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tentials of constituent 1 in the two phases is then

$$\mu'_1 - \mu''_1 = RT \ln \frac{a'_1}{a''_1} \qquad (4)$$

To obtain a measure of this difference of the chemical potentials, use is made of the experimental fact that when a certain pressure difference is imposed across the barrier there will be no net passage of constituent 1 from phase A to phase B. Experimentally, it is found that the pressure on phase B, P_B, must be greater than the pressure on phase A, P_A. Equilibrium is established with respect to constituent 1 in this manner, and the chemical potential, μ'_{1, P_A}, of constituent 1 in phase A at the pressure P_A is equal to the chemical potential, μ''_{1, P_B}, of constituent 1 in phase B at the pressure P_B.

We have now the relationships

$$\mu'_{1, P_A} = \mu^0_{1, P_A} + RT \ln a'_1 \qquad (5)$$

and

$$\mu''_{1, P_B} = \mu^0_{1, P_B} + RT \ln a''_1 \qquad (6)$$

But equilibrium obtains; hence,

$$\mu'_{1, P_A} = \mu''_{1, P_B} \qquad (7)$$

and

$$\mu^0_{1, P_A} + RT \ln a'_1 = $$
$$\mu^0_{1, P_B} + RT \ln a''_1 \qquad (8)$$

From Eqs. 7 and 8 we have

$$\mu^0_{1, P_B} - \mu^0_{1, P_A} = RT \ln \frac{a'_1}{a''_1} \qquad (9)$$

where μ^0_{1, P_B} and μ^0_{1, P_A} are determined by temperature and pressure. The system is considered to be isothermal, and we need be concerned only with the variations of μ with P. This variation is given by

$$\left(\frac{\partial \mu}{\partial P} \right)_{T, n} = \overline{V} \qquad (10)$$

where the subscripts T and n indicate that the temperature and composition remain constant, and where $\overline{V}$ is the partial molal volume of the constituent under consideration. For sufficiently small variations of P—that is, if P_A and P_B differ by a sufficiently small amount, and if the compressibility of the constituents may be neglected—we may write

$$\mu^0_{1, P_B} - \mu^0_{1, P_A} = \overline{V_1}(P_B - P_A) \qquad (11)$$

From Eq. 11 and Eqs. 4 and 9, there follows immediately

$$\mu'_1 - \mu''_1 = \overline{V_1}(P_B - P_A) = RT \ln \frac{a'_1}{a''_1} \qquad (12)$$

whence

$$P_B - P_A = \pi = \frac{RT}{\overline{V_1}} \ln \frac{a'_1}{a''_1} \qquad (13)$$

where π is the so-called osmotic pressure.

It is important to note that the osmotic pressure multiplied by the partial molal volume of the constituent under consideration is equal to the difference between chemical potentials that obtained when the pressures on the two phases were equal. Nowhere does "the pressure of solute molecules" enter.

The limiting law, or van't Hoff law, of osmotic pressure is derived from Eq. 13 by the use of simplifying assumptions. The activity, a, is the product of the mole fraction, N, and the mole fraction activity coefficient, f. We may then write for Eq. 13

$$\pi = \frac{RT}{\overline{V_1}} \ln \frac{N'_1 f'_1}{N''_1 f''_1} \qquad (14)$$

The convention that the activity of constituent 1 in phase A is unity is now applied. Equation 14 reduces to:

$$\pi = \frac{RT}{\overline{V_1}} \ln \frac{1}{N''_1 f''_1} = -\frac{RT}{\overline{V_1}} \ln N''_1 f''_1 \qquad (15)$$

If constituent 2 is present in sufficiently small concentrations, it may be assumed that $N''_1 f''_1$ approaches N''_1 in value. In the present simplified system, we have $N''_1 + N''_2 = 1$. Hence, $N''_1 f''_1$ may be replaced by $(1 - N''_2)$. We now have

$$\pi = -\frac{RT}{\overline{V_1}} \ln (1 - N''_2) \qquad (16)$$

The logarithmic portion is expressed as a series to give

$$\pi = -\frac{RT}{\overline{V_1}} \left[-N''_2 - \frac{1}{2}(N''_2)^2 - \frac{1}{3}(N''_2)^3 - \cdots \right] \qquad (17)$$

