MASTER

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MAY 19-22, 1964

IRVING A. SINGER, MAYNARD E. SMITH, AND EUGENE W. BIERLY, EDITORS

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IRVING A. SINGER, MAYNARD E. SMITH, AND EUGENE W. BIERLY,* EDITORS

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BROOKHAVEN NATIONAL LABORATORY
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PREFACE

Unlike many of the larger groups of scientists associated with AEC activities, the meteorologists have never been organized under a single division of the Commission. This is both natural and appropriate since their functions at the various sites differ widely, but it tends to inhibit information exchange. It is therefore doubly important that representatives of the various groups have an opportunity to meet at reasonable intervals. Initial meetings of AEC meteorological people were first held in Washington at the instigation of the Division of Biology and Medicine, and, partially because of security restrictions, they were limited to Commission and contractor personnel. The two most recent meetings, however, have established the principle of rotation of the conference site among the national laboratories, each of which has a meteorological staff. The first was held at Hanford in 1960 and this is the second.

The current meeting departs from previous practice in that the list of participants is much more extensive than at earlier conferences. A deliberate attempt has been made to include junior members of the contractors' staffs, and a large number of representatives from organizations other than the AEC are present, including several from foreign countries.

The current meeting also represents a departure of another sort, since the agenda for the first time formally included review of critical subjects as well as descriptions of site programs.

MAYNARD E. SMITH

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The Brookhaven Meteorological Program

MAYNARD E. SMITH

The meteorological work at Brookhaven has developed into three distinct areas of activity, each of which occupies an approximately equal portion of our time and effort. These areas include (1) studies of low level atmospheric structure and wind flow, (2) diffusion-deposition experiments, and (3) the development of instruments and techniques required for the foregoing.

Inasmuch as you will hear more later about certain portions of our studies, such as the copper and pollen deposition work and our investigations of wind profiles, I shall deliberately emphasize portions of the program with which you might not otherwise become familiar. I shall also introduce the names of the individuals primarily involved, so that you can associate the proper people with specific subjects.

LOW LEVEL WIND STRUCTURE, TURBULENCE, AND FLOW PROBLEMS

The Brookhaven group has long had an interest in the vertical wind profile in the lowest 100 meters, and within the last year or two (partly in association with Panofsky and Blackadar who have been here as summer guests) we have intensified our attempts to reach a better understanding of these problems.

The initial requirement for development in this area is an extremely good measuring system, a fact sometimes ignored in previous investigations. Perhaps our most important activity in this area, therefore, has been the development of a series of accurate and sensitive anemometers. Our instruments were designed to compromise between the sensitivity of equipment such as the Beckman and Whitley or Climet units and the durability of the Aerovane. A unique snap-lock anemometer cup design, together with a lot of hard-won accomplishments in the bearing and transistor areas have given us our basic piece of equipment for this work. The instrument will respond to winds of slightly less than $\frac{1}{2}$ mph but will survive gusts of more than 125. It is possible to match these instruments

to considerably better than 1%, a requirement necessary to establish reliable profile information. The recording systems associated with these will be described in the final portion of this presentation. R.M. Brown, W.A. Tuthill, and M. Smith are primarily associated with this work.

To summarize our most recent developments in regard to profiles, we are encouraged by the reproducibility of our data, but we are intrigued by the precise details of the profiles under various conditions. There is a strong suspicion at least that the time-honored relationship between the neutral meteorological conditions and the simple logarithmic profile may require a much closer investigation that it has had heretofore.

A second portion of the work on low level wind involves a systematic review and development of both our ideas and knowledge of atmospheric flow and structure. This includes a careful investigation of the simpler types of flow about which we know a great deal (those which have been investigated thoroughly in the wind tunnel) and an attempt to extend that knowledge into the atmospheric realm by detailed consideration of both meteorological and fluid-dynamics fundamentals. One of the major concerns is the description of stable atmospheric flow. We are, of course, equally interested in the spectral estimates of various fluctuations in the flow.

A third area of interest involves the effects produced by roughness elements and changes in roughness on low level turbulence and wind profiles. We have quite a number of projects in this field involving the study of the wind within, above, and at distances from forested areas. In this we are making use of three fixed towers; two in relatively open terrain, 420 ft and 160 ft high, and a smaller tower constructed within the forest itself. Four mobile towers are being used in studies of the changes in turbulence and profiles as a function of distance from specific roughness elements.

Another important phase of the work is a study designed to determine the best method of predicting the variation of wind with height. This is sim-

ilar but by no means identical to our interest in a better theoretical understanding of the problem. The studies of the low level wind are conducted primarily by I. Singer, A. Arnold, and J. Frizzola.

DIFFUSION-DEPOSITION STUDIES

Our field activity involving Cu^{64} , uranine dye, and pollens constitutes a major portion of the experimental work. At present we are working in several areas.

An open field (which is being repiped at present) is used to study small-scale diffusion and deposition over open ground and grass, and in another series of experiments we are investigating the penetration of copper particles in forested areas. This work will be discussed at greater length later on in the program. The same can be said of the pollen project, which is a cooperative effort between our group and the New York State Museum and Science Service. In this area our studies concentrate on the behavior of various pollens such as the 20- μ ragweed, 32- μ timothy, and 90- μ corn. G. Raynor and L. Cohen are primarily concerned with this in our own group. E. Ogden of the New York State Museum and Science Service and our consultant, B. Davidson of NYU, have also been involved in this work.

I would like to elaborate on two aspects of the diffusion-deposition program which are not likely to be discussed at length later on in the program. The first is the study of the rise of hot clouds of gas created at ground level by the burning of rocket fuel. Our program was originally cooperative with the Atlantic Research Corporation, which has a very practical interest in the dispersion of effluent from solid rockets. ARC developed an aluminum analog of its beryllium fuel for testing in this program.

Our initial series of experiments consisted of the burning of varying quantities of the fuel to produce as quickly as possible high temperature clouds, the total heat content of which was deliberately varied over a wide range. Fuel amounts burned, for example, ranged from 2 to 3 lb to as much as 400 lb in ≈ 1 min.

No tracer was added to the cloud, since combustion produces a fine smoke of aluminum oxide, and all measurements were either photographic or visual. In brief, the plan involved estimating such parameters as the rate of rise, maximum height of rise, and geometric configuration of the cloud by a

combination of time-lapse photography and personal observations on a large tower. Further data were obtained from a light aircraft which followed the cloud at least to the point where it leveled off.

In our first series of runs, we were exceedingly fortunate in being able to obtain nearly perfect examples of all four of the gustiness conditions which are representative of typical atmospheric conditions here, and the results are quite gratifying. These data have been analyzed, and the results will be presented formally next month.*

We have found surprisingly close agreement between our results and the predictions of Sutton, Holland, and Priestley. The last particularly seems to represent a most appropriate description of the cloud behavior.

I regret to add that some of our enthusiasm has been damped by the realization that the results are not completely reliable. Our main difficulty with these first runs was that the fuel grains did not burn with acceptable uniformity, so that, while it is possible to define the total heat release quite accurately, the rates of heat release varied far more than we would wish.

Following the initial series of runs, we asked the Atlantic Research Corporation to attempt to prepare a fuel more suitable for this type of work. We have just finished testing a viscous liquid form, which we spread into a very thin layer of varying surface area to obtain the desired change in heat release rate. These new fuel elements will approximate much more accurately an instantaneous release, since we can accomplish the entire burn in ≈ 10 sec. Test experiments indicate that the rate of burning is constant from about $\frac{1}{2}$ sec to about $9\frac{1}{2}$ sec with a very sharp cutoff when the fuel is exhausted. A new series of cloud experiments will begin as soon as this conference is completed.

Since this conference is one in which we are to review work in progress and plans as well as accomplishments, I should like to mention a study of I^{131} which is about to be undertaken. The cooling air of the Brookhaven Graphite Research Reactor contains, on the average, a small amount of radioiodine which at full reactor power is released at a rate of 8×10^8 Ci/sec. Theoretical calculation of the scavenging of this iodine by precipitation (based on Chamberlain's work) suggests that we should be able to obtain measurable quantities with a

*Singer, I.A., Frizzola, J.A., and Smith, M.E. The prediction of the rise of a hot cloud from field experiments, *J. Air Pollution Control Assoc.* 14, No. 11, 455-8 (1964).

6-ft-diameter collector, even at rainfall rates as low as 0.01 in./hour. Preliminary studies of actual deposition at a nearby site indicate that this is probably true.

The objectives of this work are twofold. One aim is to determine the efficiency of I^{131} removal from the reactor plume by natural rainfall. In this, we are relying on extensive knowledge of the diffusion characteristics of the plume obtained over many years of experiment. We will also assay the deposition of the I^{131} on vegetation accompanying the rainfall by means of grass plots grown at the sites of the rainfall collectors. Artificial collectors will be evaluated at the same time. C. Nagle and M. Smith are the individuals mainly concerned with this study.

INSTRUMENTS AND TECHNIQUES

I have already mentioned our interest in more suitable wind equipment, particularly anemometers. We are also continuing work on the development of a completely digital wind vane output suitable for all sorts of automatic summarization.

As is true of many other groups participating in this conference, we are interested in automatic systems. A paper tape recording system, in use for

our main tower installation since 1959, is to be revised to place the output on magnetic tape so that the entire instrument will be capable of considerably more rapid sampling speeds. In addition to this, we have stressed the development of inexpensive, integrated recording wind systems, one of which uses beam-tube scalers and prints its output on cards by means of the inexpensive IBM 526. These data require no further processing for computer use. The other is a highly reliable and very simple counting system utilizing Sodeco printing counters, suitable for long-term mean studies.

We also are currently working on the development of an inexpensive and adaptable temperature sounding system, which I first discussed with R.C. Wanta at the Symposium on Air Pollution Meteorological Instrumentation in 1959. The basic idea of this is a temperature sensing element controlling the frequency of a very simple transistorized transmitter feeding into a receiver, a simple rate meter, and an Esterline Angus recorder at the ground. We hope to keep the transmitter small and light enough to make it exceedingly easy and cheap to use, and estimates of the cost of the ground-based equipment would be of the order of \$1000 as opposed to many thousands for the standard radiosonde.

The Argonne Meteorology Program

HARRY MOSES

I will describe our program, and Dr. Strom will discuss, in the next paper, how we relate results obtained with models in the wind tunnel to experimental measurements in the field. We have a rather small group. It consists of three professional meteorologists, two technicians, and a secretary as our permanent staff. During the summer we augment our group with two or three visiting scientists and two or three student aides who help with data analysis. Our work is divided into several sections. One is the climatology program, another deals with atmospheric diffusion. We study diffusion in two ways: one involves obtaining data on the geometry and height of smoke plumes, the other deals with measurements of variations in time and height of concentrations of natural radon emitted from the ground. We also carry out special studies, which I will describe later. It is inevitable in work of this type that some effort be devoted to instrument development. Later in the program Dr. Paul Frenzen will discuss a low-inertia high-torque anemometer he has developed, and he will also discuss work we are conducting on basic turbulence. Another part of our program, at present relatively minor, deals with an interdisciplinary area, high energy physics and meteorology: we have been looking into the use of a high energy proton beam as a meteorological tool. Some of the details of our program and a few of our findings are discussed below.

CLIMATOLOGY

As part of our climatological program, we have an automatic data processing system to handle our data. Information is recorded in three ways: (1) teletypewriter, (2) pen and ink traces, and (3) paper punch tape. All three are needed. The automatic data processing system records 38 different meteorological variables once per hour in this way. The following measurements are handled:

1. Wind speed at 9.38, 18.75, 37.5, 75, and 150 ft.
2. Wind direction at 18.75 and 150 ft.
3. Dew point at 1.17, 2.34, 4.69, 9.38, and 131 ft.

4. Stability as measured by the temperature difference between the top and bottom of 4 atmospheric layers. These layers extend from 5.5 to 18.75; 5.5 to 37.5; 5.5 to 75; and 5.5 to 144 ft.

5. Solar and sky radiation on a horizontal surface 6 ft above the ground.

6. Net radiation flux at 6 ft above the ground.

7. Precipitation.

8. Air temperature.

9. Soil temperature at 7 depths: 1, 10, 20, 50, and 100 cm; and 10 and 29 ft below the surface.

10. Soil heat flux at 1, 10, 20, 50, and 100 cm.

11. Soil moisture at 1, 2, and 4 ft. Soil moisture soundings can be made from 1 to 9 ft below the surface. The measurements are made with a neutron probe.

12. Soil temperature at 1 cm below the surface under 4×4-ft plots of each of the following surface types: white sand, black dirt, and Kentucky blue grass.

Other measurements not handled by the data system are soil density by means of a gamma-ray probe, atmospheric pressure, the information provided by two hygrothermographs, and wind direction at 9.38, 37.5, and 75 ft.

In the next few years we hope to prepare for publication our climatological data for 15 years, July 1, 1949 through June 30, 1964.

At this point it might be well to issue a plea for stock-taking concerning the kinds of climatological analyses that have been published by the meteorology groups at AEC installations. Many of the climatological measurements are quite good, but there is room for improvement in the types of analyses made. Granted, there are advantages to differences in data presentation because of variations in the climate at the meteorological sites. Nevertheless, guidelines for data presentation would be desirable. One group in the AEC which requires meteorological data in carrying out its functions has indicated to me the inadequacy of climatological summaries published by the meteorology groups. It appears desirable that a working group be established in the near future con-

sisting of representatives from all the AEC meteorological groups and other users of meteorological information within the AEC to formulate recommendations on the types of climatological analyses needed.

SOIL MEASUREMENTS

Examples of analyses carried out by Dr. James E. Carson of our group, based on our soil measurements, are shown in the following four figures.

Figure 1 shows the variation of soil density in pounds per cubic foot from 1 to 9 ft below the surface. It is of interest that the density increases rapidly from 1 to 4 ft below the surface but remains relatively constant to 9 ft. The specific gravity is

slightly less than 2 in the upper layers but slightly greater than 2 below 4 ft.

In Figure 2 the variation of soil moisture with depth is shown for two different days: July 23, 1963 and August 15, 1962. The July 23 sounding shows that the soil was relatively moist throughout, near 30% by volume. The August 15, 1962 sounding shows that the 1-ft level was quite dry, about 21%, but that the moisture increased rapidly from 1 to 2 ft and remained quite high for the remainder of the sounding.

Figure 3 shows the variation of soil moisture at the three measuring levels during the period August 10, 1963 through April 10, 1964. Daily values of precipitation are shown as vertical lines in the lower part of this chart. It is to be noted that little

Figure 1. Soil density profile, 2 Oct 1962.

Figure 2. Soil moisture profiles.

Figure 3. Rainfall and soil moisture, Argonne, Illinois.

Figure 4. Rainfall and soil moisture, Argonne, Illinois, 22 Nov 63.

change occurs at the 4-ft level but that marked changes occur at the 1 and 2-ft levels. This chart shows the relatively dry conditions at the 1-ft level during the summer and the increasing moisture during the fall and winter. During these two seasons the moisture accumulated exceeds that evaporated, which results in a net gain of water in the upper levels of the soil. In our radon measurements this information is of importance, since the radon emanation rate is reduced when the soil is moist.

The effect of a heavy rainfall during a 24-hr period on soil which was dried out substantially in the upper layers is shown in Figure 4. On November 22, 1963, for example, it is evident that the substantial amounts of rainfall which occurred between 1500 and 2200, shown as vertical lines in the lower part of the chart, caused a marked increase in soil moisture at the 1-ft level and a measurable amount at the 2-ft level. At 4 ft there was no measurable increase during this period.

Figure 5. Ion concentration as a function of height 10, 20, and 30 μsec after exit of proton beam's leading edge.

Figure 6. Ion concentration as a function of height 30, 40, and 80 μsec after exit of proton beam's leading edge.

It is well to point out that the neutron probe used for soil moisture measurements is an instrument that requires a substantial amount of servicing. We have recently discovered that our calibration for the above 6 months prior to May 1, 1964 was too low by about 5%. However, the instrument can be made reliable by frequent calibrations.

THE PROTON BEAM AS A METEOROLOGICAL TOOL

Dr. Ronald L. Morton, Associate Director of our Particle Accelerator Division, asked me, "What would happen if we shot a high energy proton beam vertically at a thunder cloud?" This was an intriguing idea. At Argonne we have a machine called the Zero Gradient Synchrotron which can accelerate protons to energies of 12.7 BeV. About 10^{13} protons can be accelerated in a burst once every 4 sec. It is possible to take the beam out of the machine in 10 μsec, or longer if desired. The area from which the beam is emitted is 50 sq cm. As it comes out into the atmosphere, it would have a divergence of 0.4 milliradians. A 12.7-BeV proton beam has the capability of ionizing the air from the ground all the way up through the atmosphere, since it takes only about 2 BeV to pass through it.

Assuming that it takes 35 eV to create one ion pair and a recombination coefficient of 1.6×10^{-6} , calculations were made of the atmospheric ionization as a function of height. These are shown in Figures 5 and 6, assuming that it takes 10 μsec for the beam to be emitted. Note in Figure 5 that at the end of 10 μsec there would be 10^{12} ion pairs/cc near the surface. Figure 6 shows that at 80 μsec the ionization near the ground is still appreciable, 10^{10} ion pairs/cc, but falls off rapidly with height. This is due to attenuation of the beam by nuclear interactions and by divergence.

It is intriguing to speculate how a proton beam may be used as a meteorological tool. For example, such a beam of suitable energy and having an appropriate number of particles may be injected into stratus or fair weather cumulus clouds and may thus bring about a rapid redistribution of the charges within them. Although such clouds are formed primarily by thermodynamic or hydrodynamic processes, the occurrence of a measurable effect on the cloud circulation pattern such as a

change in the growth or dissipation rate would provide valuable information on cloud dynamics.

The ionization produced by a proton beam may well create a preferred path for a lightning discharge. We are not at all sure that this would happen, but there is evidence to indicate that it may. If one assumed that a thunderstorm cell has a radius of 1 to 2 km, then natural cloud-to-ground lightning would occur anywhere in an area of about 10 sq km. The advantages for meteorological research that would result from lightning occurring repeatedly in the same place instead of randomly distributed over a sizeable area would be substantial. For example, information could be obtained on both the areal and temporal distribution of rainfall with respect to the location of the lightning stroke.

Moore et al.* have observed increased radar echo development followed by heavy rainfall within a few minutes after a lightning flash occurred within a given region. These investigators state that the lightning flash probably contributes to the formation of the "rain gush" by greatly enhancing

*C.B. Moore, B. Vonnegut, J.A. Machado, and J.H. Survilas, Radar observations of rain gushes following overhead lightning strokes, *J. Geophys. Res.* 67 (1), 207-20 (1962).

the rate of coalescence of rain droplets. This, in turn, modifies the circulation within the storm cell. If the Moore-Vonnegut hypothesis is correct, and if a proton beam can indeed induce a lightning stroke, then one would expect to find a maximum in the isohyetal (rainfall) pattern some distance downwind from the proton accelerator. A sufficient number of such experiments could establish with an acceptable level of statistical significance whether the Moore-Vonnegut hypothesis is valid. If it were valid, then one would be tempted to argue that a proton beam can indeed modify the distribution of rainfall on the ground. To establish the magnitude of this effect would require careful consideration, but the economic value might be substantial.

SMOKE STUDIES

At Argonne an experimental meteorological stack is used for conducting smoke studies (Figure 7). These studies have been concerned primarily with relating height of plume rise with meteorological conditions. It is possible to heat the stack effluent to about 140°F above ambient tempera-

Figure 7. Argonne meteorological stack and surrounding terrain.

ture by means of a furnace coupled to the stack. To date 151 runs have been made consisting of measurements under neutral stable and unstable conditions. Various combinations of stack temperatures and stack emission rates were used. During the experiments stack temperatures were set at 30°, 60°, 100°, or 140° above ambient and stack emission rates at 1500, 3000, or 4500 cfm.

The smoke plume is photographed with two Nikon 35-mm cameras. One camera looks at the plume from the bottom, and one from the side; this allows photogrammetric measurements of positions in space of selected points on the smoke plume. These measurements are currently being analyzed.

RADON STUDIES

We have carried out studies with natural radon used as a tracer. We have made two sets of radon profile measurements and have conducted another series of measurements over a period of about a year in which 8-hr samples at 3 ft above the ground were obtained consecutively during the working week. These 8-hr samples were taken from about 8:00 AM Monday morning until about 5:00 PM Friday afternoon so that 3 samples per day were obtained. An interesting finding, shown in Figure 8, was that a snow cover of 4 in. or less is an inefficient barrier for the emanation of radon. Nevertheless, it is an efficient thermal blanket preventing heat loss from the soil to the air above.

In another radon experiment observations of temperature, wind speed, and radon were averaged for 20 min at 90-min intervals, for periods of ≈ 24 hr. Wind speed and temperature were measured at 0.5, 1, 2, 4, 8, and 16 m above ground. Radon samples were obtained at 1 cm and 0.5, 1, 4, and 16 m above ground on a portable TV tower as shown in Figure 9, and also at 40 m on the 150-ft meteorological tower. Cassella anemometers were used for measuring wind speed, and copper-constantan thermopiles consisting of 6 thermocouples each were used for temperature measurements.

In Figure 10, wind speed, temperature, and radon concentrations are shown as functions of time and height in a three-dimensional plot. The diurnal variations are clearly shown. Note the relatively high concentrations of radon accumulated near the ground in the early morning hours of July 29, 1960.

Figures 11 and 12 are based on one of the studies carried out with these data. In Figure 11 the integrated amount of radon per sq cm over the layers 0 to 50 cm, 0 to 1600 cm, and 0 to 4000 cm are compared with atmospheric stability as measured by the temperature difference between 50 and 1600 cm. The relationship between stability and integrated radon content is clearly shown. In Figure 12 the same radon data, but plotted on a semilogarithmic scale, are compared with the total kinetic energy in the column from 50 to 1600 cm. An inverse relationship between these variables is clearly evident. If one considers a bulk Richardson's number as related to $\Delta t/u^2$ where Δt represents the stability and u the mean kinetic energy of the column, it is readily seen that the integrated radon concentration is closely related to this variable.

Comparison of the data plotted on semilogarithmic and linear scales shows that the variation amplitude of the lowermost layer of integrated radon concentration, i.e., surface to 50 cm, is greater on the semilogarithmic than on the linear plots. This difference occurs because the semilogarithmic chart is a ratio chart and brings out the percentage variation, while the linear chart brings out the incremental changes. It is, therefore, evident that the percentage variation of radon content is greater in the lowermost layer than in the total air column extending from either the surface to 1600 cm or the surface to 4000 cm; however, the variation in actual radon content of the higher columns is larger.

PLUME RISE SURVEY PAPER

I spent the past few months in collaboration with Professor Gordon H. Strom and Dr. James E. Carson in writing a survey paper on smoke plume rise. In this paper we have examined as many reports as we could find providing plume rise measurements. Special emphasis was devoted to the three most commonly used formulas, the Davidson-Bryant, the Holland, and the 1957 Bosanquet formulas. This paper will be published in *Nuclear Safety* within the next few months.

A major portion of the measurements used for checking the plume rise equations is quite poor. Most of the measurements suffer from the fact that large-scale eddies cause changes in plume rise by a factor of 2 or more within a few minutes. This is

Figure 8. Relative frequency distributions with snow cover.

Figure 9. Instruments on portable meteorology tower.

Figure 10. Three-dimensional diagram relating height and time with the meteorological variables wind speed, temperature, and radon concentration.

Figure 11. Time variation of total radon content per sq cm, stability, and kinetic energy, 8-9 June 1960, Series 1 (stability scale).

Figure 12. Time variation of total radon content per sq cm, stability, and kinetic energy, 8-9 June 1960, Series 1 (kinetic energy scale).

Figure 13. Plume rise as a function of distance based on 10 runs. Individual symbols $\circ$ and $\blacksquare$ indicate the two highest runs and $\bullet$ and $\square$ indicate the two lowest runs. Hatching indicates areas where intermediate plume rise measurements were obtained. The symbol $\times$ indicates height calculated from Bosanquet's formula. Data taken by Rauch at Duisburg, Germany, 16 Sept 1959.

Figure 14. Time variation of plume rise at a horizontal distance of 450 m from stack. Data taken by Rauch at Duisburg, Germany, 16 Sept 1959.

illustrated in Figure 13, which is based on 10 runs by Rauch* at Duisburg, Germany, over a 31-min interval. The hatched area represents the region within which measurements were made. Also shown are the plume rises calculated by the Holland, the Davidson-Bryant, and the 1957 Bosanquet equations. In addition, the Holland and Davidson-Bryant estimates are shown with a correction factor as proposed by Stümke.** The effect of eddies is demonstrated perhaps more clearly in Figure 14, which gives the time variation of plume rise at a horizontal distance of 450 m from the stack. It is evident that this effect must be taken into account in computing ground concentrations of contaminants from an elevated continuous point source.

We have concluded from an examination of the data that the Holland formula is probably the most practical for routine use; however, the following correction factor, first suggested by Stümke and extended by us, is necessary: A factor of 2 should be used for small stacks, that is, those with a diameter of 1 to 2 ft, and which have little buoyancy; for larger power stacks with diameters exceeding 10 ft, where appreciable buoyancy exists, a factor of 3 is appropriate.

WIND PROFILES

Another study using the wind data obtained during the radon measurements involves comparing the wind profiles with the following three well-known profile equations:

$$\text{Power law: } \bar{u} = cz^p$$

$$\text{Logarithmic law: } \bar{u} = \frac{u_*}{k} \ln \left(\frac{z}{z_0} \right)$$

$$\text{Log plus linear law: } \bar{u} = \frac{u_*}{k} \left[\ln \left(\frac{z}{z_0} \right) + Bz \right]$$

In the equations, $\bar{u}$ is the average wind speed at the level, z , to be investigated; u_* is the friction or dynamic velocity and equal to $\sqrt{\tau/\rho}$ where τ is the shearing stress and ρ the density; z_0 is the roughness parameter; k is the von Kármán constant; p is the power law parameter; and B is a constant. A least-squares technique was used for fitting each of these equations to the observed profiles.

*H. Rauch, Zur Schornstein-Überhöhung, *Ber. Meteorol. Inst. Techn. Hochschule Darmstadt*, 1962.

**H. Stümke, Zur Berechnung der Aufstiegshöhe von Rauchfahnen, *VDI-Forschungsh.* 483 (Ausgabe B, Bd. 27) 38-43 (1961).

In air pollution problems such as determining the height of plume rise as a function of distance from a stack or estimating the ground concentration of an effluent at various distances from the source, accepted formulas may yield values which differ by a factor of 2 or more from those observed. This study shows that the wind profile formulas may be made to fit measured values much more closely.

Figure 15 shows a "less than" ogive for values of maximum deviation from the fitted regression line for each of the three profile equations. It is evident that the log plus linear formula provides the smallest maximum deviation values, as may be expected. However, even the greatest deviations are relatively small, that is, <0.4 m/sec. Figures 16 and 17 show the "less than" ogives for values of the root mean square deviation from the regression lines for data obtained during the day (0700 to 1800 CST) and during the night (1830 to 0630 CST). It is evident from these charts that a close good fit is obtained.

It may be pointed out that for the power law, 98% of the cases had root mean square deviations from the fitted equation of <20 cm/sec and 87% of the cases had maximum deviations from the least-squares line of <20 cm/sec. Corresponding percentages for the logarithmic law were 96 and 87, and for the log plus linear law 100 and 99.

DISCUSSION

PACK: In your 20-min sampling of the radon, did you have to use high flow rates or did you have additional sensitivity in counting so that you could cut your sampling time so short?

MOSES: We used 50 to 100 liters in the 20-min period.

HOLLAND: Concerning the proton beam, what would be the minimum usable energy for protons or electrons for a meteorological experiment that one might consider as a pilot study without going immediately to 12 BeV?

MOSES: It takes about 2 BeV to penetrate the entire atmosphere. I can give you some numbers from which this may be calculated. At relatively high energies the absorption rate is 2 MeV/g air. A gram of air is contained in a column of 1 sq cm cross section and 8 m high. Thus, 2 MeV are lost per 8 m, and 20 MeV in 80 m. For a homogeneous atmosphere of about 8000 m the absorption will, therefore, be 2000 MeV, or 2 BeV.

HOLLAND: That is with the 12-BeV beam?

MOSES: The curve relating absorption rate to energy has a minimum at about 2 or 3 BeV. This minimum is at an absorption rate of 2 MeV/g per sq cm. Thus, with a 12-BeV beam the absorption rate would be somewhat higher than 2 MeV/g; as a result, with a 12-BeV beam there would be greater ionization per unit length tra-

Figure 15. "Less than" ogive for root mean square deviation values during the day (0700 to 1800 CST).

Figure 16. "Less than" ogive for root mean square deviation values during the night (1830 to 0630 CST).

Figure 17. "Less than" ogive for maximum deviation values for day and night.

versed. In considering this problem we wondered what would happen if a beam was of such low energy that it traversed only $\frac{1}{2}$ or $\frac{3}{4}$ of the way between the ground and the thunderstorm charge center. With such a beam there would be an increased amount of ionization near its stopping point. This increased ionization might be more effective in triggering a lightning stroke than a beam that went all the way through the atmosphere.

HOLLAND: To go $\frac{3}{4}$ of the way through a thunderstorm, what sort of energy did you calculate was needed?

MOSES: Speaking from memory, I think it was about 500 MeV.

SMITH: Is your accelerator well designed for lightning?

MOSES: This is a question often asked in connection with the proton beam problem. If enclosed in a Faraday cage, which may take the form of a thin aluminum hemisphere, the accelerator is protected. The high energy protons from the accelerator would have sufficient energy to pass through the aluminum enclosure, but the ions in the lightning stroke current would flow to the ground.

REIFF: How do you propose to apply the information you have obtained on the soil studies?

MOSES: It depends on what you mean by "apply."

REIFF: It is not clear to me what application of the resulting data you have made with respect to meteorological predictions. For example, can you employ this information for assistance in the location of reactor sites?

MOSES: First of all I should like to think that we are doing basic research and are making measurements because we are interested in what is happening in nature, such as the processes which control the interchange of energy between the atmosphere and the surface below.

Soil measurements such as we are making are applied to this kind of problem. I might also indicate that we give soil moisture and soil temperature information to the news media, such as radio and TV. Those who conduct farm programs are especially interested in this kind of information. We feel that we are able to perform a service to the public by providing data on the moisture content of the soil at 1 or 2 ft below the surface. This service requires very little work on our part and is accomplished by a phone call periodically, at present only about once or twice a month. Information on soil moisture is very important for radon studies, since the emanation of radon varies with the amount of moisture in the soil. This is an example of direct application of this kind of measurement to our research. There is no difficulty in justifying this program.

SMITH: I agree that not all these things need an obvious justification to begin with, but we conducted a very similar study - in fact, we used some of Moses' data - when the question came up at BNL as to how deep we should bury the AGS installation in order to achieve appropriate temperature stability. This of course was extremely important to the designers, since they were working with an accuracy of a few thousandths of an inch over a very large distance. This is the sort of application for which studies of this kind are useful.

LIBARKIN: You mentioned measuring the solar heat flux also. Did you use a solar heat flux plate?

MOSES: Yes. We used the Beckman and Whitley net radiation flux plate.

LIBARKIN: Are you still using it?

MOSES: Yes. We have had no problems with it.

Scale Model Diffusion Experiments

GORDON STROM

The Atomic Energy Commission through the Argonne National Laboratory is sponsoring a program of scale model diffusion experiments in the Air Pollution Wind Tunnel at New York University. The general objective of this program is to model in the wind tunnel at appropriate scale those motions of the atmosphere significant to diffusion including convective turbulence. The activities fall into two broad categories. One is the development of equipment necessary to achieve the objective, and the other is the conduct of correlation experiments for evaluation of equipment development and to show where improvements are needed. The correlation experiments are conducted on a scale model (96:1 linear scale) of the Argonne experimental smoke stack described earlier.

At the time the program was started the New York University wind tunnel had undergone considerable development in the direction of achieving diffusion modeling with thermal control. Low speeds needed for Froude number modeling of buoyant gas plumes were obtainable under neutral thermal conditions. Equipment was available for producing vertical temperature gradients, but the resulting airstream was not usable because of the creation of spurious convective air currents.

The wind tunnel is of the open return type in which the air supply is taken from within the building and exhausted outside through the roof. The test section is rectangular in cross section, 3½ ft high, 7 ft wide and 40 ft long. One side of the test section is glazed for visual and photographic observation of smoke plumes.

Determination of the causes of the spurious air currents was difficult because, as was later determined, they were generally induced by rather small differences in temperature in the various parts of the wind tunnel structure exposed to the airstream, particularly those parts upstream of the test section. Over a period of time there was developed additional and improved thermal control equipment and instrumentation. The point was

reached where stable and unstable temperature gradients could be produced without the spurious air currents.

Following the equipment development a series of experiments was conducted to determine the convective turbulence characteristics of the wind tunnel. Diffusion of the smoke plume from a model of the Argonne stack was determined in terms of its spreading characteristics. It was found that there was very little convective turbulence except with quite strong unstable gradients. There was also very little difference in diffusion between neutral and stable temperature gradients. The boundary layer in the test section was that which developed naturally over the floor on which some roughness elements were in place. This layer had negligible thickness near the beginning of the test section and grew to a depth of 10 to 15 in. 30 ft downstream where the model stack was located. Only the lower part of the smoke plume was in the boundary layer, with part or most of it in the free stream where there was relatively little mechanical turbulence.

A series of experiments was conducted with a velocity profile induced at the beginning of the test section similar in shape to that found in the field experiments. This was accomplished by introducing resistance to air flow in the lower layers by a vertical array of strings stretched horizontally across the airstream. This profile changed with downwind distance and was quite different at the model stack. There was some improvement in diffusion characteristics, but diffusion was smaller than that of the field experiments. The conclusion at this point was that the degree of mechanical turbulence was inadequate, and that no decision could be made on whether convective turbulence was adequate until the mechanical phase was improved.

Increase in airstream turbulence as well as control of velocity profile was accomplished with a set of roughness plates located at the beginning of the test section. These were metal plates placed in a

vertical array with the plates extending horizontally across the stream. On these plates were placed, normal to the stream, roughness elements which consisted of square or rectangular pieces of wood. By varying the size and number of elements as well as the spacing of plates, the turbulence and velocity profile could be changed.

Before presentation of experimental data obtained with the roughness plates, the thinking at this time on similarity criteria will be described. This has remained substantially the same since then. Figure 1 shows the variables considered significant to diffusion experiments. Smoke plume characteristics are expressed in terms of the plume height z_p and plume width b , the latter being a measure of diffusion. These are considered as functions of the 10 variables on the right side of the first equation in the figure. The first three are linear dimensions as shown in the sketch, with δ as the boundary layer thickness. ρ_s and ρ are plume and ambient air densities, respectively. $\Delta(dp/dz)$ is the difference between actual air density gradient and the adiabatic gradient. g is acceleration due to gravity. By dimensional analysis the second equation can be formed. Here the boundary layer thickness is taken as the reference dimension, although another dimension could be used. With this scheme of variables, proper modeling requires that the 7 dimensionless products on the right side of the second equation must have the same values in the model as in the prototype. It has not been established whether all are important, but experience has shown that all are potentially important. At the present state of development some cannot be produced in the

Figure 1. Variables used in similarity criteria.

model and may never be. There should also be added another variable which gives a measure of the airstream turbulence. One difficulty in achieving the above similarity criteria is that they are not all independent. For example, velocity profile, density gradient, and turbulence are interdependent.

The first two of three dimensionless products require geometric similarity between model and

Figure 2. Plume half-width in horizontal plane for model and prototype at downwind distance of 50 m.

Figure 3. Plume half-width in vertical plane for model and prototype at downwind distance of 100 m.

prototype. $\delta\Delta(dp/dz)/\rho$ can be expressed in terms of the airstream temperature gradient. $V^2/\delta g$ is Froude number. The density gradient requirement together with Froude number are equivalent to Richardson number. Some have proposed or used Richardson number as a sufficient modeling requirement in place of the other two, but experience with this program indicates that it may not be sufficient. More needs to be done on this subject.

Plume characteristics obtained in the last two series of experiments are shown in the remaining figures with prototype field data in three of them. Velocity and temperature profiles were also measured. These remained nearly constant with distance. Shown in Figures 2 and 3 is the diffusion of the plume as measured by the half-width in the horizontal and vertical planes. These were obtained from multiple exposure photographs of the type discussed by Mr. Moses in the preceding presentation. The model test results are expressed in terms of the full scale variables calculated by the similarity criteria given earlier. These have been analyzed in terms of stability ratio and temperature gradient. The stability ratio form is shown here because it appears slightly better than the temperature gradient form. $d\theta/dz$ is the potential

temperature gradient and u is the wind speed at approximately plume level. While the model results have a trend similar to those of the prototype, the magnitude is less. This suggests that at least the mechanical turbulence is deficient.

Plume rise, which is the height of the plume centerline above stack top, is shown for the two series of experiments in Figure 4. The downwind distance is 60 m, which was selected because of the availability of field data at this location. Plume rise is plotted against V_s/u , a ratio on which it is strongly dependent. The model plume is composed of a mixture of helium and air in a proportion necessary to give the desired density. Δt is the difference in temperature between stack effluent and ambient air which would give an all air plume the same density as the model plume. It may be a little premature to present these data as their analysis has not been completed. It is evident that series III shows larger plume rise values. The test data do not show the reason for the difference. One series was conducted during the winter and the other during the summer. This produces a difference in thermal conditions within the building which may have had an influence. It is felt that because of improved equipment and addi-

Figure 4. Plume rise obtained in series III and IV scale model experiments at full load ($V_s = 12.2$ m/sec) and downwind distance of 60 m.

Figure 5. Plume rise for model (series IV) and prototype at 60-m downwind distance.

tional experience, series IV gave the more reliable results.

Comparison of model and prototype is shown in Figure 5. Only $\Delta t = 60^\circ\text{F}$ data are shown as this is close to the prototype condition. Three-quarter load is also included as some of the prototype data are under this condition.

The next series of experiments will be conducted with a greater amount of mechanical turbulence. Under construction is a moving ceiling made of chain belts over rollers. This is being added to reduce or eliminate the ceiling boundary layer and improve the constancy of velocity profile with downwind distance. Turbulence measurements will be made with a hot wire anemometer.

DISCUSSION

McCORMICK: A few years ago Cermak from Colorado State reported at a meeting in New Orleans some experiments he had conducted on the diffusion of a plume in a wind tunnel, I believe with SO_2 , but that is not too important. The floor of the wind tunnel was heated, and in one series of experiments he kept the floor smooth and in others he put in roughness elements. He reported orally a very intriguing result, that when the floor of the tunnel was rough the apparent diffusion of the plume was less than when the floor was smooth. Have you performed any experiments on this, and, even if you haven't, would you comment on it?

STROM: I am not aware of encountering such a result in our wind tunnel. We have conducted a number of scale model experiments in which roughness of the surface played an important part, but we have not made a systematic study of the effect of changing the degree of roughness. Increase in roughness as caused by more rugged terrain or more and larger structures certainly causes greater diffusion. I imagine that in the Colorado State experiments the degree of roughness was quite small. The range of test variables was probably quite different. We are modeling with Froude number, which requires extremely low wind speeds for models of atmospheric winds. I believe our highest wind speeds are lower than the lowest used in the Colorado State experiments. Our temperature gradients are also smaller, since at our low speeds large temperature gradients are not needed to obtain the convection motions we are trying to model. In the diffusion experiments I described earlier, we used floor roughness as a means of controlling the velocity profile. The roughness plates are adjusted to give the desired

profile at the beginning of the test section. By adjustment of floor roughness the lower part of the profile which envelops the plume can be made substantially constant with distance.

McCORMICK: He offered the tentative hypothesis that in the case of a smooth boundary the diffusion was by buoyant bubbles or cells, but the roughness would break up discrete cells, so it was less apparent.

STROM: Dr. Cermak's group has conducted fundamental studies on heated and cooled boundary layers and may have been looking for characteristics of this type.

McCORMICK: He reported this orally several years ago and to my knowledge hasn't published it.

STROM: They are in the process of building a new laboratory out there.

GIFFORD: I don't believe you mentioned the time of averaging that you used in making the photographs.

STROM: In the prototype 16 exposures were made at 15-sec intervals to give a total elapsed time of 4 min. For the model the same number of exposures was made but at 5-sec intervals, which is the shortest we could obtain with the electronic flash equipment we were using. I believe that if all the exposures were taken in a few seconds we would find substantially the same results.

GIFFORD: We find - I think many people have found - that in taking time-exposure photographs of plumes, as the time over which exposures are taken is increased, of course, the appearance of the plume smooths out. This simply means that at the point where the plume looks quite smooth and doesn't wiggle except in response perhaps to gross changes in the wind direction, the length of time involved will be characteristic in some sense of the turbulent fluctuations. I was curious as to what exposure time periods are required to reach this invariant status in the atmosphere and in the wind tunnel. You say in the wind tunnel it is perhaps a few seconds. We find at Brookhaven that under type A or type B conditions it is several minutes at least.

STROM: The confinement of the airstream by the walls of the wind tunnel test section prevents the formation of long-period fluctuations. I don't see how turbulence can be produced with wavelengths substantially larger than a cross section dimension or length of the test section.

GIFFORD: No. My point is that time-exposure photography is a good way to determine this characteristic time scale.

STROM: The 4-min period for the prototype photographs is longer than necessary to cover the range of fluctuations obtained in the wind tunnel but was needed to get a good plume outline. Perhaps at a future date someone will develop methods for obtaining the longer-period effects in the model by artificial means or by a computation which superposes results for a range of directions and speeds.

The Oak Ridge Meteorological Program

WILLIAM F. HILSMEIER

The meteorology facility at Oak Ridge is under the administration of the U.S. Weather Bureau. The meteorological activities are in the same general areas as at the other facilities represented at this meeting. We also have conducted, on a limited basis, diffusion experiments. These were done in August and September of 1963 using uranine dye as the tracer. We were fortunate in having the Experimental Gas Cooled Reactor facilities, located in Melton Valley, Oak Ridge, as a base of operation from which to release the dye. Alongside the reactor containment shell rises a 200-ft stack equipped with outside stairs. We were able to release 160 g of dye at a height approximately halfway up the containment shell or 70 ft above the ground.

Some interesting results, which have not been published and perhaps will not be published until further verified, were obtained from this particular experiment. It was noted that the Halitsky model for diffusion from the reactor shell did not apply in this case. The deficiency was that the K , or the distribution function, values which we determined experimentally in the wind tunnel did not apply to the whole complex of buildings near our sample points, which were located a few hundred meters from the buildings and also from the source. Another interesting point, diffusion calculations using the Pasquill curves of σ_y and σ_z and the Gaussian formula worked very well out to a distance of ≈ 3700 m. However, samples taken at 5000 m revealed a higher concentration than predicted. Perhaps the Valley was restricting the plume diffusion in the horizontal as the plume traveled down the Valley. This has not been further verified, but it is the only explanation we can offer at this time. The uranine dye was dispersed in the usual manner, with the exception that the dispersal nozzle was a paint spray gun of the type used for very fine touch-up, almost like an artist's air brush. We were able to reduce the deposition rate of the dye to 6 cm/sec, as nearly as we could determine.

I would like to describe briefly our climatological network in Oak Ridge. Some stations have been maintained since 1949. Since 1950 we have had a minimum of 5 stations on our telemetering system and a maximum of 9. The network of stations has standardized more or less to 7 sites covering 59,000 acres. The oldest stations in the area are the Oak Ridge Weather Bureau office in the town itself, sometimes known as Public Works and Townsite in the early publications and data, and X-10, now ORNL, Oak Ridge National Laboratory. These stations were installed in January 1949 and have been operated continuously. All stations provide temperature, wind velocity, wind direction, and dew point data automatically. The Townsite station also provides precipitation readings automatically, while the other 6 stations have weighing rain gages for obtaining precipitation data. All stations except Townsite provide temperature gradient readings. A new limited data station is contemplated for the network. It will be at the Experimental Gas Cooled Reactor. Wind equipment has been installed and will supplement a two-stage temperature gradient sensor. For historical and length-of-record advantages plus the advantage of a wide-area coverage for climatological purposes, I invite you to use our data, which have been published in ORO-99 and its revision and supplement, ORO-199.

