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Sandia Corporation

REPRINT

PROCEEDINGS OF THE UPPER ATMOSPHERE SAMPLING SYMPOSIUM
PART I

Held at Sandia Laboratory,
Albuquerque, New Mexico
April 11-13, 1961

Compiled by

J D Shreve, Jr

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Proceedings of the Upper Atmosphere Sampling Symposium

Part I

Sandia Laboratory, Albuquerque, New Mexico
April 11-13, 1961

Compiled by J. D. Shreve, Jr.

(Part II, the classified portion of the Proceedings, will be published as SCR-421)

July 1961

FOREWORD

In early February, 1961, the Fallout Studies Branch of the Division of Biology and Medicine of the AEC asked Sandia Corporation to host a Symposium on upper atmosphere sampling at their Sandia Laboratory. Invitations were sent by DBM/AEC after the dates of April 11 through 13, 1961, were chosen for the meeting. Papers were invited. As participants declared themselves, a trial agenda was assembled to assess the coverage likely for the various subjects of importance. Seeming inadequacies led to specific requests for additional participation. The response was most heartening as the final agenda attests. Discussion topics were interspersed with contributed papers as given topics were treated. These too are reproduced as edited transcriptions of recordings made at the time.

The Proceedings have been divided for publication into Part I (Unclassified) and Part II (Classified). Those persons who attended the Symposium and for whom there exist AEC-approved mail channels will receive both Parts I and II. Recipients on TID-4500 distribution normally are eligible to receive only unclassified material. Individuals or agencies not included in the initial distribution but who are eligible to receive the classified portions of the Proceedings should place their requests through regular channels.

Editing of the Proceedings has been held to a minimum. Continuity of the text required use of an outline system to indicate the continuity of the Symposium presentation and panel discussions as shown in the Agenda: hence the assignment of roman numeral-letter designations in the table of contents and textual headings. Readers who attended the Symposium may readily key the system to the Agenda.

The reader's attention is called to the omission from the Proceedings of the address by Dr. C. E. Junge, Air Force Cambridge Research Laboratories, entitled Diffusion and Sedimentation of Fine Particles in the High Atmosphere, which appears on the April 11 agenda but which is not listed in the table of contents. Unfortunately, Dr. Junge was unable to make the presentation because of his absence in India. Also omitted from the Proceedings is the text of Dr. E. A. Martell's address at the official banquet held April 12.

ACKNOWLEDGEMENTS

The success of the Symposium stemmed principally from the quality of papers presented and, no less, from the unusually participative attitudes of those in attendance. Our gratitude extends therefore to all who attended.

Special acknowledgement is due Dr. E. A. Martell for his thought-provoking banquet address prepared in addition to his formal session paper. Finally, the idea of the Symposium, first momentum given the plans, and continuous encouragement through preparatory and final phases, were the work of Joshua Holland and George Anton. We warmly thank them.

CONTENTS

The table of contents for Part I reflects the entire agenda of the Symposium; applicable notations indicate classified papers to be found in Part II (SCR-421). The table of contents for Part II lists only the classified portions of the Proceedings.

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*Speaker presenting the paper.

†Classified paper, published separately in Part II, SCR-421.

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*Speaker presenting the paper.

†Classified paper, published separately in Part II, SCR-421.

‡A Classified appendix to this paper appears in Part II, SCR-421.

ADDRESS OF WELCOME

C. F. Quate
Vice President, Research, Sandia Corporation

Good morning, Gentlemen:

It is with great pleasure that we welcome you to Sandia. We are pleased to be able to host this meeting as arranged by Mr. Holland of DBM.

This conference is to deal in various ways with one of the most difficult areas of all nuclear energy--that of radioactive contamination of the atmosphere. I am certain that lack of knowledge on the part of our public contributes immeasurably to the difficulties we encounter in obtaining public acceptance of the many scientific programs which must be carried on to maintain the advances in this field.

In addition to the scientific implications of the Upper Atmosphere Sampling program, there is further need for accurate information. I have heard people discuss the implications of using rather small sources of radioactivity--such as SNAP devices--for space probes and they are inhibited by the lack of acceptance of operating such devices in the atmosphere. There is even more need for accurate information to provide a basis for legislation on such items as hazards pay, site selection, and proper safeguards. I would hope that from this conference we will see a continued and increased program of effort in this important field.

PURPOSES OF THE MEETING

J. Z. Holland
Chief, Fallout Studies Branch
Division of Biology and Medicine

The Division of Biology and Medicine conducts research on biological effects of radiation, on the beneficial uses of radiation and radioisotopes in biology and medicine, and on special biomedical problems arising in the atomic energy program generally. The Fallout Studies Branch of the Division deals with the biomedical problems arising through the introduction of man-made radioactivity into the atmosphere.

The research program includes not only studies of the dietary levels, ingestion rates, body burdens and external radiation doses from atmospheric radioactivity and fallout, but also: (1) studies of source conditions which affect the composition and properties of the radioactive materials released, (2) studies of the meteorological factors affecting the dispersion and deposition of contaminants from sources anywhere in the atmosphere or near space, (3) studies of aerosol physics and chemistry, (4) studies of actual levels of radioactive contamination of the atmosphere and the earth's surface by means of extensive sampling and analysis programs, and (5) developmental projects aimed at improving and extending the sampling and analysis capabilities. We are not engaged in radiation monitoring per se, but with measurements as a source of experimental information to increase our understanding of the phenomena and to test and improve our ability to predict.

These studies are conducted in AEC laboratories, in universities and industrial laboratories under contract, and in coordinated and cooperative programs with the Weather Bureau, the Air Force, the Navy, the Defense Atomic Support Agency, and others.

Our interest in the upper atmosphere dates back to the first weapons tests, when transport in the upper troposphere was recognized as being related to contamination patterns observed on the ground. Stratospheric studies in the period 1954-1958 were concerned with debris from Ivy, Castle, and Redwing tests and smaller amounts from the UK and USSR tests. During this period Project Ash Can was initiated. While the possibility was recognized that some debris might have been projected to altitudes greater than 100,000 feet, no serious attempt was made to verify this. Firstly, it was hard enough to get interpretable quantitative data in the altitude range accessible by simple balloon-borne, fan-driven filter systems. Secondly, only 1 to 2 percent of the atmosphere, or perhaps 5 to 10 percent of the stratosphere were above our 90,000-foot sampling limit. Thirdly, the data obtained in Project Ash Can consistently showed a strong decrease in fission product radioactivity upward from 65,000 or 80,000 feet to the 90,000-foot level.

In the period 1958-1960 the high altitude shots Teak and Orange in Operation Hardtack gave us a new incentive for pushing our sampling altitude capability upward. The addition of rhodium to the Orange device made it possible to trace the debris from this one detonation. A conceptional study contract with the Atlantic Research Corporation resulted in some ideas which you will hear about later. The Air Force Cambridge Research Laboratories, with partial AEC support, have contracted with Aerolab Development Company to design a rocket-borne sampler. We also have at least three lines of development underway, which will not be reported here, to extend balloon sampling capabilities more nearly to the limit of balloon altitudes, that is, 130,000 to 140,000 feet. These involve cryogenic adsorption, electrostatic precipitation, and thermal precipitation. It should also be mentioned that rocket-borne filter devices for close-in cloud sampling were used in Operations Plumbbob and Hardtack.

Now in 1961 we are confronted with a rapidly advancing program of development of nuclear power for rockets and satellites. True, the amounts of radioactivity likely to be introduced into the high atmosphere from these sources in the next decade are minute by weapons-test standards. But the acceptable levels of contamination over the world are not to be judged by comparison with weapons test fallout. As Dr. Quate pointed out, just what levels will be considered acceptable may depend in part on the confidence of the public in the quality of our information. Furthermore, these are peacetime programs, new programs, which, if successful, will grow. The ultimate limits on them may well be determined by the levels of atmospheric contamination which they create. If we are to have a capability for observing and studying the patterns of distribution and transport of debris from such sources when they become real some years from now, we must start now to develop the techniques. The job will be very difficult and we cannot hope to be ready if we wait until the last minute.

With this in mind, the Division of Biology and Medicine, through the Division of Military Application, has asked the Sandia Corporation to prepare a proposal for a complete program of research on high altitude radioactivity. This is to include a feasibility study, and depending on the outcome of this, the outline of a sampling and analysis program, development of sampler design specifications, design and procurement of samplers and vehicles, operation of sampling flights, supervision of sample analysis, collateral meteorological studies and measurements, and interpretation and reporting of results. As you will see, the array of talents at Sandia applicable to these problems is impressive.

As a first step, it was suggested that an information meeting be held. It should now be clear why this meeting is held at this location.

The broad questions to which we hope to address ourselves at this meeting are:

(1) What requirements exist for obtaining information on radioactivity or particulate matter above roughly 100,000 feet? This should include practical needs such as ours or military needs as well as purely scientific values to be gained.

(2) What is known or can be predicted and what is unknown about the radioactivity of the high atmosphere and about the atmospheric dynamics which would determine the life history of particulate material introduced at high altitudes?

(3) What is the state of the technology for collecting and analyzing samples at altitudes above balloon ceilings? Consideration of direct observation in situ should not be overlooked.

It should be emphasized that the Division of Biology and Medicine has as yet made no commitment to any program in this field. We have asked for funds in Fiscal Year 1962 which would permit a modest start to be made. Whether we go ahead, and just what we decide to do can be greatly influenced by the ideas brought out in this meeting, and by the degree of interest shown here and later by other agencies in the possibilities of cooperative efforts.

We hope, too, that the proceedings of this meeting will provide a good compendium of the state of the art at this time for the use of all those who have interests in this subject but could not be present.

We are very grateful to the Sandia Corporation and to Dr. Shreve for serving as host for this meeting and for all the pleasant arrangements which they have made.

UPPER-ATMOSPHERE SAMPLING SYMPOSIUM

Sandia Laboratory, 1961

April 11 Agenda

9:00 Badges, name tags, registration, seating. Coffee available.

9:30 Welcome - C. F. Quate, Vice President, Research, Sandia Corporation

9:35 Statement of Motives and Outline of Meeting Purposes
J. Z. Holland, Chief, Fallout Studies Branch, AEC/DBM

9:50 Stage instructions and meeting procedures

FIRST SECTION - DEBRIS DISPERSAL COMPUTATIONS

J. R. Banister, presiding

10:00 The Fall of Small Particles or Heavy Molecules through the Upper Atmosphere
H. L. Davis,* J. R. Banister, Sandia Laboratory

10:30 Diffusion and Sedimentation of Fine Particles in the High Atmosphere
C. E. Junge, Air Force Cambridge Research Laboratories

11:00 Atmospheric Fallout from an Extra-Atmospheric Nuclear Explosion†
J. F. Ahearne, J. A. Welch,* Air Force Special Weapons Center

SECOND SECTION - METEOROLOGICAL FORCES

L. B. Smith, presiding

11:30 Atmospheric Tracers Above 100,000 Feet
L. Machta,* R. J. List, U. S. Weather Bureau

LUNCH

1:30 Effects of Large Diurnal Density Changes on Outer Atmosphere Circulation
H. W. Church, Sandia Laboratory

1:45 A High Altitude Wind Observation from Nuclear Explosion "Teak"
J. W. Reed, Sandia Laboratory

THIRD SECTION - DEBRIS MEASUREMENTS AND INTERPRETATION

T. B. Cook, presiding

2:00 The Behavior of Nuclear Debris from the Upper Atmosphere Rocket Detonations as Determined in the High-Altitude Sampling Program
J. P. Friend, Isotopes, Incorporated

COFFEE BREAK

2:45 Results for the Rhodium 102 High Altitude Tracer
M. I. Kalkstein, Air Force Cambridge Research Laboratories

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*Will present the paper.

†Classified paper.

April 11 Agenda (continued)

3:15 Evidence for High Stratosphere Holdup of Nuclear Bomb Debris
E. A. Martell, Air Force Cambridge Research Laboratories

3:45 DISCUSSION: Adequacy of Radioassay Techniques; Alternate Assay Methods
Panel: I. J. Russell, moderator; G. A. Cowan, L. B. Lockhart, J. P. Friend, M. I. Kalkstein

4:45 Adjournment

April 12 Agenda

FOURTH SECTION - FUTURE DEBRIS INJECTIONS

R. S. Claassen, presiding

9:00 Snap Reactor Satellite Re-Entry Radioactivity[†]
F. D. Anderson, Atomics International

9:30 Release of Radioactivity into Space and the Upper Atmosphere from Nuclear Rockets[†]
R. S. Decker, Space Nuclear Propulsion Office, DRD/AEC

10:00 Some Aspects of the LASL Rover Flight Safety Program
L. D. P. King, Los Alamos Scientific Laboratory

COFFEE BREAK

10:30 On Project Orion[†]
R. F. Prater, Air Force Special Weapons Center

FIFTH SECTION
INSTRUMENTATION AND EXPERIMENT PHILOSOPHIES

J. D. Shreve, presiding

11:00 Sampling the Upper Atmosphere for Particulate Matter
M. Markels, Jr., Atlantic Research Corporation

11:30 Measurement of Air-Borne Radioactivity by Gamma-Ray Scintillation Spectrometry
P. F. Gustafson, Argonne National Laboratory

LUNCH

1:30 A Cryogenic Air-Sampling Rocket
D. F. Burcham[*] A. M. Haire, Aerolab Development Company,
H. E. Karig, National Engineering Science Company

2:00 Theory and Proposed Experiments for Study of Diffusive Collection
D. Q. Matejka, J. R. Banister, D. J. Rigali, Sandia Laboratory

2:45 Tracer Methods for Study of World-Wide Aerosol Dispersal[†]
I. J. Russell, Air Force Special Weapons Center

COFFEE BREAK

3:30 Some Recent Developments in Balloon-Borne Stratospheric Sampling Systems
R. C. Wood, General Mills, Inc.

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[*] Will present the paper.

[†] Classified paper.

April 12 Agenda (continued)

4:00 DISCUSSION: Sampling Principles and Rockets
Panel: J. R. Banister, moderator; M. Markels, P. F. Gustafson, H. R. Vaughn

5:30 Adjournment

OFFICIAL BANQUET - KIRTLAND OFFICERS' CLUB

6:00 Cocktails (East Room)

7:00 Dinner (Candlelight Room)

8:15 Address: Scientific Implications of an Upper-Atmosphere Sampling Capability (East Room)
Dr. E. A. Martell, Air Force Cambridge Research Laboratories

April 13 Agenda

9:00 DISCUSSION: Scientific Implications of an Upper Atmosphere Sampling System
Panel: J. Z. Holland, moderator; E. A. Martell, L. Machta, W. J. Otting, J. F. Ahearne, E. H. Fleming

COFFEE BREAK

10:30 Round Robin: Summary Statements of Interest by Represented Agencies
J. D. Shreve, moderator

12:00 Formal adjournment of Symposium

Afternoon available for subgroup discussions, tours of Sandia facilities, and individual inquiries.

I. DEBRIS DISPERSAL COMPUTATIONS

J. R. Banister
Presiding

(A)

The Fall of Small Particles or Heavy Molecules Through the Upper Atmosphere

H. L. Davis* and J. R. Banister
Sandia Laboratory

We have computed the vertical transport of small particles or heavy molecules through a one-dimensional model of the upper atmosphere by considering these small particles to be an impurity of small concentration. Both the effects of diffusion and the earth's gravitational field have been considered in the computation. In the model it has been assumed that the atmosphere is stagnant, isothermal, and has a decreasing exponential dependence of density upon the height; however, it should be relatively easy, if the need would arise, to use in the calculations the actual temperature and density of the atmosphere as a function of the height. Also, some simple models of turbulence could be taken into account, and more about turbulence will be mentioned after treating the model which has been described.

For the model, the flow rate, Γ, of the impurity across a horizontal surface is given by

$$\Gamma = -\left(\frac{K_1}{\rho}\right) c - \left(\frac{K_2}{\rho}\right) \frac{\partial c}{\partial x} , \tag{1}$$

where x is the height above the earth, c is the concentration of the impurity at x, and ρ is the density of the atmosphere at x. The second term in equation (1) is the flow rate caused by the diffusion of the impurity into the surrounding atmosphere, with K_2 being given by

$$K_2 = \frac{3}{8} \frac{\left[1 + \frac{m}{M}\right]^{1/2}}{(a + A)^2} \left(\frac{kT}{2\pi m}\right)^{1/2} . \tag{2}$$

This is the usual diffusion coefficient for one type of spherically symmetrical particle diffusing into another. In equation (2), m is the average mass of the air particles, M is the mass of the impurity particles, a is the average radius of the air particles, A is the radius of the impurity particles, k is Boltzmann's constant, and T is the absolute temperature of the atmosphere.

The first term of equation (1) is the flow rate caused by the earth's gravitational field, i.e., the drop rate through a viscous fluid. To obtain an expression for K_1, the equilibrium situation is considered. That is, regardless of the concentration or cencentration gradient, the flow rate equation will apply. So if a concentration profile is injected into our model of the atmosphere, the time development of the concentration profile is completely governed by the flow rate equation. But eventually an equilibrium will occur such that no flow will take place across a horizontal surface. For this equilibrium the effects of diffusion and the earth's gravitational field are still present; however, they counterbalance each other such that the net flow across any horizontal

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*Speaker presenting the paper.

surface is zero. This allows one to obtain an expression for K_1, since, when $\Gamma = 0$,

$$\frac{\partial c}{\partial x} = -\frac{K_1}{K_2} c . \tag{3}$$

Solving equation (3) gives

$$c = c_o \exp\left(-\frac{K_1}{K_2} x\right) . \tag{4}$$

Furthermore, it is reasonable to assume for the equilibrium situation that the concentration of the impurity has a decreasing exponential dependence upon the height:

$$c = c_o \exp\left(-\frac{Mgx}{kT}\right) . \tag{5}$$

Thus, by comparing equations (4) and (5), an expression for K_1 is obtained:

$$K_1 = \frac{Mg}{kT} K_2, \tag{6}$$

where g is the acceleration due to gravity.

Knowing K_1 and K_2 in equation (1) allows one to calculate the flow rate at time $t = 0$, provided the concentration and concentration gradient are known for $t = 0$. Thus given an initial concentration distribution, the flow rate at all points for $t = 0$ may be calculated. Then, using these flow rates, the concentration distribution may be calculated for $t = \Delta t$. This process can then be repeated again and again. It is thus possible to numerically obtain the concentration profile as a function of time and height for any given initial concentration profile; i.e., one may numerically solve the following differential equation:

$$\frac{\partial c}{\partial t} = -\frac{\partial}{\partial x}\left[\left(\frac{K_1}{\rho}\right) c + \left(\frac{K_2}{\rho}\right) \frac{\partial c}{\partial x}\right] . \tag{7}$$

Equation (7) has been numerically solved on Sandia's 704 Computer for several different initial concentration profiles. Two of the cases which have been considered will be discussed herein. In Figure 1 the time development of particles of mass 150 atomic units is plotted for an initial square distribution centered at 350 kilofeet. From this illustration it is seen that the injected profile rapidly goes into a profile which remains constant as it falls. This has been true for any initial profile which we have considered in our calculations. In Figure 2, the time development of particles of mass 1500 atomic units has been plotted. Here again, we have taken as our initial profile a square distribution centered at 305 kilofeet. The same qualitative behavior exists for these heavier particles as for the lighter particles of Figure 1; i.e., the initial profile rapidly goes into a constant profile which does not change as it falls.

From these illustrations and our other calculations which are not reported here, some semiquantitative conclusions can be obtained. First, as mentioned before, regardless of the shape of the injected profile, a constant profile is rapidly obtained. The half-width of this constant profile depends only on the mass and size of the impurity particles. Also, as will be immediately shown, once the constant profile is obtained, the descent of the profile is governed only by the gravitational term of the flow rate equation. This is true, since for the profile to remain constant, the entire profile must fall at the same rate as the maximum point of the profile.

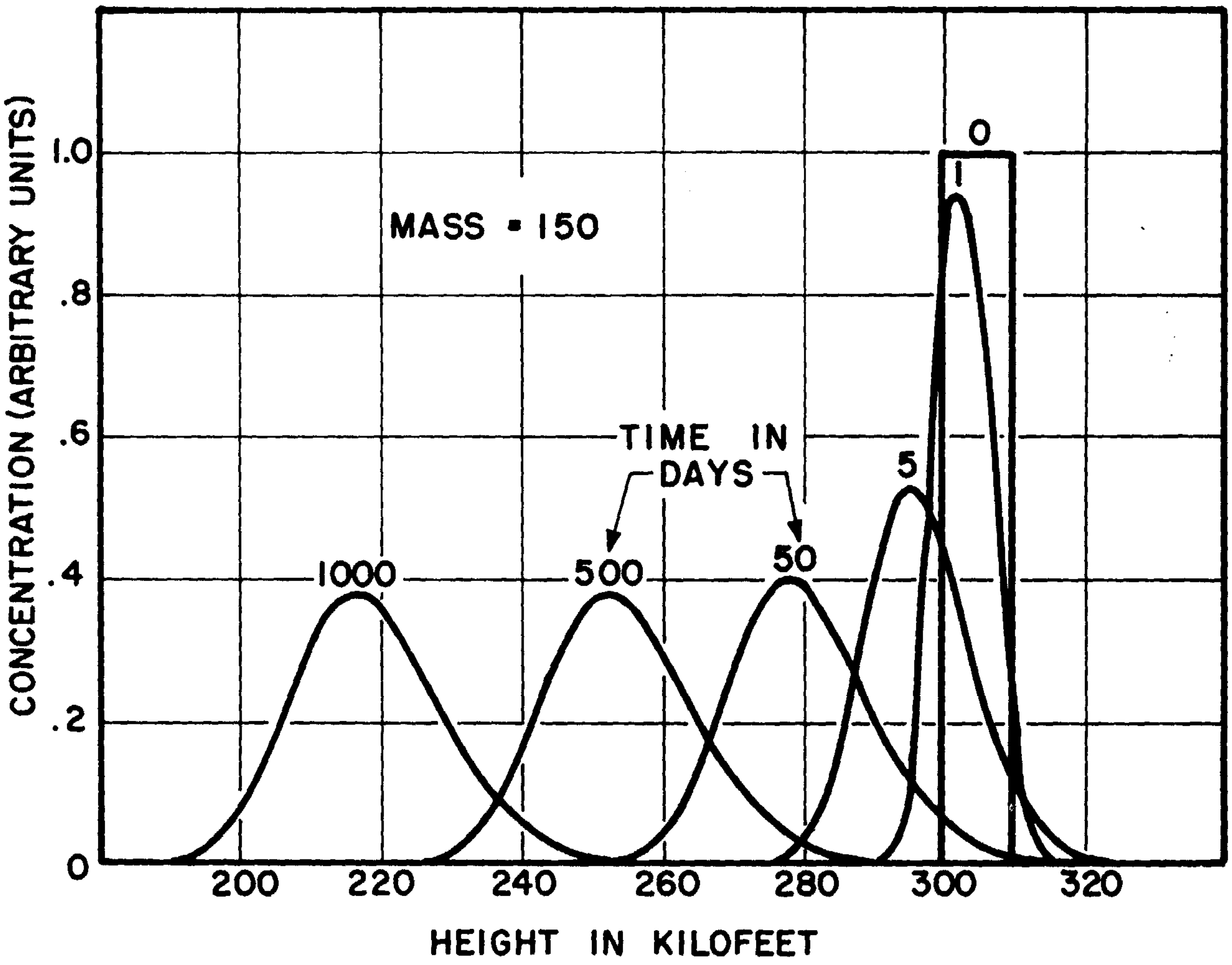

Fig. 1 -- The time development of a square concentration profile injected at 305 kilofeet for particles of mass 150. The number above each concentration profile is the time in days for that profile. Input conditions for this calculation were: $M = 150$ atomic units, $m = 28.97$ atomic units, $A = 2.15 \times 10^{-8}$ cm, $a = 1.70 \times 10^{-8}$ cm, $\rho = 1.72 \times 10^{-5}$ gm/cm^3 at 100 kilofeet, and $T = 245$°K.

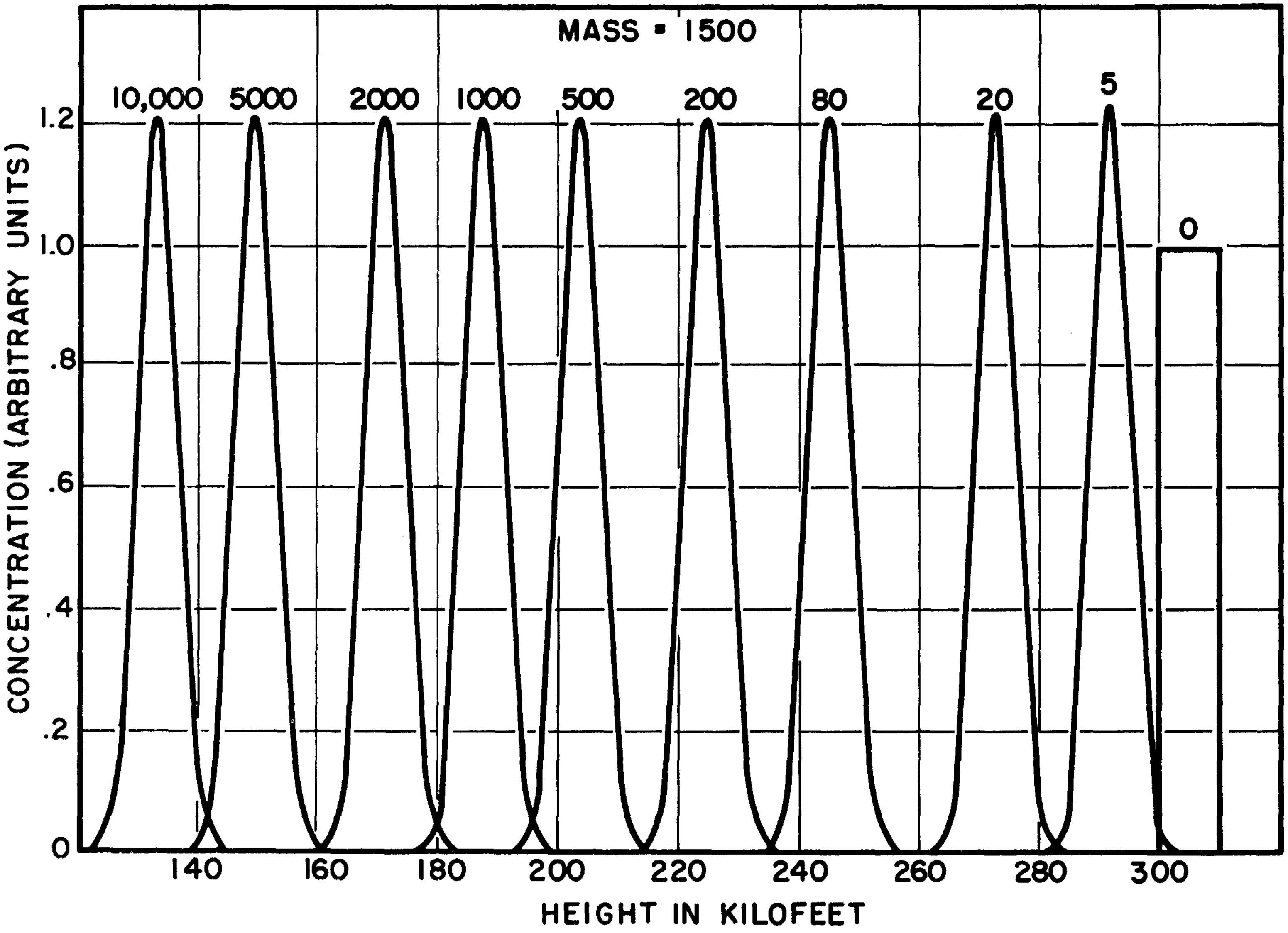

Fig. 2 -- The time development of a square concentration profile injected at 305 kilofeet for particles of mass 1500. The number above each concentration profile is the time in days for that profile. Input conditions for this calculation were: M = 1500 atomic units, m = 28.97 atomic units, A = 4.63 x 10^{-8} cm, a = 1.70 x 10^{-8} cm, ρ = 1.72 x 10^{-5} gm/cm^3 at 100 kilofeet, and T = 245°K.

But the fall rate of the maximum point is governed only by the gravitational term of the flow rate equation, since $\frac{\partial c}{\partial x} = 0$ for the maximum point.

From the considerations of the previous paragraph, it is also evident that the fall rate of the constant profile may be analytically computed, since, from equation (1), the flow rate at the maximum point is

$$\Gamma = -\left(\frac{K_1}{\rho}\right) c = v_{max}\, c \,. \tag{8}$$

In this equation, v_{max} denotes the instantaneous velocity of the maximum point and is given by

$$v_{max} = \frac{dx}{dt} = -\left(\frac{K_1}{\rho}\right) = \frac{K_1}{\rho_o \exp\left(-\frac{x}{\lambda}\right)} \,, \tag{9}$$

where ρ_o is the density of the atmosphere at $x = 0$ (i.e., sea level) and $\lambda = \frac{kT}{mg}$ is the e-fold distance for average air particles. Integrating (9) gives

$$t = \frac{\rho_o \lambda}{K_1}\left[\exp\left(-\frac{x}{\lambda}\right) - \exp\left(-\frac{x_s}{\lambda}\right)\right] = \frac{\rho_s \lambda}{K_1}\left[\exp\left(\frac{x_s - x}{\lambda}\right) - 1\right] , \tag{10}$$

where x_s is the position of the maximum point at $t = 0$ and ρ_s is the density of the atmosphere at x_s. Equation (10) expresses how long it will take the constant profile to fall from x_s to x. Or from (10),

$$x = x_s - \lambda \ \ln\left[\left(\frac{K_1}{\rho_s \lambda}\right) t + 1\right] , \tag{11}$$

which expresses where the profile will be at time t after starting at x_s for time zero.

Equations (10) and (11) govern the fall rate of the constant profile, and diffusion has no effect on the fall rate of the constant profile. The only effect of diffusion is to keep the profile constant. In fact, if one injects any profile of particles of mass greater than 100 atomic units at high altitudes and uses equation (11) to calculate the position of later profiles, an error of only a few kilofeet is made. Semiquantitatively, this means that one does not have to do numerical calculations in order to determine the positions of later profiles for impurity profiles injected into our model at high altitudes. That is, one makes a relatively small error by completely neglecting diffusion in calculating the fall rate of impurity particles injected into our model of the upper atmosphere. To give a feeling for the fall rate of various particle masses, we have used equation (11) to calculate the positions of later profiles for particles injected into our model at 305 kilofeet. These results are plotted in Figures 3 and 4.

It should be remembered that all of the discussion so far has been for a stagnant atmosphere. It would be desirable to take turbulence into account in such calculations. Unfortunately, turbulence is difficult to describe mathematically. However, we have considered a very simple model of layer turbulence and will proceed to show that such a simple model of turbulence does not appreciably affect the profile fall rate. We assume a layer of the atmosphere which is b kilofeet deep, with b being much greater than the profile width of the impurity

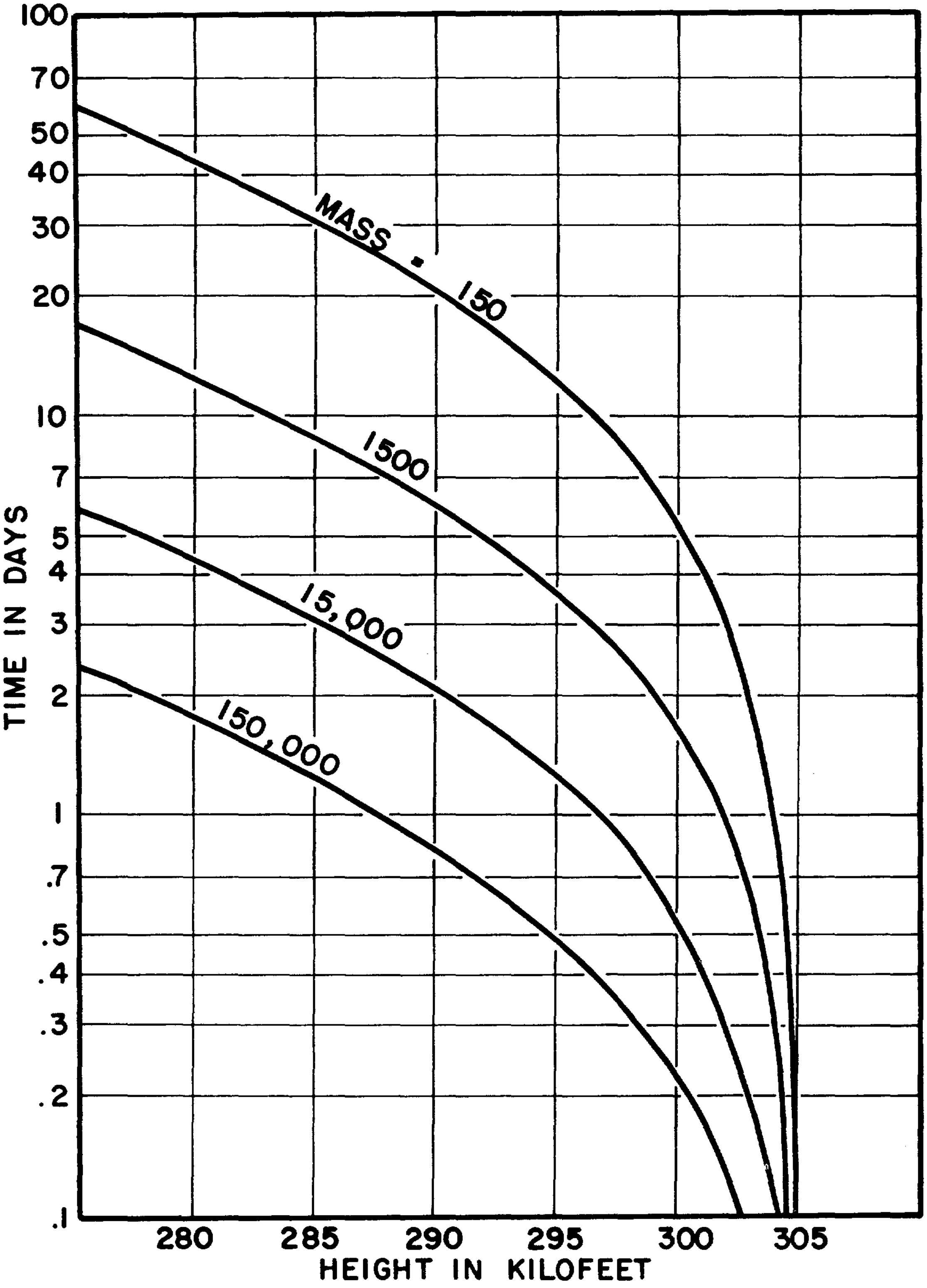

Fig. 3 -- Time versus height for particles injected at 305 kilofeet. These curves were computed using equation (11). The number above each curve is the mass of the impurity particles in atomic units and the density of the impurity particles was assumed to be 6.4 gm/cm^3. Other input conditions were m = 28.97 atomic units, a = 1.70 x 10^{-8} cm, ρ = 1.72 x 10^{-5} gm/cm^3 at 100 kilofeet, and T = 245°K.

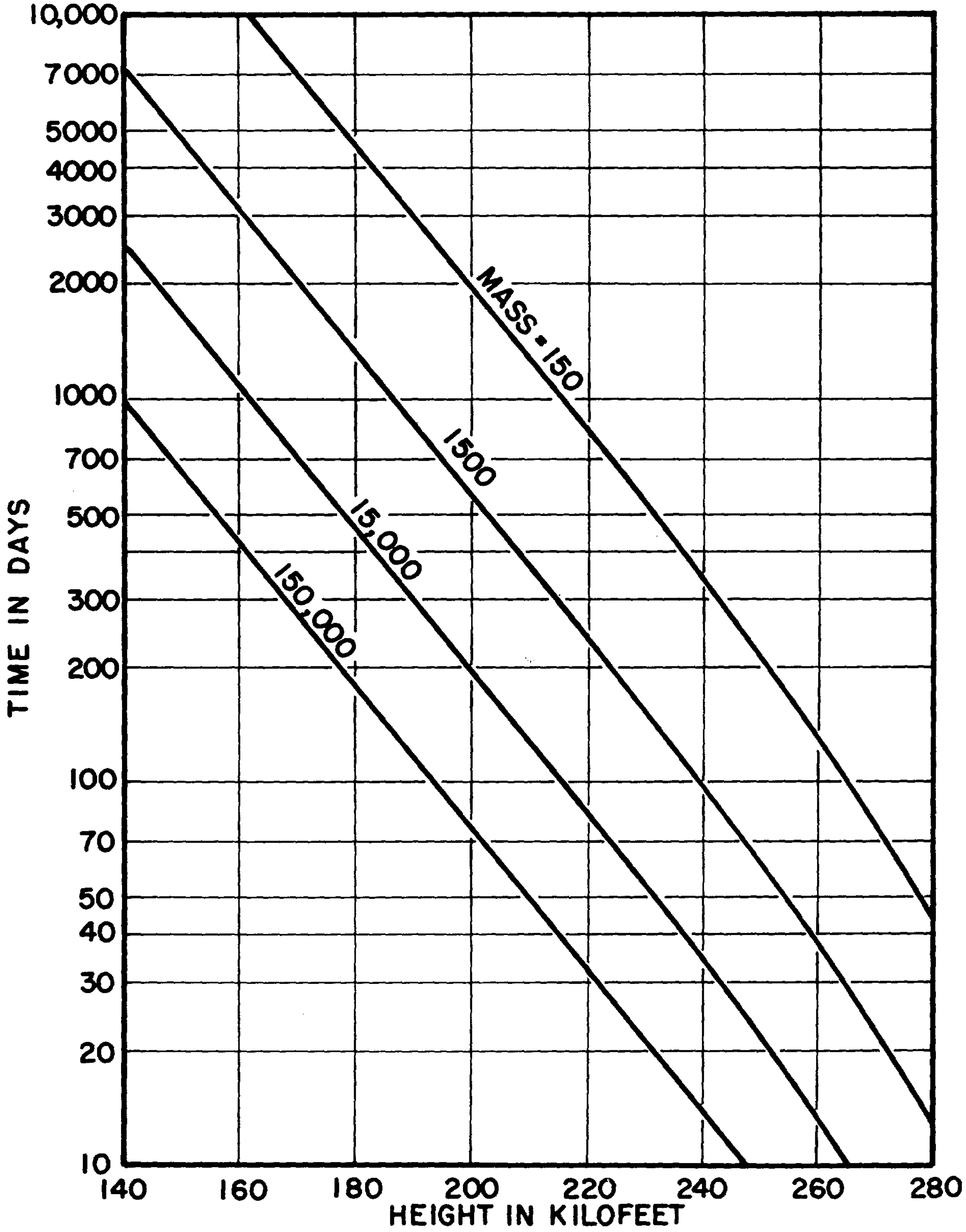

Fig. 4 -- Time versus height for particles injected at 305 kilofeet. These curves were computed using equation (11). The number above each curve is the mass of the impurity particles in atomic units and the density of the impurity particles was assumed to be 6.4 gm/cm^3. Other input conditions were m = 28.97 atomic units, a = 1.70 x 10^{-8} cm, ρ = 1.72 x 10^{-5} gm/cm^3 at 100 kilofeet, and T = 245°K.

particles. For the completely stagnant case, the time required for the profile to fall through this layer is, from equation (10),

$$\left.\begin{array}{l}\text{time to fall}\\ \text{through stagnant}\\ \text{layer}\end{array}\right\} = \frac{\lambda \rho_t}{K_1}\left[\exp\left(\frac{b}{\lambda}\right) - 1\right]$$

$$= \frac{\lambda}{v_b}\left[1 - \exp\left(-\frac{b}{\lambda}\right)\right] , \tag{12}$$

where ρ_t is the density of the atmosphere at the top of the layer and v_b is the profile fall rate at the bottom of the layer.

For the turbulent case, we assume a turbulent layer, of depth b, which is connected to stagnant atmosphere by discrete boundaries on both the top and bottom. Furthermore, the turbulence is assumed to be such that the impurity is completely mixed with the air in the layer so its concentration within the layer has the dependence

$$c = c_b \exp\left(\frac{x_b - x}{\lambda}\right) , \tag{13}$$

where λ is the e-fold distance for air particles, x_b is the height of the bottom of the layer, and c_b is the concentration of the impurity at x_b. Since the layer is deep compared to the constant profile width of the impurity, most of the material will enter the layer before much leaves. Then the total amount of material in the layer, τ, at a given time, is

$$\tau = \int_{x_b}^{x_b+b} c_b \exp\left(\frac{x_b - x}{\lambda}\right) dx = \lambda c_b \left[1 - \exp\left(-\frac{b}{\lambda}\right)\right] . \tag{14}$$

However, the rate of loss of the impurity is $c_b v_b$ and must equal $-\frac{d\tau}{dt}$, which gives

$$\frac{dc_b}{c_b} = -\frac{dt}{\frac{\lambda}{v_b}[1 - \exp(-b/\lambda)]} .$$

Integrating gives

$$c_b = c_b(0) \exp\left\{-\frac{t}{\frac{\lambda}{v_b}[1 - \exp(-b/\lambda)]}\right\} , \tag{15}$$

with $c_b(0)$ being the concentration of the impurity at x_b at $t = 0$. Thus, it is seen from (15) that the time for c_b to decrease to 1/e - th of its original value is $(\lambda/v_b)[1 - \exp(-b/\lambda)]$. So, by comparing this value with equation (12), qualitatively speaking, the time required for the impurity to fall through the turbulent layer is approximately the same as for a stagnant layer.

In summary, the authors feel the main value of the present work shows that diffusion has little effect on the vertical transport of particles of mass greater than 100 atomic units injected into the upper atmosphere. Furthermore, however crude our remarks have been concerning turbulence, we feel they will be of some value for future calculations concerning turbulence when more knowledge becomes available concerning the exact nature of turbulence in the upper atmosphere.

II. METEOROLOGICAL FORCES

L. B. Smith
Presiding

(A)
Atmospheric Traces Above 100,000 Feet

L. Machta* and R. J. List
U.S. Weather Bureau

Introduction

The design of an effective and useful high-altitude sampling system for probing the atmosphere at levels from 100,000 to 200,000 feet is dependent upon a knowledge of the constituents to be sampled and upon reasonable estimates of the concentrations to be expected. The constituents to be sampled may be radioactive or stable.

Radioactive Isotopes

A summary of the radioisotopes of interest to be expected from 100,000 feet and their expected concentrations at this time are given in Table 1. Certain of the isotopes are present only as the result of natural processes, cosmic ray bombardment of the atmosphere (S-35, Be-7 and P-32), or products of natural radioelements in the earth's crust (Pb-210); others are the result of artificial production in nuclear reactions (Sr-90, Cs-137 and Rh-102), while C-14, H-3 and Na-22 are present from both natural and artificial sources.

The results of some recent lower stratospheric measurements of several of these isotopes, which form the basis of the estimated concentrations, will be discussed briefly, together with some indication of what we might expect to learn from measurements at higher altitudes.

Carbon-14. C-14 has been measured in the stratosphere by balloon collection for several years. The results for April-September 1955 are shown in Figure 1. The units are 10^5 C-14 atoms per gram of air and are in terms of excess over natural background. This background value is assumed to be 73×10^5 atoms/gm. (The numbers in parentheses refer to the number of observations.) In view of the long half-life, the background can be considered constant throughout the atmosphere.

Since at the time of the collections virtually the only source of artificial C-14 was the Marshall Islands (11°N), the observations tend to support the view that contaminants move poleward and downward in the northern hemisphere stratosphere (evidence in the southern hemisphere is less convincing). The lower values over the equator at 90,000 feet, when compared with other latitudes, indicate either a rising motion in the tropical stratosphere or an initial injection at altitudes above the highest level sampled. Because of the large yield devices and uncertain cloud height information in the Castle and Ivy series, the latter possibility cannot be discounted. It may be that subsequent tests (excluding Teak and Orange) did not reach to these altitudes. It should be pointed out that not enough C-14 data is available to determine whether the poleward and downward movement is due to a circulation or to a mixing process.

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*Speaker presenting the paper.

TABLE 1

RADIOISOTOPES ABOVE 100,000 FEET

Isotope	Half-life	Minimum countable	Expected concentration (1961)		SCF needed for:		Remarks
		dpm	Maximum dpm/1000 SCF	Minimum dpm/1000 SCF	Maximum Concentration	Minimum Concentration	
C-14	5670 years	0.5	500 (10 x background)	50 (background)	1	10	CO_2 gas
Sr-90	28 years	1	500	10	2	100	Particulate
Cs-137	27 years	1	1000	20	1	50	Particulate
Pb-210	22 years	1	1	0.01	10^3	10^5	Particulate
H-3 (as H_2)	12 years	0.5	8 (10^5 T. U.)	0.08 (10^3 T. U.)	60	6000	Gas
H-3 (as H_2O)	12 years	0.5	6×10^4 (2×10^7 atoms/gm)	300 (10^5 atoms/gm	0.007	1.5	1 SCF = 6×10^{-5} to 3×10^{-3} gm Water Vapor
				30 (1.6×10^4 T. U.)		15	
Na-22	2.6 years	1	10^{-1}	10^{-4}	10^4	10^7	Particulate
Rh-102	210 days	1	1000	1	1	1000	Particulate
S-35	87 days	1	200	20	5	50	Particulate
Be-7	53 days	10	2000	200	5	50	Particulate
P-32	14 days	1	20	2	50	500	Particulate

To obtain ambient volumes at:
105,000 feet, multiply SCF by 100
160,000 feet, multiply SCF by 1,000
220,000 feet, multiply SCF by 10,000

The latest balloon stratospheric C-14 data (1960) which is available only from San Angelo, Texas, shows a distribution similar to that at San Angelo in 1955, with some evidence that the peak concentration is below 90,000 feet. However, there may be a peak due to Teak and Orange above 90,000 feet. Because of the ambiguity in possible sources, it will be necessary to follow the existing distribution of C-14 in space and time to study the role played by the atmosphere. It does have the advantage of being a gaseous tracer and can be presumed to move with the air parcels.

Strontium-90. A series of aircraft filtering flights were made in May and early June 1960 to collect radioactivity on a world-wide basis at four latitudes and at altitudes from 15,000 to over 65,000 feet. These data, supplemented by balloon collections made at San Angelo, Texas, to 90,000 feet and by preliminary results of surface air concentration measurements made by the Naval Research Laboratory, are shown in the figures. The data are expressed in d/m/1000 scf on sampling day, unless otherwise indicated. Values in italics refer to observations reported as tropospheric.

The distribution of Sr-90 is shown in Figure 2. As can be seen, the lowest stratospheric concentrations appear in equatorial regions, with somewhat higher values in the temperate and polar stratosphere. It is somewhat surprising to see that the southern hemisphere stratospheric concentrations are the same or slightly higher than the northern hemisphere values, despite the fact that almost all of the Sr-90 was injected north of the

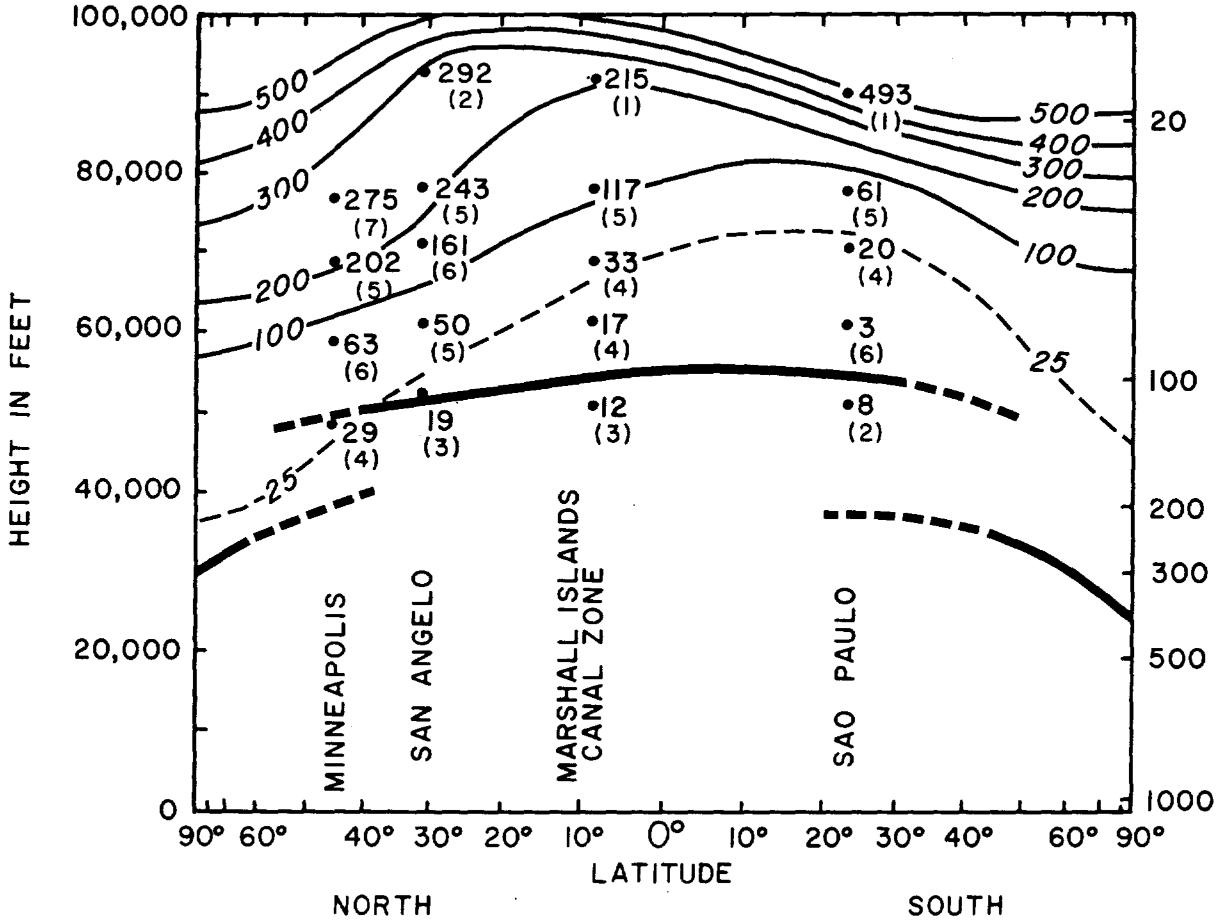

Fig. 1 -- Average concentration of carbon-14 in the stratosphere, April to September, 1955.

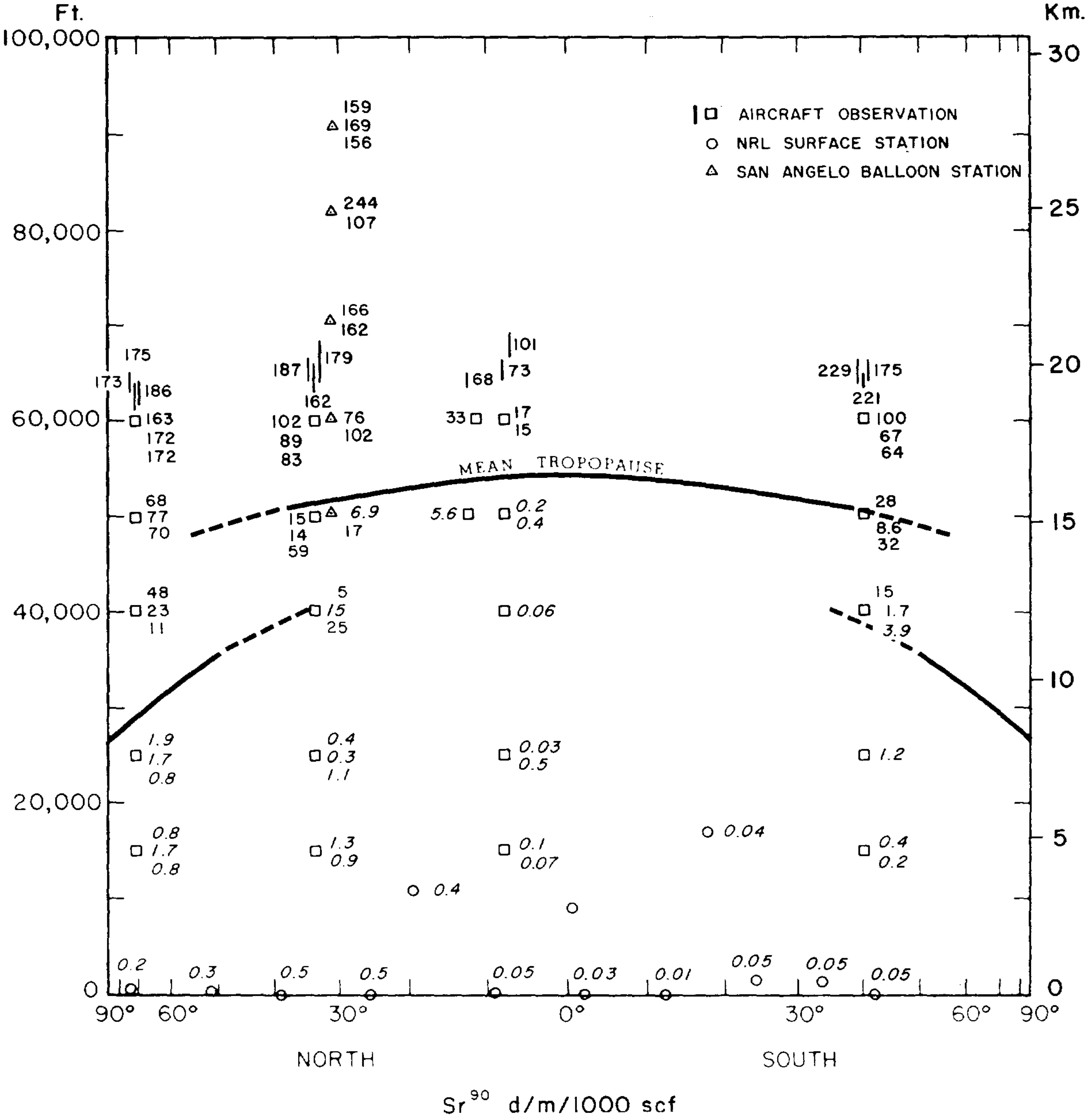

Fig. 2 -- Strontium-90 distribution

equator. The temperate latitude peak evident in the surface air in the northern hemisphere is virtually absent in the southern hemisphere despite the similarity of stratospheric concentrations. Whether this is seasonal or the result of differences between the hemispheres will become evident when results of the next series of observations, made in November 1960, becomes available.

The source of high altitude Sr-90, and its distribution above 100,000 feet are both in doubt. The peak may be at or near the top of the balloon collections as a result of injections made in the older Castle (1954) and Ivy (1952) series, which resulted in clouds extending to higher altitudes than most later tests. It is also possible that the concentrations increase above the highest altitudes sampled as a result of the Teak and Orange events.

As with C-14, the Sr-90 concentrations can provide invaluable clues to atmospheric circulations. The uncertainties inherent in using particles as atmospheric tracers are offset in part by the fact that there are no natural sources of Sr-90 and therefore no variable background.

Cesium-137. Figure 3 shows that the Cs-137 results are very similar to the Sr-90 values, as would be expected. A possible additional benefit as a tracer may result from the fact that Cs-137 is also a γ-emitter and the possibility exists that instrumentation to measure it in situ can be developed.

Lead-210. Figure 4 shows the distribution of Pb-210, a long-lived particulate daughter of 3.7-day radon emanating from the surface of the earth. Pb-210, as far as can be determined, is produced only from this source.

The variability in the Pb-210 concentration tends to be small (the two high values at 32°N in the troposphere are believed to be erroneous) when compared to the distribution of artificial radionuclides. It is seen that the equatorial stratosphere has somewhat higher values than are found elsewhere in the stratosphere. This may mean that the entry of radon (or its decay products) into the stratosphere occurs in equatorial regions, although the data as yet is too scanty and conclusions at this time would be premature. Further information on Pb-210 distribution would be valuable since this constitutes an important tracer for the movement of air from low altitudes to high.

Tritium. H-3 in the atmosphere results from three significant sources. Cosmic ray bombardment, neutron capture by nitrogen in the vicinity of thermonuclear explosions and releases from nuclear reactors. It may exist in the atmosphere as hydrogen gas or as a constituent of water vapor or other gases (e.g., methane).

No stratospheric samples of H_2 are available, but an estimate for the tritium content to be expected can be made from pre-bomb ground-level measurement. This was of the order of 1.6×10^4 tritium units (T.U.). One T.U. is defined as a concentration of 1 tritium atom in 10^{18} hydrogen atoms. The values for tritium in H_2 gas given in Table 1 are based on this pre-bomb surface air concentration and an assumption that the abundance of H_2 in the stratosphere is 0.5 parts per million. The meteorological interpretation of such tritium measurements at high altitudes will be difficult, since there is little information on natural background and the contribution due to nuclear testing is unknown; however, a knowledge of the concentration would be of interest for future research.

