases [Part IV, Chapter 18]
	Committee Discussion: Recommendations for Balancing National Security Interests and the Rights of the Public [Part IV, Chapter 19]
12:15 PM	Lunch
1:30	Committee Discussion: Recommendations on Openness [Part IV, Chapter 19]
2:30	Committee Discussion: Outstanding Issues
3:30	Meeting Adjourned

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A-2 Federal Register
Notice of Meeting

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A-3 Executive Order

Divider Title: _____

Presidential Documents

TAB A3

Executive Order 12891 of January 15, 1994

Advisory Committee on Human Radiation Experiments

By the authority vested in me as President by the Constitution and the laws of the United States of America, it is hereby ordered as follows:

Section 1. *Establishment.* (a) There shall be established an Advisory Committee on Human Radiation Experiments (the "Advisory Committee" or "Committee"). The Advisory Committee shall be composed of not more than 15 members to be appointed or designated by the President. The Advisory Committee shall comply with the Federal Advisory Committee Act, as amended, 5 U.S.C. App. 2.

(b) The President shall designate a Chairperson from among the members of the Advisory Committee.

Sec. 2. *Functions.* (a) There has been established a Human Radiation Interagency Working Group, the members of which include the Secretary of Energy, the Secretary of Defense, the Secretary of Health and Human Services, the Secretary of Veterans Affairs, the Attorney General, the Administrator of the National Aeronautics and Space Administration, the Director of Central Intelligence, and the Director of the Office of Management and Budget. As set forth in paragraph (b) of this section, the Advisory Committee shall provide to the Human Radiation Interagency Working Group advice and recommendations on the ethical and scientific standards applicable to human radiation experiments carried out or sponsored by the United States Government. As used herein, "human radiation experiments" means:

(1) experiments on individuals involving intentional exposure to ionizing radiation. This category does not include common and routine clinical practices, such as established diagnosis and treatment methods, involving incidental exposures to ionizing radiation;

(2) experiments involving intentional environmental releases of radiation that (A) were designed to test human health effects of ionizing radiation; or (B) were designed to test the extent of human exposure to ionizing radiation.

Consistent with the provisions set forth in paragraph (b) of this section, the Advisory Committee shall also provide advice, information, and recommendations on the following experiments:

(1) the experiment into the atmospheric diffusion of radioactive gases and test of detectability, commonly referred to as "the Green Run test," by the former Atomic Energy Commission (AEC) and the Air Force in December 1949 at the Hanford Reservation in Richland, Washington;

(2) two radiation warfare field experiments conducted at the AEC's Oak Ridge office in 1948 involving gamma radiation released from non-bomb point sources at or near ground level;

(3) six tests conducted during 1949–1952 of radiation warfare ballistic dispersal devices containing radioactive agents at the U.S. Army's Dugway, Utah, site;

(4) four atmospheric radiation-tracking tests in 1950 at Los Alamos, New Mexico; and

(5) any other similar experiment that may later be identified by the Human Radiation Interagency Working Group.

The Advisory Committee shall review experiments conducted from 1944 to May 30, 1974. Human radiation experiments undertaken after May 30, 1974 (the date of issuance of the Department of Health, Education, and Welfare ("DHEW") Regulations for the Protection of Human Subjects (45 C.F.R. 46)), may be sampled to determine whether further inquiry into experiments is warranted. Further inquiry into experiments conducted after May 30, 1974, may be pursued if the Advisory Committee determines, with the concurrence of the Human Radiation Interagency Working Group, that such inquiry is warranted.

(b)(1) The Advisory Committee shall determine the ethical and scientific standards and criteria by which it shall evaluate human radiation experiments, as set forth in paragraph (a) of this section. The Advisory Committee shall consider whether (A) there was a clear medical or scientific purpose for the experiments; (B) appropriate medical follow-up was conducted; and (C) the experiments' design and administration adequately met the ethical and scientific standards, including standards of informed consent, that prevailed at the time of the experiments and that exist today.

(2) The Advisory Committee shall evaluate the extent to which human radiation experiments were consistent with applicable ethical and scientific standards as determined by the Committee pursuant to paragraph (b)(1) of this section. If deemed necessary for such an assessment, the Committee may carry out a detailed review of experiments and associated records to the extent permitted by law.

(3) If required to protect the health of individuals who were subjects of a human radiation experiment, or their descendants, the Advisory Committee may recommend to the Human Radiation Interagency Working Group that an agency notify particular subjects of an experiment, or their descendants, of any potential health risk or the need for medical follow-up.

(4) The Advisory Committee may recommend further policies, as needed, to ensure compliance with recommended ethical and scientific standards for human radiation experiments.

(5) The Advisory Committee may carry out such additional functions as the Human Radiation Interagency Working Group may from time to time request.

Sec. 3. *Administration.* (a) The heads of executive departments and agencies shall, to the extent permitted by law, provide the Advisory Committee with such information as it may require for purposes of carrying out its functions.

(b) Members of the Advisory Committee shall be compensated in accordance with Federal law. Committee members may be allowed travel expenses, including per diem in lieu of subsistence, to the extent permitted by law for persons serving intermittently in the government service (5 U.S.C. 5701–5707).

(c) To the extent permitted by law, and subject to the availability of appropriations, the Department of Energy shall provide the Advisory Committee with such funds as may be necessary for the performance of its functions.

Sec. 4. *General Provisions.* (a) Notwithstanding the provisions of any other Executive order, the functions of the President under the Federal Advisory Committee Act that are applicable to the Advisory Committee, except that of reporting annually to the Congress, shall be performed by the Human Radiation Interagency Working Group, in accordance with the guidelines and procedures established by the Administrator of General Services.

(b) The Advisory Committee shall terminate 30 days after submitting its final report to the Human Radiation Interagency Working Group.

(c) This order is intended only to improve the internal management of the executive branch and it is not intended to create any right, benefit, trust, or responsibility, substantive or procedural, enforceable at law or equity by a party against the United States, its agencies, its officers, or any person.

William J. Clinton

THE WHITE HOUSE,
January 15, 1994.

[FR Doc. 94-1531
Filed 1-18-94; 4:37 pm]
Billing code 3195-01-P

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A-4 Charter

Divider Title: _____

CHARTER

ADVISORY COMMITTEE ON HUMAN RADIATION EXPERIMENTS

1. Committee's Official Designation

Advisory Committee on Human Radiation Experiments (the "Advisory Committee" or "Committee").

2. Authority

Executive Order No. 12891.

3. Objectives and Scope of Activities

There has been established a Human Radiation Interagency Working Group (the "Interagency Working Group"), the members of which include the Secretary of Energy, the Secretary of Defense, the Secretary of Health and Human Services, the Secretary of Veterans' Affairs, the Attorney General, the Administrator of the National Aeronautics and Space Administration, the Director of Central Intelligence, and the Director of the Office of Management and Budget. As set forth in section 4 of this Charter, the Advisory Committee shall provide to the Interagency Working Group advice and recommendations on the ethical and scientific standards applicable to human radiation experiments carried out or sponsored by the United States Government. As used herein, "human radiation experiments" means:

(1) Experiments on individuals involving intentional exposure to ionizing radiation. This category does not include common and routine clinical practices, such as established diagnosis and treatment methods, involving incidental exposures to ionizing radiation.

(2) Experiments involving intentional environmental releases of radiation that (A) were designed to test human health effects of ionizing radiation; or (B) were designed to test the extent of human exposure to ionizing radiation.

Consistent with the provisions set forth in section 4 of this Charter, the Advisory Committee also shall provide advice, information and recommendations on the following experiments:

(1) The experiment into the atmospheric diffusion of radioactive gases and test of detectability, commonly referred to as "the Green Run test," by the former Atomic Energy Commission (AEC) and the Air Force in December 1949 in Hanford, Washington;

(2) Two radiation warfare field experiments conducted at the AEC's Oak Ridge office in 1948 involving gamma radiation released from non-bomb point sources at or near ground level;

(3) Six tests conducted during 1949-1952 of radiation warfare ballistic dispersal devices containing radioactive agents at the U.S. Army's Dugway, Utah site;

(4) Four atmospheric radiation-tracking tests in 1950 at Los Alamos, New Mexico; and

(5) Any other similar experiments which may later be identified by the Interagency Working Group.

The Advisory Committee shall review experiments conducted from 1944 to May 30, 1974, the date of issuance of the Department of Health, Education and Welfare Regulations for the Protection of Human Subjects (45 C.F.R. 46), may be sampled to determine whether further inquiry into experiments is warranted. Further inquiry into experiments conducted after May 30, 1974, may be pursued if the Advisory Committee determines, with the concurrence of the Interagency Working Group, that such inquiry is warranted.

4. Description of Duties for which Committee is Responsible

The duties of the Advisory Committee are solely advisory and shall be:

a. The Advisory Committee shall determine the ethical and scientific standards and criteria by which it shall evaluate human radiation experiments, as set forth in section 3 of this Charter. The Advisory Committee shall consider whether (A) there was a clear medical or scientific purpose for the experiments; (B) appropriate medical follow-up was conducted; and (C) the experiments' design and administration adequately met the ethical and scientific standards, including standards of informed consent, that prevailed at the time of the experiments and that exist today.

b. The Advisory Committee shall evaluate the extent to which human radiation experiments were consistent with applicable ethical and scientific standards as determined by the Committee pursuant to paragraph (a) of this section. If deemed necessary for such an assessment, the Advisory Committee may carry out a detailed review of experiments and associated records to the extent permitted by law.

c. If required to protect the health of individuals who were subjects of a human radiation experiment, or their descendants, the Advisory Committee may recommend to the Interagency Working Group that an agency notify particular subjects of an experiment, or their descendants, of any potential health risk or the need for medical follow-up.

d. The Advisory Committee may recommend further policies, as needed, to ensure compliance with recommended ethical and scientific standards for human radiation experiments.

e. The Advisory Committee may carry out such additional functions as the Interagency Working Group may from time to time request.

5. To Whom the Advisory Committee Reports

The Advisory Committee shall report to the Interagency Working Group.

The Advisory Committee shall submit its final report to the Interagency Working Group within one year of the date of the first meeting of the Advisory Committee, unless such period is extended by the Interagency Working Group. The Advisory Committee shall issue an interim report not more than six months after the date of the first meeting of the Advisory Committee. That interim report shall advise the Interagency Working Group on the status of the Advisory Committee's proceedings and the likelihood that the Committee will be able to complete its duties within one year of the date of the first meeting of the Advisory Committee.

6. Duration and Termination Date

The Advisory Committee shall terminate thirty days after submission of its final report to the Interagency Working Group. This Charter shall expire one year plus thirty days after the first meeting of the Advisory Committee, subject to renewal and extension by the President.

7. Agency responsible for providing financial and administrative support to the Advisory Committee

Financial and administrative support shall be provided by the Department of Energy.

8. Estimated Annual Operating Costs

$3 million.

9. Estimated Number and Frequency of Meetings

The Advisory Committee shall meet as it deems necessary to complete its functions.

10. Subcommittees

To facilitate functioning of the Advisory Committee, subcommittee(s) may be formed. The objectives of the subcommittee(s) are to make recommendations to the Advisory Committee with respect to matters related to the responsibilities of the Advisory Committee. Subcommittees shall meet as the Advisory Committee deems appropriate.

11. Members

Up to a maximum of fifteen Advisory Committee members shall be appointed by the President for a term of one year, which may be extended by the President. Committee members shall be compensated in accordance with federal law. Committee members may be allowed travel expenses, including per diem in lieu of subsistence, to the extent permitted by law for persons serving intermittently in the government service (5 U.S.C. §§ 5701-5707).

12. Chairperson

The President shall designate a Chairperson from among the members of the Advisory Committee.

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B-1 Advisory Committee
Roster

Divider Title: _____

ADVISORY COMMITTEE ON HUMAN RADIATION EXPERIMENTS

Ruth R. Faden, Ph.D., M.P.H.-Chair
Director, Program in Law, Ethics and Health
Professor, Dept. of Health Policy & Management
The Johns Hopkins University
School of Hygiene and Public Health
Baltimore, MD

Senior Research Scholar
Kennedy Institute of Ethics
Georgetown University
Washington, DC

Kenneth R. Feinberg, J.D.
Kenneth R. Feinberg & Associates
Washington, DC

Eli Glatstein, M.D.
Professor and Chair
Department of Radiation Oncology
The University of Texas
Southwestern Medical Center at Dallas
Dallas, TX

Jay Katz, M.D.
Elizabeth K. Dollard Professor Emeritus of Law,
Medicine and Psychiatry
Harvey L. Karp Professorial Lecturer in Law
and Psychoanalysis
Yale Law School
New Haven, CT

Patricia A. King, J.D.
Professor of Law
Georgetown University Law Center
Washington, DC

Susan E. Lederer, Ph.D.
Associate Professor
Department of Humanities
The Pennsylvania State University
College of Medicine
Hershey, PA

Ruth Macklin, Ph.D.
Professor of Bioethics
Department of Epidemiology & Social Medicine
Albert Einstein College of Medicine
Bronx, NY

Lois L. Norris
Second Vice President of Omaha National Bank
and Omaha National Corporation (Retired)
Omaha, NE

Nancy L. Oleinick, Ph.D.
Professor of Radiation Biochemistry
Division of Radiation Biology
Case Western Reserve University
School of Medicine
Cleveland, OH

Henry D. Royal, M.D.
Professor of Radiology
Associate Director
Division of Nuclear Medicine
Mallinckrodt Institute of Radiology
Washington University Medical Center
St. Louis, MO

Philip K. Russell, M.D.
Professor, Department of International Health
The Johns Hopkins University
School of Hygiene and Public Health
Baltimore, MD

Mary Ann Stevenson, M.D., Ph.D.
Assistant Professor of Radiation Oncology
Joint Center for Radiation Therapy
Harvard Medical School
Boston, MA

Deputy Chief
New England Deaconess Hospital
Department of Radiation Oncology
Boston, MA

Duncan C. Thomas, Ph.D.
Professor
University of Southern California
School of Medicine
Department of Preventive Medicine
Los Angeles, CA

Reed V. Tuckson, M.D.
President
Charles Drew University of Medicine & Science
Los Angeles, CA

May 23, 1995

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B-2 Advisory Committee
Staff & Consultant Roster

Divider Title: _____

Staff Roster
(Includes full and part-time staff and consultants)

MANAGEMENT:

Dan Guttman
Executive Director

Jeffrey Kahn
Associate Director

Anna Mastroianni
Associate Director

Stephen Klaidman
Director of Communications
Counselor to the Committee

PROFESSIONAL STAFF:

Senior Policy and Research Analyst
Jonathan Moreno

Research Analyst
Deborah Holland

Research Associate
Miriam Bowling
Sara Chandros
Valerie Hurt
John Kruger
Shobita Parthasarathy

Information Services

David Saumweber
Director

Robin Cochran
Librarian

Tom Wisner
Senior Technology Consultant

Editorial Staff
Sarah Flynn
Editor

CONSULTANTS:

Jeff Botkin
Allen Buchanan
Jim David
Patrick Fitzgerald
Jerry Garcia
Mark Goodman
Steve Goodman
Jon Harkness
Gregg Herken
Michael Jasny
Nancy Kass
Lanny Keller
Ron Neumann
Pat Parentesis
Monica Schoch-Spana
Jeremy Sugarman
Tedd Webster
Gil Whittemore

ADMINISTRATIVE AND SUPPORT STAFF:

Sally Rhoadarmer
Office Manager

June 8, 1995

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B-3 Subcommittee Roster

Divider Title: _____

NEW SUBCOMMITTEES (REVISED 6/8/95)

RISK ANALYSIS SUBCOMMITTEE

Committee:

Eli Glatstein
Nancy Oleinick
Henry Royal
Philip Russell
Mary Ann Stevenson
Duncan Thomas

Staff:

Jeff Kahn
Anna Mastroianni
Shobita Parthasarathy

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C-1 Meeting Timetable
And Report Deadlines

Divider Title: _____

Meeting Timetable and Report Deadlines

1994	April 21-22	Meeting #1	The Ramada Plaza Hotel (DC)
	May 18-19	Meeting #2	The Ramada Plaza Hotel (DC)
	June 13-14	Meeting #3	The Ramada Plaza Hotel (DC)
	July 5-6	Meeting #4	The Vista Hotel (DC)
	July 25-26	Meeting #5	The Mayflower Hotel (DC)
	September 12-13	Meeting #6	The Ramada Plaza Hotel (DC)
	October 11,12 & 13	Meeting #7	The Donatello Hotel (CA)
	October 21	Small Panel Meeting	The Regal Cincinnati Hotel (OH)
	OCTOBER 21	**INTERIM REPORT SUBMITTED**	
	November 14-15	Meeting #8	Renaissance Hotel (DC)
	November 21	Small Panel Meeting	Westcoast Ridpath Hotel (WA)
	December 15-16	Meeting #9	Omni Shoreham Hotel (DC)
1995	January 19-20	Meeting #10	Omni Shoreham Hotel (DC)
	January 30	Small Panel Meeting	Sweeney Convention Center (NM)
	February 15-17	Meeting #11	Omni Shoreham Hotel (DC)
	March 2	Small Panel Meeting	Radisson Summitt Hill (TN)
	March 15-17	Meeting #12	Madison Hotel (DC)
	April 10-12	Meeting #13	Madison Hotel (DC)
	May 9-10	Meeting #14	Doubletree Hotel (DC)
	June 21-23	Meeting #15	Madison Hotel (DC)
	July 17-19	Meeting #16	Madison Hotel (DC)
	SEPTEMBER	**FINAL REPORT SUBMITTED**	

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C-2 Future Meeting Locations

Divider Title: _____

TAB C-2

ADVISORY COMMITTEE ON HUMAN RADIATION EXPERIMENTS
1726 M STREET, N.W., SUITE 600
WASHINGTON, D.C. 20036

FUTURE MEETING LOCATIONS

Future meetings for the Advisory Committee on Human Radiation Experiments are scheduled as follows:

June 21-23, 1995
The Madison Hotel
15th & M. Street, N.W.
Washington, DC, 20005
202/862-1600

July 17-19, 1995
The Madison Hotel
15th & M. Street, N.W.
Washington, DC, 20005
202/862-1600

✦ *A Public Comment period is scheduled for each meeting.*

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D-1 New Documents
AEC Ethics/ Classification Policies

Divider Title: _____

MEMORANDUM

TO: **Members of the Advisory Committee on Human Radiation Experiments**

FROM: **Advisory Committee Staff**

DATE: **May 25, 1995**

RE: **New Documents Concerning Early AEC Ethics and Classification Policies**

We enclose four documents recently located by staff in the Department of Energy (**DOE**) archives--including a complete copy of the November, 1947 General Manager Wilson letter--that fill in important gaps in our understanding concerning the 1947 Atomic Energy Commission (**AEC**) ethics and classification policies.

Among other things the documents: (l) further suggest that in 1947 the AEC stated a policy requiring therapeutic benefit and "informed consent in writing" in experimentation involving "substances known to be, or suspected of being, poisonous or harmful"; (2) indicate that this standard and the decision to keep secret reports of experimentation that did not conform to it was based on the endorsement by the Advisory Committee on Biology and Medicine (**ACBM**) of a secret draft recommendation of the Medical Board of Review; and (3) indicate that the Manhattan Project's use of "unwitting subjects" underlay the new AEC's 1947-48 secrecy and ethics policy discussions.