Work in instrumentation has led to the development of a wind statistics system, known as the Theta-N system. Mr. Culkowsky described the system, which uses an Optisyn* shaft angle encoder, at the 8th AEC Air Cleaning Conference in October 1963. Briefly, this is an optical system employing a moiré fringe of light and dark sectors on a circle which send out sine waves as the shaft rotates. The logic system determines whether the rotation is clockwise or counterclockwise and shapes the resulting output into square waves on

*Optisyn Miniature Optical Shaft Encoder, Dynamics Research Corporation, 38 Montrale Avenue, Stoneham, Massachusetts.

the appropriate clockwise or counterclockwise channel. The square waves or pulses are operated on by simple sum and difference circuits. The big advantage is that, whereas other wind statistics systems observe the direction first and then obtain the differential value, the shaft encoder system detects the differential first, permitting other parameters, such as direction, to be obtained by integration.

The Theta-N concept, which was worked out by R.A. McCormick of Cincinnati, is based on

$$M = \frac{1}{N} \sum \Delta \theta$$

where M is mean wind vane travel per oscillation, $\Delta \theta$ is the degrees traveled per unit time, and N is the number of oscillations per unit time. The standard deviation of a Gaussian distribution is related to the modulus mean, M , by

$$\sigma = 0.3M.$$

From this very simple concept we are able to get the extent of deviation, with which many of us like to work directly.

An improved and less expensive Theta-N system has been developed at Oak Ridge which uses an Incrosyn* shaft increment encoder. Whereas Optisyn is an optical encoder, Incrosyn is a magnetic encoder. The Incrosyn generates 1024 equally spaced pulses in a single turn of the input shaft. By operating logically on the output of the two read-out heads and accumulating these pulses, both angular travel and direction of rotation can be determined. For bidirectional operation, two output lines are provided to read out clockwise and counterclockwise pulses separately. The logic circuit reduces the 1024 pulses to 360, giving a very high resolution. The theta values and the N counters reduce the counts to pulses or steps which for our purposes are recorded on a strip chart. The standard deviation for a given period of time is found by dividing θ by N . This factor of 0.3 is reduced to 0.6 because we count both left and right reversals. Therefore we use a 0.6 modulus mean for determining σ . Literature on the Incrosyn encoder is available.

Finally, I would like to discuss the Nuclear Safety Information Center. I hope that everyone

has heard of the Center by now, but it is difficult in the early stages to get people to use such centers, so I am here partially as a salesman. The primary way the Center can be of benefit is through use. The Nuclear Safety Information Center is located at the Y-12 Section, which is very convenient to Oak Ridge. It is open to everyone. Brochures describing it are available. Briefly, the functions of the Center are as follows.

It selects information pertinent to the areas of interest from many diverse sources. The areas of interest include:

Containment of nuclear facilities.

Fission-product release, transport, and removal.

Meteorological considerations.

Nuclear instrumentation, control, and safety systems.

Radioactive effluent control, monitoring, movement, and dosage.

Reactor transients, kinetics, and stability.

This Center was an outgrowth of the *Nuclear Safety Journal*. The Center stores the pertinent information so that it is rapidly retrievable. It differs from other centers in that you can actually go into the Center, look up a word or a subject, and pick out the card with the particular subject that you are interested in. You can read briefly about that subject, and if you wish to pursue the references further, they are available at the Center.

The staff also publishes studies for the nuclear community, and is available to answer questions on a time-permitted basis for anyone who visits the Center. A *Quarterly Progress Review* is published.

In working at the Center, I have noticed that many writers in their abstracts just say what the paper is about. An abstract should do more than that. We rely a great deal on abstracts for putting information together at the Center. Of course we go through the entire paper using the conclusions to advantage. But the abstracts, which are the first approach in processing the backlog of reports, will be more useful, the more information they contain.

DISCUSSION

WEBSTER: In the comparison of measured and calculated concentration at 5000 m, what were the stability conditions? Were they unstable?

HILSMEIER: Yes. The condition was Pasquill's type B.

PACK: Will the Nuclear Safety Information Center answer inquiries by mail?

HILSMEIER: Yes.

*Incrosyn Increment Encoder, Data Tech, 127 College Hill Road, Watertown, Massachusetts, 02172.

Continuous Point Source Diffusion in the Surface Layer

FRANK A. GIFFORD, JR.

For about the last 40 years, theories of atmospheric diffusion have developed along two distinct lines: (a) *G.I. Taylor's* well-known theory of diffusion by continuous movements, which was developed into a practical, working tool by Sutton, Frenkiel, and many others culminating with Pasquill's recent work; and (b) the gradient-transport theory, or *K-theory*; which is based on the supposition that the transport of a property is proportional to its space-gradient, the coefficient of proportionality being the eddy diffusivity, *K*. Among the many names associated with this approach must be mentioned O.F.T. Roberts and K. Calder.

Because of both intrinsic interest and the requirements of practical applications to various air pollution and weapons problems, a tremendous amount of development of these two theories, for the particular case of a continuous point source located at or near the earth's surface, has been attempted. Despite these widespread and in many cases rather successful applications, however, it has long been recognized that, as a purely theoretical matter, neither of these two models can possibly be expected to apply strictly in the case to which they are usually applied; namely, to the case of continuous point source diffusion in the atmospheric surface layer. The reasons for this are the following.

Taylor's theory assumes homogeneous turbulence, and therefore does not apply to diffusion in a turbulent shear zone such as the atmospheric surface, or constant stress layer. The *K-theory*, on the other hand, assumes that diffusion is controlled by an eddy diffusivity that, to account for shear, can vary with height. This height variability is usually assumed to follow some specified function, for example a power law; but since the height variability of eddy-diffusivity is a physical consequence of vertical momentum diffusion, this amounts to building the result into the theory. Moreover there is no *a priori* reason (other than limited agreement with data), as various writers including, for example, Priestley¹⁰ have remarked, to expect the simple gradient-transport hypothesis to apply at all in the atmosphere. Corrsin has aptly summed up the position on the *K-theory* by

pointing out that this method is not useful in principle, but only in practice.

Largely for these reasons, a remark made by Batchelor¹ at the 1958 Oxford symposium on atmospheric turbulence and diffusion, to the effect that the hypothesis of Lagrangian similarity should provide an approach to the problem of atmospheric surface layer diffusion, seemed to me certain to provoke a tremendous amount of interest. New theoretical approaches to this difficult problem do not grow on trees, and the practical diffusion problems are still very much with us. It cannot however be said that events have proved the correctness of this opinion. Batchelor² and independently Ellison³ applied the Lagrangian similarity idea to the case of continuous, point source diffusion in the adiabatic surface layer, the former in an unpublished note and the latter in the midst of a review article. Gifford⁴ attempted to extend this work to the diabatic case and obtained good agreement with data, and more recently Cermak⁵ has applied the foregoing results to diffusion in a variable-density wind tunnel boundary layer. At the moment the problem rests there.

Mr. Calder recently called my attention, in a private communication, to a weak point in the argument for the diabatic case; and the object of this paper is to present an alternative derivation which avoids the difficulty. At the same time, I hope to bring this new approach to an old problem to the attention of some of you who may heretofore have overlooked it.

The idea of Lagrangian similarity follows naturally from that of Eulerian similarity which has been applied to the problem of the wind distribution in the surface layer by Monin and Obukhov⁹ and later by many other writers; and to the problem of the shape of smoke plumes by Kazansky and Monin⁷ and by Kao.⁶ According to the hypothesis of Eulerian similarity, the wind profile in the surface layer, for example, is determined entirely by the roughness length, z_0 , the friction velocity, v_* , and the Lettau-Obukhov stability length, L , a parameter that incorporates the heat flux, and plays a part similar to that of

the more familiar Richardson's number. If the flow properties in the surface layer are everywhere similar on the average, differing in their statistical properties from point to point only in the scales of length and velocity, then it follows from ordinary principles of dimensional analysis that

$$F_1(\bar{u}, z, z_0, v_*, L) = 0. \quad (1)$$

From these variables, three dimensionless ratios, i.e., the number of quantities (5) minus the number of dimensions (2), can be formed:

$$F_1(\bar{u}/v_*, z/L, z_0/L) = 0. \quad (2)$$

Solving for the dimensionless variable containing $\bar{u}$, we obtain

$$\frac{\bar{u}}{v_*} = F_2\left(\frac{z}{L}, \frac{z_0}{L}\right). \quad (3)$$

We know, from other reasoning, that in the adiabatic case ($L = \infty$),

$$\frac{\bar{u}}{v_*} = \frac{1}{k} \ln \frac{z}{z_0}, \quad (4)$$

where k is a universal constant, von Kármán's constant. Therefore it is reasonable to write Equation (3) in the form

$$\frac{\bar{u}}{v_*} = \frac{1}{k} \left[f\left(\frac{z}{L}\right) - f\left(\frac{z_0}{L}\right) \right] \quad (5)$$

where f is a universal function, since the diabatic wind profile must satisfy a similar boundary condition.

Now consider the Lagrangian problem, for which particles are emitted from the coordinate origin. Let the mean position of a particle be $\bar{x}(t)$, $\bar{y}(t)$, $\bar{z}(t)$. (If we choose the downwind direction to coincide with $\bar{x}$, then $\bar{y} \equiv 0$.) It is reasonable to suppose that

$$d\bar{x}/dt = \bar{u}(\bar{z}), \quad (6)$$

i.e., that at any point the particle moves horizontally with the average wind speed. Following a similar dimensional line of reasoning, we can also conclude that the mean vertical velocity of a particle, $\bar{w}$, will be given by

$$\frac{d\bar{z}}{dt} = \bar{w} = bv_*\phi\left(\frac{z}{L}\right). \quad (7)$$

The universal functions f and ϕ are expected to coincide with those having the same symbols that were discussed by Kazansky and Monin.⁷

Now let us apply dimensional reasoning to the problem of determining the probability that a

particle will reach some distance $r = (x^2 + y^2 + z^2)^{1/2}$ from the particle mean position, $(\bar{x}, 0, \bar{z})$. This is the same as inquiring what is the average concentration distribution, χ_i , due to an instantaneous point release. If Q_i is the instantaneous source strength, it is seen that the relevant variables are: the displacements, $x - \bar{x}$, $y - \bar{y} = y$, $z - \bar{z}$; the parameters characterizing the turbulence, v_* , z_0 , and L ; and time, t , χ_i , and Q_i .

Since the function of z_0 is only, as Ellison has pointed out, to translate the entire flow bodily by a small amount, z_0 can be deleted from the above list. For the case of the axial concentration, $y = z = 0$ also. From the remaining variables we can form the following:

$$F_3\left(\frac{\chi_i \bar{z}^3}{Q_i}, \frac{x - \bar{x}}{\bar{z}}, \frac{\bar{z}}{L}, \frac{v_* t}{L}\right) = 0. \quad (8)$$

Solving for the ratio containing χ_i , we find

$$\frac{\chi_i}{Q_i} = \frac{1}{\bar{z}^3} F_4\left(\frac{x - \bar{x}}{\bar{z}}, \frac{\bar{z}}{L}, \frac{v_* t}{L}\right). \quad (9)$$

To find the continuous point source concentration, following the ordinary practice, this equation would have to be integrated w.r.t. time, from 0 to ∞ . Since all the remaining dimensionless ratios are functions of time, this presents quite a problem.

In the adiabatic case, for which $L = \infty$ and $\phi = 1$, the integration is readily performed after certain changes of the integration variable have been made. In the diabatic case, a similar procedure will work except that it must be assumed that F_4 is not a strong function of the ratios $\bar{z}/L$ and $v_* t/L$, since these can no longer be expected to disappear so conveniently.

The writer has been motivated by correspondence with Mr. Calder about this point to propose the following alternative procedure, which avoids the difficulty. The approach is to consider a different, and at first seemingly more complicated, problem. Let us define a time interval, τ , as the time required for a particle to travel from the origin of coordinates, $(0, 0, 0)$, to any mean position $(\bar{x}, 0, \bar{z})$. In other words, τ is the particular value of t that corresponds to the average travel time, or dispersion time of a particle. Then, clearly, $\bar{x} = \bar{x}(\tau)$ and $\bar{z} = \bar{z}(\tau)$. As before, let t represent real time, and consider a receptor located at point $(\bar{x}, 0, 0)$, downwind from a source. Let $Q(t)$ be the strength of a finite, *time-dependent* source, such that $Q(0) = 0$, and $Q(t) = \text{constant}$, $t > 0$. In other words, let a uniform source, of strength Q units/sec,

be started at time $t=0$. What is the resulting finite-source concentration distribution, $\chi(x, y, z, t)$? This will clearly be a function of: the displacements, $x-\bar{x}$, y , and $z-\bar{z}$; the characteristic turbulence parameters, v_* and L (we are, as before, ignoring dependence on z_0); and also of τ , t , and Q . In addition, it appears that the particle concentration at any point will depend on the mean horizontal particle flux there, and hence, on the mean wind speed, $\bar{u}(\bar{z})$. (We provisionally will ignore, by comparison, the corresponding vertical flux due to $\bar{w}$). Tallying up the parameters and variables involved, it is seen that two additional ones have been introduced in order to set up this new problem; namely, $\bar{u}$ and τ . Even for the axial, ground-concentration case, where $y=z=0$, the variables involved in this problem are nine in number: χ , Q , $x-\bar{x}$, $\bar{z}$, L , v_* , t , τ , $\bar{u}$. Since there are three basic dimensions, we seek to equate to zero an unknown function of six dimensionless products. Comparing this with the four that were involved in the preceding problem, it is not yet clear that any real progress has been made.

Evidently τ/t and $v_*/\bar{u}$ are two of the required dimensionless products, and we may also select $(x-\bar{x})/\bar{z}$ and L/v_*t , based on experience with the original problem. A little additional experimentation provides the other products and verifies that we may write

$$F_5\left\{\frac{\chi\bar{u}\bar{z}^2}{Q(t)}, \frac{x-\bar{x}}{\bar{z}}, \frac{v_*}{\bar{u}}, \frac{L}{v_*t}, \frac{\bar{z}}{v_*t}, \frac{\tau}{t}\right\}=0 \quad (10)$$

as the unknown function. The six arguments of F_5 are independent, i.e., no single product can be formed by multiplying together powers of any of the others. Therefore, provided we have included all the relevant dimensional quantities of the problem, the above set of six products is a complete one. This may be proved formally, by the standard matrix method, but is equally evident on inspection.

Now let us determine the steady-state, downwind, continuous point source concentration, χ_{cps} , by letting t become very large. We may write

$$\frac{\chi_{cps}}{Q} = \lim_{t \rightarrow \infty} (\bar{u}\bar{z}^2)^{-1} F_5\left\{\frac{x-\bar{x}}{\bar{z}}, \frac{v_*}{\bar{u}}, \frac{L}{v_*t}, \frac{\bar{z}}{v_*t}, \frac{\tau}{t}\right\} \quad (11)$$

or, in the limit,

$$\frac{\chi_{cps}}{Q} = (\bar{u}\bar{z}^2)^{-1} F_7\left\{\frac{x-\bar{x}}{\bar{z}}, \frac{v_*}{\bar{u}}\right\}, \quad (12)$$

Q now being a constant. At a sufficient distance from the source, $\bar{u} \gg v_*$; under these same condi-

Figure 1. Dimensionless axial concentration values $\chi v_* L^2 / Q k$ plotted as a function of dimensionless downwind distance $|\xi| = kb\bar{x}/L$ for the case of unstable conditions.

Figure 2. Dimensionless axial concentration values $\chi v_* L^2 / Q k$ plotted as a function of dimensionless downwind distance $\xi = kb\bar{x}/L$, for the case of stable conditions.

tions we may also imagine the probability distribution associated with the displacement of the particle from its mean position to be swept very rapidly past the downwind point, so that $x \approx \bar{x}$. Since this assumption also has to be made in integrating χ_{cps} , it is not a new element in the demonstration. The upshot is that

$$\chi_{cps}/Q = (\bar{u}\bar{z}^2)^{-1} F_7(0), \quad (13)$$

i.e.,

$$\chi_{cps}/Q \propto (\bar{u}\bar{z}^2)^{-1} \quad (14)$$

which is the desired result.

Comparison of Observed Point Source Concentration Power Law Index m With Theoretically Derived Values

Stability	ζ_0	$\bar{x}$, m	m , obs.	m , calc.
Very stable	+0.1	100-200	-1.3	-1.3
		400-800	-1.0	-1.3
Moderately stable	+0.01	50-100	-1.6	-1.6
		400-800	-1.4	-1.6
Near neutral	± 0.001	50-100	-1.8	-1.6
		400-800	-1.8	-2.0
Moderately unstable	-0.01	50-100	-2.0	-2.2
		400-800	-2.6	-2.8
Very unstable	-0.1	50-100	-2.5	-2.8
		400-800	-3.3	-3.0

Since $\bar{u}$ and $\bar{z}$ can, even in the diabatic case, be obtained from Equations (5), (6), and (7), as has been described previously,⁵ the numerical results of that paper follow equally from the present one. They are summarized for representative values of the parameters in Figures 1 and 2 and in Table 1. The latter, a comparison of the downwind concentration distribution predicted by Equation (14) with the Prairie Grass diffusion data as analyzed by Cramer,¹¹ shows good agreement. In particular, the prediction of an increasing negative value of the exponent, m , of the curve describing ground-level concentration as a function of distance downwind in unstable atmospheric conditions is verified.

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DISCUSSION

PACK: Mr. Hilsmeier mentioned the K 's (Halitsky's model). Were they larger or smaller than in these theories?

HILSMEIER: The K 's were larger.

PACK: As expected.

BADGLEY: It worries me in looking at your dimensional analysis that by the removal of one dimensional parameter, namely t , you can strike out three dimensionless quantities, whereas I would think that by a strict application of dimensional analysis you could remove only one. Perhaps there is some physical reason behind the strike-offs. It doesn't appear to be right.

GIFFORD: You are hitting at the root of the problem of approaching any complex phenomenon by dimensional analysis and in particular by a dimensional analysis unaided by other hypotheses, aside from the one concerning L . We don't have a differential equation by which we can approach this. The thing is that t is a very convenient parameter in that it does permit simplification of this very complicated problem.

I can only rationalize by saying we are seeking an answer which is independent of t . Clearly, the steady state is independent of t . The answer must also have the property that it reduces to the known result for the adiabatic case. That is the only other principle that I can invoke, and my solution has that property. I feel that the dimensional approach must be used with a strong dose of intuition, and certainly one must give a lot of thought to a principle that will permit justification of what one has found. Dimensional analysis is not something that will work all by itself. It needs physical intuition and also, if they are available, other mathematical facts to use. And when there are 9 parameters to begin with, one does not lightly ignore the possibility of eliminating 3 of them.

HOLLAND: You mentioned getting some information on the right-hand side of your last proportionality from what is available on the structure of the boundary layers. I can see getting the $\bar{u}$ information this way, but where do you get the information about $\bar{z}$?

GIFFORD: $d\bar{x}/dt$ is proportional to v_* , for which one can write an expression involving a universal function, $F[(\bar{z}/L) - (z_0/L)]$. Similarly, for $d\bar{z}/dt$ one can write an expression proportional to w_* , which is similar to v_* but is related to vertical motion. (Incidentally, this is a characteristic difference from the Lagrangian approach, in which there is a positive mean vertical motion of the particles involving another function, $\phi[\bar{z}(L-1)]$.) Dividing the first of these equations by the second gives an equation for $d\bar{x}/d\bar{z}$ that can be integrated numerically.

The Meteorology Program at the Hanford Atomic Products Operation

JAMES J. FUQUAY

At a meeting at Hanford in June 1960 we reported on the tracer technology we were trying to calibrate, but we had no specific results yet. Since then we have completed calibration of the system and have conducted more than 100 diffusion experiments over the extensive horizontal and vertical sampling grids at Hanford. In addition we have conducted or participated in a total of 185 experiments in cooperation with the Air Force at Cape Kennedy, Florida, and Vandenberg Air Force Base, California. The data and the description of the techniques of these latter experiments were documented jointly with personnel of the Air Force Cambridge Research Laboratories. The Hanford report is *The Ocean Breeze and Dry Gulch Diffusion Programs*, HW-67435, July 13, 1963.

Since the Hanford experiments have been reported at meetings, and a paper entitled "Prediction of Environmental Exposures From Sources Near the Ground, Based on Hanford Experimental Data," will soon appear in the *Journal of Applied*

Meteorology, I will not review them in detail. The values of peak exposure and the standard deviations are presented in the *Journal* paper for 46 experiments with a ground source, covering the broad range of atmospheric stability from very unstable to very stable. The exposure data were found to order very well with the atmospheric stability when plotted as a function of travel time. The cross-wind variance of the exposure distributions was also expressed in terms of travel time and was plotted for various values of the product of the standard deviation of wind direction fluctuations and the wind speed at release height $\sigma_\theta \bar{u}$, as suggested in Taylor's work in 1921 (see Figure 1).

Prediction methods developed from these concepts permit extrapolation of the results obtained from short releases to much longer release periods. We found good agreement between predicted and observed exposure distributions in a test of these models, using independent data. These developments have also been applied in the evaluation of the consequences of reactor accidents, where the period of release is particularly protracted, as in the case of containment or confinement leakage.

At present the major emphasis in the Hanford program is on diffusion from elevated sources, which Mr. Elderkin will discuss. Dr. Engelmann will summarize some of his precipitation scavenging work.

To continue with our over-all program activity, we have prepared a comparison of the results of major U.S. field programs using sources near ground level conducted over the past 10 years, for which we could get data. This comparison is published in the *Proceedings of the Eighth Air Cleaning Conference, Oak Ridge, October 1963*. These results embrace 340 diffusion experiments utilizing ground sources encompassing a wide range of atmospheric stabilities and a variety of tracer materials. The peak exposure normalized for wind speed and source strength, when plotted against travel time, shows a stratification, when stability and growth parameters were factored into the analysis. Differ-

Figure 1. Standard deviation of plume width as a function of time and wind variability.

ences in results among sites were greatly reduced when diffusion was considered time-dependent rather than being plotted as distance-dependent.

In our work on tracer technology, we have been trying to develop multitracer - or multicolor - systems, using two colors of zinc sulfide in combination with other tracers such as fluorescein, etc. So far the fluorescein and zinc sulfide combinations seem to work well together, but our efforts to discriminate between colors of zinc sulfide have not shown the desired precision. We have also tried to increase the release rate of the zinc sulfide by using trichloroethane as a carrier in place of water. This permits the extension of measurements to larger distances - up to 20 miles - during unstable conditions from elevated source releases. For sampling at these larger distances, we are equipping a light aircraft with bulk samplers and with a so-called real-time sampler, which measures the nearly instantaneous value of the zinc sulfide concentration as a function of time of travel past the sampler, and also integrates the signal to provide the total dosage.

In addition, we have made one release of I^{131} , again over the extensive vertical-horizontal sample grids with special sampling equipment. Mr. Simpson will give further details on that work. We plan

to make several releases with I^{131} in order to study the dispersion and deposition of the various forms of iodine. We will also cooperate with the biologist at Hanford in contaminating a pasture area, grazing cows on it, and determining the I^{131} in the milk.

DISCUSSION

SMITH: I would like to straighten out one bit of terminology. In your study of the ground-level sources, when you use the term "peak exposure," don't you mean the centerline exposure rather than the peak during some very brief time?

FUQUAY: I mean centerline exposure actually at the sampler height, which in the horizontal system is at $1\frac{1}{2}$ m, and this does not mean the peak in the total plume distribution.

SMITH: I wanted to clarify that because other people have used "peak" to mean that little top of the curve that occurs for a few seconds.

FUQUAY: You mean in the plume itself?

SMITH: Yes.

HOLLAND: Is your travel time any different from distance divided by $\bar{u}$?

FUQUAY: This has been a real puzzle to us.

HOLLAND: How do you actually determine travel time in ordering the data? You say using it reduces the scatter as compared with using the distance. How do you actually get a travel time in the experimental setup?

FUQUAY: By using the mean wind speed and distance, which is essentially a direct conversion.

Diffusion From Elevated Sources

CHARLES E. ELDERKIN

Since 1962 we have been performing diffusion experiments from elevated continuous point sources, with the releases made from 56 m on our meteorology tower at Hanford, mostly for a period of 1 hr but ranging down to $\frac{1}{4}$ hr. The diffusion grid is a surface grid only, consisting of 9 sampling arcs at distances of 100 to 1600 m from the meteorology tower and concentric about it. The arcs extend over 120° to 140° of azimuth, and the sampler spacing is 4° , 3° , and 2° on the arcs. Recent expansion of the sampling course includes an arc at 3200 m and an additional 3 arcs within the 1600-m distance. At each of the sampling positions, the time-integrated concentration – the exposure, as we call it – is measured.

The results to be shown are from the 8 best tests during 1962. Analysis of the data from a second series of similar tests conducted in 1963 is under way, and the results are in general agreement with the 1962 results. The data seem to be well organized as a function of travel time in spite of the inhomogeneities in the atmosphere, and are so presented in the figures.

Figure 1 shows σ_y , the standard deviation of the plume exposure distribution measured at the surface, versus travel time calculated from the mean wind speed at 200 ft at the point of release. Generally speaking, the curves show a power function relationship with time, with the striking exception of test No. 10, which deviates strongly from a power function. No stability dependence was noted among this small group of σ_y versus t curves, nor was any stability dependence shown in a plot of σ_y versus distance. However, the individual runs do separate according to the $\overline{v'^2}$ or the $\sigma_y \bar{u}$ used to estimate $\overline{v'^2}$.

The Hay-Pasquill technique for estimating the lateral spreading of the plume, using the running average of the wind measurements, gives good agreement with the measured results. For these 8 tests the agreement with the measured values was within 20%, except for test No. 17, which involved a very short release. The length of the wind data was short enough that I think the very low periods

which dominate the plume growth were not sampled adequately.

Figure 2 shows the peak exposures normalized with the source strength and the wind speed at 200 ft, again plotted against travel time. There is general agreement in the occurrence of the maximum values at around 80 sec travel time. The peak values range over a factor of about ± 2 from the midvalue. The results of the second series of tests are fairly well contained within the envelope of these, with the maximum values again at about 80 sec travel time and distributed over a factor of ± 2 in magnitude. The slope of the centerline values beyond the maximum point is about -2 to -2.5 on this log-log plot. The curves in this time-dependence plot show no clear-cut dependence upon stability. We hope eventually to measure the vertical fluctuations of the wind at the source and possibly to relate the time of occurrence of the maximum to the vertical turbulence.

Figure 3 shows the normalized crosswind-integrated exposure versus the travel time. Here the maximum values occur at around 100 sec travel time. The second series of test results (not shown) also are grouped around the same general arc as shown by these curves. The maximum values of normalized crosswind-integrated exposure also range over a factor of about ± 2 .

DISCUSSION

TURNER: When you analyzed wind direction traces according to the Hay-Pasquill technique, they agreed quite well with the plume distribution in the horizontal. What value of β did you use to get this agreement?

ELDERKIN: One. Actually we didn't use the wind direction only. We used the v' components, calculated from the 20-sec averages of wind direction and speed at the source. The slope was about 0.9 for the log-log plot of σ_y versus time, for both the observed and the calculated values.

SMITH: You said the data showed no dependence on stability. Could you give some indication of the range of conditions under which the tests were made?

ELDERKIN: The tests were performed in neutral to strongly unstable conditions only; no stable conditions were included. Tests of the latter sort will be performed later, and I suppose stability dependence will be noted.

Figure 1. Standard deviation of cross wind exposure distributions versus time for elevated source releases.

Figure 2. Normalized peak exposure, $E_p u / Q_u$, versus time for elevated source releases.

Figure 3. Normalized crosswind-integrated exposure versus time for elevated source releases.

Precipitation Scavenging

ROBERT J. ENGELMANN

The problem of precipitation scavenging is divided grossly into two portions: the repeated scavenging of material within the cloud as the cloud evaporates and re-forms, and the scavenging or washout that occurs beneath the cloud. The latter, since it is closer to the ground and can be sampled with ground sources, is more amenable to study. This is the research that we have been doing at Hanford.

Concerning the washout by a raindrop falling through a plume, the historical work on this begins with Albrecht and Sell, who either measured the flow about cylinders, spheres or traps, or calculated it from potential and viscous flow patterns. Using these flow patterns and the nonlinear equation of motion of a particle within this flow as it attempts to go around the obstacle, successive increments of position were calculated. These trajectories were used to determine the target efficiencies of the particles on these obstacles. We understand that target efficiencies are computed, and they are not necessarily the same as the observed efficiencies of the obstacles.

In the forties, Langmuir and Blodgett were facing the problem of predicting icing on aircraft, and they had available an early form of differential analyzer which they used to calculate more accurate trajectories for viscous and potential flows. Langmuir later introduced an interpolation formula between viscous and potential flow for intermediate raindrop sizes. These efficiencies of Langmuir and Blodgett are what are currently used in scavenging predictions. Somewhat more accurate target efficiencies have since been computed by others, such as Fonda and Herne, but they are substantially the same as those of Langmuir and Blodgett. This nonlinear equation of the path of a particle around an obstacle is complicated further by the introduction of electrical forces. Cochet in France calculated the target efficiencies for a single charge on a raindrop, a rather weak charge with neutral cloud droplets. Kraemer and Johnstone left out the inertia terms and considered only

electrical effects in their calculations. Of course these are incomplete for application in the atmosphere.

Laboratory experiments have been performed on scavenging, utilizing both cylinders and spheres. These generally confirm predicted target efficiencies, but there are exceptions, such as those of Kinzer and Cobb in 1956 and 1958, in which a maximum efficiency was observed with a sphere or raindrop of about 0.5-mm diameter actually falling through a water cloud in the laboratory rather than about 1.2 mm as predicted by Langmuir's formula.

Deviations from predictions were also observed by McCully et al. in more elaborate experiments on the West Coast. In a series of something like 20 experiments chosen from a much larger number, with two tracers, each in a different set of experiments, they found that the zinc sulfide 2210 scavenging efficiency was about 0.2, and the efficiency of 2330 was on the order of 1.5, considerably more than 100%. The differences were explained by McCully, after some laboratory work on the bounce-off of glass beads from droplets suspended on wires, as being due to differences in wettability. (The target efficiency is multiplied by the retention to get the scavenging efficiency.) Pemberton went on in theoretical work to show that there should be a difference in the scavenging between wettable and nonwettable particles. Lately Dr. Rerg has shown that charges on particles may be very influential in terms of wettability, by showing with high-speed photography that the coalescence time of water droplets is a function of potential differences. (However, these times are very small, and it is debatable whether the coalescence rate really makes much difference.)

Therefore, the subject is very open, and field experiments are needed. There have been very few. May did an experiment in an actual rain, using radioactive spores over very short distances. He may have had some confusion with dry deposition. He obtained washout coefficients that in gen-

eral agreed with those he computed by using Langmuir's predictions and rain spectra summarized by Best. However, May's experiment would have been more valuable had rain spectra been observed at the same time.

At Hanford we are also doing some outdoor experiments in scavenging, using one of our diffusion towers at 200 m distance. This tower is actually an 88-ft-high pole, fitted with filters used to determine the concentration of zinc sulfide particles in a plume going past it. At the top of the pole is a standard lawn sprinkler to provide artificial rain, which falls through the plume to special rain samplers below. From the samplers we can get the sizes of individual raindrops and their individual

collections, which we can later view through a microscope.

Figure 1 (left) shows a sample of our sensitive paper. This is actually a blueprint paper but is also water-sensitive. It shows spots which we have calibrated to raindrop size. If we magnify one spot, we see something like the picture in the upper right of Figure 1, which is from a drop that originally had about a 1-mm-equivalent diameter. In the center are some green zinc sulfide particles that can be individually counted and sized, and outside the drop is some of the dry deposition that we subtract out. The result of the several experiments that we ran is shown in the lower right. The theoretical limits according to Langmuir and Blodgett are indicated by the shaded area, and the measured efficiencies for three drop sizes are shown by the three curves.

From these curves, the 0.4-mm drop appears to be the best scavenger. No inconsistencies with this

Figure 1. Measured raindrop scavenging efficiencies for zinc sulfide particles, compared with Langmuir's predictions. Also shown is an example of the rain samples collected in the field tests.

result were observed in any of our other experiments. The smaller drop was always found to be a better scavenger. The larger drop is also found to be better than the intermediate size. The scavenging efficiency for small particles is on the order of 0.2, as found by McCully, and we also find efficiencies well in excess of 1, such as he found for the larger 2330 particles. Each curve applies to 2210 tracer, or a combination of both 2210 and 2330, and we, therefore, find it difficult to explain these curves on the basis of wettability differences.

There is some risk that the curves for the larger particle size have been elevated because of anisokinetic error of our filters. Duct work is being done at Hanford to determine the efficiencies of our filters at different air speeds, and preliminary data from this work were used to correct the scavenging efficiencies; this slide should have been corrected for anisokinetic error. The duct work is presenting difficulties and is still in progress.

Another complication is the uncertainty of the charge on the raindrops. We can calculate as well as we are able the charge on these artificial raindrops and show that it would not influence greatly the target efficiency on the leading side of the

drop. However, we have a theory that particles that just missed the drop could be entrained in its wake, and here weak electrical charges might have a significant effect. Perhaps the 1.5 efficiency found by McCully could express the electrical influence in the West Coast rains. This leads to study of natural rains, which we would like to do this winter, to determine natural electrical charges. We hope to obtain equipment to measure the charges on raindrops similar to that used in 1949 by Gunn.

It is also possible to design an experiment that does not require the measurement of concentration of the plume in the air or of the source strength. It does have the stipulation that the plume not contact the ground, and for this reason we look forward eagerly to the arrival of our aircraft. We can determine the washout by such an experiment over distances of, say, 4 to 40 miles. Also, we could study snow, which has been largely overlooked in the research; it presents considerable complications but might be very important. We require for these experiments a rather large volume source, and these are not easily obtained with dry dusters. (We have had great difficulties with this in the past years.)

Figure D-1. Normalized peak exposure, $E_p \bar{u} / Q_t$, versus distance for diffusion tests with differing conditions of atmospheric stability.

Figure D-2. Normalized peak exposure, $E_p \bar{u} / Q_t$, versus time for diffusion tests with differing conditions of atmospheric stability.

As Mr. Fuquay mentioned, we are working with a trichloroethane suspension of the zinc sulfide, which gives promise. We are sampling dry deposition on the ground with the Diazo paper (shown on the left of Figure 1), which is very versatile. We have found it to be among the best means of measuring raindrop sizes, provided the wind is not too strong. This summer we are reducing rain spectra gathered at Hanford to a form usable for sampling predictions.

DISCUSSION

CHAMBERLAIN: In the calculation of the raindrop size, do you correct for the evaporation factor in the fall?

ENGELMANN: We calculated it and found it to be small. The droplet falls only on the order of 20 m through the plume, and for a 0.3-mm drop, the change in diameter is 10%.

CHAMBERLAIN: At the time you analyzed the raindrops did you also analyze the particle size?

ENGELMANN: Yes. It is a very tedious process.

CHAMBERLAIN: You mentioned that the efficiency varied as a function of raindrop size. Did you get any corresponding function of particle size?

Figure D-3. Normalized peak exposure, $E_p \bar{u} / Q_t$, versus time.

ENGELMANN: The abscissa on the graph in Figure 1 is particle size, and the curves are for three raindrop sizes. Each particle size was corrected for its own ionization, according to the preliminary applications to the ducted area.

SIMPSON: We got some data on zinc sulfide puffs measured by the real-time sampler and found that the material arrived at 200 m about twice as fast as the mean wind speed, and the tail end of the material arrived at about one-half the mean wind speed. The mean wind speed is the average speed over the release period of 10 sec at the site of emission.

GIFFORD: What was the height of the real-time samplers?

SIMPSON: It was 1½ m. The height of the release was about 1½ to 2 m.

GIFFORD: How high would you have to go to find the mean wind at twice the speed of that at 1½ m?

SIMPSON: About 4 or 5 m. In Figure D-1, E , the maximum exposure normalized for mean wind speed and source strength, is plotted as a function of distance. These data represent three actual field tests, and it is very surprising that the very stable run has lower exposures than the slightly stable, and at two distances lower exposures than the very unstable. Figure D-2 is from the same runs, but instead of being plotted against distance, the exposure is plotted against the travel time, given by the travel distance divided by the mean wind speed at 1½ m over the time of the emission. Here we are getting the proper separation and the proper order. Since these runs all showed about the same amount of lateral growth, this relationship between peak exposure and travel time must be in most part due to vertical growth differences. These are selected experiments and show the relationship very nicely, but all the Hanford data show this, and when we compared various sites, for example, Hanford with Prairie Grass, we obtained a much better comparison by using the same travel time. The differences are very large in comparisons using travel distance. This is the effect we have been noticing, and it seems physically reasonable.

GIFFORD: How about gradients in the vertical at the sampling distance, for example, in the very stable case?

SIMPSON: Offhand I can only say that it is very stable. The plume was contained within the tower network below 200 ft.

GIFFORD: What I am really trying to understand is whether or not there was an actual stratification taking place, so that what you happen to measure at 1½ m may be quite unlike that at, say, 25 cm or 50 cm.

SIMPSON: We were working near the peak in a very stable case. Our releases were near the ground at 1½ to 2 m, and the peak exposure was very close to 1½ ± ½ m. The variation over ½ m might be 10 or 15%.

GIFFORD: And the mean wind?

SIMPSON: The mean wind speed for a very stable case was in the order of 1 or 2 m/sec, and in the unstable case probably 3 to 5 m/sec. The effect of that is seen by running through the transformation of $x/\bar{u}$. A comparison in terms of wind speed brings about the stratification that is not seen in a comparison in terms of travel distance. When we got involved in this program, we started out

naturally in terms of travel distance, because that is the way one normally thinks of the data. When we plotted against the travel time and got the stratifications, we were encouraged to go ahead, and we have studied in this way data from Hanford, NRTS, Ocean Breeze, Vandenberg, Prairie Grass, and Dugway. We have seen this trend to some degree in all these data, that we do better in terms of travel time than in terms of travel distance. The travel time data as a whole look more reasonable.

CHAMBERLAIN: Is there not an artificial separation of the lines plotted in terms of elapse time, since the elapse time depends on wind speed, which is correlated with stability? It seems to me that your original observation, where you plotted the results in terms of distance and showed no effect of stability, was very important, and that the method using elapse time is doing violence to your data

BADGLEY: I do not agree; it seems to me that it is valuable to look at it both ways. Seeing the data in this form, one realizes that the traditional results are perhaps invalid. Seeing it in the former, one gets some insight as to why they are invalid.

SIMPSON: Figure D-3 shows a summary of all Hanford data in terms of this travel time concept. The three shaded areas on the right are all for stable conditions. They vary according to the differences in plume width. The spread of the shaded areas exceeds an order of magnitude and approaches two orders of magnitude in some cases. Yet when exposure is plotted against travel time and further separated in terms of lateral growth, the limits are very well defined. The upper curve is for very unstable conditions with a small plume. The middle

curve is for very stable conditions with a moderate plume. The third curve is for stable conditions with a wide plume. The last curve is for unstable conditions. Thus, this is a very useful concept, and I find it difficult to believe that with this kind of stratification, with data from 46 experiments, one would get this kind of separation if there were not an underlying physical basis for it.

McCORMICK: I think wind speed is a very unreliable indicator of stability. Taking the extreme case, Brookhaven experience shows that the same winds can be associated with either very unstable or very stable conditions.

GIFFORD: I approve of this method, and it puzzles me that there should be any great objection. The only thing in favor of plotting the distance is that it has been done a few times. What you regard as traditional depends on how far back you go. The original formulation of the problem was as a time problem, because it is Lagrangian, and certainly that seems to me a natural way to present the data correctly.

SLADE: First, regarding stability, if one makes the assumption that seems fairly popular today, that wind fluctuation is in some way related to the spreading of the plume, then under very stable conditions one may get a very narrow plume with small fluctuation, and under other stable conditions, a very large plume. Second, when Fuquay mentioned this a year or two ago, I didn't believe him, so I took just about all the data he has used and quite independently did much the same thing; and I found the stratification he discusses. If distance is used things don't work out: the result is just a broad smear of data points. Thus, stability itself is perhaps not the best of stratification agents.

The U.S. Weather Bureau Meteorology Program

DONALD H. PACK

The work Dr. Angell and I will be reporting and some of the work Dr. Gifford and Mr. Hilsmeier have reported are part of an integrated program of the Weather Bureau under the direction of Lester Machta of the Air Resources Laboratory on transport and diffusion in the atmosphere, covering a range of scales from theoretical micro-meteorology in the boundary layer to meso-scale dispersion. As Mr. Holland mentioned this morning, we are concentrating primarily on smaller-scale transport and dispersion. The global scales will not be covered *per se* in these reports. Our own group in Washington handles a variety of things: research, operational support, and some administrative matters.

For this presentation we have selected individual topics which alone might seem like a rather heterogeneous conglomeration but actually are all motivated by our desire to predict the characteristics of atmospheric transport and dispersion. Each of these topics is related to our interest in the atmosphere and to the interests of the AEC and the Public Health Service. We will emphasize primarily very small-scale and meso-scale studies.

As shown this morning, primarily by the discussions of the Hanford group, we are doing rather well on predictions of diffusion over relatively flat terrain at medium distances, say up to perhaps 10 miles. However, there are a number of fascinating problems connected with the larger scales that are also of considerable practical interest in applications to nuclear rockets, nuclear reactors, and urban pollution, and we will concentrate on these.

RADON

The work on the measurement of radon products is being done by Mr. Charles Hosler. A paper describing the counting techniques and the theoretical development of the use of radon daughters in determining radon concentration will appear shortly in the *Journal of Meteorological Research*. Figure 1 shows a filter and a timer. The tube at the

upper left runs to the outside. Down below is a pump with a flow rate meter that measures the amount of air passing through the filter. This is the setup for measuring radon daughter products alternately at 1 and 15 m at the Observational Test and Development Center, Sterling, Virginia. A similar setup, but with much longer tubes, is now installed on a communications tower in Virginia to measure the differential daughter product concentrations at 1 and 91 m.

Figure 2 shows the counting setup, a lead jug and a Nuclear Chicago check counter. The counting technique depends on the timing of the counts. It is the meteorological aspects that are of interest here. The idea is to determine the differential concentrations of radon daughter products over height z at two different levels, to relate these to the radon concentration, and from this, during steady state conditions, to derive the vertical diffusion coefficient, K . A second strong motivation is to make the profile measurements during conditions of extensive meteorological stagnation in conjunction with the forecasts on stagnation issued by McCormick's group in Cincinnati. We would like to confirm that radon is a good vertical tracer during these conditions.

The counting scheme is based on determining the departure from equilibrium of the daughter products in relation to the parent radon. The comparison between the concentrations of daughter products and actual radon was made by taking grab samples of the air in steel flasks and sending them to the AEC New York Operations Office for analysis for their radon content. The analysis is stated to be accurate to within $\pm 10\%$. Table 1 shows the results of a large number of determinations comparing the radon and the radium B counter measurements and the standard deviations. Considering that the radon is not measured to better than 10%, it appears that one can have some confidence in the use of the radium B measurements, particularly the differential radium B measurements.

Figure 1. Air intake and filter system.

Figure 2. RaB counting apparatus.

Table 1

Summary of Calibration Data Comparing Filter Results With Direct Analyses of Radon

Counting interval, min	Site	Height, m	No. of comparative runs	Mean concentration ratio RaB/Rn	Standard deviation
t(25, 27)	Sterling, Va.	1	8	0.80	0.16
t(21, 31)	Sterling, Va.	1	36	0.73	0.11
t(21, 31)	Tysons Corner, Va.	1	12	0.84	0.15
		91	12	0.82	0.12

There is a discussion in Hosler's paper of the problem of the contribution of radium A, and one of the significant things is that, in a hypothetical case at least twice as bad as anything that has been seen, he found that the radium A contribution could not cause more than 10% error. Furthermore, in Dr. Lockhart's work, we are not finding any radium A in the filters. He is doing some alpha counting. This problem leads to the interesting questions of whether the radium A is not filtered, whether it is not attached to particles, and other characteristics of its physical behavior in the atmosphere. These remain to be resolved and will be worked on in the next year or so.

Figure 3 shows some examples of the types of differential radium C/B ratios and inferred radon concentrations. This is an inversion case, and we find, as expected, much higher concentrations

near the ground and lower concentrations aloft. We will ignore for the time being the radium C/B ratio, which incidentally provides a qualitative indication, although not an absolute one, of the departure from equilibrium between the daughter products and the radon. Of interest in this figure is the sudden drop in inferred radon concentration after a peaking of the wind speed. We have only begun to study the correlation between meteorological conditions and radon concentration. Figure 4, also from an inversion case, is drawn to a little better scale. It again shows the peak in the wind speed and the drop in the radon concentration. From this one can compute K 's, as will be discussed below. Figure 5 is from the daytime case and shows, as expected, good mixing and essentially no difference between the radon daughter products.