It is now assumed that all terms in the expansion of degree higher than 1 may be neglected. Thus

$$\pi = \frac{RT}{\overline{V_1}} \cdot N''_2 \qquad (18)$$

But

$$N''_2 = \frac{n''_2}{n''_1 + n''_2} \qquad (19)$$

where n denotes number of moles. But n''_2 is small compared with n''_1; therefore, we may write $N''_2 = n''_2/n''_1$. We then have

$$\pi = \frac{RT}{n''_1 \cdot \overline{V_1}} \cdot n_2'' \qquad (20)$$

But $n''_1 \overline{V_1}$ is simply the volume occupied by constituent 1 and may be written V_1. Then n''_2/V_1 is the molal concentration of constituent 2 and is denoted by C''_2. We have finally

$$\pi = C''_2 RT \qquad (21)$$

This is van't Hoff's limiting law of osmotic pressure. It is an approximation. It

does no more than indicate the approximate relationship between the concentration of constituent 2 and the pressure difference that must be imposed in the present system in order to establish equilibrium with respect to constituent 1.

Conclusions

The relationship is analogous in form to the simple gas law. However, this *analogy* does not require that the pressure difference π, experimentally determined, be the "bombardment pressure of the solute molecules against the barrier." Indeed, if this were the case, the pressure "exerted" by the solute molecules against the barrier would have to be a *negative* bombardment pressure in order to account for the passage at equal hydrostatic pressures of the solvent from the pure solvent phase to the solution phase. It may be pointed out that there is no justification or basis for the concept of *negative* pressure: pressure approaches zero as the molecular density approaches zero, but pressure does not and cannot assume negative values.

Use of the term *osmotic pressure* in the sense of the "pressure of the solute against a membrane permeable only to the solvent" cannot be justified either on theoretical or on experimental grounds (see, for example, 6, 8, 9).

On occasion, the convenient operational designations, "solvent" and "solute," have been used with the implications of fundamental differences in properties. It will be noted that these designations have been avoided in the derivation given here. In a system such as is considered here, "solvent" would designate constituent 1 and "solute" constituent 2. But the meaning of such designations would be simply that the barrier is permeable to constituent 1 and not permeable to constituent 2. The designations would be reversed if a barrier permeable to constituent 2 but not to constituent 1 were used. A system frequently considered in physiology is that formed by the plasma, the capillary walls, and the interstitial fluid. In a simplified form of the system, water would be the solvent, the plasma proteins the solute, and the capillary walls the barrier. In such a simplified system, the osmotic pressure of the plasma as determined in the laboratory provides a measure of the effect of the proteins on the properties of water.

The term *osmotic pressure* is in itself misleading. It is probable that this has contributed to the confusion surrounding the meaning of the term. As we have noted, osmotic pressure as experimentally determined is a pressure *difference* and not an absolute pressure. The experimental implications have been retained in, for example, the terms *freezing-point depres-*

sion and *boiling-point elevation*; there is no connotation of absolute measurements but only of relative measurements. The freezing point of a pure solvent is, in effect, that temperature at which a liquid phase and a solid phase can coexist without change of mass of the individual phases. The freezing point of a solution is that temperature at which a solid, pure-solvent phase can coexist with a liquid-solution phase without change of mass of the individual phases. The difference between these two temperatures is the freezing-point depression. In other words, the temperature of a *solution* and of the pure solvent must be decreased below the freezing point of the pure solvent in order to establish equilibrium with respect to the solvent between the solvent in the solution and solvent in the pure solid phase. Similar remarks apply to boiling point elevation. Osmotic pressure is then simply the pressure *increment* that must be imposed on a solution in order that pure solvent phase and solution phase, separated by an appropriate barrier permeable only to the solvent, can coexist without change of mass of the individual phases. In other words, osmotic pressure as a pressure *difference* is similar to the freezing-point *depression* and to the boiling-point *elevation*.

This should provide an adequate nega-

tive answer to the often asked question: "Does osmotic pressure exist as such in a solution?" A negative answer is also provided from the derivations we have given. It may be pointed out, in addition, that Gibbs referred to the "so-called osmotic pressure" and indicated clearly that the pressure calculated by the laws of Boyle, Charles, and Avogadro for the solute in the space occupied by the solution was *calculated* and not experimentally found (*10*). Pressure can be measured in a single phase, just as temperature can. But just as the determination of freezing point or boiling point requires the coexistence of *two* phases, so does the determination of osmotic pressure require the coexistence of two phases (*11*).

One does not subtract freezing-point depression from the actual temperature of a solution to calculate an "effective temperature." No more should one subtract osmotic pressure from the actual pressure of a solution to calculate an "effective pressure."