The documents are described in chronological order below:

Attachment 1: **June 5, 1947 Letter from Carroll Wilson to Robert Stone**

This letter indicates that the decision to classify reports on Manhattan Project human experimentation reflected concern for the use of "unwitting subjects."

In the spring of 1947 AEC officials considered public relations implications in classifying reports on human experimentation. The June 5 letter refers to a request by Dr. Stone to declassify research in order that it be published in the public realm. In response. AEC General Manager Wilson stated:

> With reference to your third paragraph dealing with medical experiments on human subjects. this action was taken upon the

advice of the Interim Medical Advisory Committee.[1] It is believed that if human beings were the unwitting subjects of experimentation. such experiments might have an adverse effect upon the position of the Commission.

Wilson told Stone that he could seek reconsideration of his request for declassification. but should take "into full account the responsibility of the Atomic Energy Commission in the eyes of the American people and the medical profession in general."

Attachment 2: August 12, 1947 Letter from Carroll Wilson to Robert Stone

This letter confirms that the AEC withheld declassification because some medical reports "involve[d] experimentation on human subjects where the material was not given for therapeutic reasons." Wilson explained that this issue was considered in the June meeting of the Medical Board of Review, the group established by AEC Chairman Lilienthal to advise the new agency on biomedical program and policy.

Wilson explains that the Medical Board of Review had deferred a decision, and recommended that "all Project reports on human experimentation, except where the experimenter is the subject, be withheld from declassification until the question of medical and legal policy for the Commission can be determined."

"I am sure," Wilson concluded, "you appreciate the questions of medical ethics and legal rights that are involved."

Attachment 3: November 5, 1947 Letter from Carroll Wilson to Robert Stone

This document, a continuation of the dialogue with Dr. Stone, is the complete text of the November 5, 1947 Wilson letter. As staff has previously reported, a portion of the letter was extracted in a 1951 letter from Shields Warren to Los Alamos, in response to Los Alamos' inquiry on human experimentation policy. The complete text shows that the questions of ethics and declassification policy were linked, and that the policy cited in 1951 reflected the ACBM's affirmation of an "unpublished and restricted draft" of the Medical Board of Review report to the Commission.

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[1]This group. headed by Stafford Warren. met in January 1947 to formulate a biomedical research agenda for the new AEC. Following the January meeting, the AEC lawyers asked to help formulate legal and financial groundrules for AEC funding of contract research. The result was the April. 1947 letter from General Manager Wilson to Warren, which required documentation from attending doctors that the patient understood the "clinical testing" which he/she was being subjected to.

Wilson told Stone that his declassification concerns had been taken up at the October 11, 1947 meeting of the Advisory Committee on Biology and Medicine. The ACBM:

> reaffirmed the stand of the Medical Board of Review, which met in June, and considers the matter of human experimentation classified where the conditions cited below were not complied with. For your information, the following is quoted from the preliminary unpublished and restricted draft of the report read to the Commissioners:
>
> > The atmosphere of secrecy and suppression makes one aspect of the medical work of the Commission especially vulnerable to criticism. We therefore wish to record our approval of the position taken by the medical staff of the AEC in point of their studies of the substances dangerous to human life. We believe that no substances known to be, or suspected of being, poisonous or harmful should be given to human beings unless all of the following conditions be fully met: (a) that a reasonable hope exists that the administration of such a substance will improve the condition of patient, (b) that the patient give his complete and informed consent in writing, and (c) that the responsible nearest of kin give in writing a similarly complete and informed consent, revocable at any time during the course of such treatment.
> >
> > Were it not for the extreme value and pressure for securing reliable information on the limits of human tolerance of radioactive substances there would be no need for explicit reference to this subject. We wish to see immediate and steady increase in the gravely important subject of human tolerance of radioactivity, but we believe that since secrecy must of necessity mark much of the medical research supported by the federally-sponsored AEC, particular care must be taken in all matters that under other circumstances would be open to investigation and publicity.

It may be recalled that in its public report, the Medical Board of Review spoke strongly

of the need for openness in scientific inquiry, declaring that "in so far as is compatible with national security, secrecy in the field of biological and medical research be avoided." [2]

Attachment 4: November 5, 1947 Letter from Carroll Wilson to Alan Gregg

Gregg served on the Medical Board of Review and as the chairman of the ACBM. Following his letter to Stone, Wilson indicated to Gregg that the ACBM should not issue a policy on human experimentation because the policy articulated by the Medical Board of Review in June, 1947, was adequate.

[2]See page 11 of the Board's June, 1947 report, which appears in the May Briefing Book.

LIST OF ATTACHMENTS

ATTACHMENT 1

June 5, 1947 Letter from Carroll Wilson to Robert Stone.

[Includes marginal notes by ACHRE Staff]

[redacted]

UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON 25, D. C.

June 5, 1947

Dr. Robert S. Stone
University Hospital
The Medical Center
San Francisco 22, California

Dear Dr. Stone:

Your letter of May 7, 1947 relative to the classification of Technical Series Volumes appears to touch upon two classification matters.

Under present policy, sections of the Technical Series Volumes which have been declassified through the declassification system are, when assembled in complete Technical Series Volumes, "Restricted". This procedure is intended to insure that the Atomic Energy Commission has the opportunity to evaluate the over-all effects of the compilation of such quantities of Project information as are represented by the Series Volumes. It is expected that the Commission will take prompt action when the assembled volumes have been presented for scrutiny prior to final release.

With reference to your third paragraph dealing with medical experiments on human subjects, this action was taken upon the advice of the Interim Medical Advisory Committee. It is believed that if human beings were the unwitting subjects of experimentation, the release of information concerning such experiments might have an adverse effect upon the position of the Commission. It would be perfectly proper for you to bring this matter before the Interim Medical Advisory Committee for reconsideration. It would be of assistance to them if you would prepare specific reasons for the release of such material, taking into full account the responsibility of the Atomic Energy Commission in the eyes of the American people and the medical profession in general.

Research without subject consent would be unacceptable from a public relations stand point.

Stone's Research involved blood studies, although it is unclear if what studies Stone referred to in May, 1947

Classification Cancelled
~~Or Changed To~~
By Authority Of DOE
By [illegible]
Date

[redacted]

Dr. Robert S. Stone

June 5, 1947

Thank you for your continued interest and help in our complex medical problems.

Sincerely yours,

Carroll L. Wilson
General Manager

cc: Stafford L. Warren, M. D.

ATTACHMENT 2

August 12, 1947 Letter from Carroll Wilson to Robert Stone.

[Includes marginal notes by ACHRE Staff]

[redacted]

UNITED STATES
ATOMIC ENERGY COMMISSION

AECK-81

Oak Ridge, Tennessee
August 12, 1947

Dr. Robert S. Stone
University Hospital
The Medical Center
University of California
San Francisco 22, California

Dear Dr. Stone:

Subject: DECLASSIFICATION OF BIOLOGICAL AND MEDICAL PAPERS

Thank you very kindly for your letter of July 31, 1947. We welcome constructive comments and criticisms from men, such as yourself, who have been closely connected with the Atomic Energy Project.

Your letter caused me no little concern, and I took immediate steps to learn the full facts of the case. We are withholding declassification of the papers referred to in the letter to Dr. Young because they involve experimentation on human subjects where the material was not given for therapeutic reasons. The letter to Dr. Young emanated from the Declassification Office at Oak Ridge rather than from the Washington Office of the Atomic Energy Commission and was signed by C. L. Marshall, Deputy Declassification Officer.

At their meetings on June 16 and 19, 1947, the Medical Board of Review considered the question of human experimentation with radioactive material lacking therapeutic value. However, they decided to postpone a definite opinion in the matter until a later date. It was the recommendation of the Board that all Project reports on human experimentation, except where the experimenter is the subject, be withheld from declassification until the question of medical and legal policy for the Commission can be determined. I am sure you appreciate the questions of medical ethics and legal rights that are involved.

Clinical and biological reports that are based on studies involving personnel that have had incidental exposure to materials during their work on the Project are not included in the foregoing and are being declassified.

[redacted]

[redacted]

Dr. Robert S. Stone August 12, 1947

I hope that I have been able to clarify the matter for you. If I have not, I should be happy to discuss it further with you.

Very truly yours,

Carroll L. Wilson
General Manager

[redacted]

ATTACHMENT 3

November 5, 1947 Letter from Carroll Wilson to Robert Stone.

[Includes marginal notes by ACHRE Staff]

UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON 25, D. C.

5 NOV 194

Dr. Alan Gregg, Director
for Medical Sciences
Rockefeller Foundation
Room 5500
49 West 49th Street
New York 20, New York

Dear Dr. Gregg:

I want to thank you for your letter of October 14 concerning the questions raised by Dr. Stone in his letter to me of September 18 regarding declassification of biological and medical papers containing information on the experimental use of radioisotopes in human beings conducted under AEC sponsorship.

* I have checked the unpublished and restricted draft of the report of the Board of Review and note that their attitude was clearly stated on this problem. Accordingly, I agree with you that it is not necessary for the Advisory Committee for Biology and Medicine to make a further statement on this subject.

* My reply to Dr. Stone included an extract of the Board of Review remarks. I am sure that this information will assist Dr. Stone in evaluating the present problem and inform him as to the conditions that must be met in future experiments.

Sincerely yours,

Carroll L. Wilson
General Manager

cc: Mr. Wilson
Mr. Derry
Mr. Volpe
Dr. Fidler
Medical Director
Major Dauer

the conditions described by the Medical Board of Review, as described by Wilson to Stone on 11/5/47, were the policy for future experiments

ATTACHMENT 4

November 5, 1947 Letter from Carroll Wilson to Alan Gregg.

[Includes marginal notes by ACHRE Staff]

[redacted]

[redacted]

5 NOV 194

Dr. Robert S. Stone
University of California
Medical School
The Medical Center
San Francisco 22, California

Dear Dr. Stone:

Your letter of September 18 regarding the declassification of biological and medical papers was read at the October 11 meeting of the Advisory Committee on Biology and Medicine. The Committee reaffirmed the stand of the Medical Board of Review, which met in June, and considers the matter of human experimentation classified where the conditions cited below were not complied with. For your information, the following is quoted from the preliminary unpublished and restricted draft of the report read to the Commissioners:

for "dangerous" experiments, informed consent should be obtained from both the patient and next of kin.
Policy applied to AEC-sponsored experiments with patients.

"The atmosphere of secrecy and suppression makes one aspect of the medical work of the Commission especially vulnerable to criticism. We therefore wish to record our approval of the position taken by the medical staff of the AEC in point of their studies of the substances dangerous to human life. We believe that no substances known to be, or suspected of being, poisonous or harmful should be given to human beings unless all of the following conditions be fully met: (a) that a reasonable hope exists that the administration of such a substance will improve the condition of the patient, (b) that the patient give his complete and informed consent in writing, and (c) that the responsible nearest of kin give in writing a similarly complete and informed consent, revocable at any time during the course of such treatment.

"Were it not for the extreme value and pressure for securing reliable information on the limits of human tolerance of radioactive substances there would be no need for explicit reference to this subject. We wish to see immediate and steady increase in this gravely important subject of human tolerance of radioactivity, but we believe that since secrecy must of necessity mark much of the medical research supported by the federally-sponsored AEC, particular care must be taken in all matters that under other circumstances would be open to investigation and publicity."

[redacted]

We trust that this information will assist you in evaluating this problem.

Your continued interest and support in our biological, medical and health physics program is deeply appreciated.

Sincerely yours,

Carroll L. Wilson
General Manager

Copy No. 1 Addressee
2 Mr. Wilson
3 Mr. Derry
4 Mr. Volpe
5 Dr. Fidler
6 Medical Director
7 Major Dauer
8 File

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D-2 Project Sunshine
"Body Snatching"

Divider Title: _____

✦✦✦DRAFT✦FOR DISCUSSION PURPOSES ONLY✦✦✦

MEMORANDUM

TO: **Members of the Advisory Committee on Human Radiation Experiments**

FROM: **Advisory Committee Staff**

DATE: **June 9, 1995**

RE: **Documentary Update on Project Sunshine "Body Snatching"**

In a February staff memorandum (Briefing Book 11, Tab I-1) we discussed the Atomic Energy Commission's (AEC) fallout data gathering programs, Projects Gabriel and Sunshine. As part of Project Sunshine, which sought to measure strontium-90, the AEC engaged in an effort to collect baby bones from domestic and foreign sources. As discussed in the prior memorandum, the project involved the use of a cover story (those without clearance being told that the skeleton collection would be used to study naturally occurring radiation, and not that from fallout). Key participants in Project Sunshine at its onset included the AEC's Division of Biology and Medicine (DBM), its Director John Bugher, Columbia University's Dr. J. Laurence Kulp, and the University of Chicago's Dr. Willard Libby (who became an AEC Commissioner).

A 1955 transcript classified as "secret" (located in the classified materials at the National Archives and recently declassified at the Committee's request), sheds more light on the role of tissue sampling in Project Sunshine. The transcript shows that considerable thought had been devoted to best ways to establish channels to procure "human samples," and the impact of secrecy on the effort. AEC Commissioner Willard Libby, who was a primary proponent of Project Sunshine, explained the great value of "body snatching," and noted that the AEC had even employed an "expensive law firm" to "look up the law of body snatching." The transcript further shows, as was also shown by the materials in the February Briefing Book, that the AEC tissue sampling program relied on personal contacts with medical professionals and other acquaintances who themselves lacked security clearance and were not aware of the still secret Sunshine efforts. The transcript does not indicate discussion of the need to reveal the activity to the subjects of sampling (where alive) or their family members.

We attach pertinent excerpts from the transcript as Attachment 1. Please let us know if you would like the complete 196 page transcript.

✦✦✦DRAFT✦FOR DISCUSSION PURPOSES ONLY✦✦✦

The Importance of "Body Snatching" to Project Sunshine

The transcript records a January 18, 1955 "Biophysics Conference" convened by the DBM to discuss the Gabriel-Sunshine program in light of the hydrogen bomb testing that was taking place in the Pacific.

At the onset of the meeting, DBM director Bugher explained that the hydrogen bomb tests in the Pacific required an acceleration of the research program: (Tr.3)

> The picture we had two years ago of being able to continue in a reasonably measured pace with respect to problems in environmental contamination has obviously not been possible to maintain. That is, events have overtaken us, and we must of necessity put on an accelerated program here as an expression of the fact that weapons have taken an enormous leap in energy release during the intervening time.

Dr. Bugher alluded in particular to the worldwide public attention that followed upon the accidental irradiation of the Marshall Islands following the March, 1954 Bravo test: (Tr. 2-3)

> The events of this spring during the Castle [nuclear weapons test] series were such as to bring very dramatically to the public attention the fundamental character of the things with which we are dealing and the necessity for precise knowledge and good prediction.

The meeting was then turned over to Dr. Libby, who was now an AEC Commissioner. Dr. Libby began by stating that there was no effort more important to the AEC than Sunshine. However. "[t]here are great gaps in the data." He explained that: (Tr. 6)

> By far the most important [gap] is human samples. We have been reduced to essentially zero level on the human samples. I don't know how to get them but I do say that it is a matter of prime importance to get them and particularly in the young age group.

The supply of stillborns had evidently been shut off: (Tr. 7)

> We were fortunate, as you know to obtain a large number of stillborns as material. This supply, however, has now been cut off also, and shows no signs, I think, of being rejuvenated.

Therefore, Libby told the audience, expertise in "body snatching" would be highly valued: (Tr. 8)

> So human samples are of prime importance and if anybody knows how to do a good job of body snatching, they will really be serving their country.

Libby recalled that when Project Sunshine was created in 1953, a law firm was hired to study this problem: (Tr. 12)

> I don't know how to snatch bodies. In the original study on the Sunshine at Rand [the Rand Corporation] in the summer of 1953, we hired an expensive law firm to look up the law of body snatching. This compendium is available to you. It is not very encouraging. It shows you how very difficult it is going to be to do legally.

The law firm memorandum has not yet been located by the staff.

The Availability of "Channels" of Supply

The conferees discussed the need for a wide enough variety of samples to cover age ranges and potential variations among body parts. Dr. Kulp, from Columbia, explained that there were "channels:" (Tr. 16-17)

> Dr. Kulp: . . . we have the channels in these places where we are getting everything. We have three or four other leads where we could get complete age range samples from different other geographic localities. These three are Vancouver, Houston, and New York. We could easily get them from Puerto Rico and other places. We can get virtually everyone that dies in this range.
>
> Commissioner Libby: These are operative materials?
>
> Dr. Kulp: No. This is all deaths between one and thirty.
>
> Commissioner Libby: That is wonderful.
>
> Dr. Kulp: We have 20 coming from Vancouver and 20 from

> Houston in this range that have already been taken. So the channel is there, and the samples are flowing in.
>
> Commissioner Libby: That is fine, Larry. Maybe you can reveal your technique to the other groups.

Dr. Kulp further explained: (Tr. 81)

> Down in Houston they don't have all these rules. They claim that they can get virtually and they intend to get virtually every death in the age range we are interested in that occurs in the City of Houston. They have a lot of poverty cases and so on . . . .
>
> Furthermore, before coming down to this conference I talked at some length with the Dean of the Columbia Medical School, and he has contacts all over the world where he is sure we can develop identical programs. In particular, we could develop a program in Australia, South America, Africa, in the Near East, and in Scandinavian countries, if the conference and the people here would like to have this developed.

Secrecy and the "Body Snatching Problem"

The conferees discussed the way in which secrecy inhibited the program, but also noted the public relations consequences of openness.

Dr. Libby suggested that the Commission might help make human samples more readily available by downgrading the "Sunshine classification:" (Tr. 12)

> At least the existence of the project I hope we will get away with revealing. Whether this is going to help in the body snatching problem, I don't know, I think it will. It is a delicate problem in public relations, obviously. I don't know. This is a major objective.

Dr. Kulp explained that the difficulties could be overcome by good personal relations: (Tr. 186-87)

> You have to have personal interest in and almost a friendly tie to develop this, because you have to have the medical records. Yet

> you have the security problem to deal with, too. I found in these cases that we have been able to develop that there was enough understanding and confidence so that the men did not require you to tell them anything except that they realized it was something confidential. They could guess, and they probably didn't guess very far wrong, but they were willing to cooperate on the basis that this was an important thing. I think with this connection through one of the top medical people who is internationally known, it will not be hard at all to establish the sites that we should establish.

Dr. Bugher of DBM said the AEC was exploring a special security clearance: (Tr. 187)

> There is the system of L clearance, whereby when the need exists, we can disclose even restricted data to individuals not Q cleared, but who have had a preliminary check. That might be something which could be done without that individual having to fill out any forms or anything of the kind.
>
> For this purpose you are not dealing with irresponsible people. You are dealing with directors of hospitals and pathologists, and persons in general who have an understanding of the seriousness of the project in which we are engaged . . . .

There was also discussion of the use of a "blind:" (Tr. 187-188)

> Dr. Bugher: . . . Then the other side of it is that we have a real interest in other things in the bones. I sometimes get worried that we don't take full advantage of our material. We are interested in trace elements of all kinds for that matter. So we have a potential chemical study here which is quite far-reaching.
>
> The thing that Dr. Kulp outlined is one way of getting human material. Are there any other schemes we might employ?
>
> Mr. Eisenbud [an AEC New York office official]: If you need to do it behind a blind as you might even want to do in the event it was declassified--you would still have a potential public relations problem--the blind could be the trace elements program as we have discussed. I am currently exploring with a medical examiner in

> New York the chance of using his toxicological experiments for that. There would be a three way tie. We would give them a little support. That would give them the incentive of correlating everything in the New York metropolitan area, and we would take a good slice of it.