Tritium is present in the atmosphere as a constituent of water vapor. As with C-14, it is important to be able to differentiate the natural background. One attempt at establishing this value consists of comparing the

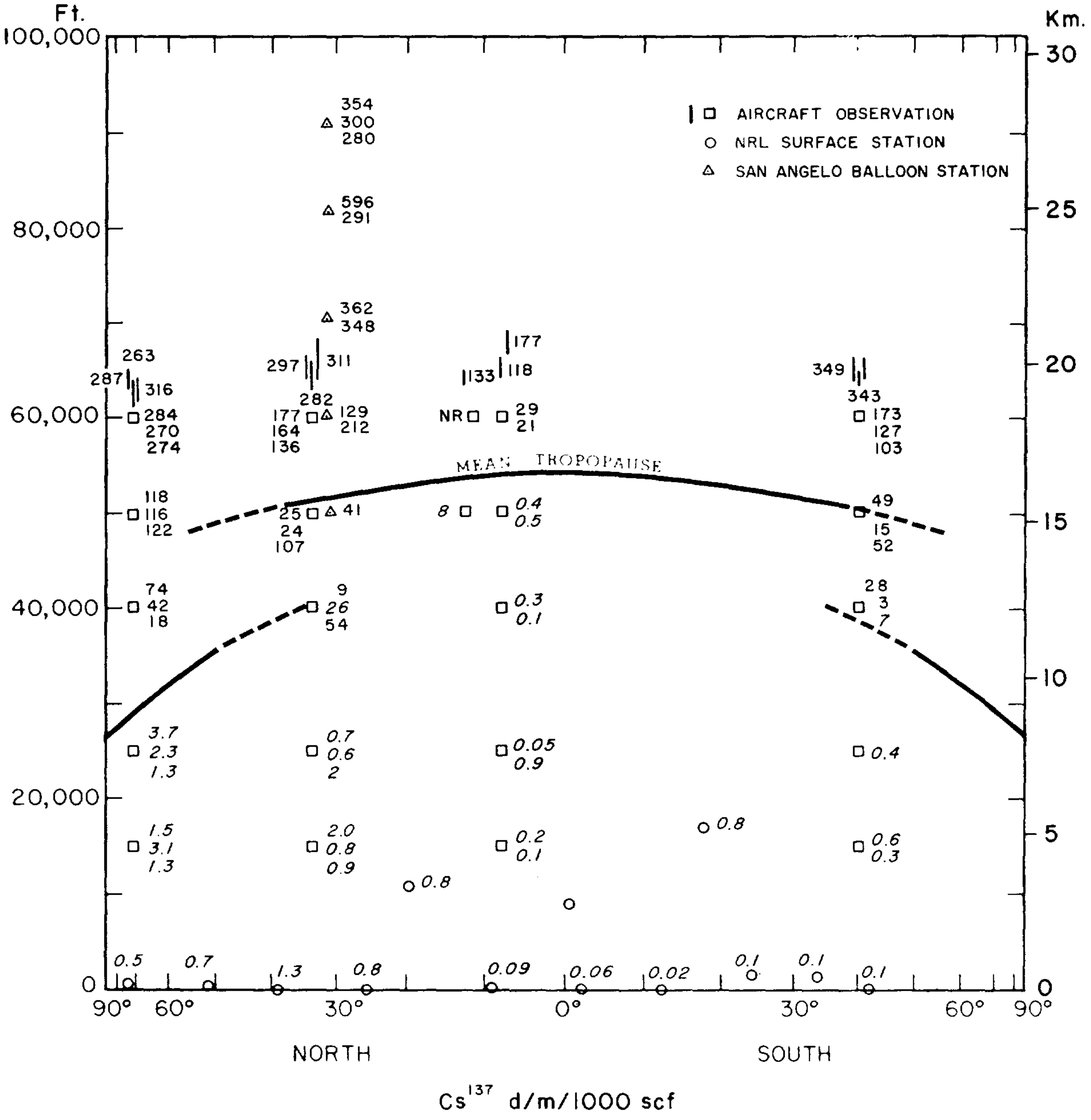

Fig. 3 -- Cesium-137 distribution

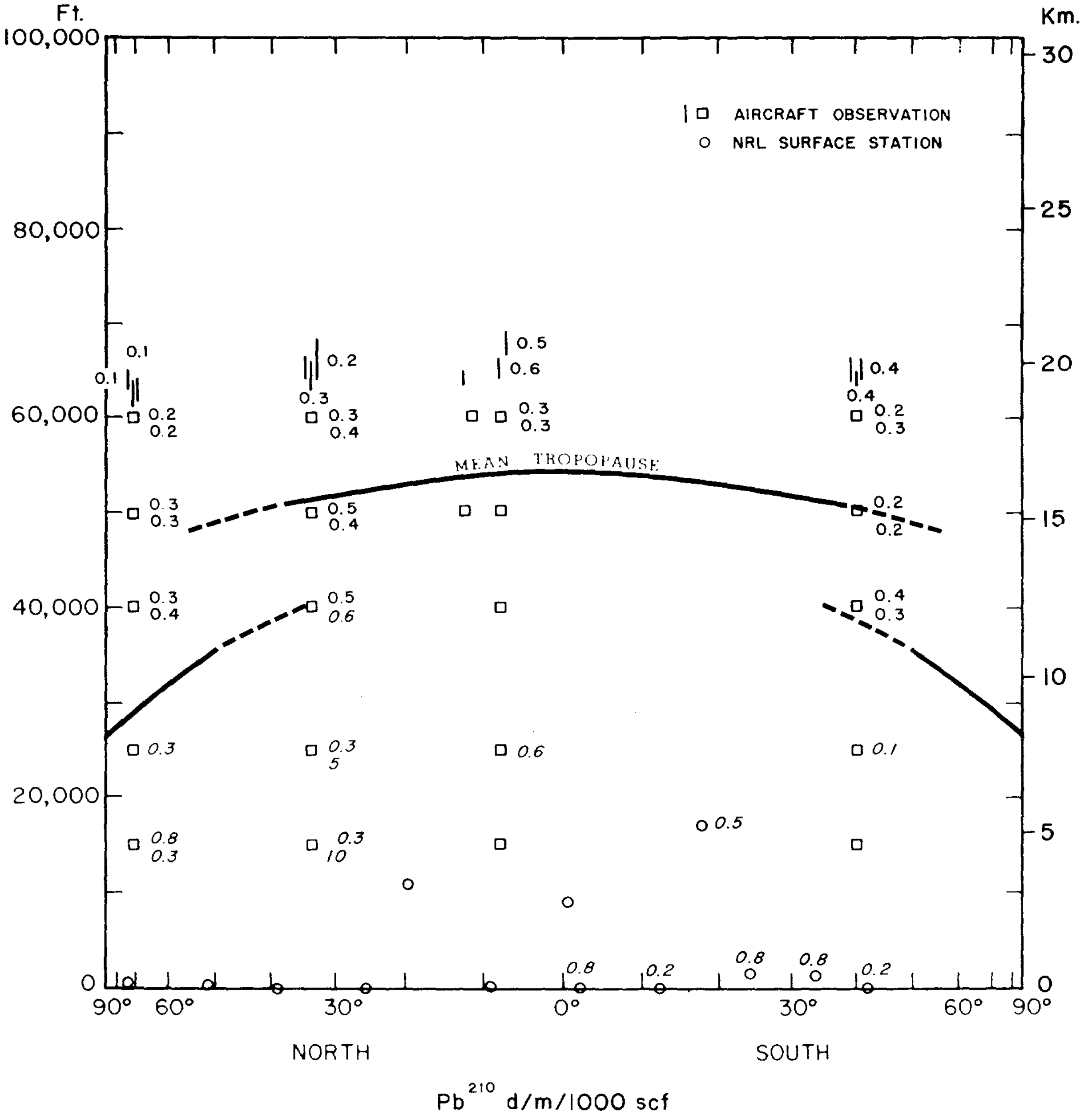

Fig. 4 -- Lead-210 distribution

C-14 excess with the tritium values (Figure 5). If the observations at 50,000 feet are extrapolated to a zero C-14 excess, 7×10^5 atoms of tritium per gram of air are present. This value formed the basis of the estimates of expected concentrations (10^5 to 2×10^7 atoms/gm of air). An alternate minimum concentration estimate can be made assuming the 1.6×10^4 T.U. found in pre-bomb surface hydrogen gas is in equilibrium with the water vapor in the stratosphere. Such concentrations would be an order of magnitude smaller.

The remarks concerning the utility of C-14 measurements also apply to tritium, although in the latter case uncertainties in background values are much larger.

Rhodium-102. An important tracer experiment was conducted in connection with the Orange shot in August 1958. Three megacuries of Rh-102 were injected by a megaton device detonated at over 100,000 feet. The resulting cloud rose to much higher altitudes. About 0.3 megacuries were produced in other experiments, so that over 90 percent of the Rh-102 in the atmosphere is of very high altitude origin.

Figure 6 shows the distribution of this tracer in May 1960. These data clearly show the non-uniform distribution in the lower stratosphere. At about 65,000 feet the polar and temperate zones of both hemispheres show about an order of magnitude higher concentration than the equatorial region. Although the injection occurred at 17°N, the debris seems to be partitioned equally in the lower stratosphere in both hemispheres.

It should be pointed out that a difficulty, which is now being resolved, exists in interpreting the Rh-102 data. There is a long-lived isomer, as well as the 210-day isomer, so that Rh-102 follows a complex decay scheme. All data reported here has been corrected from counting day to August 12, 1958, assuming a 210-day half-life. (This value has likewise been used to estimate current concentrations.) Therefore, although absolute values may be in error, the relative concentrations shown in Figure 6 are valid.

This has been a unique experiment and still holds great promise of providing a useful high-altitude tracer. A maximum effort in the next few years should be directed to collecting and interpreting the rhodium data.

Other Cosmic-Ray Isotopes. Several shorter-lived radioisotopes are produced by cosmic ray interactions in the atmosphere and may be useful in studying stratospheric models. Data for Na-22, S-35, Be-7 and P-32 are included in Table 1. The first, Na-22 may still be present from artificial sources. The other isotopes are so short-lived that only naturally produced quantities are now present in the atmosphere.

The most fruitful way of utilizing data on these isotopes is by determining the expected equilibrium concentration of the isotope in a quiescent atmosphere and examining the deviations from equilibrium in the actual atmosphere. The production by cosmic rays is a function of geomagnetic latitude and altitude. Several investigators have computed the equilibrium concentrations to be expected in the absence of transport or diffusion.

As an example of this type of analysis, Figure 7 shows a comparison of the observed Be-7 data with the computed equilibrium concentrations. (The latter are shown symmetrically about geographic latitude as an approximation to geomagnetic latitude.) As can be seen, the tropospheric concentrations are well below the equilibrium values, showing evidence of the rapid tropospheric removal process. Stratospheric concentrations are much closer to equilibrium values, with the ratio of observed to equilibrium values approaching one at the highest altitudes. The stratospheric departures from equilibrium can be the result, among other things, of mixing with the troposphere or of a poleward circulation bringing in Be-7 poor air from the tropics.

[redacted]

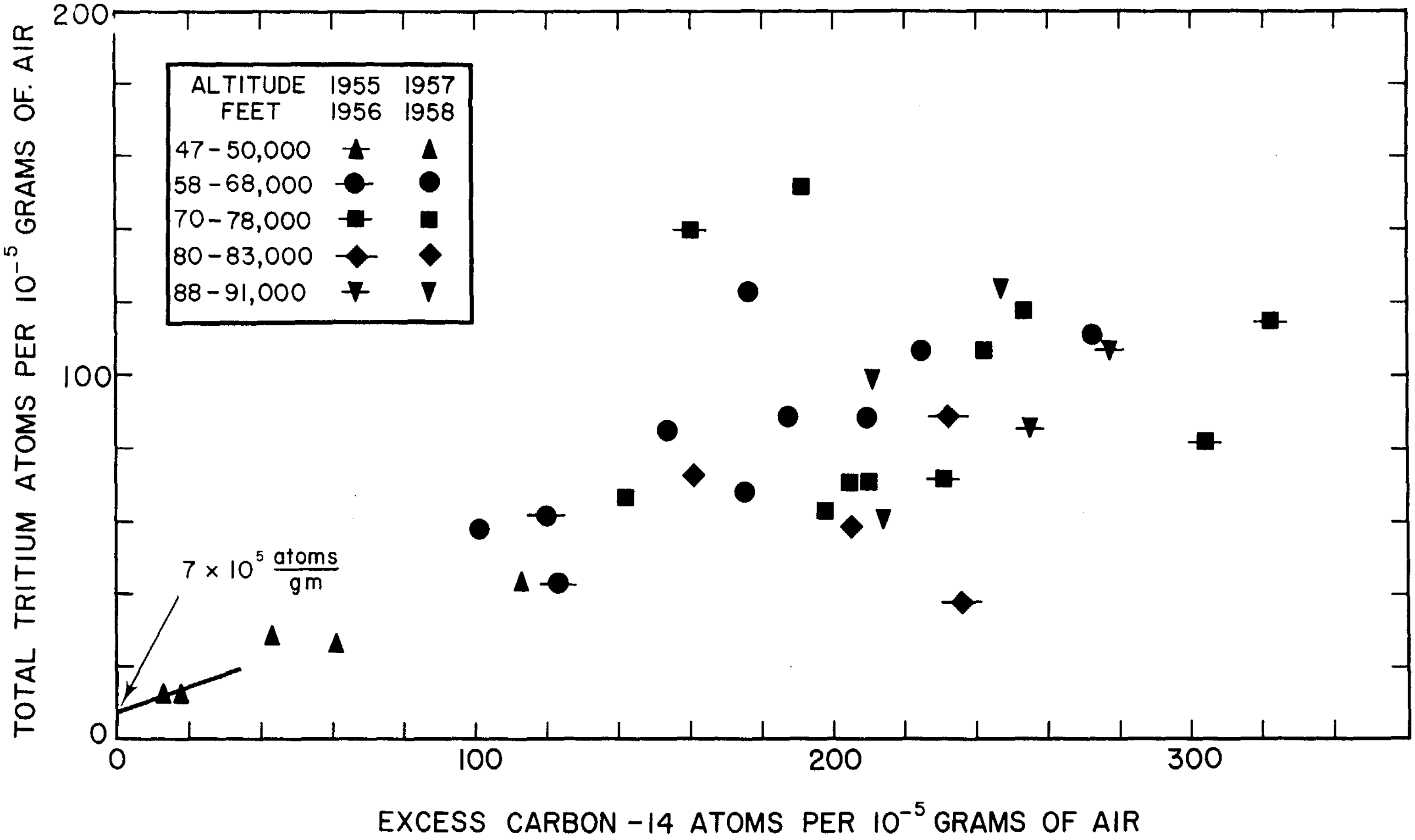

Fig. 5 -- Excess C-14 versus total tritium

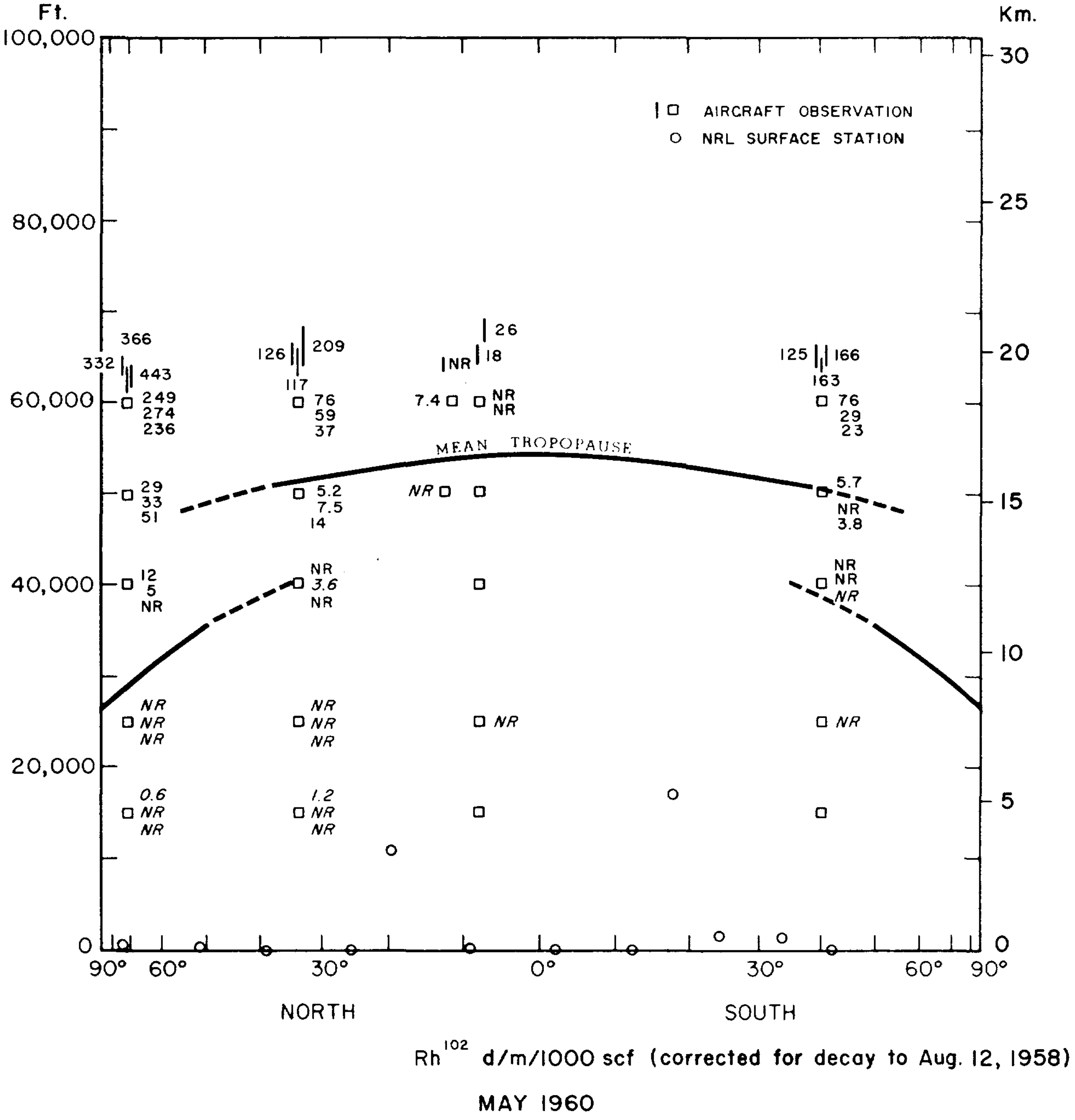

Fig. 6 -- Rhodium-102 distribution

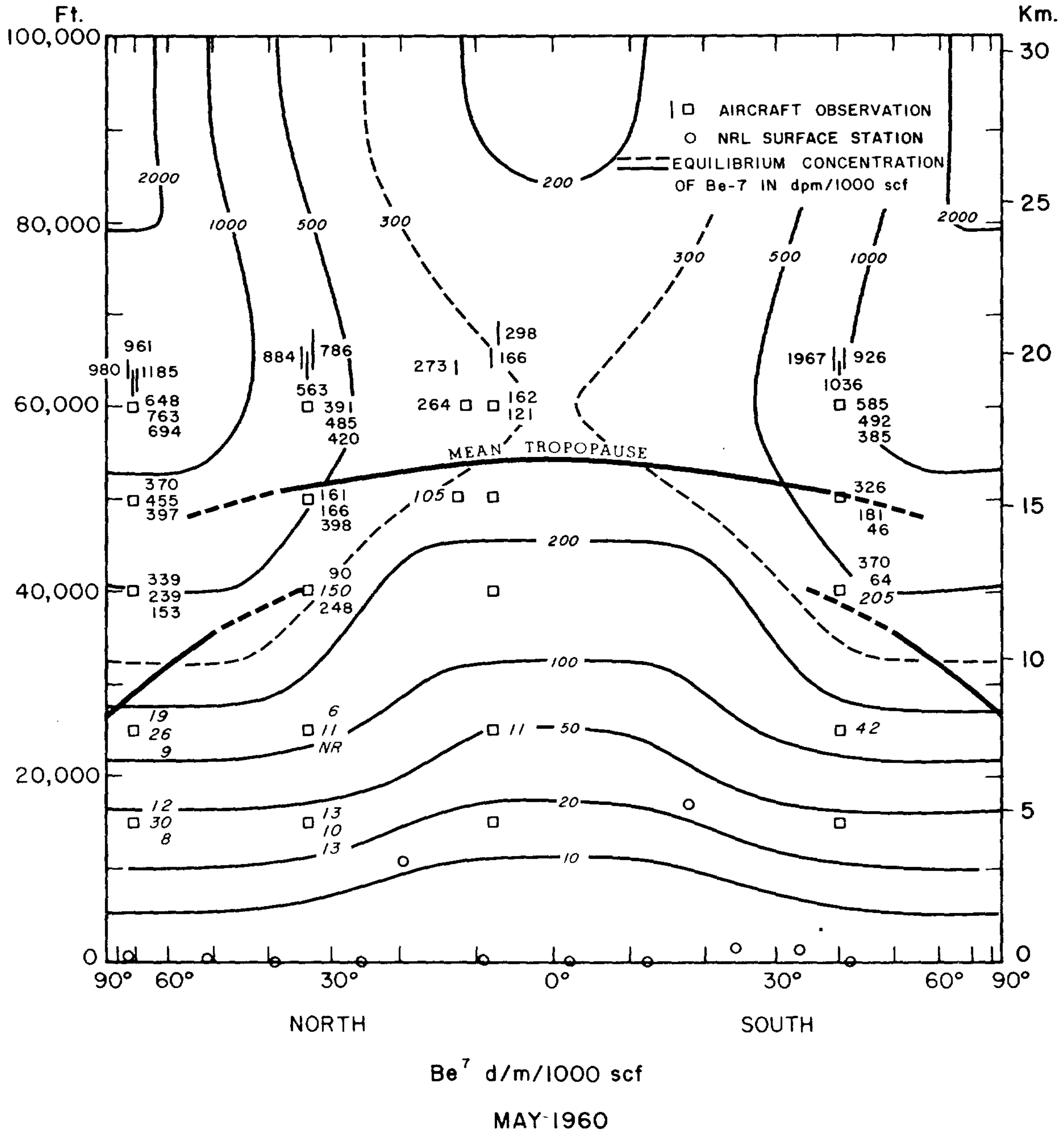

Fig. 7 -- Comparison of observed beryllium-7 data with computed equilibrium concentrations.

Since the equilibrium concentration lines are essentially vertical at 100,000 feet, the range of concentration at this altitude was used to estimate the expected concentrations shown in Table 1. It should be pointed out that studies of departure from equilibrium require that observations be made with greater precision than when examining the movement of tracers. Overall errors of collection and counting would have to be much less than 10 percent for meaningful meteorological interpretation.

Stable Elements and Compounds

Sampling of stable elements and compounds above 100,000 feet can also be of great importance in providing information about the atmosphere. Table 2 lists a few of the substances of interest.

TABLE 2

STABLE ELEMENTS AND COMPOUNDS ABOVE 100,000 FEET

Substance	Amount needed	Expected values	SCF required
CO_2	10^{-5} cc at NTP	300 ppm	0.03
H_2O	10^{-5} gm H_2O	10^{-3} gm/kg - 1 gm/kg	300 - 0.3
0-16/0-18	10^{-3} gm H_2O		3×10^4 - 30
H-2/H-1	10^{-3} gm H_2O		3×10^4 - 30

Carbon Dioxide. Carbon dioxide abundance measurements are of particular interest. The addition of CO_2 to the lower atmosphere resulting from our increasing industrialization may be reflected in a gradient of CO_2 in the upper atmosphere. Attempts to measure this gradient have been unsuccessful. Figure 8 shows the results of measurements made from balloon samples as several altitudes and latitudes (different symbols). The horizontal lines show limits of uncertainty in individual observations. It is evident that no conclusions concerning the gradient of CO_2 abundance between 40,000 and 100,000 feet can be drawn and that more precise techniques are needed.

A great advantage of having CO_2 abundance data in future samples would be in providing an accurate estimate of the volume of air sampled.

Water Vapor. There is a tremendous interest in the water vapor content of the high atmosphere (Figure 9). The British meteorological research flights (M. R. F.) have clearly shown the persistence of a very dry layer between 45,000 and 50,000 feet from the latitude of Britain to the tropics. This was explained by the very cold upper tropospheric temperatures in the tropics wringing the moisture from ascending currents. Measurements at much higher altitudes by freeze-out traps on balloons (Barclay) and by spectroscopic observation giving mean values through thick layers (vertical lines) all indicate an increase in the water vapor mixing ratio with altitude above about 50,000 feet. The source of this water is at present a meteorological mystery.

Stable Isotope Ratios. There has been considerable recent work on relating the ratios of stable oxygen (0-16/0-18) and hydrogen (H-2/H-1) to the sources of moisture and the conditions surrounding the evaporation and condensation cycles experienced. Data on these ratios may be of value in unravelling the mysteries of the high atmosphere.

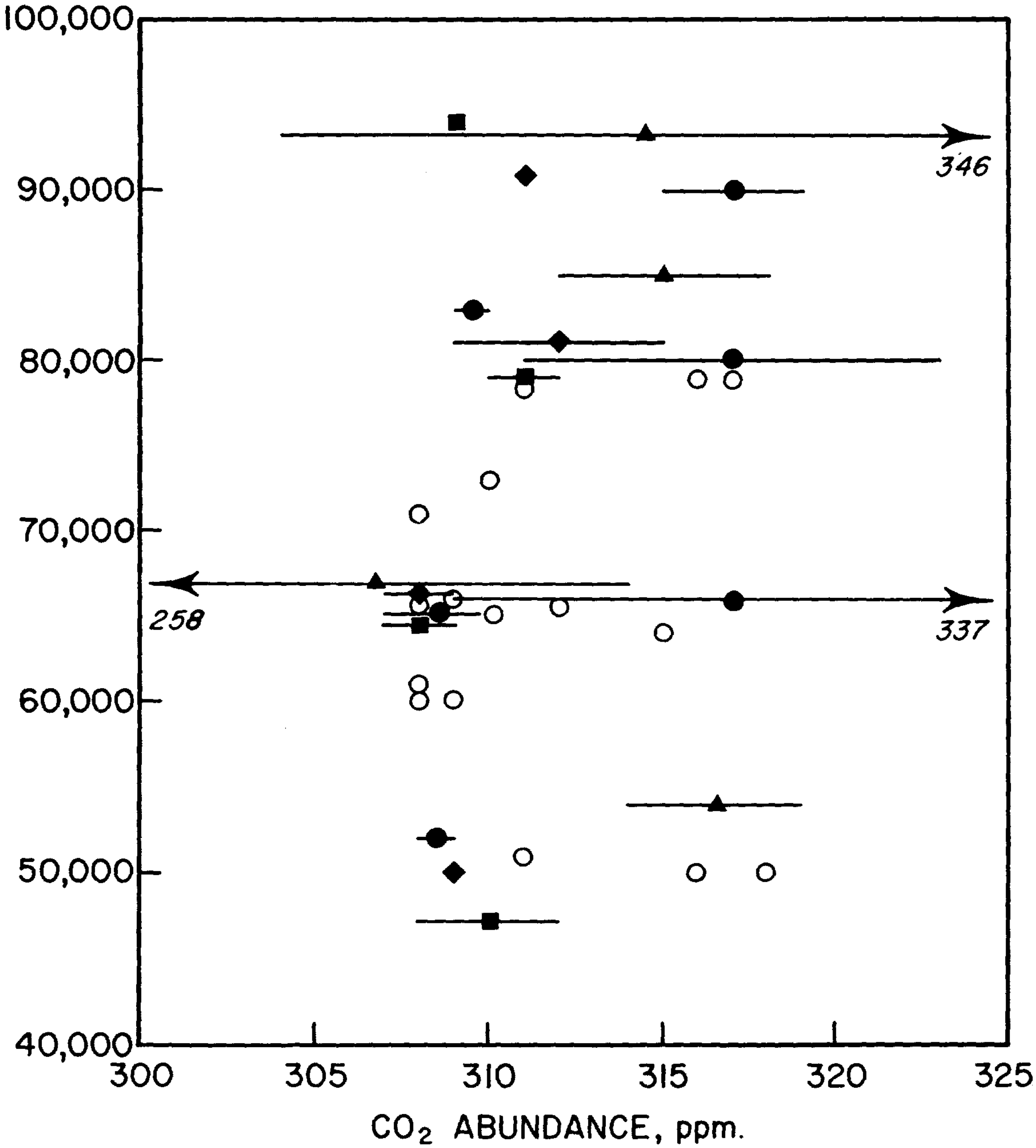

Fig. 8 -- Carbon dioxide gradient measurements attempted in the upper atmosphere

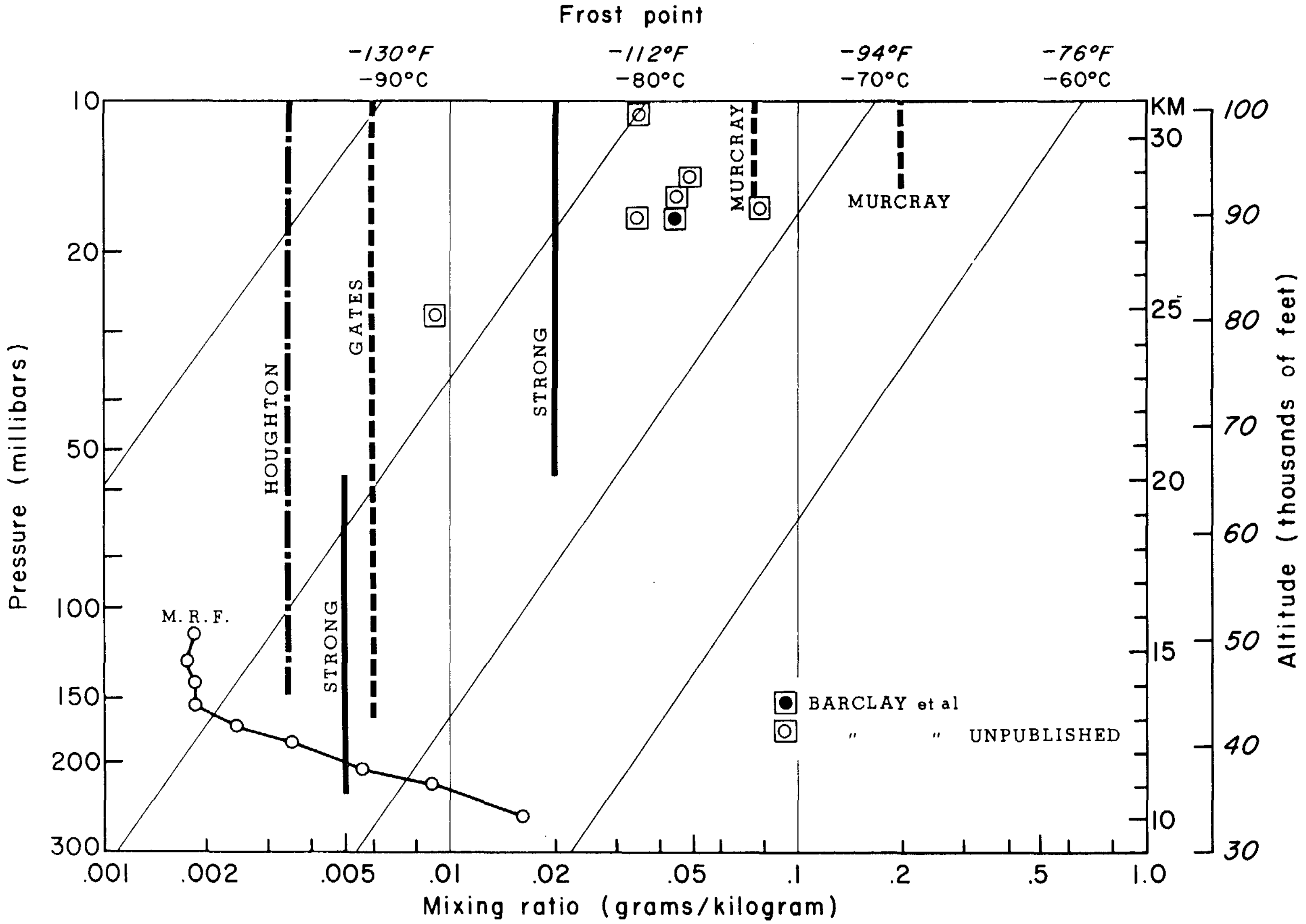

Fig. 9 -- Water vapor content of the high atmosphere

Conclusions

The design of a sampling program must be consistent with the objectives and must take into consideration the expected concentration of the substances to be measured. A device suitable for some measurements may be inadequate for others.

(B)

Effects of Large Diurnal Density Changes on Outer Atmosphere Circulation

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Introduction

Recent investigations[1, 2] of upper-atmosphere density as derived from satellite orbital period data have shown diurnal density changes ranging from a few percent at 200 km to orders of magnitude above 700 km. To a rough degree of approximation, the density-versus-time curve can be pictured as a cosine function periodicity of 24 hours, with peak amplitude at 1400 hours true local time (Figure 1).

A postulated cause for such a variation is the significant absorptional heating by solar ultraviolet radiation down to the F2 layer. It is further noted that absorption creates an integrated effect upward such that the amplitude of density variation increases with altitude. Such large density and accompanying pressure and temperature changes must have a profound effect on the motion of a neutral parcel of air (wind). Therefore, it is interesting to construct a simplified model in a coordinate system fixed to the earth and to perform some first approximation calculations on resulting winds caused by this fast-moving (500 meters per second) pressure system.

Model

From the density cosine function pictured in Figure 1, it is possible to construct a horizontal pressure field map using corresponding pressure computed by Kallmann-Bijl[2] for the 660-km level (Vanguard I perigee) on a mercator projection with time or longitude as abscissa (Figure 2).

The simplest case to consider is that of a point fixed relative to earth near the equator coming under the influence of the pictured pressure system as the system moves with the sun at about 500 meters per second from east to west. An air parcel initially at rest at 300 E longitude is in a region of maximum pressure gradient (0800 hours) which decreases to zero at 1400 hours (local real time), at which time wind will be a maximum from east. The acceleration vector reverses direction, accelerates wind from the west, reversing its direction at 2000 hours until, at 0200 hours, when the low-pressure center is reached, it returns to zero acceleration.

If the region of interest is displaced away from equator toward pole, e.g., to 20 N latitude, then the north-south component of pressure gradient must be considered. From Figure 2 it can be seen that the north wind component is a maximum at 0800 hours, decreases to zero at 1400 hours, is maximum southerly at 2000 hours, and returns to zero at 0200 hours. The resulting wind field is shown in Figure 3.

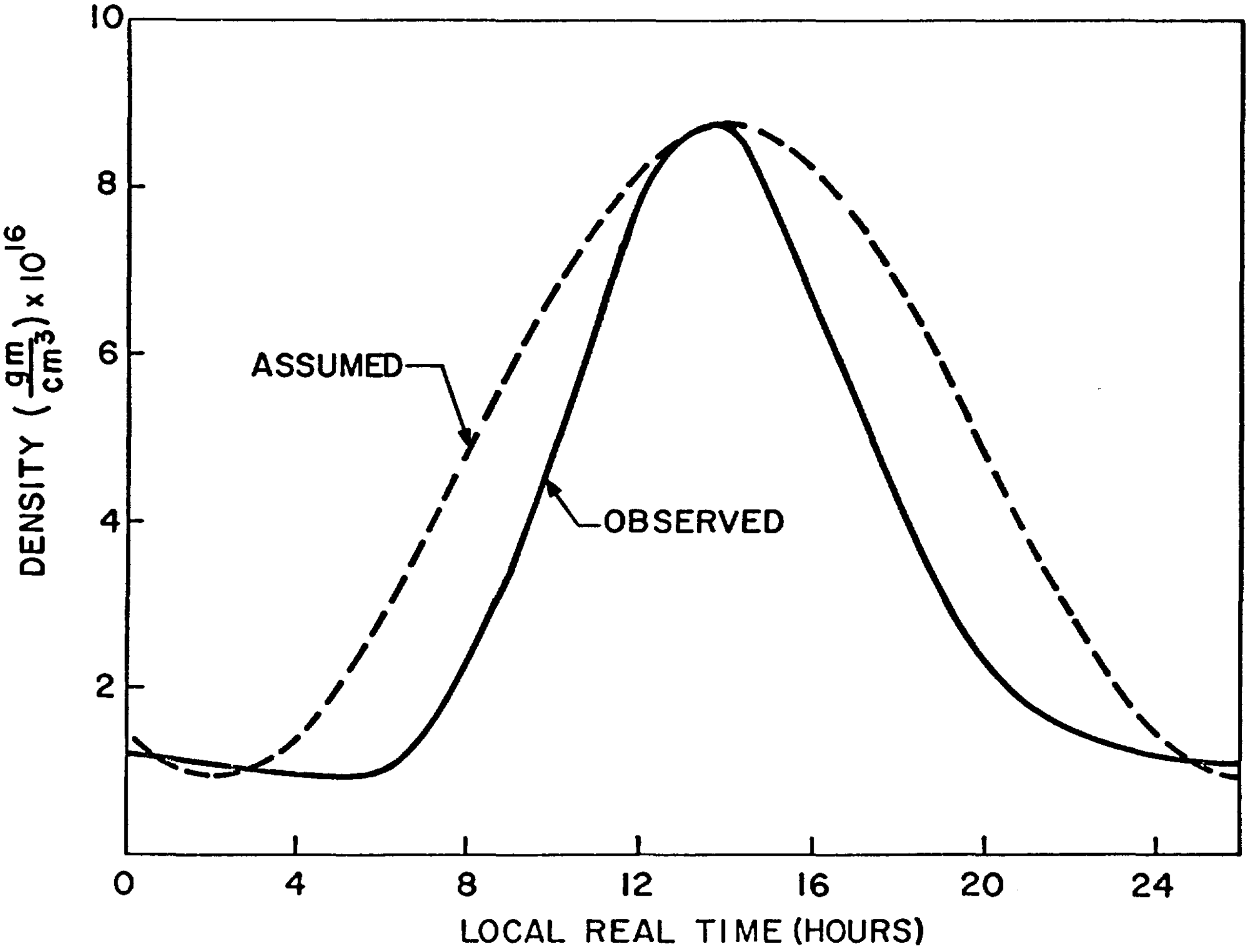

Fig. 1 -- Observed and assumed density at 660 km

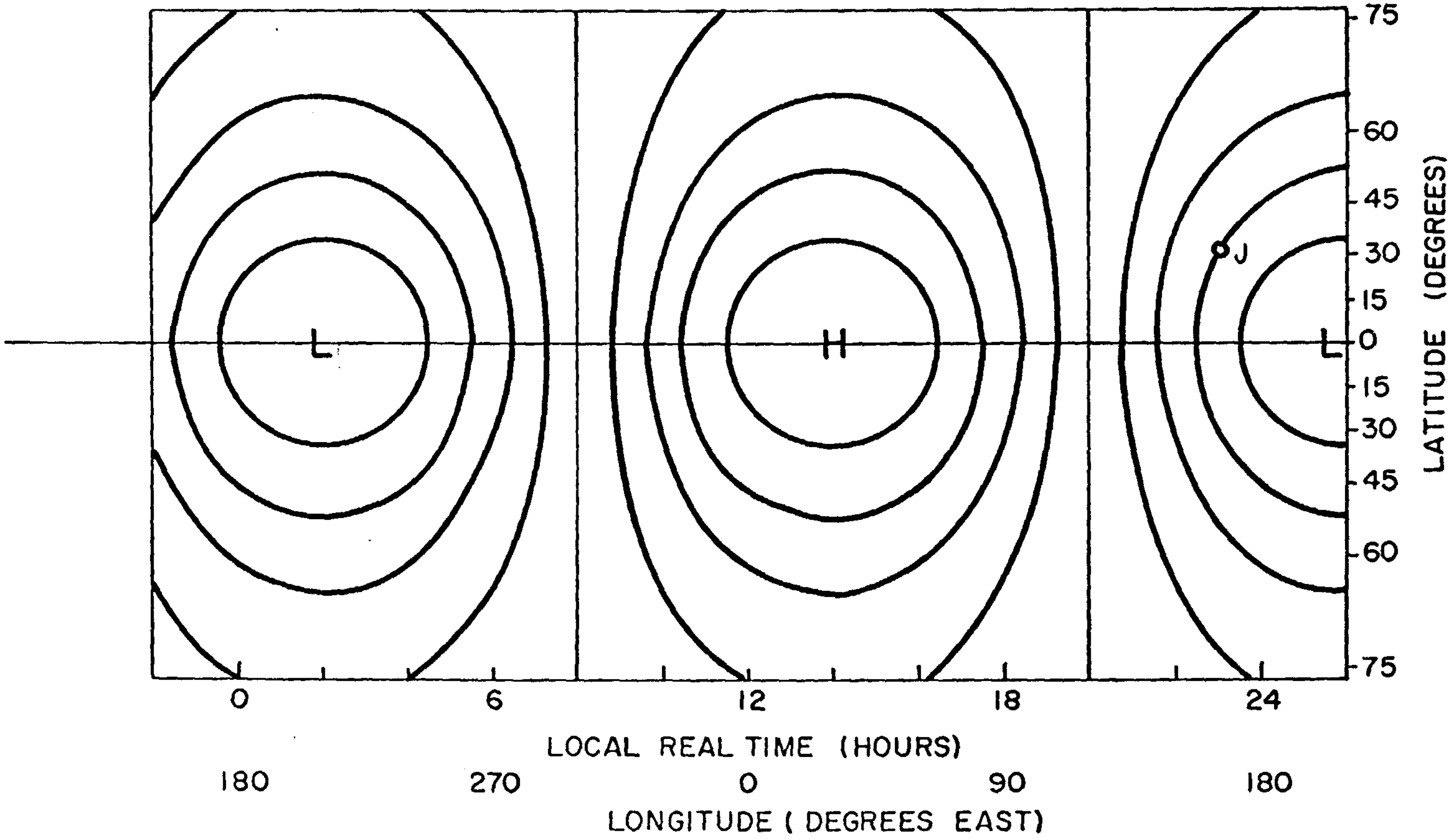

Fig. 2 -- Horizontal pressure field at 660-km altitude

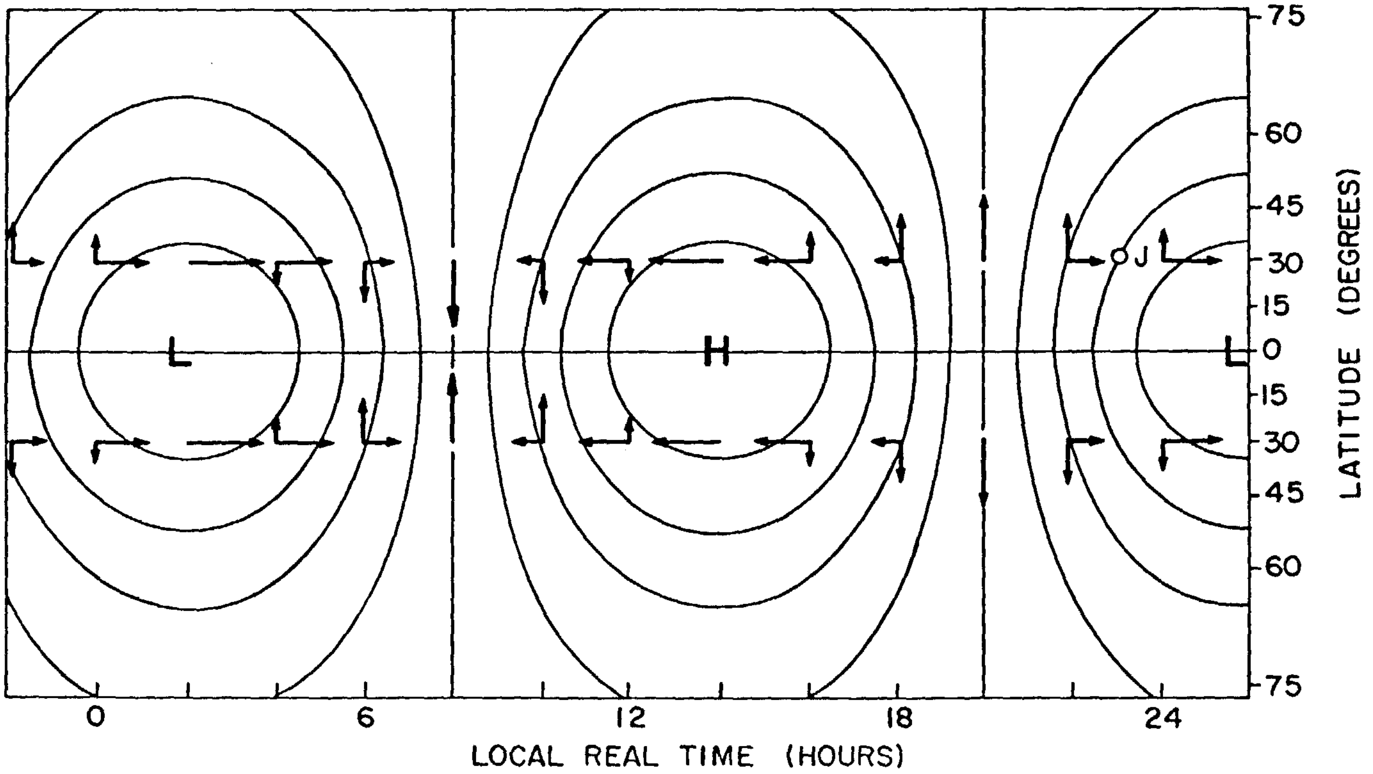

Fig. 3 -- Horizontal component velocity at 660-km altitude

Calculations

With the above qualitative discussion in mind, some simple numerical values will be considered. The equation of motion for an ideal fluid of density ρ, horizontal velocity V, in a frame of reference fixed to earth is

$$\vec{V} = -\frac{1}{\rho}\nabla p + 2\Omega \sin \phi(\vec{V} \times \vec{k}) , \tag{1}$$

where ∇p is horizontal pressure ascendent, Ω is earth's angular speed, ϕ is latitude, and $\vec{k}$ is local unit vertical vector. At the equator the last term vanishes and the problem becomes one-dimensional along the equator (x direction). Thus equation (1) becomes

$$\frac{du}{dt} = -\frac{1}{\rho}\frac{\partial p}{\partial x} . \tag{2}$$

By finding expressions for p and ρ as functions of t, and holding x fixed, the goal of computing u will be realized.

An expression for the variation of pressure as a function of time and distance along the equator can be written as

$$p - p_o = (\Delta p)_{max} \cos\left(\frac{x - x_o}{a} + 2\pi \frac{t - t_o}{b}\right) , \tag{3}$$

where p_o is mean pressure, $(\Delta p)_{max}$ is peak pressure difference, a is geocentric radius to level of interest, $x - x_o$ is in πa units east along the equator, b is time for one period (24 hours = 8.64×10^4 sec). Since pressure and density change in phase with each other, a similar expression results for density:

$$\rho = \rho_o + (\Delta\rho)_{max} \cos\left(\frac{x - x_o}{a} + 2\pi \frac{t - t_o}{b}\right) . \tag{4}$$

Integration of equation (2) after substitution from (3) and (4) yields

$$u = \frac{1}{a}\int_{t_1}^{t} \frac{(\Delta p)_{max} \sin\left(\frac{x - x_o}{a} + 2\pi \frac{t - t_o}{b}\right)}{\rho_o + (\Delta p)_{max} \cos\left(\frac{x - x_o}{a} + 2\pi \frac{t - t_o}{b}\right)} dt . \tag{5}$$

Evaluating the right-hand side of (5) with boundary conditions that u = 0 at $x = x_1$, $t = t_1$, where

$p - p_o = \rho - \rho_o =$ zero:

$$u_{max} = \frac{b(\Delta p)_{max}}{2\pi a(\Delta\rho)_{max}} \ln\left(\frac{\rho_o}{\rho_o + (\Delta p)_{max}}\right) . \tag{6}$$

Now (6) can be evaluated with use of some typical numbers

$b = 8.64 \times 10^4$ sec ,

$a = 7.0 \times 10^8$ cm ,

$\Delta p = 3.85 \times 10^{-6}$ μb (dynes/cm^2) ,

$\Delta\rho = 3.90 \times 10^{-16}$ gm/cm^3 ,

$\rho_o = 4.70 \times 10^{-16}$ gm/cm^3 , which yield u = 1.35×10^5 cm/sec = 4900 km per hour.

Recalling the earlier flow description and using the above calculated equatorial extreme, a reasonable picture of winds for any time near the equator may be deduced. To avoid arriving at invalid conclusions, emphasis must be placed on the several simplifying assumptions used. Probably the most serious assumption is that of ideal fluid flow, ideal implying an inviscid fluid and a continuum concept. Estimates of mean free path and collision frequency at 660 km altitude are 100 km and 1 in 100 seconds, respectively.[3] Disregard of electric or magnetic forces is probably another serious shortcoming. Characterizing the diurnal bulge with a sine function instead of the observed sharply defined peak (Figure 1) probably would readjust the shape of the resultant velocity field (Figure 3) but would not drastically change its main features. Restricting analysis to the equator eliminates consideration of Coriolis force which becomes 4 times as large as meridional pressure gradient at about 35 degrees latitude. A small error arises also from stating that the pressure field is symmetric about the equator instead of about the ecliptic which is inclined 23-1/2 degrees to the equatorial plane.

In support of the reported density observations and the resulting air motion just computed, there is a qualitative observation from operation Hardtack I which supports the previous flow descriptions.[4] At 1050 hours, 1 August 1958, GMT, a nuclear explosive test was conducted over Johnston Island. Calculation performed from Reed's[4] observations of the behavior of the resulting debris cloud suggests a south wind (at the point marked "J" in Figure 3) of about 6700 km per hour near 1500 km altitude. This, of course, is some 2-1/2 times higher than the previously considered altitudes, but nevertheless represents the nearest thing to a direct observation yet available.

Summary

From reported diurnal density changes and a cloud drift observation, it is concluded that very large wind velocities are probable at great altitudes (600 km), and that these winds vary in a regular diurnal pattern. There yet remains the task of obtaining direct observations to verify these conclusions.

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(C)

A High Altitude Wind Observation From Nuclear Explosion "Teak"

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An article by Church[1] speculates on circulation speeds at 660 km altitude caused by diurnal density fluctuations reported by Priester, et al.[2] from satellite orbital data. There is some observational evidence, not previously publicized, for these extreme speeds.

At the high-altitude nuclear burst (Teak) experiment above Johnston Island, 1 August 1958, a cloud observation was made which seems to imply fantastic horizontal cloud motion and altitude. Few systematic photographic or optical measurements of Teak phenomena were made. Electronic measurements did not support the cloud motion interpretation. Now, however, with several bits of rough data assembled, a reasonable assertion can be made.

From the U. S. S. Boxer, about 90 km northeast of Johnston Island, the familiar toroidal atomic cloud from Teak appeared to rise rapidly from the burst vicinity, separating from the feathery glowing region which was photographed for several minutes from Hawaii by Steiger and Matsushita.[3] After a few minutes, only fragments of this high debris cloud could be discerned. At H + 20 minutes, during observation of the auroral phenomena produced by the shot, a wisp of white cirrus was noted about 20 degrees above the northern horizon, and just to the right of the auroral belt which extended out of sight into the north. This first wisp gradually grew and soon came to appear as remains of the Teak cloud. To a vivid imagination it still seemed to retain the toroidal shape. Thus, the debris cloud must have risen and drifted north into the sunlight to become visible again. Rough calculations showed that sunlighting on this bearing would have occurred 1500 miles away from surface zero at hundreds of miles altitude in 20 minutes. The subject was dropped.

With reports of radical diurnal density and allied circulation patterns at high altitudes, it is reasonable to attempt more accurate calculations. There are still no direct measurements, but a combination of subjective arguments shows that a large error in the conclusion is unlikely.

A Teak configuration was sketched by H. E. Bell of Sandia Laboratory as he viewed it from French Frigate Shoals. Pertinent portions of his view at H + 2 minutes are shown in Figure 1. An approximate scale for his sketch was obtained from the red sphere diameter at H + 2 minutes as photographed by Hawaii-based cameras. His line of sight on the auroral base is established by construction in Figure 2, since the belt was directed toward magnetic north, 14 degrees east of true north. Local magnetic dip, 25 degrees[4], establishes the inclination of the belt. From the Boxer, azimuth of the belt segment, 20 degrees above horizontal, is triangulated in Figure 3 to give cirrus cloud bearing 2 degrees east of north. If any of the observations are varied by as much as 10 degrees, the resultant cloud bearing is changed a comparably small amount.

At H + 2 minutes (1110Z), the sun was 2 degrees west of north from the Boxer, 15 degrees above the equatorial plane. A simple section through the poles in Figure 4 shows how cloud position is determined to be at 1470-km altitude and 2250-km range. This implies that an average southerly wind of 6750 kph moved the debris cloud. This motion is in general agreement with the expected direction for this global and solar position in Church's pattern, and the speed is of correct order of magnitude. The altitude is double the 2-minute cloud height from Bell's sketch but, under entirely different atmospheric conditions of low-altitude atomic tests, many ring clouds eventually reach twice their 2-minute height. Elevation angle of the cloud is the main source of error; other parameters have less effect on resulting wind speed and altitude. Even so, varying the observed elevation angle ± 10 degrees changes the inferred speed and altitude by less than 30 percent, as shown in Figure 5.