Figure 3. Radon daughters versus time and height (night).

Figure 5. Radon daughters versus time and height (day).

RADON RUNS: 20 MINUTE RUNS OF BETA COUNT
AUGUST 30-31, 1961; STERLING, VA.

(MIDPOINT OF 20-MINUTE SAMPLING PERIOD PLOTTED)
(ALL DATA INCLUDES BACKGROUND CORRECTION)

Figure 4. Radon daughters versus time and height (night).

Table 2

Vertical Diffusivity (K_z , cm²/sec) Estimates From Radon Concentration Measurements Over a 300-ft Height Interval During Steady State Conditions at Tysons Corner, Virginia

$\bar{u}$ =wind speed (knots) at 100-ft elevation; $\Delta t/\Delta z$ =temperature gradient (°F), 300–6 ft.

Superadiabatic ($< -1.6^\circ\text{F}$)			Neutral ($-1.6 < 0.0^\circ\text{F}$)			Inversion ($> 0.0^\circ\text{F}$)		
K_z	$\bar{u}$	$\Delta t/\Delta z$	K_z	$\bar{u}$	$\Delta t/\Delta z$	K_z	$\bar{u}$	$\Delta t/\Delta z$
2.9×10^3	2	-3.4	4.4×10^3	3	-1.0	1.2×10^4	3	+2.8
7.0×10^4	12	-2.5	4.3×10^3	3	-1.0	1.1×10^3	12	+2.6
5.0×10^4	13	-2.7	2.2×10^3	3	-0.8	1.9×10^3	11	+1.1
1.1×10^4	12	-2.3	1.0×10^4	14	-0.9	2.1×10^3	7	0.0
2.1×10^4	15	-1.7	1.0×10^4	12	-1.5	2.3×10^4	6	+0.3
5.6×10^4	13	-2.4	1.9×10^5	17	-1.1	2.9×10^3	7	+3.4
1.2×10^4	24	-2.5	2.3×10^3	8	-0.6	2.1×10^3	6	+3.7
5.2×10^4	4	-2.0	2.7×10^4	8	-0.5	7.0×10^3	11	+0.6
6.2×10^4	6	-3.3	2.9×10^4	10	-0.6	6.2×10^3	17	+0.2
5.6×10^4	3	-2.4	1.8×10^4	13	-0.8	2.5×10^3	8	+2.7
2.6×10^4	8	-2.0	1.2×10^4	7	-1.1	1.2×10^3	8	+2.5
4.0×10^4	7	-2.5	2.8×10^3	19	-0.5	5.0×10^2	9	+4.4
1.1×10^5	7	-2.6	8.6×10^3	5	-1.3	1.1×10^3	7	+1.7
"			1.1×10^5	2	-1.4	1.3×10^2	10	+3.2
"			1.3×10^3	5	-0.5	5.2×10^2	9	+1.6
"			1.1×10^4	6	-1.3			
			Average					
4.4×10^4	9.7	-2.5	2.8×10^4	6	-1.3	4.3×10^3	8.7	+2.1
$K_z \propto z^{1.17}$			$K_z \propto z^{1.12}$			$K_z \propto z^{0.92}$		

Recently we have begun to concentrate primarily on work with a greater height difference. Table 2, prepared by Mr. Hosler, lists 44 cases of steady state conditions. Steady state means at least 1 hr of no significant change in the radon daughter product profile; in some cases, this continued for 3 hr or more. The 44 cases are divided into three classes, superadiabatic, neutral, and inversion, as measured with the temperature gradient equipment for the height interval of 1 to 91 m. The interesting thing is the variety of K values found, and I think one should realize that when using a 20-min collection period, or, as was done in a number of these cases, three 20-min collection periods (possibly slightly longer), we are getting what amounts to a vertical diffusion coefficient that represents an area. The minimum area for the wind speeds used is of the order of 3 to 4 miles in horizontal extent. The maximum is about 30 miles and would represent the mixing brought about by the thermal and the roughness conditions upwind of this particular tower.

To summarize, in these cases we found that under superadiabatic conditions K was roughly 4×10^4 cm²/sec; under neutral conditions, 3×10^4 ; and under inversion conditions, 4×10^3 . Thus they behaved in appropriate fashion. Within each stability class, however, there was quite a range. It turns out that K_z is proportional to $z^{1.17}$ under superadiabatic conditions and to $z^{0.92}$ under stable conditions, the average, of course, being near z .

Much work remains to be done both in radiochemistry and in the relationship of the various daughter products to the parent radon, and also in the meteorology. We are now making bivariate measurements at one level, the top of the tower, and the few that we have made have not shown any correlation between the vertical standard deviation and appropriate K values. This is not surprising when one considers the wind directions at the top, middle, and bottom of the tower, because the top of the tower may show almost no vertical variation, the middle may show a slight bit, and the bottom quite a bit, and the question is

at what level one should measure vertical motions in order to characterize the K that would pertain to a given depth of atmosphere.

If air pollution stagnation episodes occur this fall, we will probably also have radon daughter measurements that can be correlated with these episodes. Measurement of the daughter products, at least near the surface, is easy and relatively inexpensive.

TETROONS

In our work on trajectories and dispersion on the meso-scale, we are using constant volume balloons. We started in about 1958 with the idea that it was time to try to reach out beyond 10 miles. We would like to explore the turbulence and the trajectories over distances of the order of 100 miles from the source. The first year and a half or so of

experimentation was completely fruitless. Later on we obtained some very interesting data. This technique will be mentioned in many of the later presentations, as it has been applied in both research and operations at some of the other offices.

Figure 6 is the familiar picture of the tetrahedral balloon with a radar beacon or transponder attached. The balloons are made of 2-mil Mylar plastic. Although they do expand slightly with superpressure, they maintain a volume of ≈ 1 cubic meter. Internal superpressure must exceed about 100 millibars for the balloon to change volume radically.

Figure 7 shows the details of the radar beacon, the transponder that is currently being used. This was manufactured for us by the Cordin Company. The total length is roughly 10 in. without antenna. The weight is 121 g. The battery pack weighs 130 g. We have flown these and tracked them continu-

Figure 6. Tetroon and transponder.

Figure 7. Transponder (with and without weatherproofing).

Figure 9. Tetroom trajectories, Hatteras, North Carolina (tracked by SP-1M).

Figure 10. Tetroon trajectories, Las Vegas, Nevada (tracked by M-33).

ously for at least 21 hr, and the maximum range we have so far obtained, using a WSR-57 radar, is 100 statute miles. The current models of this transponder can be triggered by 3-cm or 10-cm radar. They transmit a return signal at a nominal 400 megacycles, which does not interfere with any other radars. The power output is roughly 500 milliwatts.

Figure 8 is a schematic drawing of the system, showing how the signal is obtained. The radar triggers the transponder, which transmits a 403-Mc signal that is received by a separate antenna system and funneled back to the radar console so that range, azimuth, and elevation are displayed.

The first successful experiment was made at Hatteras, North Carolina, with tracking by an SP-1 radar. Figure 9 shows the series of trajectories obtained. The scale is somewhat too small to show details, but it is interesting that we skin-tracked, obviously over the horizon, to about 110 miles. This was during a time when a hurricane was coming up the coast. Although this run was successful, in our next experiment we used FPS-16

Figure 11. Simultaneous tetroon trajectories, Cincinnati, Ohio (transponder tracking by WSR-57).

and SCR-584 radar, with the cooperation of NASA at Wallops Island. The ranges here were somewhat less, primarily because we were not allowed overtime.

The next experiment was done at Yucca Flats with M-33 military radar provided by Phil Allen of Los Angeles. The trajectories obtained are shown in Figure 10. Of particular interest here is the turning of the trajectories with time. The trajectories with higher numbers were progressively later in the day. Nos. 1 to 5 moved from west to east, probably because – although one can't be sure of this – in the earlier part of the day the hills to the left of the average trajectory were heated, but by afternoon the hills on the right side were heated, and the winds turned. The final trajectory, No. 6, took place at night. These measurements were the first that showed any evidence of helical circulations in the trajectories; more will be said about that later.

Figure 11 illustrates our first attempt to use transponders. This schematic drawing shows dual release of two tetroons at Cincinnati. Of interest here is the crossing of the trajectories. The isochromes show how the balloons overtook one another. More will be said about this dual flight later. Subsequently Dr. Angell, on a year's exchange with the British Meteorological Office, did a rather extensive series of trackings at Cardington, England, which will be described later. Our final series of tracks was made in May 1963 in the Los Angeles area. Figure 12 shows a family

Figure 12. Trajectory ensemble, Long Beach, California.

of trajectories in the Long Beach area. It shows the wide coverage of a series of releases in a complicated flow such as that near Los Angeles. Two of the trajectories, about which more will be said later, indicated sea breeze reversal and provided some extremely interesting data.

Some types of data other than trajectories have been acquired from these experiments. Figure 13 shows height versus time after release at Las Vegas. We were extremely impressed with the 6500-ft vertical oscillations shown here, but these have been eclipsed by the many later runs by the Las Vegas personnel, in which this typical day-time behavior has been repeated again and again. It is interesting to note the contrast in the lowest height trace at the bottom, which was made at

night and displays a difference of several orders of magnitude in the vertical velocity.

Figure 14 is from the dual tetron release at Cincinnati, and shows contra-rotating helices. Although not at the identical time, the two balloons are making helical motions in opposite directions. It would be interesting to do this type of experiment over absolutely flat terrain. The data shown here are very similar both in the geometry and in the absolute values to the helical circulation observed by Woodcock in the soaring of sea gulls, and they have certain implications with respect to the transfer of eddy kinetic energy in shear flow. A.A. Townsend has made some speculations in this area, but unfortunately we do not have enough data for a detailed analysis.

Figure 13. Tetroon vertical motions, Las Vegas, Nevada.

Figure 14. Contra-rotating helices measured by two-tetron flight, Cincinnati, Ohio.

Figure 15. Two-tetron trajectory, Los Angeles, California.

Figure 15 shows a 5-hr dual tetroon run launched from Marineland in Los Angeles. The interesting thing is that this represents a two-particle measurement of relative dispersion. These flights were ballasted with extreme care, and the average height versus time patterns were almost identical. The section of the trajectory in the radar shadow behind Palos Verdes was not obtained. Later the two balloons crossed 5 times. They obviously were not spreading very fast. They were much closer together at the end of 5 hr than after 1½ hr. Unfortunately one of these balloons was brought down by a police helicopter because of a report of flying saucers.

Currently we are using tetroons in both research and operations at Las Vegas with M-33 radar for tracking from installations on mountain tops and nearer sea level. We are also attempting to use existing radar facilities, the Air Defense Command and FAA radar net, in this case skin-tracking rather than using transponders. At the National Reactor Testing Station at Idaho Falls the tetroons have been used in at least two operational roles, the SNAP test and the SPERT test; they are also used in research, particularly in the development of the techniques of radar use in this work. At Cincinnati, McCormick's group is using the WSR-57, the standard Weather Bureau radar, and releasing particulate tracers at the same time the tetroons are released; they are getting some of the more interesting data on the use of tetroons in actual practice.

Last Tuesday we tried to find out if an aircraft could be used in lieu of ground-based radar in order to extend the tracking range. Consideration of the number of radars, the number of crews, and the cost of setting up facilities to obtain trajectories over, say, 1000 miles around a given point at any time indicates that the number of radars increases exponentially, and the budget goes up even faster. Use of the Weather Bureau's

Research Flight Facility appeared to offer a way around this. This aircraft, a DC-6, is equipped with a 2-MW, 10-cm airborne radar originally designed for Airborne Search and Detection. The research flight people were interested in trying this technique.

Last Tuesday we launched two tetroons from the Florida Keys. We found that we could indeed trigger the transponders with the airborne radar. It was possible to receive the signal on the aircraft radar-scope and to position the tetroons relative to the aircraft and continue to track them. The maximum range we were able to obtain in this essentially *ad hoc* experiment was ≈ 70 miles from the aircraft.

We hope to improve both the range and the possibilities of what can be accomplished. I think this is of as much interest to meso-scale meteorology in regard to convection as it is to atmospheric transport and dispersion. It would be nice to stand off from a large cumulonimbus and try to trace the various vertical and horizontal motions from the aircraft. We plan further developmental work both in the acquisition of data and in the development of the system. We have learned that it is possible to transistorize the current 403-Mc transponder so that the weight can be reduced by $\approx 1/2$, the battery weight can be reduced by $> 1/2$, and the lifetime can be extended by at least a factor of 3. We also plan some balloon research, particularly on the nature of the response of the tetroons to atmospheric motion.

So far as data collection is concerned, we are now planning a study of the sea breeze at Atlantic City, which we hope to carry out in July 1964. This will be a corollary to the study in Los Angeles, but in a less restricted flow area. We hope also to improve on the Research Flight Facility approach so that we can track transponders over a rather wide area, we hope to the limit of the aircraft's flight range of ≈ 14 hr, in order to trace trajectories over rather long distances.

Aerospace Meteorological Problems and Diffusion Climatology

ISAAC VAN DER HOVEN

AEROSPACE METEOROLOGICAL PROBLEMS

The research program of our group in the Weather Bureau covers scales in motion from micrometeorological motions to meso-scale motions. I would like to discuss what I call aerospace meteorological problems. At first blush, it would appear that these problems would involve global-scale motions, but we must remember that the space vehicles which we control have to be launched from the planet earth, and therein lies the problem on which I am going to concentrate.

The Division of Reactor Development of the AEC, through the Office of Nuclear Safety and through the Space Nuclear Propulsion Office, has been supporting our group in investigations with regard to the transport and diffusion of nuclear effluents or nuclear materials as a result of a launch pad abort of a missile which has a nuclear powered stage or component. This is not to say that we can't have mishaps in space. However, if you are interested in global dispersion and diffusion, we have other experts here from the Weather Bureau and from the AEC, who can discuss that aspect of it.

As a typical postulated mishap at or near the launch pad, we envision the simultaneous occurrence of what we will call a nuclear excursion or some sort of nuclear emission and the explosion of the rocket propellant, because usually these missiles are a combination of rocket stages and the nuclear stage. In order to assess the downwind concentrations of the radioactivity, we have concentrated on three problem areas, although these are not the only ones. The first problem is the height of rise of the fireball that results from the propellant explosion and possibly has nuclear debris imbedded in it. The second problem concerns the effects of meso-scale motions (by which I mean sea-breeze type motions) on the dispersion of this cloud, which moves downwind, that is, the effect of the horizontal and vertical extent of these motions on the transport of the cloud. Our two launch areas at Vandenberg and Cape Kennedy

are seacoast areas and have typical motions of this type. The third problem area is the diffusion out to tens of miles of essentially a puff rather than a plume.

With regard to the first problem, concerning the height of rise, we act as a clearinghouse for our sponsors, the AEC Division of Reactor Development, for information on height analyses of rocket propellant explosions. Obviously we are much interested in the work being done here at Brookhaven, on which some reports will soon be issued.*

We have taken on the responsibility for the meteorological analysis and the cloud height analysis for the joint NASA-Air Force propellant burn tests at Edwards Air Force Base, California, called Project Pyro. The burns involve a series of explosions with 200 to 25,000 lb of rocket propellant. This is approaching the orders of magnitude in which we are interested, considering that rockets of the future may contain as much as a million pounds of propellant.

Arrangements have been made at Edwards to obtain micrometeorological data, temperature structure, and wind structure from the 100-ft tower and from a 200-ft tower a few miles away, and also to get phototheodolite tracking of the cloud as it develops. The goal of our analysis is a prediction model for cloud height of rise from the energy content of the explosion and the pertinent meteorological parameters. We hope that the NASA-AF group will give us some idea of the energy content. At least we know the type of propellants, which will give us the energy input. The tests are scheduled to start June 1, 1964, and continue for a year.

On the second problem, the effect on effluent transport of the sea-breeze circulations, we are running experiments in Atlantic City, as Mr. Pack mentioned. We hope also to do some at Cape

*SINGER, I.A., FRIZZOLA, J.A. AND SMITH, M.E., The prediction of the rise of a hot cloud from field experiments, *J. Air Pollution Control Assoc.* 14, No. 11, 455-8 (1964).

Kennedy. The Air Force has already done some work on this under the direction of Colonel Peter Romo. Last year they used a network of surface-based anemometers to try to delineate inland penetration of the sea breeze. This year they are also going to use radar following spherical neutral balloons to aid in delineating the motions. We will be interested in the results.

Our activity with regard to the third problem area, that is, diffusion outward to tens of miles of essentially puffs, is currently concentrated on a demonstration of an atmospheric tracer technique involving neutron activation of the appropriate gaseous - I emphasize the gaseous - and particulate matter, which is tagged with an I^{129} isotopic tracer. This work is being done by the Nuclear Science & Engineering Company, under contract to the AEC Division of Reactor Development. Laboratory development of the tracer and of the techniques of emission, collection, and analysis is about complete, and we are now almost ready for a field demonstration, which we hope to accom-

plish at the Weather Bureau Research Station at Idaho Falls in July 1964.

This is not an inexpensive way to trace motions of the atmosphere, but our primary goals are initially twofold. One is the quantitative detection of a continuous point source at distances of some tens of miles under good diffusion conditions. The other is to compare a truly gaseous, inert tracer with the more commonly used aerosol or sub-micron particulate type. Looking into the future, we think that because of the method's extreme sensitivity (the Company is hopeful of being able to detect 10^{-13} or 10^{-14} g of this material) we (or someone else) might be able to simulate instantaneous puff-type sources. Since the half-life of I^{129} is some millions of years, it can be stored without problems of decay. One of the advantages of the method is to be able to make runs and analyze them not immediately but when it is convenient.

Most of the work on aerospace meteorological problems that I have discussed is at the stage of more future plans than results.

Figure 1.

Figure 2.

Figure 4.

Figure 3.

DIFFUSION CLIMATOLOGY

Because of our Weather Bureau group's association with the AEC Division of Reactor Development and Division of Reactor Licensing, we have been very active in both the preparation and review of hazard reports for nuclear reactor sites or any site where a nuclear emission is anticipated. Consequently, considerable attention is currently being given to the regional and climatological description of atmospheric measurements relating to the diffusive quality of the atmosphere within, say, the first few thousand feet of the surface.

A member of our group, Charles Hosler, in 1961 did a study of low level inversion in the contiguous United States,* in attempting to quantify on a geographical basis the inhibiting effects of this nighttime inversion. As an example of his results, Figure 1 shows low level inversion in percent frequency during the fall season. Note that there is an area in the southeast that shows a maximum and also an area in the southwest with a maximum. This is essentially a nighttime phenomenon. It is an inversion below 5000 ft, associated with surface cooling, and it is not unusual to find the maximum in the desert areas of the southwest where there is extreme radiation during the nighttime. Along the coast there are essentially areas of minimum where the wind speeds are expected to be a little higher.

A companion work† along this line for the daytime type of index for diffusion in lower layers is Holzworth's study on mean mixing depths, which is an attempt to index the enhanced diffusion in the daytime and to index a layer in which diffusion will not be inhibited. Figure 2 shows roughly how this was determined. One assumes that the radiosonde observation for the previous night describes the upper portion of the current lapse rate. The lower section is determined by extending a straight line having the slope of dry adiabatic lapse rate from the afternoon maximum temperature to the point where it intersects the earlier radiosonde trace. The height of this point is called the daytime mixed layer.

Figure 3, like Figure 1, shows data for the fall season. Note here that in the southwest there is an

unlimited area as far as vertical mixing goes; again, this is not unreasonable since desert areas typically show extremes in temperature both at night and in the daytime. This study was an attempt at a broad scale assessment of dilution capacity which will serve as useful information for siting and engineering of nuclear facilities, especially if more routine emission of effluents is anticipated.

Among the other techniques we use is to infer diffusion regimes from conventional meteorological observations. We have used the technique suggested by Pasquill, which is based on wind speed, cloud amount, and cloud height, and a further adaptation by Turner, using the sun elevation angle. For example a diffusion frequency study was made on 18 months of data from Cape Kennedy. (These are usually hourly observations which are readily available on punched cards or punched tapes from Asheville.) Figure 4 shows the type of analysis we have done. Along the abscissa is time of day, and along the ordinate is cumulative frequency. The conditions are the types explained by Pasquill, A through G.

Note that during the wintertime there is quite a great percent frequency of neutral stability, with not an overly great amount of bad diffusion conditions and not too great an area of good diffusion conditions. In comparison, the summer data show a rapid transition from good diffusion to poor diffusion. For choosing a site, if only routine data were available, this would give a sort of idea of the percent frequency of diffusion types. The analysis has been carried a little further by relating the diffusion indices to the standard de-

Figure 5.

*HOSLER, C.R., Low level inversion frequency in the contiguous United States, *Monthly Weather Rev.* 89, No. 9, 319-39 (1961).

†HOLZWORTH, G.C., Estimates of mean maximum mixing depths in the contiguous United States, *Monthly Weather Rev.* 92, No. 5, 235-42 (1964).

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Figure 6.

Figure 7.

viation of the crosswind and vertical cloud distributions, which are two parameters in the Gaussian diffusion equation for computing downwind concentrations.

In the emission of radioactive effluents over long periods of time (for example, an accident occurring over 72 hr) an important question is, how persistent is the wind? This is a form of turbulence, although it is over a much longer time scale than we are accustomed to. We made a persistence analysis of 5 years of data on hourly wind direction from 64 stations over the United States. These data have shortcomings in that the wind direction is recorded on the hour, and what happened in between is unknown. For our analysis, if the wind was from a certain compass point and stayed within a sector of $22\frac{1}{2}^\circ$, $67\frac{1}{2}^\circ$, or $112\frac{1}{2}^\circ$ (1, 3, and 5 points of a 16-point compass) we counted the number of hours it stayed in that sector.

This analysis is still in process, but we have made plots for the individual stations. For example, Figure 5 shows a comparison between the wind persistence at Denver and at Laredo. Along the abscissa are plotted the wind directions and along the ordinate the duration in hours. The dashed curve is for 10% probability, and the solid curve is for the maximum observed in a certain sector. At Laredo there is a peak persistence in the southeast, which is not unusual since the situation there is of the southeast sea-breeze type; this also is typified by the maximum observed for the 5 years of data, some 40 or 50 hr. This sort of estimate of wind persistence can be useful in reactor siting.

We have just about completed one more type of diffusion climatology study. Because of the rapid growth of population and industry in Southern California, the AEC has asked us to compile and

evaluate the available meteorological data pertaining to diffusion for use in siting nuclear reactors in that area. Figures 6 and 7 illustrate the kind of information in our report, which is almost ready for printing. Figure 6 shows surface stream lines, averaged over the month during a certain period of the day, and is an example of a typical sea-breeze type of situation. Note the various convergence zones that occur around the San Fernando Valley and Palos Verdes hills. Figure 7 shows the situation in the nighttime. It indicates that the seaward flow is not nearly so strong as in the daytime and does not appear to extend very far over the ocean.

DISCUSSION

SIMPSON: In the experiments you have planned for Idaho, what are the source heights?

VAN DER HOVEN: Surface.

SMITH: Did you say this is a costly tracer technique?

VAN DER HOVEN: Yes.

FUQUAY: Could you indicate the cost of activation?

VAN DER HOVEN: We asked the contractor but have not gotten a definite answer. It is of the order of \$100. There are possibilities of using this in conjunction with more conventional techniques. However, if there is no other way of getting diffusion out to 50 miles in lapse condition, you may or may not want to spend \$100 per sample. Hopefully the cost will come down with more general use.

CHAMBERLAIN: What would happen with the use of a more conservative tracer, one whose source strength is smaller with distance?

VAN DER HOVEN: This is a fluorinated methane, an inactive gas, tagged with I^{129} . The particulate tracer is a diiodofluorescein, which is also inert. Thus the tracer is a gaseous and a particulate compound tagged with I^{129} . The gas is mixed with air and emitted under pressure. The particulate will be emitted in very much the same way as the uranine dyes are emitted.

Turbulence Characteristics Near the Ground and Sonic Anemometry

ABRAM B. BERNSTEIN

Turbulence characteristics near the ground and sonic anemometry are not really two separate topics. They are two aspects of one portion of our research program. We have a rather modest effort in the area generally called micrometeorology. Gary Herbert, who has been working on sonic anemometry, and I are working on this. Most of the work on sonic anemometry so far has hinged on learning just what the technique can do, and I will limit my remarks on that topic to showing where it fits into our program.

First I would like to show where our work fits in the whole scheme of things. If we visualize a plume diffusing from a continuous point source, the concentration isopleths have the familiar cigarlike shape. If, now, we visualize a line or an area source, and if this source is sufficiently large, there is a region in the middle of the plume where the isopleths are parallel to the source. If we now rotate the whole picture through 90° , so that the source lies in the plane of the ground, we have the usual micrometeorological picture, in which the isopleths parallel the earth's surface. So in a sense the diffusing plume and the micrometeorological situation are limiting extremes of the same general problem, one where the source is a point, the other where the source is infinite in extent and in a horizontal plane. The atmospheric diffusion process affecting both situations is the same, and the ultimate hope of all people working in this field is to come up with a physical model that will satisfactorily describe both these extreme cases.

The work that we are doing is basic research, in the sense that we are trying to approach this ultimate solution – not that we think we will reach it, but we will take a few steps along that path – and consequently we are not concerned with practical techniques for predicting plume concentrations or predicting profiles as much as we are with the development of this physical model.

The approach that we are using makes use of the dimensional analysis, and in dimensional analysis one starts out with a certain physical

situation and somewhere along the line makes certain assumptions. The assumptions made in talking about the diffusing plume are a little different from those made in the micrometeorological problem. This is partly because of the difference between the point source and the infinite area source, but is largely because in the diffusing plume the mean flux is along the mean wind while in the micrometeorological problem the mean flux is perpendicular to the mean wind. In the diffusing plume the downwind fluctuations are usually neglected, whereas in the micrometeorological problem downwind and crosswind fluctuations are neglected and only those in the vertical are considered.

In pursuing the dimensional analysis path, it is important to impose certain checks, as has been mentioned before. For instance, if the wind profile is known to be logarithmic then, under neutral conditions, any solution must reduce to the logarithmic law for neutral conditions; similarly, in view of the relationship between the micrometeorological problem and the diffusing plume, any truly basic model derived from micrometeorological considerations must be consistent with what is known of diffusing plumes. We are trying to keep these things in mind in the work we do.

Dimensional analysis is something that different people have different feelings about. I think of it as a simple "black box" technique, whereby one makes a list of all the relevant variables in a problem, puts it into a "black box," and gets out a list of dimensionless products, which are then measured and plotted on a graph in order to show something about what is really going on. Thus there are three primary parts to the dimensional procedure: choosing the complete and proper set of relevant variables to put into the "black box"; performing an experiment or carrying out a measuring program to allow calculation of the various dimensionless products; and analyzing the observations, not merely by plotting something as a function of something else, but by doing so in a

way designed to clarify the physical process under study in terms of known physical laws.

I have gone through a little exercise in dimensional analysis which I will not go into in detail now. I will simply mention it briefly to show the direction in which we are trying to go and some of the problems that we are running into. In fact, I will throw out some questions and hope that the audience will provide some answers instead of doing it the other way around.

The exercise I have gone through deals with the relationship between the flux and profile of any general property that meets the usual requirements of being conservative, etc. Basically it consists of a generalization of work that has been done previously for the profiles of wind speed and temperature. One of the interesting results of this exercise is that the Monin and Obukhov parameter L , which appears in the dimensionless ratio z/L in considering the wind and temperature profiles, does not arise in the more general analysis. Instead of z/L one finds the flux form of the Richardson number. The flux Richardson number can be written z/β , thus defining a variable β which is somewhat analogous to L except that it varies with height. It seems reasonable to suppose that the variation of β with height is closely related to the more usually studied aspects of turbulent structure, and we hope to investigate this more closely.

The analysis I have performed has led me to consider a hypothesis that says that for any variable S , the ratio $\gamma_{ws}\sigma_s/(z\partial S/\partial z)$ should not depend upon stability. To evaluate this ratio one must have, in addition to measurements of the mean gradient of S , values for the correlation coefficient and the standard deviation, which means that rapid-response measurements are needed. What I have done so far is to examine the three sets of experiments for which such measurements are available – the experiments carried out by the Australians at Edithvale in 1951-1952, and the two O'Neill experiments. I have not been able to use the more recent Australian work since in these experiments the standard deviations and correlation coefficients were not determined. Unfortunately, my analysis of these data did not lead to any conclusion. The reason for this is that the measurements made in these programs were not adequate for the determination of the shearing stress and σ_u .

The Australians have shown that a drag coefficient defined as the ratio of u_* to the wind speed

at some low level, such as $\frac{1}{2}$ m, does not vary with stability. Yet when this ratio, as measured in these experiments, is plotted against the Richardson number, a marked variation is found, the ratio being large under neutral conditions and decreasing as one departs from neutral on both the stable and unstable sides. I think that the explanation for this is simple. These early experiments used averaging times of about 1 sec and sampling times ranging from 5 to 20 min. Under unstable conditions, a major contribution to the $u-w$ cospectrum lies in the low frequency range, and the relatively short sampling period is unable to detect it. Under stable conditions, a major portion of the eddy energy lies in the high frequency range, and the averaging period is too long to detect this. It seems that under neutral conditions these losses at each end of the spectrum are minimized, although in all likelihood the values measured under these conditions are also smaller than they should be.

It is easy to correct for this underestimation by assuming a constant value for the drag coefficient – say the value measured under neutral conditions – and recalculating the stress. Then one is faced with the problem of determining $\gamma_{wu}\sigma_u$. Since the shearing stress may be written as the product of the correlation coefficient r_{wu} , σ_w , and σ_u , this is equivalent to asking how the error in the stress is apportioned among these variables, and I don't know of any way to answer this question. What I have done is to look at the way these three variables behave with stability, by assuming the variable to be measured correctly, and also by assuming that all the error in the stress lies in that one variable. I assume that the true behavior lies somewhere in between. I find that the correlation coefficient, r_{wu} , as measured, is greatest under neutral conditions and decreases with stability and instability. If I assume that σ_w and σ_u are measured correctly, and therefore only r_{wu} is in error, and I correct it accordingly, I find that it is smallest under unstable conditions and greatest under stable conditions. Is this a reasonable sort of behavior for the correlation coefficient? I don't know. I would be interested in any reactions from the audience.

I have also plotted the gustiness ratios, $\sigma_w/\bar{u}$ and $\sigma_u/\bar{u}$, and I find that they show a slight increase with increasing instability, but nowhere near as great an increase as previous investigators, namely Panofsky and Deacon, would lead one to expect.

There are many ways of juggling and "correcting" these data, but one does not have much confidence in the results. The ratio $\gamma_{wu}\sigma_u/(z\partial u/\partial z)$, which the theory says should be independent of stability, actually shows a marked increase in going from stable to unstable conditions. If, however, we assume that all the error in the stress lies in σ_u , and we make the appropriate correction, it becomes more nearly constant, except under conditions of extreme stability, where it shows a strong increase. Thus it does not seem to be a constant, and although this may well mean that my hypothesis is wrong, it suggests that I have to learn more about the behavior of the standard deviations and correlation coefficients before I can draw a firm conclusion.

In order to do this, we need two things – an experimental program of our own, with adequate sampling and averaging times, and an analysis procedure designed to shed light on the specific questions we are asking. In terms of the analysis, we have several things in mind. One of them is to incorporate into our spectral analysis an examination of the correlation coefficient as a function of spectral frequency. This will give us information not generally available from the usual coherence analysis. Another thing we want to do is to look for a stability index more suitable than the Richardson number. We have thought of using the degree of departure of the wind profile from logarithmic as a stability index; this would be ideal, in principle, but may prove impossible in practice since it involves ratios of wind differences and may well be beyond the limits of accuracy of a cup anemometer system.

In planning an experimental program, we have a number of things in mind. We do not want to make the usual assumption that fluxes are constant with height, because strictly speaking this is not so; the stress, for example, is not constant with height as long as the wind is not geostrophic. I think that this assumption has been made in the past because it is not terribly bad, and no one has been in a position to measure the variation of the flux with height. We hope ultimately to be able to measure this. Another thing we have in mind involves the determination of steady state conditions. Everybody doing a micrometeorological experiment recognizes that he needs a steady state, but so far as I know, no one has devised a way of measuring quantitatively just how steady a steady

state he has. There are many possible approaches to this problem, such as seeing how long a period is needed to get a value of σ_u that does not vary from one period to the next. We intend to try to quantify our data in some way that will provide this information. Another point is that in planning our experimental program we are thinking of studying wind and temperature profiles. We will do this, not because these are the profiles in which we are really interested, but because these are profiles we can measure. In looking for the ideal conservative property to study, we would choose something which is completely inert, something which isn't subject to pressure forces, as the wind is, something which doesn't have a built-in tendency toward an upward flux, as has the heat flux because of its relation to buoyancy forces. We would much rather look at the flux of water vapor or some other such property. But there we would run into severe measurement problems, so we will, at the beginning at least, look at wind profiles, but this is not what we would really prefer to deal with.

Our plans for experimental work hinge on use of the sonic anemometer. This will be a device working on the principle that Dr. Badgley referred to, very similar to the one being developed by the people at AFCRL. My associate has been working on this. So far he has been using a vertically oriented sonic anemometer which measures vertical velocity only, and he has been studying the characteristics of the instrument. He plans to develop in addition a horizontal system so that he can measure all three components of the wind, u , v , and w , and we hope to be ready for a field trial with this system something like a year from now.

I would be quite interested in hearing comments as to what one might expect in terms of the stability dependence of the correlation coefficient between w and u and the gustiness ratios $\sigma_w/\bar{u}$ and $\sigma_u/\bar{u}$. The data I have analyzed are old, and there have been many advances both in instrument technology and in the state of our knowledge, but there are a few things about these data that puzzle me and need to be explained. For instance, in the Prairie Grass experiments measurements were made at 5 different points, all at the same height, and although the values of $\bar{u}$, σ_w , and σ_u are frequently nearly the same at all 5 points, the values of the shearing stress vary considerably. I am not certain how to interpret this. Does this mean that σ_w and σ_u are correct, but that the covariance is

wrong, or does it mean that the u measurements are correct while those of w are wrong? If anyone can shed any light on this, I would appreciate it.

DISCUSSION

GIFFORD: Neither L nor the Richardson number were originally supposed to be stability indices. One is a criterion and the other is a length, although they are both related to stability. As you point out, many useful indices of stability can be devised. The reason L is a useful length for characterizing the aspects of the boundary layer problem related to the vertical heat flux and the stability of the temperature gradients is essentially that it combines the fixed parameters of the flow, whereas the Richardson number is a function of height. This makes the latter awkward to use, although they are both related and could be used interchangeably. Would you care to comment on this?

BERNSTEIN: The dimensionless product that usually arises is the ratio z/L , and the question is, what does this

represent physically? The only answer I have heard is that it represents the ratio of the contribution to turbulence from buoyancy to the contribution from mechanical effects, and it really represents this only when the wind profile is logarithmic, which happens only when the buoyancy effects are zero. The flux Richardson number, on the other hand, represents this ratio all the time. Therefore I don't see why L should enter the problem. We don't want to introduce something that is convenient in that it doesn't vary with height if the price we have to pay is not to know what it means. I achieve my dimensional result not by simply writing down such things as the shearing stress and the heat flux, but by asking what physical thing I really want, and then writing down the appropriate expression. Thus I argue that the reason I need the heat flux is because I must account for the contribution of buoyancy to the turbulence, and then I write down what I know is the correct expression for this, namely, $gH/c_p T$. Similarly, I need the shearing stress to represent mechanical turbulence, so I write down the correct expression, $\tau \partial u / \partial z$. Then L doesn't arise at all, but the flux Richardson number does.

Revision of *Meteorology and Atomic Energy*

DAVID H. SLADE

The decision to revise the manual was made about 5 years ago. Originally the plan was to revise 12 or 13 chapters, but it soon became obvious that the topics should be reorganized into 8 chapters. The book deals primarily with low level meteorological diffusion turbulence, that is, it is restricted to the planetary boundary layer. It contains very little on the bomb type of meteorology, fall-off and high level transport and diffusion. This type of meteorology belongs in a separate companion work because it is quite different from that at the lower levels. About 80% of the revision is completed and will soon be sent out for review.

The first chapter is a general introduction and deals with the air pollution aspects of the nuclear energy industry and the role of the meteorologist; the history of diffusion theory and experiments; and sensors and working techniques for diffusion estimates. There is some discussion of advances in general meteorology and of the impact of nuclear technology on meteorological studies.

The second chapter is a broad survey of meteorological fundamentals for atmospheric transport and diffusion studies and touches on many of the topics extended later in the book. In general, it covers the physical basis of local climate; weather systems, including the hierarchy of organized motions and the smaller-scale chaotic motions, from the large scale down to microsystems; turbulence and the atmosphere's temperature structure; local wind structure; mean vertical structure of the horizontal wind; temporal fluctuations of the horizontal wind; spatial fluctuations of the horizontal wind and the vertical component of the horizontal wind; atmospheric diffusion; various source types in a continuous line area, etc.; and meteorological time and space series.

The third chapter deals with the theory of atmospheric turbulent diffusion, divorced from washout and fallout – this is the turbulence depletion mechanism. There is a discussion of the mean state of the atmosphere: composition, vertical temperature structures, the mean vertical structure of the lower layers, etc., the Ekman

spiral, structure of the adiabatic surface layer, etc. The next section presents theories of turbulent dispersion; Fickian diffusion; the K -theory; and statistical theories of turbulent diffusion. Another section deals with dispersion models such as the Gaussian plume model and a fluctuating plume model; estimation of dispersion coefficients; quantitative use of smoke observations; and special dispersion formulas, including cross-wind integrated concentration, volume source, fumigation, long-period averages, and various working techniques.

The fourth chapter describes the experimental determination of dispersion processes. Here there will be quite a change from the first volume, which came out in 1955, because of the large number of experiments that have been carried out since then. There will be sections on elevated sources, surface sources, peak to average concentrations, area within an isopleth, speed and direction of travel, dispersion over nonhomogeneous terrain, constant level balloon flights, deviations from a Gaussian model, long-period average air concentration measurements, diffusion from an instantaneous source, estimates of deviation from routine data, and much other experimental information that serves either to verify the theories or to cast some doubts.

The fifth chapter will be essentially non-meteorological, a discussion of radioactive dosage calculations. It will begin with basic information: relation to receptor, exposure time, short and long-term effects, uncertainties, exposure paths. There will be a discussion of radiological concepts: alpha, beta, and gamma radiation, radiation units, radiation measurements, and radiation effects. Various types of sources such as reactors, separation plants, and nuclear devices will be discussed. Finally, there will be four sections on dosage: external beta radiation, external gamma radiation, inhaled and ingested materials, and internal and external dosages.

The sixth chapter, on applications of meteorology to nuclear energy problems, will contain the nonturbulent diffusion aspects of cloud diminution

such as cloud rise, aerodynamics, deposition, re-suspension, washout, and chemical and physical changes. It will also contain discussions of various aspects of data, such as collection, transmission, storage, summaries, and applications.

The seventh chapter, on environmental safety analysis, essentially refers to the AEC requirements for hazard analysis and is a fairly thorough

discussion of types of analysis, sample calculations, reactor accidents, various types of routine stack release limits and their calculation, and applications of meteorological information to acute nonnuclear toxicity problems.

Finally, the eighth chapter deals with instruments and equipment and covers the entire range of measuring and other devices used.

Program Review and Summary of Recent Accomplishments at NRTS

NORMAN F. ISLITZER

REVIEW OF PREVIOUS DIFFUSION STUDIES

At the time of the last meeting on AEC meteorological activities at Hanford, Washington, in 1960, this office reported on the preliminary results of diffusion studies from an elevated source. Since that time, additional conclusions have been drawn from the elevated source studies, and extensive experimental work has been done with the ground-level continuous point source. The results of both the elevated and ground-level source studies have been published.²⁻⁴ I will review some of the main results from these studies before discussing the present program and some of its preliminary findings and expected goals.

Elevated Source Studies

A series of 16 releases of fluorescent tracer, uranine dye in solution, were carried out in unstable atmospheres from a 45-m source height and analyzed. Techniques of dispensing, collecting, and analyzing the uranine dye tracer have been published and are fairly well known. The sampling grid extended to 3200 m from the source with arcs 60° wide spaced at 100, 200, 400, 800,

1600, and 3200 m from the apex. Meteorological instrumentation was used for gustiness measurements from bivanes, temperature lapse information, and wind profiles from three cup anemometers spaced at various levels on a 45-m tower. Releases were conducted for 30-min periods over a test site which was fairly flat with desert-type characteristics. Some of the main results of these elevated source studies are as follows:

1. Lateral particle standard deviation, σ_y , at the ground was closely related to the standard deviation of the horizontal wind direction, σ_θ , at source-height level. The bivanne data were averaged by eye for 5-sec periods to eliminate the effects of bivanne inertia and errors in reading the higher frequencies. The relation of σ_y and σ_θ at source heights is shown in Figure 1.

2. Distance to maximum concentration on the ground was found to be inversely proportional to the vertical wind direction standard deviation, σ_ϕ , in which the vertical wind data were also averaged for 5-sec periods. These results are shown in Figure 2, which also shows the predicted relation-

Figure 1. Comparison of measured ground-level horizontal particle standard deviation, σ_y , with the horizontal wind-direction standard deviation, σ_θ .

Figure 2. Distance to maximum ground-level air concentration versus the standard deviation of vertical wind direction from 5-sec-averaged wind data.

Figure 3. Computed versus measured peak ground-level air concentration for each arc (mean of all 16 tests).

ship assuming that the vertical profile of concentration is Gaussian in form and that the vertical distribution parameter, σ_z , can be related to σ_ϕ by a form similar to that shown in Figure 1 for the horizontal particle spread. The value of the regression coefficient, b , shown in the plot in Figure 2 is the same as was derived from the horizontal dispersion data shown in Figure 1.

3. There was good agreement between the measured centerline concentrations on the ground and those computed from the Gaussian interpolation formula assuming the symmetrical forms for the lateral and vertical particle standard deviations, σ_y and σ_z , given from Figures 1 and 2 in terms of σ_θ and σ_ϕ . The mean of these comparisons for all 16 tests is shown in Figure 3. The agreement is good out to about 1000 to 2000 m, but there appears to be a departure from the simplified model beyond this distance.

Ground-Level Source Studies

Studies similar to the elevated-source study discussed above were conducted at the NRTS during stable and unstable conditions to 3200 m over the same sampling grid. In addition, a row of 30-m towers equipped with air samplers was used at either 200 or 400 m from the source to determine tracer depletion due to deposition and to attempt to verify the Gaussian assumption of the vertical profile of concentrations at that one distance. About 40 releases for 60-min periods were carried out, and gustiness data from bivanes were once

Figure 4. Measured lateral particle standard deviation versus downwind distance.

Figure 5. Measured versus predicted lateral particle standard deviation for lapse cases ($\beta=5$).

again obtained. In addition, deposition measurements on flat plates were also attempted out to 200 m from the source for comparison with continuity data. Some of the main results of these studies are as follows:

1. The 60-min lateral particle standard deviation did not classify well by stability, and because of apparent plume meander the σ_y values in stable atmospheres approached those values measured in unstable atmospheres (see Figure 4).

2. The lateral dispersion was closely related to the lateral wind direction fluctuation, σ_θ , in sev-

Figure 6. Nomogram of concentration versus distance from NRTS data.

eral ways. With use of the Hay-Pasquill hypothesis of similar shapes for Lagrangian and Eulerian autocorrelation functions, an average value of 5 and a most frequent value of 3 for β was found from simultaneous tracer and bivanne measurements. A plot of the measured versus computed σ_y using an average value of 5 for β for lapse conditions is shown in Figure 5. However, σ_y was almost as well correlated to the linear form used for elevated source data in Figure 1, and to the interpolation formula used by Cramer,¹ $\sigma_y = a\sigma_\theta x^p$. It appears for distances of 2 miles during unstable conditions that all three methods of using horizontal wind data give equally satisfactory results. In addition, empirical curves of concentration versus distance sorted by different ranges of σ_θ also appear useful. The results of this sort of an approach from NRTS data are shown in Figure 6. The relationship between σ_θ and vertical temperature lapse rate was poor.