Foreign Sampling

Finally, there was discussion of the need for resources from other countries. Colonel Maxwell of the Armed Forces Special Weapons Project suggested the Armed Services could provide some help for specimens of the local population. The possibilities included Germany, a native hospital in Formosa and the Navy unit in Cairo. (Tr. 189-190)

Dr. Bugher explained that, in other contexts, it had been necessary to pay by the sample: (Tr. 190-191)

> I don't think that in other areas where one wants human material collected you have ever been successful unless you had someone locally responsible who got paid by the sample. Over the years in the Rockefeller Foundation in the yellow fever studies, we got tens of thousands of liver specimens all through Latin America by that scheme. Of course, we lost a few agents, too, who got shot or knifed, but not very many actually.
>
> It was a complete failure in Africa, though, for interesting reasons irrelevant to this thing. You almost always have to have somebody who sees some reward for himself in doing this work. It is too much to ask of people that they maintain a pure exalted scientific enthusiasm and are just collecting samples.

The Sunshine Research Becomes Public

As noted above, following the irradiation of the Marshall Islands in 1954, weapons testing and fallout became the subject of growing international debate. In 1956, Democratic Presidential Candidate Adlai Stevenson raised the issue during the Presidential Campaign. In October of that year Dr. Libby gave a presentation on the strontium research at the dedication of the American Association for the Advancement of Science's new building in Washington. In February, 1957 Dr. Kulp's report on "Strontium-90 in Man," based on data from the worldwide network, appeared in Science. (Attachment 2)

✦✦✦DRAFT✦FOR DISCUSSION PURPOSES ONLY✦✦✦

In May and June, 1957 the Congressional Joint Committee on Atomic Energy held its first major hearing on fallout.

In June, in response to a proposal from the National Institutes of Health (NIH) for the collection of children's milk teeth to measure strontium-90, Commissioner Libby wrote that the idea was good, but: (Attachment 3)

> However, I would not encourage publicity in connection with the program. We have found that in collecting human samples publicity is not particularly helpful.
>
> We could get the teeth going by having investigators make their own collections. The samples need not be too large. Dentists would help.

LIST OF ATTACHMENTS

ATTACHMENT 1:	**January 18, 1995 transcript classified as "secret" of the Biophysics Conference, held in Washington, DC**
ATTACHMENT 2:	**February 8, 1957 article by Dr. Kulp's on "Strontium-90 in Man" in *Science* Magazine**
ATTACHMENT 3:	**June 10, 1957 letter from W.F. Libby, Commissioner, AEC to Dr. Herman M. Kalckar, NIH, in response to a proposal from NIH.**

ATTACHMENT 1

January 18, 1995 transcript classified as "secret" of the Biophysics Conference, held in Washington, DC

SECRET

This document contains 196 pages
No. 1 of 2 Copies, Series A

UNITED STATES ATOMIC ENERGY COMMISSION

DIVISION OF BIOLOGY & MEDICINE

In the Matter Of:

RESTRICTED DATA

This document contains restricted data as defined in the Atomic Energy Act of 1954. Its transmittal or the disclosure of its contents in any manner to an unauthorized person is prohibited.

BIOPHYSICS CONFERENCE

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Place - Washington, D. C.

Date - January 18, 1955

Pages.....1-196.....

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Biophysics Conference Notes (Title Pg + 196 pgs).

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UNITED STATES ATOMIC ENERGY COMMISSION,

DIVISION OF BIOLOGY AND MEDICINE

- - -

BIOPHYSICS CONFERENCE

Room 1201, T-3 Building,
Washington, D. C.
Tuesday, January 18, 1955.

The Biophysics Conference convened at 9:00 a.m., with the following in attendance:

FORREST WESTERN, AEC
JOHN BUGHER, AEC
R. A. DUDLEY, AEC
H. WECHSLER, USWB
LESTER MACHTA, USWB
WILLIAM W. KELLOGG, RAND CORP.
JERALD E. HILL, RAND CORP.
A. E. BRANDT, AEC, NYOO
ROY E. ALBERT, AEC
N. S. HALL, AEC
N. D. GREENBERG, LT.COL., AEC
GORDON DUNNING, AEC
WALTER D. CLAUS, AEC
KENNETH W. ERICKSON, SANDIA-WSEG
EDWARD A. MARTELL, U. OF CHICAGO
EDWARD B. GILFILLEN, JR., ORO, JHU
ROX D. MAXWELL, AFSWP
HAROLD H. MITCHELL, M.D., AFSWP
RONALD G. MENZEL, USDA
VICTOR J. KILMER, USDA
STERLING HENDRICKS, USDA
KERMIT H. LARSON, AEP/UCLA
W. F. LIBBY, AEC
H. L. VOLCHOK, LAMONT GEO. OBS.
J. LAWRENCE KULP, LAMONT OBSERVATORY
GEORGE A. WELFORD, AEC.
IRA B. WHITNEY, AEC
JOHN H. HARLEY, AEC
LYLE T. ALEXANDER, USDA
MERRIL EISENBUD, AEC.

AEC

P R O C E E D I N G S

DR. BUGHER: We will come to order, please. We are a little late, and I notice the clock is four minutes slow anyway.

The conference here as before is a classified conference. I will assume that no one is present who is not cleared for restricted data. I also will ask Dr. Dudley to make sure that we have circulated a sheet of paper for each one to sign his name and his affiliation so that we have an incontrovertible record of attendance. There seem to be more people in the room than the list of names I have. So the conference seems to have grown somewhat from the small working one that we first had in mind.

This rather reflects the expanded interest in the subject, I think, inasmuch as at the time we met last here we were concerned with planning in part for the securing of the maximum amount of data that we could get from the Castle operation.

We now have the possibility of hearing from various people how well those plans worked out and how we stand with reference to the problem as a whole. We are all aware that the whole problem implied in Gabriel-Sunshine had increased in its consequences and certainly in public interest since we last met. The events of this spring during the Castle series were such as to bring very dramatically

to the public attention the fundamental character of the things with which we are dealing and the necessity for precise knowledge and good prediction.

The picture we had two years ago of being able to continue in a reasonably measured pace with respect to problems in environmental contamination has obviously not been possible to maintain. That is, events have overtaken us, and we must of necessity put on an accelerated program here as an expression of the fact that weapons have taken an enormous leap in energy release during this intervening time.

We are, however, in some aspects in a gratifying situation. It is always much more satisfying scientifically to know what one is talking about, and if you think back over the past years you will realize that at the time of the last conference, particularly, we had data from which to talk rather than speculation. We hope that during the day this situation will be even more satisfactory to you.

We have a great deal of factual material. We have obvious need for a great deal more. But nonetheless we are able, I think, now to speak quantitatively of many of these factors in environment and base our future planning on what is fairly clearly lacking in a quantitative sense.

I think it is perhaps not pertinent at the moment to go too much into these problems, but I think if you keep in mind during the day that the many very important things

which remain to be determined are very fundamental -- such matters as the rate with which such elements as strontium 90 mix into the whole environment, both the inanimate and the biologic environment, the rate at which such material may reach equilibration in nature is something that we have not yet determined, but is very fundamental to the whole problem.

The ultimate implication biologically of these problems of environmental contamination have not been assessed. You will have noted the great upsurge of comment by geneticists, for example, which range all the way from one of extreme apprehension at one end, to almost equally extreme disregard of the whole subject on another. The thing that is wrong is that they are all talking largely from a basis of opinion rather than conclusion.

I met with a group of geneticists not long ago and told them that they had to regard themselves now as a branch of science which had grown up, and they had to accept certain responsibilities, and realize that they could not with complete intellectual stability, if you want to call it that, or freedom, perhaps, indulge in all sorts of speculation based on nothing at all particularly except a desire to be dramatic. They have now a social responsibility which they must recognize and that we would be very grateful for scientific conclusions based on good data. We would be less

grateful for pure opinion based on nothing at all. But nonetheless, we will continue, I think, to be subjected to a great many pressures which spring from serious concern, even though the individual concerned may not be in full possession of good facts.

I see I am down here for purpose, scope and goals of conference. They are the same as before, of course. To re-examine what we have learned during these past months since the last conference, to try to put all of this material in perspective, to see how nearly we can come to answering some of the essential questions of Gabriel-Sunshine.

In this conference the problem of strontium 90 is of special emphasis, and to lay out what more is needed in the immediate future. I think you will be pleased part of the time and you will be disturbed part of the time during the day at our present status. We have gone a long way in the last two years, and we are faced with some problems of major character for which at the moment we do not have adequate data and yet it is most important that those data be accumulated -- some of them -- within the very near future. We do not, I think, expect to lay before you a completely satisfactory situation. There is a certain sense of urgency which will come out during the day in some of these aspects.

I think we will consider those to be my opening remarks for whatever they are worth, and get to some remarks

of more specific substance. We are fortunate in having Dr. Libby with us, on the Commission now, and while his time is not wholly his own, as is true with most people around here, he has been able to get away from other things and spend at least part of the day with us.

I would like to turn the floor over to Dr. Libby now for his comments on our present program.

COMMISSIONER LIBBY: Thank you, John.

I am sure the importance of the Sunshine project is obvious to all of you, but to keep the record straight, I would like to say that there is no effort which the AEC is engaged in which to my mind is more important than this project.

I would like to remark a little bit about the data as obtained so far. There are great gaps in them. I think we can call feel quite proud that we have as much as we have, but there are very great gaps in the data, and it should be our prime objective to fill those holes. I don't think I need to tell you what these gaps are. They are probably obvious to all of us. Again to keep the record straight I will mention the most important of those.

By far the most important is human samples. We have been reduced to essentially zero level on the human samples. I don't know how to get them, but I do say that it is a matter of prime importance to get them and particularly

in the young age group. It is clear, I think, from the data and from the systematics of the biosphere that adults are not of much interest in that adults are not expected to pick up strontium 90 or to have picked it up, and though we should from time to time check with adult materials, adult materials will not in general serve our purposes.

We were fortunate, as you know to obtain a large number of still born babies as material. This supply, however, has now been cut off also, and shows no signs, I think, of being rejuvenated. These were not perfect samples. These materials undoubtedly reflected the metabolic pool of the mother, and were therefore low in their assay. As you recall, they ran about 1/6000th of tolerance, so we might suppose that equilibrium humans -- if we can define this being as one who has been raised and has lived his whole life on the strontium 90 contaminated calcium which we measure in the milk shed samples, in the soil samples and the calf bone samples -- the equilibrium human I should imagine would be about as contaminated as the calf bones.

This is not too comforting because the calf bones run about -- I have forgotten the number -- 1/300th of tolerance. 1/300ths of tolerance on the calf bones at least would seem to me to mean that one to three year old children would have the same tolerance if we could get the samples. We have never had such a sample. We have never had a single

individual in this range. We have had certain pool samples consisting of operative materials of rather small amount. There is a dangerous thing to watch here. When you get a small sample of bone, the error of measurement may be too large to make the sample of any use.

We must also watch in operative materials to be sure that they are significant. I don't know that you would expect a forearm would differ from the whole body ash or not.

DR. BUGHER: That is one of the things to be determined, I think.

COMMISSIONER LIBBY: We must be careful.

So human samples are of prime importance and if anybody knows how to do a good job of body snatching, they will really be serving their country.

I see another gap which is very serious, and that is in the ocean. We tried at Chicago to measure ocean water and were never able to do it. We didn't try hard enough. I am sure it can be done. It has not been done to my knowledge. So we ought to work on the ocean.

In some other connections a colleague of mine and Iat the University of Chicago just before I came down here, obtained a most interesting result, namely, that the oceans mixed to a depth of 100 meters in a period of 18 years on a world wide basis. The evidence for this might interest you.

The cosmic ray tritium content of surface ocean water is one fifth of the content of the ocean rain. The lifetime of tritium is 18 years. You figure this out and it means one hundred meters is the equivalent mixing depth for dilution of rain by seawater in 18 years.

This has important consequences for fission product disposal in that what we thought was an infinite sink if this result is right. Maybe this result is wrong. You can examine those data if you wish from that point of view. If this result is right, we can expect that the soluble fission products will be retained in the top 100 meters of seawater essentially indefinitely.

What is happening to strontium in this respect. Strontium is not soluble. Itis not very soluble. The question whether the strontium is there or not I think is one of some importance and some interest. It is probable that the fish, since they live in this top 100 meters., will come to a level of strontium 90 essentially I suppose the same as this top 100 meters of soluble calcium. So I think it is an important objective to check the strontium 90 content of seawater, and compare it with the strontium 90 content of fish living in the sea.

One can easily show that if this 100 meters is right, seafood is always going to be pretty low. It is even going to be a difficult measurement to make. This affords

a wonderful opportunity to integrate the total strontium fallout, and we should do it for this reason as well as assurance that our general understanding of the whole strontium 90 problem is correct.

One other gap of less importance is in the sampling of air. I think you probably noticed as you have looked at the data there is some system and sense in the soil data in that if you integrate to the center of the earth, so to speak -- that is, integrate down a foot or so -- there is not as large a variation from spot to spot as there is if you take the top one inch. In other words, if you take soil samples and take all the strontium 90 contained in them, then there is a regularity in this integrated total strontium 90 fallout as recorded in the soils which indicates that soil sampling in itself affords the opportunity to measure the total strontium 90 fallout.

An additional chance to check on this is by measuring the strontium 90 content of rain, which, of course, is the means by which the bulk of the strontium 90 gets in the soil. If you take the rain data that are available, you will find that these numbers are not too unreasonable.

Presumably there is in the atmosphere some place a storehouse of strontium 90 which makes it possible that the rain be contaminated by strontium 90 months after any bomb test. There is only one place for this, and this is

above the tropopause. So high atmospheric sampling is of extreme importance, and you gentlemen in New York have done a very good work on that. I think more needs to be done. I am not sure that we know the right methods. I think there is no doubt that this is an extremely important objective.

What are we aiming at? We are aiming at an equation which shows us that everything checks. We should have the storage time of strontium 90 in the stratosphere. We should have the strontium 90 content of the stratosphere. We should settle that rain is the mechanism by which strontium 90 is carried down, once it gets below the tropopause. I am sure that this is it, but this needs to be proven. We should show that all of the things check.

What are these things? The content of strontium 90 in the sea, the content of strontium 90 in sea rain, the content of strontium 90 in land level, in high level atmospherics, and in ordinary air, as recorded by air filters, plus the frequency with which the air is washed down by rain. When these things are all measured and checked, I am confident they will fit together.

Then we can say without doubt that we know what we are talking about when we say there is so much strontium 90 in the atmosphere, and so much has fallen. In other words, what we are aiming at is a complete worldwide assay for strontium 90, not only in the biosphere, but in the

lithosphere itself. We must have the whole world assay for strontium 90 with the objective of finding out its effect on human life.

These are all the gaps I see. I think that there probably are some more, but I think these are the major ones. I don't know how to snatch bodies. In the original study on the Sunshine at Rand in the summer of 1953, we hired an expensive law firm to look up the law of body snatching. This compendium is available to you. It is not very encouraging. It shows you how very difficult it is going to be to do it legally. We may be able to help -- I speak now of the Commission -- in that we hope to downgrade the Sunshine classification. At least the existence of the project I hope we will get away with revealing. Whether this is going to help in the body snatching problem, I don't know. I think it will. It is a delicate problem in public relations, obviously. I don't know. This is a major objective.

There are certain points in the data which are intriguing to me. I guess you notice that calf that Dr. Martell measured which was 18 sunshine units. Was there anything extraordinary about that calf?

DR. ALEXANDER: It is in a higher rainfall area in Wales. The three calves from the east side of Britain in the low rainfall area are rather consistent, and down a third or so.

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COMMISSIONER LIBBY: It is very interesting to see that you can explain that. Perhaps it follows from that that there are palliative measures. Last summer in Berkeley I took a week off and worked on the problem of purifying milk. We took milk and loaded it with strontium isotope and calcium isotope for tracer and removed the alkaline earths with ion exchange resin, first by using a column, and then by just stirring the powder. Stirring the powder worked very well. It was possible to remove the calcium and strontium from the milk essentially completely by adding, I would say, a couple of tablespoonfuls of resin to a pint of milk. I guess it was less than that. Maybe half a pint of milk -- and stir it and let it settle.

I don't know whether this is conceivable as an economic method of the purification of the milk, but I think it is something that should be followed up. Milk is the main way in which strontium gets into use, and I think it certainly should be followed up.

The questions are, is this thing cheap enough and is the milk still nutritive. Of course, you would obviously have to fortify it with calcium afterwards, but have some other essential values been removed in this treatment.

The whole problem of palliative measures which I think nobody on Sunshine has had any time to give any thought to really, if it be true that soils rich in calcium produce

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particularly safe food, this is not an unimportant observation. I must say it certainly seems reasonable.

DR. ALEXANDER: Dr. Harley will have some data to present today on the lowest place we know in the United States down at Tipton. I went down there this fall and picked an improved pasture, compared with this native range, and it is reflected both in the vegetation and bones of the animals.

DR. KULP: Concerning this human material we have excellent sources in three places for human material. They are entirely measurable.

COMMISSIONER LIBBY: How big were they?

DR. KULP; Two to 20 grams.

COMMISSIONER LIBBY: But these were adults.

DR. KULP: The examples we are getting will cover the entire range.

COMMISSIONER LIBBY: The only ones that are of interest are the young ones. Is this true?

DR. BUGHER: No.

COMMISSIONER LIBBY: Why are adults interesting? There is no strontium in them.

DR. KULP; There is some strontium in the 30 year olds. It cuts of f at about 40.

COMMISSIONER LIBBY: Really? How many Sunshine units?

DR. KULP: .1, .2.

COMMISIONER LIBBY: That is very frightening. What

is your error?

DR. KULP: .02.

DR. MARTELL: We just completed measuring for calcium adult samples and these assayed .001, plus or minus .001, and they are essentially dead now. There might be some basic reason for this, but the adult samples do not look very interesting to us.

COMMISSIONER LIBBY: Let us bear down on this point a bit. It certainly seems reasonable that the calcium in adults is pre-atomic. Isn't this reasonable?

DR. BUGHER: Mostly.

COMMISSIONER LIBBY: Therefore, it will have to be lower than youngsters.

DR. KULP: I certainly would not debate this. What were the ages of these?

DR. MARTELL: These are in the ages above 20.

DR. KULP: Do you know exactly the age?

DR. MARTELL: I have not received the full detail.

DR. KULP: This is very important.

COMMISSIONER LIBBY: You can get it today?

DR. KULP: We have 20 to 30 samples completed now ranging from 70 down and they are indeed dead at 40, and then they pick up. It happens these are the easiest to get. Up to now we have gotten down to 30, and they increase at 30, and at 30 we have three samples that are .2.

COMMISSIONER LIBBY: What part of the body is this?

DR. KULP: These were ribs.

COMMISSIONER LIBBY: Is there any reason that ribs would interchange calcium faster?

DR. BUGHER: The difficulty there is we haven't yet any good analyses of different locations in the same individual. That remains to be done. Whether or not the ribs constitute a fair sample of the whole body pool I really don't know, but that is something to be demonstrated.