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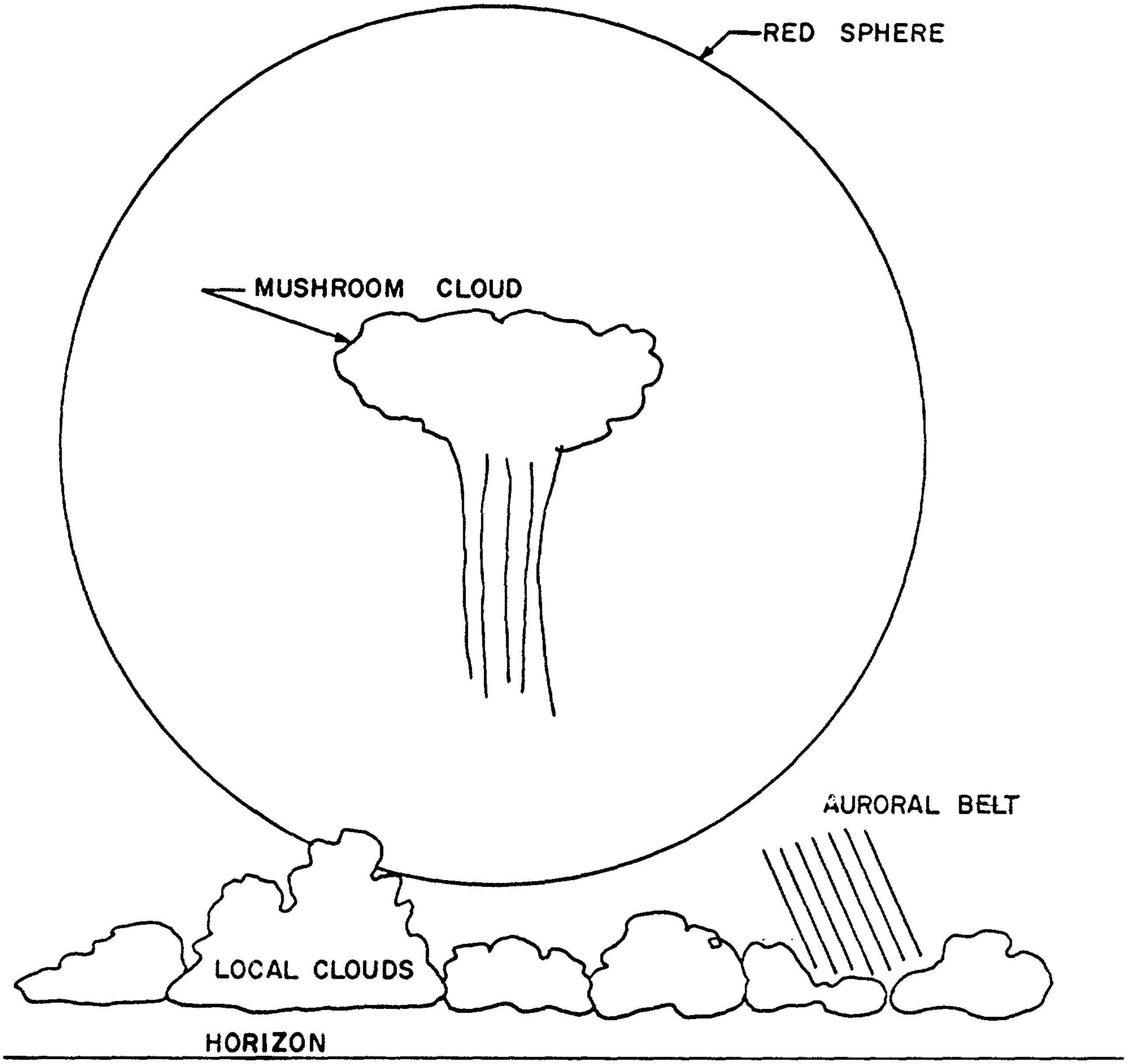

Fig. 1 -- Teak from French Frigate Shoals at H + 2 minutes (after H. E. Bell)

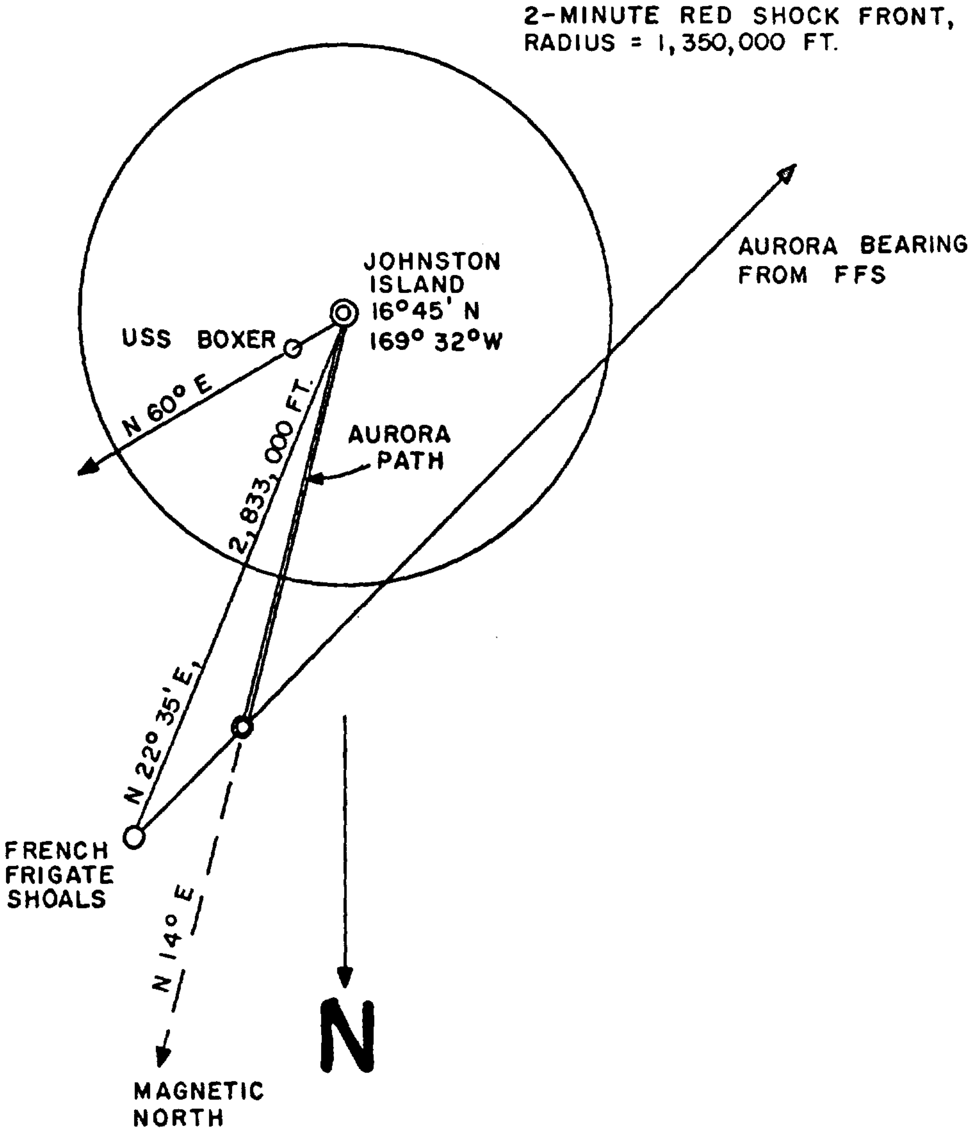

Fig. 2 -- Aurora path plan

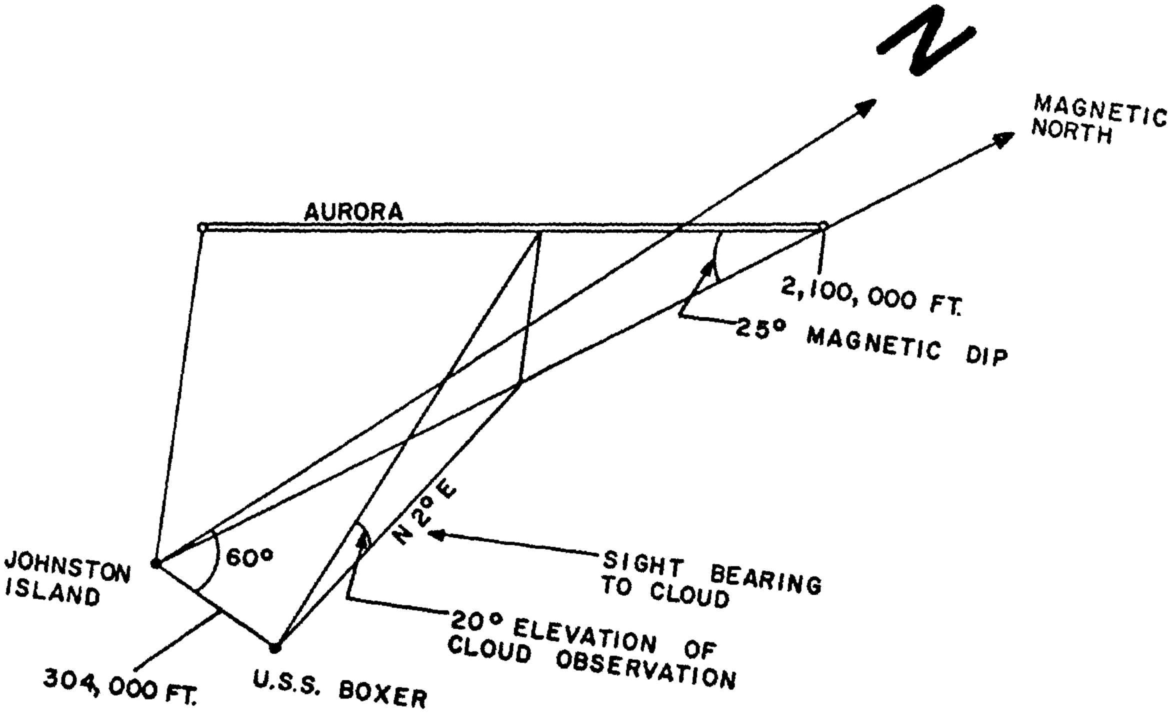

Fig. 3 -- Construction of cloud sight bearing

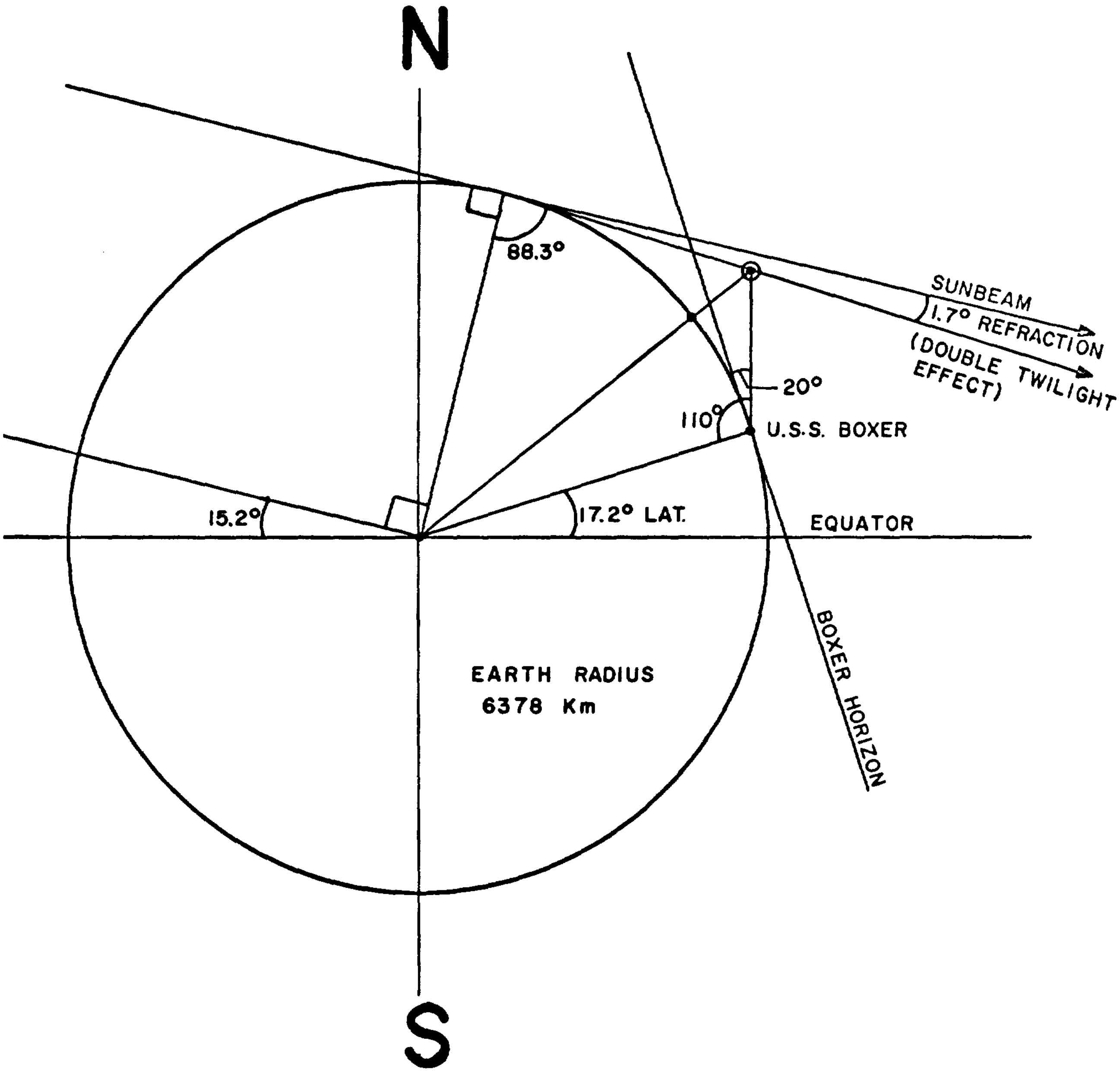

Fig. 4 -- Cloud range and altitude determination

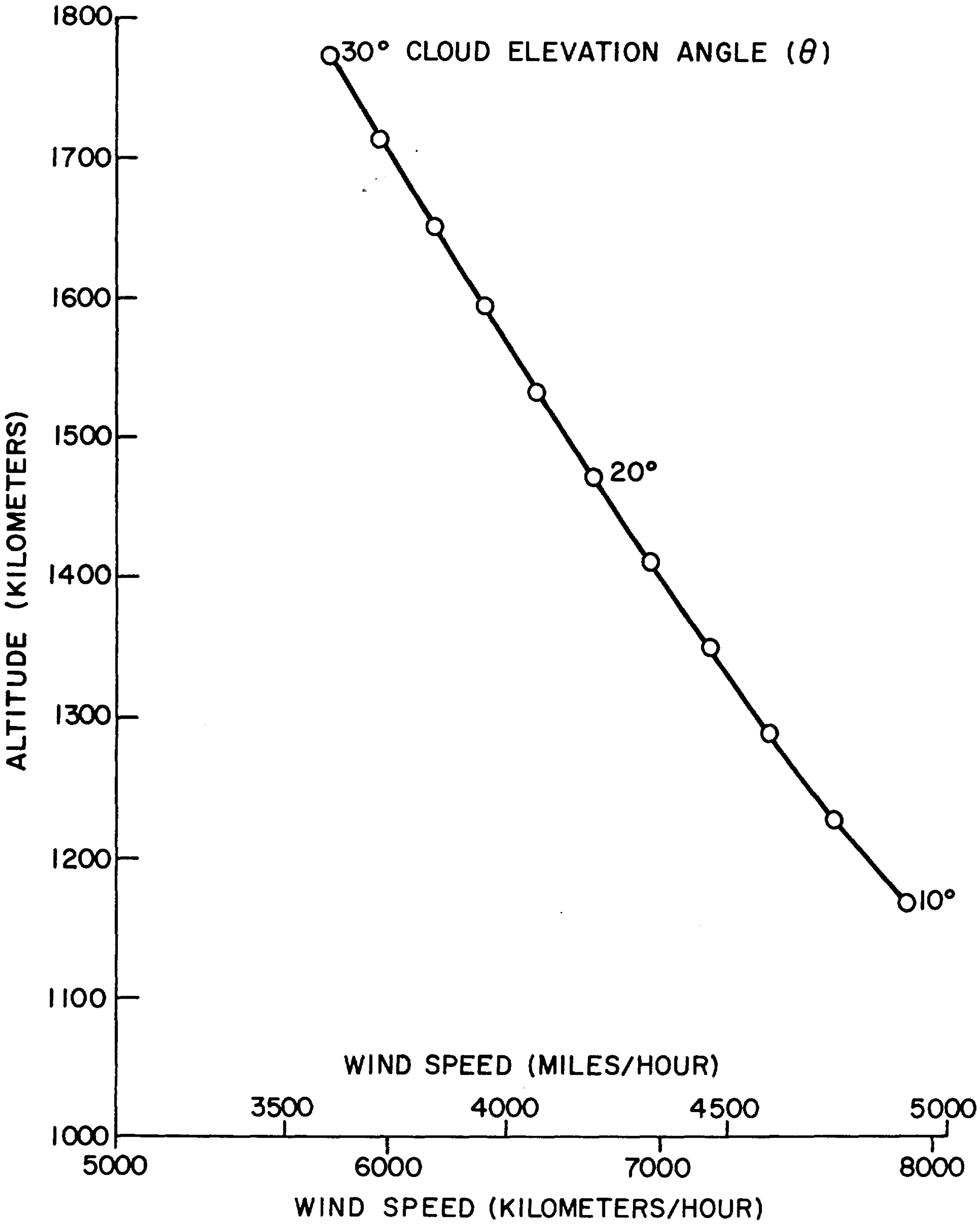

Fig. 5 -- Range of wind speed and altitude solutions

III. DEBRIS MEASUREMENTS AND INTERPRETATION

T. B. Cook
Presiding

(A)

The Behavior of Nuclear Debris from the Upper Atmosphere Rocket Detonations as Determined in the High Altitude Sampling Program

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Isotopes, Incorporated

Introduction

In the planning of a program for sampling the upper atmosphere by rockets, it is instructive to examine some of the information on the behavior of nuclear debris in the high atmosphere which has been deduced from measurements made in the lower stratosphere using U-2 aircraft. Samples of stratospheric dust collected at altitudes between 30,000 feet and 70,000 feet during early 1960 contained nuclear debris which originated from the detonation of the high altitude rocket-borne hydrogen bombs (Teak and Orange shots) in August 1958. Interpretation of the results yields some general guide lines for the design of an upper atmosphere sampling program.

The High Altitude Sampling Program has been described in detail elsewhere.[1, 2] For the present purpose it is sufficient to say that HASP involved sampling of the lower stratosphere (up to 70,000 feet) by U-2 aircraft equipped with duct samplers for collection of particulate material on fibrous filter media and impaction probe samplers for collection of particulate material on surfaces suitable for electron microscope examination.

Concentrations of various fission product and tracer nuclides in the lower stratosphere were calculated from radiochemical analyses of the filters using duct air flow rates computed from flight data and in-flight duct calibration results. Particle size-concentration distributions were calculated using electron microscopy of the impaction probe samples combined with flight data and semi-empirical aerosol impaction theory.[3] Figure 1 is a photograph of a U-2 showing the sampling ducts and the mounting strut for the impaction probe.

Detection of Teak and Orange Debris

Debris from Teak and Orange should be characterized by the presence of rhodium-102, a large quantity of which was produced by Orange. A number of HASP samples have been analyzed for this nuclide and its distribution in the stratosphere during early 1960 has been delineated. The highest concentrations were found in the northern and southern polar stratosphere and somewhat lower concentrations were found in the high tropical stratosphere. Unfortunately there exists a large uncertainty in the estimate of the total amount of rhodium-102 produced and, in addition, the radiochemical data are difficult to interpret because of the presence[4] of a long-lived (~5 year half-life) isomer of rhodium-102.

Teak and Orange debris may also be detected in the lower stratosphere through analysis of cerium-144/ strontium-90 ratios. In Figure 2 the average Ce^{144}/Sr^{90} ratios have been plotted for samples collected

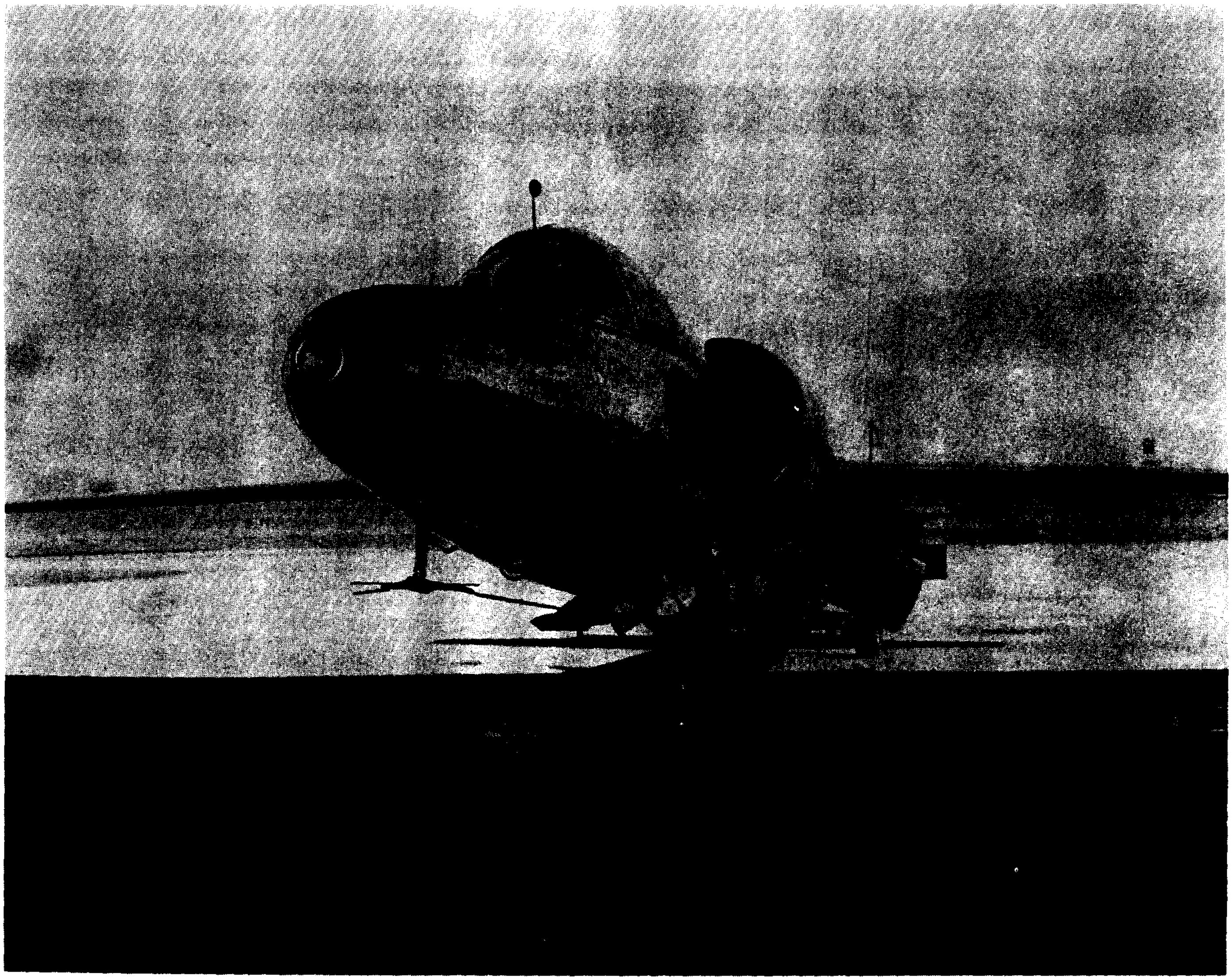

Fig. 1 -- U-2 aircraft showing sampling ducts and mounting strut for the impaction probe

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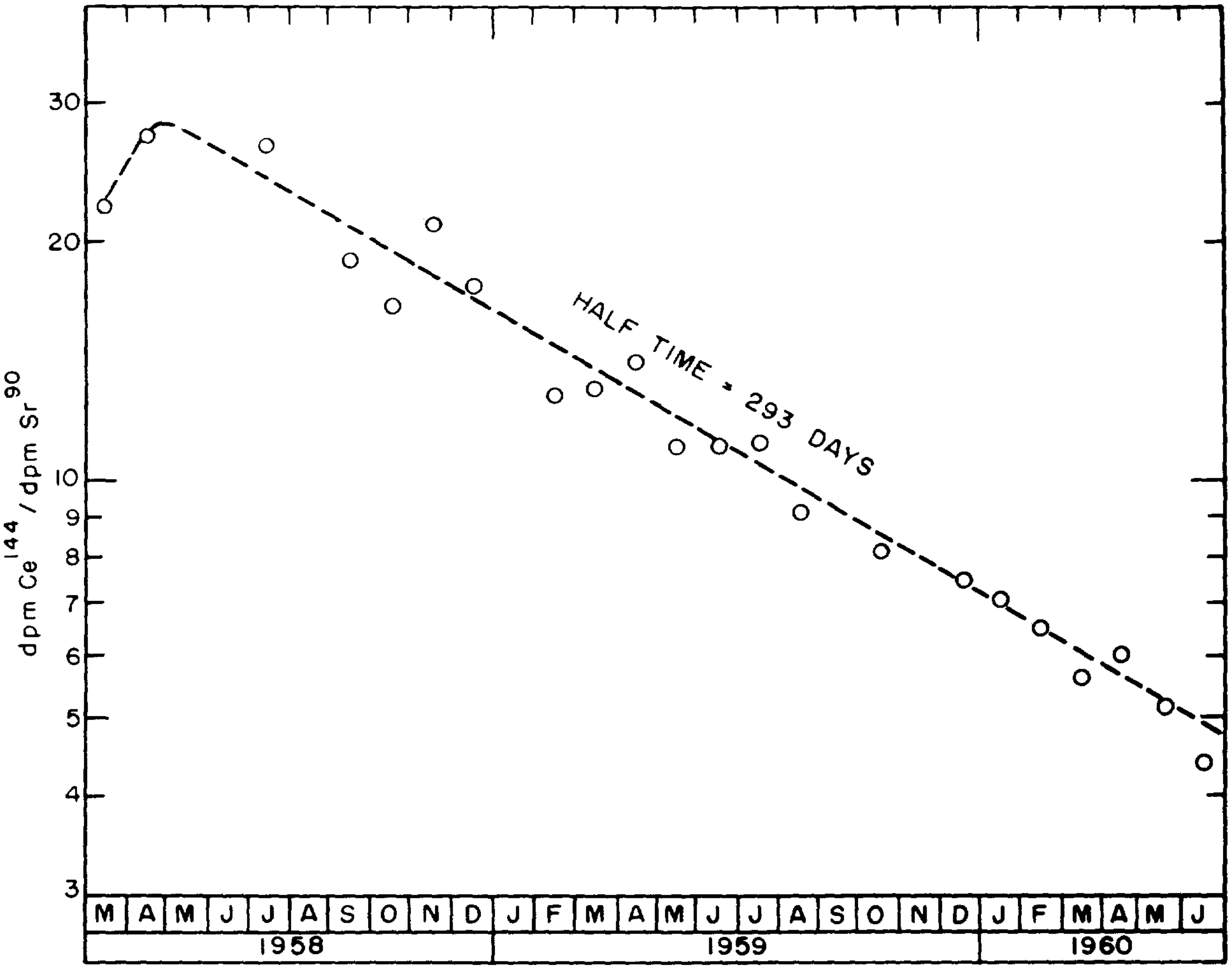

Fig. 2 -- Variation with time of the monthly average for Ce^{144}/Sr^{90} in samples collected between 60,000 and 70,000 feet in the tropical stratosphere (30°N - 30°S)

between 60,000 and 70,000 feet in the latitude band 30°N-30°S. After June 1959, when the effects of individual shots had been eliminated by mixing of debris, the points closely fit a decay curve representing the 293 day radioactive decay half-time of the ratio. Most of the debris in the lower tropical stratosphere after the summer of 1958 had probably been produced by the Hardtack test series and UK tests in the fall of 1958 (as can be shown by data on the concentrations of tungsten-185, a tracer for early Hardtack debris, and W^{185}/Sr^{90} ratios). Since all megaton tests in the Hardtack series were surface bursts (with the exception of Teak and Orange) it is a near certainty that cerium-144, which has no rare gas precursor, was to a large degree fractionated from Sr^{90}, which has a gaseous precursor, and was deposited as local fallout through the scavenging effect of the soil picked up by the fireball. As a result the Ce^{144}/Sr^{90} ratios in the lower tropical stratosphere were lower than would have been expected if the weapons which produced the nuclides had been detonated in the air.

By early 1959 mixing between the northern polar and tropical stratosphere had carried debris from Hardtack into the polar stratosphere at altitudes between 60,000 and 70,000 feet, and had raised the Ce^{144}/Sr^{90} ratio in the debris in that region to a value close to that in the tropical stratosphere (see Figure 3). Below 60,000 feet the ratio was even higher, doubtless due to a residue of debris from the autumn 1958 Soviet tests. By mid-1959, the fallout of Soviet debris from the lower polar stratosphere together with further vertical and meridional mixing had resulted in the establishment of a fairly uniform Ce^{144}/Sr^{90} ratio throughout the lower stratosphere of the Northern Hemisphere.

Rather suddenly during December 1959 there was an increase in the Ce^{144}/Sr^{90} ratio between 60,000 and 70,000 feet in the northern polar stratosphere, probably as a result of the movement into that region of unfractionated debris from some past air burst.

A similar increase occurred between 40,000 and 55,000 feet in April, 1960. After the Ce^{144}/Sr^{90} ratio had passed through a maximum during January, 1960, in the 60,000 to 70,000 feet altitude layer it decayed according to its radioactive half-life. There can be little doubt that the admixture of "unfractionated" Teak and Orange nuclear debris from the high stratosphere (>70,000 feet) was the cause of the sudden increase of the Ce^{144}/Sr^{90} ratio in the polar stratosphere, for rhodium-102 concentrations rose at the same time as the Ce^{144}/Sr^{90} ratio and there is no evidence that significant quantities of debris from Soviet air bursts rose above 60,000 feet. The general picture, then, is one in which the nuclear debris from the upper atmosphere entered the lower stratosphere, predominantly in the polar regions, during the winter months and penetrated down almost to the tropopause by June, 1960.

Estimation of Teak and Orange Debris in Lower Stratosphere

We may estimate the contribution of Teak and Orange to the total lower stratospheric burden of fission products during early 1960 by considering the strontium-90 concentrations together with the Ce^{144}/Sr^{90} ratios in the stratospheric debris. The strontium-90 concentrations (in dpm/1000 SCF) at three altitudes, 60,000 feet, 65,000 feet, and 70,000 feet, are plotted against time for the latitude bands 90°N-30°N, 30°N-30°S, and 30°S-60°S in Figures 4, 5, and 6, respectively. These plots indicate the same general trend as do the Ce^{144}/Sr^{90} ratios. While the strontium-90 concentrations in the tropical stratosphere were decreasing after the middle of 1959 (about a factor of two by June 1960), those in the northern polar stratosphere remained essentially constant and those in the southern polar hemisphere increased. In the period July to August 1959 the strontium-90 concentrations at 65,000 and 70,000 feet in both tropical and northern polar stratosphere were

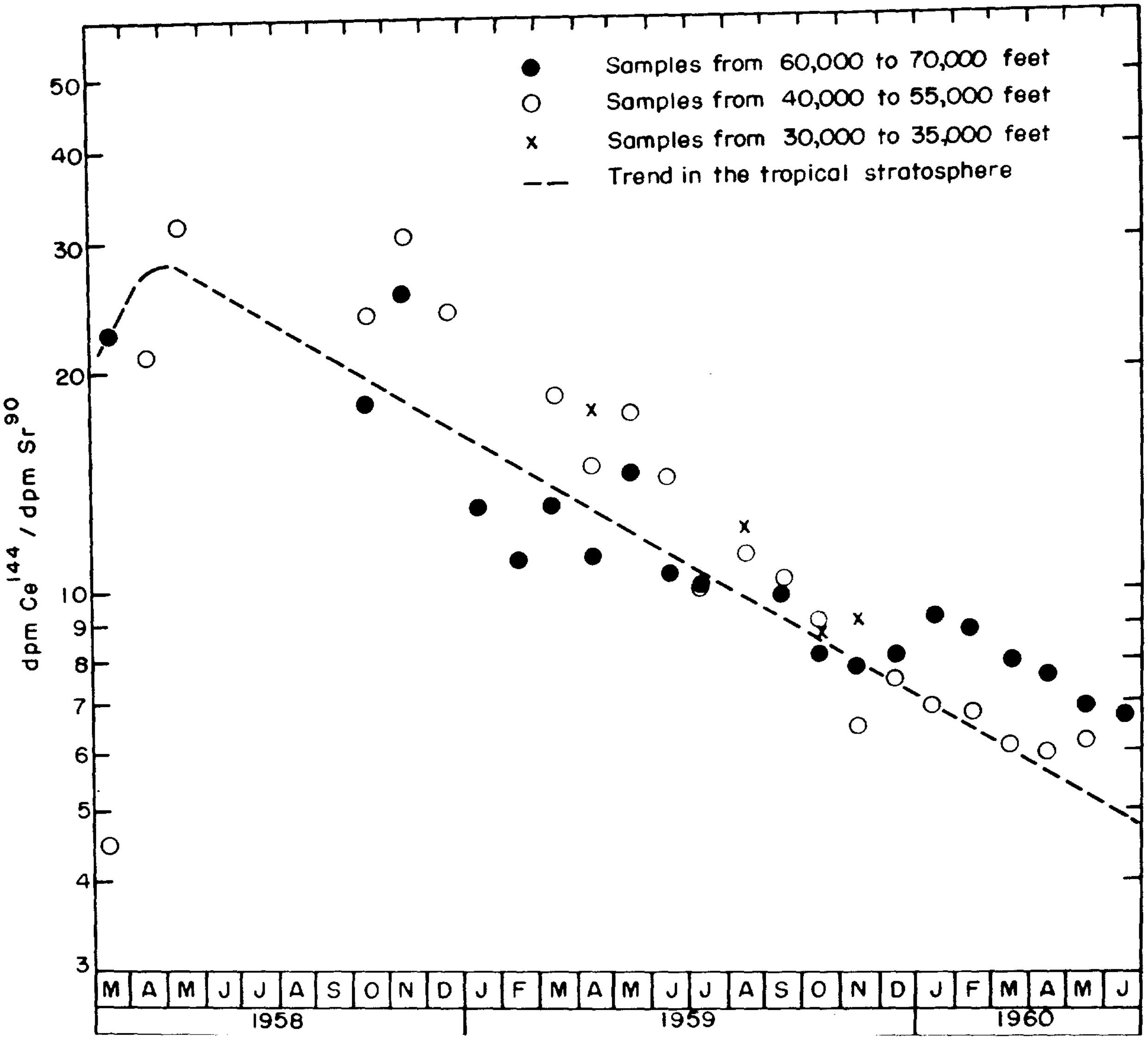

Fig. 3 -- Variation with time of the monthly average for Ce^{144}/Sr^{90} in samples collected in the northern polar stratosphere (30°N-90°N)

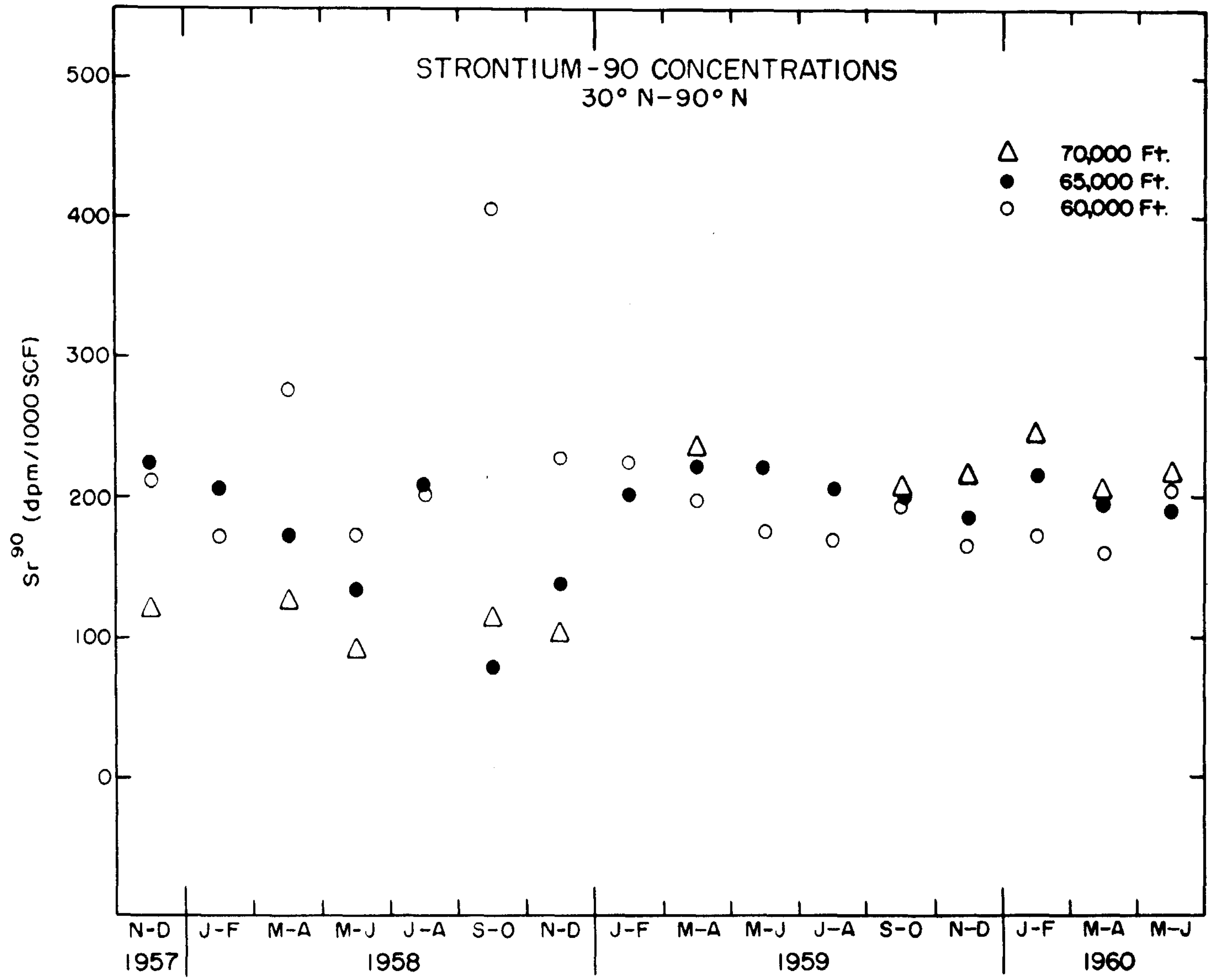

Fig. 4 -- Strontium-90 concentrations (in dpm/1000 scf) at 60,000, 65,000, and 70,000 feet versus time for latitude band 90°N-30°N.

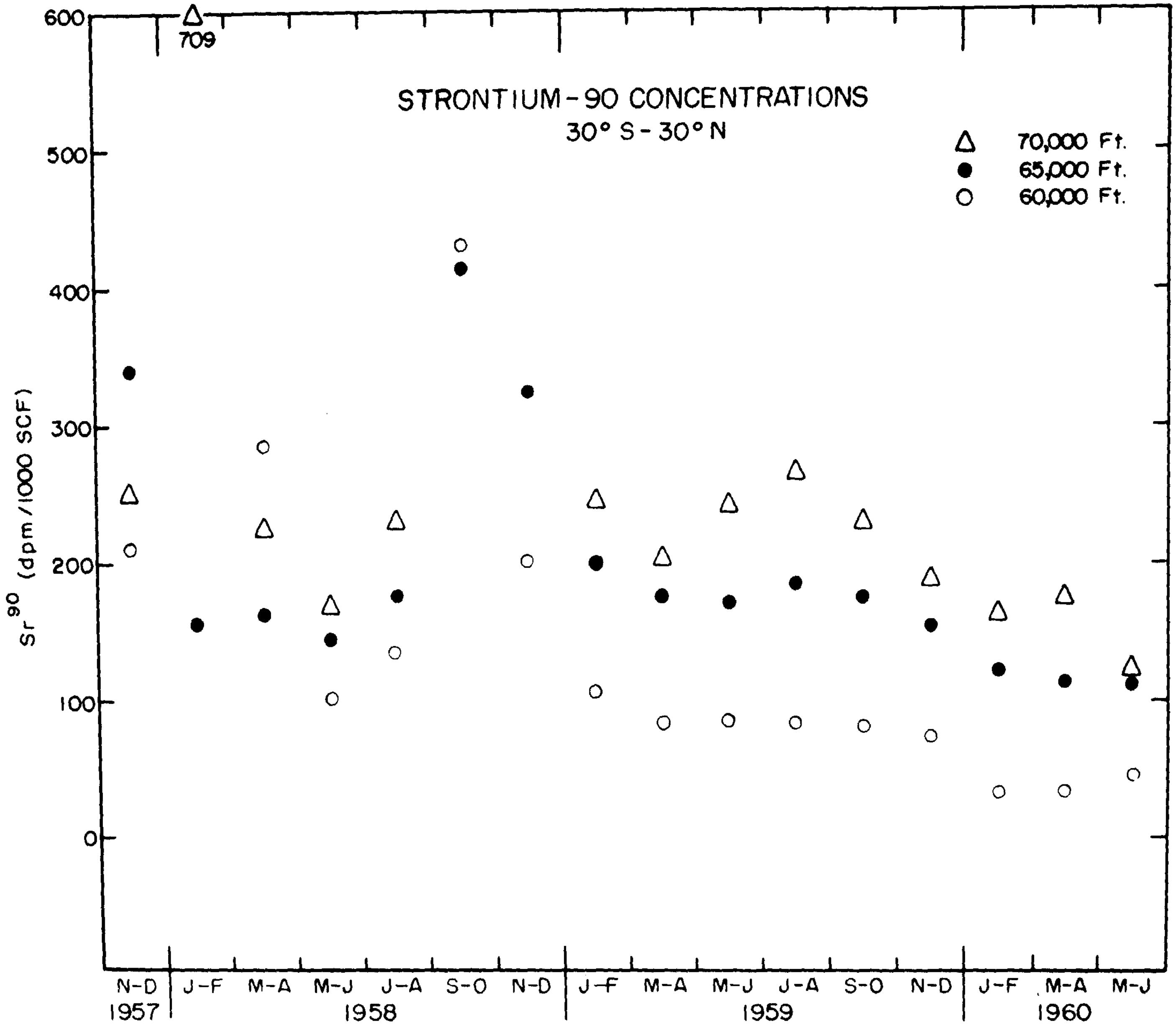

Fig. 5 -- Strontium-90 concentrations (in dpm/1000 scf) at 60,000, 65,000, and 70,000 feet versus time for latitude band 30°N-30°S

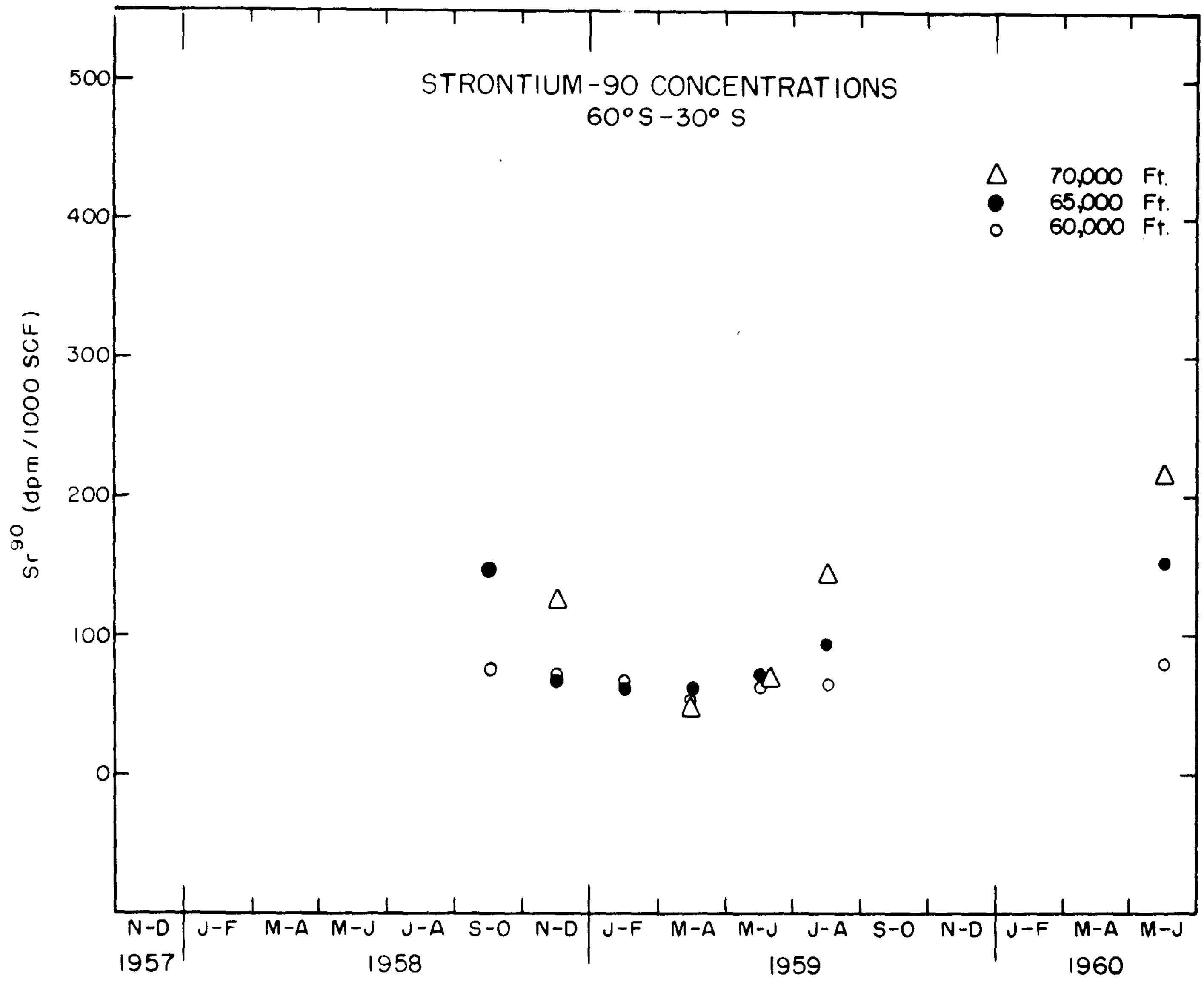

Fig. 6 -- Strontium-90 concentrations (in dpm/1000 scf) at 60,000, 65,000, and 70,000 feet versus time for latitude band 30°S-60°S

nearly the same value, about 200 dpm/1000 scf. By May-June 1960 the values for these altitudes in the tropical stratosphere had dropped to about 120 dpm/1000 scf while those in the northern polar stratosphere remained about 200 dpm/1000 scf.

The distribution of nuclear debris in the northern hemisphere during March-April 1960 is shown in Figure 7, in the form of cross sections of the atmosphere. The data presented include tungsten-185 concentrations, rhodium-102 concentrations, and Ce^{144}/Sr^{90} ratios. The Ce^{144}/Sr^{90} ratios indicate that Teak and Orange contributed more than 20 percent of the strontium-90 above 50,000 feet in the northern polar stratosphere during early 1960. Integration of the strontium-90 distribution yields approximately 0.37 megacuries of strontium-90 in the lower atmosphere of the Northern Hemisphere during early 1960, of which about 0.053 megacuries was from Teak and Orange. Unfortunately, HASP has no data for the Southern Hemisphere between August 1959 and May 1960 and the data which exist for May and June 1960 do not provide sufficient coverage of the hemisphere to permit an accurate calculation of the contribution of Teak and Orange to the strontium-90 burden in that region. Thus, for want of a better procedure it is tentatively assumed that about 0.05 mc of Teak and Orange were also present in the lower atmosphere of the Southern Hemisphere by the end of the 1960 winter season in that hemisphere. This gives a very crude estimate of 0.1 mc of strontium-90 from Teak and Orange in the lower stratosphere (below 70,000 feet) by the latter half of 1960.

Teak and Orange Debris in the Stratospheric Aerosol

A rather unique opportunity to observe particles bearing Teak and Orange debris was afforded by the use of impaction probes mounted on the sampling aircraft. A total of ten samples were obtained between February 25 and May 19, 1960. Of these, three were taken from the region of the northern polar stratosphere in which Teak and Orange debris was detected as previously described.

The samples were examined by electron microscopy and electron diffraction. From the electron diffraction patterns it was determined that most of the particles consist of ammonium sulfate and ammonium persulfate. Figure 8 shows electron micrographs of some typical particles of the sulfate and persulfate types.

The particles in the electron micrographs were classified according to radius (or equivalent spherical radius in the case of flat particles). The number of particles in each class was corrected for impaction efficiency for a cylindrical surface according to the method of Ranz and Wong.[3] This procedure made possible computations of the number concentrations (in particles per cubic centimeter of air) and volume concentrations (in cubic centimeters of particles per cubic centimeter of air) of the aerosol. Table 1 shows the results of these calculations. Also shown are the ratios of strontium-90 concentrations to aerosol volume concentrations, i.e., the calculated concentration of strontium-90 (in dpm per cm^3 of solid) in the particles. The strontium-90 concentrations (in disintegrations per minute per cubic centimeter of air) were computed by averaging the concentrations as determined from the filter papers over the flight path for the duration of the probe sampler exposure. Except for sample P-5 all of the samples in Table 1 which were collected in polar air have strontium-90 concentrations in the particles which are at least a factor of two higher than the average for samples collected in tropical air. The average strontium-90 concentration in polar samples (except for P-5) is 2.18×10^8 dpm/cm^3 of aerosol while the average for tropical samples is 4.49×10^7 dpm/cm^3 of aerosol.

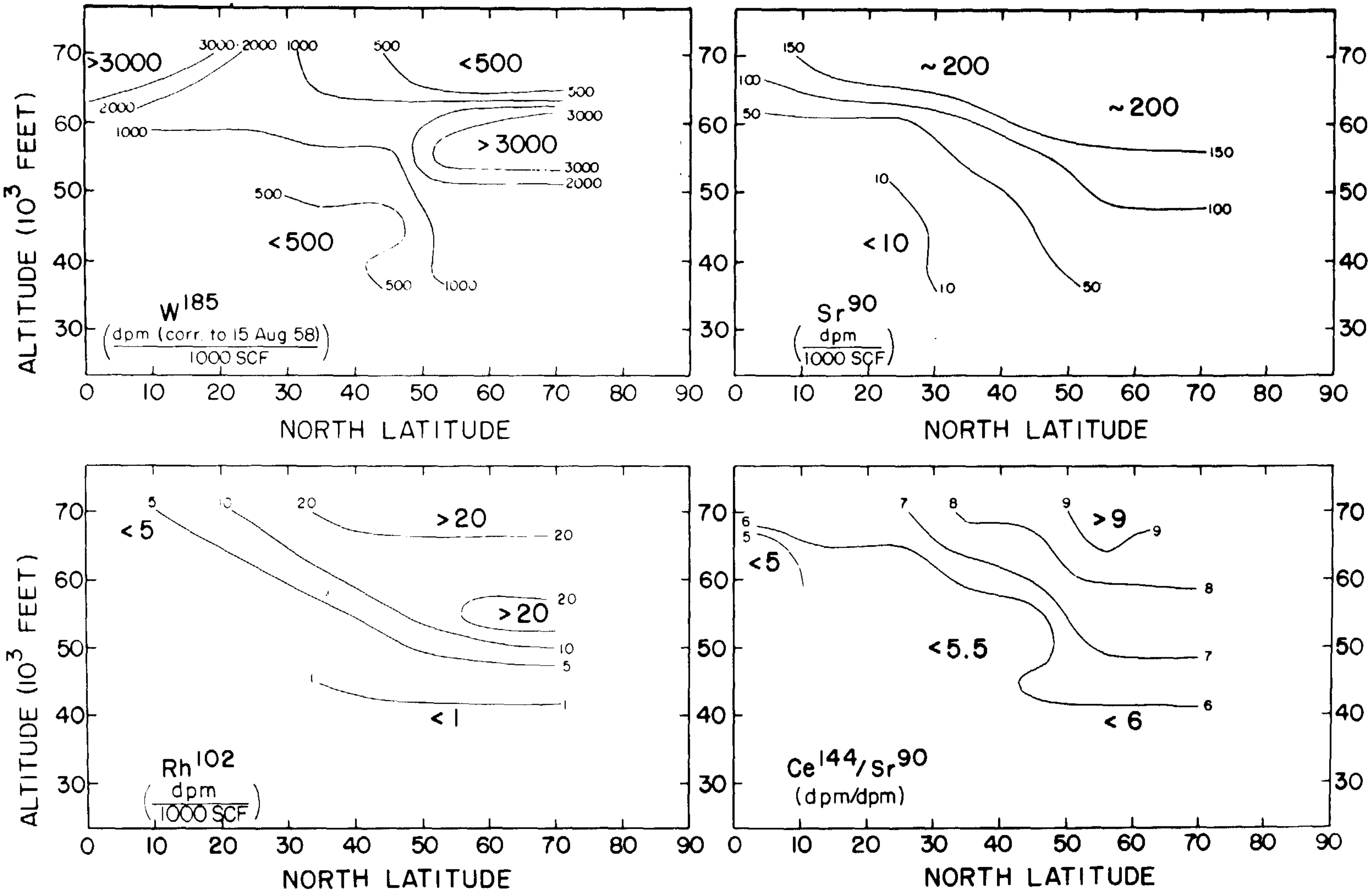

Fig. 7 -- Nuclear debris in the stratosphere of the northern hemisphere, March-April 1960.

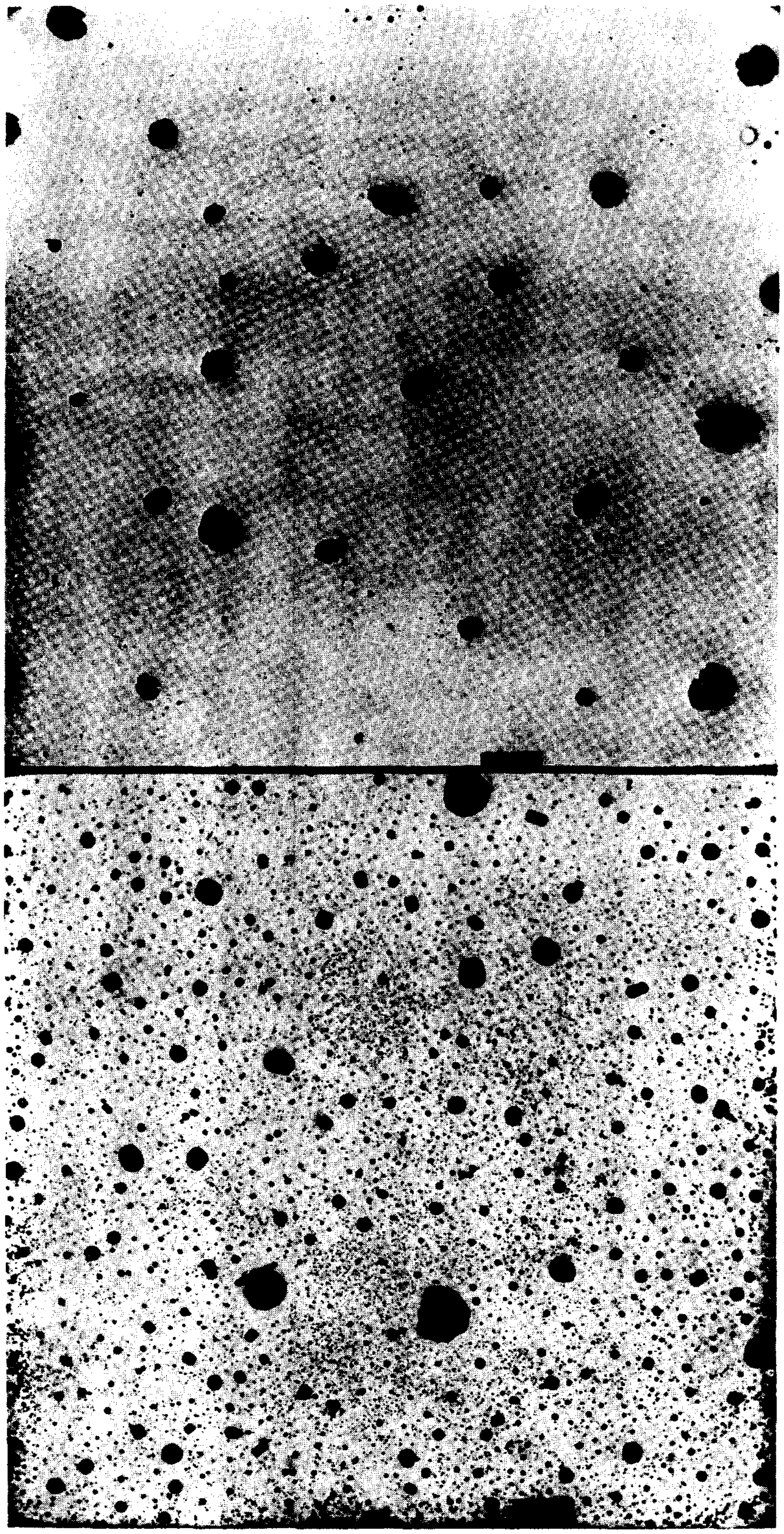

Fig. 8 -- Electron micrographs of some typical particles of the sulfate and persulfate types.

TABLE 1

Sample Number	Date Collected	Altitude (thousands of feet) Duration (minutes)	Latitude	Number Concentration (cm^{-3})	Volume Concentration (cm^3/cm^3)	Sr^{90} Concentration in aerosol (dpm/cm^3 of particles)
P-1	3/3	60 (205)	71:00→51:17N	0.30	2.81×10^{-15}	--
P-2	3/22	50 (208) 55 (176)	27:43→48:00N	0.18	1.39×10^{-15}	1.02×10^{8}
P-3	3/29	40 (60) 55→65 (319)	Orbit 45:45N	0.19	1.01×10^{-15}	4.87×10^{8}
P-4	3/31	55→64 (406)	37:00→48:25N	0.16	2.35×10^{-15}	1.81×10^{8}
P-5	4/12	40.5 (171)	67:00→50:25N	0.21	5.19×10^{-15}	7.40×10^{7}
P-6	4/30	65 (147)	65:50→50:20N	0.45	4.59×10^{-15}	1.09×10^{8}
P-7	5/19	50 (377)	36:40⇄40:00S	0.09	9.93×10^{-16}	2.90×10^{8}
P-8	11/17	60→68.6 (341)	62→32N	0.044	3.01×10^{-15}	--
AVERAGE POLAR				0.17	2.67×10^{-15}	2.18×10^{8}
T-1	2/25	60 (181) 65 (186)	27:42⇄13:42N	0.28	3.25×10^{-15}	6.86×10^{7}
T-2	3/16	66.8 (357)	27:42⇄22:00N	1.23	9.95×10^{-15}	3.65×10^{7}
T-3	4/5	64.5→68.6 (401)	15:00N⇄8:00S	0.38	8.38×10^{-15}	3.22×10^{7}
T-4	4/21	60→65 (210)	Orbit 48:00N	1.33	2.10×10^{-14}	2.94×10^{7}
T-5	4/21	50 (193) 55 (181)	27:43⇄48:00N	0.48	5.22×10^{-15}	5.98×10^{7}
T-6	5/12	63→67 (442)	16:25N→29:20S	0.33	9.35×10^{-15}	3.04×10^{7}
AVERAGE TROPICAL				0.67	9.52×10^{-15}	4.49×10^{7}

Junge, Chagnon, and Manson[5] have shown that a stratospheric aerosol layer exists between the tropopause and about 76,000 feet, above which the particle concentration decreases rapidly with increasing altitude. If it is assumed that the nuclear debris, and therefore strontium-90, from testing prior to Teak and Orange had been mixed throughout the altitude range in which the aerosol exists, then the strontium-90 had become incorporated into the stratospheric sulfate particles. The value of the average strontium-90 concentration in the tropical stratospheric aerosol of 4.5×10^7 dpm/cm^3 of aerosol might then be used to characterize particles containing pre-Teak and Orange debris in the lower stratosphere. It thus appears, in view of the previous arguments concerning the origin of the debris in both polar and tropical stratospheric air, that with the possible exception of sample P-5, all of the listed "polar" samples contained material from Teak and Orange shots. However, it seems that the contribution of Teak and Orange debris in these samples is at least 50 percent, whereas from the Ce^{144}/Sr^{90} ratios, it was estimated that the Teak and Orange contribution between 60,000 and 70,000 feet was about 30 percent. One explanation of the apparently high proportion of Teak and Orange debris in the stratospheric aerosol may be that prior to the time when sampling of particles began some Hardtack debris from the altitudes in the tropical stratosphere above the aerosol region (~80,000 feet) had mixed into the polar stratosphere at lower altitudes (60,000 to 70,000 feet). Particles with a higher strontium-90 content could have resulted from this process since the debris from the tropical stratosphere could have been associated with relatively small concentrations of sulfate aerosol until it became mixed to lower altitudes in the polar stratosphere. That such a mixing process can occur has been indicated by Feely and Spar[6] based on studies of the stratospheric distributions of tungsten-185 produced by Hardtack tests. Of course it is not possible to extract detailed knowledge of the relationships among nuclear debris, aerosol concentrations, and atmospheric transport properties since there exist uncertainties in estimating the particle volume concentrations and the average strontium-90 concentrations.