It is recognized, as Haldane pointed out a number of years ago (*12*), that the concept of osmotic pressure as something which exists in solutions even when no external pressure is applied is firmly entrenched. It is hoped, nonetheless, that this discussion may help to clarify the meaning of osmotic pressure and that the concept of osmotic pressure as the bom-

bardment pressure of solute molecul; against an impermeable barrier w: eventually be abandoned.

References and Notes

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11. The equilibrium condition under which o; motic pressure is determined is frequentl referred to as a membrane or partial equilib rium: equilibrium obtains with respect t the solvent but not necessarily with respec to the solutes. Similarly, freezing-point an; boiling point equilibria are partial equilibria equilibrium is established with respect to th solvent but not necessarily with respect to th solutes. The phase boundaries in these deter minations are analogous to the semipermeabl membranes used in determinations of osmoti pressure; it is assumed that the solutes canno enter the pure solvent phases.
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World-Wide Travel of

Atomic Debris

L. Machta, R. J. List, L. F. Hubert

For centuries meteorologists have thought of exploring large-scale atmospheric circulations by means of tracers. The literature describes how man has successfully tracked fluorescent particles to a distance of 100 miles (*1*), used radioactive tracers across the United States (*2*), and followed volcanic ash and forest fire smoke over distances of the order of 1000 miles (*3*). Only the dust from a major volcanic eruption, such as Krakatao, has been tracked on a truly global scale.

During two of the nuclear test periods in the Pacific Proving Grounds of the U.S. Atomic Energy Commission, suffi-

cient radioactive debris was thrown into the atmosphere to be deposited in both hemispheres. Measurements of the deposited radioactivity were obtained from exposed sheets of gummed film. The details of the network and the sampling and measurement techniques have been described by Eisenbud and Harley (*4*). It should be noted, however, that the deposition of particles on the adhesive surface depends either on the presence of precipitation or, in dry weather, on turbulence to assist the impaction of the particles on the horizontal surface of the paper. It is thus possible to have a cloud of radioactive particles pass two stations

simultaneously and have only the station with rain note the presence of the particles overhead. The gummed-film method of collection is recognized a; being as crude as it is simple.

The nuclear explosions are treated in this article, the Mike shot on 1 November 1952 and the Bravo shot on 1 March 1954. The shots were similar in that both are described as having had energy in the megaton range, both were detonated at or near the earth's surface on a coral island, and both had atomic clouds that penetrated into the stratosphere. To the meteorologist, the main difference of interest between the two events is the season.

Winds

The winds acting on the two atomic clouds at the time of detonation are illustrated in Fig. 1. The wind structure has been estimated, when necessary, from observations at nearby locations and times. On both days the tropopause was found at an altitude of about 55,000 feet, and it separated winds blowing from different directions. The easterly winds

above the tropopause increased in speed to the highest altitude of the available wind information for the Bravo shot, while for Mike the easterly winds decreased in speed and ultimately changed to westerly winds. The easterly winds in the trade-wind layer, the moist maritime air mass lying near the sea, extended up to about 20,000 feet during the detonation of the Mike device, while for the Bravo shot they were below 10,000 feet. Between the trade-wind layer and the tropopause, one normally finds westerly winds. During the Mike shot these westerlies were temporarily interrupted and became southerly winds, while for the Bravo shot they were toward a more normal bearing.

In Fig. 2. is found the approximate area covered during the early days by that part of the nuclear cloud from the Mike shot which was located below the tropopause. The shaded areas in Fig. 2 have been deduced from meteorological considerations alone, and, in many cases, are subject to considerable uncertainty. Shading was discontinued when the meteorological data no longer warranted any reasonable estimate of the path. The light winds and sparsity of upper-wind observations have made tracing the upper tropospheric portion of the Mike cloud particularly uncertain. For this reason, the time of passage across the North American mainland is unknown. Tracing was discontinued on 7 November. The tradewind portion of the nuclear cloud appears to have split south of Japan, the upper portion (near 20,000 feet) curving around a Pacific high cell and entering the United States about 9 November.

The estimated meteorological path of the Bravo cloud is shown in Fig. 3. The upper tropospheric portion of the nuclear cloud was traced to the Central American area by about 5 March, and an offshoot extending northward into the United States at about 20,000 feet was detected approximately 1 week later.