DR. KULP: The main point I wanted to bring up now was not to get into a detailed discussion of the data, but to emphasize that we have the channels in these places where we are getting everything. We have three or four other leads where we could get complete age range samples from different other geographic localities. These three are Vancouver, Houston and New York. We could easily get them from Puerto Rico and other places. We can get virtually every one that dies in this range.

COMMISSIONER LIBBY: These are operative materials?

DR. KULP: No. This is all deaths between one and thirty.

COMMISSIONER LIBBY: That is wonderful.

DR. KULP: We have 20 coming from Vancouver and 20 from Houston in this range that have already been taken. So the channel is there, and the samples are flowing in.

COMMISSIONER LIBBY: That is fine, Larry. Maybe you can reveal your technique to the other groups.

DR. WESCHLER: May I make a comment about the slow rate of mixing in the ocean? I am very puzzled by that. Just from energy considerations, it seems as if the mixing should be much more rapid. For example, knowing how much solar radiation falls, and how much it sends back in space in rough numbers, and knowing also the very small seasonal range of temperature at the sea surface, it seems that if the net heat which is given must be mixed through a layer at least 100 meters thick in the order of days, certainly within a year.

COMMISSIONER LIBBY: It mixes air incidentally, but it doesn't go beyond.

DR. WECHSLER: I thought you said 18 years.

COMMISSIONER LIBBY: I am sorry. What I meant is this. The rain falls and it mixes immediately, but it doesn't go beyond.

DR. WECHSLER: 100 meters.

MR. EISENBUD: We have data to support that.

COMMISSIONER LIBBY: It is statification. The only thing we have added to it, Dr. Wechsler, is the time. The oceanographers have long known about this stratification but they didn't have the time. Maybe we haven't got it, but that is what our data indicate -- 18 years. We have

that is a very interesting point. The argument hangs together qualitatively. I am not suggesting that it is quantitative.

DR. CLAUS: In order not to put the squeeze on Dr. Kulp's preparation, willyou make it a point to bring this up this afternoon in the discussion of the possible future program as being something which might be looked at very seriously in continuing studies on the whereabouts of material?

Dr. Kulp.

DR. KULP: I would like to separate my remarks into a few comments on technique development during the past year, a summary of some of our results which I think are sufficiently now that many of you have not seen them -- in fact, some of the critical points have come up only in the last two months with some of the samples that have been measured -- and finally just a word or two about our present intentions for the immediate future.

Regarding technique, we have worked along with this low level beta counter which was described in detail in our last annual report. It has proven very simple in operation and very reproducible and we now have a routine background of 2.40 plus or minus .01 reproducible from week to week and day to day.

This enables us to take relatively small samples

and as long as we can allow accounting time of at least a day we can do with fair precision down to samples of one gram or two.

Obviously you would like to get smaller than this, but with human examples, this is it.

Secondly, with technique, we have recently developed a change in our chemical procedure which we think is a significant improvement. This may be of interest only to a very few of you here, but I think a word is worthwhile. Those of us who have been running these bone and cheese samples and milk samples have been aware that the calcium phosphate milking, the yttrium phosphate milking is quite a messy step. You have to run it hot. You have to control the pH. You have to filter and the temperature and pH changes. It is common with this milking to wind up with a very dirty looking precipitate. This does not make a large difference, but it is not the kind of thing you like.

Within the last six months we have been able to develop a procedure in which we first precipitate the calcium strontium at a pH below one, then we ignite this to calcium oxide, and take it up to calcium chloride, and we have a simple clean solution. There are no problems after that. All you have to add is the yttrium carrier, raise the pH to 8 or 9, and then take this back to solution, and finally precipitate. It is a nice coarse granulate oxylate for

counting. That seems to have been a worthwhile improvement.

Regarding the data, our main emphasis this year has been on human samples. We have set up the following stations for complete sampling. We have a center at Vancouver, at Houston and New York. While the samples are just coming in now, at least the younger samples -- when we first asked for samples, we got the easiest ones which are people over 60 years of age -- I think it would be worthwhile to recount very briefly what these results show.

At the present time from the New York City area we have age range, let us say, greater than 70 years, several samples, all less than .01. Within the experimental area, some of the experiments you can say less than .03.

DR. CLAUS: These are Sunshine units?

DR. KULP: Yes. In the age bracket o f 60, we have six or eight samples, and this is less than .01 or .02 depending on size of sample.

In the 50 range, we have four or five in this report, and several others in the mill, and that is also less than .01 and .02.

Then we get down in the forties, and the lowest one we have that is less than .01 is .043. We have a 43 year old male there.

Then from the New York City area we have a 37 year old male at .04. These people died in 1954. 34 year old

female, .07. This is a plus or minus .01, and the second plus or minus .02.

We have a 32 year old male, .21 plus or minus .04. We have a 31 year old male, .20, plus or minus .08.

Then we have an interesting pair of a mother that died in childbirth, and the offspring, a 42 week old fetus. The mother was 29 years old, gave .20 plus or minus .05; the child, 42 weeks, gave .65 plus or minus .19, a very small sample.

Down in Houston they don't have all these rules. They claim that they can get virtually and they intend to get virtually every death in the age range we are interested in that occurs in the City of Houston. They have a lot of poverty cases and so on. They have at least one or two of this kind of pair per month, which is an interesting thing to look at. So we intend to get this. So far this is the New York story. This involves a total of maybe 30 measurements.

We have now told our people in the medical school to cut off here and only send us one or two in the forties every once in a while. They are now concentrating and they are going to fill out between 40 and one. We have a good guarantee in the next two months of at least 20 in that range.

From Houston we have about 30 samples in the laboratory of which at this time -- they all arrived in the last month -- at this time we have only two of them. Let us

put them down under here. We have a Houston female, 25 years old, of .15 plus or minus .01. We have a 39 year old male, .22 plus or minus .05. This has only been run once. We can reduce this error.

In other areas, we hwe a Philippine Island 30 year old female, .17 plus or minus .04. Of course, we have inter-compared with the New York group on this India sample, which I might list, .15 plus or minus .02, a 16 to 20 year old. We don't know the sex.

It seems rather clear to us that it is of more than casual interest to monitor in certain metropolitan centers the age range from one to 40 and get as many of these samples as we can. That is our present plan.

To say just a word about the next few months, we expect between 10 and 20 samples per month of humans in the range of one to 40 from all three of those sources.

DR. MITCHELL: Are these ribs?

DR. KULP: These are ribs. In the case of Houston we have gotten some leg bone because they don't have to worry how the individual looks when they got through. Most of the Houston stuff will be rib. But they actually did take out some pieces of leg. It seems to me that by June or July we should have a very good picture of the current curve in these areas, and this can be monitored continuously if it is desirable.

Furthermore, before coming down to this conference I talked at some length with the Dean of the Columbia Medical School, and he has contacts all over the world where he is sure we can develop identical programs. In particular we could develop a program in Australia, South America, Africa, in the Near East, and in Scandinavian countries, if the conference and the people here would like to have this developed. Within a matter of three or four months this could be set up so we could get 10 to 20 samples a month from these sites integrating all the humans if we want to see what the effect of diet and locality is. That is the human story.

Next, during the summer we had a sample of ocean water, ocean rain, and both cheese and bone in Europe. The European samples ranged from Hammerfest, Norway, down to southern Spain, and also the Azores. The value in this is that we were able to obtain a current survey of the bone and cheese materials there over a rather wide latitude range to see what the status was in the summer of 1954.

Without taking time to write these down, in brief the European cheeses are running one to three Sunshine units. These were taken as far north as Norway. In the few cases where we have comparison by date in early 1953, it looks like one Sunshine unit and the summer of 1954 it looks like two Sunshine units. We are getting this sort of increase.

controlled animal samples help in that they give some kind of an upper measure as to what could happen if, for instance, a population was all on vegetation. In other words, an upper ceiling that an ordinary population won't touch. Those one year old lamb samples are rather valuable for that reason.

I agreed with Dr. Libby this morning if you could get three, four and five year old children, that would be good, because that is a little hard to come by.

DR. BUGHER: It should not be difficult actually. Any large medical center, particularly with an active pediatrics service, could have quite a large number of autopsies on children who have not had long and complicated illnesses that would profoundly disturb their calcium metabolism. It is a question of getting to the people who can make such material available.

DR. KULP: You have to have personal interest in and almost a friendly tie to develop this, because you have to have the medical records. Yet you have the security problem to deal with, too. I found in these cases that we have been able to develop that there was enough understanding and confidence so that the men did not require you to tell them anything except that they realized it was something confidential. They could guess, and they probably didn't guess very far wrong, but they were willing to cooperate just

on the basis that this was an important thing. I think with this connection through one of the top medical people who is internationally known, it will not be hard at all to be able to establish the sites that we should establish.

DR. BUGHER: I think it is pertinent to remark on the classification side of it, we are hoping to get this Sunshine program, the existence of it, and its objectives, declassified. I thought I had it a month ago, but something slipped. I hope in the next few weeks we will get that in fact into the public domain.

There is one other device which we are exploring under the new act. There is the system of L clearance, whereby when the need exists, we can disclose even restricted data to individuals not Q cleared, but who have had a preliminary check. That may be something which could be done without that individual having to fill out any forms or anything of the kind. We are exploring that mechanism. We have not used it, but it looks like something that might be useful at this point.

For this purpose you are not dealing with irresponsible people. You are dealing with directors of hospitals and pathologists, and persons in general who have an understanding of the seriousness of the project in which we are engaged, and yet that don't wish to go through all the labor of complete Q clearance for this sort of need. We will

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be able, I hope, soon to inform you as to the machinery which can be used for that.

Then the other side of it is that we have a real interest in other things in bones. I sometimes get worried that we don't take full advantage of our material. We are interested in trace elements of all kinds for that matter. So we have a potential chemical study here which is quite far-reaching.

That thing that Dr. Kulp outlined is one way of getting human material. Are there any other schemes which we might employ?

MR. EISENBUD: If you need to do it behind a blind as you might even want to do it in the event it was declassified -- you would still have a potential public relations problem -- the blind could be the trace elements program as we have discussed. I am currently exploring with a medical examiner in New York the chance of using his toxicological experiments for that. There would be a three way tie. We would give them a little support. That would give them the incentive of correlating everything in the New York metropolitan area, and we would take a slice of it.

DR. BUGHER: It sounds very good.

DR. KULP: What do you think of other metropolitan centers in this country? Do you think we have this covered enough? It seems to me that if you have ten stations supporting

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work at this level, the other seven ought to be outside but I would like to know the feeling of the group. It is easy enough to establish enough in this country, but it seems to me that New York, Houston and Vancouver cover it.

DR. BUGHER: Dietary patterns do not differ too much, do they? They are essentially dairy product consuming people from one side to the other. When we get into Latin America, the situation is quite different. We certainly should have some resources in Central America and in South America, Central Africa and Europe.

COL. MAXWELL: I am sure the Armed Services could give you some help, but I am afraid it would not be too much, because they are scattered. There is no pattern as far as the diet is concerned. But I think it could be worked.

DR. BUGHER: For specimens of local population.

COL. MAXWELL: I wouldn't be surprised, but what it could be worked otherwise, too. I don't know how extensive it could be. What did you find out, Bob, or did you find anything out?

DR. DUDLEY: I talked with them again a few days ago. They do have some contacts and they listed certain places from which they thought they might be able to get bone abroad, but nothing really too extensive. They tried Europe before and have gotten nothing from one particular individual they know in Switzerland. Another person from AFSWP is going

to Germany shortly, and if we asked him, he would see what he could get there through informal contacts. They also have a man on Formosa working in a native hospital where they could get samples and perhaps something from the Philippines. They think we ought to get material from Cairo through the Navy unit there, whatever it is called. So they did have a few spots where they thought it should be possible to get samples, and I think probably something more can be done with that than we have yet done. Of course, we did approach them last year and got nothing.

DR. KULP: I think there is a serious problem there. If you don't get enough samples in a given period of time in the age groups you want, you won't get the story. You have to have intensive cooperation. It involves paying an assistant to get the stuff out. That is what we do. You don't have to pay him a lot, but it has to be on a business-like basis.

DR. BUGHER: That is a good point. I don't think that in other areas where one wants human material collected you have ever been successful unless you had someone locally responsible who got paid by the sample. Over the years in the Rockefeller Foundation in the yellow fever studies, we got tens of thousands of liver specimens all through Latin America by that scheme. Of course, we lost a few agents, too, who got shot or knifed, but not very many, actually.

It was a complete failure in Africa, though, for interesting reasons irrelevant to this thing. You almost always have to have somebody who sees some reward for himself in doing the work. It is too much to ask of people that they maintain a pure exhalted scientific enthusiasm and are just collecting samples.

It may be that when you talk with the people at Hartwell later this month, Dr. Dudley, you may be able to shake loose a few human specimens from the British colonies that way. I think they will have the same sort of difficulties that we have. It is probably just as much a laborious thing for them.

Are there any further points you would like to bring up now in connection with this problem of human sampling? There are two things which we want to do. One is to arrive at the degree at which a particular bone may represent an individual skeleton, and second, an acceleration of the sample collection in these various respects that we have mentioned, of age, location and type of society.

It is now 5:25 and if there are points we have missed, now is a good time to bring them up. Walter, do you have something here?

DR. CLAUS: I might make a very short speec. You have been getting off very easy.

I have been so overcome by the wealth of detail

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presented as to how to go about sampling that it is a bit difficult to find any guiding principles, which is really the reason for holding this meeting. I think we are the ones who have to eventually put the whole program into being.

If we tried backing away so that we don't see so much trees and look a little more at the forest, I wonder if this is a fair statement of what we are trying to do, especially with respect to the biological sampling. What we are really looking for is the integrating effects of nature in correlating the activity which finds its way into man as a function of the environmental contamination. That really is a rephrasing of what you said a moment ago. That I think is still the basic principle.

Now, how do we go about this, getting the integrating effect, without worrying ourselves too much about the details. True, we have to do a certain amount of detailed work if we want to really understand the thing. But the answer to this particular porject, I think, lies in the broader viewpoint, rather than a detailed viewpoint. From that point of view what we really want to know is if you know how much activity has fallen out on the ground anywhere in the world, how much of that finds its way into the human skeleton.

Then we have the question, how much of this do we want to do all around the world? How much do we want to do in

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the United States. I think we can perhaps very properly take this viewpoint, as you have now, the greatest amount of activity has fallen in the United States, rather than elsewhere, so that from a purely political viewpoint, if we have to worry about such problems, if we can show that we are not in too bad shape in the United States certainly no one ca worry very much or complain too much about what the situation is in their own country.

I think for our foreign sampling again, we should very carefully correlate the activity which has fallen in the various countries by observing wht has fallen on the soil by soil analyses, and by the analyses from the gum papers to convince ourselves that one or the other is truly representative of our particular type of measurement. Perhaps we might continue to do both of them.

In foreign countries, certainly we would want to have a fair collection of human material representative of the areas about which we know something of the fallout. Whether we want to go into any detail picking up the animal bones and plant material and the soil and the human material, I don't know. Perhaps it might be well to do so in one or two places.

I think from my viewpoint at the moment, I think the plants are the least important. The soil, the animals and the people are the important ones. I don't think we need

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do an awful lot of that around the world. Two or three places ought to be sufficient, particularly if we have a good knowledge of the soil contamination. I think the United States we should do most of our work. We have better access to material here, and we can make more detailed measurements of the soil and the fallout, the plants that are growing on this soil, the animals and the man. If we can show that the uptake in man as a function of his environment is not too serious, then we have gone an awfully long way toward solving our problem.

It was mentioned today that maybe contamination of animals will become of vital importance some time before contamination of man becomes important. In other words, the animals would die off, because of the fact that they absorbed perhaps ten times as much as man. They would disappear as a food supply before man himself is ready to disappear. I am inclined to doubt that particular viewpoint, but the fact remains it is well to keep it in mind, I suppose, and to measure animals or the contamination of the animal skeleton as well as the contamination of the human skeleton.

Also, I believe, as has just been brought out before I got up, the United States is a fairly uniform dietary pattern. It probably doesn't make an awful lot of difference whether you study people in the New York area, in the Houston area or whether you just gather up everything you get and

integrate them as a function of the integrated fallout pattern. These particular pastures here I think are very well selected, and I am not saying that this work should not be continued. But I still believe that it represents a detail which is probably not necessary to answer our overall project. As a scientific problem it is of interest because we would like to know what the chain of migration is of the activity from the soil to man.

I think that we can simplify our problem tremendously if we cut through an awful lot of this detail. As of now, as a person who has a good deal of responsibility in trying to get this material together and come up with an answer to this project, I would like to make a plea that we try to decide exactly what it is that we need and solve that problem first or at least put more effort into that, allowing the details to sort of lag along beside, if necessary.

DR. BUGHER: Thank you, Walter. I think the record which has been made will be abstracted as minutes and condensed. You will send those around to the various people for their corrections, will you not, Bob?

DR. DUDLEY: Yes.

DR. BUGHER: That will become a document then which should cover these points. We can introduce a summary into the report at that time.

I think Walter has already summarized some of the

things. We have already stated two or three times some of the other matters. So a dictated summary at this time I don't think will be necessary.

We thank you very much for your having sat on these seat all day long, and given us your thoughts and advice. I think you will find this program will continue to develop interest, and also new facets of fundamental knowledge, and in a practical sense. We still need a lot more information, I think.

Thank you very much.

(Thereupon at 5:35 o'clock p.m., the Biophysics Conference was concluded.)

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ATTACHMENT 2

February 8, 1957 article by Dr. Kulp's on "Strontium-90 in Man" in *Science* Magazine

8 February 1957, Volume 125, Number 3241 SCIENCE

Strontium-90 in Man

J. Laurence Kulp, Walter R. Eckelmann, Arthur R. Schulert

Radioactive fallout at great distances from atomic explosions produces both internal and external hazards to the human race. The external hazards result from the interaction of gamma rays in the environment on the genes of individuals, which produces an increased mutation rate. The increase in normal gamma background owing to fallout is very small, so far, and it is being carefully monitored (*1*). The tolerable level of external gamma radiation for genetic effects is not well defined (*2*). The internal hazard is primarily the development of bone cancer, because of the presence of strontium-90 (half-life 28 years). Libby (*3*) has discussed the general problem of strontium-90 in fallout and has presented considerable data (*4, 5*) on the concentration of this isotope in various parts of the chain from the atmosphere to man.

This article (*6*) summarizes the results obtained at Lamont Geological Observatory on the strontium-90 content in man (based on a world-wide sampling network) and attempts to evaluate the potential hazard. The work presented here is a part of a more comprehensive study of the geochemistry and biochemistry of strontium-90 that has been in existence at Lamont for several years (*7, 8*). The first experimental verification of measurable quantities of strontium-90 in animal bone, milk products, and soil was made at Lamont in August 1953 following the prediction by Libby, Eisenbud, and others in July 1953 (*9*) that it might be found in detectable quantities if low-level techniques were employed.

At the present time strontium-90 can be found in all human beings, regardless of age or geographic location, provided that a sample of adequate size is available. As is shown here, these quantities are small compared with the maximum permissible concentration (MPC) (1.0 millimicrocuries of strontium-90 per gram of calcium) established by the National Committee on Radiation Protection (*10*). However, the existence of measurable quantities makes it possible to analyze the present distribution of strontium-90 with regard to age, sex, diet, geography, and time. Such information is fundamental for making predictions about the probable effects of future nuclear explosions.