The scanning of the electron micrographs fails to reveal significant differences in particle shape or type between samples containing Teak and Orange debris and those not containing that debris. Some particles were found in most samples which were different in appearance from the sulfate particles. These were predominantly of radii greater than one micron, some spherical, some irregular in shape. Their composition could not be determined by electron diffraction. These larger particles contributed less than 10 percent of the calculated volume concentrations.

While there is no direct observational evidence to provide detailed knowledge of the trajectories of the particulate matter resulting from the high altitude nuclear detonations, some reasonable speculations may be made to explain the observations described above. It is assumed that most of the mass of a nuclear weapon detonated in the atmosphere appears in particles of less than 0.1 micron diameter and that, in the absence of indigenous particles, most of the fission products, even those with noble gas precursors, eventually reside in the particles of weapon debris. These small particles, which have low sedimentation (about 0.01 cm/sec) also exist in concentrations low enough so that coagulation to produce particles with appreciable settling velocities is not a significant factor in the ultimate history of most of the debris. It is thus possible that the distribution of nuclear debris in the upper atmosphere has been primarily governed by the motions of the air. Initial horizontal sperading of the debris (most likely by turbulent mixing) followed either by large scale vertical mixing or by subsidence in the polar regions especially during the winter season, would be consistent with the observations of the Ce^{144}/Sr^{90} ratios in debris in the lower polar stratosphere. It is most likely that as the debris from the upper atmosphere detonations reaches the lower altitudes at which the sulfate aerosol layer exists, the small particles of debris rapidly coalesce with the larger sulfate particles, and that their subsequent fate is determined by the behavior of the sulfate aerosol particles. While the detailed history of particulate matter in the stratosphere is still not known, the High Altitude Sampling Program[2] has provided some knowledge of the mean stratospheric residence times involved. Observations of nuclear debris from Soviet tests indicate a fallout rate from the lower polar stratosphere commensurate with a residence half-time of about six months depending on seasonal effects. While some of the particles of the stratospheric aerosol may have appreciable settling velocities (greater than 0.1 cm/sec), it seems reasonable that most of the particles have settling velocities small enough to permit air motions to govern their fate. Once the particles leave the stratosphere (by any of several possible processes) and enter the troposphere, they are rather quickly removed by rain and deposited on the surface of the earth.

Comments on Rocket Sampling of the Upper Atmosphere

Although the above discussion of the behavior of particles in the upper atmosphere is quite speculative, it can be seen that knowledge of the physical behavior of the particles is vital in understanding the processes of air movements. For instance, if particles of about one micron radius were placed in the upper atmosphere (say at about 200,000 feet) their fall rates would be quite appreciable (initially about 10 cm/sec) until they reached the lower stratosphere (about 80,000 feet). Measurements of mass or radioactivity of aerosol samples, collected by rockets in the upper atmosphere, without regard to the particle size might lead to erroneous conclusions since the measured distributions would have been governed by both gravitational effects and air motions. It thus seems that any program which involves sampling of the upper atmosphere must include determinations of particle size-concentration distributions along with those of composition, radioactivity, and the like.

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(B)

Results for the Rhodium-102 High Altitude Tracer Experiment

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Introduction

About 3 megacuries of Rh^{102} were produced as a tracer in the Orange Shot of the Hardtack series on 11 August 1958 at 2330 hours. This shot was an air burst at about 43 km above Johnston Island at about 16°N latitude and 170°W longitude.[1] The bulk of the debris is estimated to have almost certainly risen to above 100 km and probably went at least as high as 150 km.

Rh^{102} decays mainly by electron capture. Measurement of its concentration was made by x-ray counting of the rhodium fraction which had been chemically separated from the original filter samples. In addition to the 210-day half-life isomer, a longer-lived isomer of roughly four years half-life and the long-lived Rh^{102} isomer were also produced.[2] The x-ray counts were corrected for the presence of these other activities by use of an empirical decay curve obtained for several stratospheric samples. Most of the results discussed are for stratospheric air filter samples collected by high-altitude aircraft.

Results

Results for samples collected above 19.4 km are shown in Figures 1 through 5. Figure 1 presents the concentration as a function of time for different northern latitude regions. Figure 2 does the same for southern latitudes. Figure 3 presents the concentration as a function of latitude during the first half of 1959. Figure 4 contains later data, and Figure 5 presents a latitude profile for samples collected during a single brief period.

Discussion of Results

As can be seen from Figure 1, there was a sharp increase in Rh^{102} concentrations beginning in the late fall of 1959 and continuing through the early winter. This coincides well with the time one would expect downward mixing associated with the development of disturbances in the polar vortex region. These disturbances begin in about November and continue throughout the winter, being strongest about January.[3] The results are consistent with a picture of great downward mixing at high latitudes (centered about 60°) in the winter followed by horizontal movement in the low stratosphere. Figure 2 shows an increase in concentration during the Southern Hemisphere winter, again suggesting high latitude winter downward mixing as the mechanism. The Southern Hemisphere results also suggest that a relatively small fraction of the tracer appeared the first winter with a much larger fraction appearing in the second winter.

From Figure 3, it is apparent that mixing across the equatorial region is slow at least on the time scale and the altitude region (about 20 km) represented by these values. Comparison of the early concentrations with later values indicates that the early rhodium in the low stratosphere is equal to only a few percent of the Orange

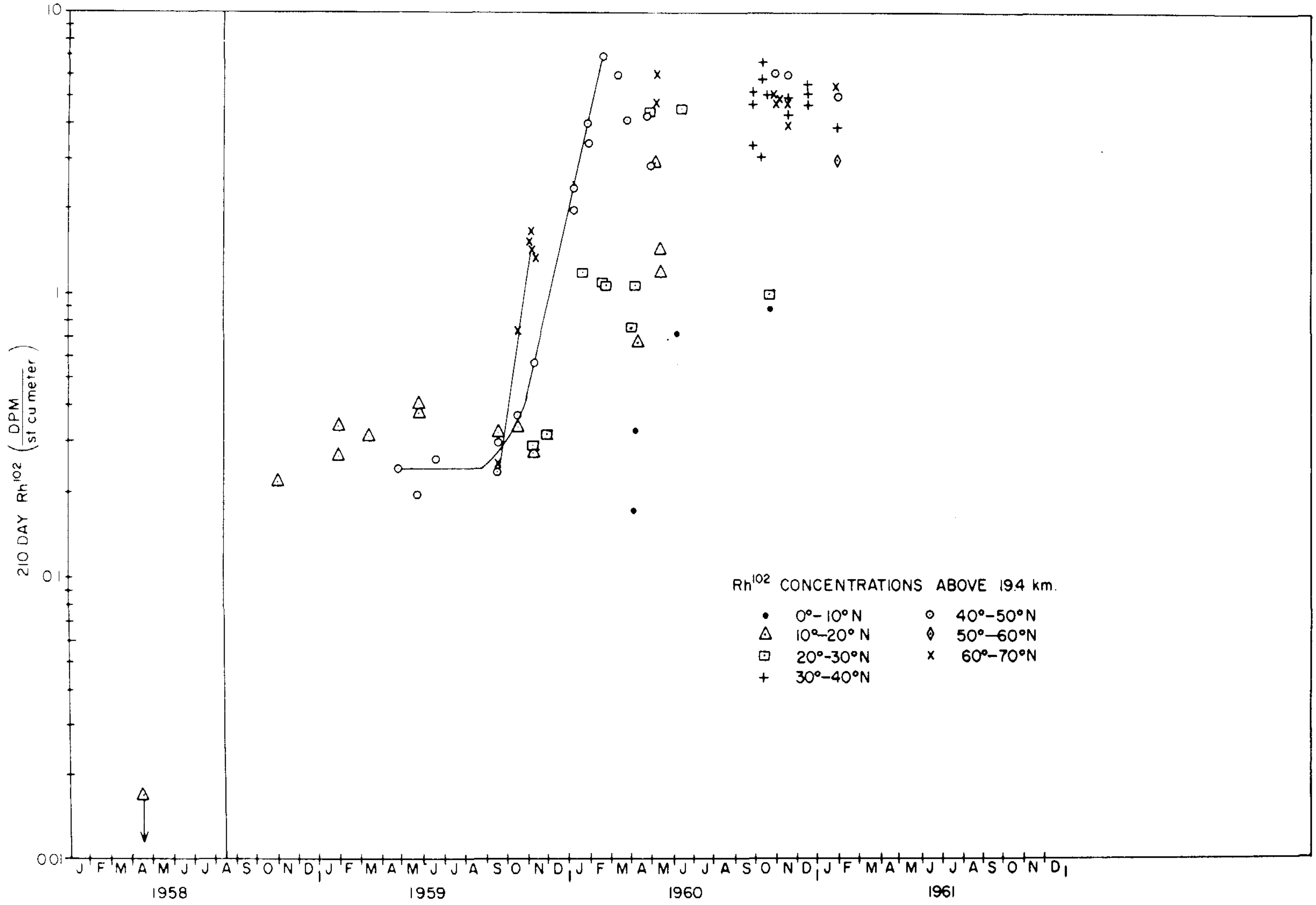

Fig. 1 -- Concentration as a function of time for different northern latitude regions

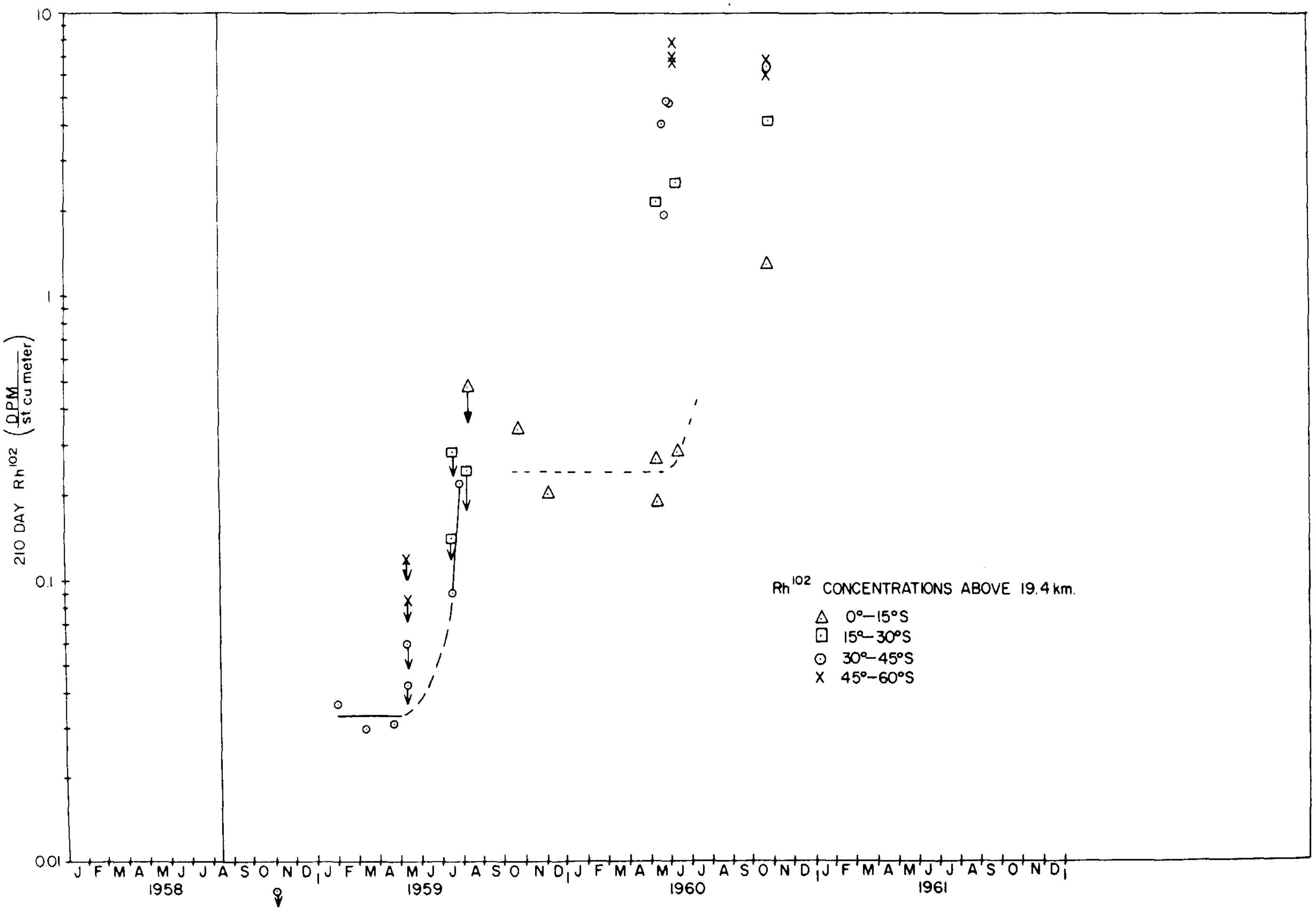

Fig. 2 -- Concentration as a function of time for different southern latitude regions

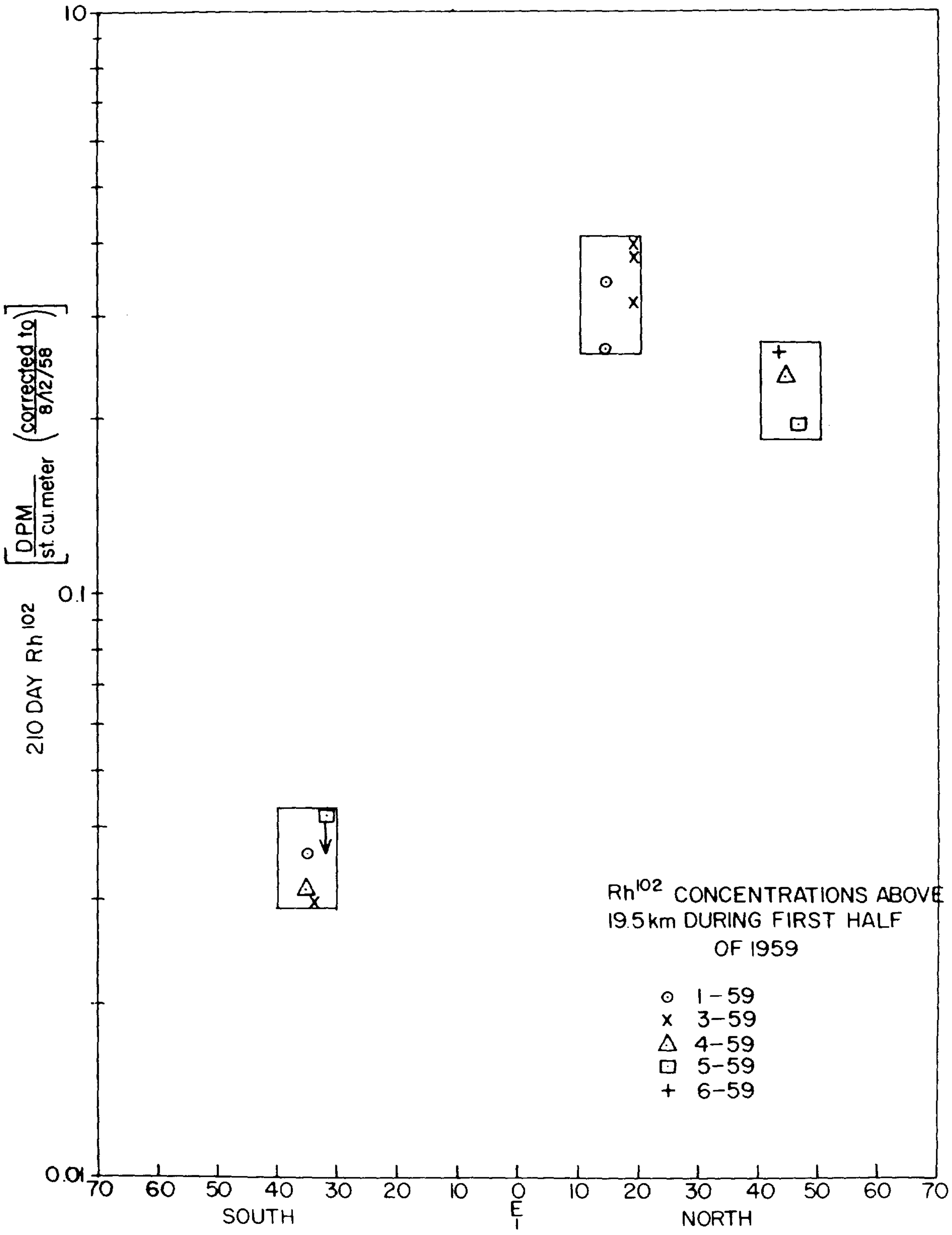

Fig. 3 -- Concentration as a function of latitude during first half of 1959

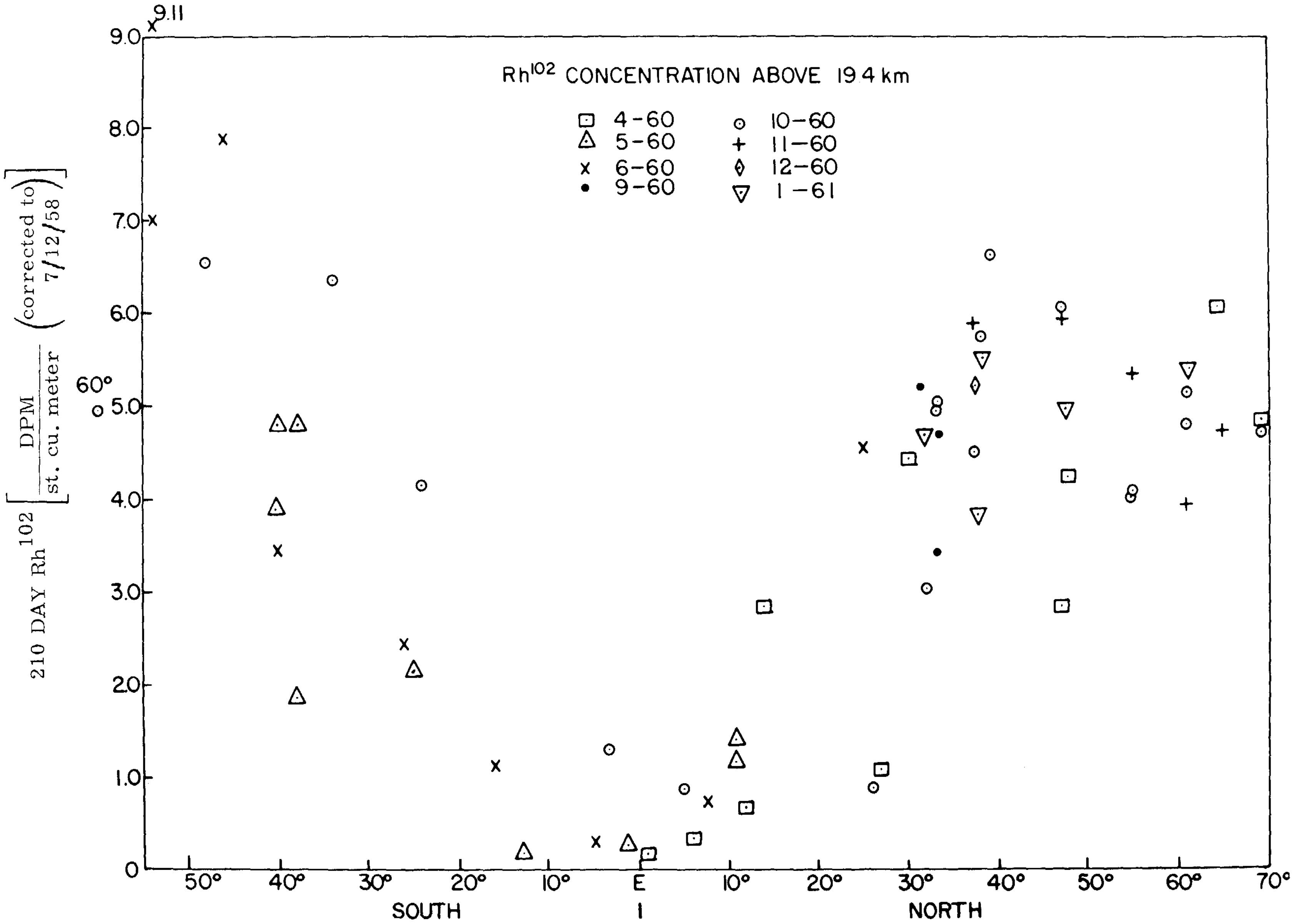

Fig. 4 -- Concentration as a function of latitude

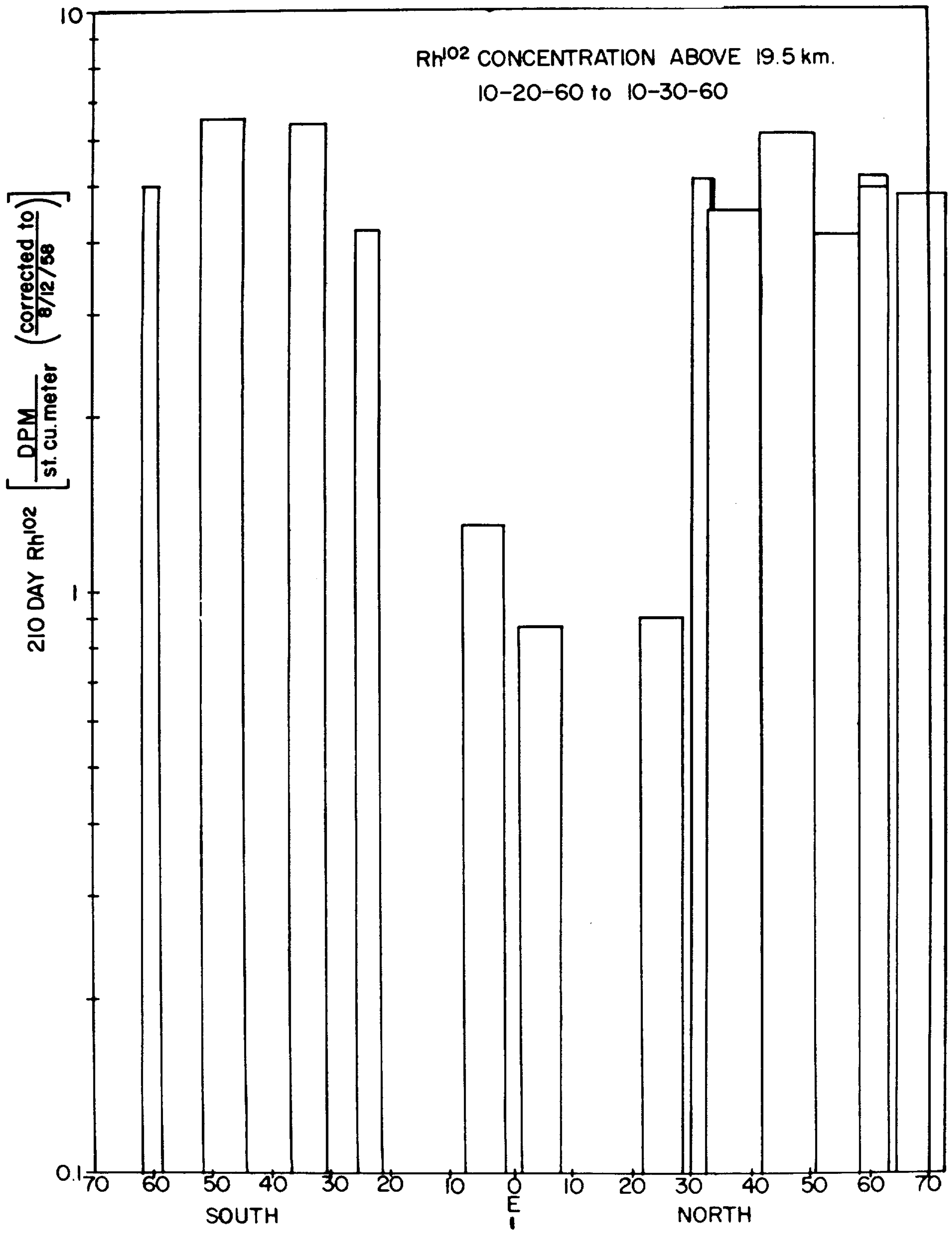

Fig. 5 -- Latitude profile for samples collected from 10/20/60 to 10/30/60.

rhodium. Since there was about 0.3 Mc of Rh^{102} produced in other tests of the Hardtack series, the possibility that some of the early Rh^{102} is due to sources other than Orange cannot be entirely ruled out. In any case, the conclusion of slow mixing between hemispheres seems valid.

From Figures 4 and 5, it can be seen that there is considerable variation of concentration with latitude. The most obvious features of this distribution pattern are the relatively uniform and equal high concentrations at high latitudes in both hemispheres with a rather sharp drop to low concentrations at the low latitudes. It would indicate that there is a large difference in the mixing pattern in the two regions (high latitude and low latitude). This is to be expected on the basis of meteorological evidence.

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(C)

Evidence for High Stratosphere Holdup of Nuclear Bomb Debris

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Introduction

It is the purpose of this symposium to explore the need for and the possible methods of sampling the high atmosphere. It is the object of this paper to review some of the evidence for possible substantial high stratosphere retention of radioactive contamination from past nuclear tests.

During and following the November 1952 Ivy and spring 1954 Castle tests of multimegaton thermonuclear devices at the Pacific Proving Grounds, there existed no adequate stratospheric sampling techniques that could be employed to assess the distribution and inventory of stratospheric contamination. The fraction of total bomb debris retained in the stratosphere was estimated as the difference between total production and the fraction deposited as local fallout. Estimates made on this basis suffer from the inherent difficulties of integrating local fallout over tens of thousands of square miles of ocean area. Estimates for the stratospheric fraction for high yield surface water shots range from 80 percent[1] to 30 percent.[2,3] The difference reflects uncertainties in estimation as well as possible influence of total energy-yield differences for the several events on which the estimates were based. Thus the stratospheric inventory for past high-yield surface detonations, which make up a substantial fraction of total test production, is at best quite poorly known.

The integration of bomb debris deposited in world-wide fallout, based on radioassay of rainfall, soil and seawater samples, places further limits on the stratospheric inventory. However the complexity of the fallout pattern, the question of representativeness of samples, and the limited amount of good data for remote locations and ocean areas restrict the accuracy of cumulative world-wide fallout estimates. Thus material balance estimates of the residual contamination of the stratosphere, total fallout production less local fallout and cumulative world-wide fallout, are very uncertain. The present burden of long-lived fission products in the stratosphere above 100,000 feet lies somewhere between an appreciable fraction of total past production and a lower limit of nearly the total production of the Teak and Orange shots, two megaton yield nuclear detonations at high altitudes over Johnston Island.[4]

Estimates of the high stratosphere content of residual contamination on the basis of fallout observations in the lower atmosphere are clearly unacceptable. Changes in the lower atmosphere fallout concentration and isotopic composition with time would provide a useful index of the atmospheric burden only if the stratosphere were well mixed. There is a mounting volume of evidence that the stratospheric residence time of nuclear debris clouds varies widely with injection altitude and latitude[5-7] and that the stratosphere is not well mixed.[7,8]

The question of the burden, distribution and mixing history of nuclear debris at levels above 100,000 feet is one of the more important and interesting unsolved geophysical problems on world-wide fallout. Its solution will contribute substantially to knowledge of stratospheric circulation. Two obvious experimental approaches are direct investigation by high altitude sampling and the use of selected atmospheric tracers.

Some of the fallout evidence having bearing on the question of high stratosphere holdup of nuclear debris from past tests are presented and discussed below.

Cloud Height Considerations

The development and rise of nuclear clouds and the influence of meteorological factors on their height and shape have been discussed by Machta[9] and Kellogg.[10] Approximate stabilization cloud heights for equatorial surface bursts of several yields, based on results of Kellogg,[10] are given in Table 1. Time of stabilization apparently increases somewhat with yield, but is usually taken as 6 minutes. For megaton yield shots, horizontal growth continues to 10 minutes or longer at a rate too rapid to be due to natural wind shear.[10] The observational data for megaton burst clouds are inadequate to reliably assess the vertical extent and heights of cloud tops at times beyond the first few minutes. Subsequent vertical extent due to meteorological mixing on a time scale of weeks and months is unknown.

Table 1
Approximate Stabilized Cloud Heights for Equatorial Surface Bursts

Yield	Height, Center (ft)	Vertical Extent (ft)	Height, Top (ft)
10 MT	82,000	46,000	105,000
1 MT	63,000	28,000	77,000
100 KT	40,000	16,000	48,000
50 KT	34,000	12,000	40,000

Considerations about debris condensation, particle growth and cloud distribution have been discussed by the Rand Corporation[11] and by Lapple.[12] Lapple argues convincingly that the radioactive debris cannot be restricted to a discrete small portion of the cloud volume, but must be distributed generally throughout the cloud. However, in stratospheric clouds for high-yield surface shots, the upper portion of the cloud should exhibit higher activity concentration, a greater proportion of the very fine particles which persist in the stratosphere, and some enrichment in fission products like Sr^{90} and Cs^{137} which condense late from gaseous or volatile precursors. Subsequent upward migration of the more concentrated zone of the debris cloud on a time scale of hours, days, and longer, may occur under the influence of local heating from residual radiation and from debris absorption of radiation from external sources. The evaluation of these as well as meteorological factors which may modify the vertical distribution of nuclear debris clouds following stabilization appears to be a neglected subject.

U-2 aircraft, used in the High Altitude Sampling Program,[2, 8] provide excellent means of sampling up to levels of 70,000 feet. However, it is clear from the cloud height data, Table 1, and from other considerations mentioned above, that a substantial proportion of debris from megaton-yield tests was injected to higher levels.

Balloon-borne air-filter equipment has been employed in the U. S. Atomic Energy Commission Ash Can Program for sampling up to levels of about 95,000 feet. Until recent improvements in balloon filter-sampling techniques, the balloon data have been unreliable as well as limited in amount. Since the tops of Ivy and Castle test clouds reportedly rose to heights of 25 miles,[13] the altitude range of past balloon sampling was inadequate to assess the subsequent distribution from these early tests. Unless sedimentation, circulation or mixing are effective in the rapid downward transport of debris from all levels above 95,000, the need for a higher altitude sampling capability is clear.

Particle Size of Stratospheric Debris

The importance of gravitational sedimentation as a mechanism for the removal of stratospheric fallout has not been fully assessed due to lack of experimental data on the size distribution of radioactive bomb debris in the stratosphere. The long storage time exhibited by stratospheric debris[14] provided the first indication that the particles must be in the sub-micron size range. The apparent large altitude and latitude influence on stratospheric storage times[6] indicate that circulation or mixing, not settlement, govern stratospheric fallout. Recent evidence from tritium and strontium-90 concentration of Bedford rains[15] show a similar pattern and thus a similar mechanism of removal for stratospheric water vapor and particulate debris from the October 1958 Soviet tests at Novaya Zemla.

Direct experimental evidence obtained by my associates[16] on the size distribution of radioactive particles in the stratosphere is presented in Figures 1 and 2. The balloon-borne instrument used was a large-volume two-stage impactor, based on a design of Junge[17] backed up by a polystyrene microfiber filter. Particles larger than 0.15μ radius and between 0.02 and 0.15 μ radius were collected on the first and second stage of the impactor, respectively. Particles of smaller radius were retained on the backup filter with presumably very high efficiency. The instrumentation was built, calibrated, and flown by General Mills, Inc., under Contract No. AF19(604)-6202.

The vertical profile temperature and total β-activity data are shown in Figure 1 for the two 1960 flights over Minneapolis. The vertical lines indicate the altitude interval over which each sample was taken. The vertical distribution is distorted by the procedure of continuous sampling during balloon rise over each altitude increment. The 30,000 to 50,000 foot value is most affected since essentially all of the activity over this interval was collected above the tropopause.

The size distribution data, Figure 2, show a marked trend with altitude. About 90 percent of the activity in the lower stratosphere is below 0.15 μ radius. Between 90,000 and 100,000 feet about 60 percent of the total is below 0.02 μ radius. In view of the small sample volumes and low total and fractional activities, the similarity in results for the two flights, Figures 1 and 2, is reassuring.

Preliminary comparison with the size distribution observed by Junge[17] for natural sulfate particles in the stratosphere indicates a smaller mean size for the radioactive particles. The results indicate that sedimentation must not play a significant role in the removal of stratospheric debris, at least at relatively late times following injection. Sampling instruments for use at levels above 100,000 feet must be designed for collection of particles from a few hundredths micron radius to very much smaller sizes.

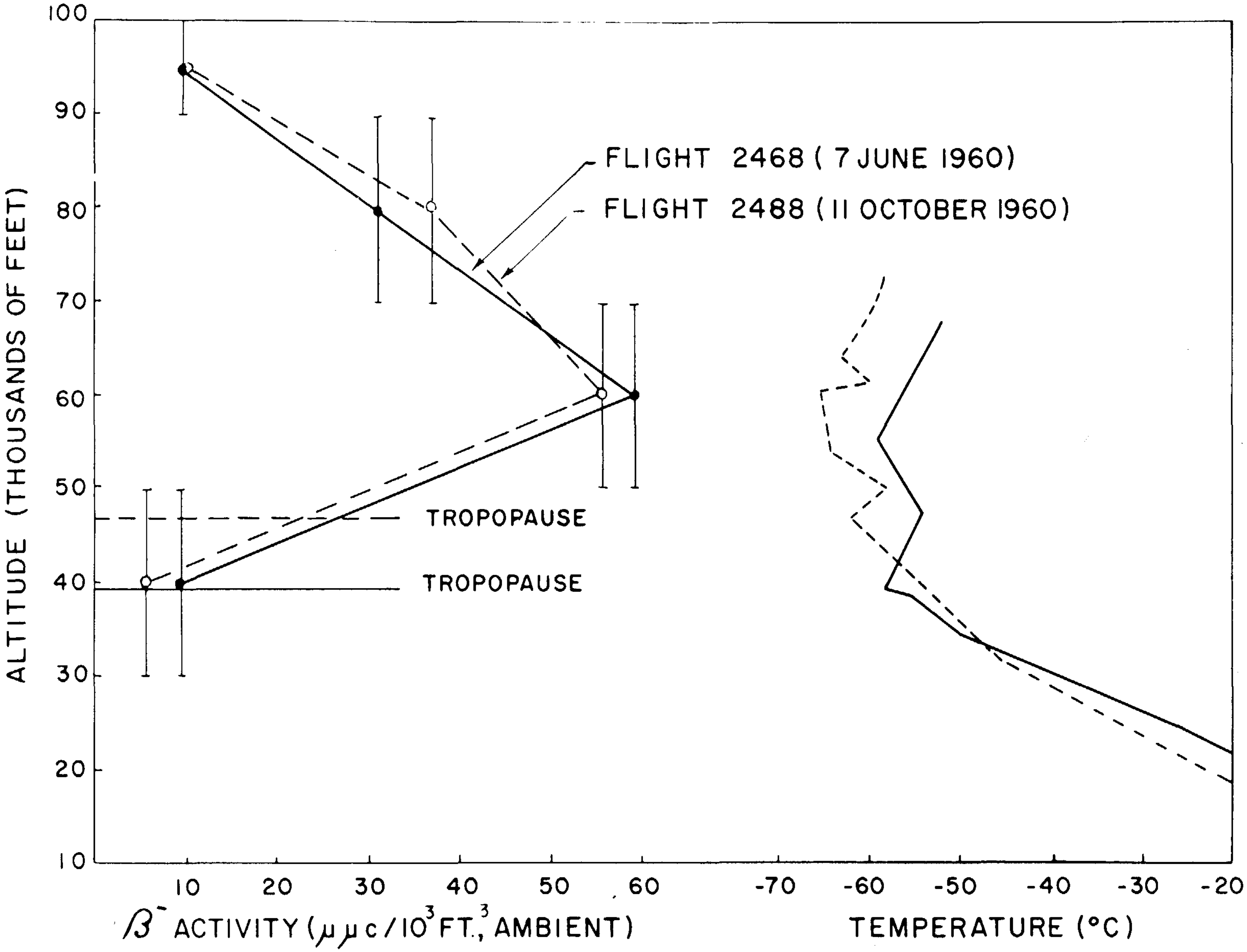

Fig. 1 -- Impactor-filter flights, Minneapolis, Minn.

Circulation Considerations

Recent observations[7, 8] of the stratospheric distribution of W^{185} and Rh^{102} shed considerable light on stratospheric circulation and mixing processes. The W^{185} activity, injected at several levels in the lower equatorial stratosphere from the spring 1958 Hardtack tests at 11 degrees north latitude, provides evidence against a Brewer-Dobson[18, 19] type of organized meridional circulation. From about September 1958 to the most recent observations, the maximum W^{185} concentration has persisted in the equatorial stratosphere at U-2 aircraft ceilings of about 20 kilometers. A typical latitude profile at this level, based on HASP data[20] is shown in Figure 3. The stratospheric distribution of W^{185} indicates that debris in the lower equatorial stratosphere mixes downward toward middle latitudes into both hemispheres mainly by the process of turbulent diffusion. The distribution of W^{185} at levels above 20 kilometers has not been assessed.

The Rh^{102} activity which was injected to great heights at about 16 degrees north latitude over Johnston Island in August 1958 shows a very different pattern of mixing and distribution.[7] A typical latitude profile for Rh^{102} following its buildup in the lower stratosphere is given in Figure 3. Debris from high-altitude sources apparently mixes downward into the low stratosphere selectively at higher latitudes during winter. The Rh^{102} data indicate very limited horizontal or vertical mixing into or through the lower equatorial stratosphere. The observed levels of Rh^{102} in the lower stratosphere indicate that most of the Teak and Orange debris is still present in higher layers of the stratosphere over both hemispheres.

For clouds in the equatorial stratosphere at heights greater than the main W^{185} source, it is expected that, with increasing altitude, the mixing pattern will shift from that observed for W^{185} to that exhibited by Rh^{102}. Thus the 1959 spring peak observed for W^{185} in Bedford rains[6] may reflect a Rh^{102} type behavior for the highest altitude portion of the multiple W^{185} source. Similarly, the Ivy and Castle clouds apparently exhibited a Rh^{102}-like behavior. Figure 4 indicates the apparent variation in the Castle component of Sr^{90} fallout during 1956 based on the isotope-ratio data interpretation of Peirson.[21] Uncertainties in the Peirson analysis are discussed elsewhere.[22] It is indicated that, at the higher levels of the equatorial stratosphere, the dominant transport pattern involves horizontal mixing to higher latitudes followed by downward mixing into the lower stratosphere at high latitudes in winter. Subsequent mixing into the troposphere follows the pattern for Soviet tests.[5, 6, 15] Thus the spring peak contribution of fallout from equatorial tests would appear to involve an altitude effect.

Summary and Conclusions

The evidence against an organized stratospheric circulation of the Brewer-Dobson type is convincing. It is also evident that the Teak and Orange debris, and possibly a large portion of the debris from Ivy, Castle, and other multimegaton test events, was injected into the stratosphere above aircraft ceilings and past balloon-sampling levels and may still reside there. Material balance estimates are too unreliable for estimating the residual contamination burden of the high stratosphere. Neither sedimentation nor organized circulation appear to be effective in the downward transport of high-altitude debris.

There appears to be no alternative to the direct experimental investigation of residual contamination by high-altitude sampling. Evaluation of the distribution of contamination in the high stratosphere will contribute

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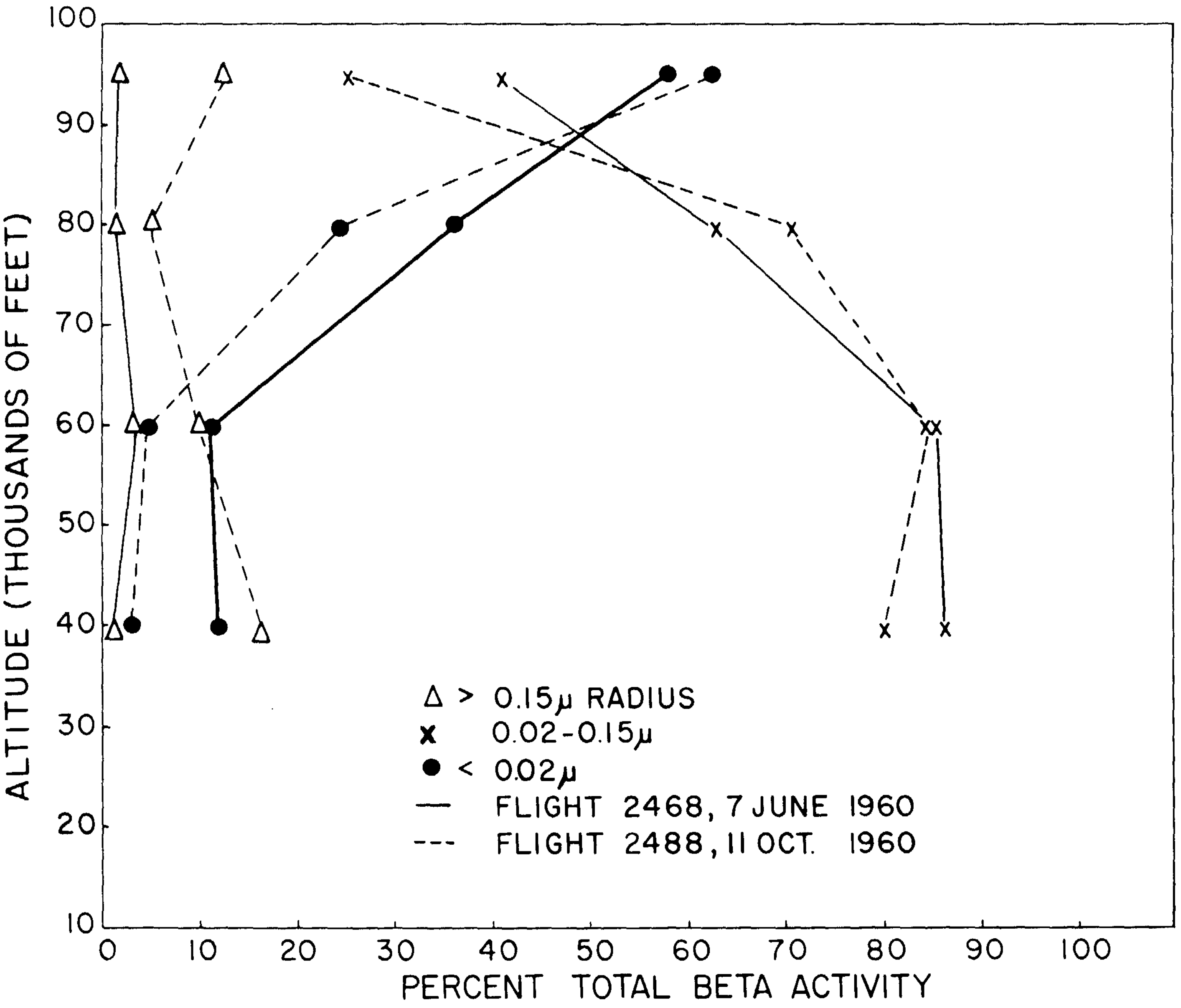

Fig. 2 -- Size distribution of fallout particles vs altitude

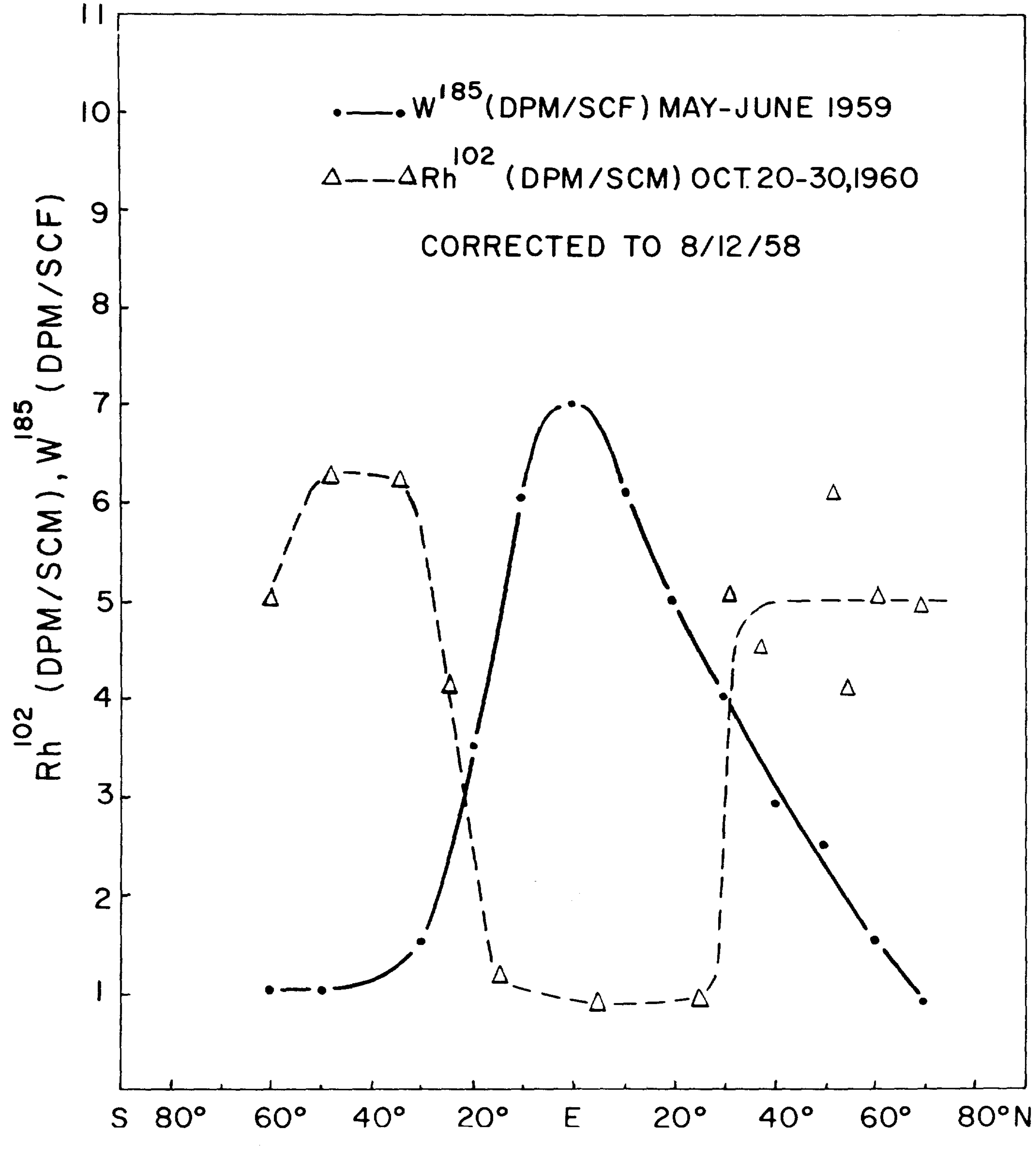

Fig. 3 -- Latitude distribution of rhodium-102 and tungsten-185 at ~20 km

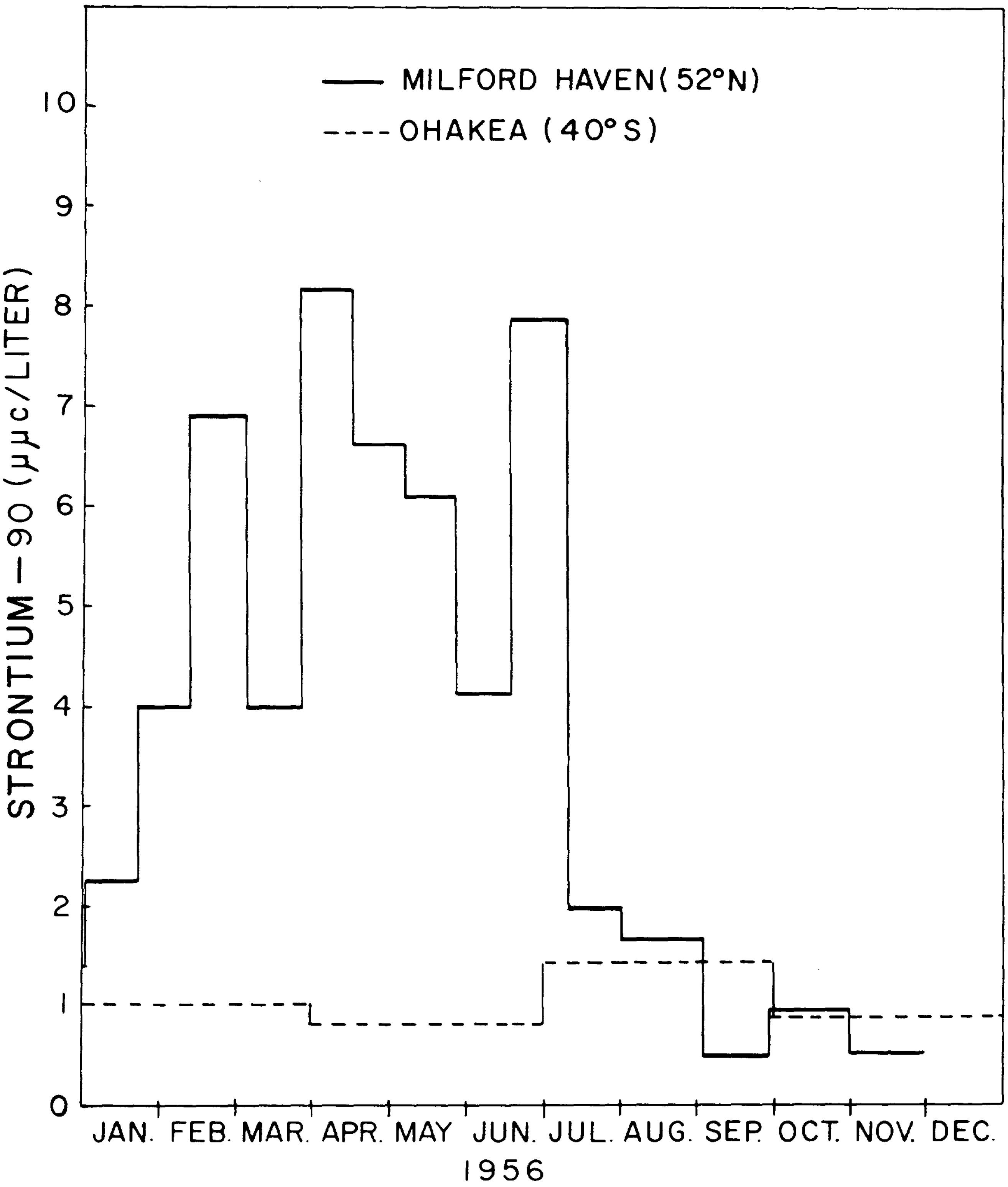

Fig. 4 -- Castle Sr^{90} in 1956 rains according to Peirson (AERE-R3358)

substantially to knowledge of atmospheric behavior. A high-altitude sampling capability will be useful in many other aspects of high-atmosphere research and will improve the possibilities of carrying out selected high-altitude tracer experiments.

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Discussion: Adequacy of Radioassay Techniques; Alternate Assay Methods

Panel: I. J. Russell, moderator; G. A. Cowan
L. B. Lockhart, J. P. Friend,
M. I. Kalkstein

Col. Russell:

I should like to start by making some personal observations concerning the adequacy of radioassay techniques. I think we are in pretty good shape. One can analyze for almost any nuclide produced in bomb debris and do it with pretty good precision. I think what is needed, is a little better understanding of how to treat data from a given laboratory satisfactorily in relation to those obtained from another laboratory. This state of the art has by no means been reached. Let me give you a couple of examples of what I mean. Three tungsten isotopes of interest were produced in the Hardtack series, and most people have concerned themselves only with one or two of the three. It is possible, if one looks at all three and knows production ratios of the individual sources, to arrive at some conclusions concerning how much of a given source is in one's sample. This has been done by only one laboratory; it is very unfortunate that samples have not been looked at from this point of view by others and substantiated or questioned as appropriate. Then, one could say that, given shots A, B, and C as principal producers of tungsten, source A is 18 times source B and about 1.2 times source C. This is valuable information. Sources A and C had about the same yield; source B had a very high yield, a yield that would, according to the figures Dr. Martell just presented, have put the cloud at an extremely high altitude - indeed above U-2 sampling capabilities. It leads one to question, as unambiguous, the attribution of such things as the transient increase in Strontium 90 to the downward excursion of Teak and/or Orange. I noted from Dr. Friend's graph that this represented something of the order of 30 percent increase. Cerium 144 produced in a high-yield shot is not going to be distinguishable, by very small differences in half-life or in detonation time, from the Cerium 144 produced by Teak and/or Orange. Mind you, these data were presented not on the basis of Rhodium 102, but on the character of the ratio _per se_. Practically everybody has looked at Strontium 90, Strontium 89, and Cerium 144. It is nice to know one's numbers have a good relative-absolute sense such that from the Strontium 89, for example, you can say there are so many fissions attributable to a given source. If the fissions based on a Cerium 144 source represent a significantly larger number than those inferred from Strontium 89, one can subtract to get excess Cerium 144 fissions which now have to be Hardtack and things prior to Hardtack. To do this, however, one has to be on an absolute-relative basis and believe that the subtraction of two numbers close to each other can give significant results. I have, taking the liberty of using somebody else's data, done this. And for 1959, a rather peculiar feature is observed between 40,000 and 65,000 feet. The ratio of atoms of Rhodium 102 corrected to time of production, assuming time of production was Orange, to fissions attributable to anything earlier than the Soviet series, decreases with time. Rhodium 102 to Cerium 144 fissions are decreasing with time, and this ratio has a value which is 2 percent of the production ratio in Orange, if one can believe the number for Rhodium 102 production that Dr. Cowan has quoted.

With regard to the adequacy of radioassay techniques, as I said, I think they are good, but I think calibrations have to be polished up considerably so that any laboratory could look at a given sample and say with confidence that they have so many fissions attributable to this or that source.

Dr. Martell:

May I comment on your remarks about tungsten isotopes differing for various sources? We looked at this briefly but, unfortunately, the only two isotopes that have been studied to any extent are 181 and 185, which show a maximum variation of a factor of 2 if you examine their production in the single, very high altitude shot, and consider the total production of each of them in the lower altitude shots taken as a group. They don't differ very much in altitude. A factor of 2 isn't very much to play with if you are attempting to make an inner comparison to get ratios for these two isotopes and other components of these two sources, especially when you add to this the fact that you are comparing radioassay data from two or more laboratories.

Col. Russell:

That is true. However, if you remember, I mentioned that the Tungsten 188 went into the analysis and that the Tungsten 188-to-185 ratio differed by a factor of something like 5 or 6. The high yield tungsten-bearing shot would be expected, I think, to go considerably higher than the others.

Dr. Kalkstein:

One thing with regard to Tungsten 188 is that is was produced in a lesser amount, by a factor of about 10, than 181 or 185. This makes 188 a difficult measurement, particularly if we are not seeing it yet. Looking at our radium data in the early days and worrying about the amount of tungsten was a means of saying that this was Orange. In the early balloon samples rhodium went up somewhat, but tungsten just wasn't there. With Tungsten 185 and 181 as with Rhodium 102, you don't have to know much about the tracer you are using if you want to do just relative measurements. Conversely, if you want to make comparisons with other isotopes - if you want to compare with other labs - then it is rather important to know the decay scheme and how you are going from cpm to dpm to atoms. Tungsten 181 is particularly bad in this respect, and I suspect that there will be very poor agreement between laboratories. The reason is the following: If one looks at the table of isotopes, one finds the best value for the L-to-K electron ratio. Tungsten 181 decays by electron capture and both L and K X-rays are emitted. If you count K X-rays, which are what most of us are counting, the energy is about 57 kev. If you want to talk about dpm, you have to correct for the L X-rays. This reference, let's say, gives an L-to-K ratio of 1.5, so, you have to multiply K X-ray count by 2-1/2 to compute the number of electron capture events or the total X-rays out. Actually, there is some reason to doubt this reference. Early work has shown that there were gamma rays of about 130 kev and 150 kev in low abundance associated with Tungsten 181. The man who made measurements of the L-to-K ratio said that he looked for these gamma rays and didn't find them. He then actually measured L X-ray abundances and K X-ray abundances, and came out with the L-to-K ratio of 1.5. These measurements are very difficult to make. It is a difficult experiment, and my feeling is that it was poorly done. The implications are the L-to-K ratios are the best means to use where there aren't other means available for measuring disintegration energies for electron capture. The L-to-K ratio also led this author to disintegration energy of 90 kev. If there really were 150-kev gamma rays subsequent to decay, then energy for disintegration between ground states has to be greater than 90 kev, perhaps greater than 150 kev. This L-to-K ratio as a function of energy is fairly steep in certain regions. The current best values of L-to-K ratios have been measured at Livermore; they saw the gamma rays and came up with a figure of 0.23. Thus, depending on what people use to correct their X-ray counts, they will multiple by 2-1/2 in one case, and by 1.23 in another, and come out with a factor of 2 difference on this basis alone. This is an example of some of the things that one is liable to get into.

Col. Russell:

I think you would agree, wouldn't you, Marv, that despite the complexities of units, decay scheme, and Tungsten 181, you should count the thing in such a way that it is completely comparable, laboratory to laboratory, after you have worked out your fudge factor.