Differences between the paths of the Mike and Bravo clouds are evident from Figs. 2 and 3. In part, the differences are seasonal and in part due to the specific meteorology for the shot days. Thus, in November the mid-tropospheric westerly winds are not as strong as they are in March, and they are located farther north, on the average. Further, in November one finds an anticyclonic circulation not far from the Marshall Islands which is not typically present in March. The shallowness of the trade-wind layer during the Bravo shot is an example of a feature unusual for the region during any season.

There has been no attempt to track the stratospheric portions of the atomic cloud because of the sparsity of wind observations at these altitudes. Evidence from numerous isolated high-level winds, not necessarily obtained during the periods

of the two nuclear tests, suggests a path that would travel around the earth at about the same latitude as the point of origin. It is interesting to note that in no case was it imperative to rely on stratospheric transport of the nuclear debris to account for the earliest arrival at any point, for the transport of the nuclear cloud in the troposphere appeared to account for the first observations of radioactivity.

An attempt to determine the earliest arrival time at the ground at each point of observation has been undertaken. The results, which are shown in Figs. 2 and 3 as the number of days after the shot day, should in many cases be viewed with caution. First, in many of the stations in the Southern Hemisphere, the deposited activity was so low that it made the arrival date almost meaningless. Second, despite elaborate precautions, it is likely that some gummed films were contaminated during handling. Finally, as noted in the second paragraph the apparent arrival time of the cloud at many stations coincided with rainfall, suggesting that the nuclear cloud may have been overhead some time earlier but that precipitation was required to bring its activity to earth.

Fallout

It is noted that, in accordance with the meteorological estimates, the fallout over the United States progressed roughly from west to east during the Mike shot. Fallout from the Bravo event did not appear at the West Coast stations in the United States until 2 weeks after one of the cloud protuberances entered the central United States. Of perhaps greatest interest, although also of greatest doubt, are the comparatively early arrival times in the Southern Hemisphere. Thus, for example, a literal interpretation of the chart reveals that every station in the Southern Hemisphere showed an earlier arrival time than did the United States West Coast stations for the Bravo case. Also of interest are the comparatively late arrival times for the mid-Pacific stations west of the Hawaiian Islands during the Mike fallout. These stations were south of one branch of the nuclear cloud and north of the other.

The actual fallout at each station and an analysis of the data are shown on Figs. 4 and 5. The units are cumulative decayed beta activity for the first 35 days following each event and are approximately equivalent to millicuries per 100 square miles (the values have not been corrected for the efficiency of the gummed film.) Several features that differentiate the two maps should be noted. First, an average value for all United States and Canadian stations was obtained for the Mike shot, as opposed to values for individual

stations during the Bravo shot. Second, the isolines located between points on the West Coast of the United States and points in the Western Pacific Ocean are also based on fallout observations obtained from transport vessels for Bravo. Finally, as is evident, the network was expanded between the two events, primarily in an attempt to locate stations in rainy areas. In many cases, when the period of record is incomplete or the data are suspect, parentheses have been placed around the number. No attempt has been made to reconstruct the isolines for the fallout that occurred within the first 24 hours of the shot.