Dispersal

The route of strontium-90 from the time of fission to its uptake in human bones is known in broad outline. The explosion releases the strontium-90 into the air, where it is then carried for great distances. Eventually it is transported to the soil and becomes a part of the base-exchangeable alkaline-earth-metal ions in the upper few inches. Since plants take up this radioactive strontium along with their necessary calcium, human beings ingest strontium-90 from vegetables and milk products.

Kiloton explosions produce debris mainly in the lower atmosphere (troposphere), from which the strontium-90 is deposited in a few weeks (*5*). This debris is deposited in a restricted latitude. Thus the Nevada test series distributed strontium-90 largely over a narrow latitude band in the Northern Hemisphere, with a higher concentration in the United States than elsewhere. Megaton weapons, on the other hand, appear to put most of their strontium-90 into the stratosphere, where it is more or less uniformly distributed with respect to latitude. It then passes very slowly (about 10 percent per year) back into the troposphere (*4*), from which it is rapidly washed out. Megaton explosions, therefore, tend to equalize the world-wide fallout pattern. The local distribution will be modified by rainfall, vegetation cover, and topography, but the latest total fallout data (*1*) support a rather homogenous distribution that shows maximum variations of only a factor of 3 at the longitudes of Africa and New York. The distribution in the Northern Hemisphere remains higher than that in the Southern Hemisphere, because of the kiloton shots from the Soviet and United States test sites.

It is now reasonably well established (*5*) that the scavenging action of rain is responsible for most of the deposition of tropospheric debris. Experiments made at this laboratory during the past 6 months confirm this conclusion. Other experiments at Chicago (*11*) and at Lamont (*8*) indicate that at least 60 to 70 percent of the strontium-90 which has fallen out in the United States is in the soluble form and is therefore available to plants. It is suspected that the megaton debris from the Pacific tests has a higher fraction of soluble strontium-90 than debris from other tests, but this remains to be confirmed. About 80 percent of the strontium-90 is found in the upper 2 inches of the soil, but in some cases detectable amounts may be carried down as far as 12 inches (*8*), because of the type of soil, topography, and drainage pattern. Measurements of the soil and plant content of strontium-90 per gram of available calcium in 1953 (*4*) can be interpreted as meaning that some plants have significant surface retention of strontium-90, or that the concentration of strontium-90 per gram of calcium in the 0- to 2-inch interval is not representative of the true environment of the roots. As the ground becomes progressively more contaminated with strontium-90, however, the surface effects become obscured. Thus, on the East Coast of the United States during the Nevada tests in the spring of 1955, the surface fallout of strontium-90 was only a very small fraction of the total strontium-90 in the plants (*8*).

The total fallout of strontium-90 was

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estimated by Libby (*5*) at the end of 1955 to be about 13 millicuries per square mile in the upper midwestern region of the United States. The soil data of Hardy and Morse (*4*) suggest an average of about 15 millicuries per square mile for the United States. On the basis of the fallout of mixed fission products and an estimate of strontium-90 fractionation at long distances from test sites, Eisenbud and Harley (*1*) calculated a fallout of 13 millicuries per square mile for the United States in late 1955. By comparing the average total fallout on gummed paper for the United States in the fall of 1955 (*1*) with that for the world, a world-wide average deposition of strontium-90 on the soil of 8 millicuries per square mile can be calculated.

The amount of strontium-90 in the soil which gets taken up by the plant depends on the root depth, the calcium content of the soil, and the biological fractionation factor. Menzel (*12*) has shown that strontium is discriminated against by a factor of about 1.4 when it goes from soil to plant. The calcium content of the soil varies greatly. Values of 0.4 to 40 milliequivalents of exchangeable calcium per 100 grams of soil are common. Further, as noted in a foregoing paragraph, the concentration of strontium-90 drops rapidly with depth. These factors make it possible for the concentration of strontium-90 per gram of available calcium to vary by a factor of more than 100 for a given amount of fallout. Since the biological hazard may be stated in terms of concentration of strontium-90 per gram of calcium, it is clear, then, that merely to consider average values of strontium-90 in soil is not sufficient. Thus, although the fallout of strontium-90 per square foot in the New York area in 1955 varied by a factor of 7 (111 to 1010), the range in micromicrocuries of strontium-90 per gram of available calcium exceeded a factor of 40 (6 to 250) (*6*).

Biochemical Considerations

Most people in the United States obtain their calcium through milk products. Here, fortunately, there is a discrimination factor of 7 against strontium from the plant that the cow consumes to the milk produced, according to experiments by Comar (*13*).

Regardless of the dietary source (milk products, vegetation, meat, or water), strontium-90 will follow calcium in the body, but it is discriminated against in going from the intestines to the blood by a factor of 2 to 3 (*14, 15*). Studies on human beings who have been given intravenous tracer doses of strontium-85 and calcium-45 simultaneously show that strontium is also discriminated against in the process of bone deposition. This, together with the fact that the body preferentially excretes strontium, results in a progressive enrichment of the bone in the calcium isotope following a single administration of the two tracers, the experimentally determined factor being 2.0 at 1 month and gradually increasing (*16*). On constant dietary intake, it would appear that an equilibrium enrichment of about 3 would be obtained in going from blood to bone, so that the total discrimination against strontium in going from the food to bone is about 8.

Appreciable local variations in strontium-85 content per gram of calcium occur in individual bones after a single dose. The "hot spots" that appear on autoradiographs are probably of less consequence in the case of strontium-90 than they are in the case of radium, because the dimension of these localizations is usually much less than the range of the beta particle that is emitted from the strontium isotope. Table 1 shows that real differences in strontium-85 content per gram of calcium exist among the various bones of a particular skeleton. Although the data shown are for an individual who died 39 days after administration, the ratios proved to be relatively uniform for seven other cases ranging from 3 hours to 125 days.

From the unpublished data of Trotter on the weights of individual bones in the human skeleton and from information on the percentage of calcium in the bones and the distribution of strontium-85, it is possible to calculate the total skeletal load of strontium-90 from any given bone. The bone most frequently obtained at autopsy is the rib. It may be noted from Table 1 that the concentration of strontium-85 per gram of calcium in the rib is twice that of the average body. Other bones frequently received in the world-wide survey are femur and vertebrae, which contain 0.72 and 4.2 times the average strontium-85 concentration of the whole skeleton, respectively.

These are the primary concepts and data that must be used in interpreting the world-wide human assay.

Table 1. Relative size of bone and distribution of strontium-85 in human skeleton.

Bone	Percentage of skeleton (dry wt)*	Percentage of calcium of skeleton	Percentage of Sr^{85} per gram of calcium†
Long bones‡	52.5	57.9	0.0187
Femur	18.7	20.6	0.0219
Humerus	6.9	7.6	0.0171
Radius	2.3	2.5	0.0170
Skull and mandible	17.9	19.7	0.0094 (skull)
Rib	3.7	6.3	0.0618
Vertebrae	6.6	7.1	0.128
Sternum	0.3	0.2	0.136
Weighted average			0.0303

* Data kindly supplied by Mildred Trotter, Washington University School of Medicine.
† Data from Schulert, Lazzio *et al.*, giving concentrations 39 days after administration of isotope. Other data taken from 3 hours to 125 days after administration of isotope show similar relative distribution of strontium-85.
‡ All the limb bones plus the pelvis. In averaging, it was assumed that the three analyzed are representative of the total.

Sampling

Autopsy samples of human bones were obtained from 17 stations in a world-wide network (*17*) (Fig. 1). To date, more than 1500 samples have been received, and about 600 analyses have been made; the bulk of the samples have come from about ten stations. The size of sample ranged from 1 to 200 grams of wet bone. An attempt was made to get as wide a geographic and dietary distribution as possible, but the distribution was necessarily limited by our contacts with physicians in certain centers. Future sampling will utilize a wider network, and considerable use will be made of integrated samples.

The bones employed were usually ribs, but those from Germany were femur shafts, and those from Switzerland, England, and Denmark were vertebrae. In all cases the entire rib, shaft, or vertebrae section was ashed and analyzed to avoid local variations that do appear both laterally and vertically in the bone.

The early results suggested that there was negligible strontium-90 in persons over 40 years of age; hence, sampling was concentrated in the younger age groups. It is clear that this is no longer true and that a broader spectrum is now desirable.

A few samples of adult bone measured at Lamont and many stillborns analyzed at Chicago (*4*) date as early as 1953, but for most localities the samples were procured in 1955, so that a clear definition of the rate of change of strontium-90 concentration cannot yet be made.

Analysis

The radiochemical procedure (*18*) consists briefly of ashing the bone, dissolving in hydrochloric acid, precipita-

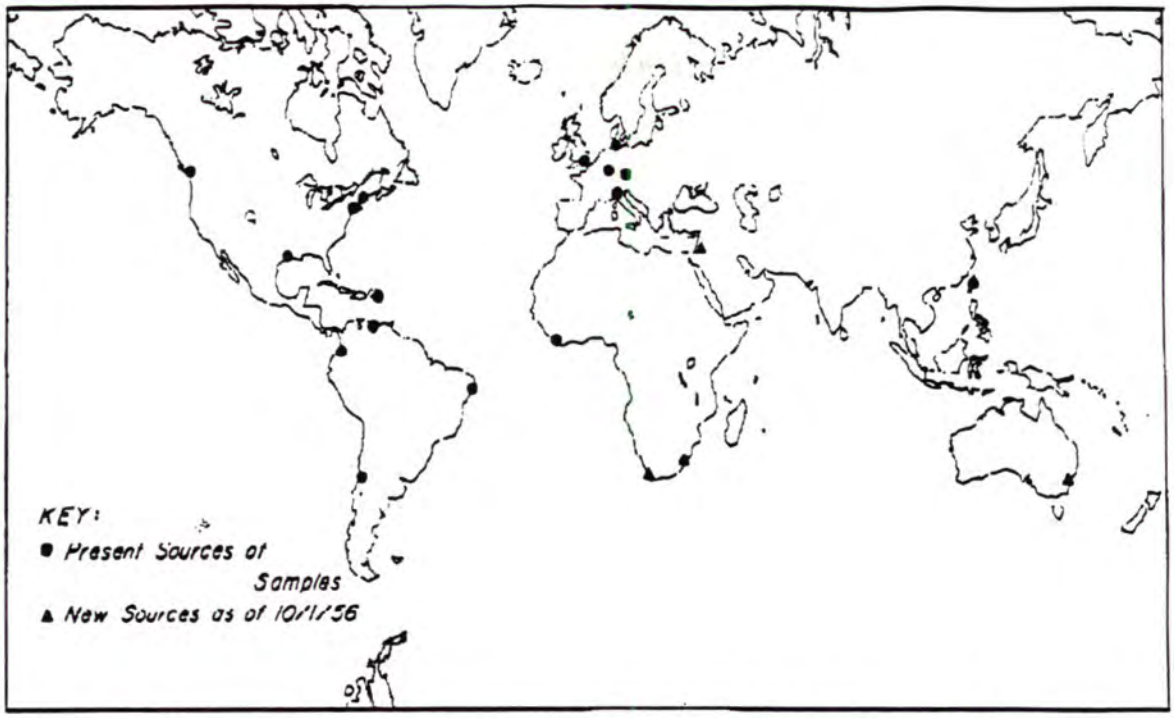

Fig. 1. World-wide network for collection of human bone samples. The triangles indicate new stations for which data are not yet available.

ting calcium oxalate, igniting to calcium oxide, resolving in hydrochloric acid, adding nonradioactive yttrium carrier, and milking the yttrium-90 daughter of strontium-90 as the oxalate. Usually the first milking brings down some foreign activities so that a second one is required for the quantitative assay of yttrium-90. Purity is checked by monitoring the decay of the yttrium-90 precipitate. The yttrium oxalate precipitate is counted in a convenient low-level system that has been described elsewhere (*19*). This procedure makes it possible to determine less than 1 disintegration per minute of the strontium-90 sample. At the level of 10 disintegrations per minute, per sample, the precision is a few percent. Even at low levels of activity, the variation in the individual samples is considerably larger than the experimental error.

The data reported in this paper were obtained at the Lamont Observatory and at two commercial laboratories: Isotopes Incorporated, Westwood, New Jersey; and Nuclear Science and Engineering Corporation, Pittsburgh, Pennsylvania.

Results

All of the analyses of strontium-90 in human bone reported in this study are summarized by locality in Fig. 2. The results are given in micromicrocuries of strontium-90 per gram of calcium. The dashes mean that the sample contained equal to, or less than, this amount. The error in a determination is generally less than 20 percent, but for a few of the very small samples or those with only a few hundredths of a micromicrocuric of strontium-90 per gram of calcium, it may be considerably higher.

Table 2 shows the averages for all localities broken down into age groups. All values in this table are normalized to an average skeleton, using the aforementioned factors. The weighted averages for each age group were calculated for each continent and for the world. Finally, a total maximum world average of the concentration of strontium-90 in the human skeleton was obtained. The average deviation for a given age group and locality was commonly around 50 percent. All samples whose analyses were reported as "equal to or less than x" have been assumed to contain x micromicrocuries of strontium-90 per gram of calcium for the purposes of this averaging. Thus Table 2 actually represents the maximum average strontium-90 content. For most localities, however, this makes little difference, because the number of such analyses was small or the analyses were in the range of low concentration. In the case of Chile and Brazil, however, the actual average concentration is probably about 25 percent lower than this maximum value.

One sample with very high concentration of strontium-90 was not included in the average. This sample was from Vancouver, British Columbia (49 years; tibia), and it had a concentration of 6.6 ± 0.3 micromicrocuries of strontium-90 per gram of calcium. This would give a skeletal average of about 9.1 micromicrocuries of strontium-90 per gram of calcium. Analytic error owing to contamination appears unlikely, since other samples that were processed at the same time showed very low activity.

Discussion

From the analytic data shown in Fig. 2 and Table 2, the following tentative conclusions may be drawn.

1) The present world-wide average content of strontium-90 in man is about 0.12 micromicrocuries per gram of calcium, or 1/10,000 of the presently accepted maximum permissible concentration.

2) The averages for the different continents are surprisingly similar, indicating that already the stratospheric drip of strontium-90 from megaton explosions is swamping the local concentrations from both the Nevada and the Soviet test sites. There is evidence, however, that Chile and Brazil have clearly lower concentrations than those localities in the Northern Hemisphere for which a large number of samples are available. The close similarity between Houston, Texas, and Bonn, Germany, for which good sampling is available, emphasizes that

the differences as a function of longitude in the Northern Hemisphere are small. Since Taiwan has appreciable fallout from Pacific and Soviet tests (*1*), it is not surprising that its values are similar to those for North America.

3) There is clearly an age effect, at least in the first 20 years. Young children have 3 to 4 times more strontium-90 per gram of calcium, on the average, than adults. This effect reflects the larger proportion of active bone in children.

4) As was expected, the average strontium-90 content of human bone does not vary from one locality to another more than the average concentration of mixed fission products (*1*). For identical periods of time there is a fair correlation between these two factors for the 17 localities that were sampled for human bone.

5) By averaging all samples from persons above 10 years of age, a large enough set is available for comparison between localities. In North America, for exam-

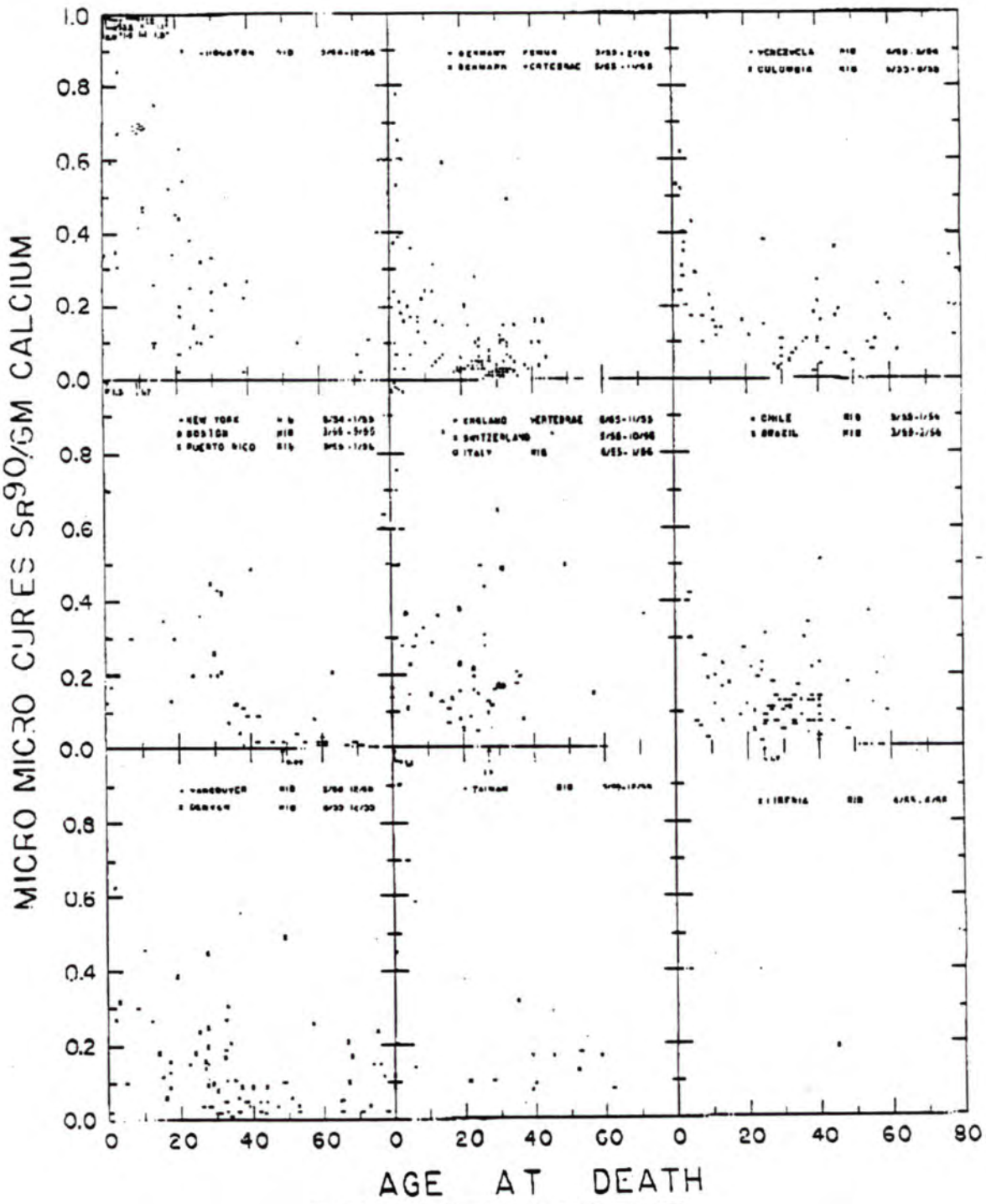

Fig. 2. Strontium-90 content of human bone in 1955.

ple, it is readily seen that the concentration in Vancouver is essentially the same as that in Houston, whereas the concentration in Denver is definitely lower (despite its proximity to Nevada). The New York average is not strictly comparable; it is low because samples from individuals 40 to 60 years of age comprised half of the total. These are the only ones in the table that were obtained in the spring and fall of 1954. At that time the strontium-90 content of adult bone was distinctly lower than it was in late 1955. (Note that the concentration in New York City milk had increased at least by a factor of 2 toward the end of 1955 from the level in 1954 (*4*).