3. A subjective criterion, such as wind speed and insolation, for the sampling times involved, 60 min, was found to be a good classifier for axial concentrations.

4. The variation of vertical dispersion with stability is shown nicely in Figure 7. It should be re-

Figure 7. Vertical dispersion estimates.

membered that the σ_z values in Figure 7 were computed from ground-level concentrations and measured lateral dispersion by using the Gaussian interpolation formula. The accuracy of this cannot be resolved at this time because of the lack of direct vertical dispersion measurements.

5. Vertical dispersion measured at one distance on the 30-m towers was in fair agreement with vertical gustiness data assuming a β of 5 with the vertical bivanne trace. However, the measured σ_z 's from this technique were considerably smaller than those computed from the Gaussian interpolation formula at this distance. The latter, on the average, were a factor of 2 to 3 too large.

6. Axial concentrations computed from the Gaussian interpolation formula, using horizontal and vertical wind direction standard deviations in the manner used for the elevated source, were unsuccessful. Computed concentrations were too high. A correction for deposition seemed to be necessary.

7. Tracer depletion from continuity measurements on the tower suggested values of 20 to 50% loss to 400-m distance. The deposition velocities needed to account for this tracer loss, and also to bring predicted and measured concentrations into agreement, were a factor of 10 higher than those measured on the flat plates. For lapse conditions the average flat-plate deposition velocity varied between 0.02 and 0.3 cm/sec, whereas an average

velocity of 5 cm/sec was indicated from the tracer continuity and axial concentration data. It appears that the effect of the ground upon the vertical profiles of air concentration due to reflection, deposition, and wind shear still are not perfectly understood. Therefore, empirical curves of axial concentrations may be drastically in error if extended out to, say, tens of miles without further modification.

PRESENT EMPHASIS - RESEARCH

Meso-Scale Transport (10 to 50 Miles) Using Radar

The complex transport problems at the NRTS that are often experienced due to the apparent complicated interaction between pressure gradient

and topographical factors have not yielded successfully to measurements from fixed stations or soundings of temperature and winds. The density of stations required to study three-dimensional flow under such conditions is prohibitive. An M-33 radar with tetraon-transponder capability similar to that described by Pack and Angell⁵ in their work at the Los Angeles basin has been developed. The light-weight transponder easily borne aloft by the tetraon has given ranges >50 miles, which is the present limitation of the radar plotting board. The transponder is now being mass produced for delivery in large quantities, and a large-scale program is being initiated. Several interesting trajectories showing the retreat and the onset of drainage winds off the high mountains north of the NRTS are shown in Figure 8.

Figure 8.

Reactor Destruct Tests

Four reactor destruct tests have been conducted in the last 18 months at the NRTS with heavy meteorological support both for safety purposes and also to determine the relationship between the dispersion and transport of airborne radioactive material and meteorological parameters. Transient tests, in which the reactor was violated and fission products were released to the atmosphere, were conducted with respect to the SPERT (Special Power Excursion Reactor Test) and the SNAP (Systems for Nuclear Applied Power). It was found, apparently because of the water in the reactor system that sometimes spurted out in the form of a geyser during the excursion, that virtually no iodine was detected in the airborne effluent. The airborne effluent consisted largely of daughter products of noble gases, krypton and xenon. Calculations of the source were made from the radioactivity data in the field, using precise measurements of gustiness with bivanes and anemometers supplemented by Lagrangian-type data from the radar-transponder tracks. The SPERT released <1% of the fission products formed in the excursion from the reactor despite extensive damage to the core in one case and minor damage in two other tests. The SNAP destruct resulted in complete destruction of the reactor with 5% excursion products released into the air. The amounts of noble gases released were such that radioactive material suitable for meteorological analyses extended only to several miles from the source. It was found that the plume dispersion parameters such as crosswind and downwind particle variances were given quite well by bivan data in the manner prescribed by the uranine dye studies discussed before.

Waste Calciner - Long Period Diffusion

Operation of the Waste Calciner, a process to convert liquid to solid radioactive waste, results in the evolution of ruthenium-106 for long periods of time at a fairly steady rate. This source of opportunity, which has a 1-year half-life, appears to be ideal for studying long-period average diffusion, say for weeks or months. A ring of 12 air sampling stations was placed around the Waste Calciner 250-ft-high stack at a distance of 1 mile. Weekly averages of the Ru¹⁰⁶ concentration were collected and compiled in 2-month averages. The mean concentration in each direction sector is compared with the predicted value using the Gaus-

Table 1
Measured Versus Calculated Concentration for 7 Days
at 1 Mile From 250-ft Source

Wind direction	χ , mCi-sec/m ³	
	Measured	Calculated
N		1.25×10^0
NNE	7.91×10^{-1}	1.58×10^{-1}
NE	7.35×10^{-1}	3.48×10^0
ENE	5.37×10^{-1}	3.18×10^0
E		7.11×10^{-2}
ESE		5.83×10^{-1}
SE	4.66×10^{-1}	1.58×10^0
SSE	- ¹	1.17×10^0
S	5.25×10^{-1}	3.07×10^{-1}
SSW	7.95×10^{-1}	1.42×10^0
SW	9.65×10^{-1}	2.79×10^0
WSW	4.90×10^{-1}	3.43×10^{-1}
W	3.44×10^{-1}	2.60×10^{-1}
WNW		5.49×10^{-1}
NW	2.09×10^{-1}	4.65×10^{-1}
NNW		7.62×10^{-1}

sian model and the apportionment of the source by the observed wind direction frequency at the 200-ft level. Lapse conditions only were considered, since little contribution to the Ru¹⁰⁶ concentration during stable conditions would be expected at that distance. Table 1 shows the results for 7 days. It can be seen that this crude extension of the Gaussian model for longer periods of time by the observed wind direction and stability features is fairly adequate.

A 2-month period of snow cover with virtually no melting was in existence for the months of December and January. The long half-life of Ru¹⁰⁶ allowed the vertical snow depth sample to be a good measurement of the rate of deposition of Ru¹⁰⁶ for that length of time. A mean deposition velocity of 0.5 cm/sec was computed from the observed Ru¹⁰⁶ concentration and snow deposition for the 2-month period. Since the Ru¹⁰⁶ is in a very small particulate form on leaving the stack, mass median diameter of 0.01 μ , these numbers are in agreement with those observed by other investigators. These studies are continuing, and it is hoped that long-period average concentration relationships for summer can be obtained.

CERT (Controlled Environmental Radioiodine Test)

The CERT project consists of a series of planned releases of radioiodine over different vegetation

under various meteorological conditions, a final objective being to measure the amount of iodine through the air-vegetation-cow-milk-human chain. The first experiment over a natural crested wheatgrass pasture has been conducted. About 1 curie of iodine-131 gas was released for a 30-min period in a moderately unstable atmosphere over a small pasture, 150×300 m in size. The crested wheatgrass covered about 15% of the total plot, the remaining surface between plants being soil covered. Figure 9 is a sketch of the experimental site, showing a hot pasture where 6 cows were grazed on successive strips, moving inward daily from the outer strips towards the source. A cold pasture which was uncontaminated was used for background studies and to acclimate the cows to the vegetation. A line of 5 iodine generators was formed to make an even distribution of I^{131} across the pasture. Meteorological measurements consisted of gustiness and stability on a 16-m tower located near the hot pasture. Human volunteers were allowed to drink the milk on a daily basis to determine the uptake of iodine in the thyroid. The following results were found:

1. Deposition velocities on the grass varied between 0.4 and 0.8 cm/sec with an average value of 0.6. Soil deposition velocities were quite similar in magnitude, but there was some uncertainty as

to the accuracy of results because of difficulties in the process of soil analysis.

2. Several type of plates were used to compare deposition on artificial collectors with that on the natural vegetation. It was found that a fallout plate covered with carbon gave velocities quite similar to those found on grass and soil. Flat plates using sticky paper or sand had lower deposition velocities than the carbon by factors of 3 and 4 respectively. The I^{131} was of either gaseous or very small particulate form, since most of the material was collected on a carbon trap in the air sampler behind a prefilter. The prefilter is used to scrub out particles in the micron range.

3. The effective half-life of the I^{131} on the grass was about 3.5 days.

4. The average ratio of activity of milk to that of grass was about 240 (picocuries per liter/picocuries per gram). The average thyroid uptake of ingested I^{131} in milk was 19%. Thyroid doses to the volunteers averaged 0.39 rad.

Attempts to relate measured deposition to computations employing either vertical gustiness, friction velocity, or vertical Austausch were not successful. Computations were from bivane and anemometer data from 1 to 16-m heights and greatly overpredicted deposition.

Additional tests over a more uniform irrigated pasture are planned with considerable refinement in some of the experimental techniques. Sublimation or suspension of the I^{131} into the air from the grass will also be studied.

Boundary Layer Turbulence and Diffusion

It is planned to investigate the features of the vertical profile of concentration in stable or neutral atmospheres on a 200-ft tower. Air samplers have been installed on this tower to measure the effects of the ground surface upon the vertical distribution of tracer for at least one distance. The releases would be at distances of 1 to 2 miles upwind if it is determined that the top of the plume can be effectively sampled from those distances. Apparent plume rise, due to either the reflecting effect of the ground or deposition, as well as the effect of wind direction shear, would be investigated. It is also planned to release the tracer in a line source from a moving vehicle over some 1 to 2 miles in an attempt to determine differences of shear effect upon a line source compared with a point source. The apparent twisting of the plume

Figure 9. A sketch of the I^{131} release site and meteorological installations.

from wind direction shear from a point source such as was found in the Green-Glow studies may be eliminated by a line source approach, so that non-Gaussian appearances of vertical particle distribution may be attributed to deposition or ground reflection effects.

A multiple-tracer capability is being investigated by the Analysis Branch of Idaho Operations. Multiple fluorescent tracers such as uranine dye and rhodamine B were discarded, since there is too much overlap in their emission spectra. Because of the availability of facilities for irradiation and a 256-channel gamma analyzer, more promising techniques using activation procedures are being explored. Particles would be disseminated originally in nonradioactive form, collected on filter media in the air, and irradiated in the MTR reactor. Elements which have suitable half-lives after irradiation, and different energy levels for easy separation and detection on the 256-channel gamma analyzer, will be selected. Preliminary tests indicate that copper would be useful at short range only because of large amounts of this material in natural background. Manganese, indium, and iridium are also being explored by the Analysis Branch. If this multi-tracer capability proves feasible in the laboratory, as well as in prototype runs in the field, simultaneous tracer releases at different upwind distances from the tower can be made. In this manner the variation of vertical dispersion with distance may be obtained.

Boundary-layer turbulence and wind profiles and stability will be obtained from high-speed transducers placed on several levels on the 200-ft research tower. Bivanes with proven responses of 0.5 sec at winds of 5 miles/hr or more, Beckman and Whitley anemometers, and temperature elements will be used to measure the variation of gustiness and the stability length, L , with height. Interpretations of the vertical dispersion versus wind shear will also be made. A 10-channel high-speed digital system is available to record the analog inputs on punched paper tape for computer processing on an IBM 1620 or an IBM 704 computer.

OPERATIONS AND SERVICE

An extensive operational and service effort is performed by the U.S. Weather Bureau in support of reactor tests and associated activities. Before a reactor experiment in which significant

amounts of radioactive materials may be released into the atmosphere is undertaken, an appropriate set of diffusion weather conditions is selected by the Weather Bureau from long periods of record. These conditions are selected such that health and safety controls are adequate and the objectives of the test experiment can be realized without excessive delays. A practice forecasting program is usually initiated before the test so that maximum accuracy and minimum delays in getting the test off are possible. Forecasts of the expected diffusion conditions are given in weather briefings to contractor and AEC personnel for the conduct of the test. During the test meteorological conditions are monitored by extensive telemetry systems so that quick computations of streamlines, centerline trajectory, and plume parameters can be made. These are radioed to mobile and aerial monitoring teams for optimum sampling of the reactor effluent.

Nuclear events, including major accidents such as the SL-1 accident, and routine releases of radioactive material useful for diffusion studies are documented. Efforts are made to use the material for studying any departures of the transport and dispersion of radioactive material from that measured in greater detail with fluorescent tracers. If significant information is available, special reports such as that issued for the SL-1 accident are prepared. Meteorological records are studied to explain any unusual rise in levels of environmental activity, particularly to determine if the source is local or extraneous to the NRTS.

Safety analysis reports for siting and hazard studies are reviewed for all reactors planned for the NRTS. Evaluations of the effects to the environment from both accidental and planned releases are made for Idaho Operations. An extensive observational program is conducted so that the meteorological statistics needed for reactor safety and siting purposes as well as planning for special tests are available. This information is digested into reports at intervals.

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The Meteorology Program at the Nevada Test Site

PHILIP W. ALLEN

The Weather Bureau Research Station in Las Vegas is oriented primarily toward meso-space and macro-scales of meteorology, and toward operations applications. It pursues little that can be termed micrometeorological research. This is a support facility to the AEC Nevada Operations Office, particularly the Operational Safety Division. Some of the staff actually have a staff function in the Nevada operations, planning and assisting in the carrying out of nuclear tests as well as being a service agency to the Nevada Test Site. The Nevada Operations Office manages, and therefore we support, all United States tests of nuclear explosives in the weapons development program, the DASA* weapons effects program, the AEC civil effects program, the ARPA** Vela Uniform program for the long-range detection of underground explosions, and the Plowshare program for the application of nuclear explosives to peaceful purposes.

Under an international treaty banning atmospheric tests, all these programs are at present limited to contained underground explosions. The possibility of conducting nuclear explosions to produce craters and to perform other earth-moving tasks for peaceful purposes within the treaty is under study at the top levels of government. Such explosions tend to vent only small fractions of their fission products to the atmosphere if they are used efficiently. The Research Station also provides meteorological support to the Nuclear Rocket Development Station in Jackass Flats. This facility is under the management of the Space Nuclear Propulsion Office, a joint NASA-AEC management organization.

There is no nuclear laboratory as such at the Nevada Test Site. All or most of the tests performed there would have been held elsewhere if it were not for the radiation or explosive hazard. Therefore, perhaps some of our problems are more interesting than our solutions. Among the deci-

sions influenced by the weather are the following: First, the selection of sites for experiments and for installations. Site selection has been a major problem in several cases, perhaps the best known being the various Plowshare tests. For the SCHOONER event, which was supposed to be a large-scale cratering experiment, the site was selected on the basis of geology, but it became necessary to move it at least partially on the basis of meteorological considerations. The frequency of occurrence of winds potentially carrying fallout or clouds of debris away from populated areas has to be high enough to be operationally useful. This was not the case at the initial SCHOONER site, and at the revised location it was satisfactory but not good. The SCHOONER event has since been indefinitely delayed.

The dilution rates are dependent on vertical temperature structure, shear, and other factors. These should be consistent with distance downwind to which high concentrations of radioactive material can be permitted. This involves a study of the inversion frequency, inversion depth, height to the inversion, depth of the mixing layer, etc. The frequencies of clouds, rains, and snows should be low enough not to present undue likelihood of hot-spot hazard or otherwise seriously limit necessary activities. Trajectories from the site should not tend too frequently to cross a radiation-sensitive area so that an individual event is delayed, or to cross the same area with such frequency that accumulated dosages would exceed permissible limits for an entire series if multiple events are scheduled. Although this is not usually a problem in southern Nevada, trajectories from other radiation sources should not cross those from the new site with very high frequency.

We assist also in the design of new facilities; for example, the orientation of the exhaust plumes from some of the nuclear rocket engine test stands in the Rocket Engine Development Station was designed on the basis of our climatological data. The engineering design of experimental facilities

*Defense Atomic Support Agency.

**Advanced Research Projects Agency.

involves such things as wind loading, thermal expansion problems, humidity effects, evaporation rate, and so forth. Like most sites, we aid in the scheduling of construction activities of support services. We are involved in the design of some technical experiments; for example, in the ROLLER COASTER experiment, a series of small tests involving the dispersion of radioactive particles done about a year ago, the meteorological criteria were very strict.

Our primary responsibility is in the scheduling of events. Considerations of climatology (wind or weather) have entered into the scheduling of numerous experiments and have resulted in schedule modification in a few cases in recent years, e.g., the SMALLBOY nuclear explosion, the ROLLER COASTER series, and the SWET test, which is a simulated water entry test designed under the aerospace program. In scheduling these, the conflict between the meteorology and the technical requirements had to be resolved. Atmospheric nuclear detonations and power runs of nuclear engines have frequently been delayed for hours, days, or weeks in order to take advantage of wind and weather conditions that would provide adequate safety with respect to fallout or blast. Predictions of rainfall as far away as the Mississippi River and as much as 3 days in advance were considered in the final decision to fire one nuclear cratering shot about 2 years ago, project SEDAN.

Connected with the scheduling of nuclear events is the control of work areas and camp areas within the confines of the Nevada Test Site. Prior to each event from which significant radioactive effluent is expected (such as engine tests) or is possible (such as underground explosions) controls are set up governing the movement and access of people to work and camp areas based to a large extent on predictions of radiological hazard, wind, and dilution rate in the atmosphere. The radius of concern varies from the order of 2 miles in the case of a small underground nuclear explosion to 50 miles or more.

Also of concern to us are the safety of off-site populations and public relations. The decision to proceed with the nuclear phase of each experiment is made upon assurance from meteorological predictions and measurements that people off site will not be subjected to more than an acceptable amount of radiation or blast. To further assure safety, actions are usually taken to verify the

amount of radiation, if any, in the atmosphere, to verify the blast levels, and, if appropriate, to set up emergency procedures, based on the predicted intensities in the case of blast. The latter is done by Reid's group at the Sandia Corporation, from meteorological soundings taken by the Weather Bureau up to the maximum or burst height of the normal radiosonde, and from meteorological rocket information taken by the Sandia Corporation at the nearby Altoona test range up through the ozonosphere. To further verify blast predictions, they set off small high explosive detonations and check the blast received in various directions to assure that the transmission of blast will not include serious focusing.

With regard to safety, the predicted and measured trajectories of radioactive effluent are also considered. Prior to each nuclear event, trajectories are predicted out to at least 24 hr and often to several days beyond. The pre-event predictions and the post-event measurements of particle deposition and residual radiation dosage - i.e., fallout - are taken into account. The predicted diffusion rate of suspended materials and the resulting external radiation dosage from passage will be further discussed below. The possibility of hot spots at long range due to rain-out is considered, and after the event everything that has happened in the atmosphere is documented to the best of our ability. The trajectories of air parcels at a number of altitudes out to varying lengths of time are followed and become part of the permanent record of the event. These trajectories are checked for radioactive material in the atmosphere by aircraft flying the patterns and taking samples.

The Station also provides meteorological support to some technical programs. Many events require measurements of atmospheric pressure, temperature, wind, humidity at various altitudes, etc.; soundings or surface instrumentation are used as required. When atmospheric testing was being done, we supported the fallout measurement programs in considerable detail. We supplied meteorological information to Holland's group in connection with the measurement and study of the fallout in the SMALLBOY event 2 years ago. We provide an advisory service for meteorological problems to all the agencies that use the Nevada Test Site. A number of studies are in progress related to their nuclear operations; some of these are described below.

First, we are continually developing the radiation models for underground nuclear events. One normal characteristic of nuclear experiments is that the source term is unknown or at best uncertain for radioactive contamination of the atmosphere. In the case of venting of underground tests, the source strength in curies is never known within a factor of 10 – it is predicted and designed to be zero – so that the problem of prediction is not an easy one. No two ventings are sufficiently alike to be compared. However, we have come to expect certain kinds of trouble in certain test configurations. The maximum venting experienced from each configuration thus becomes a model for the next test having the same possible weakness.

The process of collecting and analyzing the radiation data from these events has become a long-term task of some difficulty. Venting always seems to occur during an awkward situation: the wind has just changed, leaving monitors on the wrong side of the valley; or the wind is blowing across inaccessibly rough terrain, where monitors and equipment cannot get; or the samplers are out of order because the generators stopped or the gas tanks went dry; or for some reason the monitors have not recently calibrated their instruments; or some other difficulty comes up. The data are always too sparse, even if everybody gets in the right place at the right time, and they are frequently suspect; the analysis of these data and putting them into proper context through meteorology is a difficult and long task.

Once a consistent picture is produced of surface dose rates from cloud passage, surface dose rates from particle deposition, and (one hopes) dose rates in the cloud from aircraft reports, this information becomes a model which may be used for future similar tests. The radiation patterns are adjusted for predicted differences in cloud height obtained from the predicted temperature sounding; for the differences in mean wind speed and wind shear; for differences in the efficient yield of the device; for possible inclusion of induced activities from thermonuclear devices; and so forth. Quite a checklist of items must be covered in producing a predicted radiation pattern. The final prediction can be called neither the expected radiation hazard – because none is ever expected from underground tests – nor the maximum credible, because we never know when some worse form of venting may occur. It is simply a guide on

which the test manager and his advisers may take action in the interests of safety.

We also predict the radiation levels versus distances for nuclear rocket engines. Since our experience with the release of large volume sources of debris is rather limited (only 7 KIWI and 2 TORY tests), we have used dual approaches in the prediction of radiation levels downstream. One has been the Van der Hoven diffusion-deposition (or tilted plume) calculation, which utilizes classical approaches. The other has been to attempt to predict by comparison with the first KIWI test, which was well documented and produced significant amounts of radiation downstream so that it provides a good model. We use the same scaling procedures as in the underground tests. We adjust for the integrated power levels to get the dose for a normal run, and we adjust for the presence in the pattern of the order of 10^5 curies at hour H plus one to get the dose for the maximum credible accident. This number of curies is a variable depending on the total length of time that the reactor has been run and the total amount of fission products accumulated within it.

These estimates are always for three different kinds of prediction: (1) the surface deposition of particles, the residual radiation or fallout; (2) the concentration that would be observed at the surface; (3) the external exposure of a man standing in the path of the passing cloud. The standard diffusion techniques are difficult to apply to these cases, because we are interested in dilution out to many tens of miles since the areas close to the test cell are cleared of people, and in some cases the terrain is very rough and difficult to reach. Another difficulty is that the clouds from upward firing reactors rise to many thousands of feet. The momentum from the initial force of air or hydrogen propellant upward through the reactor tends to carry the material up to 1000 or 2000 ft, and then there still is buoyancy left in the remaining plume so that it tends to rise higher and to diffuse as the atmosphere permits. Thus the total cloud heights may be of the order of 4000 ft (the lowest we have had was about 3500 ft) up to a maximum as high as 12,000 or 15,000 ft above the surface.

At such altitudes the clouds encounter several different meteorological conditions involving several diffusion constants and several layers of wind direction, so there is plenty of directional shear in many cases. The scaling approach can handle

these shears for the particular fallout, but not until the more recent tests have we been able to get good information on the concentrations of activity within the cloud. We hope that we can now begin to check out the diffusion approaches at the higher altitudes. It is important that better verification information be obtained so that we will not have to hedge estimates on the high side. It will become more and more difficult to get public acceptance of the release of radioactive materials, and it does not help to overestimate the levels to be expected.

Among our other current projects is a study of the lower atmosphere using tetroons. Howard Booth has a file of information on some 180 tetroon releases during September 1963 and January and February 1964. We have early detailed lapse rate information to go along with the releases, and the tetroons were tracked carefully by radar. Booth and his associates have been developing the tetroon inflation technique to the greatest possible accuracy in order to be able to program a tetroon to fly at the desired altitude. They are also involved in a study of desert valley winds and temperatures and have made several cross sections through the Jackass Flats, which is a relatively broad, elongated valley, fairly typical of southern Nevada. The kinds of information desired here are slope winds as a function of the incidence of sunshine; the effects of pressure gradient winds on the slope winds; the size, duration, and movement of eddies; and the projection of particles under so-called typical flow.

Another project concerns the use of numerical methods to forecast the winds in the lower atmosphere. The prediction of wind direction is our biggest problem, to the extent that it overshadows our present capability to predict the levels of radiation downstream. Our ability to predict radiation is better than our ability to predict wind direction. Much of our future effort will go into this one problem.

The last project to be described is a study of trajectory techniques. Some of the actions we are taking to improve our ability to predict trajectories are as follows: (1) More careful measurement of the meteorological conditions at the source of a release. (2) Use of long-range tracking of tetroons. (3) Making measurements and analyses of meteorological conditions at the samplers that the various radiation people have downstream. This is related to the radioiodine programs described here today. The Public Health Service has special equipment around the test site to sample radioiodine, and they are planting farm plots with alfalfa, samples of which will be analyzed. We will provide the meteorological instrumentation to determine how the material got on the alfalfa. (4) Making measurements and analyses of the previous meteorological conditions of the alfalfa plots.

Most of our other small studies are oriented strictly toward the solution of immediate problems at the test site.

The U.S. Public Health Service Meteorology Program

ROBERT A. MCCORMICK

I am representing the Weather Bureau Research Station at the Robert A. Taft Sanitary Engineering Center in Cincinnati. We function as the Meteorology Section of the Laboratory of Engineering and Physical Sciences of the Division of Air Pollution of the Public Health Service. There is an arrangement whereby meteorological competence for the Public Health Service is provided through transfer of funds to the Weather Bureau. We are the only meteorological group within the Division and therefore provide meteorological assistance not only to the Laboratory of Engineering and Physical Sciences but also to other branches in the Division. Three major project activities comprise the work of our Station.

TECHNICAL ASSISTANCE

Technical assistance to other branches of the Division of Air Pollution is an important and growing activity. We have a total of 11 meteorologists on our staff, and 3 of them are engaged in this. The services performed are always provided through the Technical Assistance Branch or Field Studies Branch of the Division of Air Pollution to states and local communities. They involve various activities such as helping to prepare meteorological portions of air pollution surveys or studies, or giving specific advice and consultation to states and communities in connection with local problems. For example, a company may wish to put a power plant at a given location, or another company may wish to test rocket motors containing beryllium or other toxic fuels. These companies will try to get the approval of the local air pollution or health agency, and generally the local agencies are not set up to handle the meteorological aspects of such problems. Their technical staffs are usually very limited, so that we must act, as the Technical Assistance Branch must often act, as consultants or advisors to the local agency on the technical problems.

We have one meteorologist assigned to the Field Studies Branch of the Division of Air Pollu-

tion. He provides the meteorological competence in the epidemiological studies, helping to interpret the data. One of the most interesting problems in which he is now involved is the investigation of "New Orleans asthma." This problem is very serious. On an average night 10 to 12 persons are admitted to Charity Hospital, a part of Tulane University, suffering from acute asthma attacks. But some nights the hospital is flooded with as many as 100 or more patients suffering from acute asthmatic distress, some of them terminal cases. The cause is unknown at present. A great deal of circumstantial evidence points to air pollution as a factor, but beyond that the problem is quite unsolved, and a great deal of detective work will be required by medical men and meteorological engineers. The clinical symptoms are quite similar to those of the "Yokohama asthma" which afflicts many of our Service men and their families in Japan. Asthma is also a problem in certain sea-coast cities of the Netherlands.

TRAINING PROGRAM

Another of our major activities is the training program. We are one of the few groups represented here who have an organized, regularly scheduled training program to teach both meteorologists and non-meteorologists the meteorological aspects of air pollution. I have the agenda available of two of the courses. One is aimed primarily at non-meteorologists, although several meteorologists have taken it, and the other is a course for meteorologists on air pollution transport and dispersion. Our staff is responsible for the presentation of both courses. One meteorologist is on this duty full time. We also get invaluable aid from outside guest lecturers.

AIR POLLUTION FORECASTING

Our third major activity is the air pollution potential forecasting program, a service which we are now providing to the entire 48 contiguous

states. This program began 4 or 5 years ago on an experimental basis. The forecast data were first issued on a regular basis to the public, or at least to Weather Bureau stations, some years ago for the eastern two-thirds of the United States, and in October 1963 this service was extended to include the western states.

Most of our 11 meteorologists have individual research projects for which they are responsible, and I would like to describe four of these.

ST. LOUIS DIFFUSION EXPERIMENTS

Francis Pooler is the project meteorologist for these experiments. Their specific purpose is, by means of a number of series of diffusion experiments over a 2-year period, increasing in complexity and instrumental sophistication as our experience and knowledge grow, to investigate gross dispersion characteristics of continuous point sources in an urban setting under a representative variety of weather conditions. We have hundreds if not thousands of similar diffusion experiments over level terrain, in Salisbury Downs, the Great Plains, etc., but I don't believe there has been reported even one single continuous point source quantitative diffusion experiment conducted in an urban setting.

We are using zinc cadmium sulfide particles as the tracer. This is the standard fluorescent material, No. 2267 prepared by the U.S. Radium Corporation, fluidized, sized, and assayed for us by the Metronics Group. The rate of release ranges from 30 g/min under stable conditions with light winds to 150 g/min under unstable conditions with moderate winds, normally for 1-hr periods.

Figure 1 shows the arrangement of the two dispensing sites and the sampler sites. One dispensing site is in the southeast corner of Forest Park located at ground level. Its distance from the river is ≈ 5 miles. The second site is on the roof of the three-story Knights of Columbus Building, south of the main business district of St. Louis. The sampler sites are on concentric arcs. The arcs for the Park site are at distances of $\frac{1}{2}$, 2, and 5 miles, roughly extending through 180° . From the K of C Building site, the distances are 2, 4, and 8 km, and we are setting up another arc at 16 km. The original spacing of the sampler sites on these arcs was 60° , but this was found to be inadequate under all

meteorological conditions other than quite unstable; therefore, the spacing is now being increased to 3° in the two outer arcs. We have about 100 pieces of sampling equipment: 10 drum samplers, each with 100 effective positions on each drum, with time interval settings of 30 sec or 1, 2, or 4 min.; 30 membrane filter samplers; and 60-odd "roto-rods" with 12 balloon cable mounts for positioning the roto-rods in the vertical.

For the meteorological input the standard data are available from the Weather Bureau Airport Station (WBAS on Figure 1). At Hazelwood and Lindberg High Schools and at a Police radio station are located aerovanes plus hygrothermographs in standard shelters. On the KMOX tower downtown are located the following: at the 486-ft level an odometer, aerovane, and thermohm; at 393 ft an odometer; at 249 ft an odometer, aerovane, and thermohm; and at 121 ft an odometer, aerovane, and thermohm, and a bivane will soon be added at this level. On the roof of the Federal Building downtown we have facilities for taking radiosonde observations, both the normal free-ascent type and also by means of a modified radiosonde (which is aspirated) lifted by a tethered balloon.

Figure 1.

We have 6 aerokites of about 100-cu ft capacity which can be used to lift the roto-rods. In a recent test we were able to raise 3 roto-rods on one of the aerokites. A WSR-57 radar is located at the Weather Bureau Airport Station. During each experiment involving the release of F.P. (fluorescent pigment), a tetroon is also released from the dissemination site, to float at about 500 to 1000 ft, and tracked with the radar. We are also getting a Barnes IR thermometer for use in the next series of experiments to make areal surveys over the city to map heat sources and sinks.

We have run 4 series of experiments, in May, July, and September, 1963, and April, 1964. Of the total of 18 runs, one was a dry run, two were aborts, and 15 gave usable results. At least 10 were under unstable conditions, 4 were under stable conditions, and one was under neutral conditions. So far, of course, the results are tentative. First, during unstable conditions, the mean wind indicated by the tetroon appears to be the actual transport wind, both in direction and speed. Second, the crosswind particle distribution statistics appear to be describable from the tetroon crosswind displacement statistics. Third, relative to results reported in the literature on lateral diffusion rates from continuous point sources in unstable conditions over open terrain, at distances < 2 miles the St. Louis dispersion rates are greater and at distances > 2 miles they are comparable. This applies only to unstable conditions, as we have far too few stable cases for comparison.

SO₂ DIFFUSION PROJECT

The purpose of this project, under Bruce Turner, is to extend to another city, St. Louis, with certain improvements, the diffusion modeling effort begun in Nashville, Tennessee (*J. Appl. Meteorol.*, Feb. 1964). Figure 2 shows the sampling net for the SO₂ sampling sites which will be available in St. Louis. There will be 40 24-hr SO₂ sampling locations; at 10 of these 2-hr SO₂ data will also be obtained. One of the greatest improvements over the Nashville study will be an increase in the sensitivity of the SO₂ analytical techniques by a factor of 5, to 7 $\mu\text{g}/\text{m}^3$ SO₂.

This study will operate for 3 months during the winter of 1964-5. In 1963-4, 29 stations operated for 3 months, December through February, on a preliminary or pilot basis. The SO₂ source inventory will be more comprehensive than for Nash-

ville because we can now get Census Bureau data on fuel use practices, domestic, residential, and industrial, by census tracts. The inventory work-up will be by square sections, 5000 ft on a side, shown in Figure 2, as in Nashville. We hope to take into account the effect of local sources more adequately than was done in the Nashville model, and the initial diffusion parameters for 2-hr intervals will be somewhat larger.

URBAN POLLUTANTS

The third project, which has been carried on by John Clark, is the development of a simplified model for estimating urban pollutant concentrations. The purpose is, with use of Continuous Air Monitoring Program data for validation and evaluation, to develop a diffusion model which can be worked out by means of a desk calculator to estimate the time variation of an air pollutant at a given point. This is quite similar to the pioneering work which Frenkiel undertook in Los Angeles some years ago. The source inventory methodology has been developed by other units of our Laboratory, and it took only about 10 weeks to work it up for Cincinnati. For verification we are using data from the CAMP station located in the center of Figure 3. This work was reported by Clark at the 1963 APCA meeting in Detroit and will be published in the *Journal of the Air Pollution Control Association*. Standard Weather Bureau meteorological data are utilized in this model, and diffusion parameters are determined from Turner's interpretation of Pasquill's stability classification system in terms of cloud travel time rather than distance.

The results to date are as follows: For SO₂ (24-hr data) the correlation coefficient between computed and observed concentrations is 0.71, and for oxides of nitrogen it is 0.67. The model has been tested for applicability at another location in Cincinnati, Kettering Laboratory, where the correlation coefficient between computed and observed values of oxides of nitrogen was 0.81, a considerable and encouraging improvement. No SO₂ data were available at Kettering. It is gratifying that the same model seems to work for SO₂ and the oxides of nitrogen, since the major sources of the two are quite different, the SO₂ coming primarily from elevated sources and the oxides of nitrogen primarily from automobiles and other ground-level sources. We are going to try to apply this model to

Figure 2. Interstate air pollution study, St. Louis-East St. Louis.

Figure 3.

Washington, D.C., and a source inventory is now being made.

VERTICAL EXCHANGE COEFFICIENTS

My own project is concerned with investigations of the magnitude of the effective vertical exchange coefficients for heat and aerosol matter over a city. The experimental arrangements are such that we take helicopter soundings of atmosphere turbidity and temperature upwind of Cincinnati up to heights of about 2000 ft. When a sounding is about half finished, a tetroon is released at the site to float at between 500 and 1000 ft. After completing its sounding, the helicopter follows the tetroon, i.e., follows the motion of air as it goes over Cincinnati. When the tetroon arrives over one of our 35 or 40 locations over Cincinnati which has been cleared beforehand so that the helicopter can be landed (in "dirty" air), another sounding of both temperature and turbidity is made. The change of turbidity at comparable levels is proportional to the vertical flux of aerosol through the level per unit time. From the turbidity soundings the verti-

Table 1					
Frequency Distribution of $K_A(z)$					
Height, m	$K_A \times 10^{-5}, \text{cm}^2/\text{sec}$				No. of observations
	0.1 to 5.0	5.1 to 10.0	10.1 to 15.0	>15.0	
600	4	3	2	0	9
500	5	3	1	1	10
400	9	3	1	1	14
300	9	5	1	1	16
200	11	8	1	1	21
100	11	8	1	1	21

Comparison of $K_A(100)$ and $K_H(0)$		
Experiment No.	$K_A(100) \times 10^{-5},$ cm^2/sec	$K_H(0) \times 10^{-5},$ cm^2/sec
3	4.3	0.4
4	6.5	0.2
8	0.5	0.2
10	1.2	3.8
17	0.9	0.8
20	2.9	2.6
21	0.7	0.7

cal gradient of aerosol material can be determined. Hence, division of the flux by the gradient gives the effective vertical coefficient for aerosol. From the temperature soundings, we can also determine the heat flux at the surface of the earth and the vertical gradient of potential temperature. Again, division of the flux by the gradient gives the vertical exchange coefficient for sensible heat.

The results are shown in Table 1 in terms of frequency distribution of the K_A at 100-m levels. In the majority of the 21 observations, the K_A range between 0.5×10^5 and $5.0 \times 10^5 \text{ cm}^2/\text{sec}$. In one anomalous case K_A was about $40 \times 10^5 \text{ cm}^2/\text{sec}$. The K_A 's generally decreases with height. These cases are all under unstable conditions. According to the textbooks, the K_A are supposed to increase according to the $\frac{1}{2}$ power of the height under unstable conditions, but these cases are outside the range of the textbook conditions.

K_A and K_H are compared in the lower part of Table 1. We were able to determine the K_H in only 7 cases; in the other cases the heat flux could not be uniquely determined from the soundings. Considering that we are dealing with effective vertical exchange coefficients, and that the methods of determining the K_A and K_H were completely inde-

pendent, I think the agreement is really quite remarkable. The experimental data indicate that we are now able to get direct information on what the proper order of magnitude of these effective coefficients should be over urban areas.

DISCUSSION

ALLEN: How high did you fly the samplers in the St. Louis vertical studies?

McCORMICK: At 350, 250, and 150 ft. We can go as high as 2000 ft over St. Louis, however.

MOSES: Is there any other country where studies of urban diffusion are being made?

PACK: No tracer tests that I could find out about. A number of people are trying to use sources of opportunity - sulfur dioxide and so forth. The Viennese are doing quite a bit of work on turbidity, very similar to McCormick's. Unless it is being done in Russia, I am not aware of European controlled tracer studies in an urban environment.

SLADE: Is there any comparative urban work, similar to the St. Louis experiment, showing the meteorological differences between cities as compared to the diffusion differences?

McCORMICK: None that I am aware of.

HOLLAND: You mentioned some differences between the diffusion at St. Louis and over level country. Do you find corresponding differences in the wind traces showing horizontal wind direction?

McCORMICK: That has not yet been investigated.

The Meteorology Program at the Air Force Cambridge Research Laboratories

DUANE A. HAUGEN

I will describe several different programs of the Boundary Layer Branch of the Air Force Cambridge Research Laboratories.

OCEAN BREEZE-DRY GULCH PROGRAM

The Ocean Breeze-Dry Gulch program, which Mr. Fuquay mentioned yesterday, has been described in a recent two-volume AFCRL Research Report, the first volume of which is a joint AFCRL-Hanford publication and contains descriptions of the experimental techniques and the experimental data. The second volume contains a description of the analysis and engineering effort at AFCRL accomplished in answer to the TITAN II "toxicity problem" at Cape Kennedy and Vandenberg AFB. Until a month or so ago, Volume II was restricted in its distribution, but we are now permitted to distribute it to anyone interested.

The Ocean Breeze-Dry Gulch program was motivated by the fact that the TITAN II missile contains toxic fuels. Range safety officials had ruled that the missile could not be launched or fueled under on-shore wind conditions in order to preclude air pollution incidents which could arise from launch-pad accidents. The amount of time of off-shore winds at Cape Kennedy is so small that the TITAN II test schedule was greatly restricted. It was then decided that a "quantitative" determination of safe launch conditions be made by the TITAN II Weapons System office, at which time Range Safety would reconsider its "off-shore wind" ruling. It was at this stage that we at AFCRL were asked to work on this problem.

We were asked to provide an "operationally useful" solution to the problem that could be used on a 24-hr basis, 7 days a week, as a tool for Range Safety people to decide whether the operation would be safe within predefined limits. We felt that there was a need for actual diffusion experiments both at the Cape and at Vandenberg and a need for a continual monitoring system which would monitor the meteorological conditions which we felt to be important.

The diffusion experiments were accomplished only by being able to work with the people at the Hanford Laboratories, Richland, Washington, who really did an excellent job. We alerted them about the problem in late November of 1960, and asked for their assistance. By May 1961, the diffusion course had been completely instrumented at the Cape, all the field equipment had been bought, including the engines and vacuum pumps, and a complete course had been laid out for conducting the experiments.

What I would like to discuss is the type of prediction equations that we came up with and some of the reasons for the approach we took. We wanted to start out with essentially the Gaussian diffusion model, but we wanted to go a little further than that if possible in order to provide the type of solution required. As usual, with this approach, one starts with σ_y as a function of $\sigma(\theta)$ and σ_z as a function of $\sigma(\phi)$ where σ_y and σ_z are the standard deviation of concentration distribution about the centerline in the horizontal and vertical, respectively; $\sigma(\theta)$ is the standard deviation of wind direction fluctuations; and $\sigma(\phi)$ is the standard deviation of wind elevation angle fluctuations.

We were a little reluctant to measure something like $\sigma(\phi)$ in an operational system and decided to use instead a bulk parameter of some sort to indicate thermal stability. Furthermore, we made the decision even before we conducted any experiments that we would try to develop an empirical prediction equation using statistical techniques. We started by choosing as predictors $\sigma(\theta)$; $\bar{U}$, the mean wind speed; and a stability ratio for the bulk stability parameter. The predictand was the normalized centerline concentration where normalization was accomplished as usual by using the mean wind speed and the source strength.

We soon had to abandon this approach, primarily because we had wind speed entering in three places: as a normalization parameter of the predictand, as a predictor, and as a parameter in the stability ratio. We therefore embarked upon a statistical analysis using $\sigma(\theta)$, ΔT , and $\bar{U}$ as simple

predictors. Normalization was accomplished only by the source strength. The thought was to let the actual statistical techniques determine the relative importance of the meteorological parameters. (The bulk stability parameter, ΔT , was defined to be the temperature difference between 54 and 6 ft.)

We began our analysis by taking one parameter at a time in order to determine the relative importance of the various parameters. The most important parameter, not surprisingly, is the downwind travel distance X . The data we used are the 69

Prairie Grass experiments, with emission time of 10 min, SO_2 as the tracer, and travel distance up to $\frac{1}{2}$ mile; the 76 Ocean Breeze experiments from Cape Kennedy; and the 109 Dry Gulch experiments from Vandenberg AFB. We divided these data into two sets, using as a dividing parameter the stability index or ΔT , which gave us two data samples of 321 values of normalized centerline concentration, C_p/Q , in each sample. Prediction equations were developed on one data set and tested on the other.

Figure 1. Observed vs predicted C_p/Q as a function of distance.

Our results are summarized in Figures 1 through 7 which are scatter diagrams of observed vs predicted values of C_p/Q . The dashed line in the center of each diagram represents perfect prediction. The two lines on each side of it show a variation by a factor of 2; i.e., the predicted value within 50% to 200% of the observed value, and the outside set of lines is for a factor of 4, where the predicted value is between 25% and 400% of the observed value.

We used a factor of 2 and a factor of 4 to test the prediction equation accuracy because we found the normal way of trying to choose the best equation of fit by percent reduction of error did not work very well for our purpose. The reason it did not work very well is that the linear regression analysis techniques we used were applied to the logarithms of the predictors and the predictand, whereas we were interested in the prediction accuracy of the quantity C_p/Q . The nonlinear trans-

Figure 2. Observed vs predicted C_p/Q as a function of distance and ΔT .

formation of logarithms and antilogarithms led to many frustrations in using percent reduction of error as a goodness of fit parameter. Such things as negative percent reductions, for example, occurred on occasion, although no significant differences could be found in scatter diagrams with negative and positive percent reductions. We therefore chose, somewhat arbitrarily, to use percent of cases observed versus predicted within a factor of 2 and a factor of 4 as a criterion to decide

which equation would be the most operationally valuable and most operationally suitable.

Figure 1 is a scatter diagram using only distance as a predictor. Note that 36% of the cases fall within a factor of 2 and 78% within a factor of 4. This figure is interesting in that it provides a statistical summary of how well one can do with no meteorological predictors at all. It gives, in a sense, the base from which improvements must be made by adding meteorological parameters.

Figure 3. Observed vs predicted C_p/Q as a function of distance, ΔT , and $\sigma(\theta)$.

The meteorological parameter we found of most importance in trying to improve prediction accuracy is ΔT , the bulk stability parameter. This parameter is unquestionably much more important than either $\sigma(\theta)$ or $\bar{U}$. In fact, under many conceivable operational circumstances, a stability parameter could be the only meteorological parameter required for quantitative prediction of C_p/Q . Figure 2 is the scatter diagram using ΔT as the only meteorological predictor. The number 10 is

added to ΔT to prevent raising a negative number to a power. We now have 65% of the cases within a factor of 2 and 94% within a factor of 4.