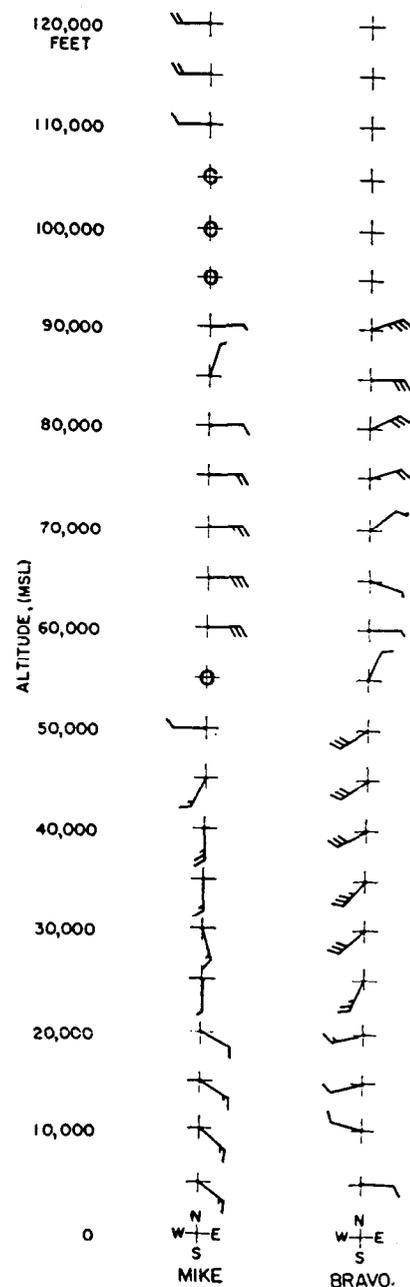The comparatively small values ob-

Fig. 1. Upper winds at shot time. Arrows blow with the winds, and barbs indicate wind speed; full barb, 10 knots; one-half barb, 5 knots.
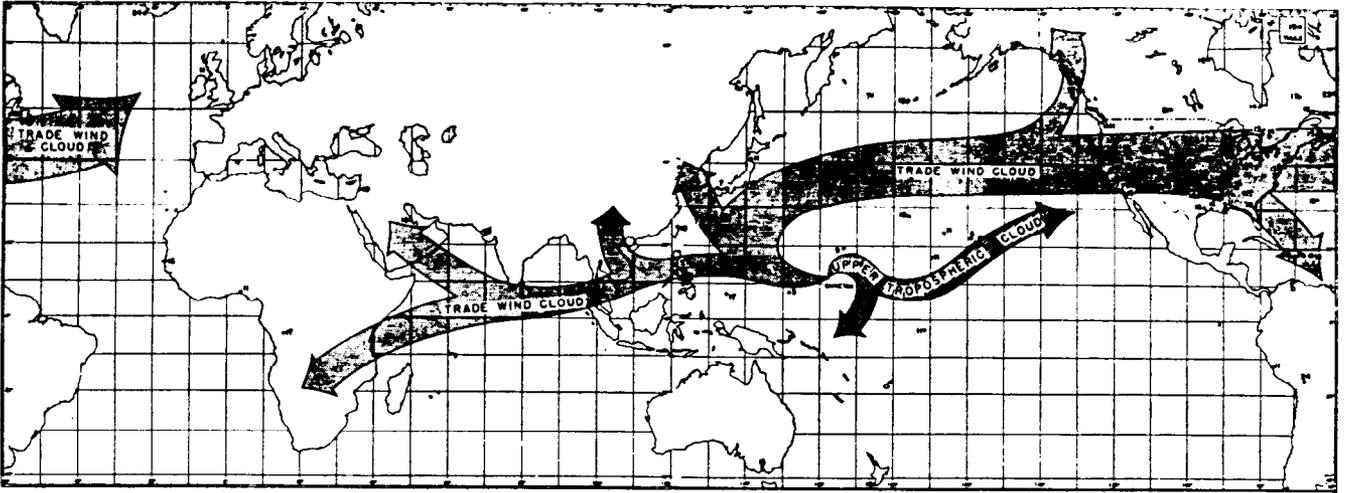
Fig. 2. Early history of the Mike cloud. The figures indicate the number of days between detonation and the first ground observation of fission products.
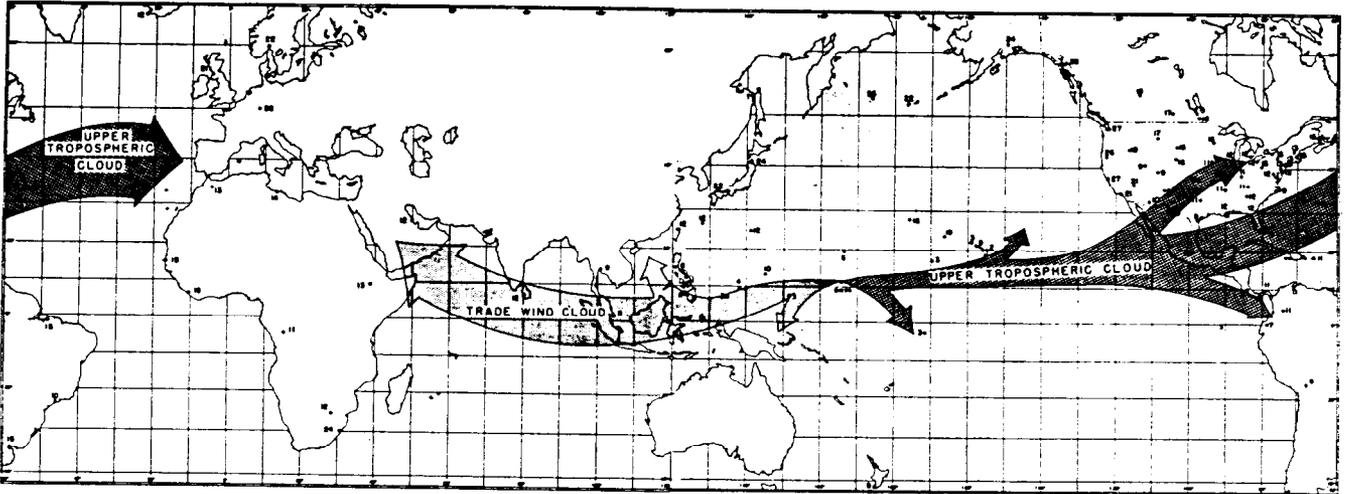