6) There are large deviations from the mean in the strontium-90 content in individuals of a given locality. The average deviation for most 10-year-age sets is about 50 percent. Figure 3, a log normal plot of the North American samples, illustrates that some individuals may have at least 10 times the average concentration. This is most probably related to diet.

An attempt was made to find the time-dependence of the strontium-90 contamination in human bone. The major difficulty is the limited time interval over which samples have been taken (mainly 1955). The New York samples of the spring and summer of 1954 in the older adults (40 to 60 years) were definitely lower (the average was less than 0.01 micromicrocurie of strontium-90 per gram of calcium) than in areas of similar total fallout in 1955—for example, Puerto Rico and Denver (averages were 0.14 and 0.08 micromicrocurie of strontium-90 per gram of calcium, respectively). Here an increment of 0.1 micromicrocurie of strontium-90 per gram of calcium, per year is evident. It now appears that stillborns may have a higher concentration than the average of the mother skeleton, at least during this early period of nonequilibrium. The Chicago data (*4*) suggest a 1953 stillborn average of about 0.12 micromicrocurie of strontium-90 per gram of calcium; this climbed to 0.17 by the spring of 1954. Six samples from Chile gave about 0.35 in mid-1955. The values for the comparable mother skeletons would have been 0.03 micromicrocurie per gram in Chicago in 1954 and 0.07 micromicrocurie per gram in Chile in 1955.

The samples from Germany were of sufficient size and number to make possible a significant comparison between the periods March to September 1955 and October 1955 to January 1956. The biggest difference was observed in the youngest age group, whose members would be the most sensitive to change in the strontium-90 concentration of the diet, because of their rapid growth. For the 0 to 9 age group, the averages were 0.21 and 0.34 micromicrocurie of strontium-90 per gram of calcium for March to September 1955 and for October 1955 to January 1956, respectively. A major increment in the known bone data should be observed in the present winter 1956–57 collection.

The average American obtains about 80 percent of his calcium from dairy products and the remainder mainly from vegetation (*20*). In 1955 the strontium-90 concentration in milk in the United States was about 3.5 micromicrocuries per gram of calcium (*4, 5*). During the same period, field vegetation averaged about 20 micrimicrocuries per gram of calcium (*5, 8*) so that the *average* human population in the United States probably had a diet of about 7 micromicrocuries of strontium-90 per gram of calcium. Using the discrimination factor of 8 between diet and human bones, an equilibrium concentration of

Table 2. Average strontium-90 content in man 1955. (All values in micromicrocuries of strontium-90 per gram of calcium, normalized to the average whole skeleton. The numbers in parentheses give the total number of samples in the category.) The world average for all ages and locations is 0.12 micromicrocuries per gram of calcium; the average, including one sample of high value, is 0.14; and the world average for all samples assuming "equal to or less than" values are zero is 0.10.

Location and sample	Age at death								Average 10–80
	0–4	5–9	10–19	20–29	30–39	40–49	50–59	60 →	
				Europe					
Germany (femur)	0.44(12)	0.25(6)	0.14(15)	0.065(33)	0.065(20)	0.11(2)			0.085(77)
Switzerland (vertebrae)	0.13(3)		0.088(6)	0.076(3)	0.088(2)	0.12(1)	0.096(1)		0.076(13)
England (vertebrae)	0.19(3)	0.060(4)	0.048(2)	0.026(9)					0.032(11)
Denmark (vertebrae)	0.044(1)	0.052(2)	0.14(1)	0.029(4)	0.024(3)	0.029(4)			0.036(12)
Italy (rib)	0.18(1)	0.14(1)	0.10(4)	0.10(6)	0.11(7)				0.11(17)
Average	0.33(20)	0.15(13)	0.11(26)	0.06(55)	0.085(41)	0.08(7)	0.04(1)		0.078(130)
				North America					
Texas (rib)	0.49(15)		0.22(9)	0.14(17)	0.12(8)	0.13(1)	0.05(1)		0.15(36)
Denver (rib)	0.12(5)	0.085(5)	0.09(11)	0.075(9)	0.09(4)	0.13(1)		0.07(7)	0.085(32)
New York City (rib)	0.44(2)	0.16(3)	0.17(2)	0.10(2)	0.075(8)	0.005(4)	0.01(4)		0.06(20)
Boston (rib)	0.30(2)								
Vancouver (tibia)	0.46(2)		0.28(3)	0.10(4)	0.066(11)	0.06(3)	0.06(3)	0.092(7)	0.10(35)
Puerto Rico (rib)	0.06(1)		0.065(1)	0.21(2)	0.13(3)	0.15(2)	0.04(1)	0.16(6)	0.14(15)
Average	0.41(25)	0.12(8)	0.17(28)	0.12(34)	0.09(34)	0.07(13)	0.02(9)	0.10(20)	0.11(150)
				South America					
Colombia (rib)				0.016(1)	0.04(5)	0.04(1)			0.035(7)
Chile (rib)	0.13(1)	0.06(5)	0.075(7)	0.07(16)	0.06(22)	0.05(10)	0.10(4)		0.065(59)
Brazil (rib)	0.21(1)		0.14(1)	0.035(1)	0.035(1)	0.03(3)			0.06(12)
Venezuela (rib)	0.19(10)	0.11(4)	0.08(4)	0.085(4)	0.04(6)	0.085(10)	0.065(6)	0.105(6)	0.075(39)
Average	0.19(12)	0.085(9)	0.08(15)	0.07(27)	0.055(34)	0.065(24)	0.08(10)	0.105(6)	0.065(117)
				Africa					
Liberia (rib)				0.83(1)		0.093(1)			
				Asia					
Taiwan (rib)	0.34(3)	0.16(3)	0.25(1)	0.55(2)	0.08(7)	0.055(3)	0.08(3)		0.12(16)
				World-wide					
Average	0.31(62)	0.14(33)	0.12(70)	0.09(118)	0.08(106)	0.07(47)	0.06(22)	0.09(26)	

about 0.9 micromicrocurie of strontium-90 per gram of calcium would be predicted for the average American. The actual concentration in 1955 was probably about 0.3 for young children and 0.1 for adults. These lower values reflect the time required for bones to equilibrate with the strontium-90 calcium ratio in the diet.

An estimate of the world-wide burden can be made by assuming that the average total strontium-90 fallout was 8 millicuries per square mile in 1955, and that the average amount of exchangeable calcium is 75 grams per cubic foot (*4*). Thus, if half of the fallout is in the upper 1 inch of soil, which contains 6 grams of calcium per square foot, an average concentration of about 25 micromicrocuries of strontium-90 per gram of calcium would be available to grains and grasses. Using a soil/plant fractionation of 1.4 and a plant/milk fractionation of 7, the average world-wide concentration of strontium-90 in milk would be about 25 micromicrocuries per gram of calcium, and in vegetation about 18 micromicrocuries per gram of calcium. Assuming that 80 percent of the world-wide dietary calcium comes from milk products—as is true for the average American diet (*20*)—the predicted concentration of strontium-90 in the diet would be about 5 micromicrocuries per gram of calcium. This in turn would yield a predicted value of 0.8 micromicrocurie per gram of calcium for the average man when equilibrated with the fallout that existed at the end of 1955. The major uncertainties in the calculation are the sources of calcium in the average world diet, the average calcium content of the soil, root depth, and possible direct foliar uptake.

In terms of hazard to man, there are two problems to be considered: (i) the average value for the world population and (ii) possible maximal concentrations. Locations near atomic test sites are not included in these considerations.

With regard to the strontium-90 burden of the population of the world in the fall of 1955, it can now be said that this is reasonably well known (0.12 micromicrocurie per gram of calcium) and that this burden is very small compared with the maximum permissible concentration (1/10,000).

The matter of predicting the maximum average human burden that will ultimately be occasioned from atomic explosions through the fall of 1956 involves several factors. The average burden will be determined by the average strontium-90/calcium ratio in the diet at equilibrium. Thus, if the fall 1955 concentrations in the diet were maintained, the average human being at *all* ages would reach a maximum of about 0.6 micromicrocurie of strontium-90 per gram of

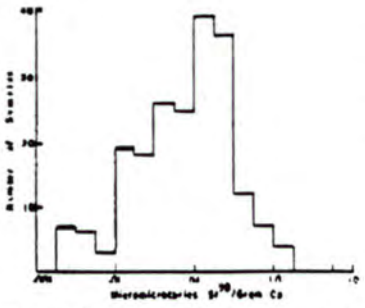

Fig. 3. Distribution pattern of strontium-90 in human bone in North America.

calcium. This would be the result of a world-wide fallout of about 8 millicuries per square mile by the end of 1955 (*1*). From the end of 1955 to the fall of 1956, another 2 millicuries per square mile would appear world-wide from stratospheric fallout. An additional 8 millicuries per square mile has fallen out since 1955 from high-yield weapons that have deposited all of their debris in the Northern Hemisphere (*5*). Libby (*3*) states: "In fact, we estimate at the present time that the total stratospheric reservoir, counting all sources, is about the same as it was 2 years ago—that is, 12 millicuries of strontium-90 per square mile, or the equivalent of 24 megatons of fission products calculated as a uniform world-wide distribution." Taking decay into account, a total quantity of 26 millicuries per square mile would be available in the United States for equilibration with the human skeleton by 1970. Thus, from explosions that have already occurred, the average human bone in the United States should contain about 2 micromicrocuries of strontium-90 per gram of calcium by 1970, whereas the world wide average concentration should be about 1.3. This will have been the result of about 50 megatons of fission. On this basis, 35,000 megatons of fission would be required to bring the average concentration in the world's population up to the maximum permissible concentration.

The most important problem lies with individual variation. By direct experiment, it has been found that the distribution curve is quite sharp (Fig. 3 and Table 2). Although food grown in restricted areas of low available calcium content could have 10 to 100 times the mean, it is clear that the general mixing of food sources in the diet of an urban population would make it impossible for most people of the world to exceed the average concentration of strontium-90 by more than a factor of 10.

The theoretical estimation of the maximum concentration of strontium-90 that some individual or a small group of individuals might receive at long distances from atomic test sites is a complex problem involving a number of parameters that are at present subject to large uncertainties. The highest concentrations will be found in isolated individuals who obtain their total food supply from a restricted area that has very low available calcium in the soil (*21*).

Summary

The world-wide average strontium-90 content of man was about 0.12 micromicrocurie per gram of calcium (1/10,000 of the maximum permissible concentration) in the fall of 1955. A few values as high as 10 times the average have been obtained.

This value is in accord with the predicted value based on fallout measurements and fractionation through the soil-plant-milk-human chain.

With the present burden of strontium-90, this average level should rise to 1 to 2 micromicrocuries of strontium-90 per gram of calcium by 1970.

References and Notes

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5. ———, "Current research findings on radioactive fallout, address to AAAS, Washington, D.C. (1956).
6. This article is Lamont Geological Observatory contribution No. 231. This research was supported by the Division of Biology and Medicine of the U.S. Atomic Energy Commission. We wish to acknowledge the very substantial contribution of many medical scientists who cooperated so willingly in procuring the autopsy material.
7. This research was initiated at the suggestion of W. F. Libby. The encouragement, support and criticism of W. D. Claus, P. Waterna, R. A. Dudley, and D. L. Worf of the Division of Biology and Medicine and M. Eisenbud, J. Harley, and L. Whitney of the New York Operations Office are much appreciated. We also wish to acknowledge the scientific contributions of H. L. Volchok of Isotopes Inc. and W. S. Broecker and K. K. Turekian of this laboratory. Laboratory assistance was provided at various times by J. E. Geerjon, E. Hitchcock, E. Hodges, P. Kluft, A. Long, R. Lupton, R. Janes, E. Peers and R. Slatter.
8. J. L. Kulp *et al.*, Project Sunshine, Annual Rept., 1 Apr. 1956.
9. Conference at the Rand Corporation, Santa Monica, Calif.
10. *Natl. Bur. Standards Handbook 52* (20 Mar. 1953).
11. F. A. Martell and W. F. Libby, Project Sunshine Bull. No. 11, Enrico Fermi Institute for Nuclear Studies, University of Chicago (1 Nov. 1955).
12. R. Mensel, personal communication. Early experiments had suggested a discrimination factor as high as 2.8 for the Sr/Ca fractionation ratio in going from soil to plant. This was used by Libby for computation (3). It now seems likely that the correct value is close to 1.4 for several reasons. (i) The early experiments involved some uncertainty either in the quantity of tracer or the degree of equilibration. (ii) New experiments by Mensel on four widely different soil types using both radioactive and common isotopes gave a factor of 1.4. (iii) The majority of the Sr^{90} has now

been in the soil for long periods compared with the laboratory experiments; hence near equilibration appears probable. (iv) Other workers have recently reported experiments that gave about 1.4 [K. Larson, *UCLA Rept. No. 380* (6 Nov. 1956); H. L. Bowen and J. A. Dimond, *J. Rapid. Botany* 7, 264 (1956)].

13. G. L. Comar and R. H. Wasserman, Agricultural Research Program, Semi-Annual Progress Report for 1 July–31 Dec. 1953, A.E.C. Rept. ORO-110 (April 1954).
14. G. E. Harrison, W. H. A. Raymond, H. C. Tretheway, *Clin. Sci.* 14, 681 (1955).
15. D. Laszlo, private communication.
16. A. R. Schulert *et al.*, in preparation.
17. The following physicians kindly supplied bone samples for this study: C. Brown, J. de Bruz, W. Civin, E. Diego, M. Foo, L. Callindo, F. Haeberlin, H. Hamperl, E. Koppisch, W. Leach, J. Legendre, J. Lowry, J. McNaught, D. Melamoro, H. Meneses, J. Montalvan, L. Pessana, D. Rosenberg, A. Stewart, G. Teilum, C. Treip, E. Uehlinger, G. Volante, S. Warren, Shu Yeh.
18. M. L. Vaichok *et al.*, in preparation.
19. H. L. Vaichok and J. L. Kulp, *Nucleonics* 13, No. 8, 48 (1955).
20. *Agricultural Statistics*, U.S. Dept. of Agriculture, 1956 annual report, Government Printing Office, Washington, D.C.)
21. A detailed discussion of the problem of the probable maximum concentration is in preparation.

Science, Ethics, and Politics

Albert Szent-Györgyi

We are often told that science has no moral content. Certainly, if I measure the respiration of a tissue, I have little to do with morals or ethics, but on the same ground one could deny a religious content to the Holy Communion, drinking wine and eating bread not being, in themselves, religious acts. If there should be a Creator, then scientific research would be tantamount to worship, there being no greater compliment to a creative artist than an effort to understand his work.

The scientist is searching for truth, for truth's sake, and, if it is found, he processes it without fear of consequences. This demands the highest ethical standards and brings him into line with the religious and moral leaders of mankind. What the scientist really wants to know are the internal laws that hold the universe together with all that is in it. Morals are the laws that hold human societies together. So science is not devoid of relations to ethics and morals.

Moral Law

Morals are practical prescriptions that tell us how to live to be able to live together. The moral outlook of a scientist has to be wider than that of the average, simply because his society is wider, not being limited by time or space. The community in which I live has Galileo, Newton, and Lavoisier as its active members, and I cannot help feeling more affinity to Chinese or Indian scientists than I do to my own milkman.

The author is director of the Institute for Muscle Research at the Marine Biological Laboratory, Woods Hole, Mass.

As to politics, up till lately, there was no need for the scientist to take cognizance of its existence. However, lately, politics has penetrated not only into science but also into the private lives of individuals, forcing the scientist, too, to make a stand. That science, in certain countries, is dictated by political dictators is so crude a matter that it demands no discussion.

A subtler question may be asked about the aims of science. The main driving force of researchers is mostly some sort of a curiosity, the gratification of a mental need, which makes research a selfish occupation. However, from a higher point of view, scientific research is one of the human efforts aimed at elevating man. Within the last decade, science has created the most powerful tools, which, like any tools, can be used for construction or destruction. The scientist cannot remain a neutral spectator and refuse all moral responsibility when he sees the politician run away with these and turn them into tools of destruction.

We all have the bad luck to be born in an age of a moral crisis, and, according to Dante, the hottest places in Hell are reserved for those who remained neutral at times of a moral crisis. So we all have to take a stand, simply as human beings. Humanity has its well-established moral code on which human relations are based. It is these moral laws that enable man to live in a society, and the problem is whether these morals apply only to the individual or also to groups of men, whether crimes which are punished by death in one country should be suffered to be practiced on a big scale as a routine by governments in another country, being "internal affairs." This is more than an ethical problem. As a society could not exist without a moral convention among its members, so countries cannot exist, side by side in peace, without a moral code. I am deeply convinced that this is the simple root of all our political troubles, the whole political superstructure being but a "pseudo-problem."

One Moral Code

I also believe that there cannot be two moral codes, an individual one and a political one. There is but one, and this one is very deeply written into our minds by our education. It is so deeply engraved that we see no need to restate it every time we make any agreement, and we tacitly suppose all written agreements to be based on this unwritten moral code. For instance, if we make an alliance, we see no need to state explicitly that it is made so that we may help one another and not to enable us to stab our ally in the back, as we were advised to do by Lenin.

It is natural that any system can achieve a great temporary advantage by rejecting the moral code that is written so deeply in its adversary's mind that he will believe and fall, over and over again. How often the world believed, *ad nauseam*, when Hitler called every demand his last one. The Hungarians fell for the famous "salami technique" (one slice at a time). We saw just the other day, how, with a boring repetition, all leaders of a revolution could be trapped and marched to jail by an invitation to a "discussion."

There is but one moral code, and, if any government rejects it inside its borders, it will reject it in its international relations as well and create disorder. The question is whether any deviation from moral convention should be suffered by the rest of mankind. There are international laws to control pestilence, for fear that that pestilence may spread across borders. Why not the same for moral pestilence?

Political Questions and the Individual

For most of my colleagues, these questions may seem so crude, and the an-

ATTACHMENT 3

**June 10, 1957 letter from W.F. Libby, Commissioner,
AEC to Dr. Herman M. Kalckar, NIH,
in response to a proposal from NIH.**

AN ATOMS FOR PEACE PROPOSAL

An International Milk Teeth Radiation Census

The formulation of this proposal assumes that we all have a common goal, atoms for peace. It is not meant here to express any opinions concerning the discussion of the test program of today. The present proposal is rather meant as a contribution towards education in the program for prevention of atomic war. There is hardly any doubt that any family, irregardless of whether it belongs to a nation which is far ahead in atomic weapon development or not, would lend deep sympathy toward an educational and scientific program on atomic radiation which is concerned with the health of their own children. From this reasoning the following proposal is made:

I. It is generally a recognized fact that minor children have a more intense uptake of radioactive strontium and ce ium than adolescents and adults and that they also have higher radiosensitivity irregardless of whether it stems from radioactive

strontium, cesium or iodine, or from X-rays. Hence, radiation hazards must be considered much higher during early childhood. These facts, plus the great need for obtaining precise knowledge about the biological factors involved, should be stressed in explaining the problems to the general public in order to encourage them to active participitation (see next section).