Dr. Kalkstein:

Les Machta showed curves of dpm. You are talking about atoms. I think if the data had remained in cpm, then this problem wouldn't arise. Further, there is always going to be confusion unless one makes clear what factors are being used. If one laboratory compares atoms of Tungsten 185 and 181, the manner in which they arrive at values in atoms should be investigated initially and standardized.

Col. Russell:

The solution, of course, is to have some laboratory responsible for production of these nuclides. Say they claim that they produced 2.0×10^{23} atoms. Maybe they didn't. They give you a sample which you measure and you get so many cpm. You then have a factor from belief of the 2×10^{23} atoms. I don't see any reason for confusion in this area. You can get confused on decay factors when you get into fission products, because yields can vary from device to device for a given fission product; certainly fractionation plays a large role in how validly you can convert from cpm to atoms or fissions, even though you know the counting efficiency of a given assembly quite accurately. I think a greater emphasis on dealing with things which are time-independent, and on getting back to the fundamental data, as recorded by the tracer producer, would be a very good thing. For example, today some things are recorded in dpm per standard cubic meter and cpm per standard cubic foot. I prefer to talk in terms of fissions or atoms assignable to given events.

Dr. Kalkstein:

I think all you really want to ask for is that people be very explicit about what they have done and how they get their numbers. If you have this, then you can apply your pencil.

Floor:

Dr. Machta showed a table which indicated certain sensitivities of analysis. In most cases you can resolve one dpm or perhaps one-half a dpm. Based on this, you need samples for various nuclides ranging from a few to a few thousand standard cubic feet if you are above 100,000 feet. At 160,000 feet the requirement would be 1000 times this, and you would need something like a few hundred thousand ambient cubic feet sample. Marv said that if you have an area as small as 100 square centimeters or perhaps a square meter you could do something with the sample collected. There is a lot of difference between 100 square centimeters and a square meter. If you have a square meter and you go through a 5-kilometer layer where we can, at the moment, neglect density variations, you have 5000 cubic meters (about 150,000 ambient cubic feet); with 100 square centimeters, you would have only 1500 ambient cubic feet, and you are two to three orders of magnitude short of the sample that Lester says you have to have. My question is, if somebody tells you that he could get a 1000-ambient-cubic-foot sample, but he can't get 100,000 ambient cubic feet, would you say, "Don't collect the sample?" Or would you have some ideas of what you might be able to do with this sample?

Dr. Kalkstein:

I can't at the moment, without pulling out a pencil, even relate the volume. I think in dealing with this problem it is more meaningful to talk about what fraction of the earth's surface is covered by the cross section of the column you are going to collect. Depending on some of these things, talking in terms of volume assumes that what you are looking for goes as the density. I hope that for some of the things of interest this won't be so. For this reason, I think that whether the sought particulate goes with the density or not, you know where you are if you talk about sweeping out an area.

Floor:

What do you want, 100 square centimeters or a square meter? I think 100 square centimeters is very reasonable to talk about without an awful lot of complicated technology. If you have to sweep out a square meter, this is much harder, and, if you have to sweep out 100 square meters, then I think we are going to have to start talking about windows and some of the things that we hear about.

Dr. Kalkstein:

People are already playing with samplers for different experiments, looking for micrometeorites and such, in which they do have on the order of a square meter. I wouldn't say they have been successful. They unfold the nose of the rocket. You easily can expose the square meter and just carry it up after you are high enough to avoid disturbing the rocket's flight.

Dr. Lockhart:

One thing more that I think should be considered here is what, specifically, you are looking for. Are you looking for a general something that is up there now, or are you going to look for something specific that may have very low concentration and there is nothing you can do about it?

Floor:

Let's say that, of all the things that Lester put on the slide, we can get three of them; then we could start studying these and new things that get put in. With this we might hope to get additions in terms of some of the constituents already there. What size sample do you need? Or, do you feel that there is a potential for increasing sensitivity one or two orders of magnitude and in so doing be able to use samples smaller than those that your present methods would allow?

Dr. Lockhart:

You can increase sensitivity of radioassays. I think the limits that Les indicated were pretty good on most of those things. One count per minute per sample is going to be just about the lowest limit for beta counting with a 10-percent standard deviation. You are assuming then that your chemistry and everything else is perfect; that is going to be a little hard to do. This is just about it on a statistical basis alone; there is not a lot you can do to improve it.

Floor:

I would agree generally with the numbers on activity requirements per sample that Les mentioned. I don't feel that it is at all necessary to agree with respect to the area of sampler body. This is so sensitive to the vertical distribution of material we are concerned with in these higher layers. Let's assume, just for the sake of reference, that we have one megacurie of activity in the high stratosphere, held up in a rather shallow layer. One megacurie would give you about 100 disintegrations per minute per 250 square centimeters integrated through the middle. This gives you 100 times the activity you need to measure Strontium 90 to 10 percent, using a discriminating low alpha-beta technique. Thus, you can see 10 kilocuries. Further, if you relax to 20 to 30 percent - in the case of Strontium 90, if you push present techniques, and if you put research effort toward this improvement proportionate to the importance of the cost of analyses - you might beat this by a factor of 4, because you are dealing with quite hard data and you can make some other improvements. If you depended upon chemistry alone you could beat it by an order of magnitude, but if you want to discriminate by decay schemes, you are stuck in the range of 3 or 4 to 10 with the best possible improvement. I think one dpm is a good limit, therefore, for betas, and you would probably have to get to five, ten, or more dpm, depending on the decay scheme, for other things, particularly if you want to get a discriminating thing. You see, if we have a concentrated layer in some region of the upper atmosphere we can easily pick up small fractions adequate for counting of 1 to 250 square centimeters. If things are not measurable at these levels, they are either of no consequence or our assumptions of distribution are erroneous. So we have to do this at more than one point in space before we can be sure.

Col. Russell:

I'd like to mention another possible assay method here: mass spectrometry. As you are probably aware, the mass spectrometer has reached a pretty high state of the art and is going to do pretty well at 10^{-11} gram samples. With regard to the question that Ed Martell proposed, of getting hold of the sample at 150,000 feet, or something like that, with the hope that one could detect an upper reservoir which may still contain significant quantities of some of the earlier very high yield thermonuclear tests, there have been some data published recently which give some information on the multiple neutron capture products resulting in very high flux gadgets. One which is quite accessible, for example, is Plutonium 242. About 50 percent of all the Plutonium 242 -- if my figures are correct -- produced to date was produced during our earlier thermonuclear test series. So that by examining the isotopic ratios of Plutonium 242, 241, 240, and 239, I think one might be able to assign a certain fraction of the debris to the earlier tests, say Castle, if any is still up there. Although this isn't in the realm of high altitude discussions, I think, too, examination of ocean waters and ocean sediments for plutonium would lead to some rather interesting results and might, in fact, give us a little better balance than we now possess for total fissions we know we produced.

Dr. Kalkstein:

I might add just one thing to this: we are looking at this from a somewhat different angle. We are not qualified as mass spectroscopists; we are not equipped, as it is expensive. There are new means of alpha counting. Just about all of these things decay by alpha emission, or a certain fraction of the decays are by alpha emission. By separating out different elements, even where there are differences in the production of the heavy elements, one gets some discrimination, and then, even within a given element where there may be several isotopes decaying by alpha emission, there are means now by solid state devices where one can have a relatively cheap detector and one with sufficiently good resolution that one could make these measurements by alpha counting. There is some literature on both the method and results, and the first thing is to look at the results and get what you can out of them. We are trying to do this, too. It may turn out that we don't have to do much more measuring if the results give us what we want.

Col. Russell:

Are any of the particles you have been getting in the impactor-type samplers sufficiently large to be susceptable to electron microbeam analysis?

Dr. Friend:

There are particles ranging up to about 10 microns radius. The population is very low between about 1 micron and 10 microns radius. These particles appear to be of different nature from the persulphate which I showed earlier. Of course, there is speculation that these might be micrometeoritic material. Some of the sulphate particles do get up to around a micron radius or so, and these would also be amenable to electron microprobe.

Dr. Kalkstein:

This is being done; some of the sulphur data are just by X-ray fluorescence, but people are looking also at individual particles by electron microscope.

Dr. Friend:

For an individual particle, I don't think anyone will ever quote less than 1-micron-diameter particles. You need a couple of microns or a big collection of many small particles.

Floor:

You are really saying that particles you can analyze are the background, natural aerosols in the stratosphere. We can discriminate the radioactive particles from background particles directly, and the preliminary data show that we have two quite different systems. I don't think that analysis of any of the particles over the particle spectrum would help you to determine constituents associated with the radioactive debris.

Dr. Friend:

If we take the natural stratospheric aerosol that we have right now and look at these particles, the signal we get is all sulphur from the electron microprobe. There is so little mass of anything else there, although the Strontium 90 is detectable, that you just don't see that. Of course, if we are talking about concentrated fission products and no other solid material in the upper atmospher around 150,000 feet, then with a large collection of many small particles, or a collection of just a few middle-sized particles around 0.2 micron in radius, there would be a possible chance of analysis and classification.

Col. Russell:

I was thinking of this in terms of a possible application to study of materials produced by burnup of a re-entry vehicle.

Dr. Friend:

Right. I think that that is one of the important problems faced by this meeting, in that the mesosphere is essentially the place where re-entering vehicles will burn up, that is, between 50 and 80 kilometers.

Floor:

One might comment that, if this is possible, the place to look is well above the layer that you have been studying. The background should be less. All this is assuming that we do have a preferred region of holdup or layer of holdup at higher levels.

Dr. Friend:

According to Chris's (Junge) balloon data, the number concentration drops off very rapidly above 75,000 feet. Of course, we haven't made too many measurements at 90,000 feet to determine exactly what is going on there and above it, but the problem with re-entering vehicles is: can you get them to burn up in a significant fraction so that you know what you are studying? This is also the problem of injecting a tracer at these altitudes. If you could be guaranteed that it is all going to be nonsedimenting material, it would be glorious; but, if you have the sedimenting material mixed in with it, the resulting problem is difficult.

Floor:

There is the problem of what is a micrometeor material background that you are contending with even here. You have a large enough mass that is unique enough at high atmosphere to discriminate this particular source.

Dr. Friend:

This is essentially right. I might put in a plea from the people who are studying micrometeorites: Please, let's not put the elements in the higher atmosphere which will confuse the micrometeorite problem.

Dr. Kalkstein:

To get back to the last question, in a more general sense of this panel - the use of this method as an alternative technique of assay - let me add a hopeful comment. When we talk about 1-dpm beta as a limit of what one can do, appreciate that this figure was gotten by lots of experience in low-level counting. Part of electron microprobe work is the count, and the people, so far, in electron microprobe work are not low-level people as far as counting techniques are concerned. I think that a lot can be done. As a matter of fact in the early days, at least with some of the work done with Junge's samples, we have been able to suggest things and see a lot of improvement in some of their results. I think that there is some hope that, whatever we talk about as a limit, whether it is a 1-micron particle, or whatever, if it were proven valuable and worth while, microprobe analyses could be greatly improved by applying some of the knowledge gained in low-level counting.

Floor:

There was a statement about re-entry vehicles burning up at certain altitudes. Can you restate that?

Dr. Friend:

I said that, as far as I have been able to determine, the mesophere is where the peak burnup occurs.

Floor:

I don't think you would find general agreement on that.

Dr. Friend:

This is where they would like to plan to have it burn up.

Floor:

How are you defining the mesophere?

Dr. Friend:

Fifty to eighty kilometers.

Floor:

This is far above any place where any normal re-entry vehicle would burn up.

Dr. Friend:

The burnup is actually lower than 50 kilometers?

Floor:

Yes, 30 kilometers or less.

Dr. Friend:

For things like SNAP we are talking about a 1-degree entry angle at 25,000 feet per second.

Floor:

This is fine, but you have to do something pretty drastic to make something burn up at 80 kilometers.

Dr. Friend:

I can at least say where I saw it. It's in the Physics and Medicine of Outer Space, the book edited by the people from Brooks. There is an article on re-entry in there and, from what I gathered, the plan was to try and make maximum burnup occur somewhere in the mesophere.

Floor:

Getting back to adequacy of radioassay techniques, I'd like to request comments on this: Dr. Machta has shown us what possible levels we might be able to attack, and Dr. Martell pointed out that if we had some very low-yield nuclear bursts put a few kilotons of fission product debris into the higher atmosphere (assuming the atmosphere is perfectly clean, that is, without any background material) we should be able to detect the fresh debris. The question is: what kind of background presently exists and how long do you feel it will exist to obscure some of these low levels that Dr. Machta suggested as measurable?

Dr. Friend:

Have you considered neutron activation?

Col. Russell:

Neutron activation is so sensitive that it poses background problems.

Dr. Lockhart:

What kind of neutron activator do you have on the bomb itself that these things were exposed in, unless you wait long enough for that original activity to decay?

Col. Russell:

There are many things, about a kilogram of which, if irradiated to 30 days or to top of saturation (whichever comes first) with about 5 x 10^{14} neutrons per square centimeter per second, are detectable in 10^{-14} microgram quantities. In other words, if you put up a kilogram of material and you get yourself a very high rocket fracture back, or whatever you want to call it, you can detect it; you get something like 10 counts a minute on activation. The problem, though, is: I challenge anybody to select something that isn't already up there, radioactive in many, many times these quantities.

Dr. Martell:

This is a problem, Irv (Russell), but we have looked at it in a couple of more sensitive cases. Take gold or europium, for example, which yield activation products on a very high cross section. You can, in principle, see 10^7 atoms, which means that only a 10^{-16} sample of a uniform layer is necessary, and 200 square centimeters is adequate in principle. The main point is: what is background? There are ways of getting around this. Let's assume that we have to go to 100 or 1000 kgm - this is still feasible to carry - to match backgrounds, and we are looking at high levels where these backgrounds are quite low. It is not out of the question that one could carry on such experiments. Europium is a very interesting case. In the background you can distinguish it because the distribution of rare earths in vaporizing meteoritic material is likely to be very consistent. However, it may not be exactly what we have seen in the earth's crust or in certain specific types of meteorites. So all we need to do is measure the effects and change of ratio, due to an addition of one added carrier in this region, and look at two. So, say you change the potential for gathering Europium by a factor of 2; you can discriminate the added tracer in that sample. You push the total mass of the tracer up to the point where you can get a signal above the background. The background work certainly has to be done, and this is a tough low-level problem, more because of the background in the collector than from what may be the high stratosphere.

Dr. Kalkstein:

I think the natural background can be taped just by looking at what is there and putting up more than there is by a large factor. We have been thinking of ways of suggesting cooperative experiments with the Russians, and, if we have to go to large payloads, maybe this is the way we can cooperate. We also have neutron-activated IPC tape, and we find that everything is in IPC paper. The difficulty with neutron activation is that, for certain elements anyway, it is so sensitive that you have really got to be careful how you handle your samples, what is in your samples, and what material you use to collect your samples on. I really think that this is where the big problem is if one wants to get into this.

Dr. Friend:

For instance, I happen to have some results. In the experiment that we did, we used Mylar to wrap up the ash samples. We analyzed the Mylar, which you might think to be very clean material. This was all handled with rubber gloves with no powder to put on them, and in as dust-free an atmosphere as we could manage. Copper came out 10^{-8} grams; zinc, 10^{-7} grams; phosphorous, 10^{-7} grams; calcium 10^{-5}, grams; cobalt, 10^{-9} grams; and so on. We did not measure uranium.

IV. FUTURE DEBRIS INJECTIONS

R. S. Claassen, presiding

(C)

Some Aspects of the LASL Rover Flight Safety Program

L. D. P. King
Los Alamos Scientific Laboratory

The ultimate usefulness of nuclear reactors for rocket powered space flight does not depend only on the technical demonstration of a reactor which has satisfactory thrust and performance characteristics. Of equal importance is that one can demonstrate beyond any doubt, to government agencies concerned, that all public safety requirements are met. One must not overlook the fact that unsolved safety problems can seriously slow down the ultimate use of nuclear rockets and may require changes in design concepts.

The potential gains from the use of nuclear energy in space must be evaluated in comparison to the risks of physical or financial damage. It is the responsibility of governmental bodies to regulate and evaluate hazards to the public in such a way as to minimize the risk and maximize the gain. It should be the responsibility of the technical staffs of research organizations involved in the nuclear rocket program to supply the necessary technical data for a correct evaluation of the safety problems.

The Los Alamos Scientific Laboratory recognizes the variety of skills and general complexity of the safety problems arising from the use of nuclear rockets. Since the successful completion of the Kiwi A series of propulsion reactor tests, the technical feasibility of nuclear rockets motors has been demonstrated. Increased effort on solving the safety aspects of flight and operational use of such devices now requires additional effort.

A Rover Flight Safety Committee has recently been formed at the LASL with the express purpose of studying and coordinating the contributions the laboratory can make towards the solution of nuclear rocket flight safety problems. Substantial work dealing with Rover safety had already been done at the laboratory prior to the formation of the committee, but this was done more or less on an individual interest rather than a laboratory wide responsibility basis. For example, the classified document, LA-2409, Nuclear Safety Aspects of the Rover Program, was issued in March 1960 and has provided most of the presently available reactor technical data directed towards Rover safety considerations.

The Rover Flight Safety Committee has been working on a program which seeks to identify safety problems which can be foreseen and evaluated so that a suitable experimental and theoretical program can be pursued. In particular, the LASL will be concerned with aspects of reactor core operation and design. An effort is being made to place emphasis on those items which appear to have the greatest urgency.

The most difficult, and probably the most important problem at present, appears to be the disposal of a nuclear rocket reactor core and its components on return from orbit or ballistic trajectory when the point of impact is not known. This is particularly true if the present safety rules required of the engine contractor cannot be modified. The requirement that no reactor or large fragments thereof re-enter and impact on the earth's land masses may be troublesome or impossible to guarantee under all conditions.

Work planned, completed, or in progress at the LASL dealing with this type of disposal problem include:

1. Destruction by nuclear explosion.
2. Destruction by chemical explosion.
3. Post operational additives, and if necessary, core design changes to assist in core disposal.
4. A careful evaluation of whether the present stringent disposal requirements can be modified.

A second important consideration which requires immediate attention is the evaluation of launch pad hazards during nuclear rocket tests.

The following program either in progress or planned at the LASL should contribute to the solution of this problem:

1. A series of deliberate destructive excursions. These will probably consist of one or more of the following core experiments:
    - (a) Water flooding
    - (b) Abnormal liquid hydrogen flow conditions
    - (c) Sudden coolant shut off after full power operation
    - (d) Abnormal control rod insertions
2. Nondestruct excursions to assist in evaluating which destruct excursions should be performed, and in providing basic reactor response information.
3. Dose and dose rate measurements during an excursion.
4. Fallout measurements after an excursion.
5. Radiation source characteristics as a function of time during an excursion.
6. Experimental and theoretical criticality and transient studies such as:
    - (a) External reflector effects
    - (b) Critical mass and radius determinations which might be created by an accidental core drop
    - (c) Effects of hydrogen additions to the core
    - (d) Control rod effects
7. The behavior of fuel elements both structurally and with respect to fission product release under abnormally high temperature conditions.
8. Operational reliability studies.
9. Theoretical radiation environmental studies.
10. Refinements in fallout and air contamination determinations.
11. Fission product leaching from used fuel elements.

Of particular interest to this meeting would be a knowledge of the time rate of release of fission products and their dispersion in space for both normal and abnormal operating conditions. This information is not known at the present time, and must await (1) measurements to determine the characteristics of the final fuel elements, and (2) a choice of the most feasible method for the destruction and dispersion of the core and associated components on re-entry.

Preliminary work has indicated that destruction by a nuclear explosion is not promising. Work is now under way with scaling experiments to determine the shatterability of core materials by a chemical explosive. This exploratory work should indicate what chunk or particle size is to be expected from a chemical explosive detonated inside of the reactor core. This information is needed to permit calculations on the burnup of materials prior to rocket re-entry. Some sort of rather complete dispersal is probably essential when the rocket impact area is unknown.

Studies are continuing on the normal rate of release of fission products from Kiwi reactor fuel elements. These will also be carried to abnormally high temperatures to test the maximum attainable release by such processes as after heat with or without chemical additives.

If burnup on re-entry cannot be achieved by some combination of explosive, high temperature, and chemical means to satisfy the final re-entry safety criteria, core design changes may be required.

V. INSTRUMENTATION AND EXPERIMENT PHILOSOPHIES

J. D. Shreve
Presiding

(A)
Sampling the Upper Atmosphere for Particulate Matter

M. Markels, Jr.
Atlantic Research Corporation

Introduction

Sampling the upper atmosphere for particulate matter is necessary to predict accurately radioactive inventory in the atmosphere and its rate of descent or fallout. Currently, high-altitude balloons carrying a blower and filter unit can sample the upper atmosphere to about 100,000 feet. But analyses of these samples, together with world-wide fallout studies, suggest that considerable radioactivity is present at altitudes above 100,000 feet.

Atlantic Research Corporation, under Contract AT (30-1)-2404, entitled, "Sampling the Upper Atmosphere for Particulate Matter", has evaluated various particle-collection systems for sampling in the altitude range extending from 100,000 to 200,000 feet. Since a rocket appears to be the most practical vehicle for reaching these altitudes, the feasibility of the sampling method selected was determined on the basis of use with a rocket vehicle. To sample the required 1,000 std cu ft of air, supersonic impactors which use the velocity of the rocket vehicle for removing the particulate matter from the air are impractical because of the excessive drag imparted to the rocket. The low density of air above 100,000 feet requires a long sampling time to avoid supersonic flow and large pressure drop through any duct-type particle collector. Therefore, a device must be added to slow the fall of the particle collector. A second consideration resulting from the large sample size is the large amount of work which must be expended to move the air through the sampling device and to separate the air from its load of particulate matter. The cost and complexity of rocket vehicles capable of moving large payloads indicates the desirability of using as large a fraction as possible of the potential and kinetic energy of the rocket in the sampling operation.

A rotorchute with impactor arrays can collect the required sample efficiently. A rotorchute is a bladed device which operates like a gliding helicopter or Autogiro. Energy from the vehicle descent provides the power to rotate the blades. The blade lift in turn slows the fall of the vehicle to descent rates only slightly faster than those of parachutes of the same over-all diameter. The energy which keeps the blades rotating can also be used to move impactors through the air which collect the particles much the same way dust collects on a fan blade. The large mean-free path of the air molecules at 100,000 feet and above greatly increases the efficiency of impactor devices, and such a rotorchute and impactor system gives a simple method for processing large volumes of air.

It is essential to know the amount of air available for sampling in the region from 100,000 to 200,000 feet. The density of air decreases exponentially with altitude as shown below:

Altitude (ft)	Density (lb/cu ft)
0 (at sea level)	0.0765
50,000	0.0116
100,000	0.0010
150,000	0.0001
200,000	0.00002

The weight of air existing in a column of one square foot area limited by various altitudes can also be calculated as follows:

Altitude (H) (ft)	Weight of Air (M/A) (lb/sq ft)
200,000 - 150,000	2.8
150,000 - 100,000	20.6
100,000 - 73,300	55.9
150,000 - 73,300	76.5
100,000 - 68,500	76.5
60,000 - 51,400	76.5

This shows that a perfect ducted sampler with a one-square-foot throat would only sample 2.8 pounds of air while falling from 200,000 to 150,000 feet. The total amount of air sampled from infinity to 200,000 feet would only be 0.47 pounds.[1]

Feasibility Study

To select the most promising sampling method the study was divided into two parts -- air sampling methods and particle collecting methods. Air sampling is concerned with the technique of insuring that 1000 std cu ft are actually processed by the device. The particle-collecting technique must insure that at least 50 per cent of the particles in the air sample are collected.

Air-Sampling Methods

Two air-sampling methods were thoroughly analyzed -- a parachute sampler and a rotorchute sampler.

Parachute Sampler. A rocket has two major disadvantages as a vehicle for atmosphere sampling: short sampling time, and small payload. To overcome these disadvantages, a parachute system was considered in which the parachute would act as a scoop to collect the air and also to increase sampling time by limiting the rate of descent. A duct and sampling device would be located at the vent hole in the center of the parachute. Increased stability could be obtained by placing a second parachute above the first, with the first functioning merely as a scoop or air-collector.

Design calculations[2] indicate that this method of sampling is feasible, but a major disadvantage is the inefficient use of the rocket's potential energy to overcome friction in the flow of air over the canopy. This reduces the energy available to force air through the collector scoop and creates the need for a pump or blower in addition to the parachute to process the required amount of air.

Rotorchute Sampler. In view of the shortcomings in the parachute method, the rotorchute method of sampling was studied. A schematic diagram of a rotorchute is given in Figure 1. A rotorchute is a rotating bladed device which operates like a gliding helicopter or Autogiro. The rotary motion of the blades can be used to power impactors which sweep the particulate matter from the air. These impactors can be mounted under the blades, within the blades, or they may protrude from the nose cone separately. The rocket motor case provides some stability to the rotorchute in flight. The general operation for this device would include firing the rocket vehicle with sampler to the maximum altitude desired. The blades would then extend and the sampling would take place during the drop to the lower level of altitude desired. The impactors would then retract and the blades or a small parachute would return the entire device back to earth.

To illustrate the feasibility of the rotorchute for upper atmosphere sampling, the theory of operation and sample design calculations for a practical rotorchute were investigated. In the example used, a gross weight of 100 pounds is assumed for the burned-out rocket motor plus payload at the start of sampling, and the impactors were assumed to be mounted on brackets below the blades. A 5-bladed device with 5 impactors per blade was taken to represent a reasonable maximum in collector area. Since a rocket plus nose cone having a 100-pound burnout weight will generally be about 17 feet long, the blades are taken to be 14 feet long. Particles as small as 0.01 micron in diameter are to be removed from the air with better than 80 percent efficiency. The sample is collected in descending from 200,000 to 100,000 feet. The density of collected particles is taken as 128 lb/cu ft. Calculations give a simplified relation for the rate of descent of a rotorchute with forward motion as

$$V^*_R = 13\left(\frac{W_T}{\pi r_o^2}\right)^{1/2}\left(\frac{\rho_o}{\rho}\right)^{1/2} \tag{1}$$

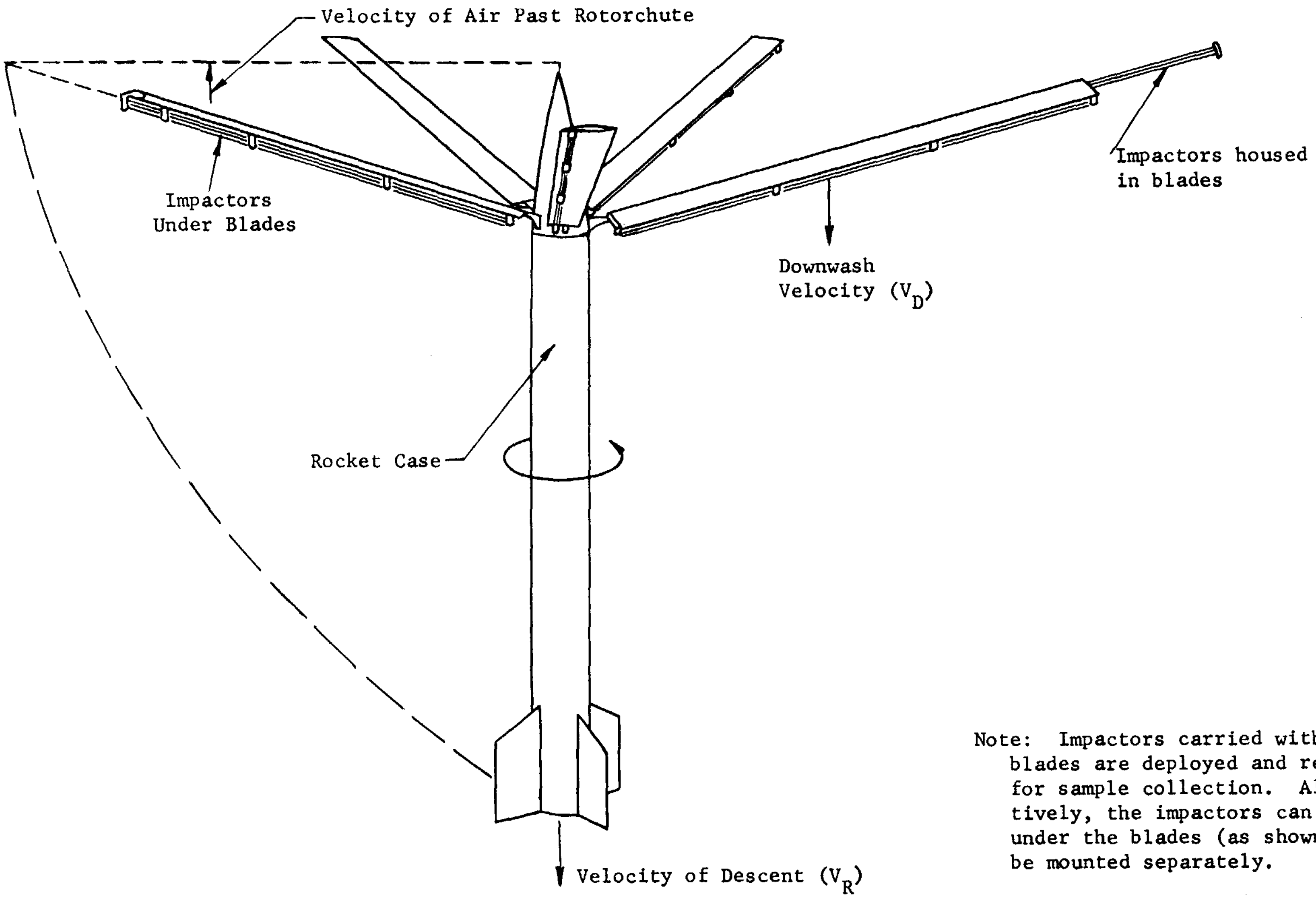
Particle Collecting Methods

Five methods of collecting particles have been analyzed for use with either the parachute or rotorchute samplers.[2,3] Of these, three are by precipitation-thermal, electrostatic, and centrifugal; one is by filtration, and one is by impaction. Since the impaction technique is the most practical of these to use with the rotorchute, it is the only one described in detail here.

The impactor should be designed so that the minimum size of particle is removed from the air stream at maximum efficiency while processing the maximum air volume. Work by Stern, et al,[4] has indicated that a focusing array impactor gives collection efficiency at low values of the inertia parameter, K, defined by

$$K = \frac{k_m \rho_P d_p^2 V_B}{9\mu L} \tag{2}$$

The collection efficiency versus inertia parameters, as given by Stern, is shown in Figure 2. This graph was computed by Stern using an electrical analogue technique employing an electrolytic tank and an analogue computer to determine the particle trajectories in the vicinity of the impactor surface. The focusing array, because



Note: Impactors carried within the blades are deployed and retracted for sample collection. Alternatively, the impactors can be mounted under the blades (as shown) or may be mounted separately.

Fig. 1 -- Schematic of Rotorchute

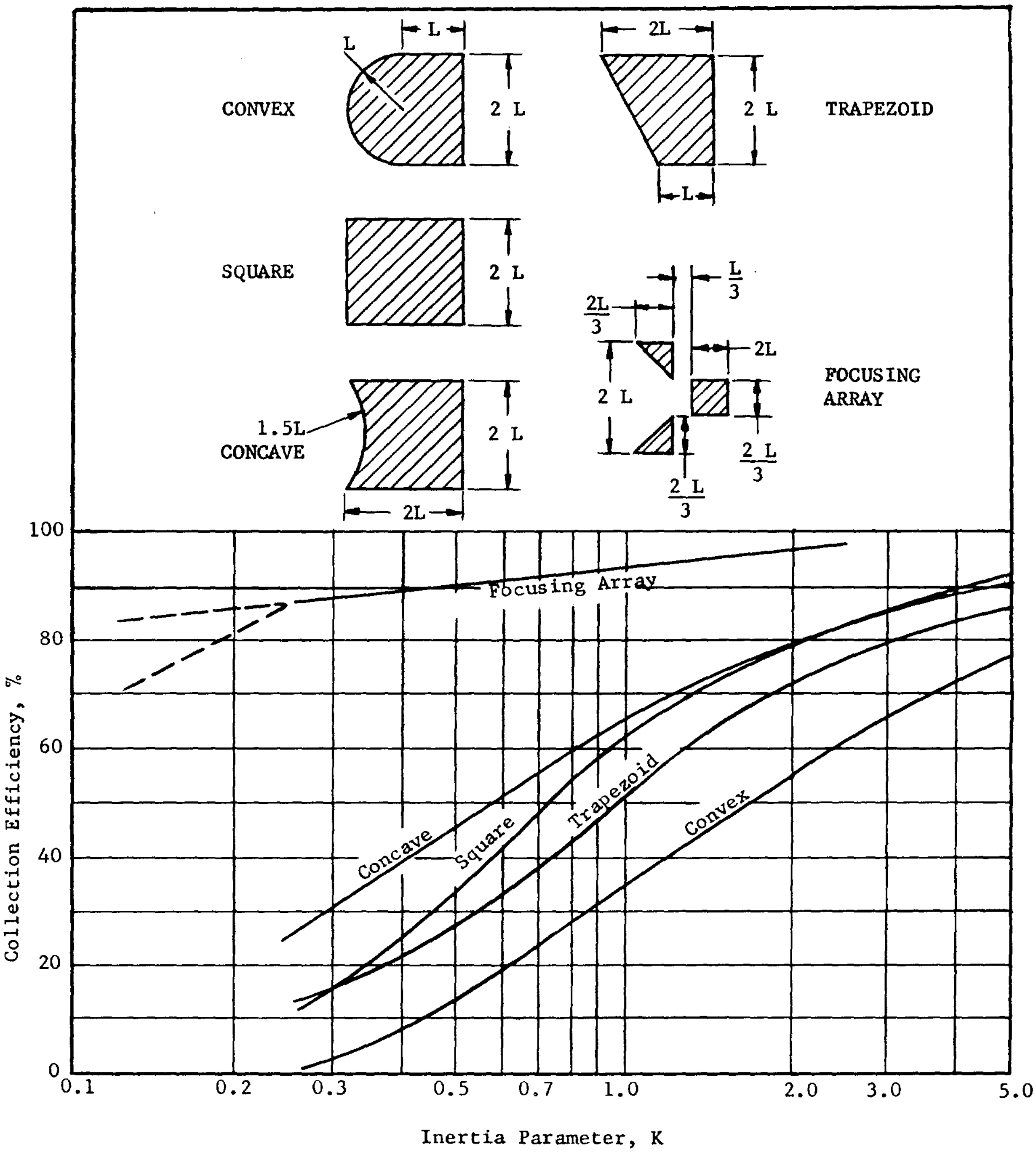


Fig. 2 -- Effect of Collector Body Shape on Collection Efficiency

of its high-collection efficiency, especially for low values of K, was selected as the blade shape for rotating impactor experiments. The theoretical flow conditions of the analogue trajectory studies are quite different from those which may be encountered in actual sampling. However, reported experiments[4] using a suspension of 0.365 micron polystyrene spheres indicated general agreement with the predicted values. At 50 millibars pressure, where the outer 1-1/2 inches of the 4-1/2 inch rotating blade array was operating at an impaction parameter, K, greater than 0.2, a collection efficiency of 73 percent was measured. It is expected that further work can produce an array with higher efficiency and considerably reduced drag. This is especially noteworthy since the actual impactors will operate in the partial slip flow region with a mach number close to unity in the throat. The focusing impactor shown starts to choke at a mach number of close to 0.2 and so should be improved for any final design. Difficulties have been experienced with rotary impactors when each impactor operates in the wake of the preceeding one. But, owing to the long spacing of the spiral, this should not be a problem with the rotorchute device. To maximize sample size it is necessary to operate several impactors in parallel. Although the proposed rotorchute system will permit separation of the impactors, the general problem of parallel operation must be investigated before an optimum impactor design can be developed. This problem does not materially affect the feasibility of the system.

In the region of interest the mean-free path of the air molecules is larger than the particle diameter with the result that the Cunningham Correction factor, k_m, in Equation (2) will reach very large values. This is advantageous since it permits the collection of small particles at low air velocities using large impactor surfaces. It appears that the Cunningham Correction factor can be used up to the region where the mean-free path approaches the impactor dimension. This occurs around 250,000 feet with impactors sized for 100,000 feet, and above 400,000 feet for impactors sized for 200,000 feet. This is above the maximum altitude considered in this study.

The minimum value for the inertia parameter in the sample design calculation is taken as K = 0.2 which corresponds to a collection efficiency of over 80 percent. To maintain a constant collection efficiency, K must remain constant. The velocity of the air relative to the blade V_B is a function of blade radius r. By making the impactor size, L, also a function of radius, the radius cancels in Equations (2) and the inertia parameter becomes a constant over the entire length. Substitution into Equation (2) for pressure and Cunningham Correction factor as a function of height gives for the minimum particle size collected at 80 percent efficiency as a function of the maximum half-size of the focusing impactor array and altitude the relation

$$d_p = 10.6\, L''_{max}\, e^{-H/25,700} \tag{3}$$

This is plotted in Figure 3, which shows that the minimum particle size increases rapidly as the collector descends. Therefore, the lowest altitude of interest, 100,000 feet in the design calculation, sets the size of the impactor. The maximum impactor size, L''_{max}, at the tip of the rotorchute is shown for various altitude increments of interest.

The weight of air sampled as a function of altitude is equal to the volume of air swept out by the impactor array times the density of the air integrated over the altitude range sampled. The volume of air is equal to the volume per revolution times the revolutions per second times the vertical distance the rotorchute travels divided by the velocity of descent. Using a constant tip speed of 700 ft/sec and the velocity of descent from Equation (1), gives a sample size which is proportional to the integral of the air density to the 3/2 power.

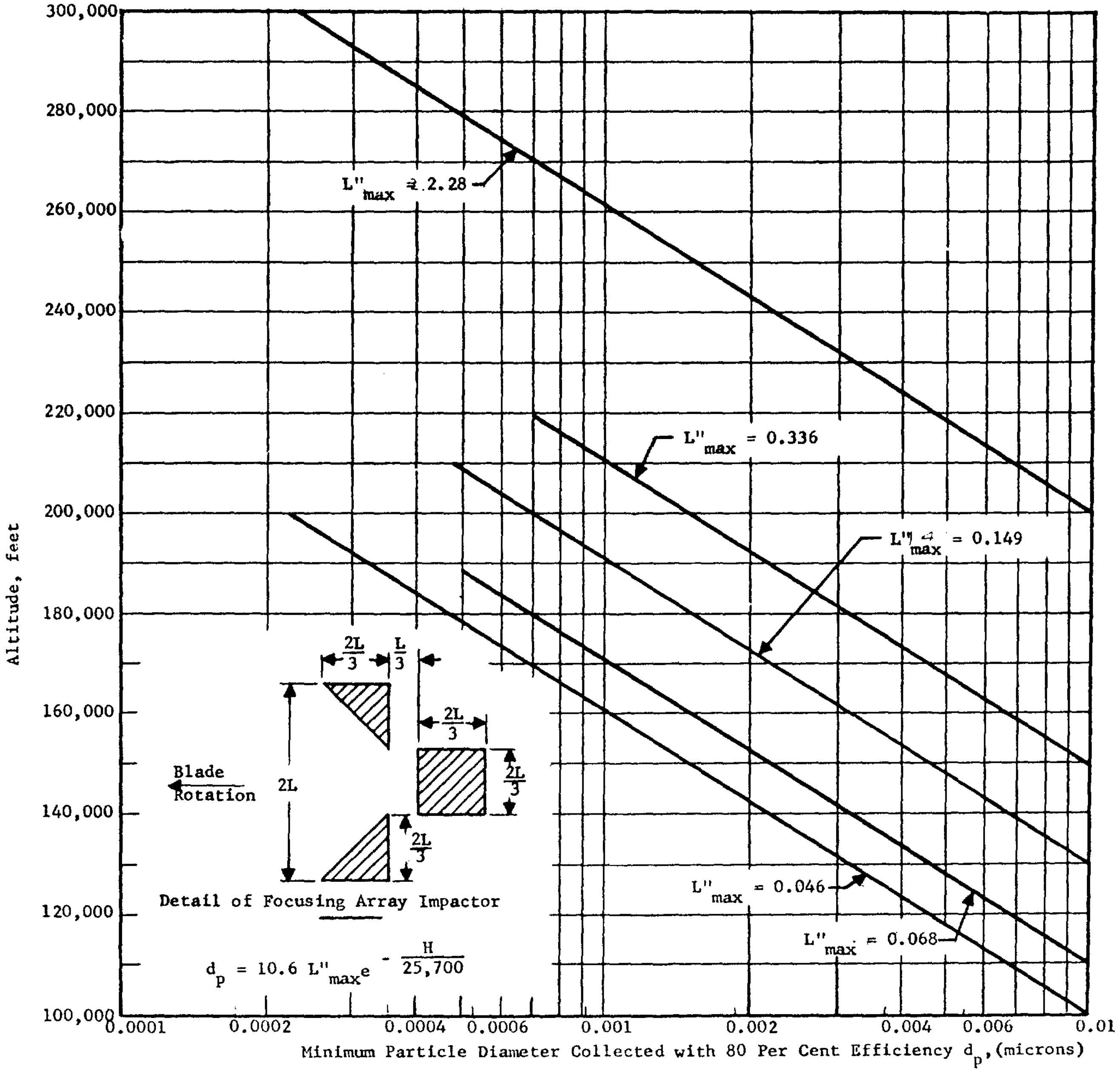


Fig. 3 -- Effect of Focusing Array Impactor Dimensions on Minimum Particle Size Collected

Solving for the weight of air sampled between a given altitude and 100, 000 feet gives

$$W_A(100,000) = 75,000\ \frac{1}{340}\ - e^{-H/17,120} \tag{4}$$

This relationship is plotted in Figure 4 as the constant tip-speed curve.

As can be seen from Figure 4, the weight of air sampled in dropping from 200, 000 to 100, 000 feet is over 200 pounds for the constant tip-speed design which is equivalent to over 2, 000 std cu ft of air at a sampling efficiency better than 80 percent. If the optimum impactor size is used from Figure 3 for altitude increments of interest, a considerable increase in sample size can be realized in the higher altitudes. The calculated incremental sample weights are shown in Figure 4. These weights of air sampled are indicative of what is possible, but do not include refinements for such effects as slip flow. In order to achieve the sample size calculated, especially in the 200, 000 to 300, 000 feet increment, some redesign of the rotorchute and impactor system would be required. In order to obtain the low wing loading and large impactor size needed special techniques, such as inflatable structures, would probably be required. The rotorchute sample sizes shown are ten times as great as the best that have been calculated for any other system with this payload weight.

Considerable thought was given to the problem of high altitude aerodynamics of this device. Little experimental work has been done on the aerodynamics of rotorchutes above 100, 000 feet. However, present theory indicates that while the rate of descent may be somewhat greater than indicated by the calculations, the device should operate satisfactorily. There has been considerable interest lately in the use of rotorchutes to slow reentry vehicles.[5] This is a definite indication that satisfactory aerodynamic capabilities in the altitude range above 100, 000 feet can be achieved.

Since there will not be any serious aerodynamic heating problems with respect to the impactor surfaces, strippable coatings or double-surface pressure-sensitive films can be used for sample collection at the discretion of the experimenter. Since such coatings or films are made from organic materials the background radiation from an unexposed surface would be small.

The total area for impaction is quite small, 53 to 70 sq in, in the example, depending upon r_1. Therefore, a concentrated sample for counting can be achieved. The coating can be stripped off and packaged in the field or the entire impactor assembly can be shipped to the counting laboratory for examination.

The sampling system described will weigh about 40 pounds, including the rotor blades, impactors, hub, controllers, and telemetering. This collection system would be compatible with the Arcon rocket developed by Atlantic Research Corporation. This vehicle will lift a 40-pound payload to over 300, 000 feet, but the aerodynamic drag associated with the folded fins would probably bring the apogee to the maximum desired altitude of 200, 000 feet. The total length of the rocket is about 13 feet which is quite close to the 14-foot blades assumed in the calculation.

The major cost for each firing would be the cost of the nonreuseable rocket motor, which sells for about $ 5, 000 per round depending, of course, on quantity purchased. The entire payload assembly will probably cost somewhat more than the rocket motor but the fact that it may be made fully recoverable should make the cost of this component under $ 2, 000 per test. A somewhat larger rocket capable of sending a 50-pound payload to 100

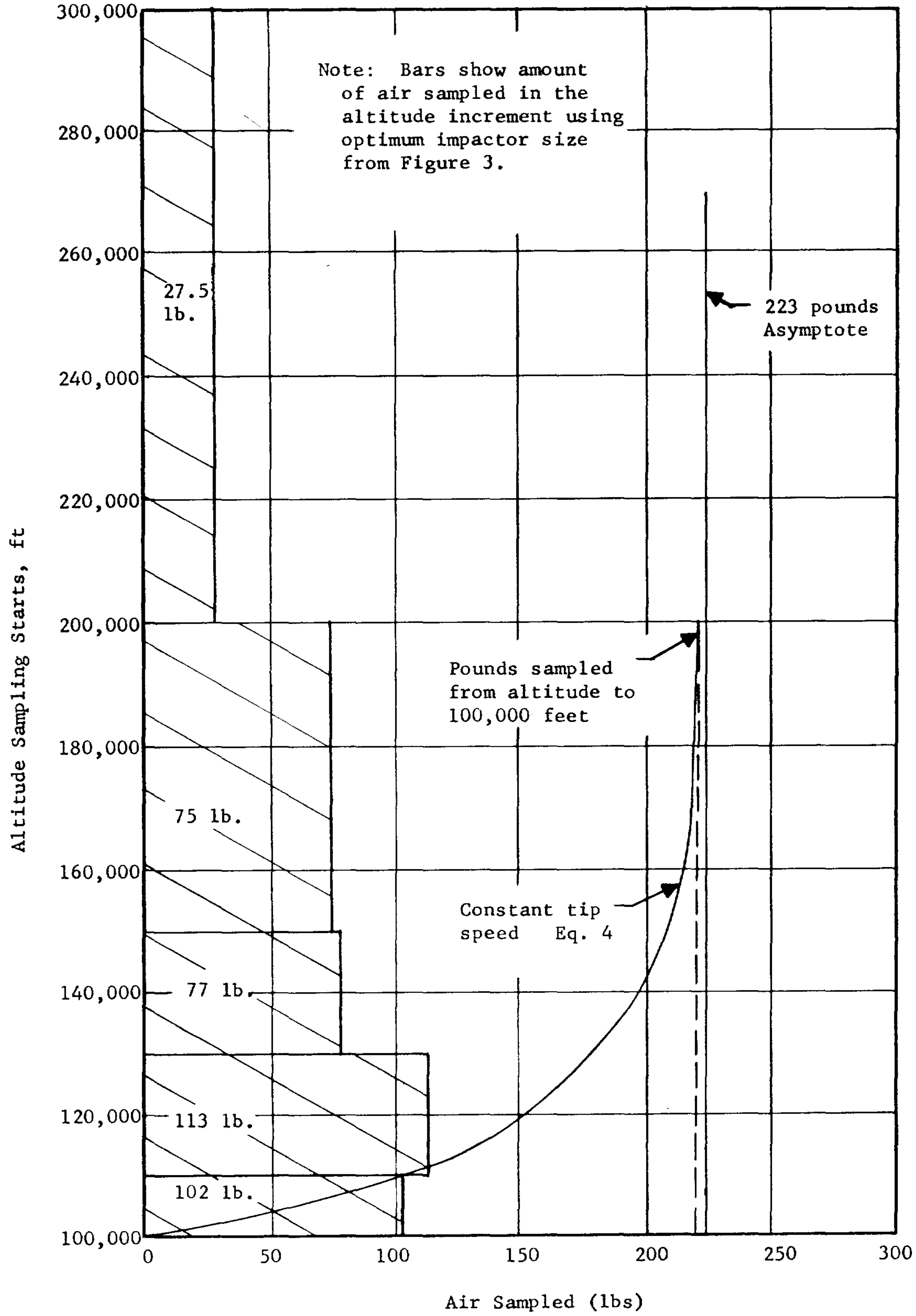


Fig. 4 -- Pounds of Air Sampled versus Altitude

miles is the Nike Cajun. This vehicle would allow more instrumentation and weight than the Arcon vehicle. However, the Nike Cajun motor cost is presently estimated at about $ 7, 500.

The large sample sizes calculated for the rotorchute system indicate that a smaller rocket-borne sampler may be of interest for particular tests and for repetitive test programs. The Arcas rocket manufactured by Atlantic Research is capable of transporting a 12-pound payload to just over 200, 000 feet. The maximum length of the missile is 100 inches and the burnout weight of the motor is 24 pounds. This rocket should be capable of carrying a collector which would sample about 1/4 the amount indicated in the design calculation. The cost of the Arcas motor is about $ 850 in lots of 150 units. However, the cost of the payload would probably not be much less than for the Arcon.

Nomenclature

d_p	Diameter of the particle	microns
H	Altitude above sea level	feet
K	Inertia parameter	
k_m	Cunningham correction factor	
L	Half width of individual impactor	feet
L''_{max}	Maximum half width of impactor at blade tip	inches
P	Absolute pressure	(lb force)/(sq ft)
r	Distance along blade from center line	feet
r_o	Radius of rotorchute or parachute	feet
V_B	Velocity of air relative to the rotorchute blade	ft/sec
V^*_R	Velocity of descent of rotorchute with forward motion	ft/sec
W_A	Weight of air sampled	lb force
W_T	Total weight of descending vehicle plus sampler	lb force
ρ	Density of the air	lb mass/cu ft
ρ_p	Density of the particles	lb mass/cu ft
ρ_o	Density of air at sea level	0.0765 lb mass/cu ft
μ	Absolute viscosity	lb mass/ft sec

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(B)

Measurement of Air-Borne Radioactivity by Gamma-Ray Scintillation Spectrometry

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Introduction

Soil is unquestionably the medium in which total accumulation and a running inventory of fallout radioactivity at a given location may be measured most directly. The major difficulty encountered in the direct measurement of fission product activity in soil, particularly by γ-ray spectrometry, is due to the relatively high concentrations of natural radioactivity which lead to errors of 10 to 25 percent in the determination of fission products in soil. Such errors preclude accurate determination of the monthly differences in soil fission activity (caused by the radioactive decay of old debris and the deposition of fresh fallout) which in general amount to only a few percent of the total accumulation. In order to observe quantitatively the variations in fallout rate it is necessary therefore to refer to air or rain samples. It was for this reason, namely to supplement the soil fallout measurements, that we have turned to the investigation of fission product radioactivity in surface air.

The usefulness of fission product radioactivity in surface air as an index of fallout rate is predicated on the following conditions:

1. The mixing time in the troposphere is sufficiently rapid so that sampling at one or a limited number of sites over a wide area will be representative of the average air concentration over the entire area.
2. The removal of radioactivity by local precipitation does not reduce permanently the concentration of activity at subsequent times at that location below the level pertaining at other sites in the area which did not experience similar precipitation.
3. The average concentration of fission products in the troposphere should bear a reasonably constant relationship to that found in the adjacent stratosphere.

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The rapidity of mixing in the troposphere is illustrated by the work of Lockhart et al.[1] which indicated that debris from Hardtack I in surface air spread from 10 to 50 degrees N latitude in a matter of a few weeks. At a given time, the concentration in surface air differed by less than a factor of 10 over the entire region. Similarly, in the spring of 1959 comparable concentrations of fission activity were observed simultaneously from the Central United States to Northern Norway.[2] Except for dry deposition and impingement on surface materials (vegetation and minor variations in terrain) the removal of air borne debris occurs via rain and snow. During prolonged rains, a decrease in concentration of activity with time has been noted, implying a depletion of activity per unit volume of air; however, replacement by masses of air not similarly depleted is rapid, and a lower value of activity in air does not persist for more than a few hours under moderate rainfall conditions. That this does not materially change the average air concentrations over periods of weeks or months is shown by the findings of Hvinden[2] in which air levels were almost identical during the same month between localities of widely varied rainfall. Condition 3 is fulfilled in that approximately 10^3 higher concentrations are observed on the average in stratospheric air than are found in the surface air below. The foregoing would seem to justify the use of surface air measurements as a means of determining, in a gross way at least, the time rate of depletion of the stratosphere as well as the mechanism and rate of transport via the stratosphere of debris from distant detonations.

Measurement Techniques

Air borne particulates were collected on HV-70 (Hollingsworth-Vose) 9 mil filters, 40 square inches in area, exposed in Suterbilt air pumps at flow rates of 25 to 30 cubic meters per hour. The efficiency for particulates of 0.25 micron and greater is 96 percent. Weekly filters from a number of collectors located on the Argonne National Laboratory site and surrounding area were grouped by calendar month.[3] Each monthly group contained particulate matter collected from approximately 2×10^5 cubic meters of air. Approximately one-half of each monthly group (i.e., one-half of each filter) was partially ashed at 300 degrees C and compressed into a cylindrical disc for γ-counting. Total ashing was not possible due to the asbestos base of these filters.

Analysis for γ-emitting radioactivity was done by NaI scintillation spectrometry using a 5-inch diameter, 4-inch-thick crystal in conjunction with a DuMont 6265 photomultiplier tube. Pulse analysis was implemented using an RCL (Radiation Counter Laboratory) Argonne Type 256 channel analyzer. Both sample and detector were placed inside of a 9-inch-thick steel shield, with an additional inner shield consisting of one inch of mercury inside a stainless steel annulus. Repeated spectrometric analyses of intact and ashed portions of monthly groups of filters showed no observable loss in activity of specific radionuclides due to the ashing process.

At least two weeks elapsed between the time of collection and of counting a given batch of filters; thus any daughter products of radon or thoron would have largely decayed out. Cosmic-ray produced Be^7, however, is still present in nearly its initial concentration after a two week delay. The method whereby the spectral data are qualitatively examined for the amount of activity due to specific radionuclides utilizes a standard reference source of known activity for each isotope present. The composite spectrum is then broken up into the appropriate number of energy regions, one for each isotope, and then the requisite number of simultaneous equations are solved. Due to spectral similarities, such as those existing between Ru^{103}-Ru^{106} and Ce^{141}-Ce^{144}, use has been made of the radical difference in half life between members of these pairs. Hence repeated measurements were made of a given group of filters and the difference spectra analyzed for the short-lived components. An example of this technique is illustrated in Figure 1 where the presence of Ce^{144}, Be^7, and Zr^{95}-Nb^{95} in the difference spectrum is shown. The number and intensity of the individual spectral components changes drastically with time after collection or after a test series. Spectra of air borne fission debris collected 3, 15, and 25 months after the cessation of large scale weapons tests are shown in Figure 2. The decay of Zr^{95}-Nb^{95} and the emergence of Cs^{137} as the dominant radionuclide some two years after testing is clearly apparent. Depending upon the circumstances, difference spectra and simultaneous equations are used, or a combination of simultaneous equations and successive approximations. An attempt has been made to routinely analyze for $Ce^{141,144}$, $Ru^{103,106}$, Sb^{125}, Cs^{137}, and Zr^{95}-Nb^{95} in surface air filters. In addition, Be^7, W^{181}, and Rh^{102} have also been assayed, the latter was frequently determined by chemical separation and subsequent γ-measurement.