Fig. 3. Early history of the Bravo cloud. The figures indicate the number of days between detonation and the first ground observation of fission products.
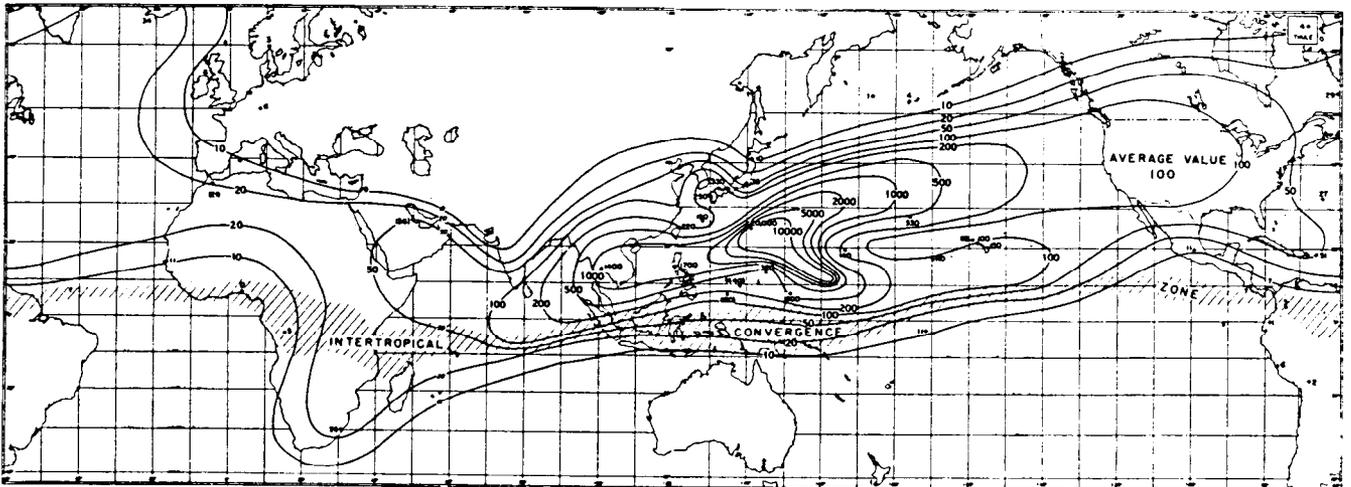

Fig. 4. Total radioactive fallout from the Mike cloud in the period from 2 to 35 days after detonation, in millicuries per 100 square miles. Hatching indicates the approximate November position of the Intertropical Convergence Zone, the belt of low pressure that tends to separate Northern and Southern Hemisphere air near the surface of the earth.
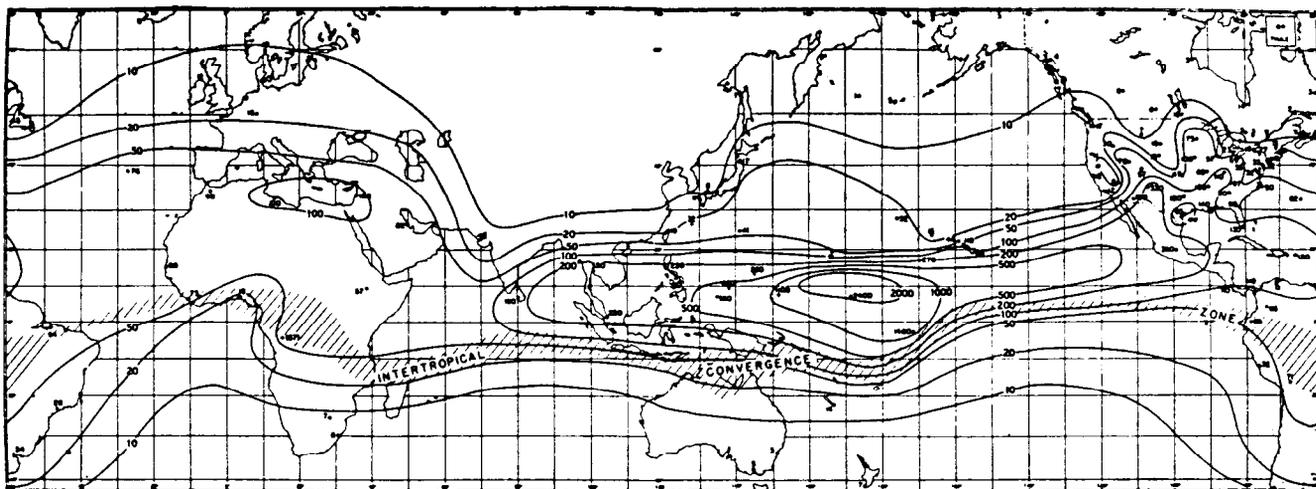
Fig. 5. Total radioactive fallout from the Bravo cloud in the period from 2 to 35 days after detonation, in millicuries per 100 square miles. Hatching indicates approximate March position of the Intertropical Convergence Zone, the belt of low pressure that tends to separate Northern and Southern Hemisphere air near the surface of the earth.