II. The Public Health Service in every nation and especially in the large nations such as U. S., U.S.S.R. and China should organize, perhaps on a county-wide basis, a colossal collection of milk teeth (with dates of appearance and of shedding, and child's age) and do radioactivity measurements on this material. Thousands of countings performed on a decentralized basis, i.e., on a small community basis, might be particularly effective because the public would feel that they participated actively in the program. A possible occurrence of rise in radioactivity in childrens' tooth structure would soon be detected. The results should be

conveyed to the public without unnecessary interpretations which might either raise complacency or fear, but rather in a spirit so as to encourage a sober, active concern. Any family, be it in America or in Russia, who see for themselves how the teeth of their younger children grew more radioactive would soon, out of concern for the fact that their own children carry the main burden, develop a realistic restraint on the question of 'atomic might'. Moreover, they would have a natural incentive for taking an active constructive part in the constant search of our society for affecting atomic disarmament.

III. An 'International Milk Teeth Radiation Census' might also contribute greatly toward collecting important data for the most sensitive and most precious part of any population, the children. At present only rather erratic data exists derived from autopsy bone samples stemming mainly from adults.

IV. Measures to keep radioactivity of teeth and bone structure down should be initiated with full public account and motivation and honest emphasis of the limitations of such measures. Measures for protection would presumably follow the pattern of isotope dilution, i.e., addition of rock calcium to the diet and to the soil. In connection with the diet, it would be of interest to compare the radioactivity of milk teeth of children on soybean milk with those on cow's milk. The former might be less radioactive because the added calcium stems from rock.

V. It would be most effective if the 'International Milk Teeth Radiation Census' could be carried out fairly simultaneously in various nations, especially in the big ones. United Nations might be the right agency to handle such a project. Yet, it would be immensely impressive if the United States took the initiative and, for instance, made the bold suggestion that all the large nations of Asia, including China, be participants. This country, who has developed the anti-coincidence counter and other ingenious

tools for the peaceful use of atomic energy, might even offer technical help or advice. Such a concrete token of respect and reverence for life, irregardless of creed, color or political system might well create a landslide of confidence all over the world towards this country.

The other possibility is to let the small Scandinavian countries who do not have colonial disputes nor conflicts with the big powers become the main spokesmen for the international handling of such a program. However, the United States should in any event take the first step.

Date: 5/10/57

OFFICE OF COMMISSIONER W. F. LIBBY

To: Dr. C. L. Dunham

Dr. Libby would like your approval of the attached letter before it is dispatched.

Mary Sweeney

Office of Commissioner Libby

GPO 902860

June 10, 1957

Dr. Herman M. Kalckar
National Institutes of Health
Department of Health, Education,
and Welfare
Bethesda 14, Maryland

Dear Dr. Kalckar:

I think your idea of using children's milk teeth for strontium-90 measurement is a good one. However, I would not encourage publicity in connection with the program. We have found that in collecting human samples publicity is not particularly helpful.

We could get the teeth going by having investigators make their own collections. The samples need not be too large. Dentists would help.

I will pass your idea along and thanks for sending it in.

Sincerely yours,

W. F. Libby
Commissioner

cc: Dr. C. L. Dunham
Director of Biology & Medicine, AEC

MEDICINE, HEALTH & SAFETY 3-6

Dear Dr. Libby,

This is just a brief formulation of a proposal which I have emphasized along educational and international lines. I am trying to bypass the controversy on our present day atomic program and bring up another type of approach which might contribute to the present discussion. I am sure that somebody else can formulate such a proposal much better. It is, so far, only meant as a personal communication between a limited number of scientific colleagues.

My goal is to improve the formulation in such a way that it is free of any biased opinions. I would, of course, be greatly thankful for advice in this trick.

Your views will of course be of special value in this matter. I have also approached Dr. Will Armstrong at U. of Minn.

Kind regards

Sincerely yours

Hans M. Kalckar

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D-3 1966 Proposed Plutonium
And Prometheum Experiments

Divider Title: _____

TAB D-3

✦✦✦DRAFT ✦ FOR DISCUSSION PURPOSES ONLY✦✦✦

MEMORANDUM

TO: **Members of the Advisory Committee on Human Radiation Experiments**

FROM: **Advisory Committee Staff**

DATE: **June 9, 1995**

RE: **Documentary Update: 1966 Proposed Plutonium and Prometheum Experiments**

Documents recently retrieved by Committee staff describe a 1966 program for human experimentation with plutonium and prometheum at the Atomic Energy Commission (AEC) Hanford site. With the assistance of the Department of Energy (DOE), we are seeking further information on the extent to which the experiments went ahead as proposed.

Attachment 1, a March 11, 1966 letter from the Hanford Occupational Health Foundation (an AEC contractor) to the AEC's Richland Operations Office describes a proposal by Battelle Northwest (another AEC contractor) regarding:

> . . . use of paid volunteers, and possible employees, who will receive controlled doses of Pm144 and Pu 237. Estimated doses will be in the range of 0.1 to 0.15 roentgens.

The Hanford Occupational Foundation wished to participate because the data could be used in "treatment of radiation and contamination victims as well basic research." The Foundation was writing to the AEC for information on the legal implications of participation.

Attachment 2, a March 29, 1966 memorandum, appears to be a legal memorandum from the Chief Counsel of the AEC's Richland Operations Office, which states the terms on which the experiments can proceed.

Attachment 3, a March 31, 1966 memorandum from the Assistant Manager for Administration of the Richland Office to the Director the Division of Biology and Medicine (DBM), requests DBM's guidance on the proposed research.

Attachment 4, a May 13, 1966 memorandum from the AEC Associate General Counsel to the Director of the DBM provides the terms for written consent.

LIST OF ATTACHMENTS

ATTACHMENT 1:	**March 11, 1966 letter from the Hanford Occupational Health Foundation (an AEC contractor) to the AEC's Richland Operations Office**
ATTACHMENT 2:	**March 29, 1966 legal memorandum from the Chief Counsel of the AEC's Richland Operations Office**
ATTACHMENT 3:	**March 31, 1966 memorandum from the Assistant Manager for Administration of the Richland Operations Office to the Director of the Division of Biology and Medicine**
ATTACHMENT 4:	**May 13, 1966 memorandum from the AEC Associate General Counsel to the Director of the DBM**

ATTACHMENT 1

March 11, 1966 letter from the Hanford Occupational Health Foundation (an AEC contractor) to the AEC's Richland Operations Office

HANFORD
OCCUPATIONAL HEALTH
FOUNDATION

MEDICAL-DENTAL BLDG. SWIFT AND GOETHALS RICHLAND WASH. 99352 TELEPHONE AREA CODE 509, 942-1111

March 11, 1966

U. S. Atomic Energy Commission
Richland Operations Office
Richland, Washington

Attention: A. M. Waggoner, Assistant Manager
for Administration

Subject: PROPOSED PROMETHIUM AND PLUTONIUM STUDIES

Gentlemen:

As part of the Whole Body Counting - Metabolic Studies of Radioisotopes in Humans studies, Battelle Northwest personnel propose to perform ingestion, injection, and inhalation studies involving Promethium ^{144} and Plutonium ^{237}. This program will involve the use of paid volunteers, and possibly employees, who will receive controlled doses of Pm ^{144} and Pu ^{237}. Estimated doses will be in the range of 0.1 to 0.15 roentgens. Dr. Dave Bruner of the AEC Division of Biology and Medicine concurs in our belief that such studies may prove extremely useful.

Since these experiments will provide valuable data for use in possible treatment of radiation and contamination victims as well as basic research information, the Hanford Occupational Health Foundation is quite anxious to participate in the program.

Participation in such a program, however, raises several legal questions. Article XVI - Litigation and Claims, of Contract AT(45-1)-1837 provides for reimbursement to the Foundation of costs incurred in the settlement or defense of a claim if not otherwise covered by insurance. The Foundation liability insurance does not provide protection against a claim which might arise as a result of participation in the subject program.

Since this program is a part of an approved Commission program, would the Foundation be protected under the provisions of Article XVI, insofar as reimbursement of costs is concerned?

HANFORD OCCUPATIONAL HEALTH FOUNDATION

RICHLAND, WASHINGTON

The Foundation would be involved in the examination of the volunteers, administration of the radioisotopes, interpretation of results, and payment of the volunteers. It is felt that a document executed by the volunteers, possibly an authorization or consent form, would be desirable and appropriate. Guidance as to content and format of such a document from the Commission Legal offices is requested.

A third problem involves licensing. It is our understanding that AEC approved programs involving the use of radioisotopes which are conducted in Commission owned facilities do not require a license. It is possible that some of the work will be performed in Kadlec Hospital. If so, would this require a license? Would a license be required for work in our Richland headquarters in the Kadlec Medical Dental Building?

Your reply to the above questions and request at the earliest convenient time will be appreciated. If you have any questions or desire additional information, please feel free to contact me.

Very truly yours,

W. D. Norwood, M.D.

W. D. Norwood, M.D.
Medical Director

WDN:JG

ATTACHMENT 2

March 29, 1966 legal memorandum from the Chief Counsel
of the AEC's Richland Operations Office

John Reich, Asst. Gen. Counsel — March 29, 1966
Research and Development
Office of the General Counsel, HDQ
Chester G. Brinck
Chief Counsel
Richland Operations Office
USE OF HUMAN VOLUNTEERS IN BIOMEDICAL RESEARCH - CPFF CONTRACTS

GC:RHB

As the attached letters indicate, two Research and Development programs involving internal administration of by-product materials in human volunteers are contemplated. Our occupational health contractor, Hanford Occupational Health Foundation (HOHF), would support our R & D contractor, Battelle Memorial Institute/Pacific Northwest Laboratory (PNL). In the "Whole Body Counting" program the volunteers would be adult college students (each paid $15.00) or PNL employees, or both. Inmates of our State penitentiary would be the only volunteers in the second program.

Under the HOHF and PNL operating contracts - CPFF - the Commission would have a direct responsibility (as distinguished from the Washington-designated type involving the use of human volunteers, such as AT(45-1)-1781 with the University of Washington, where the contractor accepts sole responsibility for the work and the manner and consequences of its conduct); therefore, the matter has been referred to headquarters for guidance (See enclosure 1).

Some time ago we concluded and gave an "off-the-cuff" opinion that it would be proper for HOHF to undertake its portion of the "Whole Body Counting" in view of the dosage involved (0.1 to 0.15 roentgens) and upon advice that Idaho Falls has made similar studies using volunteer plant personnel. We would think that the use of a disclaimer would be reasonable protection against all claims but those grounded in willful and wanton negligence: In Bailey v. Safeway Stores, Inc., 55 Wn (2d) 728, 349 P (2d) 1077 (1960), the court, after equating the doctrine of assumption of risk and the maxim volenti non fit injuria, adopted the following language from Emerick v. Mayr, 39 Wn (2d) 23, 234 P (2d) 1079 (1951): "Under the doctrine of assumed or incurred risk, it is the rule that one who voluntarily exposes himself or his property to a known and appreciated danger due to the negligence of another may not recover for injuries sustained thereby. 65 C.J.S. 848, Negligence, Sec. 174. Walsh v. West Coast Coal Mines, 31 Wn (2d) 396, 197 P (2d) 233. In order to invoke the doctrine, it is essential that the plaintiff shall have exposed himself or his property voluntarily. The doctrine can apply only where a person may reasonably elect whether or not he shall expose himself to a particular danger. Also, it is essential that the risk of danger shall have been known to, and

3. The volunteers must understand that what is being done is an experiment and is not for treatment or diagnosis of their own conditions.

To avoid the problem of disaffirmance only volunteers over the age of 21 would be considered.

As to the type of volunteer (convict, student or employee) we have these few comments: We have researched the point and believe that the Washington felon has a right to contract and consequently would be bound by a disclaimer. Cost, convenience and control are policy factors in that the state penitentiary is about 65 miles from Richland. Workman's compensation problems might be created by the use of employees. Local adult college students could be a logical choice.

Attachments: 3
1. Ltr. dtd. 3/30/66 - Waggoner to Dunham
2. Ltr. dtd. 3/11/66 - Norwood to Waggoner
3. Ltr. dtd. 3/1/66 - Bruner to Robinson w/attach.

bcc: AM Waggoner, AMA
RD Wildman, R&D Div.
CN Zangar, H&S Div.
CG Brinck
RH Bennett
Subject File
Reading File

CHIEF COUNSEL CHIEF COUNSEL

Bennett:bmc Brinck

ATTACHMENT 3

March 31, 1966 memorandum from the Assistant Manager for Administration of the Richland Operations Office to the Director of the Division of Biology and Medicine

UNITED STATES GOVERNMENT

Memorandum

TO : Charles L. Dunham, M.D., Director
Division of Biology and Medicine
Headquarters

DATE: ~~MAR 8 1 1966~~

FROM : A. M. Waggoner, Assistant Manager
for Administration
Richland Operations Office

A. M. Waggoner

SUBJECT: USE OF HUMAN VOLUNTEERS IN BIOMEDICAL RESEARCH

RS:RDW

Attached is a copy of a letter we received from Dr. W. D. Norwood of the Hanford Occupational Health Foundation (HOHF), our occupational health prime contractor, regarding certain legal aspects of a proposed 06 research program entitled "Whole Body Counting." This program, which will be primarily the responsibility of the Pacific Northwest Laboratory (PNL), involves the use of human volunteers. We understand that PNL and HOHF now contemplate using local college students as paid volunteers and possibly PNL employees in this program.

In addition to this program, PNL is planning to initiate a study entitled "Excretion Rates vs. Lung Burdens in Man" in FY 1967. This project also involves the use of human volunteers; in this case PNL plans to use inmates at the Walla Walla State Penitentiary. Initiation of this program is consistent with Division of Biology and Medicine (DBM) FY-1967 Program Assumptions transmitted to us March 4, 1965. As you know, DBM is supporting research programs at the University of Washington under Dr. Paulsen (Contract AT(45-1)-1781) and at the Pacific Northwest Research Foundation under Dr. Heller (Contract AT(45-1)-1780), both of which involve the use of human volunteers at penal institutions. Also the DBM supported research project conducted jointly by Battelle's Radiological Physics and Dr. W. B. Nelp of the University of Washington on ^{99m}Tc utilized human subjects at the University's Medical School. Although these projects involve different types of volunteers, we believe that many of the legal questions are common to all of them.

Several questions are raised in the letter from Dr. Norwood which require clarification. On the question of licensing requirements it should be noted that HOHF would perform its work in privately owned off-site facilities. We have taken the position that the Contractor would be exempt from licensing, under authority of 10 CFR 30.12, for work performed in its own facilities (in the Kadlec Medical-Dental building) on the basis that the site is Government "controlled", i.e., we approve the annual lease and allow the lease payment as a proper item of cost under the HOHF prime contract. However, we request that you assist us by determining if HOHF's activities in Kadlec Hospital (no lease) would remove the Contractor from its exempt status.

MAR 21 1966

Our Office of Chief Counsel advises that the use of humans presents several legal problems, especially since we have a direct responsibility under the HOHF cost-type contract.

We request your guidance on the questions contained in Dr. Norwood's letter and a statement of DEM policy with regard to these research programs involving human volunteers. Our Counsel's office is contacting John Reich, Assistant General Counsel for Research and Development, on this matter. A copy of their memorandum to Reich is attached.

Enclosures:
1. Letter, 3/11/66
   Norwood/Waggoner
2. Memo, 3/30/66
   Brinck/Reich

cc: John Reich, Assistant General Counsel
for Research and Development, HQ

ATTACHMENT 4

May 13, 1966 memorandum from the AEC Associate General Counsel to the Director of the DBM

OPTIONAL FORM NO. 10
MAY 1962 EDITION
GSA GEN. REG. NO. 27

UNITED STATES GOVERNMENT

Memorandum

DATE: MAY 19 1966

TO : Dr. Charles L. Dunham, Director
Division of Biology and Medicine

FROM : Bertram H. Schur
Associate General Counsel

SUBJECT: USE OF HUMAN VOLUNTEERS IN BIOMEDICAL RESEARCH

This is in reference to a proposed memorandum to bring to the attention of the General Manager the contemplated new AEC biomedical research project involving the use of human volunteers. As we understand it, convicts, college students, and other individuals, twenty one years of age or older, will be invited to volunteer, and monetary consideration will be paid for their services. These individuals will ingest or be injected with promethium-147 or plutonium-237. This program will be conducted for AEC by the Hanford Occupational Health Foundation. You have asked for our views.

From the legal standpoint, there are two areas of potential risk which should be considered in the use of human volunteers in experiments:

1. Risk of adverse effects from the radiation experiments properly carried out; for example, the risk that the intended dosage of 0.1 roentgens might cause some pathological condition in certain individuals.

2. Risk of injury from negligent conduct in the course of the experiment; for example, the risk that an experimenter might be negligent in the operation of the radiation source and give dosage of 1.0 rather than 0.1 roentgens causing injury to the volunteer.

It is our opinion that each volunteer should sign a written, witnessed agreement in which he or she states that he or she is in sound mental and physical condition and is participating in the experiment on his or her own volition, that he or she understands that what is being done is for experimental purposes and not for the treatment or diagnosis of the individual, and that he or she understands the nature, procedures, and probable effects of the experiment. In addition, the agreement should set forth the nature and purpose of the experiment, the procedure to be used (including a description of the individual volunteer's participation or use in the experiment), the known possible risks, if any, and the arrangements, if necessary, for possible termination of the experiment or research (e.g. if the volunteer later desires to withdraw from the experiment). It would be advisable if the agreement also negated any inference that the paid volunteers are employees of the contractor for purposes of the experiment. The agreement should be signed only after the volunteer has had an

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opportunity to ask questions. Assuming complete understanding and no unequal bargaining factors (e.g. pressure on prisoners to submit), such an agreement would protect against liability for unauthorized invasion of the person.

The consent form may also contain language absolving the contractor and the AEC from responsibility for injuries to a volunteer resulting from the experiment. This would probably be binding on the volunteer if no negligence occurs. It is doubtful, however, that the clause can be so drawn as to be binding on the volunteer if negligence causes the injury. Not only is the law uncertain on this point, but we cannot be sure as to what law will apply. (Developments in the field of conflicts of law have been such that it is not clear whether the law of the State of Washington would be applied in every case; for example, a volunteer residing in some other state when his injury first becomes apparent might contend that that other state has the most significant contacts with the claim and that its law should be applied.)

Whether or not language absolving the contractor and the AEC from liability - either with or without mentioning negligence - should be incorporated in the consent form is a matter of policy. It may well be that the Commission would prefer not to attempt to add any special provisions designed to eliminate or minimize possible liability. In this regard, it should be borne in mind that the consent agreement itself, without specially added language, would probably be considered as tantamount to a disclaimer of liability for injuries resulting from the experiment properly carried out - that is, where no negligence occurs.

We understand that the contractor has no special view with respect to the obtainment of any special disclaimer agreements, and, also, that liability insurance is available at a reasonable premium; presumably, the contractor and RLOO are exploring the advisability of obtaining such insurance.

cc: Chester G. Brinck, RLOO
Dr. Wm. T. Doran, Jr., OS

DIVISION OF BIOLOGY AND MEDIC
INTER-OFFICE ROUTE SHEET

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	Comments		Contact Me
	Signature		File

From: MAIL ROOM

MAY 3 1 1966

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ASS'T DIR. FOR RADIOLOGICAL PHYSICS		
ASS'T DIR. FOR ADMINISTRATION		
SPECIAL ASSISTANT TO DIRECTOR		
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REMARKS:

① What research is contemplated?

② Every effort should be made to utilize AEC employees if such experiments have need for be done.