The next most important meteorological parameter was $\sigma(\theta)$. Use of ΔT and $\sigma(\theta)$ as predictors gives the results summarized in Figure 3. We now have 72% of the cases within a factor of 2 and 97% within a factor of 4.

I might emphasize that in the course of this analysis we used a multiple regression program

Figure 4. Observed vs predicted C_p/Q as a function of distance, ΔT , $\sigma(\theta)$, and $\bar{U}$.

Figure 5. Observed vs predicted C_p/Q as a function of distance, ΔT , and $\sigma(\theta)$; Prairie Grass independent data test (see Figure 3 also).

which took each predictor in turn, so when I say ΔT was best, we had also tried using $\sigma(\theta)$ as the only meteorological predictor, and then only $\bar{U}$, only to conclude that ΔT contributed most to prediction accuracy.

Figure 4 is the scatter diagram obtained using all three meteorological predictors, ΔT , $\sigma(\theta)$, and $\bar{U}$. It shows no improvement at all. We now have 71% of the cases within a factor of 2 and 96% within a factor of 4. Our results were actually slightly (albeit not significantly) better using only ΔT and $\sigma(\theta)$ as predictors. We were surprised and puzzled by this result and offer the following explanation.

In addition to the diffusion experiments of the Cape and Vandenberg, there were also data collection efforts to measure ΔT , $\bar{U}$, and $\sigma(\theta)$ four

times a day at 6-hr intervals. This observational program was carried out between the series of diffusion experiments and provided data for periods of several months. Analysis of these data disclosed, among other things, a very systematic, although complex, relationship between $\bar{U}$ and ΔT . Thus, with the use of ΔT as a predictor, no increase in prediction accuracy was obtained by adding $\bar{U}$ as a predictor. We found no such relationship between ΔT and $\sigma(\theta)$, which is consistent with an improvement in accuracy when $\sigma(\theta)$ is added as a predictor.

I would like to comment on another feature of these data, viz., the value of the index on X , the downwind travel distance. Note that in Figures 2 through 4, it has a value of about -2.0 . Before we

Figure 6. Observed vs predicted C_p/Q as a function of distance, ΔT , and $\sigma(\theta)$; Ocean Breeze independent data test (see Figure 3 also).

had conducted all the experiments at Cape Kennedy and Vandenberg, we developed a prediction equation using only the Prairie Grass data. One approach we took at this time was to characterize the Prairie Grass data as very stable, moderately stable, moderately unstable, and very unstable, and then to use only $\sigma(\theta)$ and X as predictors for each stability category. We found that the index on X changed as one would expect. For the very stable category we obtained a value of -1.4 which varied systematically with stability to a value of -2.25 for the very unstable category. When we took all the Prairie Grass data and used ΔT and $\sigma(\theta)$ as predictors, we obtained a value of the index of about -1.8 , the value normally found for the near neutral stability condition. Thus, with one regression equation fitted to all the Prairie Grass data, we obtained an index which can be interpreted as saying that the neutral condition represents the average of all conditions encountered during Prairie Grass. However, we find that with the data presented in Figures 2 through 4 the value of the index on X is near -2.0 . This, we feel, is a direct result of the bias which exists in the Ocean Breeze and Dry Gulch data towards experiments conducted under predominantly unstable conditions.

Figure 7. Observed vs predicted C_p/Q as a function of distance, ΔT , and $\sigma(\theta)$; Dry Gulch independent data test (see Figure 3 also).

Figures 5 through 7 show what sort of accuracies we obtained for the three individual sets of data. Figure 5 shows the Prairie Grass data. Prediction accuracies are within a factor of 2, 69% of the time; a factor of 4, 96% of the time. Figure 6 shows the Ocean Breeze data. Here we have 79% of the cases within a factor of 2, 97% within a factor of 4. Figure 7 shows the Dry Gulch data, with 76% of the cases within a factor of 2, 97% within a factor of 4.

Although it is not noted explicitly on these graphs, in our computation of $\sigma(\theta)$ we used a "running average" 15-sec filter, to filter out periods of 15 sec or shorter before computing $\sigma(\theta)$. We found in a rather detailed correlation study of what filter width to use that it really made very little difference what the value was, anywhere from something like 8 sec to a 64-sec or 128-sec filter. We chose a filter width of 15 sec, therefore, for reasons of computational ease in the computer-controlled meteorological data acquisition and processing system installed at the Cape and at Vandenberg.

SAND STORM PROGRAM

The SAND STORM program, under the direction of Lt. Colonel John Taylor, is in the process

of being finished up. This effort is related in part to a problem that McCormick mentioned, having to do with testing rocket motors having beryllium fuel additives. A number of people in the country are developing research rocket engines of this type and would like to test them. Since the rocket exhausts are toxic, the Air Force Rocket Propulsion Laboratory at Edwards AFB is considering establishing a national test range for testing all research engines that are developed using toxic fuels and/or having toxic exhausts.

The SAND STORM experiments were conducted between March and October 1963 with actual rocket motors and sampling of the exhaust products downwind. About 45 experiments were conducted, all under unstable conditions. The firing time of these motors is in the order of 2 or 3 to 5 sec, thus providing essentially an instantaneous volume source. I will not discuss the experimental details of the program, since Colonel Taylor will soon have finished the preliminary report, and the final report is expected in late fall, 1964.

It should perhaps be emphasized that these experiments were conducted over a desert floor under unstable conditions which were conducive to low-frequency vertical oscillations in the turbulent wind field; i.e., the so-called convective currents. These oscillations of course carried the puff or volume of exhaust products through vertically meandering paths, whereas the sampling course was laid out in a horizontal plane with all samplers 1.5 m above ground. For this reason primarily, use of

the Smith-Hay approach to handle the instantaneous source in terms of actually predicting or specifying centerline concentrations on the ground was very unsatisfactory, and what has been done instead is simply to make probability statements as to what was observed.

Figure 8 shows the type of result found to be most useful for the SAND STORM data. The centerline exposures, normalized by source strength, are plotted against downwind travel distance. The curves on the graph are regression lines which have been fitted to the 50, 75, 90, and 95% level of observed normalized centerline exposures at each travel distance. This summary of our SAND STORM results is submitted as the most realistic and meaningful summary of low-level diffusion data that can be obtained for an instantaneous volume source.

VERTICAL COMPONENT OF WIND

In August of 1963, Dr. Kaimal of our group took four continuous-wave sonic anemometers down to the TV tower near Dallas, Texas, where he measured the vertical component of the wind at four different levels, 150, 450, 750, and 1050 ft. Nine runs were made during this experiment with joint airplane "fly-bys" with a plane controlled by Flight Dynamics Laboratory, Wright-Patterson AFB, Dayton, Ohio, also instrumented to measure the vertical component of the wind. These latter runs were of 30-min duration. Dr. Kaimal

Figure 8. Probability levels of normalized centerline exposure vs distance, project SAND STORM.

obtained a total of 29 runs, separate from the joint airplane experiments, with sampling times varying from 1 to 10 hr for a total of 50 hours of data. These data are now in the process of being analyzed. Dr. Kaimal plans to present selected portions of this work at the AMS Meeting on Micrometeorology at Salt Lake City in October 1964.

COMPUTER-CONTROLLED MICROMETEOROLOGICAL OBSERVATION SYSTEM

Another current activity in our group, the so-called Kansas expedition, is an attempt to put together a computer-controlled micrometeorological observation system.

All the electronic equipment will be housed in a truck trailer; only a tower will be permanently located at the site. The objectives and methods are summarized in Table 1. We will measure the usual things: wind speed and direction profiles, temperature profiles, moisture profiles. We want to measure net radiation profiles and soil temperature profiles, the normal micrometeorological parameters. On our first expedition we expect to be prepared to measure only wind and temperature profiles. The fast-response measurements of wind fluctuations at three levels on the tower we hope to be able to do with sonic anemometer equipment being developed by Dr. Kaimal. Measurements of Reynolds' fluxes of momentum are all that we are planning to attempt on the first expedition.

The computer system has been designed on paper for some time. We have put a large portion of the system together and are now waiting for delivery of certain key components. We do not have a site as yet, but we think it will be in southwestern Kansas, near Liberal, which is about 5 miles north of the Oklahoma border. We are interested in joint experiments, with some qualifications, and anyone interested should contact us as soon as possible.

DISCUSSION

CRAMER: The disappearance of $\bar{U}$ from the equations probably is as much due to the fact that it is highly correlated with $\sigma(A)$, variant of instability, as that it is related to ΔT .

HAUGEN: We found a very strong relationship between $\sigma(\theta)$ and $\bar{U}$ with the Prairie Grass data. However, we found no such relationship with the Ocean Breeze and Dry Gulch data. I think this might be partly because both at the Cape and at Vandenberg most of the data were

Table 1

Description of Micrometeorological Observational System

Purpose: Study of transport of heat, momentum, and water vapor to and from the earth's surface.

Quantities to be measured:

1. Wind speed and direction profiles.
2. Temperature profiles.
3. Moisture profiles.
4. Net radiation profiles.
5. Soil temperature profiles.
6. Fast-response (1 to 50 cps) measurement of wind, temperature, and moisture fluctuations at 3 levels on tower.
7. Advection of heat, momentum, and water vapor over a 1-mile grid.

Levels of instrumentation on tower:

1. 0.25, 0.5, 1.0, 2.0, 4.0, 8.0, 16.0, and 32.0 m for profile measurement.
2. 6.0, 12.0, and 24.0 m for fast-response instrumentation.

Scan rate of system:

1. Once per sec for profile data.
2. 10 to 50 times per sec for fast-response instrumentation.

Total number of data channels: 96

Basic concepts of system operation:

1. Direct digital data-logging on magnetic tape.
2. Analog recording on strip charts.
3. Error log on IBM typewriter.
4. Calibration checks under computer control.

much biased toward one meteorological situation, the sea-breeze situation. In other words, we did not get a very wide variation in $\bar{U}$ and $\sigma(\theta)$ for the experiments.

HOLLAND: The probability lines at Edwards all seem to have the same slope. Was this done on purpose, or did it just turn out that way?

HAUGEN: What we did was to take the 50% level, for example, at each of the distances and fit a straight line, the best fit straight line, through the various arcs. They do not really have the same slope; they are all a little different.

HOLLAND: What kind of slopes did you get?

HAUGEN: Something in the order of -1.50 .

CRAMER: The problem you have at Edwards with convection being very active would be fairly difficult because one is not really dealing with a system that is regular in the sense that convection itself is time and space dependent. I think you have indicated this. As I recall from our experience in Prairie Grass and other tests, in this situation we do expect extreme variability near ground level, and occasionally, certainly, the cloud does lift off the ground, in fact, may disappear entirely; and I think the low values you have measured probably are real.

HAUGEN: We think they are real, but we do not have much confidence in their experimental accuracy.

CRAMER: Of course they are low, and operationally I don't know what this means to you, but there probably is

no problem. But I think it is a very difficult modeling situation. In terms of working out an operational problem, the thing you have to worry about is that if you set your predicted level too high or too low, there is always the question of whether there is a comparable down-wash of material in concentrated blobs. We know that the up-drafts take it and disperse it very widely in the vertical. Apparently you haven't found indications of, say, hot spots of material.

HAUGEN: We have, indeed, but this was rather rare, perhaps because the source was "thermally hot." The exhaust product of a rocket contains a lot of thermal energy that causes it to rise quickly, and by the time it leveled out to some equilibrium with the atmosphere, the arc spacing was wide enough that we may have missed some of the things that were there. From the point of view of answering the problem for the Edwards people as to whether or not firing an engine of a certain size was feasible within the amount of terrain that they had to work with, I think we answered this in a limited sense. We are able to say that the contamination is no worse than a certain level, but it is not particularly satisfying to me personally to be able to say no more than that.

PACK: In terms of possible low frequency contributions during convection, will Taylor be able to correlate the

bivane data with the firing time to see whether the rockets went off with any consistent departure from the horizontal? That is, were they fired when there was a consistent up-draft for a few seconds or a consistent down-draft, so that one could find some correlations?

HAUGEN: I do not know that he has done this. I doubt it, because the machine computations were made at Edwards. I do not think he has seen the basic data charts.

PACK: Do you have the feeling that this might be significant?

HAUGEN: I think it is probably worth looking into. I was surprised to see the amount of energy these little engines could have. I suspect that as a result of this energy, the thermal impulse to the exhaust overcomes the particular vertical motion field at the time of firing.

MOSES: You show the regression equation for C_p/Q , where you took all three sets of data, Dry Gulch, Ocean Breeze, and Prairie Grass. Did you use two sets of data to calculate your regression equation and use the third set as a test?

HAUGEN: No. The data of Prairie Grass, Ocean Breeze, and Dry Gulch were divided up into "Dry Prairie Breeze" and "Ocean Gulch Grass." Each data set was divided in half. Then half of each set formed one data sample; the other half of each set, the other data sample.

The Meteorological Program at Fort Huachuca

JAMES F. APPLERY

Our program has developed gradually since its inception in 1958, when the meteorology group at Fort Huachuca was assigned responsibilities for basic research in micrometeorology for the Army. The objective of the program is to obtain new knowledge which will contribute to a better understanding of the physical processes of the lower atmosphere and the earth-air interface. Our ultimate goal is to develop physical and mathematical models which will predict the four-dimensional state of the atmosphere in the boundary layer. Granted, this is a rather ambitious goal. If we had a perfect model which contained all the terms, it would be a monster. However, we feel that if we learn enough about the processes, we can decide which ones can be safely omitted for particular applications, and then we can reduce the equations to a usable system.

Our over-all approach to the problem can be stated briefly as follows: First, we recognize that our main problem is understanding the processes of the atmospheric boundary layer. To solve any equations representing these processes, we must specify initial and boundary conditions. The initial and upper boundary conditions we leave to the larger-scale meteorologists. We are especially concerned with factors which control energy, mass, and radiative transfer in the boundary layer, and with the energy balance processes of the surface and subsurface which determine the lower boundary condition for the atmospheric processes.

The larger share of the research is conducted by external groups through contracts, grants, or cross-service orders. These groups specialize in the single or interacting processes which best fit their particular interests and capabilities. Internally, we have a small group which monitors the external effort and works to integrate all the results. Once each year we have meetings similar to this one, to exchange information. We feel that this is a necessity because we have several disciplines involved. It took two such meetings for the meteorol-

ogists, engineers, soil scientists, and plant physiologists to understand each other. First let us look at our external effort. The tables will show where each group is working, and I will discuss very briefly some of the experiments they are conducting.

Probably the most extensive research is the work at the University of Wisconsin under Dr. Lettau, who has a large group of graduate students (Table 1). The principal effort is the study of the effects of horizontal variation in the surface and of its effect on the atmospheric boundary layer structure. They are carrying on theoretical investigations of the wind structure and are testing theoretical models, using synoptic and experimental data. Most interesting is their experimental program. Lake Mendota, adjacent to the campus, is a convenient site for collecting data. In summer, they cruise the lake on a specially instrumented barge, and in the winter these rugged individuals vary the aerodynamic roughness of the frozen lake surface by using various arrays of bushel baskets and Christmas trees. Also of interest is the airborne program of measuring the vertical velocities in the boundary layer and the albedo and temperature of the underlying surface. They are also trying to add an auxiliary system to obtain the heat flux and the Reynolds stresses with the airborne system.

The MIT Round Hill group has been active in the field of turbulence measurement for several

Table 1

TITLE: Studies of the Effects of Variations in Boundary Conditions on the Atmospheric Boundary Layer Structure

CONTRACTOR: University of Wisconsin

PRINCIPAL INVESTIGATOR: Dr. Heinz H. Lettau

PURPOSE: To develop a theory relating the behavior of the atmospheric boundary layer to definable changes in the physical characteristics of the underlying earth's surface.

Table 2

TITLE: Partitioning of Turbulent Energy in the Lower Layers of the Atmosphere

GRANTEE: Massachusetts Institute of Technology

PRINCIPAL INVESTIGATORS: Dr. H.E. Cramer and Dr. F.A. Record

PURPOSE: To establish the characteristic features of energy spectra associated with turbulent transport of characteristic air properties and momentum, sensible heat, and water vapor for the layer extending from 10 to 100 m above the ground.

Table 3

TITLE: Energy, Momentum, and Mass Transfers Near the Ground

GRANTEE: University of California (Davis)

PRINCIPAL INVESTIGATORS: Dr. F.A. Brooks and Mr. W.O. Pruitt

PURPOSE: To investigate simultaneous relationships between energy and mass transfers from below the earth's surface up to 8 m above the surface under various conditions of atmospheric stability.

Table 4

TITLE: Wind Tunnel Research

GRANTEE: Colorado State University

PRINCIPAL INVESTIGATORS: Dr. J. Cermak and Mr. E. Plate

PURPOSE: To study the structure and dynamics of turbulent shear flow in a wind tunnel under a variety of carefully controlled experiments, and to interpret wind tunnel results in terms of structure and behavior of the atmospheric boundary layer.

years (Table 2), and we are planning to publish the energy spectra computations from a comprehensive set of their observations. These include some 75 one-hour observation periods with temperature and wind component measurements taken approximately once a second, simultaneously at two levels. Fifty runs were taken at 16 and 40 m, and the remainder were divided between 50 and 150 ft and 150 and 300 ft.

The objective at the University of California at Davis (Table 3) is to try to pin down some of the fundamental relationships of the transfer coefficients for heat, water vapor, and momentum. They have a large instrumented site and are approaching the problem by replicate measurements. The installation includes several lysimeters, which mea-

sure the evaporative flux, and a shear stress lysimeter for determining the momentum flux directly. An Australian eddy correlation meter is being obtained for an independent measure of the sensible heat flux. Profiles of wind, temperature, and humidity, as well as soil heat flux, and radiation measurements, are collected for aerodynamic computations; and soil heat flux and radiation measurements are made to round out the energy balance computations. These independent measurements of the fluxes allow them to check the many theories which have been reported on flux-gradient relationships.

Table 4 refers to our wind-tunnel research. Anyone who has attempted a micrometeorology program knows that Mother Nature is not an ideal laboratory assistant. Seldom do all the desired experimental conditions appear at one time. Early in our micrometeorological program we recognized the desirability of conducting controlled laboratory-type experiments; we therefore sponsored the construction of a large low-speed wind tunnel at Colorado State University, Fort Collins, Colorado. After the usual construction delays and another year to calibrate, the tunnel is ready for operation. This is a recirculating-type tunnel, though it can be modified for open-circuit operation. The test section is nearly 90 ft long with a 6×6-ft square cross section. The very long test section was designed to achieve very thick boundary layers. With a roughened floor, boundary layer thicknesses up to 2 ft have been attained. For micrometeorological experimentation, the outstanding feature is the climate control. Air temperatures can be controlled to within $\pm 1^\circ\text{C}$ over the range 0° to 90° . A 40-ft section of the floor can be controlled to within $\pm 2^\circ$ over the range from -10° to $+180^\circ\text{C}$. Air velocities can be controlled to within ± 1 m/sec over the range from 1 to 40 m/sec.

The Army and other government agencies are sponsoring a variety of wind tunnel tests. In particular, our micrometeorological program is sponsoring studies of the basic structure of the turbulent boundary layer. This includes work with stable, neutral, and lapse stratifications and flows over smooth and rough boundaries. Since most of the effort to date has gone into the development of instrumentation and the design of experiments, I have no results to report at this time. However, a temperature probe with response characteristics

Table 5

Cross Service Order With Agricultural Research Service (USDA)

INSTALLATION	SUBJECT
Plant, Soil and Nutrition Laboratory Ithaca, New York Dr. E. Lemon - Mr. W. Covey	Turbulent Transfer Characteristics of Vegetative Canopies
U.S. Water Conservation Laboratory Tempe, Arizona Dr. C. van Bavel - Dr. L. Fritschen	Surface Energy Balance Studies
U.S. Salinity Laboratory Riverside, California Dr. L. Richards - Dr. W. Gardner	Effect of Soil Properties and Plant Characteristics on Water Transfer From Earth to the Atmosphere
Southern Piedmont Soil Conservation Field Station Watkinsville, Georgia Dr. A. Bertrand - Dr. J. Pallas	Plant Transpiration Studies

Table 6

Small Grants

GRANTEE	SUBJECT
University of Washington Dr. J. Businger	Study of Convective Elements
University of Wisconsin Dr. C. Tanner	Direct Measurements of Reynolds Stresses
University of California Dr. D. Nielsen	Physical Processes of Soil Water Movement
University of Arizona Dr. E. Titt	Application of Numerical Method in Micrometeorology
University of Hawaii Dr. M. Estoque	Boundary Layer Model Solutions

comparable to those of a hot-wire anemometer has been developed.

Perhaps the most serious criticism of wind tunnel programs in general is that it has not yet been quantitatively demonstrated that wind tunnel results are applicable to the atmosphere. But we are pressing ahead with this program because, if modeling criteria can be developed, we will save an enormous amount of time and money on micrometeorological experiments.

Since we found at the start of our program that the Agricultural Research Service of the U.S. Department of Agriculture is interested in many of the same problems with which we are confronted for the lower boundary condition of the atmospheric model, some aspects of the external program are handled under a cross-service order with the Department of Agriculture (Table 5). At

Ithaca, New York, they are making measurements and analyzing data from different vegetative canopies. Their objective is to determine the representative momentum, moisture, and heat source or sink distributions within the canopies. At the U.S. Water Conservation Laboratory, Tempe, Arizona, they are attempting to establish relationships between the moisture balance of vegetation and pertinent meteorological parameters. Dr. Gardner at the U.S. Salinity Laboratory, Riverside, California, is working on the development of an over-all descriptive theory of water transfer from the earth to the atmosphere, based on measurable characteristics of the soil, plants, and environment. At Watkinsville, Georgia, a critical evaluation is being made of the role of plants in regulating the transfer of water from the soil to the atmosphere.

We have a number of small grants, as shown in Table 6. Dr. Businger at the University of Washington is making case studies of convective elements using a sonic anemometer. His equipment is a portable continuous-wave type anemometer with a path length of 50 cm. So far, he has collected data at Round Hill, Fort Huachuca, several locations in the State of Washington, and over the Indian Ocean. He will use data from these case studies to develop a model to describe the behavior of the convective elements in the lower atmosphere.

Dr. Tanner, at the University of Wisconsin, is developing a system to measure the Reynolds stress directly. Essentially, he is coupling the anemoclinometer with sensitive pressure transducers to measure the small pressure difference between the ports.

At California, Dr. Nielsen is comparing experimental data with present mathematical descriptions of the processes involved in soil water movements and hopes to develop a more comprehensive and accurate model of the problem.

In our internal effort we ran into trouble when we tried to use the cookbook approach to obtain numerical solutions to nonlinear partial differential equations. To help us out of this dilemma we had the University of Arizona give a course in numerical methods. Through this contact, Dr. Titt became interested in some of our problems, and we are now receiving help from him through a grant. He is working on the diffusion equation and the nonsteady-state boundary layer equations. He has just finished a computer program for the steady-state diffusion from an infinite line source. The method can handle arbitrary height distributions of wind speed and eddy viscosity and could be modified to account for the apparent growth of K with distance. The computer solution takes <3 min on an IBM 7020 to reach out to about 1600 m. A report on his approach with a series of solutions using wind speed and eddy viscosity profiles derived from our neutral steady-state model should be out shortly. These solutions show that the patterns are sensitive to the height of the source in relation to the maximum K and very sensitive to the ratio of the eddy viscosity and the wind speed in the element nearest the boundary.

Last on the external program is Dr. Estoque of the University of Hawaii. He has just joined the program and will undertake to update his work in

numerical solution of the boundary with relationships related to our program.

Turning to the internal part of our program, we have been working on solutions of equations for the boundary layer and the vegetative canopy, and some investigations have been made on the characteristics of turbulent fluctuations.

We have completed some numerical solutions for the distribution of wind in the planetary boundary layer, starting with the basic equations of motion modified to incorporate the mixing-length hypothesis of Prandtl. This relates the turbulent stresses to mean flow characteristics. For an adiabatic, barotropic steady-state boundary layer, we propose a mixing-length increase with height at a rate given by a constant, but with a linear feedback which prevents unbounded growth. This can be expressed by the differential equation

$$\frac{\partial l}{\partial Z} = k - \frac{l}{L}$$

At the top of the boundary layer, the flow behaves more like free turbulence than wall turbulence, so that one would expect a nearly constant mixing length; whereas down near the lower boundary it is more like wall turbulence, and the mixing length can be expressed as kZ . With these boundary conditions, the solution of the equation is

$$l = l_0 \left(1 - \frac{e^{-2Z}}{L} \right)$$

where l_0/L is equal to von Kármán's constant. This is the expression we use for mixing-length distribution. Figure 1 shows the comparison of this expression with the mixing length computed from the Leipzig wind profile, and the agreement is fairly good until the upper part, where the small velocity derivatives make the mixing-length computations rather useless.

Using this relationship, we evolved the set of equations for numerous boundary conditions. To find out which one of these solutions was applicable to the boundary layer, we invoked the idea of similarity. We assumed that the Reynolds number of the boundary is so large that there exists a universal equilibrium structure for turbulent eddies. We also assumed that the scale of eddies as measured by the mixing length is uniquely related to the gross scale or depth of the boundary layer. The limiting mixing length, l_0 , is adequate to characterize the eddy structure, and we selected the

Figure 1. Mixing-length profile.

Figure 2.

Figure 3. Wind velocity hodograph.

Figure 4. Relative eddy viscosity.

gradient wind level to characterize the gross scale of the boundary layer.

According to our notion of similarity, the ratio of the gross length scale of the boundary layer to the length scale of the eddy structure is a universal constant. To determine this constant, we resorted to the Leipzig wind profile, and with this constant the solution we believe adaptable to the boundary layer could then be selected, and from this solution we could determine relationships between the limiting mixing length and surface Rossby number, which is the geostrophic wind divided by the Coriolis parameter times the surface roughness.

We can examine some of the features of the model. Two of these are the angle between the surface wind and the geostrophic wind, and the drag coefficients. In Figure 2 these are shown as functions of the surface Rossby number, along with all the known observations that we could find.

Figure 3 shows in arbitrary units the wind spiral. The units at the top are Z/l_0 . To find the geostrophic wind for a given Rossby number, a line is drawn from the Z/l_0 to the origin. That is a unit geostrophic wind. Any wind along this spiral can be found by drawing an arrow from the roughness to the proper height.

Figure 4 shows the vertical distribution of the relative eddy viscosity. The maximum occurs at $\approx 1/5$ the height of the gradient wind level, and it is quite similar to the profile adopted by Rossby and Montgomery many years ago.

We have also studied the turbulent transfer of momentum within a vegetative canopy. We had hoped to develop the model to express the aerodynamic roughness effects in terms of height, density, and drag characteristics in an idealized canopy of

uniform vertical and areal distribution. Theoretical development indicated that the mixing length should be constant, and wind speed should be exponential with height. Using wind profile data collected in a mature cornfield, we solved for the mixing length and found it to be nearly constant. This has been confirmed with wheat by Lemon, Inoue, and Denmead. They also found that the wind was exponential with height. This was also confirmed at Wisconsin in the work with Christmas trees on the ice.

Of course, there is a lot of canopy data that does not agree, but we feel this is a start in the right direction, and the concept can be used in vegeta-

tion that is fairly uniform both vertically and horizontally.

We realize that the mixing-length hypothesis is in disfavor, since it is based on intuition rather than fundamentals, but the approach has been successful in some areas of turbulence, and it is the only practical approach to prediction models that we know of at this time.

DISCUSSION

VAUGHAN: What is L ?

APPLEBY: It is a constant. Note that in the Leipzig wind profile it turned out to be 80. It varies with the surface Rossby number, geostrophic wind, and latitude.

The Meteorology Program at Dugway Proving Ground

PAUL CARLSON

Dugway Proving Ground is an Army installation with the assigned responsibility of testing chemical and biological weapons systems and equipment, and we in meteorology at Dugway provide operational support to the Proving Ground and also conduct a program on turbulence and diffusion. Our primary responsibility in this area is to provide prediction capabilities for the weapons systems. The source geometries we work with are those associated with the chemical and biological weapons systems, and the requirements for these prediction techniques are for all terrain, vegetation, and climate situations.

We also have a modeling program at Dugway. The accuracy requirements for a complex terrain and vegetation program are not too stringent, as we are using forecasted meteorological inputs of the critical variables in the model for this program. For the in-house program the requirements for modeling are more stringent than with this program, and we are using measured meteorological inputs. The approach to the problem at Dugway is to provide testing economies by using prediction methods to reduce the number of tests required for evaluating aged weapons systems and to provide better information for test designs, and also to give us a better understanding of the diffusion processes. We feel – and we are getting a lot of support at Dugway – that the use of modeling can introduce good economies in the testing program.

We have two contractors working with us in the in-house program at Dugway: Geophysics Corporation of America, with Dr. Cramer as the project officer, and Metronics Associates, under Dr. Perkins.

Dr. Vaughan will describe an elevated line source problem at Dugway and an F.P. elevated line source modeling program. We use the F.P. technique in all our work, and it is the only trace system we are using now. We have done considerable work in this area in cooperation with Metronics Associates, and we are using the F.P. technique for all our model verification and model development work.

I would like to review briefly the complex training programs since 1960, on which reports have already been issued. The first is the Dallas Tower Program, carried out by Meteorology Research, Inc., utilizing the Dallas Tower that was instrumented by the Air Force Cambridge Research Laboratories. On this program, we made 38 elevated line source F.P. releases upwind of the tower. We augmented the tower instrumentation with 5 bivanes, a vertical sampling, and F.P. sampling on the tower, and we also employed a 30-mile downwind sampling line.

We sponsored, with NRI Research, Inc., doing the field work and the analysis, the elevated line source program in a variety of terrain situations. A downwind sampling system was employed with an F.P. vertical 100-ft temperature gradient and bivane measurements on the tower augmented with aircraft soundings and aircraft turbulence measurements. The sites for this work were near Corpus Christi, Texas; Cushing, Oklahoma; Tacoma, Washington; and in Nevada.

We supported work done by Bendix on tropical rain forest vegetative canopy. This was an elevated line source problem, with F.P. released upwind of a forested area. The site was in Colombia, South America.

Our present program includes four projects in the area of complex terrain and vegetation. The first is a mountainous terrain project, more of a flow problem than a diffusion problem. This work is being done at the 29 Palms Marine Base. An elevated line source is released upwind of a mountain-valley complex. Nine trials have been completed. We are using two colors and two levels of releases. The data samplers are being counted, and the data analysis has been started, but we have no results as yet.

The second is the deciduous forest project in Northern Minnesota, in which both the line source and the point source are used, with two colors at the point, one color being underneath the canopy, in the line of the canopy. We are testing with leaves in the summer and without leaves in

the winter in order to determine the effects of the leaf canopy. The winter phase has been completed. The samplers are being counted now, but the results are not yet ready to report. This work is being done by what was formerly General Mills in Minneapolis; their Air-Space Division has been bought by Litton Industries.

The third problem in the complex terrain area is an urban problem. An elevated line source is released upwind of an urban complex. The work is being done by Travelers Research Center under Dr. Hilst, and the site selected is Fort Wayne, Indiana. The first test is scheduled for this month. We have attempted some tests, but so far without success. We have three or four balloons, but the field operations are not simple. We have four verticle sampling balloons upwind and downwind of the city, and we were sampling both a rural area alongside the city and the urban complex itself, trying to determine the exact effects of the urban complex in this problem.

The fourth project is concerned with the problem of a point source in a tropical rain forest canopy. This work is being done by Mel Parr, the site selected is in the Wombat Swamp Area of South Carolina, and the testing is scheduled for next month.

Our plans for complex terrain studies for fiscal 1965 are in the shoreline complex area: offshore releases with modeling prediction; prediction of downwind dosages inland; and some work with the 10 to 100- μ particle size range, which will involve both fallout and diffusion.

Our in-house program at Dugway includes work on modeling problems and support to the installation. One program, started about 1½ years ago, concerns a turbulence climatology problem. We have collected data for about 9 months. We plan to operate a 300-ft tower with three bivanes for temperature gradient and wind profile.

Another in-house program just starting is a forecasting study utilizing the Barnes aircraft infrared radiometer. We are attempting to forecast local winds at our grids, with our higher source temperature and ground temperature measure-

ments, and attempting to forecast the drainage lines.

The third in-house matter I would like to discuss briefly is the modernization of the data collection portion of our instrumentation system. The system is quite large, as we have to characterize the meteorology over fairly large areas at the Proving Ground. With some sources we are interested only in a distance of 200 m or so, and with others, 40 miles or more. Since we have to maintain flexibility in the field, we are installing a computer controlled digital system with a radio-frequency link to bring the data into our building.

We have the capability of real-time computations. We now make calculations by hand for safety purposes, and we will be able to do this on a computer. We are hooking up our pilot balloon network to the computer to have the data calculated and recorded on magnetic tape.

We are using three displays: a vector display for the flow situation, which is scanned for the instantaneous measurements so that we know whether we are within the limits prescribed in the test plans for the meteorological variables; and a two-trend recorder that will also be computing, updating every 20 sec on each individual recorder of the *K* recorder system. We also have a typewriter output for air logging.

To indicate the speed of this system, it has a capability of 600 interrogations per second with the maximum configuration, utilizing six of our remote stations; and about 300 if we are using only one remote station. The system can be programmed for different data rates. We are planning to digitize our bivane data 10 times per second initially for some of our point source problems.

This system will be installed and operating by February 1965. We will have immediately only the basic programs for data receipt and for some of our real-time calculations and analysis problems. We plan to conduct many programs later on, but programming is a big job with this system.

We have a good field laboratory and a considerable amount of dosage data. We use all the tests at Dugway to improve our modeling.

Elevated Line Source Programs

LELAND M. VAUGHAN

The first nine tracer studies in this series have been published in a technical report available through ASTIA. Others have been summarized briefly in our Dugway monthly reports. Briefly, the test consists of approximately simultaneous releases from small aircraft, L-23 and L-20, of two colors of F.P., green and yellow. The two aircraft do not have the same speed, but they try at least to reach the center of the line at the same time at two different heights over a range from 100 to 400 ft. These tests have been conducted under a variety of stability conditions, with ground sampling at one-mile intervals out to 15 miles.

For vertical sampling of the aerial release, line samplers are mounted on a 300-ft tower and on 100-ft towers at four positions downwind, and balloon-mounted samplers are provided at these same positions. These vertical distributions are unique in providing so much vertical sampling. This is one of the things which may be of interest to others, because one can actually see the displacements of the centers of the clouds themselves in terms of the dosage patterns observed; and with two simultaneous releases, at least under moderately stable conditions such that the clouds are bounded by the sampling array, these displacements are well correlated. We have made efforts to match these displacements with the potential temperature contours, a cross section of potential temperature down the sampling line; when the trajectories are good enough, this gives a fair indication of where the centroid of the cloud will be.

Concerning the expansion of the cloud, which is of course essentially an instantaneous line source with a finite dimension, the estimates of the vertical standard deviation in the calibration trials average ≈ 20 ft at the 300-ft tower, which is ≈ 100 yards downwind from the release line itself. This would provide an opportunity to test the Smith-Hay equation, for instance, but in the initial stages we didn't have adequate turbulence measurements available. Initially, we suggested using the Beckman and Whitley standard direction vane, turned on its side. This gave us an indication but

not a good estimate of what the vertical σ should be, although later I did make some estimates based on the ranges reported for these fluctuations. In the initial nine trials, at least, the analysis was done on the basis of a diffusivity derived from estimates of the vertical heat flux, which, since it was not measured directly, I estimated from the integrated rate of change of temperature through the air. This estimate of diffusivity is subject to a considerable degree of uncertainty, and yet the agreement with observed diffusion was not too bad, considering the fact that it is an estimate of a long-term average dispersal coefficient which is to be correlated with diffusion from an instantaneous source.

In the later trials some bivariate measurements were made, at least up to 100 ft, and I have made some calculations using the computed σ values, and then from some of the spectral estimates obtained at Dugway I estimated the time and length scale of the turbulence. For those particular trials, the length scale was on the order of 30 m, and the time scale roughly on the order of 3 or 4 sec.

Probably the greatest value of this type of experiment using multiple tracers and extensive vertical sampling is the possibility of replicating trials. Instead of using two heights as we have been doing, one could run successive trials at the same elevation with different colored particles of a comparable tracer and at a little more cost get several diffusion tests at the same time.

The roto-rod samplers can be reversed as well. At Dugway they were not set up so that this could be done, because each one was mounted independently and could not be switched either by direct wire or otherwise; the whole unit was complete including power supply. In the later trials we had as many as 20 samplers on one balloon, quite an extensive array.

The above should give some idea of the type of work we are doing on elevated line sources. We have been analyzing these trials and will publish a more complete summary when we finish the analysis.

DISCUSSION

GIFFORD: Concerning the several attempts described at verifying or comparing experimental data with the Smith-Hay instantaneous source diffusion model for an inhomogeneous isotropic turbulence field, I suppose if you compared this model with the data that the data would have to be appropriate to such a field; the point is that this is not a minor restriction.

VAUGHAN: No. Certainly it isn't on this scale.

GIFFORD: It doesn't make sense to make all the attempts we do to characterize the inhomogeneity of the turbulence field and then to make a comparison of a homogeneous model with something that occurs in such a field.

VAUGHAN: I agree. This is an idealized model, and comparisons are rather restrictive on it.

BARAD: Now that Metronics and the Dugway people have had a number of years of experience with the roto-rod sampler, can you give us some idea of how accurate it is and how useful it is as a measure of the air concentration? What sort of variability would you get, for example, if you put two roto-rods into the same medium at the same concentration?

WEBSTER: After a series of experiments with the roto-rods, we have come to the conclusion that the roto-rod sampler is the more accurate device, at least for sampling in the 1 to 5- μ range compared with a number of filters. The procedure for checking the roto-rod is to set up an array of 4 roto-rod samplers completely surrounded with an array of about 9 membrane filters in fairly close proximity, and then conduct a series of competitive experiments. With a base material of closely known characteristics, as I recall, with one particular lot of material the roto-rod would show some 70% collection efficiency with a coefficient of variation of <10% over 96 sets of measurements. The membrane filter data collected simultaneously showed a coefficient of variation on individual dosages in the neighborhood of 14 to 15%. Because of the unexplainable variations in filter dosages, we have been concentrating on the filters themselves and trying to reduce this coefficient of variation, but our feeling is that the roto-rod operated in the proper manner is a much more accurate measuring device than the membrane filters.

SMITH: What did you use as the basis for the actual air concentration? Was the mean value derived from the

membrane filter used as the standard of true air concentration?

WEBSTER: We had a diamond-shaped cluster of 9 filters completely surrounding our roto-rod stoppers, and took the mean values from the 9 filters as the yardstick. We have also used groups of 3 and 6.

SIMPSON: Have you studied the wind variations in these 96 cases, comparing the samplers with roto-rods?

WEBSTER: Yes. The range of winds has been from about 2 to 3 miles/hr up to about 20, and so far we don't see any definite variation with that range of wind speed. We are planning to run a test on the ridge of a mountain to try to get a wind in the 35 or 40-mile/hr region. We had thought that possibly we would get a much bigger variation, but so far, generally, the higher the wind speed, the smaller the variation. It is practically constant over the entire range of wind.

CRAMER: Without knowing the geometry of your experiment, isn't there a possibility that because of the wider spatial separation of the membranes, you may be measuring a spatial variable factor?

WEBSTER: That is a good point, but tests with various ranges don't seem to give any basis for variation due to the spacing. Two filters right beside each other will sometimes show much greater variation than the roto-rod samplers on the outside of the array.

CRAMER: What do you conclude from that?

WEBSTER: There isn't an obvious differential in composition across the array.

RAYNOR: How does this 70% efficiency compare with the theoretical efficiency for the size?

WEBSTER: We have never been able to determine the actual theoretical efficiency, because we are working with different particle sizes. About a year ago, calculations were attempted, I believe, with about 1.5 to 2 μ ; with this size the efficiency should be close to 100%, and then it tapers off.

ENGELMANN: We should remark here that in microscopic counting of particles there is a variation of perhaps 10% between persons, and also on successive days with a given individual. Then, I would like to ask, how do you determine and measure the flow rates on these respective filters?

WEBSTER: They are measured just before and after the experiment with a flowmeter directly at the filter, and, in addition, the pressure differentials are maintained constant during the experiment.

Technical Areas in the Field of Meteorology That Are Limiting Progress in the Nuclear Industry

FRANK A. GIFFORD, JR.

This topic is of interest not to scientific researchers, who are interested in their own specific problems, but to research administrators, who are interested in the entire field. I would like, however, to limit my considerations, as we all have at this conference, to the nonglobal aspects of the question, that is, to short- to medium-range, low-level problems. For example, reactor site selection, reactor hazard analysis, and related problems created by chemical processing plants; or the problems relating to cratering shots (mentioned yesterday) including nuclear excavations, and relevant rocket problems such as static tests and launch pad aborts but not re-entry burnup.

My basic opinion is that within this group of problems meteorology as such is not limiting progress in the nuclear industry.

One must recognize that meteorology is one of a large number of relevant special disciplines whose specific types of information are used by the nuclear industry to assess the problem of dispersion of material in the biosphere. But it is only one of them, and with relation to this kind of problem, meteorology has been in comparatively good shape. Our ability to predict and to describe the concentration and distribution and transport of material, although it is obviously limited (this is clear from the previous presentations at this conference), exceeds our ability (or other people's) to define the source term. This was made clear, for example, in Allen's presentation by what he had to say about cratering shots and, to a certain extent, rocket tests. Therefore we have been able to present our products, that is, the computations of atmospheric dispersion, with some confidence, knowing that although it is not perfect from our standpoint it is probably adequate for what the atomic energy industry needs and has needed for many years.

Does this mean we should now stop doing research and just coast along on the current state of the art? (In the Weather Bureau we would refer to

this as turning the problem over to the systems people.) Obviously, the answer is, no. I would like to present four points which lead me to the conclusion that we must continue to supply the research on atmospheric transport and diffusion and related questions which the atomic energy industry needs for protecting the health and safety of the public.

First, our present capability to predict concentrations, although it may in many respects be adequate, certainly has some major gaps. Some of these will be indicated in subsequent presentations. For example, we are in pretty good shape with regard to short-range dispersion, somewhat less so with regard to medium- to long-range dispersion. The problem of dispersion from sources other than continuous point sources is clear from previous discussions and will also be discussed further, including the effect of the time of emission – finite time sources. Deposition, which will also be discussed later, and its converse, free resuspension or levitation, which is probably less significant, certainly are parts of the problem. The effect on dispersion of various kinds of topography – land-water effects such as sea breeze, etc. – and also the effect of cities on dispersion and vice versa are important. These problems will be discussed by experts, but I want to say that our methods for dealing with them as far as the end product, the prediction of concentration, is concerned, as well as the degree of our physical understanding of them, suffer considerably by comparison with our ability to estimate the average dispersal from a continuous point source over level ground. Since almost no power reactor sites are located in an ideal situation (I can think of only one), we need, as a practical matter, to be more concerned about some of these nonideal location effects.

Second, we can't expect that the present situation with respect to the lack of knowledge of the source strength (Q) will continue indefinitely. The AEC is spending millions of dollars each year on a

complex, well-planned reactor safety research program, one of its principal objectives being to solve problems related to the specification of source strength. At Oak Ridge we have a nuclear safety pilot plant, and fission product release studies are being done at Oak Ridge and also here at Brookhaven. These and other related studies are all attempting to define the source strength better under conditions approximating real reactor accidents. At Idaho Falls, as we heard yesterday, reactors are intentionally destroyed in full-scale reactor accident simulation, one of the objectives being to inquire into the source strength. As these and other related studies come to fruition, ignorance of the source term will decrease, and we will no longer find ourselves ahead of the game in this respect. It seems to me that this alone makes it mandatory that we continue research and development programs on the various problems mentioned, that are designed to improve our ability to predict dispersion in the atmosphere. It does not make sense to invest millions of dollars in attempts to define one term in the equation without continuing a related attempt to define the other terms.