tained at the Southern Hemisphere stations especially during the Mike shot, are immediately evident from the fallout maps. The northern part of the Northern Hemisphere, however, received equally small depositions. The distribution of fallout for the Pacific stations appears to be consistent with the features of the meteorology described, although the branching of the cloud south of Japan in the Mike pattern is based only on scanty observational evidence.

It is apparent that radioactive debris produced by nuclear explosions does not possess all the desired attributes of a tracer for studying global circulations.

Information concerning the magnitude and distribution of the radioactivity that remains airborne after the initial fallout is not available. The debris, being particulate, is washed out of the atmosphere and cannot be strictly treated as a conservative property. Thus, for example, the depositions in the Southern Hemisphere may have been low because most of the debris was rained out as it passed southward through the Intertropical Convergence Zone. In addition, the most effective sampling program for the debris provides only the crudest measure of the fallout. Yet, despite these limitations, it appears that the meteorologist can ob-

tain useful information by operating such a network of gummed films during nuclear test periods. Although it is not proposed that special nuclear tests be undertaken for meteorological purposes, it seems reasonable to expect even greater value from future tests using an expanded network and having detonations at other locations and times.

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A. O. Weese, Ecologist

Asa Orrin Weese was born of Canadian parentage in Hutchinson, Minnesota, 7 November 1885, and died in Norman, Oklahoma, 20 November 1955. After graduating from the University of Minnesota in 1909, he taught for 2 years in an academy and, from 1911 to 1922, was a member of the department of biology at the University of New Mexico. He received the degrees of master of arts (1914) and doctor of philosophy (1922) at the University of Illinois, working in ecology under V. E. Shelford. His special interest in community ecology and suc-

cession began then and continued throughout his entire career. He was professor of biology at James Millikan University for 2 years and then came in 1924 to the University of Oklahoma as a professor of zoology.

As a student of developing biotic communities, Weese was, of course, interested in all of their components, both plant and animal, but his personal studies turned especially to the insects of the grasslands. In New Mexico he studied also the horned lizard and, at the seashore, annelids and sea urchins, all in relation to

the communities to which they belonged. The summers for him were always a time for study, and he took part in the work of various summer laboratories. He was in charge of the course in marine ecology at the Friday Harbor Laboratory in Puget Sound in 1925 and 1929. He and I were among the founders of the Rocky Mountain Biological Laboratory at Gothic, near Crested Butte, Colorado, in 1928, and he succeeded to its presidency in 1938. For many years he was responsible for the instruction in ecology at that laboratory.

He was the secretary of the Ecological Society for 10 years and its president in 1931. He served officially in various other organizations, including the Ecologist's Union, the National Research Council's Committee on Grasslands, Grasslands Research Foundation, and the Oklahoma Biological Survey. In all of these he held important offices and helped shape policies. He played a significant part in the activities of the Oklahoma Academy of

Science, and he was a member of various scientific, academic, and honorary societies. He served on both state and national committees that were concerned with ecology and its applications. As ecologist with the Oklahoma Biological Survey, he participated with his students in many field excursions, and his own published output of scientific contributions was augmented by their work.

Dr. Weese was the recipient of many distinctions within the university, such as the deanship of the graduate college for a period, the chairmanship of the Committee on Faculty Research for some years, and, during the last years of his life, the David Ross Boyd professorship

of zoology. He held elective offices in such local organizations as Sigma Xi, the American Association of University Professors, and others.