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D-4 Atomics Energy Comm.
Policy on Human Experimentation

Divider Title: _____

✦✦✦DRAFT ✦ FOR DISCUSSION PURPOSES ONLY✦✦✦

MEMORANDUM

TO: **Members of the Advisory Committee on Human Radiation Experiments**

FROM: **Advisory Committee Staff**

DATE: **June 8, 1995**

RE: **Documentary Update on Atomic Energy Commission (AEC) Policy on Human Experimentation**

Attached to this memorandum are three documents, two of which were sent by the Department of Energy's Argonne National Laboratory and one which was retrieved by staff from the National Archives.

Attachment 1: **Summary Minutes of the January 12, 1951 meeting of Argonne Division of Biological and Medical Research Program Committee states on page 4:**

> As parts of a general discussion concerning the program, it was pointed out that the current investigation of toxicity in human beings is an excellent example of the kind of activity for which a hospital is needed and use for which the Argonne Cancer Hospital is intended. The Argonne Cancer hospital can reasonably be expected to provided the access to clinical material needed by the Division in its cancer investigations and certainly the hospital is the proper place for experiments involving radioactivity in human beings.
>
> Concerning the last subject, Mr Marinelli [an Argonne doctor] asked if there is a general policy concerning human experimentation. Dr. McLean [head of the University of Chicago Toxicity Laboratory and AEC RW program] said that the Advisory Committee of the Division of Biology and Medicine of the AEC has been approached several times in the past for a general policy and had refused to formulate one. Apparently, proposals for human experimentation with radioactivity will continue to be judged on an individual basis.

In comment, Dr. Robert Schlenker, current chair of Argonne Lab's Director's Ad Hoc

Committee on Human Health Research, states that this exchange "implies that experiments proposed by ANL [Argonne Lab] must be reviewed by the AEC."

Attachment 2: **April 14, 1953 response from H.W. Patt of the Division of Biological and Medical Research at Argonne to J. Vodraska, who apparently had volunteered to serve as an experimental subject, states:**

> We do not conduct experiments on human beings. Moreover, the treatment for radiation sickness referred to in the newspapers is, in my opinion, not practical.

Schlenker observes that this document "simply states that ANL does not conduct experiments on human beings. Concurrently, of course, ANL scientists were administering radioisotopes to healthy employees for the purpose of calibrating whole body counters." According to Schlenker, no records have been found relating to these uses of radioactivity.

It should be noted that staff has found a number of letters similar to Patt's sent by the AEC in response to unsolicited offers to volunteer for experimentation. Essentially, it is a form letter.

Attachment 3: **March 14, 1947 memorandum from Dr. A.M. Brues, director of the Argonne Biology Div. to N. Hilberry, Associate Laboratory Director, concerning AEC policy on clinical testing**

Dr. Brues first quotes from a letter from Stafford Warren, chair of the Interim Medical Advisory Committee:

> At present clinical testing programs have been authorized as a part of the University of California at Berkeley and the University of Rochester contracts only. In the interim period. . . . it is requested that no other clinical testing be performed by other contractors than has been already authorized.

Brues then addresses the status of work being undertaken at Argonne, specifically Dr. Leon Jacobson's arsenic76 tracer studies using human subjects:

> I have been assuming that this [i.e., the tracer studies] is part of the approved program of this Laboratory and that the responsibility for the patients was clearly in the hands of the University of Chicago, which Dr. Jacobson assures me is satisfied with the program at Billings [Hospital].
>
> In the first place, the University of Chicago has been engaged in

work using human subjects and related to the work of the Manhattan Project. As reference to this regard, I can cite report No. CH-3607, which was declassified on the last day of 1946, entitled, "Distribution and Excretion of Plutonium in Two Human Subjects."

. . . . [i]t seems to me fortunate that we can parallel our animal observations in humans through the experience and clinical contact of Dr. Jacobson. This work will obviously give us information bearing on the extrapolation of animal work to the human which is most justifiable in the case of this short-lived isotope, which we have good reason to believe may be of actual value to these patients.

LIST OF ATTACHMENTS

ATTACHMENT 1: Summary Minutes of the January 12, 1951 meeting of Argonne Division of Biological and Medical Research Program Committee

ATTACHMENT 2: April 14, 1953 response from H.W. Patt of the Division of Biological and Medical Research at Argonne to J. Vodraska, who apparently had volunteered to serve as an experimental subject

ATTACHMENT 3: March 14, 1947 memorandum from Dr. A.M. Brues, director of the Argonne Biology Div. to N. Hilberry, Associate Laboratory Director, concerning AEC policy on clinical testing

ATTACHMENT 1

Summary Minutes of the January 12, 1951 meeting of Argonne
Division of Biological and Medical Research
Program Committee

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MINUTES

of the

PROGRAM COMMITTEE

Division of Biological and Medical Research
Argonne National Laboratory

* * *

The eleventh meeting of the Program Committee began at 10:15 A.M. on January 12, 1951, at Site B.

Present:

A. H. Brues, Chairman
R. J. Hasterlik
L. D. Marinelli
F. C. McLean
J. E. Rose
R. E. Zirkle
E. L. Powers, Executive Secretary

R. L. Cardwell

J. S. Schubert (part time)

* * *

Reorganization of Research in Biology, Medicine and Radiological Physics
(See Minutes of 10/6/50, p. 4-23; and 1/5/51, p. 10-74)

Dr. Brues noted that Dr. Schubert was invited to this meeting in order to discuss with the members of the Program Committee the relationship of himself and his group to the research activities of the Division of Biological and Medical Research and to the service activities of the Health Services Division. For Dr. Schubert's benefit, Dr. Brues reviewed briefly the scope of the reorganization plans as discussed in the last meeting. He recalled the general opinion that the Schubert group belonged programmatically and administratively within the Division of Biological and Medical Research, but that some responsibilities toward the bioassay functions of the Health Services Division should be maintained by the group. In regard to Dr. Lawrence S. Myers, Dr. Schubert said that he felt Dr. Myers could not be satisfied with an entirely routine job, but that Myers would have more ti[illegible] the future to develop analytical methods which are needed for fecal ana[illegible]es. The development of these analytical methods and the assistance

Myers would give to investigators in H^3 analyses and to investigators dealing with low levels of C^{14} would constitute an amount of research work acceptable to Myers while allowing sufficient time for the routine aspects of the bioassay services. Accordingly, Schubert agreed that Myers could be listed formally as devoting about 90 per cent of his time to bioassay and about 10 per cent to research. He agreed that it would continue to be necessary for him to have frequent contact with Myers and that he probably would be the best liaison between bioassay services and the Program Committee. In order to formally recognize this relationship, it was agreed that Dr. Schubert would be listed as devoting approximately 10 per cent of his time to distributed services, being responsible to Dr. Hasterlik as Director of Health Services for this portion of his time, and that the remainder of his time would be spent under the authority of the Division of Biological and Medical Research. Dr. Schubert left the meeting at this time.

There were several questions raised concerning the names of research groups and several suggestions were incorporated into the new listing.

Dr. McLean said that with the death of the Noyes Panel, there is no laboratory group within the AEC which now recognizes formally the need r thought on military and civilian defense problems. While he does not el that a special group carrying such a name need be set up, it should e agreed that one of the responsibilities of some group (e.g., Special roblems) would be this type of problem.

Human Radium Studies
(See Minutes of 11/17/50, p. 8-59)

There was brief discussion concerning certain legal aspects of the current study of toxicity of Ra in humans. One of the questions being considered by the Legal Department of ANL is the acquisition of bodies of persons known to have ingested Ra. It appears that a Superior Court order for exhumation with the consent of the nearest relatives is all that is necessary to obtain the bodies. If there is no kin, a Supreme Court order is needed. Dr. McLean pointed out that a person, when placed in a museum, is considered by the courts to be legally buried. Dr. Hasterlik could foresee no serious problems in obtaining authority to exhume the bodies of the people of interest to the Ra group.

Dr. Hasterlik said that one of the Ra patients, Mrs. K., is becoming quite ill and it is likely that she will die in the near future. This patient is of particular interest because external γ ray measurements indicate a total Ra content of 2 μg. The usual experience has been that necrotic symptoms are not demonstrated by persons containing less than 1 μg. For this reason, all other possible diagnoses must be carefully ruled out, and to this end a biopsy on her foot is planned for next week.

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r. Arnold, who has had no military experience, has requested of his
ard a deferment to July 1, 1951, which marks the end of his term
fellow. He is investigating autoradiographic means of locating
ers deposited in bone, a subject of considerable interest to the
at the present time in view of its concern with chronic effects
radioactive elements.

r. Brues expressed the opinion that a move to retain Dr. Arnold in
field in which he has been working for a year and a half and for
e has shown some aptitude should be considered.

r. McLean said that it was his experience that Draft Broads should
dealt with by persons other than the individual and that the Army
be dealt with after a person has enlisted. He felt that the Army
ities show much more sympathy for an appeal for the efficient
ation of a scientist or physician than the ordinary Draft Board is
to. Mr. Marinelli recalled General Cooney's appeal for referrals
inutes of 10/6/50, p. 4-21) and suggested that should all else fail,
rnold be recommended to him.

. Looney, who is responsible for a large part of the administra-
f the current studies on the metabolism and toxicity of Ra in
eings, is in the Organized Reserves of the U. S. Navy. Up to
is Commanding Officer has shown a willingness to cooperate and has
d him to remain with Argonne. Dr. Brues felt that a continuance
s type of deferment should not be expected and wondered if a more
policy emanating from a higher level is not in order. He expressed
elief that the administration of the Laboratory is not anxious to
the initiative in asking for assignment of military personnel on
al duty here at the Laboratory. Dr. Brues said he would attempt to
ver the mind of the Committee on Medical Sciences of the Panel on
al Aspects of Atomic Warfare, National Military Establishment,
rning persons like Dr. Looney.

Concerning draft deferments in general, it was agreed that it would
ry wise for the Laboratory to have a paragraph on file for each man
iting a request for deferment, describing the particular way in
he is contributing to the research of the Laboratory.

Argonne Cancer Hospital

Dr. Hasterlik discussed briefly the research program of the Argonne
r Hospital. It is his understanding that the Argonne Laboratory is
ed to assist in the determination of the research program to be
d out in this installation, and he requested members of the Program
ttee to consider the kinds of problems that are properly and most
tently investigated in it.. The program is to be discussed with
Aggeshall, Director of the Argonne Cancer Hospital and Dean of the

vision of Biological Sciences, in the very near future, and Dr. Hasterlik shed to have some definite proposals ready for consideration. As parts a general discussion concerning the program, it was pointed out that e current investigation of toxicity of Ra in human beings is an excelnt example of the kind of activity for which a hospital is needed and use for which the Argonne Cancer Hospital is intended. The Argonne ncer Hospital can reasonably be expected to provide the access to inical material needed by the Division in its cancer investigations and rtainly the hospital is the proper place for experiments involving dioactivity in human beings.

Concerning the last subject, Mr. Marinelli asked if there is a neral policy concerning human experimentation. Dr. McLean said that e Advisory Committee of the Division of Biology and Medicine of the AEC s been approached several times in the past for a general policy and s refused to formulate one. Apparently, proposals for human experints with radioactivity will continue to be judged on an individual basis.

Toxicity Laboratory

o the present time, the Toxicity Laboratory has been supported the Division of Military Applications through the Division of Biology Medicine of the Commission. Dr. McLean felt that further support on s basis will be difficult and that it is not likely the present conct which expires October 1, 1951, will be renewed. There are no lications as to what will happen to the facilities nor to the persons loyed by the Laboratory after expiration of the present contract. McLean listed several possibilities -- 1.) It may return to the ical Corps as the type of laboratory it was during the last war, 2.) ay retain an interest in radiation biology but be supported by one the Services. For example, the Air Force which has a considerable erest in the deleterious effects of radiation, their prevention and apy, has no facilities of its own and it may be interested in acring one, 3.) The Toxicity Laboratory may continue to be occupied military and civilian defense and atomic warfare as an independent tract, or as a part of the Argonne National Laboratory.

It is Dr. McLean's impression that a proposal to incorporate the city Laboratory into Argonne Laboratory would not be unwelcome in ain quarters and that the facilities and certain of the personnel may ted on problems relating to civilian and military defense (see es of 1/12/51, p. 11-80). Dr. Powers said that he doubted that the inistration of the Laboratory would feel kindly toward assuming reibility for the physical plant on the campus occupied by the Toxicity y because of the Laboratory's current heroic effort to consoli- graphically.

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e of CP-3, the number which just makes possible lethal exposures
s in reasonable lengths of time. Drs. Zirkle and Vogel are to
h Dr. McCorkle to settle design questions and it is likely that
source will finally become available to the Division.

Human Toxicity Tests

Jack Schubert's desire for a test in humans of the toxicity of
carboxylic acid was discussed. The compound shows some promise
as a remover of deposited Be. Obviously the Laboratory should not
irectly involved in this sort of testing program, and it is sug-
hat perhaps some pharmaceutical house could be persuaded to handle
similar tests. The Chairman said he would inquire of the Medical
f the Division of Biology and Medicine, Atomic Energy Commission,
ce.

Human Radium Problem

a patient referred to previously (see Minutes of 1/12/51,
) of particular interest because the low estimated radium content
cal symptoms of radium poisoning demonstrated by her, underwent a
last week. Definite diagnosis of fibrosarcoma has been made. A
of the biopsy material has been sent to Dr. Robley D. Evans for
Ra^{228} determinations. Dr. Hasterlik also reported sending to the New
erations Office information from the Barker files concerning certain
lyses which the U. S. Radium Corporation had made at one time.

e other item was reported for the information of the Committee. The
who was interested in running a newspaper story of the toxicity
as been consulted. This individual is an employee of the State
e Department and his motive in this instance was to demonstrate how
institutions are cooperating in scientific research. When the delicate
of this particular research was explained to him, he agreed not to
his publicity until such time as Dr. Hasterlik thinks it proper.

Mesothorium

orrespondence between W. P. Norris and H. Deplanche of The Rare
ls and Metals Company, Inc., 21 East 40th Street, New York 16, New
resulted in their offering the Laboratory 8 to 10 mg Ra^{228} (see
s of 1/12/51, p. 11-81). If the Laboratory were willing to divide
to lots of 8 to 10 mgs, the Minerals Company would waive
the Ra^{228}. Otherwise the only Ra^{228} (mesothorium bromide)
le from them at the present time is a tube of Ra^{228} No. T 162, which
led on April 22, 1944 and measured last on February 17, 1950, as
ing 69.1 mg Ra equivalent. The cost is $35.00 per mg ex-warehouse,

...d by Mr. Wahl, namely 50 μc per cubic meter of air as T_2 or HT, is the ...um permissible level recommended by the International Commission on ...ological Protection (see Nucleonics, February 1951, pp. 70-74). Mr. ... pointed out that this figure differs somewhat from that used by this ...atory at the present time as the mpl for operations. Allowing ...week total dosage, 23 $\mu c/m^3$ is indicated. Since an allowance for ...re to other radiations must be made, 10% of this figure, or 2 $\mu c/m^3$ ..., is the mpl for Argonne operations. The need for experiments on ...ver and metabolism was briefly discussed.

pril 19

Human Radium Studies

...r. McLean told of the recently initiated conferences with various ...ted members of the division concerning the decontamination of persons ...ing radium. This group will continue as one of the study groups pre... described (see Minutes of 10/20/50, Appendix I, pg. 5-28). Dr. ...lk reported that his recent examination of the files of the U. S. ...orporation reveals the disturbing information that beginning in ... there was rather free substitution of RdTh (Th^{228}, $T_{1/2}$ = 1.9 years) ... (Ra^{228}) in the paints used for clock dials. The Th^{228} contents ...from 2 to 25% of the total activity, indicating great difficulty in ...ing the original activity in a particular patient. Furthermore, it ... necessary now to appreciate Th^{228} toxicity in addition to that of ... Ra^{226}. It seems likely that the dial painters will not constitute ...urce of information concerning Ra toxicity in humans.

Consultants

... ...ues announced the intention of the Division to contract with ... Ward Wainwright as a consultant. Dr. Wainwright was formerly ...s. He is interested in radioautography and will assist and ... Division in this field.

* * *

...ing adjourned at 11:30 A.M.

* * *

E. L. Powers
Executive Secretary

Committ

ATTACHMENT 2

April 14, 1953 response from H.W. Patt of the Division of Biological and Medical Research at Argonne to J. Vodraska, who apparently had volunteered to serve as an experimental subject

Patt

April 14, 1953

Mr. Joseph Vodraska
306 E. 71st Street
New York City

Dear Mr. Vodraska:

Thank you kindly for your letter of April 8. It is apparent that you are a patriotic and valuable American citizen.

We do not conduct experiments on human beings. Moreover, the treatment for radiation sickness referred to in the newspapers is, in my opinion, not practical.

Your interest in being of service to your country is certainly praiseworthy, and I'm sure that you will find a way to serve.

Sincerely yours,

Harvey M. Patt
Division of Biological
and Medical Research

HMP:amh

cc: A. M. Brues
Reading file
File

ATTACHMENT 3

March 14, 1947 memorandum from Dr. A.M. Brues, director of the Argonne Biology Div. to N. Hilberry, Associate Laboratory Director, concerning AEC policy on clinical testing

March 14, 1947

Mr. N. Hilberry — Associate Laboratory Director

Dr. A. M. Brues — Director, Biology Division

Clinical Testing.

I have received, as a member of the Interim Medical Advisory Committee, a letter from Stafford L. Warren who is chairman of this committee, regarding clinical testing. He states:

> "At present clinical testing programs have been authorised as a part of the University of California at Berkeley and the University of Rochester contracts only. In the interim period.....it is requested that no other clinical testing be performed by other contractors than has been already authorised.
>
> "This memo refers only to work done under the auspices of, at the expense of, or with equipment furnished by and under the responsibility of the Atomic Energy Commission (and not).....by a doctor upon his own and his unversity's responsibility and at costs not reimbursable by or not having any connection with the Atomic Energy Commission work".

I am not certain what relation this has to the use of arsenic^{76}, which Dr. Jacobson and Dr. Neal have been preparing through Argonne and are using at Billings Hospital in tracer amounts. I have been assuming that this is part of the approved program of this Laboratory and that the responsibility for the patients was clearly in the hands of the University of Chicago, which Dr. Jacobson assures me is satisfied with the program at Billings.

In the first place, the University of Chicago has been engaged in work using human subjects and related to the work of the Manhattan Project. As reference in this regard, I can cite report No. CH-3607, which was declassified on the last day of 1946, entitled "Distribution and Excretion of Plutonium in two Human Subjects". In the second place, this work is part of the program of the Laboratory as submitted at the meeting of the Medical Advisory Board January 24, 1947, under three of the ten items on this program, namely, (1) Effects of Irradiation on Structure and Functions of the Blood, (2) Response to Different Types of Radiations of Various organs and Tissues including Tumors, and (3) Absorption, Disposition and Elimination of Radioactive Elements from the Body.

The purpose of this work falls under these categories of our program, and it seems to me fortunate that we can parallel our animal observations in humans through the experience and clinical contact of Dr. Jacobson. This work will obviously give us information bearing on the extrapolation of animal work to the human which is most justifiable in the case of this short-lived isotope, which we have good reason to believe may be of actual value to these patients.

I wonder if representations could not be made indicating that this work is an important and consistent part of our program at Argonne and also that the Argonne has included clinical research in its program for some time past.

AMB:bm

Austin M. Brues, M.D.

cc: Reading File