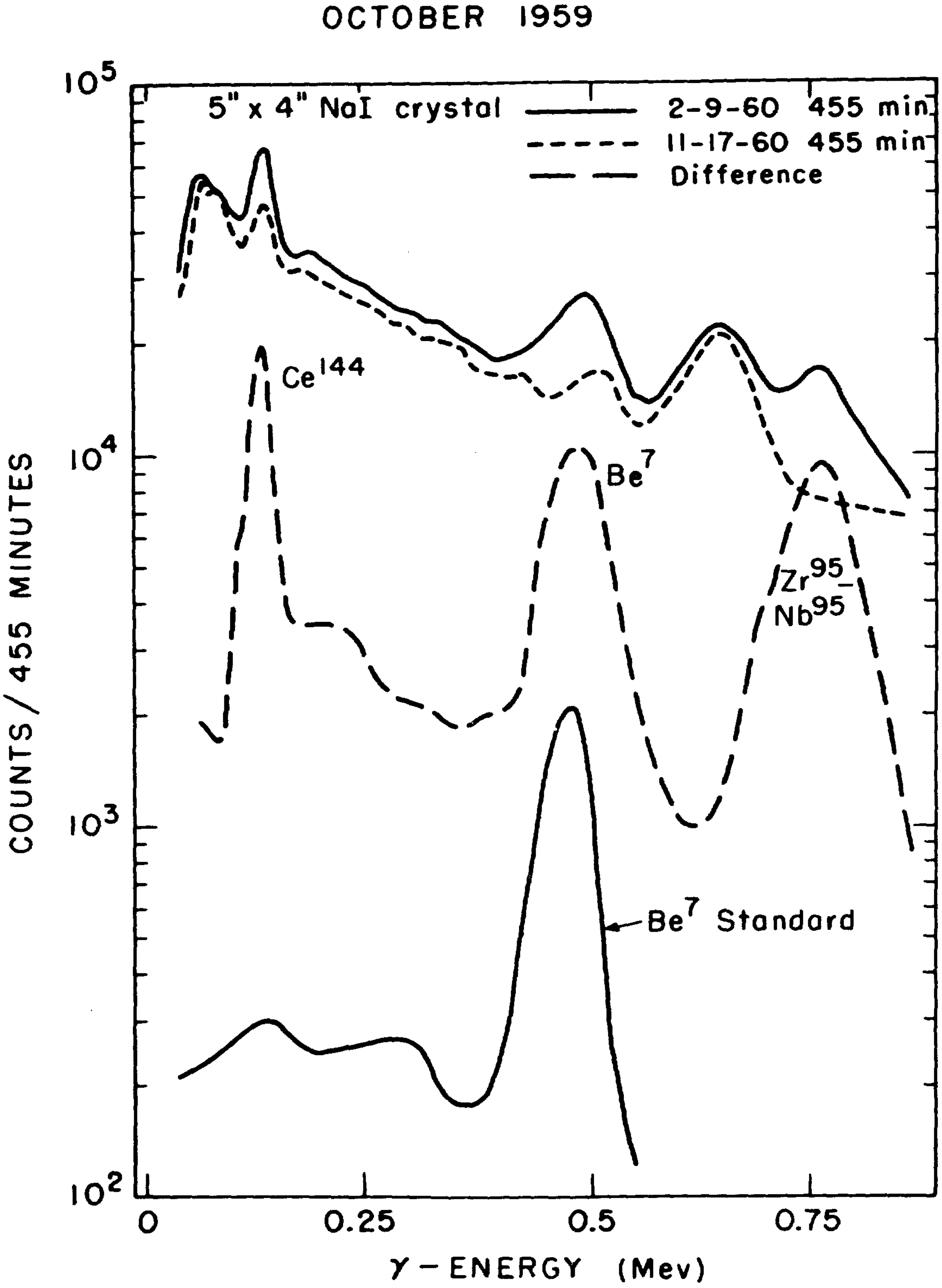


Fig. 1 -- Gamma-ray spectrum of ashed air filters

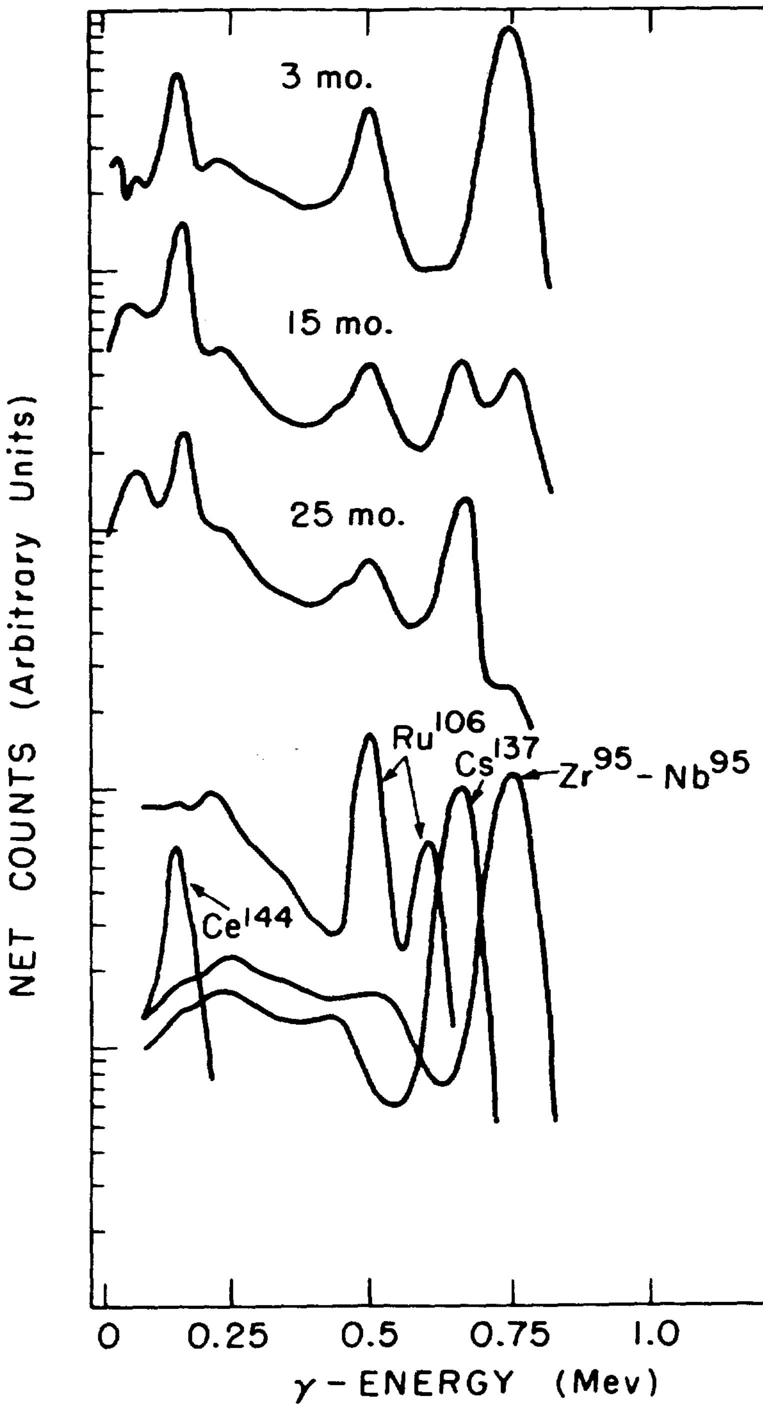


Fig. 2 -- Gamma-ray spectra of air borne fission products

The concentration of Cs^{137} in surface air at Argonne from early 1953 through mid-1960 as determined by these methods is shown in Figure 3. Variations by as much as a factor of 10 over an interval of a few months are repeatedly evident. Such variations are in part seasonal in nature, occurring during the spring months, and in part closely related to the frequency and magnitude of nuclear tests.

The Cs^{137} present in any one monthly sample consists of contributions from a variety of tests conducted prior to the time of collection. The high frequency with which tests of widely varying magnitude and geographic distribution were conducted from 1953 through 1958 coupled with the fact that spectrometric analysis of air filters at Argonne did not commence until mid-1958 have made the use of activity ratios for the dating of earlier debris quite out of the question. Some very fine work has been done along these lines, however, notably by both Martell[4] and Lockhart.[5]

A unique way of following nuclear debris from the Hardtack I Series was provided by the production of radioisotopes of tungsten by neutron activation in a number of shots in this series. Gamma-emitting W^{181} (140-day half-life) has been used in this regard in the work under discussion. In a similar way, Rh^{102} (210-day half-life), also a neutron activation product, formed in the Orange shot at high altitude toward the close of the Hardtack Series may be used to follow high altitude equatorial debris. Figure 4 indicates the concentrations of W^{181}, Cs^{137}, and Rh^{102} in surface air for the period January 1958 through January 1961. Both W^{181} and Rh^{102} have been corrected for decay back to shot time; mid-June 1958 for W^{181}, and mid-August 1958 for Rh^{102}. Both Cowan and Kalkstein have reported an isomer of Rh^{102} having a half-life of 3.4 ± 1.4 years. The Rh^{102} values have been corrected on the premise that 20 percent of the total Rh^{102} activity during the spring of 1960 arose from the 210-day isomer.

Method of Determining Sources of Radioactivity

In order to avoid using W^{181} production data for the entire series, the validity of which cannot be judged by available literature, we have used an approach which does not involve any estimate of the portion of Hardtack debris tagged with $W^{181,185}$. It was noted during the summer and early fall 1958 that the gross gamma fission activity in surface air at Argonne was predominantly from Hardtack I as indicated by high relative concentrations of Ba^{140}, Ce^{141}, and Ru^{103}. Older debris was also present because of the greater apparent age inferred from Zr^{95}/Cs^{137}, and Ba^{140}/Cs^{137} than from Ba^{140}/Ce^{141} or Ba^{140}/Ru^{103}. In order to remove the older, non-Hardtack component, the assumption was made that the shape of the Cs^{137} curve, for example, would have been the same during July through September 1958 as it was in July through September 1959 had it not been for the Hardtack series, since the testing sequence preceding these periods were very similar in both years.

The curves for the two years were normalized during the spring months (April and May), and the normalized 1959 values substracted from the corresponding monthly values in 1958 as shown in Figure 5. The resultant curve was then attributed to Cs^{137} from Hardtack only. An identical procedure was followed for Ce^{144}, Ru^{106}, and Zr^{95}. The activities of these nuclides were in all cases corrected for radioactive decay to the middle of the month of collection prior to normalization and subtraction. The values obtained by subtraction were further corrected for decay back to mid-June 1958, the mid-time of the series. The activity ratios of these four fission products relative to W^{181} were then determined using the values found during July through September 1958. The ratio of Ce^{144}/W^{181}, Zr^{95}/W^{181}, and Ru^{106}/W^{181} used at subsequent times must be

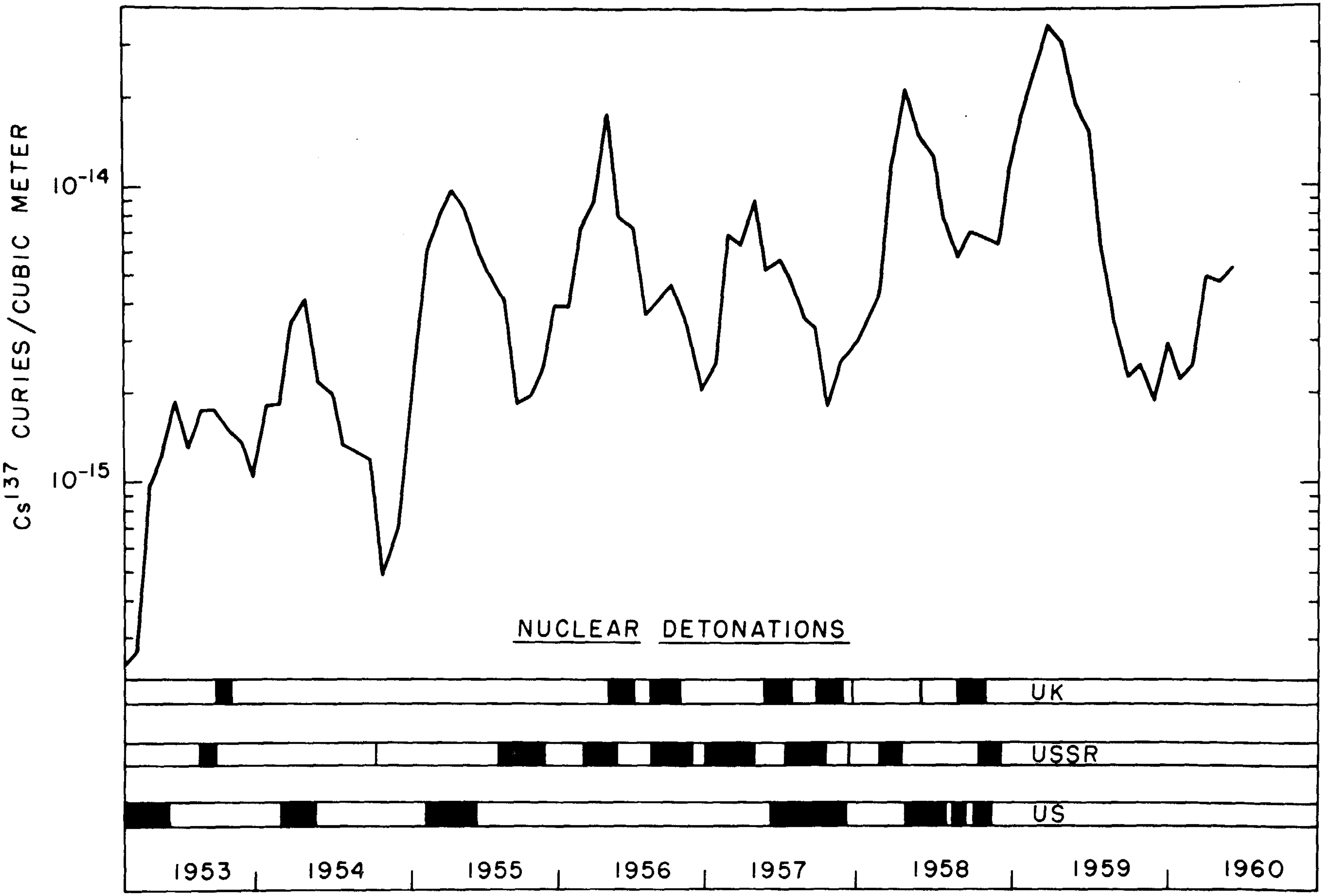


Fig. 3 -- Concentration of Cs^{137} in surface air at Argonne National Laboratory

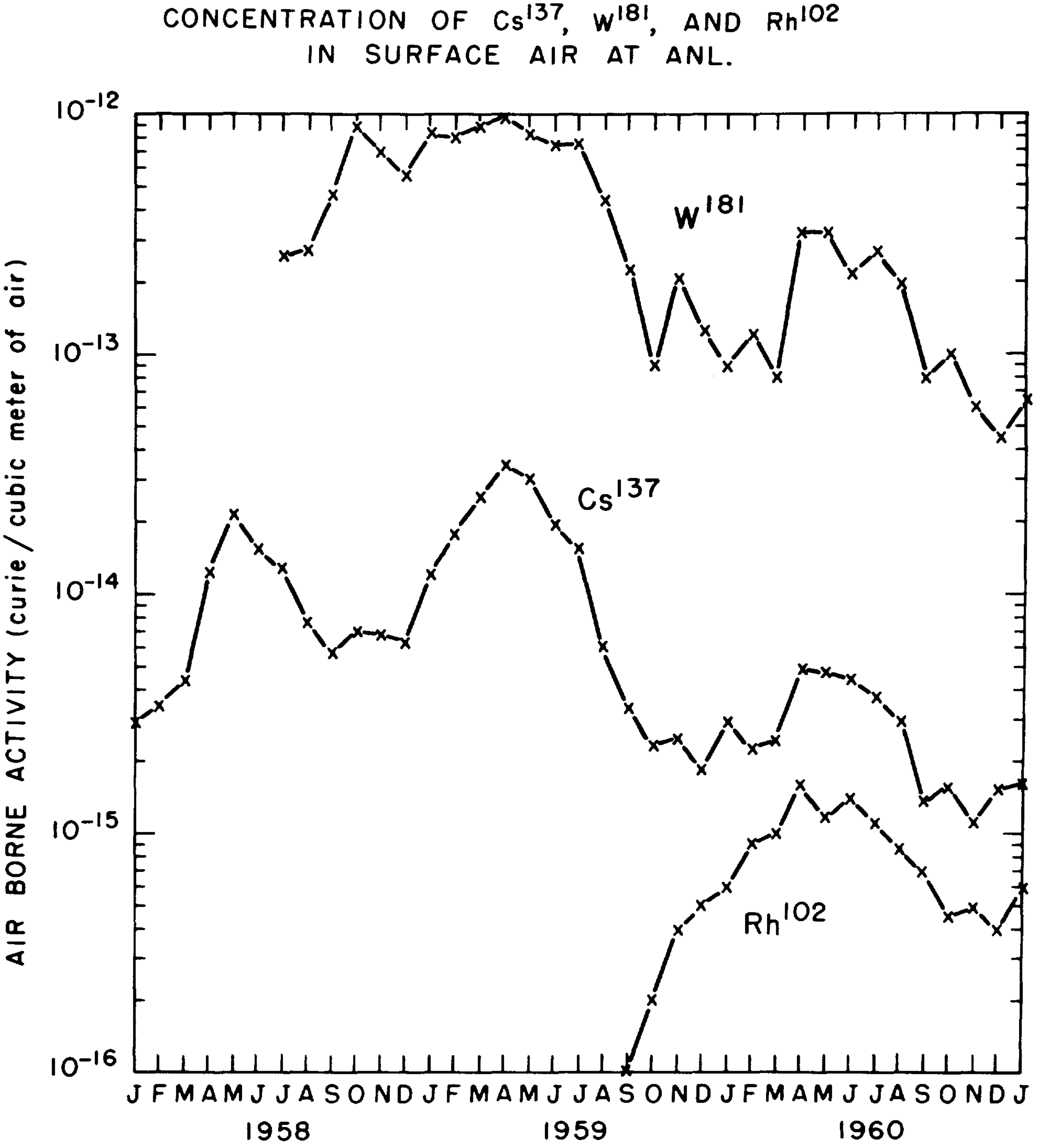


Fig. 4 -- Concentration of Cs^{137}, W^{181}, and Rh^{102} in surface air at Argonne National Laboratory

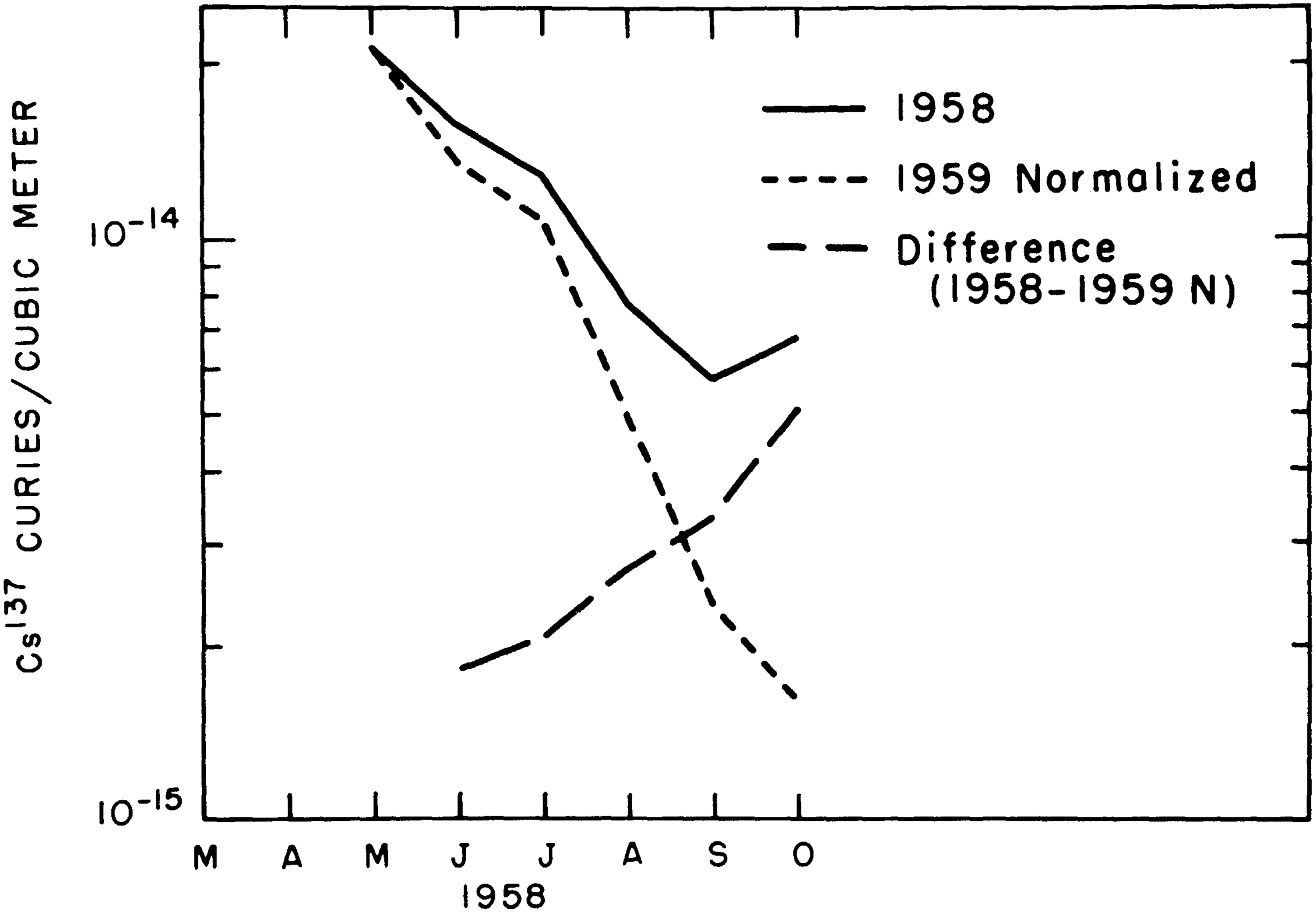


Fig. 5 -- Determination of Hardtack Cs^{137}

decreased for decay of the fission isotope in question in accordance with the time elapsed since mid-June 1958. Such correction is not necessary in the case of the Cs^{137}/W^{181} ratio.

Support of the essential correctness of this approach to the determination of Hardtack debris may be shown by examination of the activity ratios between the fission products themselves. The Zr^{95}/Cs^{137} ratio is a particularly good indicator of the age of nuclear debris in that it varies by only 10 percent between fast and thermal fission of uranium and changes rapidly with time. Values of this ratio for Hardtack and non-Hardtack debris are shown in Figure 6. Additional evidence that the assumption made in determing the Hardtack fraction are reasonable is given by the Zr^{95}/Cs^{137} ratio of the non-Hardtack fraction. The ratio for this portion of the debris follows a line of similar slope from November 1958 into the spring of 1959, indicating essentially a single component during this period, the apparent time of origin of which is October 1958, suggesting that this debris came primarily from the large Soviet series conducted at that time. By the fall of 1959 the non-Hardtack values depart from the initial slope, in a direction which indicates the presence of increasing amounts of older debris.

Some caution is in order, however, in interpreting this change of slope of the non-Hardtack component, particularly in light of the appearance of Rh^{102} in surface air at this time. The fact that the non-Hardtack Zr^{95}/Cs^{137} ratio implies the presence of solely Soviet October debris does not preclude the possibility of as much as 15 percent of older debris being present during the spring of 1959. The magnitude of Soviet debris relative to the total activity decreased very rapidly during the summer of 1959 as will be shown subsequently. Thus the relative importance of older debris, coming down more slowly, becomes greater with the passage of time. Figure 7 shows the breakdown of debris into that coming from Soviet October, high altitude, and residual, or pre-Hardtack. By the spring of 1960 the amount of Zr^{95} had fallen to such low levels that accurate measurement by these means was no longer feasible. Extension of this method of partition of debris was extended to the end of 1960 by using the Ce^{144}/Cs^{137} ratio as shown in Figure 8, where the components due to Hardtack and high altitude are indicated as well as the residual debris. As in the case of the Zr^{95}/Cs^{137} ratio data, there is evidence of pre-Hardtack debris still present in surface air during 1960. There is also evidence in Figure 8 of small amounts of activity from the French test in February 1960 as indicated by an elevation in the value of the ratio for residual debris during February and March.

On the basis of tracer and activity ratio determinations it is now possible to assign the observed Cs^{137} activity to various test series as is shown in Figure 9. In the spring of 1959 slightly under 20 percent of the total Cs^{137} came from Hardtack as indicated by W^{181} measurements, the remaining 80 percent coming primarily from the Soviet October series. Perhaps the most striking feature of the data shown in Figure 9 is the relatively short interval during which Cs^{137} from the Soviet October tests was present in surface air. The absence of debris from this source subsequent to the end of 1959 suggests that it has all been deposited. In light of the fact that the decrease of Soviet debris in surface air proceeds at the rate ascribed to tropospheric clearance (approximately 30 days) after the spring maximum, it may be argued that activity from this source seen after May 1959 actually entered the troposphere during the spring break. Radioactivity from the high altitude shot does not even appear at the surface until one year after detonation, rises fairly rapidly and manifests a broad maximum during the spring of 1960. Hardtack debris shows a broad maximum in the spring of 1959 as well as one of lower magnitude in the spring of 1960.

Considerable information may be gleaned from consideration of Figure 9 as well as referring to Figure 4. First, the fact is clearly established that the spring maximum is indeed a meteorological phenomenon as

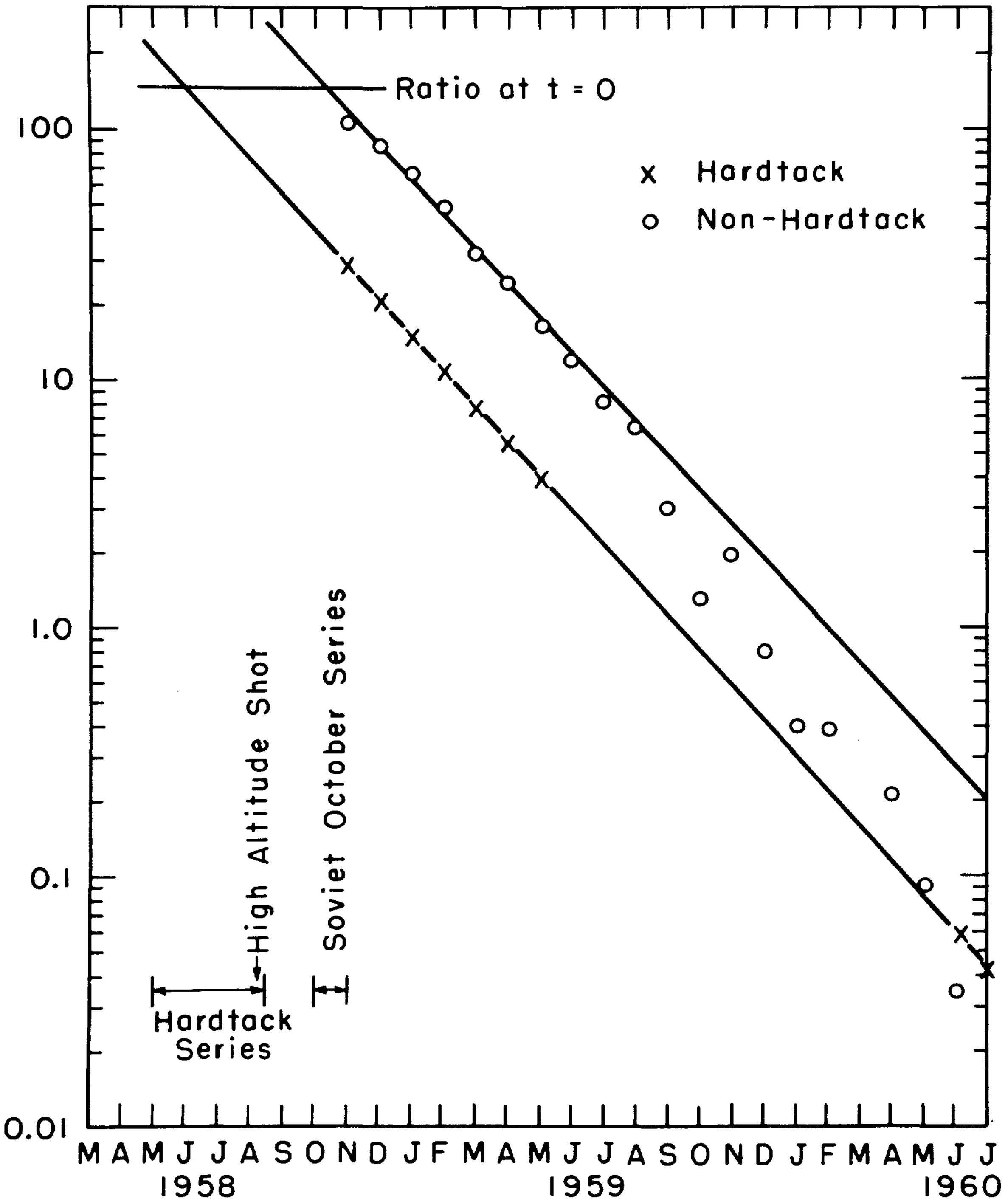


Fig. 6 -- The Zr^{95}/Cs^{137} activity ratio of Hardtack and non-Hardtack radioactivity

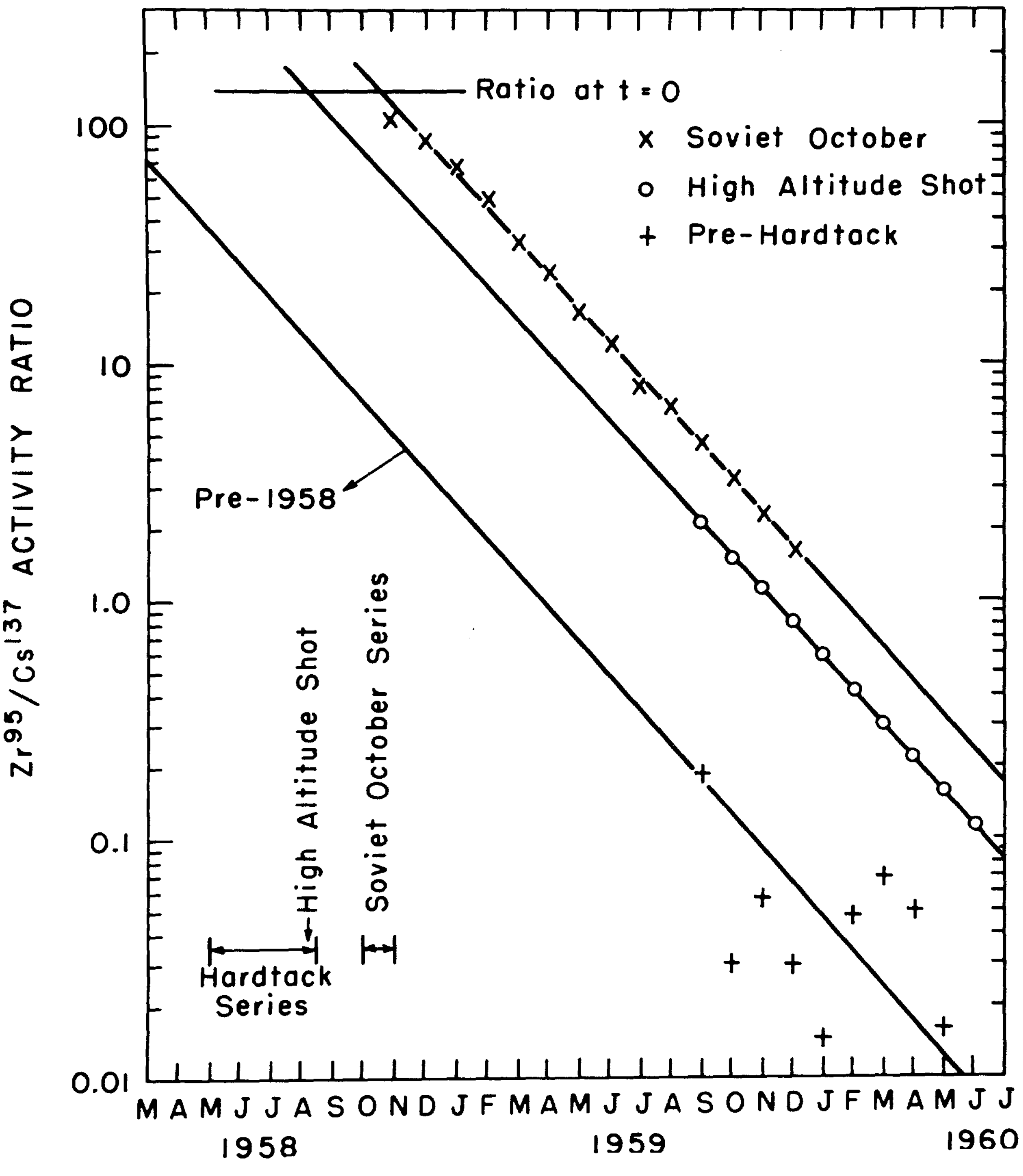


Fig. 7 -- The Zr^{95}/Cs^{137} activity ratio of Soviet October, high altitude, and pre-Hardtack radioactivity

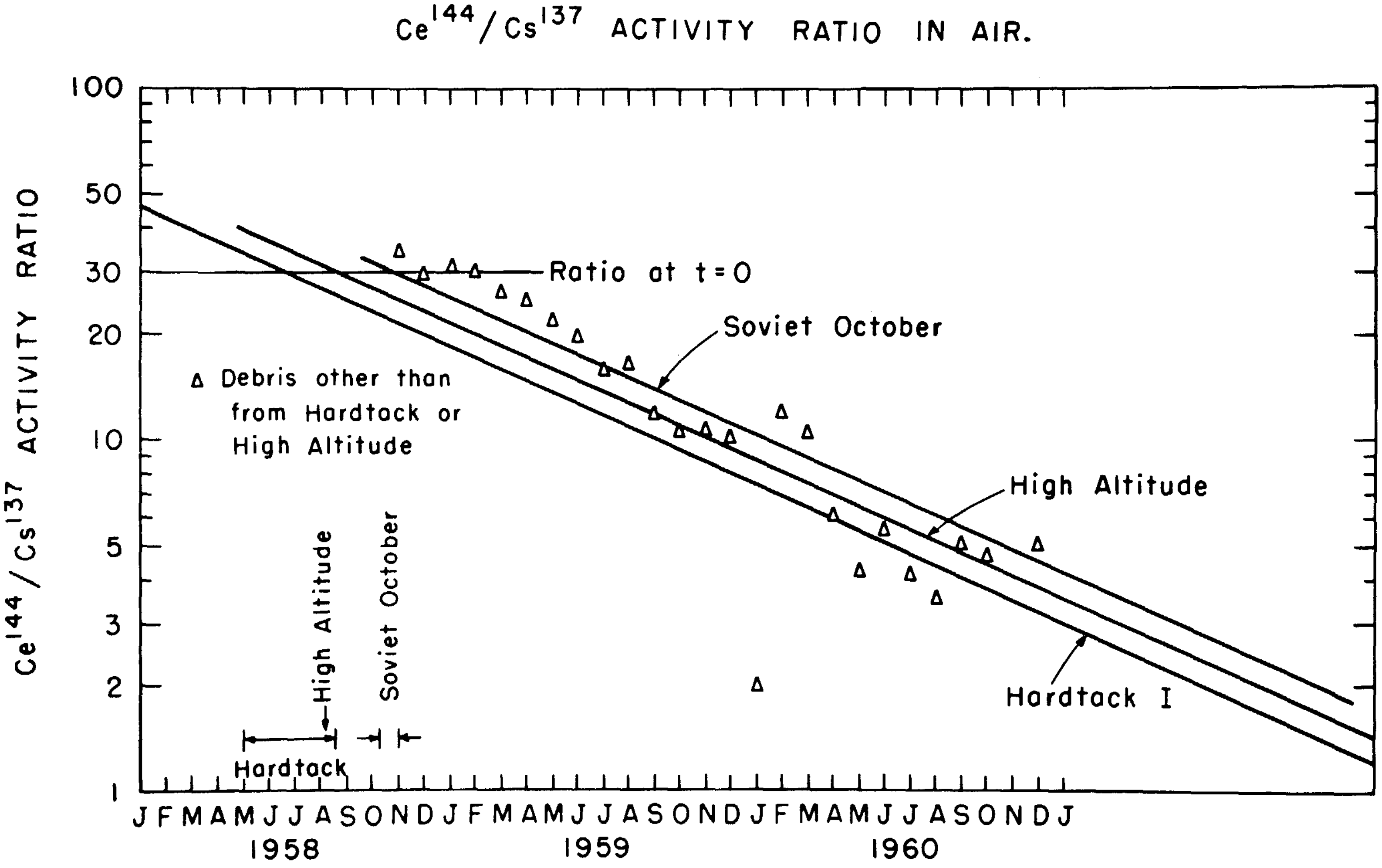


Fig. 8 -- The Ce^{144}/Cs^{137} activity ratio of air borne radioactivity

importance to some of our programs. It goes against many of the assumptions that we have made, and we will have to re-evaluate quite a few of them on this basis. One of the important points that Jack (Welch) mentioned in his paper is that, if you do explode at very high altitudes, debris particles immediately get into this eliptical-shaped region at the conjugate points. The altitude regime in which they are deposited is the 100- or 200-kilometer region, or perhaps slightly higher, and how this debris spreads and therefore affects the surrounding regions would depend critically on wind distribution and diffusion at these high altitudes. That's about all, except that I gather from Dr. Martell's comments that man's contamination is one of the problems which we are faced with. Even if the weapons testing itself decreases or stops, the use of nuclear reactors and nuclear propulsion units is bound to increase, if not in the next 5 years, certainly in the next 30 years. We may be in the hollow part of the cycle right now, but the programs will increase, and this gives some urgency to our program.

Dr. Tucker:

I don't think I elaborated enough on Vela yesterday. It is certain to be an expensive project if the treaty is anything like it is shaping up, when and if they sign it. It will take on the order of a billion dollars to implement the stations they are talking about. This is a world-wide network; it is an every-minute-of-the-day network - every second of the day. A Vela network will give simultaneous recordings of all the parameters that we can think of as helpful in detecting tests. Because of the interaction, this is quite a number. In other words, the ionosphere will be monitored far better than it is now. Fluorescence fluctuations in the atmosphere will be far better measured than now, and the question will arise as to whether we want a few good measurements, or a lot of them with more or less continuous coverage. Continuous coverage is the type of thing we are bound to get with a Vela-type network, because we have to police the ionosphere, as well as underground, year in and year out.

When we were preparing for this current Geneva conference, the objection was raised that we know we are going to fail to detect shots if they go out far enough and make the yield small enough. You cannot go out 10 million miles and see a kiloton. However, one of the most important psychological points of a treaty, and the one that started this test ban negotiation, was the contamination problem. It takes a tremendous amount of work to define what contamination to tolerate and so on, but at least the approach of determining nuclear tests on the basis of contamination has been suggested, and it does fit in very well with the political situation. This would mean sampling up to at least 600,000 feet, and possibly up to Van Allen-belt altitude if some of the fission particles would be trapped there. In any event you would have to have an elaborate research program to find out everything you can possibly find about circulation and where the particles end up. It may be that radiochemically you gain nothing by sampling continuously, but you have to prove it first; otherwise, in Vela you would start with the assumption that you must monitor almost continuously. You would be very much concerned with the pure geophysics of the problem. You would need to know exactly the circulation, residence time, trapping, charge interactions, effect on the ionosphere, and so on. Then you could do an adequate job of monitoring. In all our projects a research program always starts out this way. You cannot do your best job until you have done almost a pure research program. We will have balloons flying to measure gamma rays directly, because they will penetrate to balloon altitude. If you are flying balloons, maybe you will load each one with standard types of collectors. This kind of revolution has already occurred in seismic science. I would guess that the energy and input into seismology is up about 10^4 over what it was before these test talks started. You will get a reaction. We have already had some of that in the sense that we have made the radial-phase monitoring technique so

Mr. Holland:

I think that is a very good point. There is a danger here, especially if we conduct a classified program, that we will be operating in isolation and out of touch with a lot of closely related work. This would argue for an unclassified program, as free as possible, so that information can be exchanged with other scientists in meetings of the kind you conduct.

Dr. Shreve:

Satellite programs seem to be limited by perigee to about 120 to 150 miles, the Ash Can program similar things to 20 miles, roughly. We might well use the ingenuity that has been put into the satellite program by way of instrumentation and measure many of the things in this height realm between the two regions so that we have an integrated understanding from satellite measurements on down to the ground.

Dr. Otting:

Most importantly, you are measuring the same thing by two independent means, and if you get the same answer you feel better about it. I think if you can do this in some cases it would just be an independent check. I think the region in this more or less transition region from a continuum region of 100,000 feet where you can fly, and so forth, to 500,000 feet, where you would get into the molecular region for sure, is a tough one to sample because it is a transition, but by the same token it is important.

Lt. Ahearne:

I'd like to point out a few of the reasons why people in my business are interested in this type of thing. One of our major interests at the present time is the area around the D-region, in particular the area of about 40 to 80 kilometers. We are interested in the impurities present at these altitudes which might affect recombination rates or various air chemistry reaction rates. The studies that have been done indicate that impurities 10^{-6} to 10^{-8} may be more important in determining the methods by which electron recombination occurs or attachment occurs than has been appreciated before. At the present time, we know little of the impurity concentrations at these altitudes. We hope in the future to have fairly decent means of simulating conditions in the laboratory to study recombination rates. Unfortunately we are now faced with the unpleasant realization that we don't know exactly what to put in our containers to have this recombination proceed because we are not really sure what the impurities are. Another important thing is the ability to verify or improve on some of the theories that have been developed on methods by which these particles do settle down from the extreme altitudes. This is important for people in the Rover programs or similar programs who will be forced to inject a certain amount of impurities during operation. They are very interested in trying to find out where these go. We are interested in being able to determine what type of capability we must have for a detection program for detonations in space. In this light, detection does not look very promising when we consider the long time which is postulated for fall down into the region in which we now sample. We are essentially saying now that we may be able to tell 20 years from now whether a test occurred out in space today. This is of little use to us. However, if we can have a capability of detection at high altitudes and understand what the settling process is, we will have a greater confidence of being able to tell in a short time. I was extremely interested in the point that was raised by Sandia people about the winds at high altitude and the rise of the debris to extreme altitudes. This is of tremendous

Dr. Machta:

I also have had opportunity for my say, so I'll be very brief. As a synoptic meteorologist I recognize the variability in space. For example, at our Helsinski meetings, very beautiful experiments using sodium vapor trails were reported on by the French. They fired sodium vapor trails up to about 80 to 120 kilometers and showed that at approximately 101 kilometers there was an abrupt transition between conduction and eddy mixing below, and apparently pure molecular mixing above. This is not quite the picture that one gets from what Ed (Martell) showed on the board here, indicating the variability in space. Down in Alice Springs they did this same experiment and got this transition at a completely different altitude. I really feel that taking one or two even very beautifully documented observations is not going to be the solution to our problem. Expensive though this may ultimately be, we are going to have to have many more of these.

Dr. Shreve:

I think there are some rather powerful ideas that are being thrown out here. Perhaps at the end they will come to the fore.

Dr. Otting:

As a means of thinking of related subjects I thought of what might be given at a symposium on upper-atmosphere research rather than upper-atmosphere sampling. If you went to a meeting of this type, you could very well get into many things other than just particle sampling or static sampling. Certainly upper-air chemistry would be an important subject, as would the problems of radiation balance from the sun. All of these things could affect the atmosphere generally. In the region from 100, 000 to 500, 000 feet, air chemistry could be very important and very difficult. There you run out of the simple methods of sampling, such as balloons, and you are in a region where you can't stay long in one place by any means. It is going to make it a very difficult region to study. On the other hand, electromagnetic techniques such as ionospheric studies and this sort of thing are already exploring this. As a matter of fact, one of our important problems is the study of the D-region, which is in this part of the atmosphere. If the particles are in the charged state when you collect them, you undoubtedly are not going to have them in a charged state when you get them back to the laboratory, yet the electrical properties may be very important in their dynamics, even for general mass motions and this sort of thing. I don't have any proposal for a better sampling scheme, although I am sure some of the people concerned with upper-atmosphere chemistry are concerned with this problem. The same thing applies to the influx of particles from outside such as Captain Welch talked about. These could complicate the situation by creating various kinds of electric and magnetic forces that would cause motions as you go further up. We see mass motions and things that could contribute information on particle motions or debris motion. I think there are other fields in the physical sciences exploring these areas, and these could very well contribute independent information on the subject that might be very useful. I was interested in whether or not the people here felt that they knew enough about the other areas. I know that Cambridge, for example, and I am sure the Weather Bureau and other places hear a lot about these things, and I wonder whether you should consider some of these things in your sampling and interrelate the information?

fallout data that we have obtained, what difference it makes what the structure of the mesosphere is and what the mixing rates are in the mesosphere. What difference would it make in our estimates of the length of time it would take for the debris to get to the ground, its distribution on the ground, and the doses that people might get from it? I believe it does make a very substantial difference. The rhodium experiment brings out the uncertainty from the data we have now as to whether the mesosphere residence time is 5 years or 20 years. This would make a lot of difference for some of the nuclides that are involved in the aerospace program.

Dr. Shreve:

Before I call for the next panel member I want to comment on his talk last night and how much I valued it. I think it is very easy, when you turn your eyes toward scientific implications, to be caught in the romance of outer space and to speak rather glibly about the possibilities. What I valued about his comments last night was his qualification concerning the kinds of things that might appear possible. He pointed out some of the difficulties which might be encountered because of what man-made debris has already done to these altitude regimes. Until we can really check the reaches of man's influences, we may have a rather tough job of resolving what we might term natural in a background of man's contribution to the realms.

Dr. Martell:

I have already had much more than my share of an opportunity to speak. There were a couple of points that I meant to touch on last night that have not been mentioned in the meeting but perhaps are worth considering. No one has said anything about the priority of different experimental applications of interest to the DOD, AEC, and geophysicists. There are a very limited number of tracers that one can use. One can agree wholeheartedly with Col. Russell's suggestion about using a multitude of tracers so that one can get the details of the ablation history and the completeness of the ablation. But, unless this experiment were going to be conducted on a small scale with very small amounts of material, you might be wasting a large proportion of the limited few tracers and thereby might eliminate the possiblility of carrying out some other experiments. Let's presume that the application of high-altitude sampling techniques becomes an important method for work in connection with a moratorium violation. In this period, could we afford to carry on experiments of the type described in the Orion program which would confuse the short-lived activity component in the high stratosphere rather seriously? It seems that we have to have some sort of central committee to consider and review all the experimental areas of interest and all applications of interest, and to consider whether we should expend our best long-lived tracers, recognizing how this might confuse other applications or other experiments. If we are going to carry on experiments of this kind in the atmosphere, then certainly the Russians and other nations can do similar things. They already have bigger missiles and their experiments might more generously contaminate the high atmosphere with similar tracers. We have always felt that a few good long-lived nuclear tracers or a few selected nonnuclear tracers should be reserved perhaps for international geophysical types of experiments to study atmospheric behavior. This is the only means of controlling the experimental possibilities on an international and cooperative basis. Such things might be worked out through such agencies as the World Meteorological Organization or the National Academy of Sciences. This is a consideration perhaps more immediate to some of the people I am associated with at GRD than to many of you, but I think the air has to be cleared in this area. There has to be some kind of coordination within our own program and, in some few cases, on a much broader basis.

cannot select one of these approaches and say that that is the right way to do it. At least these two types of approach have to be taken, and maybe others in addition. This suggests that probably we need two types of samplers. These seem to be exemplified by essentially the mechanical type which does not collect everything, but only particles, presumably all the particles, you hope; perhaps you have not such precise volume control. On the other hand, the cryogenic type of sampler uses absorption or straight condensation freezing, attempting to get everything. The mechanical method lends itself to very large volume sweeping; the cryogenic system is essentially mass-limited. The mechanical system could be developed in such a way as to give considerable certainty concerning some constituents. The cryogenic system enables you to find out practically anything you want to know about the constituents of the air. These are my impressions about the scientific implications of the technology discussion that we just heard. I don't feel that the contradictions that have been brought out are really contradictions, and we must move forward, not perhaps with all approaches, but certainly with at least dual approaches. To summarize, I say the mechanical sampler gives very large volumes, relatively low precision of sampling, considerable economy in sampling, but the information content may be limited. The cryogenic sampler will tend to give a lower volume sample, with very high precision; the economy will not be so good, and it may be more expensive to do this, but you will get large information content.

Another area of scientific implication which we probably should consider is that of the implications of the information that we get about the vertical pattern of distribution and the kinematics and dynamics of motion that go with this. After all, if all these different methods gave us the same answer as to what the dose to people would be or distribution on the ground or lower atmosphere, we really wouldn't care too much from the aerospace nuclear safety standpoint which of these various methods turned out to be right. Before we had the fallout problem, if you had asked the meteorologist what would happen in the stratosphere and troposphere or in the atmosphere generally, he would say that probably there would be different mixing rates in the troposphere and stratosphere--in the troposphere relatively fast and in the stratosphere relatively slow. As soon as we had the stratospheric fallout problem, a model was immediately proposed which had two regions, both of them rapidly mixing internally, but with a slow transfer from one to the other. Immediately another model was proposed, one in which there was slow transfer by mixing in the stratosphere but with a systematic circulation. These had different implications. One said there is uniform fallout from the stratosphere with time and location; the other said there is no uniform fallout with location, and essentially the circulation is a seasonal one. These implications even had political importance, and these arguments about the circulation of the atmosphere were brought out in congressional hearings. Now we have at least a third layer to look at, the mesosphere and what goes on above this. We heard of one model which is based on sedimentation and essentially molecular diffusion. This model indicates that the higher up you inject the material, the longer it takes to get to the ground. This goes on indefinitely, although it happens fairly fast above a certain altitude, so it doesn't make much difference any more. If we put another layer on top of the stratosphere and add this too, we say the mesosphere on up is well mixed horizontally and vertically; we can say that we might actually have exponential feeding from the mesosphere into the next layer down. Then it gets involved with these other possible mechanisms, so we could have an exponential rate of some fixed residence time, essentially a residence time concept which is converted into seasonal pulses by circulation of the stratosphere. Then you would have implications with respect to time, or, within the stratosphere, you might have increasing residence time essentially, or transient times with altitude, and when you get to the mesosphere that should put a limit--that means your residence time is limited by the maximum residence time of the stratosphere. It has not been brought out in the meeting, except in attempts to interpret

VI. SCIENTIFIC IMPLICATIONS OF AN UPPER-ATMOSPHERE SAMPLING SYSTEM

Panel discussion: J. Z. Holland, moderator:
E. A. Martell, L. Machta,
W. J. Otting, J. F. Ahearne,
E. H. Fleming

Mr. Holland:

I will comment on some of these points, but I suggest that, as we get into this next panel, we handle it the same way instead of going around the table. In other words we will move now from the equipment and procedure group to the more general question of scientific implications.

Dr. Shreve:

I'd like to suggest an additional bit of informality. When a certain speaker is up and proposes an idea or makes a point, and there are questions on this point, let's not wait for questions until we walk all the way through the panel, since many inquiries are often lost this way. When a person is up there and makes a point with which you agree or disagree, stand and be heard.

Mr. Holland:

I don't know what this formula does; maybe it will blow up the place. I have tried to scribble some notes on the subject of scientific implications of upper air sampling systems during the course of the meeting. I must say, I have learned a great deal at this meeting. Particularly, I have much more of a feeling now that we must get information about the trace constituents and, in particular, radioactivity of the atmosphere in the range of 40 to 100 kilometers. I think things that have been said at this meeting convinced me that we in the AEC particularly, and a number of other people, would be derelict in not moving into these areas of studies. It is many jobs, in fact, and I think that some of the speakers before have indicated the diversity of studies that could be done along these lines. To me, two rather distinct philosophies have come out. One is what might be called the macroscopic approach and the other a microscopic approach. The macroscopic approach would deal with global distributions, many samples, statistics, maps, cross sections, inventories. It would require an inexpensive sampler, one which gives one or two variables or a small number of variables which can be measured with considerable certainty. . The microscopic approach would be perhaps the one that Dr. Magar indicated, where a sample is obtained the properties of which are precisely known: a total sample, if possible, which is analyzed extensively. This is exemplified in the programs that we have had with balloons in AEC and Air Force programs. In other words, the macroscopic approach is typified by the studies that Dr. Machta and his group have done of global inventories, changes with time, and distribution with altitude and latitude of a number of nuclides. The microscopic approach is typified perhaps by Dr. Martell and his group who have gone into the aerosol chemistry and physics, size distributions and their origins, isotope ratios in some detail, and searches for peculiar properties which would tell them some thing about the history of the material. In each case the object is to find out the origin of the material, how it moves around, and the mixing processes. It seems to me that you

Col. Russell:

I think that is a very well-made point. Certainly the Ash Can data with the frequency of sampling have demonstrated large month-to-month variations. I think another point should be considered: a good sample at 150,000 feet is a most precious item. We don't really have any. One would like to look at many things in such a sample, and I think considerable thought should be given to a sampling device which will satisfy, not only the interest of a given scientist or group, but other geophysicists and people interested in this area as well. For example, I would like to see what the Americium 241 and Tu 242 content of a 150,000 foot sample is. I think that would be vital, for example, in drawing some conclusions as to where Castle is. I think we might find some up there in greater proportion relative to other things that might be up there. Obviously, nobody is thinking about this kind of thing, and many other measurements were suggested by Dr. Machta, things that one would like to measure which don't seem to fit into any individual kind of sampling device that has thus far been proposed. One sampling device will do one thing and another will do something else, but I don't think we have given much consideration to a device that would get as many pieces of information as possible.

Dr. Shreve:

I guess the general idea here would be that anyone getting a sample he deemed successful should make public this fact so that anyone with special interests in analytical parts of the sample could propose for his own purposes that such and such an analysis be done. As you said, the first samples will be extremely precious things. The "getter" should not feel that he has full proprietary rights. I think that is implicit in what you are saying. My own opinion is that it is the only way to do it, because we want to get the most for the least. Each sample will be precious, not only because these will be only one or two of a kind, but because it took dollars to get.

Dr. Shreve:

Thank you. In light of these comments there might occur to you questions that you would like to address to the panel members, and I think we could take five to ten minutes for this purpose.

Floor:

I disagree with Dr. Fleming's interpretation of diffusion method. I think we will be able to collect data effectively.

Dr. Fleming:

I hope you are right, John. I just say from past experience that I have had in this business I am just a little bit skeptical, particularly when you get to higher altitudes. I hope I am wrong. I would like to ask a question of Dr. Magar. I don't exactly understand your last statement that, no matter how small a sample you get, if you take your time you are going to be able to find out what it is.

Dr. Magar:

I didn't mean to take time, necessarily. I said that if you have a good diagnostic laboratory you can analyze and keep on analyzing where you have the time to do this.

Dr. Fleming:

But isn't there a lower limit, given a specific technique for a specific application?

Dr. Magar:

My statement was not quite correct. If the sample is really too small, you might not have enough to perform the analysis.

Mr. Reed:

I have a question on a subject that seems to have been overlooked. Are we fundamentally trying to get a few measurements of conditions at a few levels, and assume that this integrates uniformly over the world? There are arguments in favor of uniform distribution. On the other hand, there are strong possibilities of non-uniformity, and a few measurements may be considerably in error if you attempt to integrate them around the world. My own feeling is that it may be better to try to hold down the cost and sophistication of individual measurements so that we can make a tremendous number of observations at different places and at different times in order to make a better integration than is permitted with a few precise and sophisticated measurements, assuming worldwide uniformity. In that respect, it might be good to direct our rocket development toward a minimum cost so that we can operate on a synoptic scale. We have billions of weather observations stored in the National Weather Records Center, and from this we are utterly unable to give good weather forecasts. I am afraid that a few measurements won't even begin to solve the problem for fallout prediction or future injection predictions. We have to have a lot of data even to get any kind of a handle on the situation.

Dr. Fleming:

I would like to confine my remarks largely to an assessment of the four methods we have heard described in the last day or two. One, the advantage of balloons, it seems to me, is that they can offer samples at a very steady altitude. It's the only system, apparently, which is able to do that. In principle, we can get large samples with it, and, of course, it is a cheap method. The disadvantage apparently is the ceiling. Also, I am beginning to wonder whether we can get samples by filtering much above 125,000 feet. I can't really comment on this in any scientific way. Two, the diffusive collection system has an advantage in that it gets a very large sample. Again, there is the question of whether it will work above 125,000 or 150,000 feet. It has a ceiling limitation of approximately 200,000 feet, presumably. You can't get a gas sample with it, and there is also a question of the efficiency of the sample gotten by diffusion techniques. Three, cryogenics really has no upper ceiling limit, but there may be trouble at lower altitudes, of the order of 100,000 to 125,000 feet, but it gets everything with the exception of helium and tritium, if it is tritium gas. If it is tritium in water, of course, it is taken. This includes all particle sizes even down to the molecular size, which I think is very important. This is one area, of course, where diffusion is lacking. Four, impaction seems to look good in principle: it again gets all particle sizes, and I think this could turn out to be very important. But again, it may have a problem in efficiency. My own feeling about this is that all these methods ought to be carried forward, as one man says,"with vigor" in the 1960's. I have heard several comments in the last two days about the need to improve radiochemical techniques. I am not denying this, but I'd like to point out that great strides have been made, even in the last few years, in this area, and I don't think it should be taken as an excuse for not going ahead on these various sampling systems.

Dr. Magar:

I don't really know what I can say in way of a summary. I would like to say that each one of the sampling methods mentioned here has its own areas of application. Perhaps the thing which I should stress is that it is possible to combine some of these various sampling methods to make an even more efficient sampler. For example, a combination of filtering technique and cryogenics could be used. Another thing that I would like to mention is that we have concentrated here on certain special problems of sampling, namely, those concerning radioactivity. There are other problems of sampling concerning chemical reactions in the upper atmosphere, for example, problems concerning detection of organic matter, and some of these techniques, particularly the cryogenic techniques, I think could handle these very well. However, again a new host of problems is created. Let me give an example. The problem of ceilings will be an extremely important one when we try to sample for chemical reactions which occur in the upper atmosphere. I think that the new problems and the new technology more or less by themselves generate new solutions, and the cryogenic sampling is one example of such a solution. I think we need to revise our thinking when we talk about these new solutions. For example, I have heard here during this meeting a considerable amount of concern regarding the size of the sample required. In view of some of these new techniques, this concern is not quite real to me because, if you have positively a given certain amount of material, you know how much you have of it and you can use very careful diagnostic techniques in your laboratory. No matter how large a sample or how small a sample you have, the diagnostic technique in the laboratory is at slow time. You can take your time to analyze it, and, if the diagnosis is good enough, you will be able to determine what you have in it.

(G)
Discussion: Sampling Principles and Rockets

Panel: J. R. Banister, moderator; M. Markels,
P. F. Gustafson, H. R. Vaughn,

Dr. Shreve:

I would like to ask each panel member to come forward and spend a few minutes giving his impressions of the meeting. I'll call first, for impromptu remarks, on the panel moderator, John Banister, to be followed in turn by the other panel members.