The third factor has to do with the legal aspects of nuclear facilities – nuclear reactors, chemical processing plants, etc. From the early stages in which safety was evaluated on an informal, in-house basis, the surveillance of the health and safety of the public as it relates to nuclear facilities has developed into a complex formal and legalistic licensing and regulation procedure. The various steps are hedged about with safety requirements of various kinds, starting with the initial design considerations and site selection, extending through the construction and initial operation, and terminating only with old age, as the licenses are issued for a period of 40 years. Meteorological considerations enter at various stages of this process. I particularly want to emphasize two: consideration of the effects of possible accidents to reactors, and the determination of permissible levels of routine gaseous emission.

The accident problem is subject to a federal regulation, *10 CFR Part 100*, which specifies what might loosely be called acceptable conditions for an accident – no accident is entirely acceptable, of course, in any sense. The conditions are specified under which a reactor can be granted a license for construction and operation in terms of location relative to concentrations of population,

and taking due account, among other things again, of the meteorological factors involved. Meteorology is taken into account in *Part 100* and in the related technical information document, *TID-14844*, by a discussion of a highly simplified hypothetical accident with very restrictive assumptions used for lack of more certain knowledge, for example, with the assumption of a point source, steady wind, and average poor dispersion, that is, meteorological dispersion parameters that would be relevant to average poor dispersion and other conditions. These assumptions are made for lack of ability to be more realistic; for example, if we could introduce the effect of the initial volume source, this would tend to reduce concentrations, as would introduction of the effects of meandering and wind shifts. Also, for a particular location (and there is at least one actual example) the average poor dispersion conditions assumed may be too pessimistic. If such things could be taken into account, the requirements on the amount of real estate around a reactor could perhaps be relaxed. On the other hand, some of the effects work the other way, for example, the stabilizing effect on the dispersion of an over-water trajectory: many reactors are located close to bodies of water, and this would lead to an unduly optimistic prediction if one followed closely the procedure used for hazard calculations assuming land trajectories.

The intent of *Part 100* is that reactor operators and designers make the best use of existing knowledge for handling safety problems, including meteorology where applicable. At present, the pattern of hazard analysis is frozen into the stereotype of the technical information document mentioned above, but I hope and believe that, especially as meteorological understanding and capability in the AEC's Licensing Division builds up, the trend will be towards the most realistic hazard assessment that we are capable of. Obviously this will pay off in increased safety, and in many cases it may pay off in reduced cost of the site and the required engineering safeguards. However, this will clearly require somewhat more sophisticated meteorological analysis based on better understanding of some of the factors that will be discussed later, and this in turn obviously is related to our own research program.

Concerning the routine emission problem, at present the amount of radioactive gas that an operator is allowed to release to the atmosphere on a routine basis is usually the subject of negotiation,

sometimes bitter, between the operator and the AEC's Licensing Division, often during the later stages of the licensing process. The dilution factor agreed upon depends on dispersion and climatology, and includes many concrete reactor design features, particularly off-gas systems; the same remarks about meteorological effects apply as in the case of accidents. The potential pay-off in the routine case is in the relaxation of design requirements before greater emission rates can be justified, or, conversely, the assurance of safety where a greater emission is contemplated.

My fourth and last point is difficult to phrase. Our present capability in the field of atmospheric dispersion is rooted in the early English chemical warfare work. From this heritage we have been able to carry on, to develop other methods to a very considerable extent over the years, under the enlightened sponsorship of the AEC, as is amply reflected by this meeting. There is in this area other capability in the United States than that sponsored by the AEC, but the AEC's sponsorship under its mission objective of protecting the health and safety of the public accounts for a major part of the national effort in research and development on atmospheric turbulence and diffusion. Without this, our total national program in the area would be seriously curtailed.

It seems to me that it is our clear duty as responsible research workers to respond in the most efficient way we can to the needs of the atomic energy industry for the best current atmospheric transport and dispersion information, but that the burden must also be laid on our sponsor of realizing that he too has a responsibility to the future of research in this area, to support at a reasonable level those research programs needed to improve our ability to predict atmospheric dispersion and related phenomena. This will provide not only for needs that are now seen or clearly foreshadowed but also for needs that are not as yet perceived.

DISCUSSION

SMITH: You have said that you think for the most part the application of the meteorological estimates to major hazard problems by licensing and regulation people is pretty much a routine affair.

GIFFORD: I think it varies a good deal, according to the interest of the applicant and the capability he may have built up over the years. Some people are old hands in the reactor business, and others are just getting into it with a particular reactor. The classical pattern for reactor haz-

ard analysis was, in my opinion, established by Holland's analyses in the early 1950's at Oak Ridge. This pattern is still being followed to a considerable degree, although the results of current research programs are factored in at a fairly steady rate.

The significant thing that has occurred is the formulation of *Part 100* and the accompanying technical information document. This sets up a prototype reactor hazard calculation, a straw man to be knocked down. But the straw man, in some respects, at least over about the past couple of years, has gotten to be pretty durable and now serves as a standard against which some of the - one might say - less enlightened applicants for licenses judge entirely their meteorological problem. The meteorological capability of the AEC's Licensing Division is building up, judging by the number of contacts I have with meteorologists and others who are not meteorologists but have an active interest in meteorology. They are there to see that the meteorological analysis of the hazard is not merely an exercise, but has some real meaning, and I think in the long run we will not suffer from the fact that a regulation quite specific in terms of such things as diffusion coefficients has been set down. The regulation clearly states that these things are meant to be taken not as an absolute but as a guide.

What can we do? We can continue our studies. We can continue in our contacts with reactor people to attempt to influence them towards more realistic hazard analyses. At the same time, the AEC Licensing Division can continue to study the applications it receives and to make sure that these conform to reality, for example, that they don't follow a particular fixed format of analysis when it obviously does not apply because the reactor is located on the edge of a body of water or in the middle of a deep, narrow valley, etc. I anticipate that the meteorological analyses of reactor hazards will become more and more realistic during the current epoch.

SMITH: I take it you are suggesting, among other things, that at least a significant number of the applications have been rather stereotyped in the application of these principles.

GIFFORD: That is right. There was a period when people seemed to tremble at any variation from this technical information document in spite of the fact that it was never intended to be more than a guide. Reactor hazard meteorological studies should be essentially neat but not gaudy, that is, they should address themselves to the real problems and not try to blind the reader with irrelevancies, nor should they indicate that the writer does not really understand the problem involved.

WATERFIELD: I would like to echo the last statement that it is a good idea to direct the meteorological analysis to the problem. Not too many people are really familiar with this TID document, and we are going to discuss it tomorrow.

HOLLAND: Would you comment on the following: Do your remarks apply with regard to the general adequacy of the available meteorology in relation to the other uncertainties of the reactor hazard problem? Do they apply essentially to the numerical bulk of applications which are submitted? Do they apply essentially equally across the board, or is the situation very different in some im-

portant way between reactors that might be characterized relatively as small, of a well-known type, located in a conservative location, and reactors of a novel type or very large or located near a population center or having a high power level? Do the people who do the hazard and accident calculation and those who do the meteorology have a period of worrying whether their answers have been duly conservative or whether they can shave a bit off the cost or justify a lower estimate of concentration; or does this fall out of the calculations and allow them to sleep easily during the course of the analysis and its submission and while the plant goes into operation? Isn't there a difference between 90% or more of the applications and those few that are trying to push the frontier ahead?

GIFFORD: Turning your questions into answers, I agree. Certainly the depth to which meteorological analysis goes (which I emphasize as one of several special analyses that accompany the engineering and technical details of the hazard report) will be strongly influenced by the novelty of a particular reactor. To give a concrete example, the Enrico Fermi reactor is a fast reactor of somewhat novel engineering design, and there has been a fair amount of controversy about its nuclear concept and execution. In addition to many details on other aspects of the hazards, a number of special meteorological studies and other special environmental studies were done which certainly would not have been appropriate to a more conventional reactor. I think this sort of effort does produce more enlightenment.

HOLLAND: If you think only of the licensing responsibility, especially in a statistical sense, it may be that the job can be done to a great extent with existing knowledge; but if you think of the other mission of the AEC, which is to develop the uses of atomic energy, particularly for peaceful purposes, then the problems that arise with the novel and proposed applications, even for the distant future, are just as important. These may represent a small minority of the applications and reports coming in to the licensing people today, but the problem of the meteorologist that will arise in connection with novel applications have to be faced early, and if we don't know enough to answer the questions, we have to be doing the research now.

GIFFORD: I would carry that one-step further and say that the AEC has some responsibility for problems that haven't yet been formulated, and I am grateful that it has and does continue to recognize them.

REIFF: Consider the hazard report in the case where the site is selected or the reactor is being built. The proposal includes assumptions for meteorology which are,

say, less conservative than those in *TID-14844*; one is trying to treat the problem at hand and not relate it to the example. How does one substantiate less conservative assumptions, since (to be picayune) one can never have enough supporting data on a new site? How does one convince any agency or reviewer that what meager data one has will be adequate for these, let us say, optimistic assumptions in meteorology?

GIFFORD: The answer is that you do the best you can, which is not a satisfactory answer, particularly to a lawyer. A lawyer likes to have precise criteria, and I think we have to admit that we don't have precise criteria. The minute somebody puts in what looks like a precise criterion, it is – understandably – seized upon.

REIFF: That's what happened with *TID-14844*; and, although it has been a major contribution, I think several people now feel it is also being used against them, in the same sense.

GIFFORD: True.

REIFF: I can appreciate that side of it, but consider the other side, that of the contractor. All he wants to do is, one way or another, to get this thing approved, so he makes assumptions which he thinks are realistic and certainly don't seem realistic to someone else. I know this is a difficult question because this problem has been posed to me in the same way, but what would you consider as adequate substantiation or adequate data?

GIFFORD: You could get guidance from past experience; the spectrum goes from using general Weather Bureau climatological records (which one can get for the price of the postage) to putting up a tower, which was done at Brookhaven, to establishing a large meteorological program. Consider the specific case of a reasonably large power reactor in a situation that leaves some doubt as to how large the exclusion radius and other distance factors ought to be. One should be able to obtain data that are reasonably representative of the range of meteorological conditions that can occur by some fairly short-term sampling program although it might require up to a year (data need not be generated for the entire year). I shouldn't think there would be too much difficulty in an applicant and the AEC coming to agreement upon that basis. At almost any given site almost the complete range of dispersion conditions will occur almost every day. This is not to say that one day's intensive data are sufficiently representative (obviously it might rain one day, or the wind might blow from one particular direction), but I don't think one has to think in terms of operating a large installation for several years unless there is some other reason why this would be desirable. I think a representative program of sampling can be done in less than a year.

Meso-scale and Mean Transport Problems

JAMES J. FUQUAY

We are talking about problems, not about answers. We should look for things that are hindering progress on solutions to some of these problems. Several meso-scale problems have been presented at this meeting, along with various methods of seeking solutions, but no one has mentioned meso-scale. It is obvious that meso-scale problems are involved in Smith's tracking of I^{131} over several miles, Hilsmeier's fluorescein studies, the Hanford diffusion work, Van der Hoven's missile launching problems, Islitzer's discussion of transport over several miles, Allen's transport prediction problems, Pack's trajectory measurements, McCormick's urban pollution transport studies, etc. All these studies are seeking answers generally to immediate and important problems. Almost all of them involve difficulties due to the inadequacy in our ability to describe and measure certain important parameters and difficulties in obtaining data in sufficient detail over large areas and through sufficient depth; in some cases the problem was not sufficiently formulated to determine what should be measured.

A good example is the sea-breeze regime, which has been mentioned several times. In the Los Angeles basin we found interesting and perplexing interaction between the dynamics of the sea breeze and the topography within which it is contained. The sea breeze regime at Cape Kennedy or Atlantic City would be expected to be different. Both theoretical and descriptive types of research are being done on the sea breeze, but even now some of the measurements are deficient. The point is not that limited descriptive measurements have not been useful in solving practical problems, but that we do have a long way to go with regard to prediction in some of these areas.

Equally complex problems arise in mountain-valley systems and urban areas, places where surface roughness and terrain features are irregular and the heat and energy balance of the system is complicated. These problems generally have a three-dimensional geometry, and a time dependency both in the physical causal factors and in

their description. In some cases the effect to be examined is the receiving of energy from outside the geometric confines of the region of study. Energy passes through the scale of motion. Studies down to micro-scale are needed. Consideration of the transfer of momentum from the general circulation through some local circulation to the dissipation indicates the sequence of events.

The Hanford area is an example of a somewhat complex topography. (See Figure 1.) In the center of the basin are some 500 square miles of flat terrain, except for a small outcropping of basalt. The terrain slopes from the northwest to the southeast; this contributes to a local nocturnal drainage flow which is helpful in our experimental program. These are perhaps the most predictable winds at Hanford. In our efforts to predict these winds we have to go far outside 1000 square miles and examine the flow patterns in eastern Washington and northeastern Oregon to see how they interact with the local wind systems in this valley. As the plume from a 30-min release moves downward, it undergoes some rotation, stretching, and elongation, reflecting the wind shear and changes in the air motion in relation to space. Along the trajectory path are accelerations and decelerations which significantly affect the location of the material, and we need to know the behavior of the air within the contaminated area and surrounding it. There is a very pronounced curvature in the trajectories resulting from topographic and other influences.

Some work done at Vandenberg Air Force Base with zinc sulfide releases serves as another example of the effect of terrain on a large-area basis. (See Figure 2.) Run 30 was representative of unstable conditions over the relatively flat mesa on which the air strip is situated, and the path of the material was fairly straight, probably quite predictable from a minimum of wind measurements, with few complicating factors. Run 16, during stable conditions over the flat terrain, also moved in a determined path to the southeast. Run 18, made during unstable conditions, passed over a

Figure 1. Trajectory of Green-Glow Run 15.

ridge, the plume essentially traversing the topographical obstruction. However, Run 29, a stable one, showed a tendency to follow along the contours, curving around the obstruction and then proceeding up the valley. The question is how to predict the behavior and transport of systems in such a system, perhaps with stable air coming off the sea and undergoing modifications as it goes across the land complex and assuming the resultant curvatures.

Figure 3 shows the ground source data, σ_y , plotted as function of time. Note that in the first few hundred seconds there is a very nice power function behavior; if we had no more data than that, we would be satisfied with such a function. By assembling data from various sources, we covered a larger time span, so that the curvature in the behavior of the σ_y function could be detected which is quite in keeping with Taylor's 1921 work. At large times, σ_y is increasingly proportional to the square root of time rather than proportional to time. The dashed lines show power functions fitted to some earlier data taken in the time region where a power function fits very well followed by extrapolation to large travel times or distances, which seemed reasonable. But at large travel times the lines diverge. Therefore, in evaluating a problem of this magnitude, it is quite important to examine the total problem – the behavior of the variables – and try to operate in regions where the variables are expected to show some change indicating the more fundamental behavior.

In the work reported yesterday by Pack and Angell, the tetraon data out to certain distances showed a power function behavior. Concerning

the spectrum of eddy motions as one goes from those which cause this behavior on the surface to those which might be influencing the behavior of the tetraon at some thousands of feet, we could speculate that, since the wavelength of significant eddies probably increases with height, the power function relationship perhaps goes to longer times at the higher altitudes, and therefore the curvature starts to appear at later times, ultimately approaching the square root of time behavior if the hypothesis is correct. It will be very interesting if the tetraon data also show this curvature as we became capable of doing sequential tracking to longer travel times. This will be at several tens of hours of transport, I would guess.

I have made no effort to define the meso-scale. A definition on the basis of the physics and behavior of physical variables leads to one conception, a definition in terms of geometry to another, neither one of them adequate. The definition is still quite tentative.

DISCUSSION*

GIFFORD: This may be related to detail, as you said, but it is a homogeneous isotropic theory. Therefore I don't think you should attach too much importance to the fact that the change of slope of the σ_y value as a function of distance appears to conform to this theory. There may be some other entirely different effect.

FUQUAY: We perhaps shouldn't identify it so closely with Taylor's work. However, it comes out to be this way.

GIFFORD: Yes, but the explanation may not be — —

FUQUAY: We don't propose an explanation for this at this point. We do have some tentative ones.

*This paper is further discussed after Vaughan's paper, which was presented next.

Fundamental Problems on the Meso-scale

LELAND M. VAUGHAN

I consider the meso-scale to be on the order of a few miles to a few tens of miles, that is, intermediate between the micro-meteorological scale and the macro-scale, which is primarily defined by the circulations on the weather map. Some of the fundamental problems to be resolved have already been mentioned, particularly the relation of turbulent transport of airborne material to that of momentum and heat. I would like to see more direct measurements of these fluxes to see how they compare with the transport of matter. Another difficult problem on the meso-scale is the standardization of measures of atmosphere stability. On the smaller scale we can use, say, a 16-m mast and derive the stability ratio from ΔT or $\Delta \theta$. In principle perhaps the Richardson number is also applicable on the meso-scale, but its measurement is more difficult, since it is particularly sensitive to the vertical gradients of wind speed; any comment on the measurement of stability on this scale would also be applicable to the measurement of mass transport.

In applying statistical theory, we again encounter the problem of the relationship between measurements made at a fixed point and the Lagrangian motions which are actually responsible for the transport and diffusion of matter. We have seen both theoretical and some experimental evidence that this time scale relationship is dependent upon the intensity of turbulence, and I would suggest that, since we never are able to observe homogeneous isotropic and steady state turbulence, this also has a spatial relation; in other words, if one were to get a measure of turbulence on the time scale or its equivalent, length scale, this would vary with height, with location, and with wind direction.

This is not perhaps an insurmountable problem in a reasonably homogeneous terrain, and I would like to see more direct measurements of turbulence, including the measurement of the length or time scale as well. In fact, we have set up at one installation the components of a system for mea-

suring eddy diffusivity directly. This is one type of parameter that could be put into a diffusion equation.

Finally, there is the problem of using the similarity theory, as Dr. Gifford mentioned. I would like his comments on the prospects of using this type of analysis for elevated releases; on whether there is a similarity in vertical distribution of turbulence based upon, say, the geometric properties of the terrain and some measure of stability; and on the degree to which we can apply the statistical methods which are based essentially upon homogeneous isotropic turbulence to the meso-scale problem, knowing that such conditions prevail over only a limited range of distances or times.

DISCUSSION

GIFFORD: Baldwin, Cermak, and Malhotra at Fort Collins worked with diffusion from an elevated source, in their case elevated above the surface of the wind tunnel, and did a dimensional analysis (published in the *Journal of Fluid Mechanics*). None of this work on dimensional analysis of diffusion has been extended above what we call the constant stress layer (or the surface layer in the atmosphere), and doing so would introduce other parameters when there already are too many. The first step along this line was taken by Monin and Kazansky with respect to the distribution of momentum in the entire planetary boundary layer. I don't think this is a very hopeful approach. It doesn't show too much promise of solving the complications in a situation with more parameters to rationalize. It is not a substitute for some more directly dynamical approach, but since we don't have the latter now, it is at least temporarily of considerable interest.

Concerning the applicability of the statistical theory to dispersion over long distances, I think this has to remain in a qualitative, approximate status, because there is no way, through the development of the statistical theory of turbulence, of accounting for the complexities that exist in the meso-scale in the real world, nor is there any way of allowing for them. There is no way of either changing the theory to conform or of making allowances in terms of reality. I think, therefore, that the approach of Pasquill and his colleagues, for example, with regard to longer-range dispersion, is the proper one. One should be guided strongly by the implications of the data, keep-

ing in mind the statistical theory as a descriptive approach to the problem, but one should not use the specific illustrations of statistical theory for the ideal state.

O'BRIEN: As regards Taylor's theory, it seems to me that the crucial feature, which gave the lateral dispersion as t^2 initially, is still a plausible argument even in non-isotropic and nonhomogeneous situations, because the primary reason for it is that the Lagrangian velocity field is uncorrelated, so that eventually the integration of the u, v_1 is to be applied as a number, and the integration of time as linear time. It is true that in meso-scale problems other factors enter, but at least the decrease in the σ_y value with time (as shown in Fuquay's Figure 3) is probably explained by Taylor's theory. I don't know whether there are other effects that would show that.

GIFFORD: What Taylor gives is not the dispersion in one direction but the projection of all of the dispersion into one plane, and this implies that in a nonhomogeneous turbulent field the turbulence in the y -direction can be regarded essentially as homogeneous and isotropic. I don't see how one can take a turbulence field that is strongly inhomogeneous and avoid the effects of the inhomogeneity by using Taylor's approach, which involves projecting onto one plane or another (in this case the xy plane) the particles released from a point in this inhomogeneous field. In other words, the inhomogeneity in a surface layer is in one direction, and you might think you can get away from it if you consider the dispersion in another direction, but I don't think you can. Was that your point?

O'BRIEN: No. Taylor's problem essentially was to include the inhomogeneities when there are variations in three directions. He didn't solve it, of course. To solve this problem one has to assume homogeneities, to get a nice solution. However, it depends on the time involved. The rate of dispersion is not dependent on homogeneity. It depends only on the fact that the particle loses correlation with itself, eventually, and this should be reflected in the solution.

GIFFORD: But an inhomogeneous problem involves other things besides the self-correlation. It involves multiple correlations. The problem has been solved from another standpoint, and, moreover, it has been studied observationally in great detail by people like Cramer. The slope of concentration vs distance varies widely depending on circumstances, depending on the degree of inhomogeneity, you might say, something like -2.8 to -1.5 in the range where it should be fixed at -2 . If Taylor's theory applies unequivocally, it is always -2 , but the observations don't support that.

O'BRIEN: Yes. I am talking about the fact that the dispersion rate gets slower as you go away from the source. I am not talking about specific numbers; obviously, it's too complicated for that.

BADGLEY: I agree with Dr. Gifford. It's not obvious that what Mr. O'Brien says would be true because the process by which Taylor arrives at his results implies taking $\overline{u'^2}$ outside of the integral. If you have a chart with an integral of $\overline{u'^2}$, and if $\overline{u'^2}$ is in fact a function of time, it is not obvious mathematically that this can be approached by taking any specific value or any mean value which doesn't itself depend upon time outside of the integral.

You can integrate only the variable of the coefficient, therefore reaching some limiting value. It is conceivable that the u' approaches infinity as we approach the limits of the integral.

MCCORMICK: What is the relevance of this discussion to the real world?

BADGLEY: This is an idealized world, but by analogy one can say the same thing about the distribution of molecular velocities in a fluid. The assumption that they are distributed in a Maxwellian manner serves perfectly well, in spite of the fact that if shear is taken into consideration it is wrong in theory. The same thing can be said about homogeneous isotropic turbulence: it is a simplification, but not one so far off the mark that it is completely useless.

PACK: Like the K theory, this is useful only in practice, but not in principle. I have several questions to toss out. First, at what point can we switch from a statistical plume description to a trajectory target approach on the meso-scale? It is obvious that if the plume doesn't cross a receptor, all the predictions of the concentration in the plume matter little. Second, at what level or over what height interval should one average the wind to get a mean transport wind to use as a function of travel distance? The variations in arrival time were mentioned yesterday with regard to a transport over 50 miles in a practical sense. Third, in the problem of meso-scale transport versus steady state, for transport over ≈ 100 miles with winds of $\approx 2\frac{1}{2}$ to 3 m/sec, how is the diurnal regime taken into account in practical problems? Finally, if the variations in space and time are as large as we know they are, is a probability description of the concentration the approach that one should use to answer satisfactorily the questions that our various sponsors pose? Can they live with the statistical probability that the concentration will be χ or $\chi + \Delta\chi$? These are the questions. I don't propose to answer them.

APPENDIX TO DISCUSSION

GIFFORD (May 26, 1964): Fuquay showed a curve, based on observations, of root mean square lateral particle dispersion vs time. There appeared to be a t regime followed by a $t^{1/2}$ regime. Fuquay interpreted this as bearing out the limiting cases of Taylor's dispersion theorem. I objected that this was not a case of homogeneous turbulence, and O'Brien remarked that even so Taylor's limiting cases might apply because the essential thought involved the presence or absence of a particle correlation.

I agree that something like Taylor's reasoning must apply also to dispersion from a fixed point in a stationary boundary layer. However, I am not aware that any details have ever been worked out, and surely this situation must be much more complex than Taylor's even in the adiabatic case. This, it seems to me, is proved by the result Batchelor and Ellison got for the downwind concentration in a logarithmic boundary layer, namely that concentration varies as distance to the -1.88 power. In the nonadiabatic case, the situation departs even more from the -2 power dependence that Taylor predicts.

I think the explanation of Fuquay's curve is actually the following. The time value against which he plotted

dispersion was obtained by dividing the mean wind speed at a fixed level (7 m) into the distance at which samples were taken. The further you go from the source, on the other hand, the deeper the layer through which particles arriving at the downwind sampling point have diffused. Consequently, the mean wind used to compute the travel time should increase with distance traveled, and not be a constant as he assumed. As a result, in converting from distance to time coordinates Fuquay was using a mean wind that was smaller than it should have been, the farther out he got. This led to time values increasingly too great, for greater distances, and caused the curve to bend in the sense he showed.

This explanation, which is probably correct, doesn't have anything to do with particle correlations. On the other hand, if Taylor's theory were operating here, the points in time defined by the two observed slopes would have specific implications about both the micro- and the integral scales of turbulence. I don't think these inferences should be drawn from Fuquay's curves, and this is the essential reason for my objection to his interpretation of them as Taylor-type dispersions.

O'BRIEN (June 24, 1964): I presume that Fuquay was measuring ground level concentration and could identify a mean plume centerline. Let us ignore the dynamics of deposition. First, I must agree that the effect of variation in the mean wind speed with height should certainly play a role in determining the proper time scale. It should be easy enough to estimate its effects (given the wind profile and the centerline motion) simply by assuming, say, that the average particle spends as much time above as below the centerline. My suspicion is that the effect is unlikely to be as severe as you suggest, but it would be interesting to estimate it from the test conditions.

As regards my remark about the applicability of Taylor's work to inhomogeneous flows, I meant the following: If $y(t)$ is lateral displacement of a fluid point or passive contaminant and if $v(t)$ is the corresponding particle velocity, then without any assumption of homogeneity or stationarity,

$$\frac{1}{2} \frac{d}{dt} \overline{y^2}(t) = \int_0^t \overline{v(t)v(t')} dt'.$$

The role of homogeneity is simply to make plausible the condition that $\overline{v(t)v(t')}$ be a stationary function. Lumley has shown that a *sufficient* condition for stationarity is that the Eulerian field be both stationary and homogeneous. To my knowledge, the *necessary* conditions have not been established. Since we wish not to assume Eulerian homogeneity, let us exclude stationarity and write:

$$\begin{aligned} \overline{v(t)v(t')} &= f(t, t') = g(t, \xi), \quad \xi = t - t'; \\ \therefore \frac{1}{2} \frac{d}{dt} \overline{y^2}(t) &= \int_0^t g(t, \xi) d\xi. \end{aligned}$$

My argument was that for ξ large enough $g(t, \xi)$ might still approach zero so that eventually $\overline{y^2}(t) \sim t$ or at least

$$\int_0^t g(t, \xi) d\xi$$

should become a slower function of t as t gets larger.

There is a further complication which I should note. It is possible that $\overline{v(t)v(t')}$ even though uncorrelated for large time differences may still have a nonzero value $\overline{v(t) \cdot v(t')}$ because there is no reason to assume zero mean lateral velocity. If this is so and we write, for example, $v(t) = \bar{v}(t) + v'(t)$ then it is easy to show

$$\frac{1}{2} \frac{d}{dt} \overline{y^2}(t) = \frac{1}{2} \frac{d}{dt} \overline{\bar{y}^2}(t) + \int_0^t \overline{v'(t)v'(t')} dt'$$

so that the previous analysis should really be regarded as diffusion relative to a mean centerline, i.e.,

$$\frac{1}{2} \frac{d}{dt} [\overline{y^2}(t) - \overline{\bar{y}^2}(t)] \rightarrow \text{Constant as } t \rightarrow \infty.$$

On top of all this, there is the difficulty of interpreting ensemble averages for nonstationary processes. You undoubtedly know more about that than I do; I am simply curious as to how unrealistic it might render the above argument.

The crucial point in the above is the claim that in some case $\overline{v'(t)v'(t')} \rightarrow 0$ as $t - t' \rightarrow \infty$. It is evidently unlikely to be true in, say, the case of lateral diffusion in a jet where the mean streamlines all diverge and the above average is likely to remain a strong function of time. I would thus like to exclude all cases of diverging mean flows and insist that the mean streamlines remain more or less parallel. Your remark concerning scalar transport in a boundary layer falls into the category of such flows, and all of Batchelor's 1957 results on free turbulent shear flows would be excluded. In fact, calculations from that paper of the above Lagrangian correlation exhibit a time-dependent behavior asymptotically. If Fuquay's plume was within a uniform width valley, that might be the best circumstance for observing agreement with Taylor.

Finally, I should like to mention that I have looked over the calculation you gave me concerning relative diffusion in a neutral boundary layer, and there is one problem which you probably have thought about and which seems to be crucial to the entire argument. To what extent does the friction velocity represent the detailed turbulence structure? It is evidently satisfactory as a scale for mean profiles and probably the energy-containing regions of the velocity spectrum so that single-particle dispersion is presumably well described by it. But what of the small-scale stuff which plays a fairly vital role in relative diffusion? Perhaps its structure can vary widely for flows with the same friction velocity, and thus one would need to characterize relative diffusion with Kolmogorovian parameters which should better reflect the detailed structure and also complicate the problem a great deal.

GIFFORD (June 26, 1964): Fuquay has told me that he was measuring ground level concentrations, on (or under) the plume centerline. I understand your viewpoint about the application of Taylor's theory now and certainly agree with your reasoning. On the other hand I have the feeling that, over the distance range of Fuquay's experiments, several effects come in (apart from the effect of assuming a constant mean wind) that make the real situation quite complex. I have in mind that clouds grow

out of the surface (constant stress layer), turning of the mean wind comes in, and so on. You yourself mentioned the ensemble average problem. (Maybe the change in his data represents some terrain effect.) The point is, the change to $\bar{t}$ dispersion has a definite physical implication in Taylor's theory, or in your proposed extension. I'm not very sure that this (i.e., loss of correlation; the scale of tur-

bulence becomes small compared with the distance scale of the problem) is the cause of what Fuquay observed. Other people, didn't find this so clearly. See Pasquill, F., *Atmospheric Diffusion*, pp. 250-1, Van Nostrand, 1962. The influence of averaging time vs travel time of the smoke is also a sticky problem. Nevertheless, Fuquay's data do seem to call for some kind of explanation.

Importance of the Tetroon to Meso-scale Meteorology

JAMES K. ANGELL

I think on the meso-scale the tetroon is the most feasible way of establishing a circulation pattern. It is hard for me to imagine, on this scale, any other method of obtaining data in sufficient density in space and time. In other words, at heights of 1000 ft above the earth's surface, how can one put up an Eulerian network that will provide the desired measurements? With specific regard to the matters of interest such as reactor siting, possible effluent release, etc., I would like to make a few definite points.

First, the tetroon is of use in a trajectory sense alone. I don't think there can be much doubt that the tetroon follows the horizontal air motion in a fairly nice way and can give a pretty good indication of where the substances will go.

Second, the problem of estimating dispersion from the tetroon trajectories is somewhat dubious, as seen from the recent discussion. I tried to show the general technique yesterday, and this morning Mr. McCormick presented evidence that even from the trajectory of a single tetroon one can estimate dispersion as a function of downwind distance and of time. If there is a reasonable method of tracking the tetroons – and this is undoubtedly a problem – then this is a very good way of estimating dispersion over long downwind distances. In fact, it seems to me much simpler than any other technique I know, and it should be looked into very seriously.

The tetroon technique should be considered from three points of view in determining a reactor site. In determining the climatology for the site, one might release a tetroon once or twice a day for a certain period of time to get some idea of where the material would go. This would be particularly useful in hilly terrains and also in the land and sea-breeze regime. Second, if one suspects that any effluent will be released, it is easy to release a tetroon beforehand or simultaneously to get an idea of where the material is going, and to send over an aircraft to make measurements on the material. Third, it is convenient to have a tetroon

available in case something goes wrong and there is an accidental release of an effluent. For example, at Windscale, how useful it would have been if they had had tetroons to send out as soon as the reactor broke down.

There are a few more esoteric things that have not been mentioned yet. First, the tetroons give a pretty good estimate of the vertical motion, which is of some interest and which apparently some people have difficulty in determining accurately. Second, we would like to put up three or four tetroons at the same time, to have a triad, in order to measure the divergence, vorticity, and things of that sort. It seems logical to me to try to use the tetroon in many different ways, and certainly our group will do this insofar as it can.

DISCUSSION

PACK: Let me take the view opposite to my partner's. We recognize that there are some fundamental problems with the utilization of the tetroon, primarily, how does it respond to the air motions? As he pointed out yesterday, we are rather sure that it cannot respond with true, full amplitude to the vertical motions. The determination of the percentage response remains to be done and will be very difficult. In a practical sense, one probably would not try to utilize this technique, requiring, as it currently does, a large radar, on many reactor sites. It is useful in places that have extensive experimentation, but one would look askance at the director of a university 3-MW swimming pool reactor who proposed a \$100,000 tetroon program, even though one would be interested to see the data.

GIFFORD: What about the possibility of releasing one or more tetroons in the event of an accident? Could one design some sort of semiautomatic release mechanism that could be actuated by a guard flipping a switch or some other simple mechanism?

BARAD: How would you track it?

GIFFORD: I think it would be useful at the larger AEC sites and places where they have radar.

ALLEN: How close to the time and place of the accident must the tetroon be released? Since we are unable to get very close to our ventings, the release, depending on the site, would be about 5 to 10 miles away; we figure that eventually the tetroon and the cloud will come close

together. This being the best we can do, it is the manner in which we propose to proceed.

DICKSON: As far as controlled releases are concerned, we have used tetroons at Idaho Falls in both the SNAP and SPERT tests. Since we can get within $\frac{1}{2}$ mile of the release, we can time the balloon from the countdown so that it is directly over the reactor as the effluent goes up, and then we track it for 80 miles. We have done these tests in preparing for actual releases. Penn State has done some work on inflating tetroons and launching them from aircraft. Thus, some work has been done in this field, but there should be more.

PACK: A tetroon will stay inflated with significant superpressure for 48 hr, so with an occasional squirt of gas it could be kept inflated and ready to go. Of course, there would have to be pre-instructions for release.

SMITH: This discussion brings up an important gap in our study of meso-scale problems. At this meeting only two tracers have been mentioned which give any promise of being conservative over long distances. One is the tetroon, which is conservative until hit with a helicopter blade, and the other is the I^{129} compound that Van der Hoven mentioned yesterday. We should be concerned with the fact that some of the tracers used successfully in shorter-range tests might be pretty dangerous to use at 50 to 100 km. Something that is not conservative might still give good estimates of σ_y and centerline positions, but this would be about the limit of the data to be extracted from it.

CHAMBERLAIN: I have a question regarding the use of tetroons, and the use of elapsed time as a parameter. It has been suggested that a significant source of lateral diffusion is the effect of the change of wind direction with height. The wind has a different trajectory at a height from that along the ground, by virtue of the change of direction with height. I understood from aircraft sampling that this is quite significant in lateral diffusion, and even in ground-level studies the fact that a particle which goes aloft gets on a different trajectory from one which sticks to the ground is a significant feature. At what height do you measure the wind speed when you use elapsed time to calculate distance? If you use a tetroon, what height do you set it to fly at? Have any experiments been made or planned in which several are released at the same time but set to fly at different heights?

PACK: Mr. Booth, haven't you released two at different heights?

BOOTH: Not simultaneously. We have flown tetroons in our SWET series at quite different heights, but we were not able to position them, and they were not flown simultaneously. In some cases they were underneath an inversion and sometimes above. We found quite a significant difference in change of direction with some of our positions on the same day.

ANGELL: In general we have found, especially at Las Vegas, that there is a very great change in wind direction with height. This gives a very large contribution to the lateral standard deviation, a big wiggle in the trajectory. But the beauty of it is that the tetroon gives you the mean motion, since it is going up and down in this layer, and also it gives a better estimate of the lateral standard deviation than one gets in any other way.

PACK: With a heated effluent and a lower stable layer, we have found difficulty in getting the tetroon through the stable layer. There are techniques to do this. But if one did not know at what level the heated effluent would stabilize, say at 5000 ft, while the balloon was at 1000 ft, under very stable conditions with decoupled conditions in this case, one could get erroneous results. But this presupposes that one can anticipate all accident configurations. This is quite a problem, obviously.

HOLLAND: I think the tetroon is guaranteed to give erroneous results because the one thing that one expects the air or gas parcels to do when they rise to different heights is to mix with their environment to some degree. This is one thing the tetroon never does. It never forgets its original equilibrium level and keeps trying to return to it. I have no idea how much error this will introduce into the trajectory as a representation of an air trajectory.

PACK: On this we must defer to Mr. Pooler's excellent results showing that they do work rather well.

POOLER: On that point, on the one run that we had during stable conditions the tetroon did not indicate the movement of the tracer. The surface wind was about 20° off from the angle that the tetroon followed, and I think the speed of the tracer, judging from the drum samples, was only about $\frac{2}{3}$ the speed of the tetroon. In this case the tetroon was probably at about 1000 ft, whereas the tracer was diffusing in a lower layer.

ENGELMANN: I think we have here a split between East and West, such as we have in the synoptic pattern. In the West among the mountains and on the West Coast a number of observations that would perhaps be sufficient for an Eastern area would certainly be insufficient for mountainous terrain where the plume starts off a very short distance downstream and gets into a new meteorological regime and runs into all sorts of variables, including time. We would be happy to have a tetroon at any elevation you care to give us.

SMITH: I would like to join the Westerners for a minute. I was going to take issue with Angell's statement regarding the stable cases particularly, because I think it is precisely there that the tetroon is probably of least value. The very sharp changes in wind direction and speed with height will give most of the diffusion, actually. This very possibly is the strongest diffusive factor.

POOLER: I disagree. I think here the tetroon is about the only way we have of knowing what is the major source of turbulent energy input, namely, the mean flow above the surface layer. The tetroon is the only thing that will tell us what the air is doing up there, undisturbed by the surface and presumably feeding energy down into the lower layer.

FRENZEN: The tetroon is only one dispersing particle, and it can give only one trajectory. It cannot give dispersion statistics that are valid for an entire ensemble over meso-scale or greater unless one assumes the Taylor theory to be entirely appropriate. But the atmosphere is not homogeneous, and therefore I question long individual tetroon flights. I would much rather see short flights in pairs or serial flights in pairs, perhaps using Richardson distance or neighbor relations and that sort of thing.

PACK: This has been done. The difference in slope of the σ_y distances in the two simultaneously released Cin-

cinnati flights is disturbing with regard to this effect. I did an analysis of several simultaneous flights, of which we now have eight. There is a difference in the σ_y 's determined from parallel simultaneous flights. These differences are small in percentage but important theoretically. We can now track four at a time. Clusters, with intercomparison and correlation between all possible combinations, would be very significant, and then the dispersion could be obtained by averaging these correlations.

ANGELL: I think the danger with the pair technique is that unless the tetroons are at exactly the same level, one might get an entirely erroneous diffusion because of the wind shear.

CHAMBERLAIN: In the second experiment we did at Harwell, in 1948, we tried to get neutral points of turnover from stable to neutral. We had a smoke generator operating at 10 ft to give us the direction of the plume for sampling. This was only at 50 yards, but the site at Harwell is just below. Because of the line of dunes facing the slope, the drainage flow is such that between 10 ft and the ground the difference in wind direction was 40° .

SIMPSON: Returning to the discussion of σ_y and the tetroons, I question the statement that homogeneity in the atmosphere is not valid. In this case we are looking not at high-frequency oscillations but at the meander of plumes, and I think the flow established is a matter of assumption.

Meso-scale Studies in the >100-km Range

ROBERT LIST

Some data on a scale longer than a few miles, that is, up to a few hundred kilometers, have been collected on a classified basis. I can show these without discussing them in detail. They concern a gaseous effluent from a stack, a continuous source, monitored by aircraft taking discrete samples. The aircraft were vectored to intersect the plume at distances of about 300 and about 600 miles from the source. Figure 1 shows the results in terms of the relative activity of gaseous samples taken by aircraft flying at 0.3 km and 1 km at distances of 460 km and 920 km from the source. The X's indicate the points where we had forecast, on the basis of wind, before we had the sampling results, that the plume would be found at these levels and time intervals. These are distances great enough that we computed about 12 hours' travel to the shorter one and about 24 to 36 hours' travel to the longer one, depending on the height assumed for the wind. This at least gives some idea of the kind of dispersion this plume underwent.

One of the reasons for showing this figure is that we have a problem in trying to predict dispersion at a given point, which is the same problem that Allen faces with the dispersion of nuclear clouds. We have been stuck with using a K_H type of diffusion theory, although we realize that whenever we make such measurements we have a vertical line source and therefore shears must be very important in what the ultimate dispersion is going to be. We therefore tend to view the results we get as defining an effective K_H , horizontal coefficient of eddy diffusion, that we can apply to our clouds, and these are vertical line sources. In the case of a continuous ground level source, if K_H is computed by fitting a curve to obtain σ_y , the result is about the same, $4 \times 10^8 \text{ cm}^2/\text{sec}$.

N. Heffter of our office has published a paper in the *Journal of Applied Meteorology* on dispersion over fairly long periods, using this kind of data. He searched the literature for experiments covering periods of several hours or more and plotted σ_y versus the travel period, as shown in Figure 2.

(The numbers in the key refer to references which may be found in the journal.) For periods greater than about 10 hr, there are only a few balloon observations and a lot of aircraft flights through nuclear clouds taken at fair distances from either the Pacific test site or the Nevada test site. Some of the lower points are based on standard literature values of dispersion over periods of 1 or 2 hr. For instance, the circles with a vertical line through them are Angell's tetroon dispersion combinations for a whole series of pairs. Most of the higher points come from nuclear clouds, and it is obvious that they show not only horizontal dispersion but also the effect of a vertical shear added to it. The four triangles are the data points from Figure 1.

When we are asked to give an estimate of what the dispersion of a nuclear cloud will be, we tend to say that the horizontal diffusion for travel

Figure 1.

periods of several days will be in the order of $4 \times 10^8 \text{ cm}^2/\text{sec}$ (the coefficient of eddy mixing). I only wish I knew some way to separate the dispersion from the shear phenomena.

We have one other type of data. During the Dominic test series we flew an aircraft through the stem of the cloud about 3 to 6 hr after the detonation. Isopleths for the contours of radioactivity in the cloud give some idea of the growth of this stem. Unfortunately the aircraft was at about 40,000 or 45,000 ft, which was just below the base of the mushroom part of the cloud, and therefore vertical motions interfered with the pure horizontal dispersion. In Figure 3, the dashed line shows the track of the aircraft. The pilot was vectored meteorologically and he could see the cloud. He had a dose-rate meter in the cockpit, and there was verbal communication between the pilot and the aircraft controller, the latter accompanied by a meteorologist. As the dose rate started to go down, the pilot was ordered to turn, go back, etc. The cloud is in the order of 80 to 100 miles in diameter; if it is part of the stem, it originally was

Figure 2.

Figure 3. Radioactivity isopleths.

in the order of 5 miles or so in diameter. This particular flight was at 43,000 ft. We have three other runs from which we are also going to try to estimate dispersion on this scale. I will close by saying that our meso-scale is from 3½ hr to 4 days or so.

DISCUSSION

VAN DER HOVEN: In Figure 1 your prediction of where the cloud would be turned out to be pretty good. Did you use ordinary teletype data or other information to make your prediction?

LIST: We got up at three in the morning and went to the analysis center.

VAN DER HOVEN: You used synoptic map trajectories; no tetrons or anything like that?

LIST: No tetrons. The flight was over smooth terrain between about 200 and 400 miles and over water in the last 400 to 600 miles.

PACK: I have never seen better predictions.

FRENZEN: We might say something definite about the conversion to a constant K as the limiting case of the Taylor theory, which Mr. O'Brien mentioned earlier. Perhaps after prolonged diffusion times inhomogeneities tend to average out in some way, and one ends up with the final condition of the Taylor analysis, with $R(\epsilon)$ averaging zero for all larger ϵ , this corresponding to a constant K .

HOLLAND: If there is a gap in the turbulent energy spectrum between some sort of micro- or meso-scale fluctuations and the synoptic scale, you could be in this gap; and maybe if, instead of 2 to 4 days you took 8 to 20, you might find that the curve went up again as the cloud was acted on by the synoptic scale disturbances.

LIST: Yes. I think you could get values up to 10^{10} or 10^{11} , if you used the K theory to describe global circulations.

PACK: That is what Machta finds.

FRENZEN: But isn't it true that the clouds hang together for a period?

