

INTRODUCTORY STATEMENT BY PROFESSOR MERRIL EISENBUD
BEFORE THE
HOUSE COMMITTEE ON NATURAL RESOURCES

February 24, 1994

I am Merrill Eisenbud, Professor Emeritus of Environmental Medicine at New York University Medical Center, where I served for 26 years as Director of the Environmental Studies Laboratory. I went to New York University after a twelve year career with the Atomic Energy Commission where, for the most part of that period, I was Director of the Commission's Health and Safety Laboratory, located in New York City. For many years that laboratory was known by the acronym, HASL, but the name was changed in the mid-1970's to the Department of Energy Environmental Measurements Laboratory. I hold the degree B.S.E.E. and two Sc.D degrees (honoris causa). I am a member of the National Academy of Engineering, and an Honorary Member of the National Council on Radiation Protection, a Fellow of the New York Academy of Medicine, a Foreign Member of the Brazilian Academy of Sciences, and an Honorary Life Fellow of the New York Academy of Sciences. Since my retirement from New York University in 1985, I have resided in Chapel Hill, North Carolina. I currently hold the position of Distinguished Scholar in Residence, Duke University Medical Center, Division of Occupational and Environmental Health.

I began my career in environmental science in the mid 1930's nearly 60 years ago. At various times during these many decades I have been involved with research, teaching, and administration,

in programs concerned with air and water pollution, occupational health, the urban environment, and both the ionizing and non-ionizing radiations. Environmental radioactivity has been a major interest of mine for many years. I published the first edition of a textbook called Environmental Radioactivity in 1963, the 4th edition of which will be published by the Academic Press in 1996. I have published more than 200 peer-reviewed articles in the scientific journals, and in 1990, my professional memoir, titled An Environmental Odyssey was published by the University of Washington Press. A little later I will request that certain pages from that book which relate to the BRAVO tragedy be entered into the record of this hearing.

For your purposes today, the period of my career that will be of most interest to you was from 1951, when I first became involved in studies of fallout from nuclear weapons tests, and the end of 1954, the year of the BRAVO explosion. It is important that I begin by telling you why HASL was organized, and how it became involved in fallout measurements.

When AEC was created by Congress in 1947 it was realized that there were many contractors, both in academia and industry that would require assistance in dealing with their problems in industrial and radiological hygiene. It was decided that an AEC laboratory should be created in New York to provide the required technical assistance to the many plants and laboratories under

contract with the Commission's New York Operations Office. The assistance provided by HASL in the first years was mainly to the uranium and beryllium industries, both of which were confronted with serious health problems. To provide this help, a laboratory of high quality was needed together with a staff of industrial hygienists, physicians, physicists, engineers, and chemists who were willing to work under field conditions.

When the weapons testing programs began at the Nevada Test Site (NTS) in 1951, our staff was not initially involved in any way. We learned about Operation RANGER in the news broadcasts, just like most other people. A few days after the tests began, I received a call from Rochester that the Eastman Kodak Company had detected radioactive particles in the air intake filters of their film manufacturing plant. From telephone calls I made to Washington and the NTS I was amazed to learn that no fallout monitoring network had been established by AEC. This was obviously a major deficiency in planning since it had been known that fallout from the New Mexico test explosion called TRINITY in August 1945 had spoiled photographic film packaged with interleaving paper made from fallout-contaminated corn-stalks harvested many hundreds of miles from the explosion. As a result of that experience, the Eastman Kodak Company wisely adopted the practice of routinely monitoring its plants and laboratories for radioactive dust.

We received that call on a Friday afternoon when it was snowing throughout the Northeast. Over the weekend we collected samples of snow throughout the northeastern states, and by early the following week we were able to draw a map showing the extent of the fallout. That experience was undoubtedly the first investigation of fall-out at great distances from the site of a nuclear explosion.

As a result of that experience HASL was requested by the AEC Division of Military Operations to prepare a national network of fallout monitoring stations. This network was extended world-wide in the following year. The collection system was operated on an unclassified basis, and the first summary of our findings was published in the journal Science, in February 1953. This was the first of many publications on the subject in the open literature and they are noteworthy because they show that AEC at that time was not attempting to conceal the fact that fallout of radioactive dust from test explosions was occurring.