Dr. Banister:

I haven't got my thoughts too well in order yet, since I found out about this only about three minutes ago. The thing I think that has been left out of all of our discussions is that the sampling method is intimately related with the radiochemical analysis of the samples. I think that we are going to have to go ahead and try systems, having faith that there is enough stuff aloft to get an appreciable sample and that we will be able then to carry out an analysis. I think this will be the first stage of this program if we go into it. I think, then, that we have a second stage of refinement of technique, to develop something that is economically feasible after we gain more understanding of what conditions are that we are sampling under. I think another unknown is what the actual particle size is at this altitude. We should try to design systems which will sample over a wide particle range. On the sampling approach that Atlantic Research proposed, I would suggest that their system is somewhat more effective than they have indicated in their talk. I believe diffusion is actually a rather important phenomenon in the smaller particle range, and I think their analysis could be strengthened and improved by going to diffusion. Those are about all the comments that occur to me.

Dr. Markels:

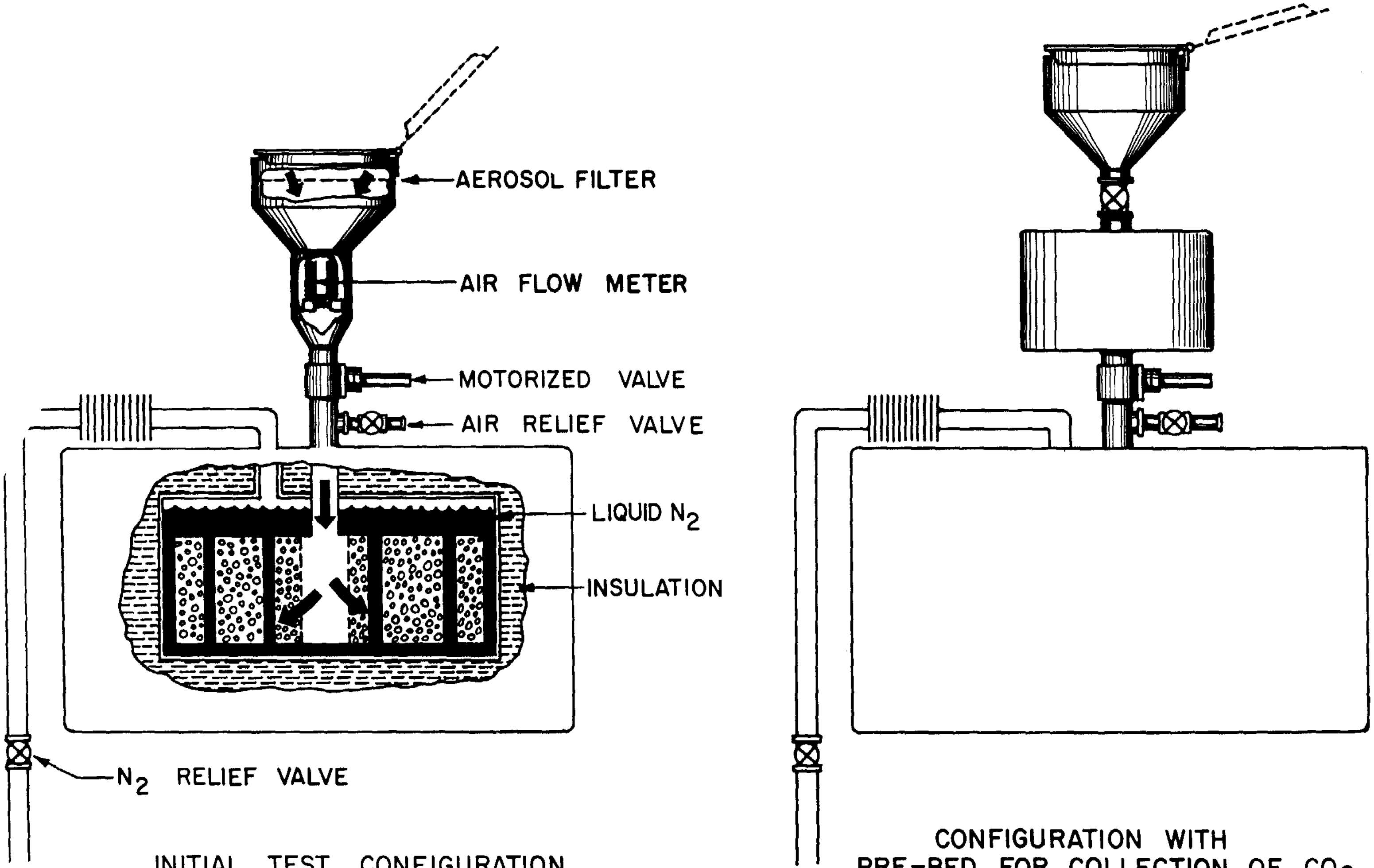
Two things have come to my attention in the course of the conference which I would like to mention. The first is that in this upper-atmosphere sampling problem, we are in a regime in which engineering possibilities for collecting the samples which were just out of range are now becoming possible. I presented an approach; Sandia has presented another one; we have a third in the cryogenic sampler. This indicates to me that there is a lot of room for creative thinking in solving the particular problems we face. As we think more and more about this and get more and more information about the physics and engineering required, the possibilities for inexpensive, effective sampling will actually improve rather than go the other way. I know that when we first started to think about this business, it looked hopeful but very difficult. We now seem to find two, or maybe three, systems that look quite feasible and require only the necessary funding and support to produce effective sampling. This brings me, then, to the next thing I would like to share with you: namely, that one of the big gaps that is holding back our imagination and holding back our working on some of these newer ideas is our lack of knowledge about a lot of the basic science of what may be going on up there - the particles and how they interact with filters, how they interact with devices such as impactors, what the aerodynamics might be, what the particles are themselves, what the atmosphere is doing, and so on. I suggest that there may be a very fertile field here for rather basic research on individual component areas of this whole problem with which we are dealing, and that such research, in support of these individual areas which make up the whole technological field which we are attaching, might be very worth while.

Dr. Gustafson:

During the course of this meeting, I have been very impressed with the amount of work that has gone into particle collection devices, filter devices, things which are very sophisticated in comparison with the filter systems which we have used in the limited amount of work we have done in this area. The comments I would like to make are in regard, not to these devices, but rather to another area which I don't believe would substitute for this type of sampling, but which might lead to the collection of a considerable amount of auxiliary data. This is gamma-ray spectrometry, particularly in situ measurements of the atmosphere as far out as one can get, whether this device is mounted on balloons or in rockets. This is, to my knowledge, something that has not been done with any great degree of fervor. Measurements have been made in balloons, but the things that have been looked at are not in the fission-product energy region nor in the region where natural activity such as beryllium emits. At first glance, the problem is a difficult one, but I think so is that of particle collection. It is a matter of how much time and effort one wants to devote to this to effect a reasonable degree of success. Certainly the first looks we had at the composite gamma spectrum of soil were anything but encouraging. Therefore, I don't know that the great secondary showers of particles that are going to becloud the characteristic line of the various fission products need discourage us to the point where we don't at least try it. I would like to make a plea that this be done, and, in the limited way at Argonne that we are able to, we are going to try to do our part in this portion of the program.

Mr. Vaughn:

I am a member of the Aerodynamics Department here, and, as such, my primary interest is in helping to furnish the vehicle to do the job. In order to do this properly we should have a very good idea of the very best means of sampling as well as the sample size. My impressions from the talks have been that, from an AEC, or particulate sampling standpoint, probably the best means would be an impaction - diffusion device, and the rotating-type vane scheme seems to be one of the best. I have also gotten the impression that cryogenic-type devices will be very important for whole-air sampling. It is not clear to me whether the AEC should undertake this sort of thing or not, but I can certainly see a lot of value in it. I have the impression that the sample size of 1000 cubic feet of air would be satisfactory, but there seems to be quite a bit of doubt along this line. We have been trying to get something on the order of 10,000 standard cubic feet of air. This may be more than we would need. However, I do want to make one point that wasn't brought out in our discussion: the rockets that we would use in the development of this device are surplus types that cost $2000 apiece or less. So, from a development standpoint, the size of the sample is not too important. However, in a final device, the size would be important as well as the number of shots that might be required. It may be too far in the future to try to determine how many shots would be necessary, but this might give us an insight into the design needs of the final device. One thing I feel very strongly about: there have been a number of papers indicating very strongly that at least there is something up there, so I feel somewhat more confident about being able to get a good sample back.



INITIAL TEST CONFIGURATION

CONFIGURATION WITH PRE-BED FOR COLLECTION OF CO_2, H_2O AND OTHER TRACE GASES

Fig. 5 -- Particle sampler configurations

To summarize with respect to future capabilities, our main objective is to achieve a sampling capability at altitudes significantly above 100,000 feet. Presently available balloons can lift substantial loads to 140,000 feet. This will eventually be increased through use of 14-million-cubic-foot balloons designed to lift 200-pound loads to 150,000 feet. Although suitable vehicles are available, sampling techniques tailored to low density operation must be developed. These may utilize alternating current blower system and high speed air turbines. The liquid nitrogen cooled adsorption pump also appears to be a promising approach to the collection of particles and gases of interest.

References

1. General Mills Report No. 1903 to Division of Biology and Medicine, USAEC, Contract No. AT(11-1)-401, 1 May 1959.
2. General Mills Report No. 1907 (as above), 1 July 1959.
3. E. A. Martell, Letter Report Dated 29 May 1961, to Mr. J. Z. Holland, USAEC.

down 40-foot lengths of polyethylene duct to avoid re-entrainment. At the end of the sampling period the exposed zeolite is dropped into canisters located under each frame. Monitorized valves close to prevent contamination.

Recovery of gases collected on the special test flight was performed using a procedure developed at this laboratory. This will not be discussed in detail, but it involved heating the zeolite and subsequent recovery of desorbed gases in a system of cold traps. The results of this test are given in tabular form below.

MOLECULAR SIEVE GAS SAMPLER

Experimental Flight March 15, 1961
Goodfellow AFB, Texas

	Unit #1	Unit #2
Altitude	72,000 feet	72,000 feet
Total Volume at STP*	678 ft^3	657 ft^3
H_2O Recovered	1.22 grams	2.97 grams
CO_2 Recovered	6.20 liters (STP)	6.30 liters (STP)
H_2O Mixing Ratio	0.049 gm/kg	0.120 gm/kg
CO_2 Concentration	0.032 percent	0.0338 percent

*Volumes obtained using calibrated PR-2 Flow Meters.

For both units the amount of CO_2 recovered was in reasonable agreement with the theoretical value which assumes a constant mixing ratio of 0.030 percent. The slightly higher values actually obtained are attributed to uncertainties as to the exact operating altitude and small errors in flow measurement.

With respect to water vapor, the mixing ratio obtained from Unit No. 1 is in reasonable agreement with values obtained by other workers in the U. S. The mixing ratio from Unit No. 2, however, was almost three times as great and this is attributed to an unfortunate last-minute change in that sampler's configuration involving the mounting of a large cardboard sun-shield over the sampler intake. This subsequently served as a source of contaminating water vapor at altitude. Additional tests of the zeolite gas samplers are planned for the near future.

Future Capabilities

In examining our present capability we find that there are a number of areas where improvements can be made. First, with respect to radioactive particulates the point is now being approached beyond which further advances in accuracy must include improvements in sample analysis. Second, the sampling capability should be extended to altitudes appreciably above 100,000 feet; and finally, at these higher altitudes there will be a need for improved light weight collection systems which can efficiently sample the larger volumes required.

Under a present contract with the Atomic Energy Commission we are working to improve our position with respect to the latter two areas.

One approach involves an investigation of alternating current systems for balloon use. Solid-state technology has now produced highly efficient light weight inverters that offer to make practicable the use of alternating current devices on balloon platforms. Devices of this type weighing less than twenty-five pounds can now deliver 3 KVA of 400 cycle power with close frequency control and efficiencies exceeding 85 to 90 percent. With respect to high altitude sampling this will allow the use of low-friction, brushless motors delivering a given horsepower at high efficiencies and lessening the critical heat dissipation problem. There will be little, if any, weight penalty in going to AC systems since the somewhat higher total efficiency should result in lower battery weight.

High altitude operation also offers some interesting advantages with respect to the use of cascade impactors. At low densities, efficient impaction can be obtained at relatively low jet velocities. This means that we may be able to abandon the relatively inefficient constant volume pumps with their low sampling rate (22 cfm) and go instead to multistage turbines which will deliver much higher volumes, giving the radiochemists more adequate samples to analyze.

A second, entirely different approach to high altitude sampling is largely an outgrowth of our past work with crystalline zeolites. This involves the idea of using an evacuated zeolite bed as a pump. If such a bed is maintained at liquid nitrogen temperatures (-196°C), ten grams of Linde Type 13X zeolite will adsorb over one standard liter of dry air at pressure altitudes equivalent to 170,000 feet (0.5 mm Hg). This is a fact we have verified many times in the laboratory. Furthermore, this process is quite rapid and tests indicate that at 130,000 feet, 85 pounds of zeolite will be able to adsorb over 100 standard cubic feet of air in less than 100 minutes.

Figure 5 shows in principle the basic features of a particle sampler which utilizes a liquid nitrogen cooled zeolite pump. Such a sampler is presently being designed for flight tests in late summer 1961 and this unit is expected to have the following characteristics:

WEIGHTS

Zeolite Adsorbent	85 lbs
Liquid Nitrogen	45 lbs
Containers and Hardware	70 lbs
TOTAL	200 lbs

PERFORMANCE GOALS

Volume Sampled	100 + scf
Sampling Rate	1.0 scf/min
Sampling Altitude	To 140,000 feet

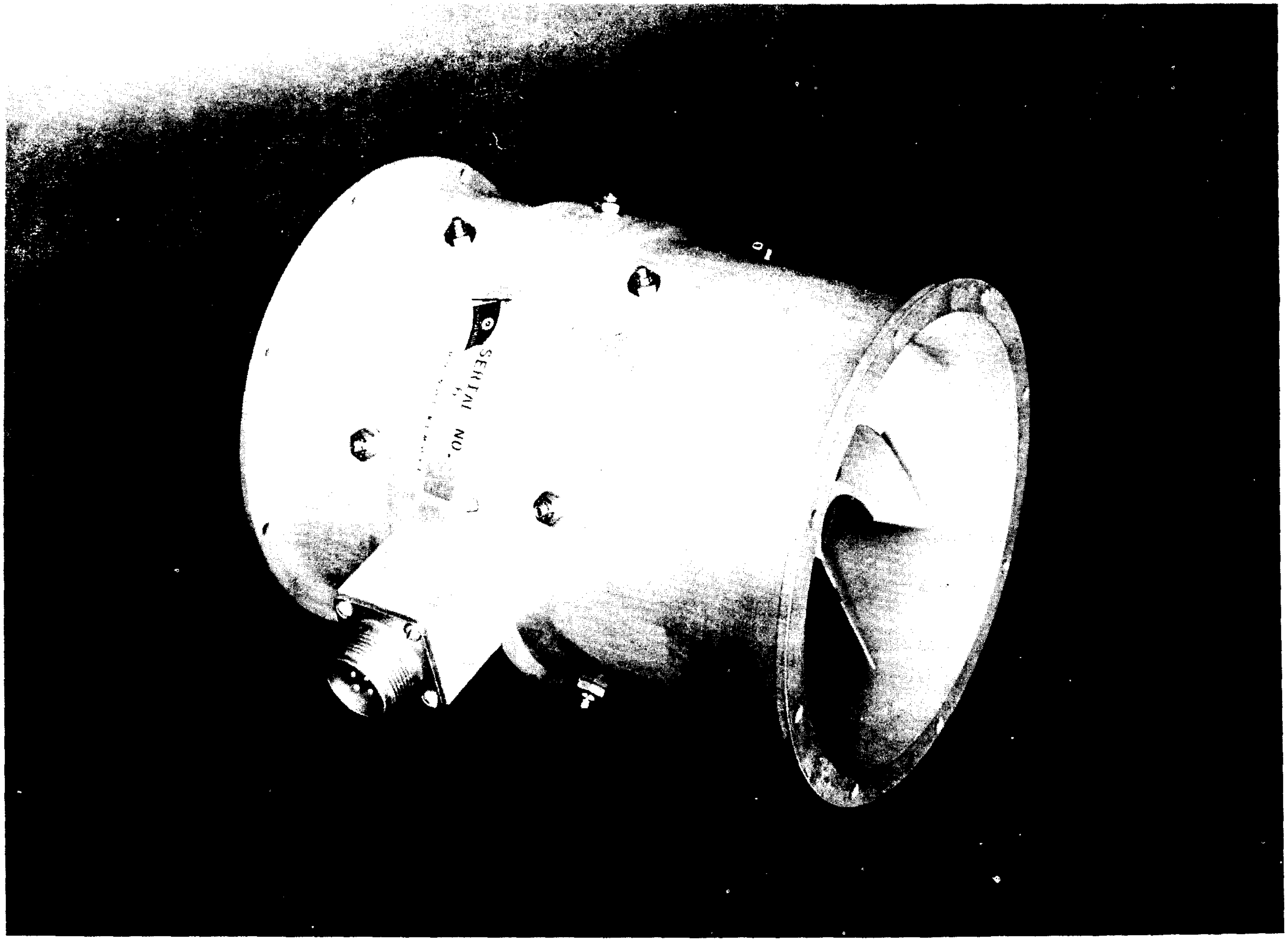


Fig. 3 -- PR-2 high-altitude air-flow meter

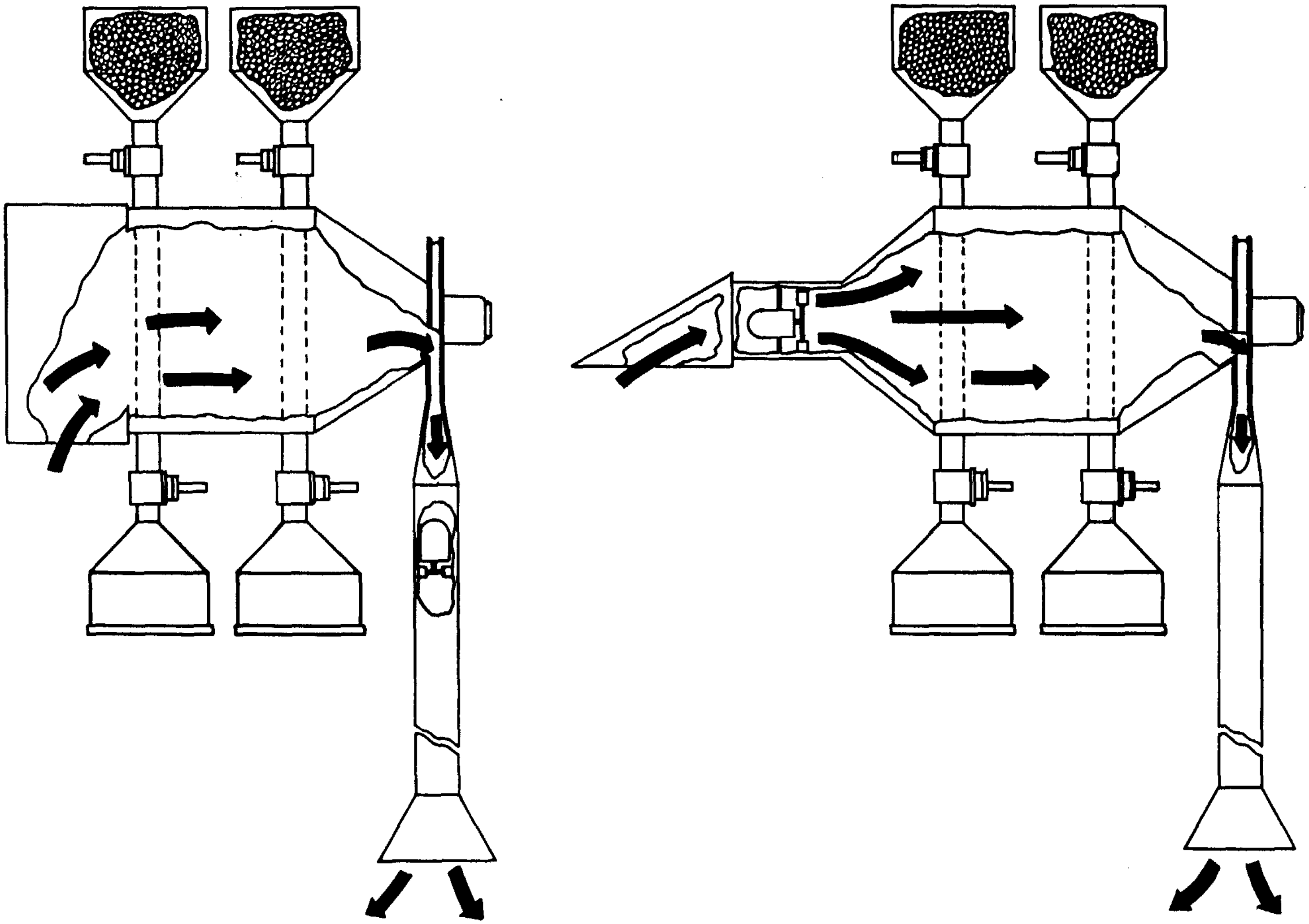


Fig. 4 -- Gas sampler configurations

The impactors have been described in past AEC reports.[1,2] This discussion will involve only the filter samplers.

Figure 2 shows the general configuration of two filter samplers developed at General Mills: (1) the Sub Micron Aerosol Collector (SMAC) unit developed for AFCRL, and (2) the Direct Flow Unit developed for the AEC. Both of these samplers utilize the same air mover, a single-stage Torrington blower powered by a half-horsepower Westinghouse aircraft 27 VDC motor. Both sample at about the same rate, drawing approximately 500 cfm at 80, 000 feet and 300 cfm at 90, 000 feet.

Both samplers have high efficiencies (virtually 100 percent) but they differ in the way that these efficiencies are achieved.

The AFCRL unit uses a Delbag or Stanford Research Institute polystyrene fiber mat having 100 percent efficiency at all flow rates. Because of the large pressure drop of this type of filter, ten square feet are utilized in a cylindrical configuration.

The AEC direct flow sampler utilizes the more porous IPC filter mat and achieves high efficiency by maintaining high linear velocities (above 500 ft/min) through one square foot of filter. The direct flow system with batteries weighs approximately 140 pounds. The AFCRL system is only a few pounds heavier. It should also be pointed out that isotopic analyses of filters obtained from the SMAC and Direct Flow samplers flown together on three comparison flights during 1960, indicated close agreement in terms of dpm per standard cubic foot of air sampled.[3]

This agreement is, in part, attributed to the use of the PR-2 high altitude air flow meter developed for the Atomic Energy Commission and first put into use during late summer, 1960. This unit shown in Figure 3 is seven inches long and five inches in diameter. It is ordinarily mounted at the blower outlet and will measure flow rates in excess of 1000 cfm through an altitude range to 120, 000 feet. Present accuracy is plus or minus 5 percent.

In addition to the collection of particles, this laboratory has also been involved in work dealing with the collection of gases, H_20, CO_2, Tritium, Carbon-14, and others. In recent years investigations have dealt with a method utilizing the adsorption of these gases on crystalline zeolites. After extensive laboratory work an experimental gas sampler was designed and test flown on several balloon flights. This unit performed well in early tests but gave doubtful results in later field trials at San Angelo, Texas . Because of these results this laboratory received permission to perform a special test flight, conducted under the supervision of General Mills' personnel and utilizing this laboratory's facilities for preparation of the zeolite rather than having this done by an outside contractor.

For comparative purposes two slightly different configurations were flown for sixty minutes at 72, 000 feet. These are shown in Figure 4. Prior to collection the zeolite material is stored in sealed canisters at the top of the sampling units. Upon reaching float altitude the motor blower is turned on for five minutes to clear entrained water vapor and other gases from the system. At the end of this period the canister valves open, allow- the zeolite pellets to fall into frames between retaining screens. Air is pulled through the adsorbing beds for sixty minutes at a rate of approximately 220 cfm. PR-2 air flow meters monitor total flow and air is exhausted

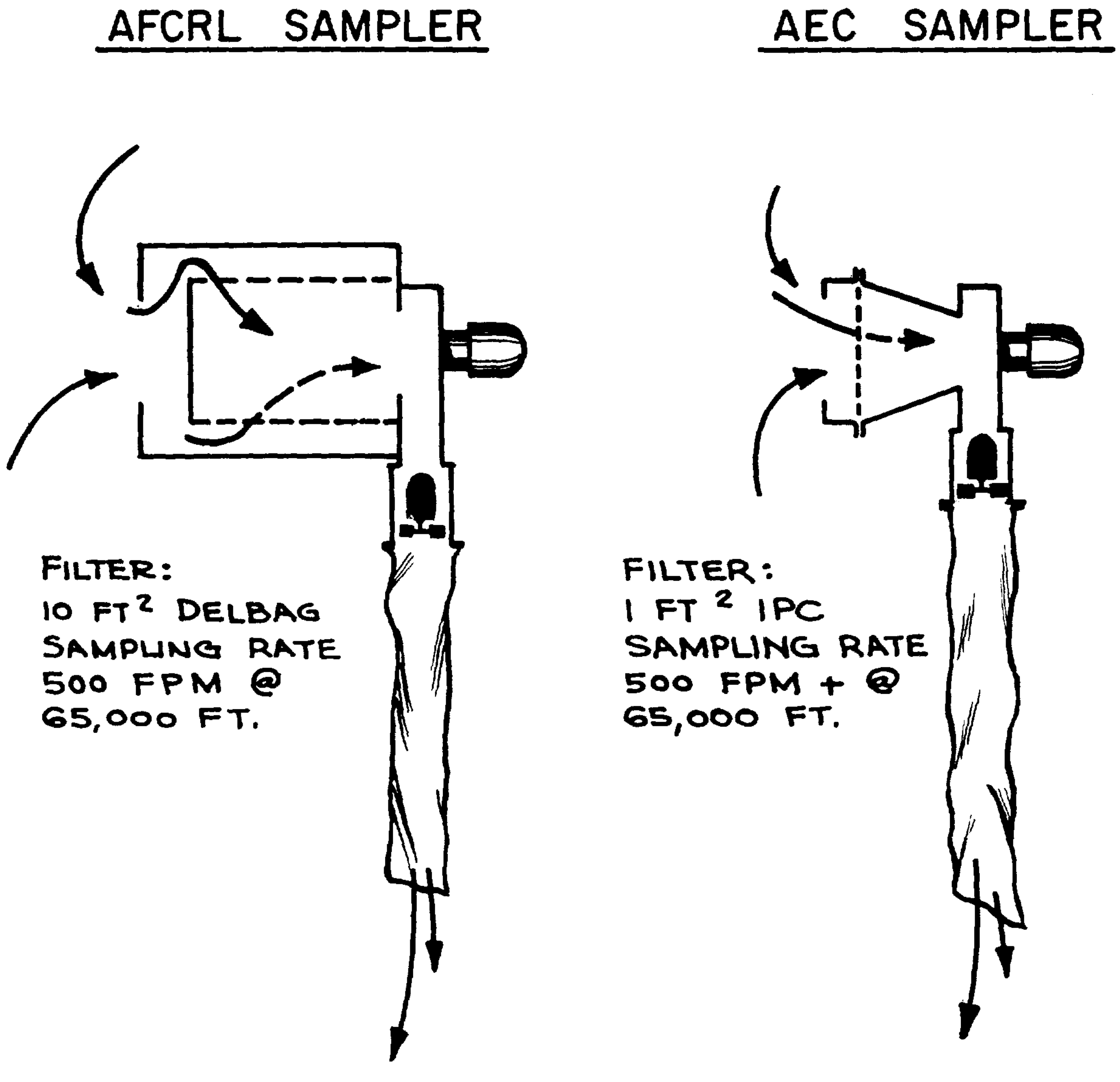


Fig. 2 -- Filter sampler configurations

(F)

Some Recent Developments in Balloon-Borne Stratospheric Sampling Systems

R. C. Wood
General Mills, Inc.

Introduction

As the title implies, this paper contains a short discussion of balloon-borne stratospheric sampling systems. More specifically, it describes equipment and techniques developed at General Mills, Inc. in connection with research programs conducted under the sponsorship of the United States Atomic Energy Commission, the Air Force, and other agencies. In this outline, three general areas are covered, some perhaps superficially. They include:

(1) Some brief facts concerning balloons in general, load carrying capacity, altitude capability and cost of research flights,
(2) A description of present sampling methods employed by this laboratory with a few recently added improvements, and
(3) Some promising new approaches toward an increased sampling capability at higher altitudes.

Some Balloon Facts

In order to convey some idea of the variety of balloon designs available for stratospheric research flights, we may refer to Figure 1 in which each point represents a specific polyethylene balloon designed to carry a given load to a given altitude. The curved lines superimposed merely serve to classify these designs roughly according to size in terms of millions of cubic feet (fully expanded at altitude). At present, a 200-pound load can be lifted to 140,000 feet using an eight million cubic foot balloon costing approximately $8000. Not shown on Figure 1 is an existing fourteen million cubic foot design which can lift 200 pounds to 150,000 feet.

Present research balloon flights at Minneapolis operating within the altitude range of 50 to 90 thousand feet carry, routinely, loads averaging 500 to 1000 pounds. The average cost of balloons used in these flights ranges between $1000 to $2500. An average flight operation costs $7000 to $10,000, including equipment preparation, launch and recovery.

Present Sampling Methods

On research flights performed at Minneapolis, two basic methods have been employed for particulate sampling:

(1) Cascade impactors which sample twenty-two ambient cubic feet per minute, and
(2) Filter samplers which operate at rates in excess of 500 cubic feet per minute.

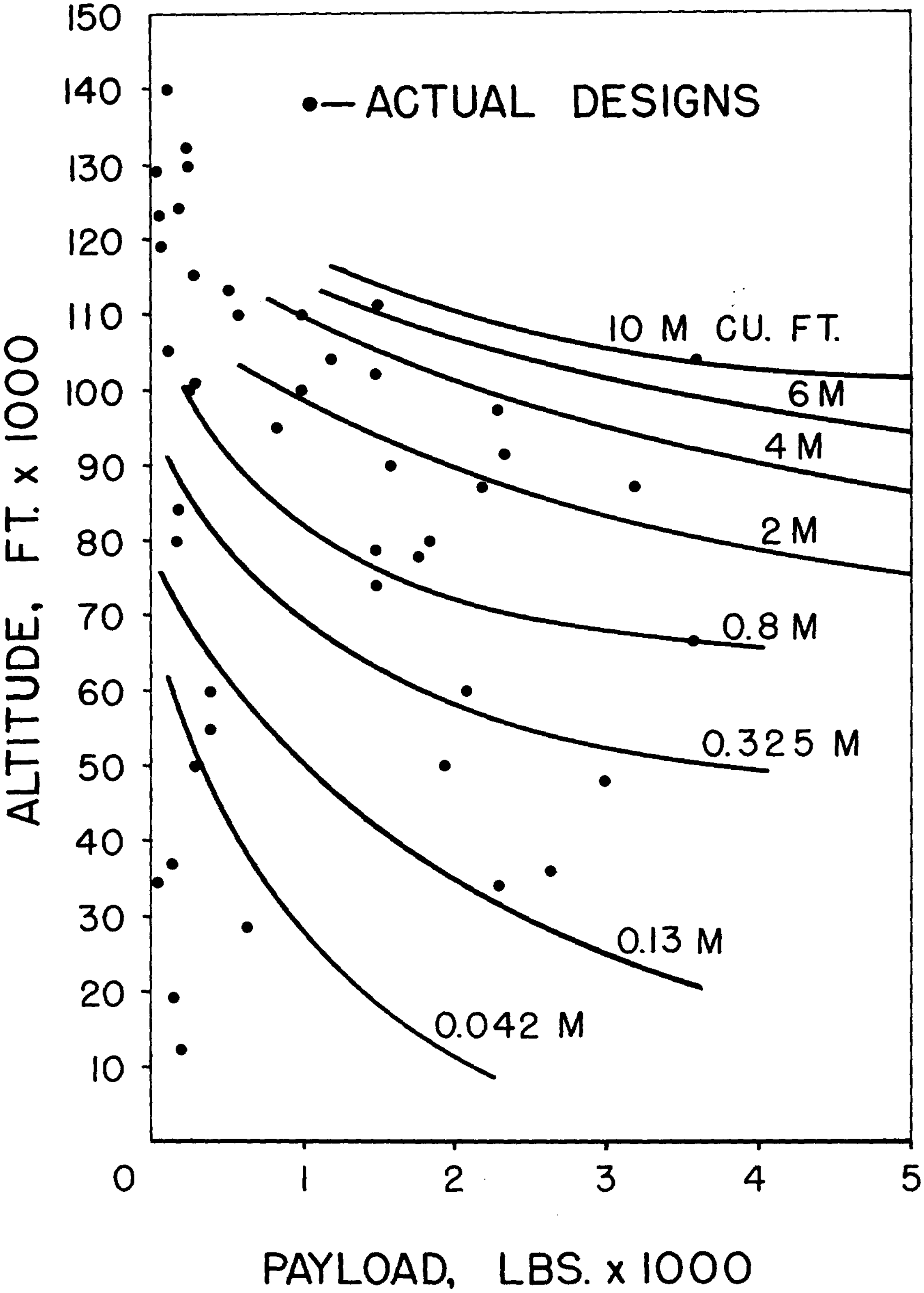


Fig. 1 -- Payload versus altitude

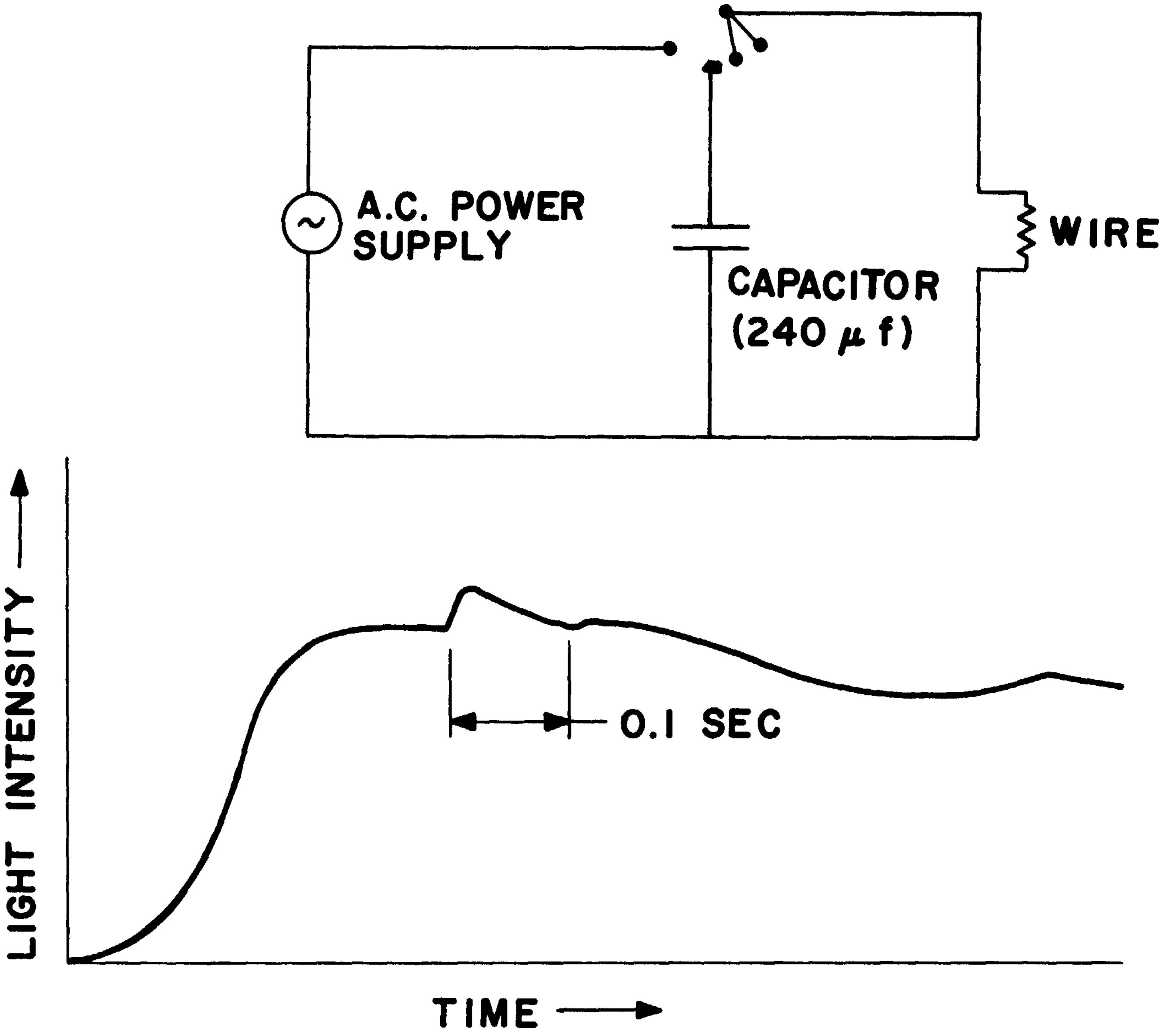


Fig. 8 -- Evaporator wire circuit

The test chamber has been made transparent to enable us to visually check its flow properties to determine if any aerodynamic modifications are necessary. Pressure transducers are located approximately one inch on either side of the filter to enable us to determine the pressure drop through the filter as well as the absolute pressure level.

Pressures to be measured during this phase of testing will vary from 0.1 to 10 mm of mercury. We presently have available a variety of pressure transducers that are being used in our hypersonic wind tunnels that will cover this range. Preliminary calibration of these transducers have indicated a probable accuracy of ± 5 to 10 thousandths mm of mercury.

The first series of tests will be run at constant mass flow, varying pressure and velocity with time. The initial test conditions will be mass flows from 10^{-5} to 10^{-2} lbs/sec and pressure altitudes from 90,000 to 200,000 feet. Later, a Roots vacuum pump can be attached to the test section and any combination of expected sampler velocities and altitudes up to 300,000 feet can be simulated for extended periods of time.

Chemical analysis of the evaporator wire and the filter will enable us to determine relative filter efficiencies. The first filters will be cup-shaped to cover the entire inside surface of the test chamber as shown in Figure 7. This will enable us to determine if any of the injected particles are diffusing to the test chamber walls. The filters will be examined in sections to determine whether the cup shape is necessary.

Figure 8 shows the evaporator wire circuit and a copy of an oscillograph trace taken during the preliminary work with the evaporator wire. This simple circuit enables us to quickly bring and hold the wire to any given temperature by varying the current to the wire.

The oscillograph record is a plot of light intensity measured by a photocell versus time. It indicates the attenuation of a background light by the particles coming off the evaporator wire and condensing on the test chamber walls. The plot clearly shows the effect of the capacitor in bringing the wire up to temperature quickly. It also indicates attenuation of the background light and the wire light by the material evaporating from the wire and condensing on the test chamber walls.

Preliminary tests with the evaporator wire technique have established a relation between the temperature and the resistance of the wire. It is estimated that particles of 1 to 10 molecules in size can be produced by this method, depending on the evaporation rate. We have a theoretical relationship between particle size and wire temperature which we are going to try to substantiate with some experimental results. For example, we might use the technique of introducing slides into the particulate flow and then examining these slides on an electron microscope to see if we can determine particle size.

In the preliminary tests, tungsten wire with a Cadmium coating, which can be readily detected in the filter materials, was used. As filter testing progresses, other more standard methods of particle injection may be used to produce larger particles in the submicron-size range.

You might well ask: what is the meaning when we have an overlap of the two processes? We haven't looked at this question in any detail. It seems to me that we are then talking about kinetics ballistics problems rather than a clear-cut case where we may speak of diffusive or impaction collection. It does seem clear that the collection mechanism, however you chose to name it, is effective.

This, then, represents our current thinking on diffusion collection, and its combination with impaction to secure an effective sampling system above 100 kilofeet. Perhaps I should mention again what I have already implied: we are not talking about normal screening action which one expects from a filter. Instead, we are talking about a regime where new phenomena that are not important at atmospheric pressure do become predominant. In fact, looking at our curves, you can see that, at lower altitudes, neither impaction nor diffusion collection would be very effective. Then screening, electrostatic attraction of particles to the filter media, and other such effects become predominant.

Because we are talking about somewhat unfamiliar collection mechanisms for conventional filters, we feel that experiment is necessary, both to examine the flow properties of the filters and to check out the effectiveness of these collection mechanisms. Mr. Rigali will talk about the experiments we are planning and the hardware we will use in these experiments.

References

1. Cook, T. B., et al., High Altitude Measurements, Operation Hardtack WT-1601, Sandia Corporation, to be published. (See ITR-1601.)
2. Banister, J. R., A Computation of the Sampler Performance for Shots Teak and Orange of Operation Hardtack, SC-4172(TR), Sandia Corporation, December 1958.

Ground-Based Testing of Proposed Sampling System (D. J. Rigali)

The ground-based testing of the proposed Sandia sampling system will be done in two parts. The first phase will be a filter test setup consisting of a 3-inch-diameter test section and a 50-cubic-foot vacuum system. This will be used to determine the best filter materials available by a comparison and elimination process. Then, an extensive evaluation of the best materials will be continued to determine filter efficiencies and pressure drops at varying values of mass flow, pressure, velocity, and temperature. The equipment for this set-up is partly complete and could be ready for preliminary testing very soon.

The second phase of the ground-based testing will be a rotating wing set-up in a presently available 10,000-cubic-foot spherical vacuum tank. This set-up will be used to check out the full-scale design and operation of the complete rotor assembly at simulated altitudes up to 150,000 feet. With the planned addition of a new vacuum pump, altitudes up to 300,000 feet can be simulated.

The purpose of Phase I is to find a filter material or materials with a high efficiency and yet high porosity to give a relatively low pressure drop. The test chamber to be used is shown in detail in Figure 7. It consists of a quick-opening valve, replaceable orifice plates to provide any number of constant mass flows, an evaporator wire to provide particle injection, a 3-inch-diameter test chamber with transparent walls, and, finally, the 3-inch-diameter filter.

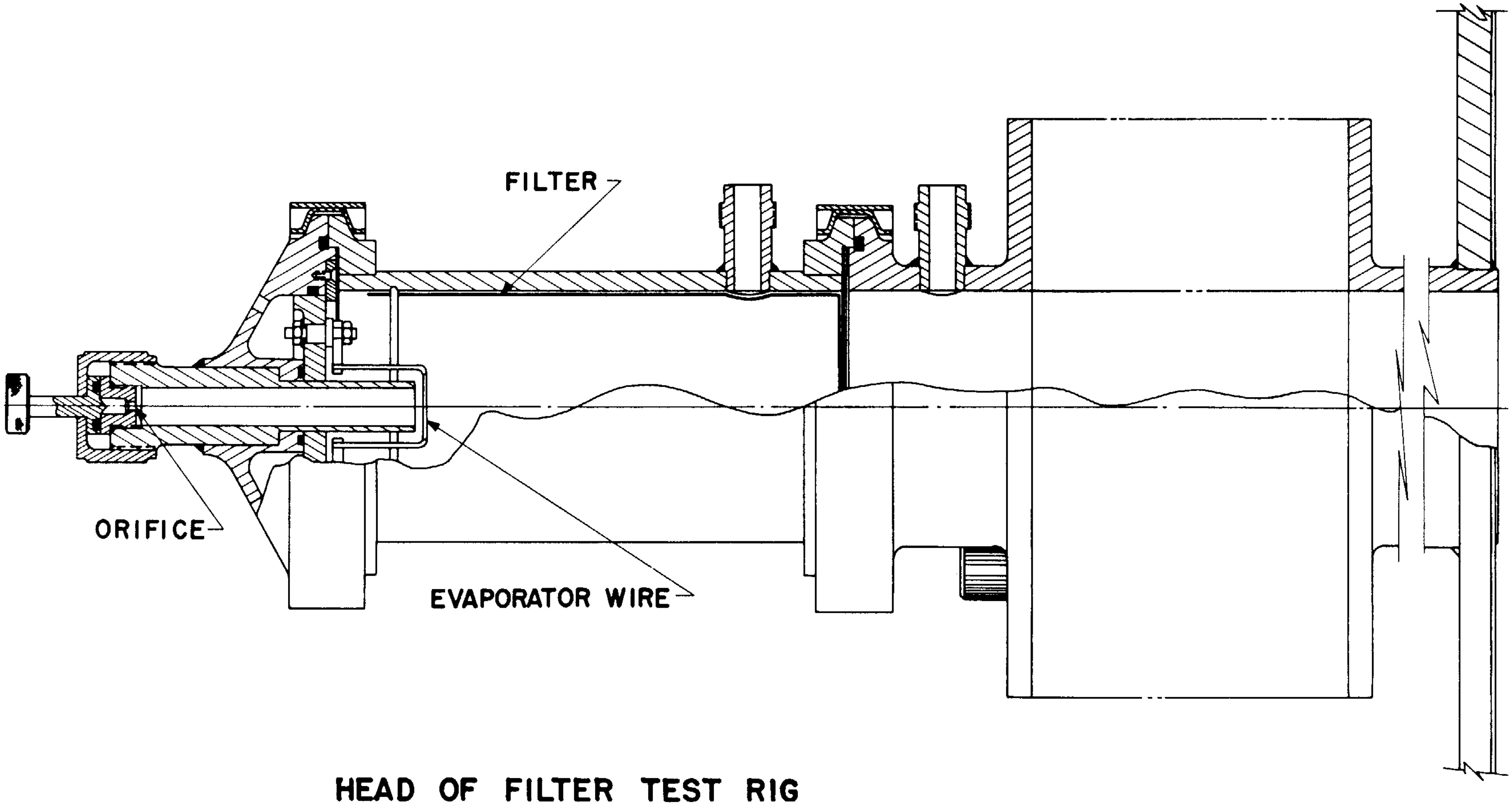


Fig. 7 -- Filter material test chamber

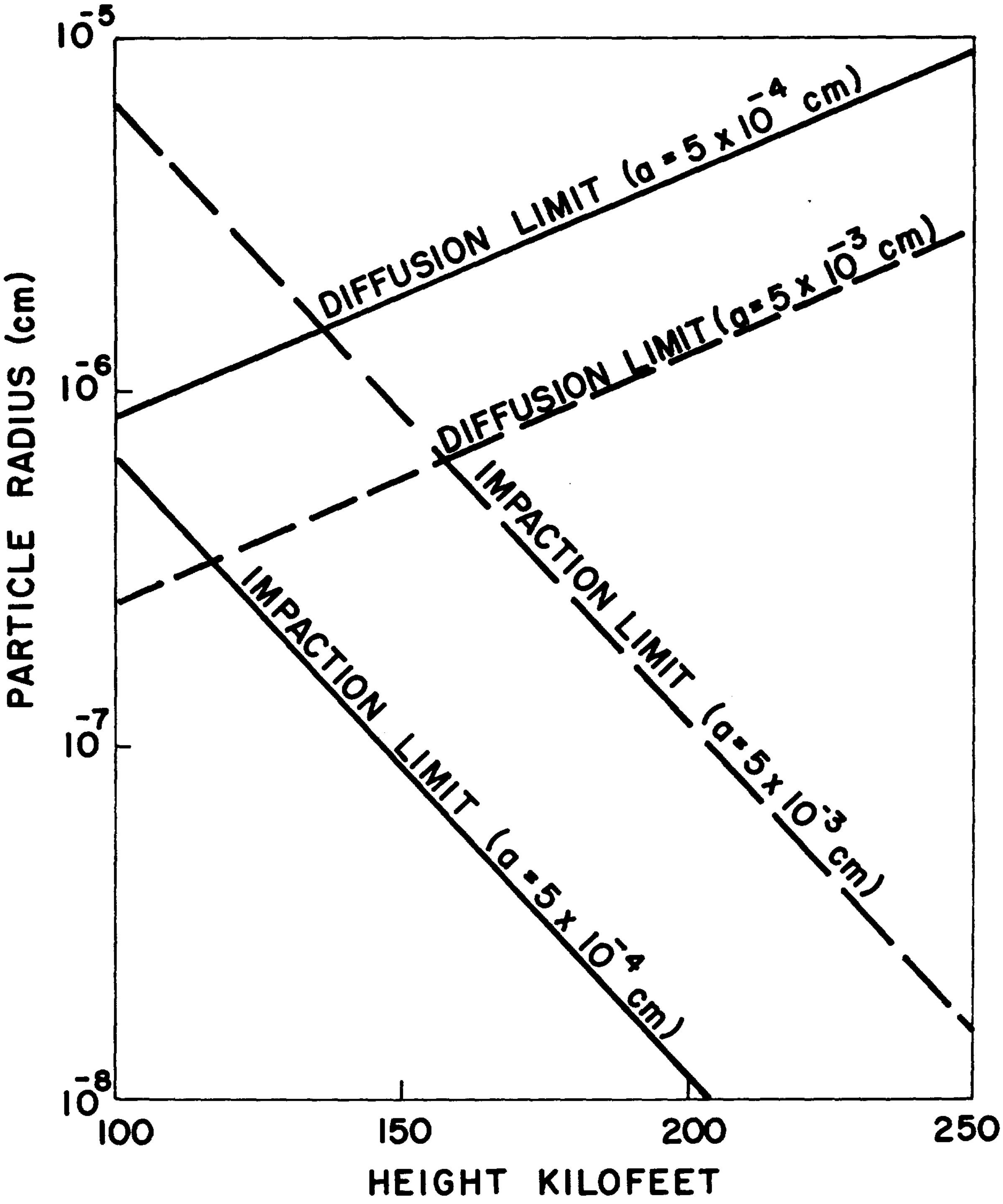


Fig. 6 -- Limits of fully effective collection for diffusion and impaction for 10-micropore (a = 5 x 10^{-4} cm) and 100-micropore (a = 5 x 10^{-3} cm) filters. The flow conditions and other filter characteristics assumed are given in the text. Diffusion collects all particles smaller than its limit, while impaction collects all particles larger than its limit.

DIFFUSION EQUATIONS

$$\frac{C}{C_o} \cong e^{-\alpha}$$

$$\alpha = \frac{\pi^2 DK(AF)}{2a\bar{v}}$$

IMPACTION CRITERION EQUATION

$$K \cong \frac{\delta r\bar{v}}{4a\mu\rho(AF)}$$

Fig. 5 -- Diffusion and impaction criterion equations

viscosity. Here we notice that the flow velocity through the filter is proportional to the pressure if we ignore the relatively unimportant changes of viscosity and temperature with altitude. This means that if one wants to sample at a constant rate, one would have to change the velocity of the sampler as the vehicle descends through the atmosphere in order to achieve a constant rate.

In general, as one descends through the atmosphere one would expect first to have the free molecular equations apply, and then as one got to lower altitudes where the mean free path was less, one would expect to move into the region where the Poiseuille flow equations are appropriate. Neither expression describes the flow through the capillary very well for a considerable range about the region when the air molecule mean free path is comparable to the capillary wall. The experimental evidence is scanty, but it indicates that there is a gradual transition between the two. For the rest of the discussion, I shall assume that we have chosen a filter which has a sufficiently small pore that one can describe the flow through the filter as always being in the free molecular region, that is, that there is a constant-flow velocity through the system. While I shall on one occasion talk about a case where this assumption would not be justified, I shall then make the assumption that we have somehow managed to alter the sampler velocity to achieve a constant-flow velocity through the system.

Figure 5 gives the diffusion and impaction criterion equations. We have derived the diffusion equation by assuming, somewhat inconsistently, that the capillary is no longer circular but square. This assumption allows a substantial simplification of the mathematics. One then sets up the boundary condition that the distribution of contaminant is uniform across a cross section of the tube as the flow enters it, and that the concentration of contaminant then drops to zero at the edge of the flow, since material there is condensed on the wall. After a couple of pages of juggling, one comes out with this expression which describes the collection efficiency. My symbols have the same meaning as they did in the flow equation, except that C now stands for debris concentration, while D is the diffusion coefficient. For the impaction criterion, I have assumed that my capillaries were not truly straight, but have sufficient curvature that a particle moving straight would strike the wall after traveling four radii. This is quite arbitrary, but should describe in an approximate fashion the impaction effectiveness on the filter. Impaction is deemed effective for values of K greater than one.

Figure 6 summarizes the result of combined diffusion and impaction collection for a reasonable flow situation. To obtain these curves, I have considered pore sizes 10 microns and 100 microns in diameter. I have also assumed that the ratio of the length of the capillary to the radius of the capillary is 10, which gives us that value for K. For a sampler velocity we use 1.34×10^4 cm/sec, which is an effective average for the system Mr. Matejka described. For the value of the product (AF) we chose 7.5. This would, for example, correspond to the filter being 75 percent open with a fold factor of ten. Turning to the flow equation we find for the 10-micron filter that the flow may be calculated using the free molecular description, and the average normal flow $\bar{v}$ is 19.5 percent of the sampler velocity. The 100-micron pore filter flow would display a transitional behavior, but we assume that the sampler velocity during the descent is altered to achieve the same value of $\bar{v}$. Applying the diffusion and impaction criterion, we then obtain these performance curves for the two pore diameters. Diffusion collection is effective for particles smaller than the limit given by that curve, while impaction is effective for particles having diameters longer than the limit curve for that phenomena. We see that for a 10-micron pore diameter, the impaction and diffusion collection processes overlap throughout the entire range from 100 to 250 kilofeet. On the other hand, for a 100-micron filter, there is an overlap from 150 kilofeet upward, but there is a gap between the two from this altitude downward. This suggests that we are limited to fairly small pore sizes in our filters if we are to rely on these collection mechanisms.

MOLECULAR FLOW EQUATIONS

$$Q_m = \frac{4}{3}\left(\frac{2\pi}{RT}\right)^{1/2} \frac{a^3}{\ell} \Delta p$$

$$\bar{v} = (AF)\left(\frac{2}{\pi RT}\right)^{1/2} \frac{1}{K} (V - \bar{v})^2$$

POISEUILLE FLOW EQUATIONS

$$Q_m = \frac{\pi}{8} \frac{a^4 \bar{p}}{\eta RT\ell} \Delta p$$

$$\bar{v} = \frac{(AF)a\bar{p}}{32\eta KRT} (V - \bar{v})^2$$

Fig. 4 -- Flow equations for capillaries and assemblies of capillaries

of events performed to obtain a sample of particulate matter from the atmosphere. According to Dr. Machta's data presented yesterday, our sampled volume will be sufficiently high that we could use a portion of the flow through passages in each of several altitude zones.

To ensure a minimum number of shots to develop such a sampler, reliability of the components and operation of the complete system should be tested in ground facilities. Figure 2 shows planned modifications to an existing 26-foot vacuum sphere at Sandia. The sphere is intended for use primarily to test mechanically all sampler components. Specifically, rotor hardware strength and thrust requirements for given rotational speeds at varying altitudes, together with flutter characteristics, will be investigated. In the latter developmental stages, such items as the deployment, retraction, and sealing of the sectional rotor blades will be tested. Finally, tests may be made in the vacuum sphere to determine flow patterns through and around the sampling rotors, and to collect samples of particulate matter that have been injected into the sphere. Density altitudes up to 150,000 feet can be simulated in this sphere with existing pumping facilities. The rotor blades would house flow-through passages that contain filter material for the diffusive collection of debris present in the sampled air.

For a given expected particle-size distribution, there should be some filter material with a specific porosity that will most efficiently obtain a debris sample. Considerable filter testing must therefore be done to ensure selection of optimum filter material for use in the sampler. A filter test rig for this type testing is now being fabricated. Figure 3 shows this test equipment. Mr. Rigali will present the details of the equipment in the final portion of this presentation.

Dr. Banister from our Physical Sciences Research Department will next present the theory of diffusive collection.

Theory of Diffusive Collection (J. R. Banister)

Mr. Matejka has just finished describing a possible rocket system which samples by means of a rocket-impelled system that sweeps up debris. I would now like to talk about the possible filtering media one could use with such a vehicle. It might be worth mentioning that our thinking in this regard has come out of the design calculations we did for the impactor sampling system developed for the early sampling of the debris of Teak and Orange shots of Operation Hardtack. These calculations are described in detail in WT-1601[1] and Sandia Report, SC-4172(TR).[2] Two important principles emerge from these calculations. For the Teak shot, we were using an impactor arrangement of canted parallel plates which were separated by one centimeter. We found that this impactor arrangement was theoretically extremely effective at low air densities. In fact, at 250 kilofeet, the collection by impaction was essentially perfect for particles down to 10^{-7} cm in diameter, when the rocket speed was about 2000 fps. A significant sidelight that came out of the calculations was that as the arrangement began to fail as an impactor, it became effective as a diffusive collector. The Brownian movement of the near molecules was sufficiently high that the particles were collected by diffusing to the walls of the impactor while they were flowing through it. The consequence was that, although on an impactor basis, one would predict that the collection efficiency would vanish as the particle diameter approached zero, the over-all performance remained satisfactory down to molecular sizes.

The second principle that emerged was that both processes became less effective with higher densities; in fact, we were forced to go to impaction on a screen for 175 kilofeet to attempt sampling of the debris of Orange shot.

When we became interested in the sampling program above 100 kilofeet, these principles came to mind and we wondered if they might be exploited in the regime above that altitude. The calculations that I am going to discuss are an effort to explore these possibilities. It should be emphasized that these calculations are preliminary and are intended to be a feasibility check and a guide for experiment initiation. Furthermore, there is a crucial assumption in my calculations which must be borne out by experiment before one could confidently attempt to use it. You will remember that Dr. Martell and Dr. Davis have spoken of the sedimentary behavior of heavy molecules. I am assuming in my diffusive collection calculations that these molecules are a super-saturated vapor which will condense on any available surface. Mr. Rigali will mention how we hope to check out this postulate. For larger particles, we will probably impregnate our filter media with an adhesive on which the particles will be trapped.