LIST: We have data only on clouds that hang together; that may be one difficulty.

FRENZEN: Tagged clouds have been followed.

LIST: Tungsten-tagged clouds were followed for a period of months up to a year or more. But again, I suppose there is a bias in these data that isn't pointed up, in that if a cloud were to rip completely apart, an aircraft just might not find it, and therefore one would have only the cases with smaller dispersion.

FRENZEN: Then you have a low-frequency filter on the turbulence?

LIST: Yes. Also, Heffter, in his paper, plotted σ_z as a function not only of travel period but also of travel distance in another diagram.

PACK: I might mention in connection with Holland's comment on the shape of the spectrum that, unfortunately, all our long balloon runs have been in oscillating meteorological situations. Indeed, this is why they were long. We have found the gap in the spectral distribution to be in the order of an hour or two, and then the contributions from the low-frequency portion of the oscillations were steadily rising, and only in one or two cases did we start over the second peak at the low frequency. In Los Angeles in particular there appears to be a spectral peak, and this was not a good sea-breeze regime, of the order of 5 to 6 hr, but it could be as long as 10 or 12, or as short as 3. The composite spectra showed a peak at about 5 hr. It remains either to use a larger number of balloons or to use an aircraft to get sufficient data over a long enough time, i.e., at least 72 hr of travel before we could begin to define the spectra.

CRAMER: In Kao and Bullock's paper in *Quart. J. Roy. Meteorol. Soc.* 90, 166-74 (1964), in which an analysis was done on wind measurements made in the conventional way - I think with Seattle as their point - the results of the analysis are not shown very clearly because the calculated spectra are plotted on log log coordinates. But plotted on semi-log log, they show a very high peak at about 7 or 6 days. Some work was also done at Salt Lake City, in which all types of comparisons were made; both

sets of results show a peak in about 6 days. Van der Hoven also has published a paper showing a peak there. I suspect that, after the diurnal wave, the next large energy input to the spectrum is probably in this region, and it must be associated with the systems that we see in weather maps and so on.

To make a general statement, it seems quite clear that what we are able to do in terms of explaining atmospheric motions - including turbulence as well as mean motion - does depend on the spectrum and on the scales involved, and that basically we cannot have very much confidence in modeling techniques unless they are tied to a gap in the spectrum and can approximate a steady state. I think this is a constraint which we have to adopt.

The energy inputs to the spectrum for a time shorter than 6 days do depend on the location and on time. I think we recognize that the kind of assumptions we like to make, namely, homogeneity and steady state, are acceptable only for very small scales; as we go to a larger scale, we must start to note many of the details about other types of circulation which are peculiar in the sense that they are related both to diurnal time and to factors entering into local circulations. There are some things that we will never be able to resolve; on the other hand, regarding a regularity that we can derive for this, we may find an acceptable answer. This situation has prevailed over the years. It is surprising that we know as much as we do about turbulence and diffusion in spite of the scarcity of empirical knowledge of the spectrum and of the scale. We could improve the situation, but there is still very much to be found out.

ANGELL: In the case of the Lagrangian spectral peak, the location depends a lot on the altitude. For instance, the transosonde flights from Japan show a spectral peak in the north-south velocity at a period of 48 hr, and this was the average time it took the transosondes to pass through the long waves in the westerlies.

PACK: The existing theory of the meteorological aspects of the meso-scale, which I think most of us agree refers to about 100 km or less, does not necessarily fit with the problems of the meso-scale on which we are working, which is often very much larger. For example, the Wind-scale accident encompassed practically all scales. The SL-1 accident at Idaho Falls covered almost the entire Snake River plain, some 150 by 50 or 60 miles.

Air Pollution in an Urban Environment

ROBERT A. McCORMICK

My remarks are set in the context of real-world, everyday problems – air pollution in an urban environment. We don't have one source for Q , or two sources. Every square centimeter of surface is a source of particles and dust in urban centers. Our sources are numbered in the thousands. They vary from ground level to hundreds of meters in the air. They vary in time and space and geometry. Some of them operate seven days a week, some five, some two, some one. Some power plants, for example, come in as needed, on an unscheduled basis.

We are dealing with a complex problem which varies in scale normally from zero to 10 or 20 miles and from 5 minutes to 5 days. We may have one or two or we may have a dozen or more wind measuring locations in this area. Our anemometers may be at the airport, near the runways. But frequently the exposure is most unsatisfactory because the plan engineer may decide the plant just needs "a measurement of wind." An instrument is bought and apparently just stuck up on the roof any old place, regardless of whether it is above the flow separation or not, or whether it is affected by local exhausts from vents or stacks, etc.

But in some cases the exposures are fairly good, and I am convinced that under light to moderate wind speeds, when there are the most air pollution problems, any wind speed one cares to name can be found somewhere in a city. The question is, what is the representative speed and where is it measured?

Hence I am particularly pleased with the results of the St. Louis experiments. At least during unstable conditions, the tetroons appeared to provide two things: (1) the effective direction and speed of the diffusing material, and (2) evidence that the statistics of the balloon displacement can be used in determining the spread of the continuously released tracer materials. These are transport and diffusion concepts that are not supposed to apply at all. They are supposed to apply to homogeneous, stationary turbulence, and the real conditions are not that simple. We are using equations that are presumably misapplied, but they work.

I do not think this is because the theories are particularly good (or bad, if that is one's point of view). However, a realistic appraisal of the progress that has been made in diffusion prediction, at least since 1958 when the Oxford symposium was held, must give most credit to the experimental findings of such investigators as Cramer, Fuquay, and Islitzer and to the "rational empiricism" of Hay and Pasquill.

DISCUSSION

GIFFORD: I would like to point out several aspects in which the problems in cities are simpler than some of those at atomic energy sites. First, in the city there is not the intense difference between having a source and next to it no source at all that occurs with a single-source problem. There is just less uniformity in the source distribution. Within a city it should ultimately be very easy to predict dispersions once one has a good idea of what the source patterns are, because they are controlled by the patterns of the source configurations much more than by the meteorology involved. Second, over a city the turbulence should be much better behaved, in some sense, than over open country. It isn't subject nearly as much to the factors which finally influence the turbulence over the country. I would not agree that a city is naturally a more complicated place. As to your remarks about the theory, the question is, now that you have solved the problem for St. Louis, will your solution work in Chicago?

McCORMICK: I agree that the city situation is much simpler in some respects provided one's tolerance limits are not too restrictive.

PACK: I am concerned with both single and multiple sources, and I think both present difficulties. There are perhaps more engineering problems in connection with the urban sources, the understanding of the emissions themselves, but I agree that it is difficult to follow a single effluent plume, particularly along some of the kinds of trajectories we have seen, and to interpret the findings.

BADGLEY: A colleague of mine often says that there is nothing so practical as a good theory, and sometimes nothing so practical as a semi-good theory. We talk as if the theory were bad because it doesn't meet every restriction that we want to put on it, but this may just as well be a strength as a weakness. It keeps us thinking about the problem and at least gives us some guidelines.

HOLLAND: I like to define a good theory as a theory that works. I think if a man develops a good theory he deserves credit for it, and if it is based on idealized assump-

tions, this only means that the things he threw out in order to solve the equations are the less important things. As you watch the data and find that they keep fitting the theory, even though the circumstances don't conform to the assumptions, it only means that the man was very shrewd in the way he truncated the problem, and this is the trick of developing a good theory. One always has to throw out a lot of things to get a theory. The important thing is to throw out the unimportant things.

PACK: There is no controversy between the multiple source urban pollution problem and the single source AEC problem. There are 9 nuclear reactors in Washington and its environs. NRTS, I think, has 12. Hanford also has a number of sources. Thus, the multiple source problem is not unique to urban air pollution, and the urban air pollution problem may not be unique much longer — Ravenswood almost went into Queens. The problems are going to mesh more and more.

Effects of Variable Source Configurations in Space and Time

ISAAC VAN DER HOVEN

I might begin with some definitions. When an engineer or project manager asks me, "What is my effluent problem downwind?" I usually reply by asking, "What kind of source configuration do you have?" By this I mean, in the spatial sense of an *xyz* framework, what is the initial distribution of material in space due to certain forces and effects. The first of these is buoyancy, which is largely a function of the difference between the temperature of the emitted material and the ambient temperature of the atmosphere. The second is the inertial force, which is a function of the stack velocity of a plume or the explosive force generated by an explosion of a rocket propellant. The third is the effect on the source configuration in space due to emission geometry, for example, the size of the chimney from which the plume is emitted or the area over which a rocket fuel is spilled or, in the case of urban pollution, the emission geometry due to the many individual sources. "Initial" distribution is that during the period before atmospheric motions come into play and start transporting the plume or effluent in a mean direction and dispersing it about the centerline of this direction. These definitions are not absolute. It is difficult to draw a line between the inertial and buoyancy forces and to determine at what point the atmosphere starts to disperse the material. But in discussing source configurations, we must limit ourselves to the initial forces and effects I have mentioned.

That is the spatial framework. There is also a time framework for the source configuration. This depends on whether the material is released instantaneously or in sequence, as from a continuous source. In the latter case, the question of whether the release rate is fixed or varies, adds to the complexity of the problem. Again the definition is not absolute. It is sometimes difficult to draw the line between instantaneous and continuous sources. We speak of quasi-instantaneous because we are not sure how short an emission period is instantaneous.

To summarize, by initial source configuration I mean the initial configuration in space due to the buoyancy of the puff or plume, the emission veloc-

ity, and the emission geometry effect, and the configuration in time due to the time interval over which the material is emitted.

What type of parameters are we interested in knowing or measuring with regard to obtaining information on source configuration? One area of interest concerns the emission point data, which are largely nonmeteorological, such as the emission rate and height of the stack. If the emission is not from a stack, is the release at ground level or from the side of a building such as a window or a door? What is the cross-sectional area from which the material is being emitted? Also needed is the energy content of the emission, which is related to the temperature of the material being emitted.

A second parameter is the cloud height. It is often called the effective stack height (as in the handbook *Meteorology and Atomic Energy*) or the stabilized cloud height. By "stabilized" height I mean that height at which forces such as inertial and buoyancy effects are no longer controlling, and the atmosphere is starting to disperse the cloud. As an example, in TVA diffusion work the centerlines of the plumes from the high TVA stacks gradually seem to rise, as measured by airplanes many miles downwind. Is this apparent rise due to atmospheric diffusion or to buoyancy?

A third parameter is the emission duration, that is, whether the source is continuous or essentially instantaneous.

The fourth type of parameter is related to the ambient atmospheric condition. It involves measurement of the wind speed, the vertical shear of the wind, the wind direction fluctuation, and the vertical temperature structure.

It is difficult to separate the effects on the source configuration of many of these parameters, especially since they are related. Initially the inertial and buoyancy effects might be the most controlling, but at the same time one cannot completely rule out the effects of turbulent atmospheric motions.

Why are we concerned with the initial source configuration? Because we wish to know the effect

of the source configuration on downwind concentrations. We want to know how it might affect, especially in the case of an elevated source, the maximum concentration downwind and the distance of this maximum. In reactor hazard analyses we are often concerned with the worst conditions and are therefore interested in the location and intensity of the maximum concentration. Also, in field sampling to get concentration distributions, we are interested in whether the spacing between measurements is close enough to give a significant distribution. The length of the time interval over which the sample is taken is also important in getting meaningful data that will allow interpretation of continuous versus instantaneous source data.

Figure 1 is the result of an exercise by one of the men at the Space Nuclear Propulsion Office of the AEC in connection with the problem of source configuration. He went through the mathematics of substituting a variable emission rate in space in place of the usual point source assumed in the Sutton equations, thus making Q , the source term, a function of height, and he solved the Sutton point source equation to get a centerline downwind distribution at the surface. Note that essentially lapse conditions were assumed with $c^2 \cong 0.2$ and $n = 0.25$. His first assumption on the source was one evenly distributed in a vertical line between the surface and 500 m, as denoted by the rectangular symbol beside the curve of the downwind distribution. The second assumption was a nonlinear increase with height such that the source was zero at the

surface and a maximum at 500 m as indicated by the appropriate symbol. For comparison, the downwind distributions of a point source at the surface and at 500 m are also shown.

Note that the maximum downwind concentrations within the first 10 miles result from a point source at the surface, and the lowest concentrations result from a point source at 500 m. This is not unexpected. The interesting thing is that under lapse conditions at a distance of about 3 miles, all the various types of source configurations tend to converge, and it doesn't much matter whether the material is all concentrated at the surface or at 500 m or somewhere in between. Let us keep this in mind when making estimates of downwind concentrations under lapse conditions at distances of tens of miles. In this case it obviously does not make any great difference what the source configuration is in the vertical.

DISCUSSION

PACK: I would like to add one other parameter to the list, the characteristics of the effluent itself. Is it a gas? Is it an inert gas? Is it particulate, and if so, what is the size distribution? Does it undergo any reactions in the atmosphere after emission that would change the source strength characteristics? This might apply to photochemical smog.

STROM: We encountered the complication of a heat source off to one side of the material source in the modeling phases. The heat source induced an ordered motion in the way of a longitudinal vortex, which brought an elevated point source down to the ground sooner than if there were no heat source.

PACK: This problem is in the intermediate area between the source configuration and the ground environment. I can think of a practical application of cold effluents passing over warm water, the Barnegat Bay reactor, perhaps.

HOLLAND: Frequently it is easier to get information on the distribution of concentration along the ground than on the vertical distribution of the source, and you have given a guide for solving this backwards. Not much can be learned from measurements made at long distances, but sampling at a proper range should help determine some characteristics of the initial source geometry.

ISLITZER: I think if you know the meteorology, that is, the dispersion as a function of time and distance, you can estimate the source effects. Source effects are negligible beyond 700 m, at least for releases on the order of 300 sec or so. I have a question: Is it mathematically proper to take an equation that was derived from a constant source and arbitrarily introduce a variable source, as apparently was done here?

VAN DER HOVEN: I saw no reason why it should not be done.

HOLLAND: You are adding a lot of incremental sources, aren't you?

Figure 1.

VAN DER HOVEN: Yes, essentially. It is a matter of integration.

PACK: The question is whether it is proper to use a constant vertical diffusion coefficient. Use of available empirical information about vertical dispersion, or use of the wind profile, would be all right. But I don't think anybody is trying to use micro-dispersion data for macro-dispersion.

CRAMER: There is a fundamental problem here: For certain states of the atmosphere we can define models that are reasonably adequate in the sense of steady state and homogeneity; but as we vary our source and release time, particularly as we shorten it down to the instantaneous source, we have to presume that whatever modeling we do represents the integrated answer over many trials, or some such specific argument. Furthermore the initial source dimension also has somewhat the same effect, namely that the most difficult prediction problem, or the widest scatter about a central value, will occur with both a small physical dimension and a short release time. But if you can change either one or both of those, the variability starts to shrink, and the effects of the initial dimensions (as Dr. Van der Hoven defined them) will depend on the initial dimensions and other things. But it is easy to visualize conditions under which very large sources, even of the instantaneous type, are determined by the initial conditions; the variation over a long time and other conditions will not matter. The solution depends on how the problem is defined.

ALLEN: Using the surface distribution to try to reconstruct the source is a common practice, especially at the Nevada test site. When we have had unknown sources, for which we did not know Q , we have compared these with prior atmospheric shots of low order, corrected for the meteorological conditions, shear, wind speed, mixing layer depth, and so forth, and come up with estimates of the source strength that were at least reasonable in comparison with what the radiochemists got. When there is no better way to estimate source strength, this method gives something that can at least be used.

PACK: There is a lot of backtracking. I recall Chamberlain's work in *Quart. J. Roy. Meteorol. Soc.* 85, 350-61 (1959) and Crabtree's work on the Windscale accident; this method was also used after the SL-1 accident. Regarding the situation at 3 miles, I think the evidence is that the source configuration certainly tends towards uniformity there; but within that 3 miles there are the problems of reactor site safety, or rocket fallback, which are very important and often very expensive problems. If the source is distributed so that wind variability can be taken into account, then the diffusion rate might become very different, and the evaluation problems become very different from those for a release all in one puff. Very real problems within the first 3 miles remain to be solved.

HAUGEN: I have heard at this meeting and elsewhere that in the first 3 miles or at some medium scale (I am not sure what this means) our estimates are pretty good, and I would like to know what pretty good means in terms of an absolute number, and to make it as explicit as possible. To use the Ocean Breeze-Dry Gulch experience as an example, I think 75 to 80% of the time, say, we can specify the expected concentration within a factor of 2. Is this pretty good?

SMITH: It is pretty good.

ISLITZER: We are getting about the same sort of results as everybody else. Maybe 10% of the time we can't make much sense out of the data. The factor goes up to 10 at great distances, but about 90% of the time our estimates are within a factor of 2 or 3 for several miles. The reactor physicists and engineers at Idaho Falls consider this to be pretty good.

HAUGEN: A factor of 2 or 3 is 150 or 200% error, and if we say this is pretty good, we can go on and work on other problems. Or do we say we can do better?

GIFFORD: I don't know who says "pretty good." We say "comparatively good."

ISLITZER: If you can specify pollution to within a factor of 2 or 3, isn't that pretty good? That's the main problem.

PACK: If we are talking about the difference between 100 rem and 200 rem it might create a problem. I'm not sure that we are not pretty good most of the time. Threshold biological damage or not detectable damage is one thing, but lethality versus illness becomes a problem.

SIMPSON: I agree our estimates are pretty good. We will have to see whether we come up with a better result by judging them in terms of plume limit. I am concerned about the distance factor after 3 miles with solutions for all source heights, and I would like to know if there are any data to show this result to be true. I have heard it inferred that it is possibly true, but I suspect that in the case of a stable aloft release mechanism, with things such as deposition or heat sources acting, we do not have enough data.

CHAMBERLAIN: Returning to the question of accuracy, in calculations for gas inhalation, the subject may be sitting quietly breathing about 6 l/min or he may be running and breathing 15 or even 50 l/min. Thus it doesn't help too much to predict the dosage too accurately, considering the possible effects of human activities.

MCCORMICK: When we predict concentrations for our customers, shall we say initially that they are correct to within a factor of 2 or 3?

GIFFORD: We have been doing it for years.

SMITH: I think it is a pretty good accuracy.

PACK: Over what time is this important - 24 hr, a 15-min exposure, etc.? Isn't this factor of 2 really for relatively short exposures, with the exception of Fuquay's extrapolation to longer times? Has anyone any evidence on a 24-hr release with concentration predicted correctly to within a factor of 2?

FUQUAY: We have primarily 30-min data from diffusion experiments, against which we can confirm our results. We can use $\sigma_{\theta u}$ divided by the length of the release to obtain the depression in the curve due to these variations, that is, take advantage of the meander averaging. We have run some experiments up to $3\frac{1}{2}$ hr. A 24-hr period is too long for our data, but we have a checkpoint of 4 hr, and I don't think we have a loss of accuracy.

SMITH: As far back as 1950 we were predicting hourly and longer-period beta-gamma dose rates from Ar⁴¹ consistently within a factor of 2.* The quantity predicted was

*Singer, I.A., *A Comparison of Computed and Measured Ground-Level Dose Rates From Radioargon Emitted by the Brookhaven Reactor Stack*, BNL 292 (T-49), May 1954.

the dosage from the argon at various monitoring stations out to about 8 to 10 miles.

PACK: Do we have a direct answer to Mr. McCormick's question? Are we officially committed now to prediction to within a factor of 2?

HOLLAND: Not always.

PACK: I hope not. Over flat terrain with reasonably stable winds, I would defy you to do this in the Los Angeles basin with some of the trajectories we have seen.

HOLLAND: Van der Hoven's graph suggests a neat trick. If in fact beyond 3 miles the levels are actually not sensitive to the source configuration, one can estimate the source strength from measurements at greater distances.

Unfortunately, this is where the errors begin to get bigger and bigger because of problems with the slope. Then one can use the trick Gifford used in analyzing smoke puffs: at the maximum point on the curve one knows that σ_z is equal to the height of the cloud. If one knew Q , and either the cross wind integrated concentration or σ_y , in addition to the central concentration, then one could measure both the source strength and the height from data of this kind. Actually the errors in this may be intolerably large. The chances are one would get the plume height within a factor of 2. As far as whether a factor of 2 is pretty good or not, Mr. Allen implied that with unexpected releases the radiochemical measurements aren't any better.

Cloud Rise Experiments at Brookhaven

JOHN A. FRIZZOLA

I will discuss some of the results of cloud rise experiments performed during the summer of 1962. Much of this will also be presented at the Air Pollution Control Association's annual meeting in Houston in June 1964.

The rocket propellant itself was in the form of cylinders about 6 in. in diameter. The procedure followed was to arrange the rods together, the length and number of rods determining the total heat release and the burning rate. The rods were placed upright, and the fuel burned from top to bottom. Sometimes little pieces of fuel broke off the top and ignited the bottom of the rods, which resulted in uneven burning rates, so that the total heat release was known but not the burning rate. The source diameter varied between $\approx \frac{1}{2}$ and 3 ft, depending on the number of rods used. Sometimes the fuel was burned in a bonfire configuration in an attempt to obtain a more even rate of burn.

The emission periods ranged from 30 sec to 3 to 4 min. Emission data therefore consisted of total emission and emission rate. The rate could be classified in the range between that for a large explosion (somewhat similar to those of the Air Force program in which the minimum amount of fuel used, a few hundred pounds, is the same as for our larger burns) and that of the heat used in orchards for frost prevention. The heat releases were $\approx 10^6$ to 10^8 cal. The term "quasi-continuous source" seems applicable since the source was not always continuous in time.

The figures are actual plume photographs. Some of the problems that have appeared in analyzing the data will be mentioned. The pictures of the plumes were taken by lapse time photography with a Bell and Howell 16-mm movie camera operating at 1 frame/sec. The camera was mounted on the small tower, 900 ft northwest of the main meteorological tower, near which the fuel was ignited. After the plume traveled beyond the camera's field of view, a person in a light plane observed the top of the plume as a function of distance downwind. Additional observers mounted on the large tower provided supporting data on the

height of the rise vs time. Some experiments were scheduled when wind conditions were near calm so that the plume rose to a maximum height. Others were conducted when the wind direction was northeast or southwest so that the plume traveled almost perpendicular to the line between the camera and the source. Analysis of the film and visual observations provided data consisting of the height of the top of the plume, elapsed time, and distance downwind. Supporting meteorological observations included wind and temperature at several levels on the 420-ft tower, and upper level winds obtained from a single theodolite and pilot balloon.

Figure 1 shows the plume rising during an inversion case. The lapse rate of potential temperature for the inversion cases was $\approx 8^\circ\text{C}$ between 5 ft and 410 ft. However, in most of the runs the inversion actually extended only to a height of about 200 ft, the lapse rate above this level being almost isothermal (in potential temperature notation). The mean wind speed near the level at which most of the plumes bent over, 150 ft, was 1 m/sec. This plume is shown in what might be called the first phase or the initial buoyant rise in the "bent-over" theory of Priestley or Scorer. Figure 2 shows the plume almost completely bent over or, as Priestley might describe it, having reached the position at which ambient atmospheric turbulence overcomes the mixing caused by the plume's own relative motion. In Figure 3, the plume is completely bent over, and its subsequent spread can be described by any of the current diffusion formulations.

In contrast to the preceding inversion run, Figure 4 shows the plume rising almost vertically upwards with little inclination. This plume behavior occurred under Brookhaven gustiness A or the very light wind-convective case. These conditions were observed after the inversion had lifted. Since the plume does not bend over in this type of case, the only important parameters that can be obtained are the height of the top of the plume vs time and the half-width of the plume. The ratio of the half-width to the height is important for determining

Figure 1. Plume during an inversion before bending over.

Figure 2. Same plume as in Figure 1, at initial bend-over.

Figure 3. Same plume as in Figure 1, completely bent over.

Figure 5. Plume during average lapse, moderate winds.

Figure 4. Plume during large lapse, light winds.

some of the initial parameters in the theory of Priestley or Scorer. The wind speed for these very light wind cases was near calm, and the lapse rate of potential temperature was $\approx -0.7^\circ\text{C}$ between 5 ft and 200 ft, becoming isothermal above. Figure 5 shows a lapse condition with Brookhaven B₁ gustiness, the prevailing daytime condition. The wind speed for this set of runs was ≈ 5 m/sec. Although the plume shows a definite bending, it is difficult to estimate visually where the buoyant rise of the plume ends and the purely diffusive part of the behavior begins. Both the camera and air-plane observations indicated that the top of the plume was still rising with increasing distance downwind. At these distances the spreading of the cloud should be due to atmospheric diffusion and not to spreading caused by the cloud's relative motion due to its buoyancy. A method is now being considered for distinguishing these two phases, using current diffusion formulations and parameters that are known to be applicable at Brookhaven.

In conclusion, some comments should be made about the measurements of lapse rate and wind speed for such experiments. Most theory requires that the wind speed and lapse rate remain constant with height. These conditions, of course, are never met in nature. It would seem important that the point or layer at which the wind speed and lapse rate are taken should also coincide with the various phases described in the theory.

DISCUSSION

MOSES: How many cameras did you use?

FRIZZOLA: Only one.

HAUGEN: How far away was the camera?

FRIZZOLA: 900 ft from the point of ignition.

SMITH: Since we had carefully considered photogram-metric errors, the only portion of the data for which the camera was used was very close to the tower. When the plume was a few hundred feet away from the tower, or above its top, we used the airplane for the measurements.

HOLLAND: I don't see why the so-called Holland formula should work.

FRIZZOLA: The "Holland" formula used was based on Sutton's quasi-instantaneous heat source solution modified for an arbitrary inversion lapse rate, and it seemed to give results with the correct order of magnitude.

HOLLAND: Sutton's formula is based on symmetrical diffusion from a central maximum, which I am sure could not be true of a hot puff, and I think Scorer's method must be much better. A comparison between Machta's method and Sutton's method appeared in *Bull. Am. Meteorol. Soc.* 31, No. 6, 215-16 (1950), and the results for some of the larger hot clouds from the bombs were widely

different by different methods. Sutton's method didn't seem to give the right order of magnitude, but it was the only thing I had at the time, and as far as taking the variations of lapse rate into account are concerned, the Sutton calculation is done assuming an adiabatic lapse rate, and you just see where it intersects the soundings. You can put in any kind of temperature sounding you want to and come up with a height, but I don't feel that the theory has any realism.

PACK: At the time, the Holland formula was the only crooked game in town. There are 4 or 5 of them now.

MOSES: I would say 15 or 20.

SMITH: A good theory is one that works, and yours is working at the moment.

HILSMEIER: Noting the work done at Brookhaven with photography, I wonder if time exposure photography might not be of benefit to determine the full extent of the cloud. Culkowski some years back developed a method for taking time exposures using neutral density filters (report ORO-359). You might be reluctant to try it, having only read his paper, but almost anyone can use the cameras. You mentioned you used professional photographers. With a little practice it is very simple to use the time exposure method yourself. Essentially Culkowski devised an exposure guide for determining the aperture setting for the filter factor which is related to the film speed and reciprocity failure. We have used this with Polaroid film and with regular 35-mm film. Since my knowledge of films is not great, I rely strictly on the information provided on the film packaging. In studying smoke plumes, time exposure photography is in my opinion a very logical approach.

Smoke photography has been used since early times in the study of diffusion, and I can think of nothing better to teach somebody coming into the field just what the effects of turbulence are. At new reactor sites, as soon as you show people the effects with smoke, they start thinking in terms of diffusion; whereas before they had practically no feeling for what actually would be happening, suddenly they know that they have a problem. The demonstration is very effective.

As far as source configuration goes, in demonstrating the effects of the shell, I refer again to the Halitsky study with the wind tunnel along with our study of the EGCR experiments. We made a time-lapse movie of several smoke bomb releases, which demonstrated the effect of the shell, the smoke clinging to the dome essentially producing an area source. (We showed this at the meeting of the Weather Bureau meteorologists at Oak Ridge.)

PACK: Could this photography be done under a bright moon?

GIFFORD: Yes, with fast Polaroid film.

MARKEE: We tried it in Idaho Falls under full moonlight. Professional photographers took all the readings, and hardly anything showed up.

GIFFORD: Didn't Houghton do some work at the Yankee site or one of the New England sites?

PACK: That was Austin. Did they use photography? They used smoke at night, illuminated with car headlights.

GIFFORD: The significant thing Culkowski did was to determine the exposure curve, taking into account the film reciprocity failure factor which makes it impossible

to use indefinitely the exposure factors supplied with the films.

ENGELMANN: Paul Nickola at Hanford is using a technique with fluorescent sources to determine the effect of source heights; perhaps something similar or a modification could be used for smoke if it were fluorescent. He supports a couple of aluminum poles at some distance downstream from the source, and puts string back and forth across the poles. After the test he winds up the cord, and unwinds it under fluorescent light. Knowing the diameter of the cylinder, he can locate the readings on the string.

GIFFORD: This was done in open air some years ago by Frenkiel, using a grid of chemical-impregnated threads.

FRENZEN: Frenkiel did that in a tunnel.

GIFFORD: I think he did it in a tunnel and also at low levels in open air. I think that would be very interesting to try, just to locate the plume.

ENGELMANN: The last thing he was using it for was to determine the dimensions of a puff.

PACK: Didn't Wilkins do something like that? Somebody used gauze, but I think he used direct sampling through gauze.

ENGELMANN: Nickola came out with an independent study.

GIFFORD: I didn't mean to dispute that. It strikes me as a really good idea and one that we ought to take up.

SMITH: I agree. We have used the same technique in the form of a grid of wires for our copper sphere release. All we do is take a count of the radiation along the surface of the wire. We don't need any additional help to determine the relative concentration configuration.

MOSES: Dr. Carson of our group has been using floodlights to illuminate the stack to get pictures at night.

GIFFORD: This probably would have some advantages in that the background would tend to be more uniform.

CHURCH: We have had some very successful nighttime photography of clouds produced from 150 to 2800 lb of high explosives near Tonopah, Nevada, during Operation Roller Coaster. Three to six K-18 aerial cameras were positioned on the ground at distances of 1500 to 20,000 ft on bearings chosen for triangulation. Cloud illumination was provided by aerial photoflash cartridges of 100 million candlepower fired 100 ft up with a mortar-type device. With illuminated cloud against dark night background, the image contrast is quite remarkable. In some cases, very clear pictures were gotten as late as 5 min after burst when the last flares were fired. An unclassified Sandia Corporation Technical Memorandum (SC-TM-243-63-72, *A Method of Using Flash Cartridges for Night Photography*, R.R. BEASLEY, October 1963) describes the physical setup used. Some further details are available in a confidential document published by DASA (POIR-2500, *Operation Roller Coaster - Interim Summary Report*, September 1963).

The Air Pollution Program in the Public Health Service

FRANCIS POOLER

We have done three major city studies since this program started 9 years ago, first in Louisville, then Nashville, and currently St. Louis. In the way of source information from Louisville, a number of inventories were made of particular industrial sources, but all the rest of the Louisville area was divided up into two areas, the west end, about 12 square miles, and all the rest, perhaps another 25 or 30 square miles. The only source information we had was average daily emission values on a yearly basis. It is obvious that we didn't do much in the way of modeling work at Louisville.

When the Nashville study was getting under way, McCormick requested more detailed source information, and as a result we were provided with inventories of sulfur dioxide. The first preliminary inventory was by one-square-mile areas, in relative units, and then this was expanded to include seasonal and diurnal factors by which these emissions should vary. It was on the results of that inventory that Turner's first model was based.

In the St. Louis study, we again wanted more information, and because we asked for it we are getting it. In urban pollution, the solution to the problem of the variability of the sources is quite clear: we ask for information. In some cases the information is not available. For instance, a couple of years ago, when Markee was writing about automobile emissions, we had so little information that all he could do was say the sources vary and treat them as a number of line sources. Since that time much more detailed studies are being made of the variation of emissions as a function of the driving pattern. These are first run in a laboratory, but are now being carried forward in active road tests, so that before very long we will have so much information on the possible variation of automobile emissions that we will be swamped with data.

This leads to the question of what to do when we get so much data that we can't handle it. Once we arrive at this point, we will have to figure out

why we really wanted the data. What use does anybody intend to make of the computations that we make from these very detailed source data? I suspect that in a few more years, at some conference on general air pollution studies rather than on their application in the nuclear energy field, we will have to face the problem of exactly what sort of detail really is wanted about air pollution measurements and distributions. At this point, I don't know what sort of simplifications might be applicable, because it will depend largely on how the data are used. The way new information on source variability has emerged confirms my opinion of several years standing that we have sufficient meteorological theory to use a good deal of source information; but the problem is that we could easily swamp ourselves with work having little point to it.

In the Nashville monthly model study we found roughly 50 to 60% of the contribution to sulfur dioxide pollution in any one area coming from the 5 source squares immediately around that area. Thus the first 3 miles, as far as air pollution is concerned, is obviously very important.

Looking into the future, I have been considering ways to simplify our problems. We could simplify considerably, for instance, the problem of inter-city transport of pollution, which the NYU group is getting into in modeling New York City pollution. They must naturally consider the pollution from Philadelphia, and, as Kussman said, they are planning to treat this as a point source. Obviously, they can't treat it as a constant source because they will have to take into account the diurnal variations. But the point is that once the source is, say, half a day's travel away, we don't really need any information about the distribution through the lowest 500 m or whether the material mixes in the depth of the mixing layer, which is 3000 ft. I think it is not the spatial relation of sources but the time relation that will be the main problem in this study.

DISCUSSION

PACK: In the use of the information eventually we encounter the problem of control, either meteorological or air pollution, and its effects. This has not been touched upon and is perhaps not germane, except that there are more sources of inadvertent or routine releases over which we have no control than there are diffusion experiments by several orders of magnitude. In the interests of economy and the statistical utilization of more tracer tests, we can model only if we know something about the source strength, and if we can learn how to handle these things, then we can extend our capabilities to develop better atmospheric diffusion models. We might be able to do something along these lines. For example, at Brookhaven, argon is much more likely to come out occasionally than F.P. over the top of the tower. Thus, this is one good reason for knowing something about source configurations.

CRAMER: In urban air pollution, in the formation of ventilation concepts for cities, for example, isn't knowing the depth of the mixing layer and transport the first approach?

POOLER: In Frenkiel's original work on modeling some of the Los Angeles pollution, he used the mixing depth and the transport. He did include diffusion too, but I think the main variations of concentration are to be explained in terms of transport rather than diffusion.

CRAMER: That is exactly the point. Sometimes in viewing a problem, we may miss an acceptable solution because we concentrate on complexities that may not be pertinent. One never knows in a complex situation what may be limiting. Meteorologists are now often in a favorable position compared with medical people and source people, whose areas of research in this field are in rather bad shape.

Deposition

ARTHUR C. CHAMBERLAIN

Deposition is important for three reasons. The most important is that the hazard analysis in many cases (though not all) depends ultimately on the deposited activity of radioactive material rather than on the dosage; therefore the rate of deposition needs to be known. The other reasons are that, even in the case of ordinary atmospheric pollution, deposition is involved in the attack of things like SO_2 on structures, furnishings, fabrics, etc., and that under some circumstances the self-cleaning properties of the atmosphere are very important in limiting the atmospheric pollution levels. Meetham showed that in the London fog of 1952, when there were 4 days of continuous heavy fog and very stagnant conditions, the limiting factor on the SO_2 levels was not dilution or mass transport of the material but the self-cleaning property of the atmosphere. The SO_2 was deposited in one form or another on various surfaces, at a rate equal to the rate at which it was being emitted from various sources.

Deposition is perhaps not an entirely satisfactory term, since some people think it refers almost entirely to the fallout of particles, i.e., to the deposition of particles by virtue of their terminal velocity. This is very important for particles of 10 or 20 μ or greater, but for smaller particles and for gases, deposition processes should really be considered as mass transfer. Deposition takes place equally on vertical as on horizontal surfaces, and terminal velocity plays a part but a smaller and smaller one as the particle gets smaller. Nearly all our work has been with particles certainly $< 30\mu$, and in many cases $< 1\mu$, so that the deposition processes are not controlled by terminal velocity. We have also worked with the deposition of gases, especially iodine. This is of fairly general interest in connection with SO_2 and also with the evaporation of water, because the same formulas that apply to the deposition of a gas to a surface apply to the evaporation of water.

In order to specify a mass transfer problem, three things are needed: (1) the boundary condition or surface condition, i.e., the value of the mass

volumetric concentration at the surface; (2) the volumetric concentration at some other height; and (3) some information about the structure of the boundary layer between the surface and the reference height. We have used for many years the expression v_d for transport velocity, which is defined as the ratio of the flux F to the difference between the volumetric concentration at some reference height, χ_1 , and the volumetric concentration at the surface, χ_0 ,

$$v_d = \frac{F}{\chi_1 - \chi_0}$$

In some cases it is more convenient to use the resistance, which is the reciprocal of deposition velocity. I think this term, $r = 1/v_d$, was first introduced by people working on evaporation, particularly at Rothamstead. It is convenient because resistances of this kind, like electrical resistances, are additive.

The limiting effect of the diffusion in the boundary layer is the sum of individual resistances corresponding to different parts of the boundary layer. The over-all resistance of a boundary layer consists of an aerodynamic term r_a , which is the resistance of the diffusion up to an area very close to the surface but not right at it, a boundary layer resistance r_b , which refers to the area very close to the surface, and finally a surface resistance r_s , which refers to the rate at which the diffusing molecule either actually reacts at the surface or is evaporated from it:

$$r = r_a + r_b + r_s$$

Sheppard (*Quart. J. Roy. Meteorol. Soc.* **84**, 205, 1958) in speaking of the analysis of mass transfer in terms of difference in vapor concentration, says:

"If one level were the surface itself, large temperature and humidity differences would be available for measurement. Unfortunately, there is as yet no generally accepted relation between flux and surface-to-air differences. The theoretical problem is formidable, but the figures provide for molecular diffusion of heat and vapor at a com-

plex surface in which, however, molecular viscosity is of no concern."

Reynolds' analogy between mass and heat transfer between two levels can be applied not very close to the surface, but it cannot be applied right down to the surface, because momentum is destroyed at the surface by the bluff body effect, which has no analogy in mass or heat transfer.

Owen and Thompson (*J. Fluid Mech.* 15, 321, 1963) analyze experiments in both heat and mass transfer. They assume that the same law applies to both, i.e., that Reynolds' analogy between heat and mass transfer can be applied right down to the surface, although the analogy between heat or mass and momentum cannot be applied right down to the surface. They define a dimensionless number B , called the sublayer Stanton number, by a formula

$$\frac{u_1}{v_g} = \frac{u_1}{u_*} \left(\frac{u_1}{u_*} + \frac{1}{B} \right).$$

$1/B$ is a measure of the departure from Reynolds' analogy, i.e., the degree to which the diffusion of a gas or the transport of heat to or from a surface differs from the transport of momentum. In the experiments analyzed by Owen and Thompson and in the experiments with ThB vapor described below, the surface resistance r_s is known to be zero, so that

$$1/n_g = r = r_u + r_v$$

where

$$r_u = u_1/u_*^2 \quad \text{and} \quad r_v = 1/u_* B.$$

Thus measurements of deposition velocity coupled with measurements of friction velocity, u_* , can be used to derive $1/B$.

In Sheppard's paper a very tentative suggestion was made that the diffusivity could be written as

$$K(z) = ku_* z + D,$$

i.e., as simply the sum of the eddy and molecular diffusivities. Though there is no good theoretical justification for this for a rough surface, it does in fact lead to a value of $1/B$ which, as shown below, is surprisingly near the correct answer. Both Owen and Thompson and Sheppard made $1/B$ a function of the friction velocity and roughness lengths. Sheppard's formula is

$$\frac{1}{B} = \frac{1}{k} \log \left(\frac{ku_* z_0}{D} \right).$$

Owen's formula is

$$\frac{1}{B} = \frac{\alpha(u_* z_0)^m}{\nu} \left(\frac{\nu}{D} \right)^n$$

where α , m , and n are constants, with values 0.5, 0.45, and 0.8, respectively.

I assume in all these cases that except very close to the surface the logarithmic velocity law will apply, as in fact I find it to apply, in the well-known form

$$\frac{u_1}{u_*} = \frac{1}{k} \log \frac{z}{z_0}.$$

Once again the essentials for measurement of $1/B$ are the volumetric concentration of a vapor at a certain height, the volumetric concentration at the surface, and the vertical flux.

For the surfaces in the wind tunnel I used an artificial grass made of quills, 8 cm long by $\frac{1}{2}$ cm wide. Its advantage over real grass is that the surface is very uniform. I have also used real grass with a roughness between 0.2 and 1.2 cm. I have used ordinary toweling on the surface, which has a roughness length of 0.045 cm. I have also used rough glass of the sort used by Owens (found in doors in some houses in Britain; I don't know whether this is found in America), a form of opaque glass with little pyramids about $\frac{1}{3}$ cm high on the surface. There is an important difference between the roughness elements of this glass and those of the other materials. With real or artificial grass and toweling, the roughness elements are fibrous, whereas with glass the elements are more obtuse and blunt; this turns out to cause a considerable difference in the value of $1/B$ because the more blunt the surface, the more the bluff body effect on the momentum transport.

For the activity in our tests, we prefer to use thorium B rather than iodine. With iodine the condition that the surface resistance be zero is not always fulfilled. For certain surfaces, such as wax, the uptake of iodine may be controlled not by the aerodynamics but by the rate of adsorption at the surface. For the ThB activity the condition $r_s = 0$ is very well satisfied. ThB is made by the decay of thorium, which is a noble gas and decays quickly. It has a half-life of 55 sec, and decays into ThA, with a half-life of 0.16 sec, and then into ThB, with a half-life of 10.6 hr, which is an isotope of lead. Thus, one can make a vapor of lead so dilute that there are perhaps 100 atoms/cm³ air, which will condense very readily on any surface. When this vapor is used in the wind tunnel, one must be sure that it remains a vapor and does not condense into

Th-232

Figure 1. Wind tunnel.

particles, and particles must be excluded from the apparatus, otherwise the ThB will attach itself to particles, and this will alter the diffusion coefficient. The diffusion coefficient of ThB in gas form (from measurements I won't describe here) is $0.054 \text{ cm}^2/\text{sec}$ and is not dependent upon humidity, so that if water vapor molecules attach themselves to these lead or possibly lead oxide molecules, the diffusion coefficient is not altered.

Quite recently, in an effort to find out more about the variation of $1/B$ with diffusion coefficient D , I have done experiments on the evaporation of water. Some of the surfaces, the artificial grass and the toweling, can be made wettable, and the temperature can be measured fairly accurately by means of thermocouples. From the temperature of the wet surface one can obtain the vapor pressure of water and thus a surface vapor pressure for insertion into the equations. Everything is the same except that the water vapor is going up whereas the ThB is coming down.

Figure 1 shows the wind tunnel, which is rather small. The working section is 1 ft square and 15 ft long. The wind velocities can be varied from $\frac{1}{2}$ to 10 m/sec. In the experiment shown, the surface is real grass or turf brought in from outside, and the volumetric concentration of ThB is measured as a function of height by small samplers. The wind speed is measured by a small Pitot tube (a hypodermic needle), and the velocity profile is obtained by varying the position of the Pitot tube. The air is monitored to make sure there are no particles in it, and a positive pressure of filtered air is maintained to exclude them.

One of the questions about work in the wind tunnel is how far it can be related to conditions in the open air. One must be sure that the working section is sufficiently long and that the boundary

layer is properly developed. This is not as severe a condition in deposition as in diffusion experiments, because it turns out that a large part of the over-all resistance to transport is in the lowest part of the boundary layer, and it is possible to work with boundary layers developed only to a thickness of about 5 cm.

Figures 2 and 3 show examples of profiles of wind speed and ThB concentration against logarithm of height. For both u and χ linear plots are obtained. The intercept on the z axis of the plot of u against $\log z$ is by definition the roughness length z_0 . The intercept of the plot of χ on the same axis can be called the roughness length for mass transport z_θ . It turns out that $z_0/z_\theta = e^{k/B}$. If $1/B$ is known, one knows where this line should meet the axis. In Figures 2 and 3 I have drawn the best line through the observed points from the known point of intercept on the axis. The extent to which the crosses lie on the lines is a measure of the extent to which the boundary layer is fully developed, and they diverge at the tops of the figures because the boundary layer for the ThB, the diffusing substance, is somewhat less fully developed than the boundary layer for velocity.