Our early studies led me to become very much concerned about the possibility that lethal levels of radiation might occur at great distances from explosions in the megaton range of TNT equivalent. In addition to the global network of monitoring stations we then maintained, HASL requested and was granted, the assignment to monitor fallout intensively in the 200 to 500 mile annulus around the test site. We first undertook these studies

in Nevada during Operation JANGLE in the Fall of 1951. This series consisted of two small explosions, one on the surface, and one just below the surface. My associates and I became particularly concerned about the levels of fall-out measured in northern Nevada, about 400 miles from the test-site. The measured fall-out intensities we measured were not so high as to be alarming of themselves, but they were the result of explosions equivalent in explosive yield to no more than about one KT of TNT. Since explosions more than ten thousand times larger were being planned for the tests in the Marshall Islands, it took but a simple calculation to show that very dangerous fallout might occur at great distances.

Using aerial survey methods of our own design, we undertook intensive monitoring of all the North Pacific atolls after the MIKE detonation in the Fall of 1952, but found no significant fallout. We concluded that whatever fallout had taken place was in the open ocean, in which the small atolls were thinly distributed.

We then began to prepare for CASTLE, scheduled to begin on March 1 1954. In the interim we had been consulting with Col. Lulegian, an Air Force meteorologist, who took quite seriously our concerns about the potential dangers from massive fall-out following explosions in megatons range of yields. He sent me a highly classified report he had prepared, in which he confirmed

by rigorous analysis that what we had concluded almost intuitively based on our limited field experience: i.e. Lethal levels of fallout could occur at great distances from near-surface explosions of megaton weapons. For some unexplained reason, that report was recalled within days after I had received it. I have not seen it since.

What happened on March 1 1954 as a consequence of BRAVO is now a part of history, but the full story is largely untold. The fact that heavy fallout was occurring was first recorded about seven hours post-firing by a continuous gamma radiation detector placed by HASL in the care of Air Weather Service personnel on the atoll of Rongerik. The HASL representative aboard the Task Force flagship was immediately notified by radio. What happened thereafter is still a matter of mystery. There were delays in confirming that the fall-out had occurred and in measuring its extent. I was then in the New York laboratory, and was notified of the Rongerik report immediately. Thereafter there was a blackout of information for many hours. The reasons why aerial confirmation was not permitted and why prompt evacuation procedures were not implemented has never been explained.

My knowledge of the events prior to the explosion and immediately afterwards are summarized from my notes as well as official documents in An Environmental Odyssey. Of particular interest to you will be the material on pages 72 to 103, which

also covers the effects of the BRAVO fallout on the Japanese fishing boat Fukuryu Maru (The Lucky Dragon). These pages have been made available to the Committee, and I request that they be included in the record of these proceedings. I call your attention to some of the key points made in my book:

1. The general lack of initial preparedness for fallout monitoring despite the experience after TRINITY.

2. That AEC recommended a standby evacuation capability for CASTLE, which the Task Force declined to provide as being unnecessary.

3. Had the instrument on Rongerik not been installed by HASL, there would have been no notification that the fallout was occurring, with possible lethal consequences to both the Air Weather Service personnel and the residents of Rongelap.

4. That had the people on Rongelap and Rongerik been evacuated by air when the HASL report was first made, the doses sustained would have been greatly reduced with far less severe consequences.

5. That the Task Force did not plan to provide a monitoring capability beyond Eniwetak and Bikini. Because of his concern for the safety of residents of the Pacific islands, Commander-in-Chief Pacific (CINCPAC), located in Pearl Harbor, supported the HASL program and provided supporting aircraft, personnel, equipment, and communications. The Task Force support of the HASL program was limited to a single billet aboard the Flagship for the use of the HASL coordinator. However, when the emergency arose, he was prevented from implementing the procedures to which both JTF-7 and CINCPAC had agreed.

6. That the AEC public announcements left much to be desired.

7. That to my knowledge there has never been a formal investigation of the circumstances that led up to the BRAVO tragedy. In another few days it will be 40 years since the events of March 1 1954, but today is the first time I have been invited to report on my knowledge of the events.

My prepared testimony is now concluded and, to the best of my ability, I will be pleased to answer any questions you may have.