If we are going to examine diffusion and impaction collection, we must first estimate the amount of flow through the filter media. I have chosen to represent this filter as a system of parallel capillary tubes. Rough calculations reveal that the filter must be extremely flimsy. We have found that to obtain reasonably high flow rates, the length of the capillary should be only about ten times its radius. I think it will be more apparent how flimsy this means our filter media should be when we discover how small the pores must be for effective collection.

The flow equations for capillaries and assemblies of capillaries are given in Figure 4. The top half of this illustration covers the flow equations for the free molecular region where the mean free path of an air molecule is large compared to the pore diameter. The first equation describes the mass flow through a single capillary. The second is our working equation which is derived by relating the dynamic pressure to the pressure drop across the filter and considering the flow through the assemblage of parallel capillaries. The flow is assumed to be incompressible. In this working equation, $\bar{v}$ is the average velocity normal to the "accordion" filter entrance, a is the capillary radius, A is the fraction of the filter surface which is open for flow, and F is the fold factor, i.e., the cosecant of the filter accordion half-angle. The term R is of the gas constant for one gram of air, T is the absolute temperature, k is the ratio of the capillary length to its radius, while V is the sampler velocity. It should be noticed that the free molecular flow region has a nice feature. The flow velocity to the system for a given sampler velocity is independent of altitude if we ignore the temperature dependence which is practically not very important. This means that if we can always have the mean free path of the air molecules large compared to the capillary diameter, the velocity programing problem for the impellers is considerably simplified. In general we find that $\bar{v}$, the normal flow into the filter area, is considerably less than the sampler velocity. This means that to secure isokinetic sampling we must provide for flow expansion from the sampler opening to filter region. In the lower portion of Figure 4 we have the equations which describe the Poiseuille flow through a capillary and an emsemble of capillaries. This expression is appropriate when the mean free path of an air molecule is small compared to the diameter of the capillary. Here again, the first equation describes the mass flow through a single capillary, while the second equation is our working equation derived in the same manner as we did for the free molecular flow region. The meaning of the symbols is the same, except we have two new terms: $\bar{p}$, which stands for the average pressure, and η, which designates

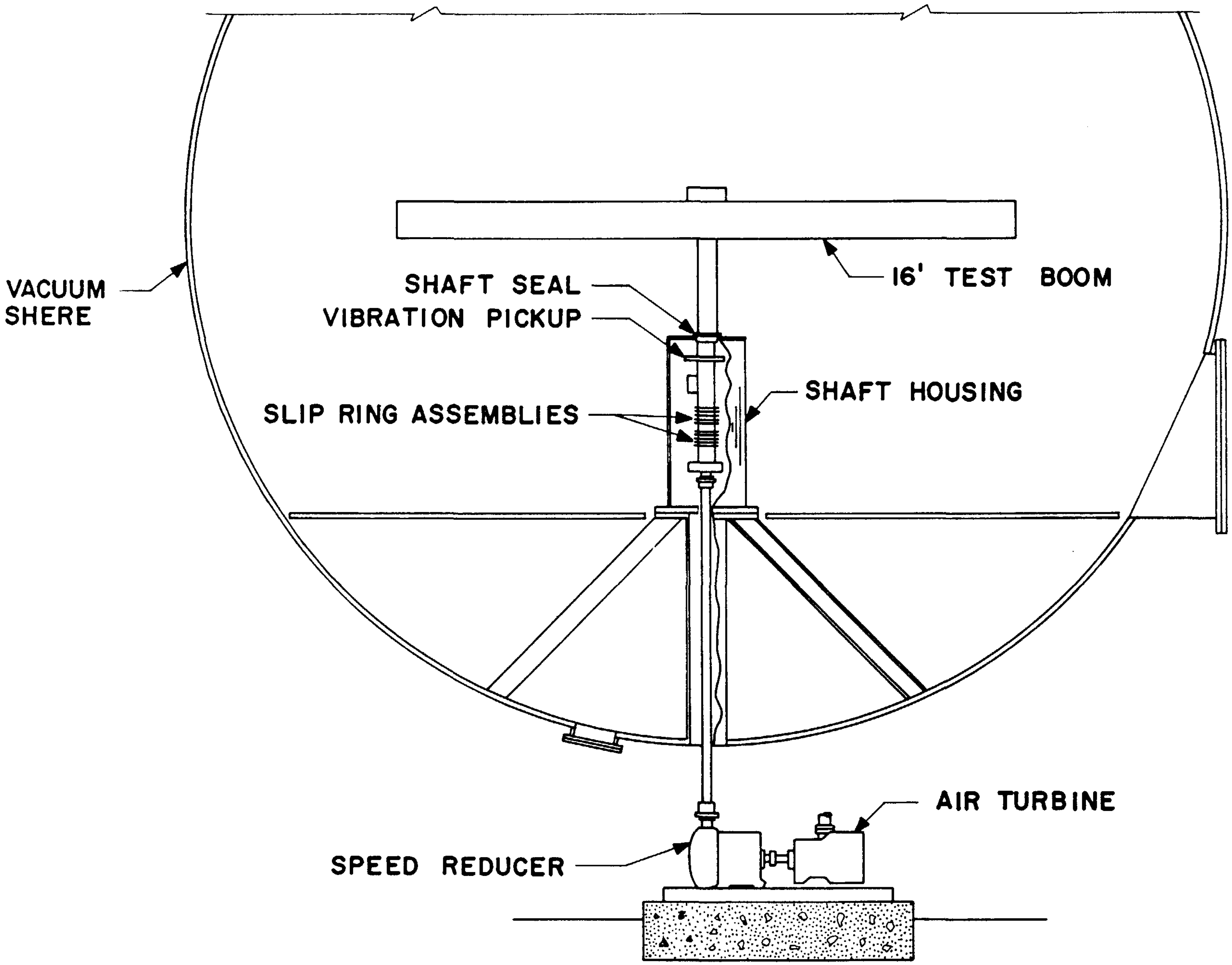


Fig. 2 -- Planned modifications to an existing 26-foot vacuum sphere

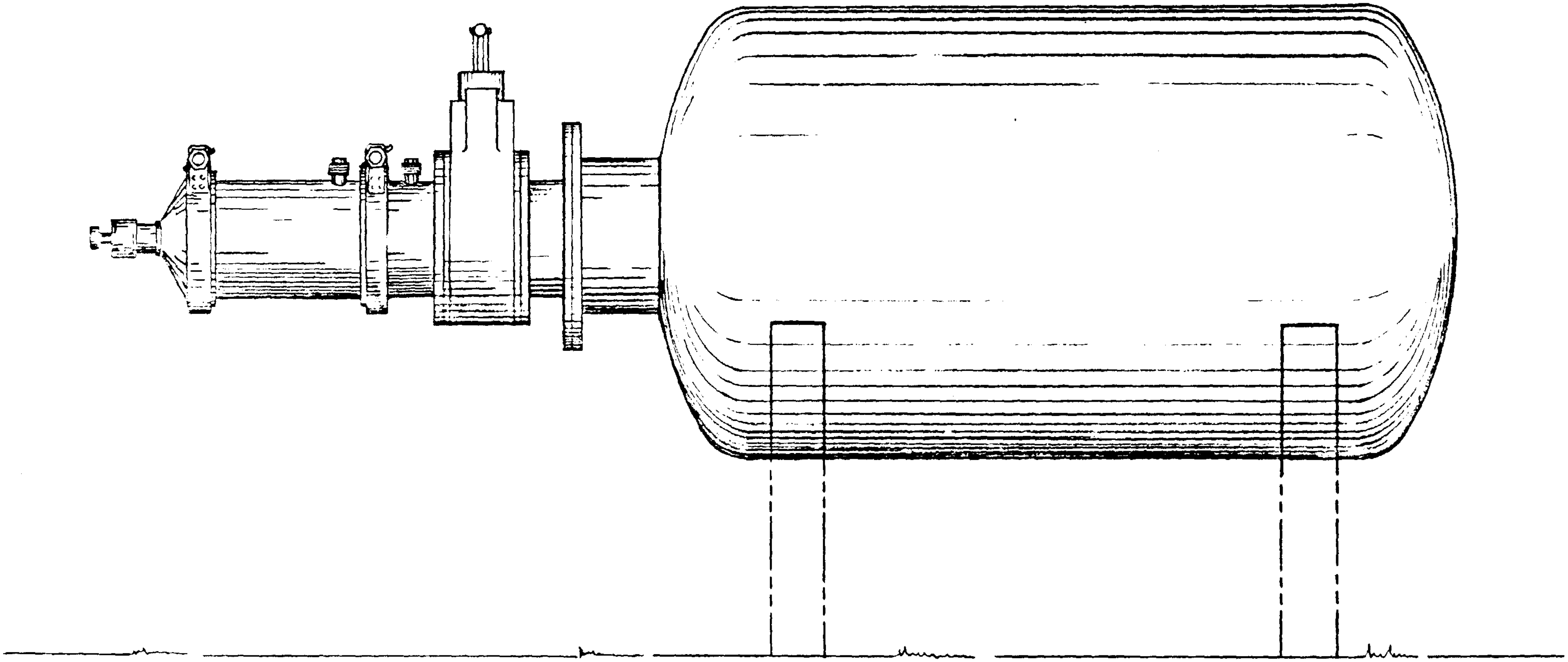


Fig. 3 -- Filter test rig for selection of optimum filter material

(D)
Theory and Proposed Experiments for Study of Diffusive Collection

D. Q. Matejka, J. R. Banister, D. J. Rigali
Sandia Laboratory

Introduction (D. Q. Matejka)

Previous studies have shown that debris from upper-atmosphere nuclear explosions should primarily be confined below 600,000 feet altitude. Rather extensive programs in effect for several years have been obtaining samples up to about 100,000 feet. The sampling program under discussion at this symposium would complement the current programs in determining the presence and time distribution of nuclear debris in the upper atmosphere.

In order to obtain a sample as efficiently as possible, the descent of the sampling device should be controlled. A parachute with a low weight-to-drag-area ratio is one of the more efficient drogue devices.

Parachute technology achieved through a large number of drop tests and rocket instrumentation recovery programs at Sandia shows that a parachute should serve as an efficient, yet economical, retardation device from about 200,000 down to 100,000 feet altitude. Above 200,000 feet, some unique, new method will undoubtedly have to be used to obtain samples. Based on these facts, it was decided first to develop a sampling device to collect samples in this lower regime. With the benefit of this experience, the design of a new high-regime sampler would then be attempted.

Experimental information is not available on the concentration of this debris in the upper atmosphere above 100,000 feet. Also, it is not known what minimum quantity of air must be sampled to obtain sufficient debris to enable an accurate analysis to be made. Necessarily, therefore, a large volume of air must be sampled on the first shots. Subsequent to this determination, the size of the sampler and possibly even its rotational speed can be decreased. If radiochemical analysis or one of the alternate methods are further resolved to the point that less particulate matter is required for analysis, then further reductions in sampler size or volume of air sampled can be made.

With the preceding requirements and limitations in mind, it should be possible to collect sufficient debris for radiochemical analysis in the following manner:

To ensure a large swept volume of air, two 8-foot collector blades rotating at 10 rps appear reasonable. These rotating blades, driven by small, solid-propellant thrust units at each tip should sample particulate matter from 10,000 standard cubic feet of air during about 5 minutes of fall, while traversing the 200,000 to 100,000-foot region, according to theoretical trajectories. The blades will be set at zero α, so no lift will be created and air flow will not be excluded from filters. These trajectories show that a large, light-weight parachute deployed at apogee, after a near-vertical rocket launch, should provide near ideal conditions for the deployment and initiation of rotation of the sampler mechanism. Figure 1 shows pictorially the sequence

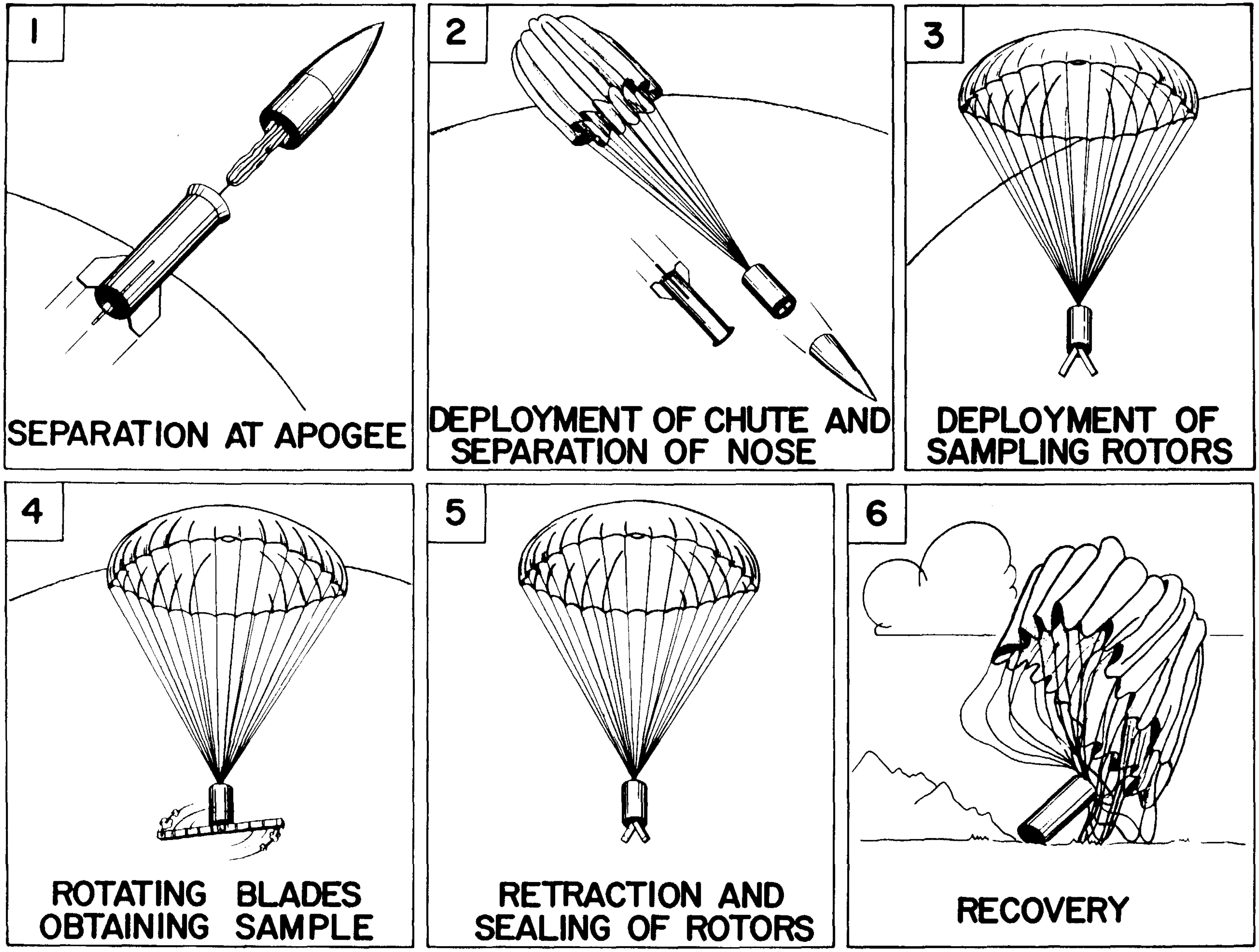


Fig. 1 -- Sequence of events performed to obtain a sample of particulate matter from the atmosphere

To obtain a sample that is representative, it is necessary to avoid impurities in the sample chamber. This is achieved by LOX cleaning each of the sub-assemblies in the sample area, LOX cleaning the final assembly, and then finally evacuating the sample chamber prior to flight. Both to avoid impurities and to avoid losing any of the sample, it is necessary to have positive seals on the entire sample area. Static and dynamic seals for this type of cryogenic application are commercially available.

A standard parachute recovery system is used to return the sample chamber part of the rocket to earth. A fitting can be attached to the sample chamber to withdraw the sample and place it in another container for transportation to the laboratory, or the liquid hydrogen that has been boiled off can be replaced and the heat exchanger section of the rocket itself used as the container.

As the hydrogen boils off, it must be vented in such a way that it does not cause excessive back pressure, which would raise the boiling temperature; that excessive liquid hydrogen is not lost; and that there is no back flow of air into the chamber, which might produce an explosive mixture of gases. This must be achieved at various combinations of acceleration, deceleration, rocket attitude, spin rate, liquid level, increasing and decreasing external pressure. If the missile is given a spin velocity, its rotary motion can be used as a means of separating the hydrogen vapor so it can be vented without carry-over or loss of liquid hydrogen in the vent system. During filling and acceleration venting is from the front of the hydrogen chamber through a vent line and out through a normally open valve. Burnout occurs before sampling; so during deceleration and sampling, venting is from the back of the chamber through a vent line which passes first to the front of the chamber and then back through the normally open valve and then out. This arrangement prevents loss of liquid hydrogen. These vents are located in the center of the chamber so that they will receive the hydrogen vapor which has been separated from the liquid by centrifugal force. After sampling and before descent of the rocket the normally open valve is closed. Venting is then through a relief valve in parallel with the normally open valve. This arrangement prevents back flow of air into the chamber on descent and prevents excessive loss of liquid hydrogen after landing.

To avoid excessive loss of hydrogen due to heat transfer through the walls of the rocket, it is necessary to provide insulation. The weight of a vacuum chamber made this method of insulating unfeasible. Freon blown polyurethene is the best solution. At liquid hydrogen temperature the freon will condense, leaving a near vacuum in the cells of the polyurethene; so good insulation is achieved with minimum weight.

Other areas in which particular problems exist are in the nose cone ejection prior to sampling, in the mechanism for opening and closing the door; fabrication of the heat exchanger, and providing for the contraction of the cooled parts. An over-all factor which must be considered in all aspects of the design is weight, which must be at an absolute minimum to achieve sampling at the high altitudes of interest.

In determining the performance capabilities of the sampling system, several factors must be considered. These are heat exchanger and sample chamber capacity; the maximum altitude achievable with a given weight, thrust, etc.; the point of the trajectory where the angle of attack is such that the inlet shock is lost; and the point where the vapor pressure of the noncondensibles in the sample equals the recovery pressure in the sample chamber. Any one of these factors can limit the capabilities of the system; and each must be optimized, considering its effect on the other factors, to achieve maximum performance of this system.

The particular system now being fabricated is designed for use with an Aerobee 100 rocket and for sampling from 40 to 85 kilometers.

Although the rocket was designed specifically to capture rhodium 102 particles in a particular altitude range, the system of sampling is not limited to these conditions. This sampling system is applicable to any situation where matter of interest is contained in atmospheric air. At this time it is the only system for capturing near molecular particles above balloon altitudes.

Reference

1. Karig, H.E., Gagnon, R.J., Bass, W.A., Analysis and Design Specifications for Cryogenically Cooled Rhodium 102 Collector, National Engineering Science Co. Report, Contract AF 19(604)-7341.

(C)

A Cryogenic Air-Sampling Rocket

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Aerolab Development Company
H. E. Karig
National Engineering Science Company

The collection of particulate matter from the upper atmosphere above the altitudes where balloons can be used can be accomplished by several methods, the most common of which is impaction. However, as the particles approach molecular dimensions, these methods become less efficient. The requirement for the system under discussion was collection of rhodium-102 particles at altitudes above 40 kilometers. The project is sponsored by the Cambridge Research Laboratories.

The most feasible collection method under these circumstances is one which captures all the air and particulate matter within a swept volume. No attempt to differentiate between molecules of air and rhodium is made during the sampling period, such identification being reserved for subsequent laboratory analyses. This necessarily implies that the volume of air captured must be reduced to reasonable dimensions and stored pending laboratory analyses of the sample. The design to be described is of a cryogenically cooled air sampling rocket. The rocket consists basically of a heat exchanger with liquid hydrogen as the coolant. Air entering the heat exchanger is cooled and either condensed or solidified depending upon whether the recovery pressure is greater or less than the triple point pressure. The most important theoretical and practical considerations in this sampling system will be presented. Details of the theoretical analysis are by Karig, Gagnon, and Bass.[1]

To assure that the sample is a representative one, the free stream capture area must be constant throughout the sampling regime. By providing a normal shock at the inlet, the capture area remains equal to the inlet area. If the shock becomes detached, air spillage from the inlet occurs, which results in a reduction of capture area. As air is spilled around the inlet, entrained particles are deflected such that there is a nonrepresentative concentration of particles in the captured sample.

The shock may become detached through perturbation of either the external or internal flow. For example, at supersonic speeds the bow shock wave will precede the body of the payload. The inlet must extend sufficiently forward of the main vehicle to avoid interaction between the standing bow shock and the inlet flow.

Internally, shock detachment may occur if the back pressure becomes too high. There are two possible mechanisms by which the maximum allowable back pressure can be exceeded. Viscous forces may cause the flow to attain sonic velocities somewhere downstream of the normal shock. Therefore, all flow passages, including the heat exchanger tubes, must be designed to minimize the effects of viscous forces. In order to

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*Speaker presenting the paper.

minimize frictional losses, the flow must be diffused as rapidly as possible. However, a practical limit is reached when the angle of divergence causes the boundary layer to separate from the wall. Secondly, the back pressure will rise if the rate of condensation is less than the inlet flow rate.

Sufficient heat transfer surface must be provided to condense all of the air entering the inlet, at a vapor pressure lower than the recovered total pressure at the diffuser exit.

The internal volume of the sample chamber must be sufficiently large to prevent excessive pressure buildup after air collection, assuming the sample is heated before it is recovered. This volume must also be adequate to prevent the vapor pressure of the noncondensible gases present in trace quantities from rising to a value equal to the recovery pressure in the sample chamber.

The design of the heat exchanger to meet the requirements for cooling, condensing, and freezing the air sample is influenced by two basic criteria. The first is the maximum rate at which heat can be transferred from the air to the hydrogen, which occurs at the maximum mass rate of air flow into the heat exchanger. The second is the total heat which must be absorbed during the entire air sampling period. Analysis of the heat exchanger indicated that the extended surfaces on the air side were required. A "bottle brush" type of surface, whereby many spines of small diameter wire were arranged in a spiral cross flow with each end contacting the inner wall of the heat exchanger tubes, was selected. This greatly extended the surface exposed to the air flow with only a small increase in weight per unit length. However, two factors could not be readily determined by analysis. First, the reduction in spine surface effectiveness due to partial surface contact between the ends of the spine and the inner wall of the tube. Second, the increase in pressure drop due to cross flow over the spines. These factors were determined experimentally.

The heat exchanger must also have sufficient surface and passage area to provide for uniform deposit of frozen air. The deposition of frost on the exchanger surface at a particular region increases its thermal resistance at that point. Subsequent frost deposition will migrate to a region having less frost accumulation. This is a stable process resulting in a uniform deposition of frost.

The liquid hydrogen used for condensing and freezing the air sample will not normally condense such gases in the atmosphere as hydrogen or helium. These gases, together with some neon, which has a boiling point only slightly above that of hydrogen, must be considered at the higher altitude limits of the collection range, since no further air can enter if the vapor pressure within the heat exchanger is equal to the recovery pressure. The altitude at which this becomes a limitation can be analyzed for any particular set of conditions. There will probably be considerable adsorption and consequent entrapment of the relatively small proportion of these gases on both the metallic and air frost surfaces, which will reduce the vapor pressure buildup that is calculated.

The cooling and freezing of the air sample as it enters the heat exchanger depends upon the satisfactory transfer of the heat into the liquid hydrogen heat sink. For minimum surface requirements all liquid hydrogen surfaces of the tubes should be continuously wetted. By using a flooded type heat exchanger, the maximum rate of heat transfer occurs at the initiation of the sampling period when the maximum amount of liquid hydrogen is available and, therefore, maximum wetted surface available.

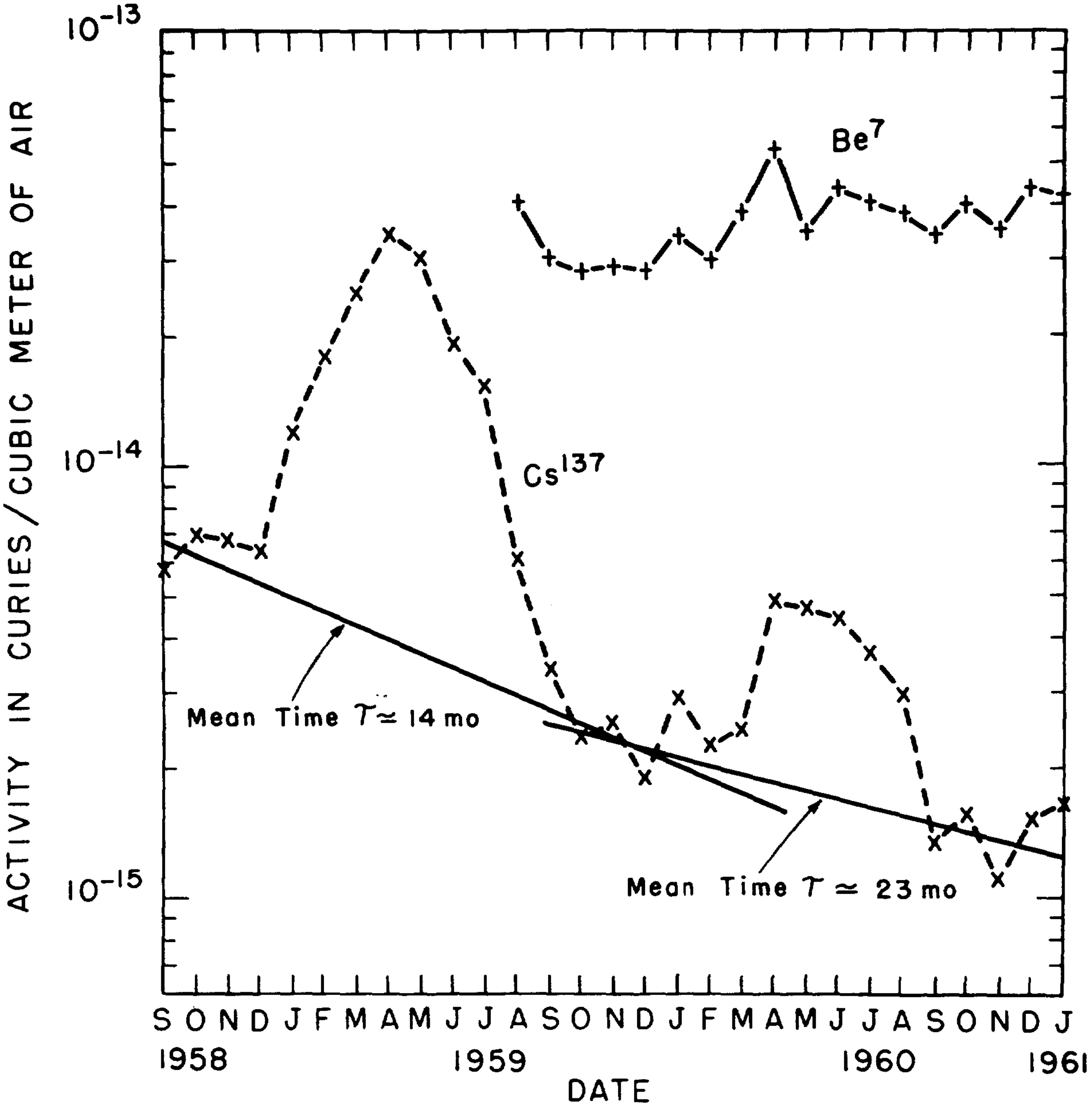

Fig. 10 -- Concentration of Be^{7} and Cs^{137} in surface air at Argonne National Laboratory

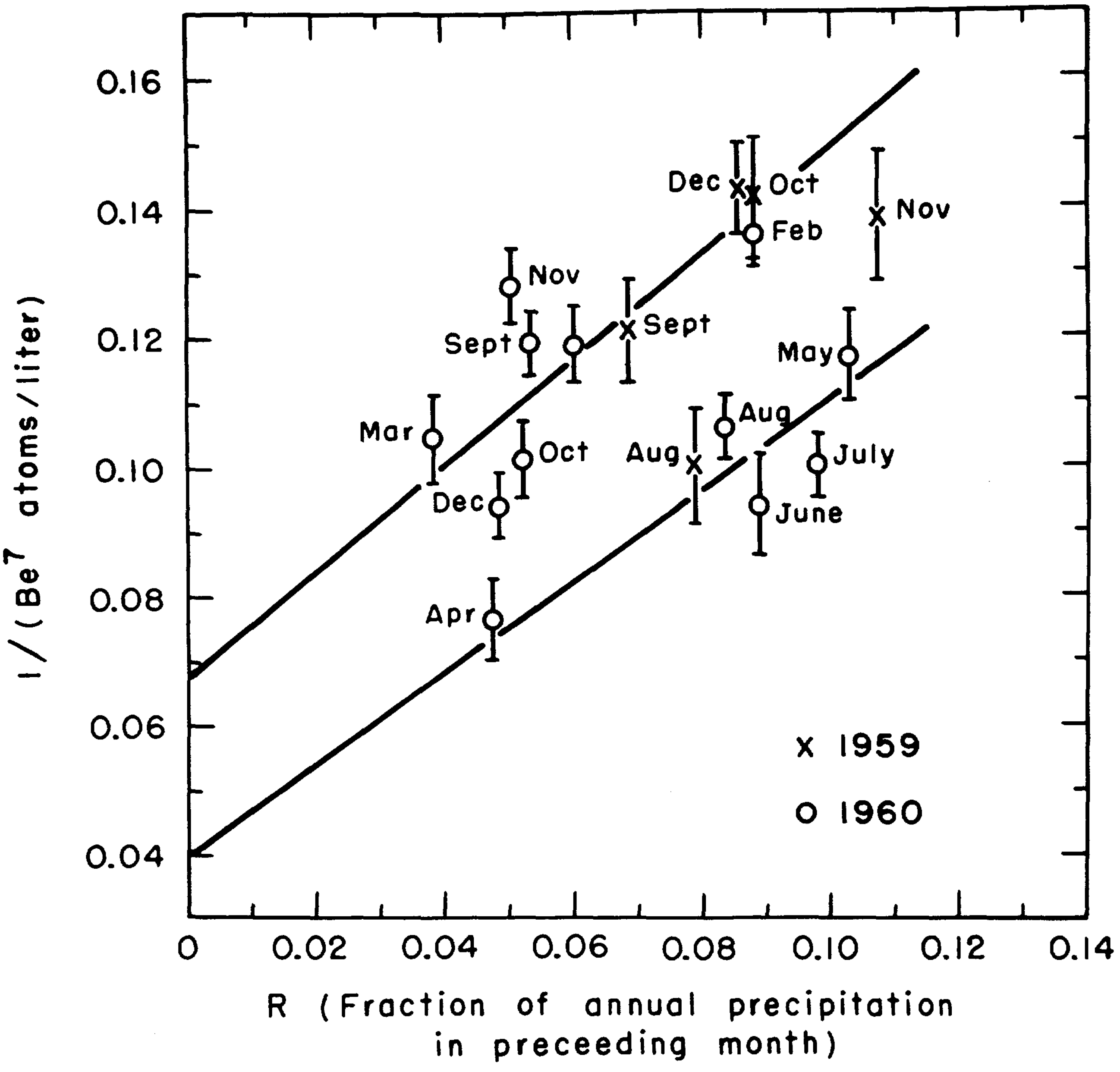

Fig. 11 -- Be^{7} concentration in air as a function of precipitation

descends into the lower atmosphere. It should also be mentioned that roughly 50 percent of the Cs^{137} found in surface air during the fall of 1960 was pre-Hardtack in origin as deduced from activity ratios. Hence it would appear that the initial estimates of stratospheric residence time of 5 to 10 years based upon measurements of debris from Castle may not have been wide of the mark.

The time dependence of stratospheric residence time coupled with the non-uniformity in the rate of removal of stratospheric debris adds greatly to the complexity of fallout mechanics; however, the proper consideration of these factors will not only improve the accuracy with which ground contamination from present stratospheric debris may be predicted, but will also further our understanding of the upper atmosphere.

(A classified appendix to this paper appears in Part II of the Proceedings, SCR-421.)

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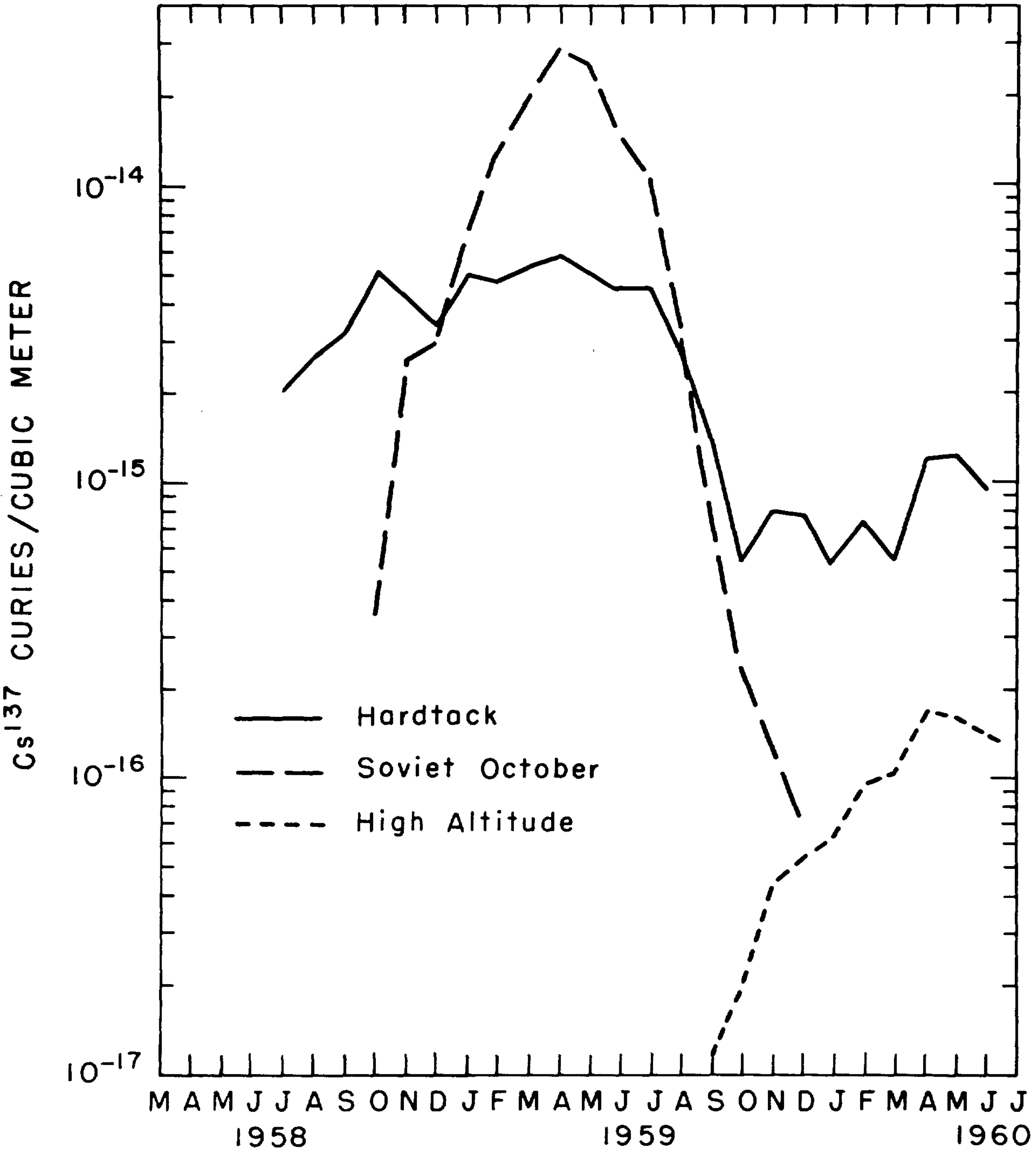

Fig. 9 -- Partition of Cs^{137} in surface air into Hardtack, Soviet October, and high altitude components

evidenced by its appearance in 1960, 18 months after the cessation of large scale nuclear tests. Such maxima, in the absence of recent nuclear tests, are attributed to the disruption of the northern hemisphere tropopause (in the case in point) in middle latitudes during the spring months, resulting in an influx of high altitude debris, much of it of polar origin, into the lower atmosphere. In addition, the mass transfer of stratospheric debris into the lower atmosphere occurs as a result of the rising tropopause during spring and early summer in middle and polar latitudes. An attempt to study the magnitude of this phenomenon has been made using naturally occuring Be^7 in surface air. The concentration of Be^7 in surface air for the period August 1959 through January 1961 is shown in Figure 10. The level of Be^7 goes through a maximum during the spring months, and indicates a higher value during the summer months than is seen during fall and winter. The concentration of Cs^{137} is also shown for comparison. Since Be^7 is being produced at a constant rate in the atmosphere (dependent upon the latitude and altitude) the air concentration of this nuclide should be a measure of production rate. This is not strictly true at low altitude, however, because removal by precipitation as well as by dry deposition prevents the establishment of true equilibrium between production and radioactive decay. Hence it is necessary to relate the observed concentration of Be^7 to precipitation. A method of doing this is shown in Figure 11 where the reciprocal of Be^7 in atoms per liter is plotted as a function of the fraction of the annual rainfall occurring in the month previous to sampling.[6] Using the expression $Q = \Lambda N$, where Q is the rate of Be^7 production in atoms/liter/day, Λ is the rate of Be^7 removal per day and is the sum of the radioactive decay constant and the precipitation removal factor, and N is the concentration of Be^7 in atoms/liter. Writing this expression as $1/N = \Lambda/Q$, at zero precipitation it becomes simply $1/N = \lambda/Q$, and thus the $R = 0$ intercept in Figure 11 gives a measure of Q. Note that the data points in Figure 11 appear to group into two distinct sets, implying two distinct production rates, the higher one occurring during the summer months. The increase in production rate in the summer months is in accord with that expected using Lal's figures for Be^7 production for 55°N geomagnetic latitude assuming that the winter tropopause height is 35,000 feet and rises to 45-50,000 feet during the summer. Concurrently it is to be supposed that a portion of the Cs^{137}, as well as other fission activity, seen in surface air following the spring maximum, has been transferred into the lower atmosphere as a result of the rising tropopause.

Another feature of fission product activity in surface air, in addition to the spring maximum, is the occurrence of an annual minimum during the fall and winter months as shown in Figure 10. It is presumed that activity seen during this period is a measure of the leakage of debris through the tropopause and represents a steady state condition as opposed to the period of tropopause disruption and mass transfer. If this be so, then comparison of the concentration of Cs^{137} in surface air between successive minima should provide a measure of stratospheric residence time. The mean stratospheric residence time found by comparing the Cs^{137} levels in the fall 1958 with those in the fall 1959 is 14 months; a mean residence time of 23 months is found between fall 1959 and fall 1960. It is interesting to note that roughly one-half of the annual fallout deposition (as derived from the product of mean monthly air concentration and monthly precipitation) comes about as a consequence of the spring break and the rising tropopause. Hence were it not for these latter two mechanisms, the mean stratospheric residence time would be twice as long. The lengthening of the residence time observed between 1958-59 and 1959-60 is in large part due to the almost total removal of Soviet October debris from the stratosphere by the end of 1959 as indicated in Figure 9. Furthermore, the mean residence time of W^{181} labeled Hardtack debris increased from 8 to 10 months in 1958-59 to in excess of 15 months in 1959-60. Such an increase in residence time is to be expected as the lower altitude, and thus more readily removable, debris sensitive that they are now detecting traveling waves in the ionosphere, and there is some question whether we saw a meteor shower last summer. I haven't heard meteor showers mentioned in connection with sampling, and whether the fission products played out of them. This would be investigated, I presume. I didn't hear the Argus shots and their fission diffusion discussed; if they have been traced, at that altitude it is unique. For the very distant shots, my own personal feeling is that there will tend to be a charged particle input at the auroral regions, but, as Welch pointed out, there is plenty of research there. This would influence the network locations. We would probably count on the aid of fission products to beat the background. I don't know how fast you can "R and D" chemical samples. You have to optimize to take advantage of getting samples as young as possible, but you can't rush yourself so much that you defeat your purpose. Again, we certainly would like to have all the details we can get into cryogenic collecting, so that it would be possible to do much analytical work on the shot.

Floor:

We can do a lot of research and originate ideas as to what we think might happen if somebody ever did fire one of these missiled things at a point 1000 miles away, but is there ever any discussion about jointly or unilaterally shooting a couple of devices to check our research?

Dr. Tucker:

Yes, they are hoping to do it underground. Everybody would like to do it at the various levels and altitudes; it is a question of getting political permission. It certainly implies that a high-low yield can be fired, or several of them, to really cinch these arguments. If the underground test program goes through, I should think this would make the political climate more agreeable to a few more Argus-type yields.

Floor:

Is nuclear testing at these ranges practical? You would be interested in information on a test program that is still out of range of detection.

Dr. Tucker:

It is easy to telemeter back. The Senate investigation of these techniques is already very much involved in electromagnetic pulses which look like lightning. If you have some indication from some other technique that a shot went off, then you can look over your EM things and you can find indication of it. Another thing, if you know the circulation well, you can get the fission, fix the date of firing, and, knowing circulation, you can establish where.

Dr. Shreve:

I don't think background can be overemphasized because, in any of these systems, the false alarm rate that you can tolerate is essentially zero, or perhaps one in 10 or 20 years.

Dr. Tucker:

The false alarms they talk about for Vela systems are one in 100 years.

VII. SUMMARY STATEMENTS OF INTEREST BY REPRESENTED AGENCIES

Round robin discussion: J. D. Shreve, moderator

Dr. Shreve:

I put four things on the board here that might serve as a guide in making comments on your agency interest. Is an upper-atmosphere sampler an attractive and appropriate research tool? If so, give specifics and also additional measurements needed. Then, time of need and schedule, and, finally, has the meeting been adequate for your interest? You can go beyond this, but it is a list of some check points that might be worthwhile covering.

Col. Barnes:

I am speaking strictly for both SNAPS. I feel definitely that we have to have upper air sampling if we are going to provide the nation with data as to hazards or probable hazards of SNAP devices. I guess this is the first time that we have seriously considered what might occur as a result of putting fission products in the atmosphere at high levels. In connection with weapon testing, we discovered after the fact that we had fission products coming back. This time we are going to test (SNAP reactors), we hope, before we put any large quantity of fission products in the atmosphere. As I mentioned the other day, we are planning to put radioactive material, as in the case of isotope power packages, into orbit around the earth. At the moment we have no way of re-entering these things in a specific location. They will re-enter at random and will burn up probably between 300, 000 and 100, 000 feet. Upper air sampling as has been defined here is going to be vital to us. At the present time we can only say that we think this material is going to burn up. We say that on the basis of our analysis of the re-entry of missiles, nose-cone data, and things of this nature. Precisely what will happen on our specific devices, our specific materials, is unknown. I think that the time schedule which I mentioned yesterday is most important, namely, that this year we expect to fly at least one or two isotope power packages containing plutonium 238. When they will re-enter is completely unknown because this will be a function of the orbit which we achieve. We do expect them to last five years if we are successful, preferably an even longer time. Plutonium 238 is rare in the atmosphere, and maybe you people will want to look for it as you sample the atmosphere to give us the background so that when our materials do come back you will be able to see if there is an increase in plutonium 238. In our studies we are interested in riding piggy-back on Atlas missiles and certain other missiles. This technique has not been discussed at the meeting here in any detail, and I can't discuss it in detail because I am not that familiar with it. The only thing I do know is that we have pods of 100-pound and 500-pound capacity which will ride on Atlas missiles. Instrumentation of various kinds can be put in these pods. This might be a fine way of sampling some upper atmosphere. Priority on these pods is quite high, but occasionally, if one has a piece of equipment ready and there is space, you can get the space available. In that ballistic trajectory, burnup will occur during a lateral distance of about 50 miles and a vertical distance of between 300, 000 and 100, 000 feet. We know precisely how much material we will have injected; we have a fair estimate of what is going to come in at the end. If we could sample the material between

100,000 and 300,000 feet, perhaps with time we could establish something of the spread of actual metal materials. Whether this is feasible or not I don't know. I am sure it would take a lot of rocket money to get these data. I feel that the meeting has been more than adequate for me from a technical viewpoint. I am a radiobiologist, and I have had difficulty understanding some of the geophysical terms that have been discussed. Many of us, particularly in the (Military) service, are forced into jobs in which we have to consider sciences and disciplines other than our specialties. It's been a real pleasure for me to be here. I hope we will have more meetings of this kind.

Lt. McCourt:

Our office is handling some of these pod tests for the AEC and has all the information on them. We have a pretty good schedule on the availability of pods, so if you come up with an experiment you think good, you could check with us. To re-emphasize the interest in this particular area of re-entry, we are planning now, along with the AEC on the SNAP re-entry program, to work out flight tests to determine the re-entry of this unit. Any method of sampling that might be useful in this type of work would certainly be of interest to us.

Maj. Decker:

The Space Nuclear Propulsion Office is a development office with the job of getting the nuclear rocket flying as soon as possible. We recognize that we have to worry about hazards to man, but, as I said yesterday, if we can see a possible hazards problem we will delve into it only far enough to find out whether there is a hazard. If there is obviously no hazard we quit worrying about it and don't try to refine the knowledge much further. I seems to me now that there is at least the possibility that in addition to hazards we might contaminate the atmosphere or confuse other experiments. We are in no position to evaluate whether this is true or not true. We are willing to work with anyone who thinks that conceivably one of our flights would give him trouble. If you need any more information about what is planned for Rover and what the release rate might be, we will be happy to talk to you. There is some flexibility in the hazards business in terms of where one might try to release the activity--in the atmosphere or far out in space. If this makes a difference to anyone, and it might, particularly in an area where we might dump 10 or 15 curies of some particular isotope, I think this would make some difference in the way we went about the hazards problem. I simply invite you to see us if you feel that there is any possibility of conflict between Rover and your experiments.

Col. Barnes:

Jim, I had one other small comment to make. This is extremely informal also because I can't give explicit details, but there will be strictly informal discussions this month in Italy with the Russians relative to whether it is feasible and proper to use reactors in space. I don't know how far along the Russians are with this type of device. The discussions are under the sponsorship of the National Academy of Sciences.

Dr. King:

As I mentioned earlier, we hope to try to determine what sort of particulate material comes out of a rocket motor in its normal and abnormal operation. I think the importance, possibly, in this meeting is in establishing the importance of such questions as: where does one dispose of this material? does it interfere with other interests? at what altitude would one release this material? Another item that has been brought out is the importance of coordinating, not only on a U. S. basis but on an international basis if possible, the introduction of isotopes so that we don't run out of possible tracer materials for basic geophysical experiments.

Dr. Shreve:

I personally feel that this idea of coordinating injections is going to become more and more important. Otherwise, the few first-jumpers will be served at the cost of putting everybody else out of business.

Dr. Fleming:

LRL is interested in these things more from a general scientific nature, I should say, with the possibility of one exception. Our Plowshare program may eventually get involved in something like this. We have thought about outer space shots for purely fundamental research: such things as measuring neutron scattering and the like. I can't say anything about the schedule on this; it depends a lot on Khrushchev, I suppose. As far as this meeting is concerned, I think it has been one of the most interesting I have ever attended. I have learned a great deal here, and, in particular, I think we all owe a vote of thanks to Jim Shreve for organizing it so well.

Dr. Shreve:

Thank you. We have tried to create an informal atmosphere, and you took it from there. I think the real success of the meeting is obviously a function of the participants. I would like now in reverse to express my thanks to you.

Cmdr. Elam:

I am from the Atmospheric Physics Branch of ONR. I came here primarily to see what was likely to be added to the knowledge of the region up to 100 kilometers by additional probes that might be made to meet the requirements that have been set forth here. We are more interested in scientific knowledge of the region than in sampling radioactive material. We intend to make geophysical sampling of this region and we are vitally interested in greater sampling of the region.

Dr. Schwob:

I have a wild idea that I would like to try out. I happened to have started in chemistry before radioactivity became popular, so I have some old fashioned ideas. One that occurred to me is the idea of using chemical rather than physical means for collecting samples. In qualitative analysis you had to separate materials in groups and then into finer subdivisions and then identify them. In radiochemistry we use a lot of these principles, but we don't use the chemistry for the actual identification or the actual detection of new material; we use radioactivity because the sensitivity is greater. I am proposing to use somewhat the same scheme during the collection, not for detection but for collection and for discriminating between items. Perhaps some day a shorter life material may be amenable to such a method. In fact, you may have successive stages. I can visualize rather vaguely, but somewhat concretely, a few simple chemical steps that should be possible in the sampling device which would help to fix such things as single molecules that might be difficult to collect otherwise. They might be converted to compounds which would spread to a certain surface. I'd like you people to think about this and tell me I am a fool or maybe get something out of it. In answer to your specific questions, my particular outfit, which is NRDL in San Francisco, is interested just now mainly in

SNAP. I think Charlie (Barnes) and his boys are doing a fine job in looking after our interests here. I would like to put in an advertisement if I may. We spoke of the sanctity of high-altitude samples and the desirability of putting in the best things to work, of getting results, and interpreting data from these samples. I'd like to say that we have a group that Col. Russell is very familiar with that would be very happy to cooperate in planning and, perhaps, in execution of some work in high altitude sampling.

Maj. Stebbins:

Since a lot of well known people turned down the opportunity to make this kind of remark, maybe I am stepping off into an area that I shouldn't get into, but I think that perhaps a few comments from DASA are appropriate. I must add my thanks to Jim Shreve because he has brought together all the people I wanted to see last year and has made it very easy for me to find out some things. In the past, when weapons testing was going on at a great rate, DASA used to be a good source of money. In recent years, with no tests, it has been harder to convince people that we still need to find out about weapon effects. People say: "You are not firing any weapons, so how can you find out about any effects?" There is, however, a considerable bit of research going with less funding than previously, at least in some areas, and fallout is one of these. In the past, there has been a sort of schizophrenic viewpoint taken by the DOD on fallout. When the word "fallout" was first mentioned, it was usually thought of in the context of local fallout. We have had a big program trying to determine the parameters necessary to predict the kind of fallout we would get from weapons in the immediate vicinity of the bursts. Many people in the DOD said that worldwide fallout was not really a problem. The high-altitude sampling program was probably one of the biggest projects in world-wide fallout that the DOD or DASA was engaged in. We spent a lot of money, but most of this money was for flying airplanes, not for the actual scientific reasarch. Of course, you have to consider the cost of the collecting agent and the people who are going to operate it as part of the program. We terminated the HASP program because we thought we knew all there was to know now about world-wide fallout. This was rather premature. The present way that DASA gets money to do research in weapons effects is through the WEB (Weapons Effects Board). The Army, Navy, and Air Force are all represented on this board. It decides how to split up what little money we have for this area. The ideas that have been presented at this meeting might be hard to sell to the WEB, so it may be that DASA will not be able to participate too actively in the program. I hope that this organization is not that inflexible. I think that we here have felt that this has been an important meeting. In the past we didn't have the means at hand to collect the samples, but now we are getting to the point where we will have the capability, and the material is up there waiting for us to look at. How can we galvanize our actions and go up and look at it? We have, represented here, people from the proper agencies who can implement this program. I hope that DASA will be able to contribute, too. The meeting has been adequate for my purposes. Like the child who goes into the candy store and is given one jelly bean, there needs to be a lot more done, but this has been a remarkably well-done first step. Certainly an upper-air sampler is an attractive and appropriate research tool. I am not in a position to give specifics, and certainly a lot of people here have given specifics much better than I can. As far as DASA is concerned, and as far as all of us are concerned, the schedules depend on when we are able to break the money loose. We are going to have to get the necessary research programs going, and we are going to have to get the hardware up there and start looking. I might make one plea: that we, as scientists, try to defend ourselves with knowledge against the politicians and the people who make decisions in the heat of anger. Certainly, when we start putting things like SNAPS and other sources of radioactivity up into these areas--and there certainly seems to be a tremendous effort directed toward putting these things into the sky--

there is going to be a political impact upon our work. If we meet ignorance with ignorance, I think that we ourselves will feel that we haven't done our job. We should meet some of these arguments that will be brought up in the next few years with facts, and the only way we can get these is to go up and get them ourselves.

Dr. Shreve:

Thanks very much, Major Stebbins. As a hobby, I do a little choir directing, and one of the things I learned long ago was that, when you get a group up to render a musical number, the first thing that starts is the mood, then the music, and then the choral part. It must end the same way. The music must stop, there must be a moment while the mood drifts off pleasantly. I think we have an appropriate parallel here. The mood was begun with Josh Holland outlining for us the motivations for the meeting and the outline of purposes. I think we are in a position now to have the mood gracefully drift away. So, with these poetic phrases, let me introduce Josh Holland.

Mr. Holland:

I'll drift away as gracefully as I can. I think if we decided to extend this meeting another day we probably could all find more things to say, but I don't feel that it is necessary to add anything at this point. I think this morning's program has really been excellent. I had apprehensions about a loose agenda without technical papers, but I think this series of summings up, questions, and so on, has been very valuable. I appreciate all of you coming to this meeting. We in the AEC, in cooperation with Sandia, decided to throw this party, and we are very thankful that you took time away from your very busy life to come and take part. Certainly the interest and excitement of the meeting and its usefulness are the result of all you people coming here and giving to it. I thank you very much.

Mr. Pope:

I have been waiting for somebody to come up with something other than things of government interest, something that the private industries could be interested in. I am reminded of "Mary, Mary, quite contrary, / How does your garden grow?/ I have it all in a soil bank, Sir, /I'm living on government dough."

Mr. Reed:

I'll answer our internal squabble right off the bat. I think in the whole sampling program the interest to meteorologists is one that is directly related to the over-all problem of weather forecasting, which I believe personally is a major factor in the whole GNP of the country. The easiest way I know of to become a billionaire is to learn how to forecast the weather. The whole country would pay that price if I could forecast it. There are a great many things in solar relationships, in high atmosphere relationships, which appear to correlate with the lower atmosphere of meteorology and weather forecasting that we cannot understand without a much better knowledge of the regime into which we want to go.

Mr. Holland:

Let me give another answer. We say that we have to have this information in order to go forward with the program. Another way of saying the same thing is that we have a responsibility to citizens of the United States and, to some extent, to the people of the whole world not to subject them to undue levels of radioactive contamination. Also, we have a responsibility to the scientists who may be conducting experiments on natural properties of the environment not to contaminate the environment in a way that ruins their experiments. I think we have a feeling of responsibility to people who are concerned with these problems. It is not the kind of thing that an individual citizen can do. The things that are going to be done will have to be done by the government agencies. I don't know whether this answers your question.

Mr. Pope:

I think it helps. This was not an attack on the way that we are going at the problem in any way; it is just too bad that we can't have more of the private interest in the country saying, "This is a wonderful thing. Our company would like to participate with its own money because it can see further benefit."

Dr. Shreve:

I think if we set this up for the benefit of all and phrase for them the kind of enthusiasm that exists, for what reasons this enthusiasm exists, it sets the stage for next steps which I hope will make maximum use of the industrial laboratories and the talent there is in them.

Lt. McCourt:

The programs which might add something to the atmosphere which is not there now should be coordinated among the various interested people. Is there any method or means of doing this? What would you consider doing to accomplish this?

Dr. Shreve:

I think the idea, so far as I am concerned, has been generated here. It is almost a golden rule of space that everything you do has the influence on somebody else. I hope that everyone will take back to his own working group this idea and, hopefully, very concrete proposals for establishing organizations that might undertake this cognizance of injections.

Dr. Otting:

There are already two groups. There is the Technical Working Group on Atmospheric Sciences under the Federal Research Council that could do this kind of thing, and I think they will, eventually. I don't know whether they would control it or would be an information exchange agency, but they could certainly serve in this way. There was also an ad hoc group that met once in the DOD, under Dr. Kaplan's direction. I think the Weather Bureau, NASA, and others were represented. They recommended that they set up a permanent arrangement with all these people. I don't know what will happen. There is a third group, the Academy of Sciences. I guess the only one that I know of that represents a government agency would be the Technical Committee under the Federal Research Council.

Dr. Shreve:

I think we would all agree that, whatever this controlling agency would ultimately be, certainly it must be composed of some very competent scientists, carefully chosen from the various disciplines involved so that nothing is treated lightly because of a simple prejudice for or against this kind of activity.

Dr. Otting:

I don't mean a controlling organization; it would be more a coordinating agency.

Mr. Holland:

I believe ultimately one of these existing inter-agency groups would come into the picture. But, rather than look at the organization of the government to find a place to plug in such a group and then hold this meeting over again to educate these people, I suggest that those of us who do represent a Washington office agency get together after this meeting to discuss these problems, make recommendations as to how we might keep in touch with each other, and establish some ground rules in this area. The Federal Radiation Council, the Federal Council for Science and Technology, and the National Science Foundation should be made cognizant of this problem. But those of us who know about it, I think, should be in touch with each other independent of the formal organization.

Dr. Shreve:

I don't think we can begin to propose concretely in this offhand manner. I am convinced that there are some powerful ideas here that need careful evaluation and careful attention, and I think it is a good note on which to declare our session adjourned with my most warm thanks to all of you for being with us.

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