His most significant trait was his wide and profound knowledge; his reading and study were extensive and not limited to his field of research. He was an able invertebrate zoologist, he was interested in the quantitative aspects of genetics as well as bioecology, and he developed a course in quantitative biology. He was able to penetrate obscure problems and to give advice and illumination on matters both inside and outside his own fields. He contributed greatly to the development of the departmental library at the

university and was responsible for many of its acquisitions. Since his death, this library has been named in his honor.

A kindly, interested, and cooperative scholar, he left a moving and permanent impression on all with whom he came in contact. He bore his own burdens simply and fully shared those of his colleagues and his community, which, in a growing university, were often not light. Like so many whose accomplishments remain long to tell their stories, he will never be replaced, and he will live on in the deeds of his students and associates.

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H. H. De Jong,
Experimental Neurophysiologist

The history of experimental catatonia is the history of the collaboration by investigators from different countries, united in the love of science and in the enjoyment of scientific research, as well as by the synthesis of data from physiology, biology, and clinical experience—a synthesis indispensable to the future progress of neuropsychiatry.

H. Holland De Jong was born in 1895 in Sneeck, Holland. In 1928 at a meeting of the Société de Neurologie de Paris, he presented a paper on the treatment of tremors by bulbocapnine, an alkaloid that he regarded as an "antagonist of tremors." In the course of plethysmographic investigations of many patients, De Jong had discovered a "vascular rigidity" in catatonics that he failed to find in other patients. He thought of the possibility of the experimental reproduction of this illness, and, on the advice of Magnus, became interested in bulbocapnine. But being unacquainted with clinical catatonia, he had not begun this work.

At that time, I had, with others, undertaken a systematic study of Kahlbaum's catatonia, with a physiological exploration of this illness by new processes and a comparison of the physiological data with precise clinical data based on a study of many patients. I had reached the conclusion that catatonia constitutes a psychomotor syndrome of toxic origin.

I approached De Jong, and we agreed to collaborate.

Our first joint experiments were undertaken in Paris in the laboratory of Claude. We compared in detail the symptoms that were produced by bulbocapnine in the cat with the symptoms of human catatonia. We found in the animal, as in human beings, the various manifestations of catalepsy: active and passive negativism, barriers, catatonic hyperkinesis, and neurovegetative disturbances, including salivation and respiratory disturbances. The same parallelism was also found in electromyographic curves and in disturbances in chronaxie, as in the experiments of Bourguignon and De Jong, and in man by Claude, Bourguignon, and Baruk.

Our next experiments were in Amsterdam in the laboratory of Brouwer. We were able to study in detail the action of bulbocapnine for a wide range of doses and for the entire vertebrate series. In this way, we formulated laws describing the stages that follow increasing doses, from sleep to catalepsy, from catalepsy to negativism, from negativism to hyperkinesis, and finally, with still stronger doses, to epilepsy and the rigidity of decerebration. In similar fashion, the study of the animal series, starting with fish through batrachians, reptiles, and birds to mammals, permitted us to verify the

role of the cortex. We described this work in our joint book (1930), which brought us both the Herpin prize of the Academy of Medicine.

De Jong and I were then separated. I remained in Paris, and De Jong went to the United States to teaching posts at Duke University and Johns Hopkins University, where he discovered experimental hormonal catatonia (adrenaline, acetylcholine, and so on), surgical catatonia, mescalinic catatonia, and so on. During this period, I discovered, in Paris, experimental catatonia that was induced by the toxin of *Escherichia coli* (1933), biliary experimental catatonia (with Camus, 1934), and other types of experimental catatonia, induced by cerebral edema, ACTH, chlorpromazine, and so on. During his last years, De Jong investigated the role of intestinal and hepatic factors. His research on catatonia by obliteration of the intestinal lumen and by ligature of the biliary ducts must have led him to consider again conceptions of the role of hepatointestinal factors in catatonia and schizophrenia, conceptions that, after the work of Buscaino and our work on this subject had converged toward our present conclusions, have had important therapeutic results.

Recently, De Jong and I began to think about collaborating again in Paris. Alas, his premature death on 16 February 1956, at the age of 61, in Osawatomie, Kansas, where he was director of research and education at Kansas State Hospital, prevented the realization of this plan. This great misfortune abruptly ended a life dedicated to science and deprived medicine of a scientist of exceptional intelligence and perspicacity, a scientist whose goal it was to discover the causes of mental illness.

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