Not all the results give quite such a good fit as in Figures 2 and 3, but, taking all the results together, one computes the flux in two ways: first, one measures the flux from the activity measurements; second, one computes the flux from the slope of the profile, using the Thornthwaite-Holzman formula,

$$F = \frac{1}{k^2} \frac{(\chi_1 - \chi_2)(u_1 - u_2)}{u_*^2}$$

I reject experiments in which these two measurements differ by more than 25%. So I think that the boundary layer is sufficiently well developed and

Figure 2. Profiles of wind speed and Pb^{212} activity over short grass.

Figure 3. Profiles of wind speed and Pb^{212} activity over artificial grass 8 cm high.

that I can make use of Owen's formula for $1/B$, because this formula gives a unique value of $1/B$ independent of the reference height of the measurements only if one is working in a developed boundary layer.

Figure 4 shows the results of Owen and Thompson, giving the correlation of $1/B$ with $u_* h / \nu$ (h = equivalent sand roughness $= 30z_0$). The different points refer to different kinds of roughness, as listed. In every case Owen finds a variation of his parameter $1/B$ which is approximately as $\sqrt{u_* z_0 / \nu}$.

Figure 5 shows some of my results with ThB; $1/B$ is plotted against $u_* z_0 / \nu$ on a log scale. More recent results follow the same general pattern. With the rough glass, the open triangles are my results with ThB and the black triangles are Owen's results with camphor. The two sets of results agree fairly well where they overlap. With the fibrous roughness elements, the values of $1/B$ are considerably smaller, and, although there is some

variation with u_* , it is not very great, about 4 to 14 over a range of more than 100 in the parameter $u_* z_0 / \nu$. The dotted line in Figure 5 is the value of $1/B$ from Sheppard's equation, which fits the results surprisingly well. Not shown are certain results with the evaporation of water. In this case, with the different value of D ($0.25 \text{ cm}^2/\text{sec}$), the slope of the line for $1/B$ vs $u_* z_0 / \nu$ is again roughly $1/2$, and the ratio of $1/B$ for water to $1/B$ for ThB is about $2/3$. The value of $1/B$ for water is slightly lower but not much.

The results can be put into a more usable form. The relation above defining $1/B$ can be considered as a relation between v_0 and u_* . The rougher the surface, the larger the value of $1/B$; but the rougher the surface, the smaller the value of u_*/u . The net result is that in plots of v_0 against u , for different types of roughness, the slopes of the lines are very much the same.

Figure 6 shows the relation of v_0 and u_* , from a number of experiments by others as well as myself.

Reference	Flow	Roughness type	σ
● Nunner	Pipe	Circumferential rings	0.72
○ Nunner	Pipe	Circumferential rings	0.72
◆ Pinkel	Pipe	Helical rings	0.72
+ Lancet	Channel	Pyramids	0.72
■ Cope	Pipe	Pyramids	7.3
□ Cope	Pipe	Pyramids	7.5
⊙ Owen, Thomson	Plate	Spanwise humps	3.2
⊗ Owen, Thomson	Plate	Irregular pyramids	3.2
▲ Dipprey	Pipe	Sand indentations	1.2
△ Dipprey	Pipe	Sand indentations	2.8
▼ Dipprey	Pipe	Sand indentations	4.4
▽ Dipprey	Pipe	Sand indentations	5.9

Figure 4. The sublayer Stanton number: Reynolds number dependence.

Figure 6. Relation between v_g and u_* .

- I. Evaporation of water from artificial grass (made with filter paper to make it wettable).
- II. Evaporation of water from toweling (Chamberlain).
- III. Evaporation of bromobenzene from blotting paper (Pasquill).
- IV. Evaporation of water from an open water surface (Rohwer) with certain assumptions about the relation of z_0 to wind speed over water.
- V. Deposition of ThB to artificial grass (paper-covered brass strip).
- VI. Deposition of ThB to artificial grass (PVC strip).
- VII. Deposition of ThB to toweling.
- + Deposition of ThB to grass in the wind tunnel.
- Deposition of I^{131} to grass in an airfield.

Figure 5. Relation between $1/B$ and $u_* z_0 / \nu$.

Note that the transport velocity is plotted against the friction velocity, not against the wind speed. The slopes of the lines would vary more if the transport velocity were plotted against the wind speed. In the case of an open water surface under a wind, with waves or considerable ripples, I think the type of roughness would be more like that of the rough glass with the pyramidal surface than the fibrous type, so that the theory for fibrous elements probably would not apply. However, if I take Rohwer's results for the evaporation of water from the water surface (which I believe were obtained over a long series of evaporation experiments in America) and use the relation between friction velocity and wind speed over water given by Sheppard (*loc. cit.*), it appears that even the transport velocity of evaporation from a water surface fits into Figure 6. Not all experiments on deposition velocity can be expected to fit into the general scheme of Figure 6. When one finds a case that does not fit, one should consider why the

theory does not apply. First, is the surface resistance in that particular application really zero? Second, has one counted the whole surface? One feature that surprised me in working with artificial grass surfaces and also in the open air is the extent to which the vapor can diffuse through the roughness elements to the underlying soil; this might be quite important and, in fact, is an important factor where the herbage is rather sparse. Third, is the surface sampled truly representative of the terrain?

We have also done some work on the deposition of small particles. If the particle is very small, less than about 0.02μ , one can still treat the deposition in the same way as for a vapor if the agency by which the particle is deposited is Brownian motion. Then possibly the same formulas would apply if a sufficiently small value of D is used. Putting a very large value of D in either Sheppard's or Owen's formula increases the value of $1/B$; it follows, therefore, that for very small particles almost the only important part of the total resistance is that very close to the surface, probably within 100μ . The observations we have made in the wind tunnel confirm this. A comparison was made of the deposition of micron-sized droplets to the two types of artificial grass, one made with PVC quills and the other with copper strip covered with filter paper. This difference in surface texture made only a few percent difference in ThB transport, and none at all in roughness length. It made more than an order of magnitude difference in the deposition velocity of the $1\text{-}\mu$ particles. It appears that for very small particles the limiting factor on deposition is the texture of the surface, in the scale of tens or hundreds of microns.

A.C. Wells in my group has continued the work on the deposition of micron-sized particles to vertical surfaces described in my paper "Transport of Particles Across Boundary Layers" (TID 7641) given at Gatlinburg two years ago. The upshot of his work is to confirm Friedlander and Johnstone's formula for the deposition of particles on vertical smooth surfaces (*Ind. Eng. Chem.* 49, 1151, 1957). This theory relates the deposition to the distance traveled by the particles as a projectile through the laminar boundary layer by virtue of turbulent motion outside the boundary layer. This theory is surprisingly successful over a surprisingly large range of particle sizes and surfaces. In our case, with micron-sized particles, it is surprising that it is successful, because it turns out that the stopping distance is only of the order of the diameter of the

Figure 7. Transport of $1\text{-}\mu$ droplets to a smooth surface.

Figure 8. Deposition of *Lycopodium* spores to grassland.

particle: for a $1\text{-}\mu$ particle, given a turbulent velocity of about 50 cm/sec , the stopping distance is only 1 or $2\text{ }\mu$. But it seems that the Friedlander and Johnstone theory for a smooth surface does work. However, we have found that as soon as the surface has roughness elements larger than the stopping distance, the deposition is greatly increased. Dust or other extraneous material on the surface increases the transport because the roughness elements stick out into the boundary layer to a distance greater than the stopping distance which is the basis of Friedlander and Johnstone's theory.

At low speeds and for very small particles the diffusion mechanism (i.e., Brownian motion) is more important than the projectile-fashion deposition of Friedlander and Johnstone. Figure 7 is a plot of the transport velocity against the friction velocity for the transport of $1\text{-}\mu$ droplets to a smooth vertical surface. One curve is for Friedlander and Johnstone's theory of impaction; the other for the theory for diffusion to a smooth surface according to the well-known Colburn correlation, which relates the deposition to a smooth surface to a $2/3$ power of the friction coefficient. We verified Friedlander and Johnstone's theory right down to the stage where the stopping distance becomes comparable with the size of the particle; thereafter the diffusion mechanism begins to take over. But if this surface is roughened, even with roughness elements about $50\text{ }\mu$ high, which don't make the surface aerodynamically rough, the deposition velocities

increase greatly. An attractive theory would be that the virtual sink for particles was at a distance from the surface which was either the stopping distance of the particle, i.e., the distance over which the projectile-type motion would occur, or the roughness length, whichever is greater; and this may be true.

We have also done experiments on the deposition of $30\text{-}\mu$ particles (*Lycopodium* spores) to grass in the field and in the wind tunnel. Figure 8 shows the results of the field experiments, with v_d plotted against the wind speed at 3 ft . Here the terminal velocity is no longer negligible, being 1.8 cm/sec as shown by the line marked v_T . The diagonal line $v_T + u_*^2/u_{90}$ is the velocity of deposition that would be expected if the only resistance to transport were the aerodynamic resistance r_a . For $30\text{-}\mu$ particles the resistance r_b is not large, because these points approach the slope of this line; and I think this is understandable, because the stopping distance of $30\text{-}\mu$ particle is $>0.1\text{ cm}$.

Goldsmith in my group is doing work on the transport of particles under the influence of vapor gradients, some of which has been published in the *Quart. J. Roy. Meteorol. Soc.* **89**, 49, 1963.

DISCUSSION

MUNN: There is an implied assumption that if the diffusivities of momentum and of mass are equal (Shepard is convinced that they are, but others are not) mo-

mentum can be transferred by pressure, whereas mass requires mixing.

CHAMBERLAIN: I know this is uncertain. Most people think that the equivalence of eddy transport of momentum and mass is more exact fairly close to the surface; it is more exact at 20 cm than at 200 cm. Another partial answer is in the fact that the fluxes deduced directly by measurement and from consideration of the slope of the profiles of mass concentration agree reasonably well. The transport across the last part of the boundary layer to the surface is not the same at all for mass and momentum. That is the whole point of the work – to determine the factor $1/B$ which represents the difference.

SMITH: Most of your experiments, especially the ones in which you have attempted to achieve a situation with zero concentration at the boundary, have been done in your wind tunnel or tube. Is your group planning to attempt to reproduce any of these in the atmosphere? I know you can't with ThB, but how about anything else?

CHAMBERLAIN: This is a difficult problem. Peter Barry and I did some experiments at Harwell to find out to what extent the boundary condition $\chi_0 = 0$ was also satisfied for our iodine data. The experiments consisted of a comparison of the uptake on leaves of various sorts and on

artificial simulants of leaves. We find that copper foil or, even better, silver foil is a good absorber of gaseous iodine up to fairly high concentrations; therefore, it is probably safe to assume that a silver simulant of a leaf will give the same geometrical arrangement and hence the same arrangement and hence the same boundary air conditions – that it will give the value $\chi_0 = 0$ at the boundary. (Some of this work has been published in *Health Physics* 9, 1149, 1963). In the wind tunnel (smaller than the one shown in Figure 1) with controlled humidity, Barry found a marked effect of humidity on the deposition of iodine. In comparing the deposition on the real leaf with that on the silver replica, he found that the uptake was less on the real leaf. In the field we don't find such big differences: we have not found differences greater than an average of less than a factor of 2, that is, the real leaf gets somewhere between 60 and 100% as much iodine as the silver replica. The reason for this may be that plants grown indoors have rather exceptional surface characteristics designed to protect themselves from the low humidity indoors. Our recent iodine experiments, in which we take into account transfer to the soil as well as the grass, do seem to suggest that the boundary condition $\chi_0 = 0$ is satisfied.

The Current Status of Deposition Research

GILBERT S. RAYNOR

INTRODUCTION

The importance of a complete understanding of processes involved in removing pollutants from the atmosphere is undoubtedly evident to all attending this meeting. In view of modern developments in our society, such understanding is not only an intellectual challenge but also a practical necessity.

Emission of particles greater than a few microns in diameter may result in a dispersion pattern considerably different from the gaseous diffusion plume, since such particles not only settle at a significant rate but may fail to follow small-scale eddy motions. The deposition rate determines what percentage of a release is available for transport to some given distance and may influence such things as site sizes and exclusion zones. Although dilution may reduce concentrations of air contaminants to tolerable levels, deposition, in the broadest sense, is the ultimate cleanser of the atmosphere. From these considerations, deposition is often beneficial. When, however, the problem of surface contamination is more acute than that of air pollution, deposition is detrimental. The problems of contamination of soil, water, agricultural crops, human communities, and natural environments by radioactive isotopes, poisonous insecticides and herbicides, and industrial wastes are too numerous to discuss here.

The point is that deposition is a universal occurrence which ultimately affects the whole earth's surface as dispersion affects its atmosphere, and deserves, perhaps, more attention than it has received. The purpose of this discussion is to focus attention on current research needs in this field.

DEPOSITION PROCESSES

Known and presumed mechanisms of deposition can only be mentioned. Among the dry processes, gravitational settling and impingement are most effective for particles above a micron or so in

diameter. Electrostatic and thermal forces may be important for small particles, while molecular diffusion, adsorption, and chemical processes are operative for gases and very fine particulates. Wash-out by precipitation is undoubtedly the most effective of the methods occurring in the presence of moisture, although diffusion of very small particles to cloud drops may also be important. Several investigators have concluded that air pollutants are of minor importance as drop nuclei and therefore are seldom removed in this fashion. Condensation of water vapor on surfaces may play a role in removing small particulates.

Consideration should also be given to the process which is the reverse of deposition, re-entrainment or reflotation, which may be significant under some circumstances.

CHARACTERIZATION OF DEPOSITION

Several methods of characterizing deposition are in use. The most direct is a measure of total deposition per unit area per unit time. This is the only method when neither source strength nor air concentration is known.

The deposition coefficient, p , which is the ratio of the deposition per unit area to the concentration per unit volume is useful where the air concentration is measured and denotes the thickness of cloud cleared or the distance the cloud apparently settles in travel over unit distance. More frequently used is Chamberlain's deposition velocity, v_p , the ratio of the deposition per second to the air concentration measured at a low level. This represents the rate at which particles are removed from the cloud. This rate may differ widely from the gravitational settling rate of the particle.

The variation of these expressions of deposition with meteorological conditions, particle parameters, surface characteristics, distance, and time has been little studied, and a knowledge of these variations is one of our most pressing research

needs. The effect on p and v_p of the height at which concentration is measured also needs study.

MEASUREMENT OF DEPOSITION

Deposition may be measured on natural surfaces or on artificial collectors, or it may be inferred from changes in the flux of airborne material. Measurement on natural surfaces is difficult or impossible in many situations, but experiments should be designed to include such measurements whenever feasible. Artificial surfaces are useful in measuring relative deposition but are meaningful for quantitative measurements only when the ratio of their catch to the deposit on the surface which they are used to represent is known as a function of the conditions prevailing during the experiment. Unfortunately, it may sometimes be more difficult to determine this relationship than to measure on the natural surface. Evaluation of deposition collectors is much needed.

Calculation of deposition by inference from air concentration measurements is normally feasible only over relatively short distances or under stable conditions when the bulk of the plume remains within the vertical sampling network. Even under these restrictions, great care must be taken to prove that the tracer is conservative under the experimental conditions, and that the representation of the airborne concentrations is accurate.

STUDY OF DEPOSITION

An understanding of deposition and the processes responsible for it is being obtained through the formulation of physical and mathematical theories, by laboratory experiments, by field experimentation with controlled releases, and by observation and measurement of natural and accidental emissions. These methods are interdependent. All are necessary but none is sufficient. Theory must be confirmed by experiment, but only theory can unify and explain experimental findings and, in the absence of unlimited empirical data, predict future results. Many processes operative in the field can be studied only in the laboratory, but results obtained under laboratory conditions may not be valid in the natural atmosphere. Because of these difficulties, we have been slow to acquire the limited knowledge now available. Future progress demands concerted effort in all phases of study. Our present status will be briefly summarized.

SUMMARY OF DEPOSITION THEORY

Schmidt⁵⁶ introduced the first useful deposition theory, the "tilted plume model" in 1925. More recent workers, Baron et al.,¹ Chamberlain,⁴ Bosanquet et al.,² Csanady,¹⁶ and Van der Hoven,⁶⁸ adapted the concept to modern diffusion theory and added corrections for depletion of the plume. Chamberlain⁴ first introduced a correction for washout. These models are all based on theories developed for gaseous diffusion with terms added to compensate for loss of material by deposition or washout.

Within the last decade it was realized that the gaseous diffusion plume model must be modified for application to heavy particles, since these do not follow faithfully all air motions and may even affect air movement. Notable contributions along this line were made by Rounds,⁵⁵ Liu,⁴⁰ Godson,²⁵ Yudine,⁷³ Smith,⁶⁰ Smith and Hay,⁶² and Csanady.^{17,18} Karol⁷ and Pressman³⁸ and Davidson and Herbach²⁰ formulated theories for the diffusion of polydisperse particulate clouds, and Calder³ called attention to the fact that boundary conditions previously assumed at the earth's surface may be invalid. Smith⁶¹ studied the effect of varying degrees of deposition on the diffusion of particles or gases in model atmospheres.

At present these various theoretical models remain largely untested. Although even the most sophisticated may not fully explain the physical processes involved, the most simple may be adequate for some engineering applications. A great need exists for experiments carefully designed to evaluate and verify existing theory. Unless this is done, development of more complicated models will have limited usefulness.

Washout has not been neglected by the theorists. Among recent studies are those by Pember-ton,⁵² McDonald,^{46,47} Vaughan and Perkins,⁶⁹ Karol,³⁷ and Makhon'ko.⁴¹ Since removal by precipitation processes is extremely complex, probably involving several little understood features such as electrostatic forces and water vapor gradient as well as direct impaction, it is difficult to judge how well these theories represent natural washout, and much experimental evaluation is needed.

SUMMARY OF LABORATORY EXPERIMENTS

Numerous laboratory studies have been reported in the literature. Kordecki and Orr³⁹ and

Corn¹⁵ have studied and evaluated the forces involved in the adhesion of particles to surfaces. Impaction of particles on obstacles was investigated by Sell,⁵⁷ Gregory,²⁶ and Gregory and Stedman.²⁷ The deposition of particles in tubes, ducts, and similar confined spaces has been studied by numerous workers including Friedlander,²² Friedlander and Johnstone,²³ Thomas,⁶⁶ Owen,⁵¹ and Postma and Schwendiman.⁵³ Slack⁵⁹ found that a cluster of particles may fall at a rate greater than the settling velocity of any individual particle.

Among gases, only iodine has been widely studied, chiefly by Chamberlain and his associates,^{5,7-10,12,13} and Megaw and May.⁴⁹

Numerous experiments on the capture of particles by water drops have been reported, including those by McCully et al.,^{44,45} Oakes,⁵⁰ and Walton and Woolcock.⁷⁰

Although these various experiments give valuable insight into the mechanisms and processes involved in deposition, their results are often difficult to apply to conditions in the free atmosphere. Many careful experiments are needed to extend this type of experiment to field conditions.

SUMMARY OF FIELD EXPERIMENTS

Field experiments will be discussed in several categories, classified chiefly by the quantities measured.

Numerous experimenters have measured deposition only, principally on artificial surfaces. Among the earliest was Stepanov,⁶⁴ utilizing various spores dispersed from point sources. Representative of more recent similar work are the experiments of Sreeramulu and Ramalingam.⁶³ Other experiments conducted with pollens, spores, and plant disease agents are reported in the agricultural literature. Yeo et al.^{71,72} reported on the deposition of insecticides released from low-flying aircraft.

Among the more valuable recent studies are those of Hage and associates at Suffield.²⁹⁻³⁴ They studied the deposition of large particles (100- μ -diameter glass spheres) and included the influence of a distribution of sizes on the deposition pattern. Csanady¹⁹ recently reported on an experiment using 100- and 200- μ -diameter glass beads in which wider lateral dispersion was obtained from the smaller particles. A similar effect was observed in experiments conducted at Brookhaven using

uranine and radioactive copper spheres 2 to 10 μ in diameter. These data tend to confirm the theories on the dispersion of heavy particles.

Simpson⁵⁸ reported the results of some of the Green Glow experiments in which air concentration or dosage only was measured and deposition was inferred from depletion of the airborne material. Islitzer and Dumbauld³⁶ also inferred deposition in a similar fashion, although some deposition measurements were taken also.

Numerous workers have measured air concentration and deposition at one or more points from experimental releases and have calculated velocities of deposition from the data. Among these are Chamberlain et al.^{10,13} using radioactive iodine and Megaw and Chadwick⁴⁸ using radioactive fission products and thorium. Tests conducted by Convair-Fort Worth for the U.S. Air Force⁶⁷ at the National Reactor Testing Station in 1958 used fission products from fuel elements as the tracer and gave values of v_p for sand, water, and sticky-paper surfaces for 5 isotopes. Gregory et al.²⁸ used two different sized spores as tracers in a series of short-range experiments. Experiments by the Brookhaven Meteorology Group using radioactive copper spheres in three sizes and uranine dye as tracers resulted in values of v_p over a 100-m-square sampling grid. Deposition was measured both on the natural grass surface and on sticky-paper collectors.

A number of workers have obtained values of v_p from accidental releases. Measurements from the Windscale accident were reported by Chamberlain and Dunster,¹¹ Stewart and Crooks,⁶⁵ and Chamberlain.⁶ Measurements following an accident at the National Reactor Testing Station were reported by Islitzer.³⁵

Measurements from experimental releases of both air concentration and deposition in sufficient detail to allow calculation of the total flux passing through two or more planes and calculation of the total deposition on the ground between these planes have seldom been either attempted or obtained, although this is the ideal experiment and the only way in which all doubt concerning the validity of the measurements can be removed. To be certain that all measurements are correct and that all factors have been considered, agreement between measured deposition and deposition calculated from the depletion of airborne material must be obtained. The experiments of Islitzer and

Dumbauld³⁶ attempt this ideal, although deposition measured on artificial collectors was about an order of magnitude lower than calculated deposition. Experiments at Brookhaven using ragweed pollen released naturally from circular plots of plants have resulted in measurements which often agree within a factor of 2 in spite of a relatively crude measure of deposition on artificial surfaces.

Several field studies of washout using either natural or simulated rain have been reported, including those by McCully et al.,⁴⁵ May,⁴³ Gatz and Dingle,²⁴ and Engelmann.²¹ Some doubt has been expressed as to whether artificial drops can duplicate natural rain with respect to electrostatic charge and other characteristics. Regardless of this, many more experiments are needed with a variety of tracers and a range of rainfall rates before definitive answers can be expected.

Few studies of the variation of deposition with other parameters have been reported. Corcos¹⁴ reported on the small-scale nonhomogeneity of fallout deposition, stressing the effect of local topography. Martin⁴² also found fallout deposition related to topography. Romney et al.⁵⁴ reported that plant foliage traps smaller particles selectively. These are but an indication of the studies that can be made.

SUMMARY OF RESEARCH NEEDS

In summary, five major areas of research exist in which additional information is essential to advance our knowledge of deposition processes. The specific needs are:

1. Experimental evaluation of existing theory.
2. Experimental determination of the behavior of particulates very close to surfaces, including more precise definition of boundary conditions.
3. Additional field data from well-designed experiments in which deposition can be compared to depletion of airborne material. Such experiments should be conducted with differing tracers, over diverse types of terrain, and under a wide variety of meteorological conditions. Such data would furnish empirical relationships useful in engineering applications even if existing theory should prove inadequate for prediction.
4. Data from experiments designed to measure variation of deposition with meteorological conditions, particle parameters, surface characteristics, distance from the source, and time after release.

5. Continued improvement in deposition theory to model more realistically actions taking place in the natural atmosphere.

If these objectives can be pursued by even a few groups, a considerable increase in our understanding of deposition should result.

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The Measurement of Deposition

NORMAN F. ISLITZER

RELATION OF DEPOSITION TO METEOROLOGICAL VARIABLES

The deposition measurements that have appeared in the literature have not yielded a clear-cut relationship between deposition velocity and meteorological factors, so that at the present time it is not clear that deposition is a meteorological problem. A relationship between deposition velocity and such quantities as the Richardson number, vertical temperature lapse rate, friction velocity (u_*), wind speed, vertical wind (σ_w), or Austausch (K_z) should be discovered if changes in meteorological conditions cause significant effects upon deposition. The Fission Products Field Release Tests #1 and #2, in which depositions on various media such as flat plates, grass, sand trays, etc., of fission products from melted reactor fuel elements were measured, showed no relation to lapse rate and a very slight relation to wind speed. Comparisons of deposition from site to site for the same meteorological conditions have not been practical because of complexities introduced by different tracers, different sampling heights and detection media, and considerable scatter of data. As a result, for practical operational use, average or best recommended values of deposition velocity for certain isotopes have been given, but variations with meteorological factors either are ignored or not recommended. A relation between deposition and speed for small particles in smooth pipes, with increasing speeds resulting in increasing deposition velocities, has been found, so that wind speed may be a controlling meteorological factor.

QUANTITATIVE THEORETICAL STUDIES

Theoretical treatments of deposition usually have related the deposition velocity to such terms as the friction velocity or vertical coefficient of eddy diffusivity¹ K_z ; vertical gustiness² σ_w ; and to the wind speed, friction velocity, and stopping distance of the particle.³ The theories of Owen and of Friedlander and Johnstone require that the par-

ticle be projected by turbulence across a boundary layer, so that the particle departs from the streamline and impinges upon the surface. The estimates of this stopping distance require that the assumptions of the variations of diffusivity and gustiness with height be applicable down to as small as 0.01 cm from the surface. These theories do not clearly introduce the meteorological factors, some of which are based largely upon assumptions or conditions experienced in a wind tunnel for smooth flow. The height at which such parameters as u_* , K_z , σ_w , etc., should be measured, the scales and eddy sizes, and other features are not discussed. Measurements of friction velocity, gustiness, etc., made at representative heights such as a meter or so above the surface yield values which do not give the right order of magnitude for deposition velocities. It appears that, if meteorological factors are important, these quantities should be measured extremely close to the surface by high speed transducers. At present there does not appear to be a comprehensive and rigorous treatment giving the deposition rates over surfaces. Some insight into the problem has been gained, at least, from wind tunnel investigations and flow in smooth pipes.

TRACER DEPLETION

Most deposition measurements have been made on plates, grass, trays, and other media placed on the surface in the hope that these would simulate the deposition on the natural vegetation and terrain. The actual relationship between such detectors and deposition on the surface is not known, and there are suggestions⁴ that deposition measurements on flat plates are not of the same magnitude as deposition over irregularly shaped terrain such as sagebrush and high grass. Therefore, deposition velocities now measured may not be appropriate for computing the depletion of the cloud, which is needed for a correct concentration computation. The effect of tracer depletion upon the vertical profile of concentration is also not understood. In

fact, computations of deposition velocities in which air concentration at some height, usually a meter or two above the surface, was taken may not give consistent results because of apparent plume rise, or effective removal of particles from the bottom part of the plume. It appears that deposition measurements should include some attempt to measure concentrations with height, so that a meaningful air concentration can be selected:

EXPERIMENTS NEEDED

1. A set of measurements using the same tracer, same detectors, and same techniques of analysis should be carried out at one site under a variety of meteorological conditions to determine if the deposition varies with meteorology. Such experiments should also be conducted using the natural vegetation and terrain as the deposition collection medium, if results on plates show that there is a meteorological effect.

2. If changes in deposition are not correlated to meteorological measurements made at convenient heights, say at 1 or 2 m or above, it may be necessary to explore the layer in the first few centimeters above the surface. This layer is suspected of being adiabatic and thus fairly constant with time, which suggests little variation of deposition with meteorological factors. Probes for exploring this layer may be difficult to develop, and data reduction and analysis may be an extremely complicated problem.

3. The effect of the ground surface upon the vertical profiles of air concentration should be examined with identical meteorology by releases at different heights. This could be performed with a multi-tracer technique, which is feasible. Different tracers can be released at the same time from different heights on a tower and the effect of source heights and the ground upon the plume can be studied in great detail under what are known to be similar meteorological conditions. Different fluorescent tracers that emit at different wave lengths, or activation techniques of originally nonradioactive particles, appear to be possible tools.

4. The deposition or cloud depletion over a high canopy of grass, foliage, or trees is also needed. The mechanism, with seepage into a canopy and also the later expulsion of air, may be quite different

from the dry deposition of particles over terrain with short grass or soil surface.

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DISCUSSION

CHAMBERLAIN: I have tried to work out deposition from the profiles of concentration in field trials, but only at very close range. There are two difficulties. If one tries to do this between two heights, say 30 cm and 1 m, one will be looking for differences in the order of 5%.

ISLITZER: Very small.

CHAMBERLAIN: If one is trying between 30 cm and 10 m, then one has a very long distance of travel before the profile is developed out to this height.

ISLITZER: This is true. The problem of what concentration measurements to make and at what heights is very complicated. This approach has limitations because of the atmosphere required for the concentration.

CHAMBERLAIN: From the difficulty which Pasquill and others have had with evaporation, the fact that one has to measure temperature to 0.01 °F, I don't think there is much hope of doing this.

PANOFSKY: Dr. Chamberlain, have you found any systematic difficulties with the Holtzman formula? Theoretically this gives too small answers under unstable conditions, and the error can be of the order of practically a factor 2 or 3. Perhaps you think the accuracy is not that good.

CHAMBERLAIN: Does this not depend on which two reference heights you take?

PANOFSKY: No. It depends on the Richardson number, and the greater the height, the more negative are the Richardson numbers, and the worse answer one gets. But if one takes 1 m and 30 m, the Richardson number will be quite a large negative, and the bias will be of the order of a factor of 2 or 3.

CHAMBERLAIN: I agree. This is an additional reason why one can't do it between 1 m and 30 m. It cannot be done between 30 cm and 1 m as the differences aren't big enough. It can more nearly be done between 1 cm and 5 cm because there the differences are big, and the Richardson number is practically zero. That is what I was trying to do in the wind tunnel. The flux so deduced was compared with the direct measurement, but it was not an essential part of the experiment.

Deposition Measurements at Hanford

CHARLES L. SIMPSON

We have conducted three kinds of experiments at Hanford during the last three years. In one type we make indirect calculations by considering the flux through arcs. Figure 1 shows the data from a completed run. The mass flux, grams of zinc sulfide per square meter, through a vertical plane at a distance of 200 m from the source, is given by the numbers on the isopleths. Since the height is plotted against the azimuth, this graph really shows the cross section of the plume. Since this plume is contained beneath the tower gridwork and on the sides, the differences in mass flux through successive arcs are equivalent to the deposition on the ground. The dots indicate the positions of the samplers at each tower. There are towers at 98°, 106°, 114°, 122°, and 130°. In addition, the dots at each 1° mark represent samplers that define the exposure at 1½ m above the ground. This is a very well contained plume. Elaborate steps can be taken to check isoplething and integration over the areas. Having several men redraw the isopleths, we obtained good consistency in the integrated areas. This was a very interesting test.

Quite aside from the deposition studies, note that the wind direction shear in this stable case was very strong – the winds were southwest at the ground and northwest at the top of the plume.

We wanted to make some direct measurements of deposition, and, since measuring zinc sulfide on sagebrush is very complex and uncertain, we tried to duplicate the experiment by releasing zinc sulfide over dried cheat grass, which is a light brown, very dry grass with burrs on top. In the field it looks similar to wheat, being very uniform and about a foot high. We released the zinc sulfide plume at the same height near the ground as we did in the diffusion experiments, under stable conditions, for a period of about ½ hr. After it passed over the field, we took very extensive grass samples to distances of 200 m from the source. The grass samples were ashed in an oven at about 500°F, and the amount of zinc sulfide was determined by scintillation techniques.

In Figure 2 the results are compared with the values from the first experiment. The ratio Q/Q_0 is the percentage of material left in the plume after

Figure 1. Cross section of mass flux 800 m downwind from source.

any given travel time. Material is lost very near the source, so that about 20% is lost by a few hundred seconds. This agrees fairly well with the estimate from the flux measurements. We would have liked to take measurements at greater distances, because we recognize that there are some uncertainties in working very close to the source. At present our technique will not allow this, and we will have to make some improvements before we can measure at distances of the order of 1000 m. One of the significant findings clearly shown in Figure 2 is the amount of the total deposition within a distance of 3200 m downwind (2 miles) or 2000 sec. This deposition is of the order of 70 or 75%; that is, 70 or 75% of the material has been taken out of the plume. This may seem rather high, but it is reasonably consistent with deposition velocities of $\frac{1}{2}$ to 1 cm/sec, which are quite common.

The final experiment to be described was done in July 1963. We released iodine for a period of 40 min near the ground into stable atmosphere at a uniform release rate. At a distance of 200 m downwind, we had prepared some rye grass plots and some geraniums, plus some artificial flowers. These tests are the first of a series. The objectives were to test the source equipment for uniformity of output; to test the relative efficiency of various kinds of samplers in the field; and to make some

basic deposition measurements to get a calibration for later tests. The sources were found to work well, and we selected from the field samplers one or two that were very efficient.

Some of the deposition results were interesting. First, the deposition velocity over the rye grass was calculated to be 1 cm/sec, but of the total deposition, 58% was found on the upper half of an 8-in. rye grass blade, 29% on the lower half, and 13% in the soil. We found that we could remove 12% of the total deposition on the grass by shaking it. This is a surprisingly large amount, and is indicative of what might happen if one were cutting grass with iodine on it. We also found that the deposition per unit area of grass was greater over rye grass, the green grass, than over natural dry vegetation in the same area. Finally, with the geraniums, we found that a certain amount of iodine was absorbed into the leaves. Of the total amount deposited, 10% was absorbed into the leaf and retained there and not delivered to the root system. Some of the people doing similar studies in the laboratory found some conflicting results. For example, they found that the geranium leaves will absorb not 10 but 90%. Thus, experimental designs and controls should be very stringent.

DISCUSSION

BARAD: Were the basic data on Figure 1 measurements of the product $u\chi$?

SIMPSON: Yes.

BARAD: In other words, there were no continuous samples of wind as well as of particulate matter.

SIMPSON: Yes.

BARAD: You had a peak at a height of about 6 m above the ground. Since you were using essentially a continuity method which involves the integration in the azimuth direction and in the z direction of the mean value over time of $u\chi$ rather than $\bar{u}\bar{\chi}$, there is still another term in the continuity expression, which unfortunately it has never been possible to measure in the field because measurements have not been available at the same point of both wind and concentration (or exposure). Therefore, there is a question here with regard to the methods. If the source is a ground source and the peak concentration at an azimuth of 114° is on the ground, one might argue as to the magnitude of the missing term. One could agree, perhaps, that whenever u' is high, probably air from above is bringing in a low χ' so that one could, in the mean, determine the sign of the mean value of $u'\chi'$. But on this particular figure the height of the maximum is at about 8 m above ground, and I wonder how sure one can be about the sign of the missing term.

SIMPSON: The mass flux shown is the product of $\bar{E}$ times $\bar{u}$, or actually the exposure. Taking into account the

Figure 2. Plume depletion versus time.

wind shear with height puts the center where it is located in the figure. The highest actual exposure is at about 1½ m. We tried to estimate what $E'u'$ would be by considering what the reasonable values of K would be, taking the ratio $\overline{E'u'}/\overline{E}u$, and letting the turbulent probability $\overline{E'u'}$ be equivalent to K_x (the partial differential of E with respect to x). We have a great deal of data on the partial differential of E with respect to x . In order for this term to be significant, K_x must be on the order of 6.

DAVIDSON: Did you measure the winds alongside these towers?

SIMPSON: Not for each tower. We measured the wind near the source and at 1 mile. The procedure has been to plot all vertical profiles. In addition, we sometimes use a short portable mast; therefore, in some cases we have three measurements which we plot all together, and they are reasonably close. In the range of 1 cm/sec the variation may be as high as 0.2, but I average them to obtain a mean profile for the entire grid.

CRAMER: Dr. Chamberlain, do you think Mr. Simpson's deposition velocities are reasonable?

CHAMBERLAIN: With regard to the finely divided particulate, I consider it rather difficult to deduce the deposition velocity from the Q/Q_0 plot because the answer is critically dependent on the height of release. As regards the iodine results, they are surprisingly much in line with what I would expect, particularly the variation of the deposition down the rye grass, 58% on the top part and 13% penetrating to the soil. But I think there is a lot of uncertainty about the deposition velocity of, say, 1 or 2- μ particles over a surface, and our results suggest that 1 cm/sec would be towards an upper limit. I think it is dependent on the micro-scale of the surface and not on macro-roughness.

SIMPSON: I should have mentioned the conservative property of zinc sulfide. Some people have found that certain kinds of zinc sulfide when exposed to sunlight lose their fluorescence, and that this may be wrongly interpreted as deposition. This is not the case with our zinc sulfide. We have run many tests, extending from hours to days in duration, and it has not shown any appreciable sensitivity to sunlight. The loss of fluorescence is negligible.

FUQUAY: In comparing deposition velocities over the Hanford surface and other surfaces, one should carefully consider the Hanford surface, which is comprised of cheat grass on the ground with 1 to 1½-m-high sagebrush, which has a small leaf and woody structure and is quite dense. In the proximity of the sagebrush, the kind of deposition process taking place is different from that in some of the other studies.

CHAMBERLAIN: I agree. Possibly the sagebrush acts as a sort of filter that raises the effective sink for particles, and it affects that much more than it affects the friction velocity.

HOLLAND: Lyle Schwendiman uses complicated non-dimensional parameters that I don't quite understand to plot his experimental data, something like a fourth power variation of v_s with the wind speed in the pipe. He seems to have a good theoretical explanation for this, having to do with the increase in both the impaction efficiency and the turbulence. Dr. Chamberlain's experiments have been conducted under a relatively narrow range of wind speeds, and therefore this sensitivity to wind speed has not been brought out well by the data.

CHAMBERLAIN: Insofar as we have varied the wind speed, our results do agree with this very strong variation.

HOLLAND: This is for particles in the 1 to 10- μ size range.

CHAMBERLAIN: I agree with it because the stopping distance of the particle is proportional to turbulent velocity, which is proportional to wind speed, and because the strong variation of diffusivity with distance in the last part of the boundary layer means that the value of this stopping distance is very critical. But with the particle size we are using, diffusion begins to take over at low wind speed. The diffusion transport, although small, does not vary so strongly with wind speed as the projectile-fashion transport. One has to use monodispersed particles to illustrate this fact. It's a bit academic, because monodispersed particles don't occur in nature except in the case of spores and pollen.

HOLLAND: This tendency would drop off with particle size, then.

CHAMBERLAIN: With increasing particle size, the diffusion transport drops out rather completely, and you do get this rapid variation.

Deposition Calculations at Oak Ridge

WILLIAM F. HILSMEIER

We have done no actual deposition experiments at Oak Ridge. One might say, however, that mathematical doodling with the generalized Gaussian plume equation has produced a rather interesting result. Culkowski in 1963 published a paper on deposition and washout (ORO 599). The surprising result that we have been working with was the evaluation of the equation for maximum washout:

$$W_{\max} = \frac{Q}{ex\sigma_y\sqrt{2\pi}} \exp\left(-\frac{y^2}{2\sigma_y^2}\right).$$

The washout coefficient, Λ , and the time coefficient, t , which usually are in the washout equa-

tion, do not appear. This makes it possible to compute the envelope of maximum values of x (distance) and y (width) without introducing assumptions of wind speed and scavenging rate. Note that the curve for area versus the maximum washout is linear in Figure 1. Although the distance that a maximum isopleth may reach varies with various weather categories, the area under the envelope of maximum washout is invariant for any weather condition.

To illustrate the use of the graph, assume that one wants to know the permissible value of Q , the emission from a reactor stack, based on an allowable contamination of an area of 2×10^6 square

Figure 1. Washout area vs $W_{\max}/Q$.

meters (≈ 5 acres) to a level of 10^{-6} curies per square meter.

From the washout graph, an area of $2 \times 10^6 \text{ m}^2$ corresponds to $W_{\text{max}}/Q \cong 10^{-7} \text{ m}^{-2}$ (maximum washout per release). Division of the maximum allowable concentration (10^{-6} Ci/m^2) by this value of W_{max}/Q gives 10 curies as the release permitted per rain period. If, from climatological records, the longest probable rainfall period is found to be $4\frac{1}{2}$ days or $4 \times 10^5 \text{ sec}$, the permissible emission rate would be $2.5 \times 10^{-5} \text{ Ci/sec}$.

If washout should be the determining factor for the maximum daily routine release from a stack, this simplifies the release calculation.

DISCUSSION

NEWELL: Would this place an upper limit?

HILSMEIER: Yes. By using this maximum rainout for the given area and setting the contamination, one would obtain the maximum release rate. By using the climatological data for periods of rain, one would find approximately various areas where a 5-day rain would be a good period to use. Considering the time factor, one would get the concentration rate or release rate. It would be the upper limit.

BADGLEY: Exactly what is meant by maximum rainout?

HILSMEIER: Maximum rainout means the whole plume is depleted. In this model it is assumed that the plume is first established and then the rain begins and depletes the entire plume, lowering it to the ground and producing the area shown in the graph.

GIFFORD: This goes back to Dr. Chamberlain's idea that for any given distance from the source there is a rate of rainfall which will maximize the surface deposition, maximize the washout; and then one assumes that this has occurred at all distances for the purpose of the calculation.

CHAMBERLAIN: You mean, a rate of deposition?

GIFFORD: Yes.

FUQUAY: In the application of that graph, you will notice that you have on the ordinate plotted areas which cover all of the Midwest and almost half of the country, perhaps, and you are saying that there is a very light type of rainfall sufficient to obtain these large areas and extended over a long period of time and over a large area; and in this calculation you take only the precipitation scavenging into account. I don't quarrel with the maximization here. The other factor which comes in is the dry deposition that accompanies the rainout. This goes on anyway; so the question is, how much beyond the real maximum have you maximized, when you consider the normal spectrum of fission products which do deposit from other causes?

HILSMEIER: You are concerned about the dry deposition in addition to the rainout. This graph can be applied only to a rainout condition, where one is trying to determine if rainout is the primary factor in the release rate.

FUQUAY: You discussed that this was an upper limit to the area.

HILSMEIER: Only in the case where the rain becomes a factor.

FUQUAY: But in the real sense it may be the upper limit plus quite a factor. You may have greatly overestimated for the actual case because you have not accounted for the dry deposition.

HILSMEIER: Yes.

Measurement and Prediction of Dispersion

DAVID H. SLADE

The field of dispersion measurement and prediction is large. Rather than attempt a general summary, I would like to summarize that portion of the field in which considerable recent experimental progress has been made, that is, diffusion experiments for release times of a few minutes to a few hours and travel times on the order of an hour (distances to perhaps 10 km). One of the practical goals of diffusion research on this time scale has been to find a simple meteorological predictor, based on theory and verified by experiment, which could be used to estimate the diffusion climate at a particular site. The common characteristic of a number of recent, extensive diffusion experiments has been the direct use of wind fluctuation statistics as predictors. This is not new, certainly, but seems to be receiving additional attention at the expense of the vertical gradient type of predictor. What I have tried to do is to summarize the results of these experiments in a manner that will enable one to construct a diffusion climatology using a minimum of simply obtained on-site meteorological data as a predictor. I have really done nothing more than to combine what I felt to be the best features of techniques that have been advanced by Fuquay, Cramer, and their respective groups.

I have used $\sigma_\theta \bar{u}$ (which is approximately equal to σ_v) as a predictor of χ/Q in a travel time framework. This is quite similar to what Fuquay has done with the exception of ascribing the entire prediction to a lateral wind parameter and ignoring any vertical parameters. Cramer also attempted to relate diffusion only to the lateral predictor. I have used a selection of about 145 experiments from the Prairie Grass, Idaho Falls, Green Glow 30, and Ocean Breeze series of diffusion trials. These experimental series were chosen because each included measurements of σ_θ .

In these experiments, σ_θ was computed for averaging times between 2 and 20 sec and sampling times from 10 min to <2 hr. This is not a handicap for the following reasons: The change of σ_θ with

averaging time in the 2 to 20-sec range is not large enough to cause a problem. Further, since χ/Q decreases with sampling time at a rate similar to the increase of σ_θ with sampling time, the different sample lengths could be contained in one scheme. Also the height of measurement of $\sigma_\theta \bar{u}$ is not too critical since, under stable conditions, the wind speed increases rapidly with height while the fluctuations decrease rapidly; and, under unstable conditions, the wind speed increases slowly with a slow decrease of the fluctuations. The various experimenters used various means of estimating vertical spreading such as Richardson number σ_ϕ and temperature gradient. Cramer used no direct measure of vertical spreading but assumed that such information was contained in the horizontal wind fluctuation data.

The "diffusion" diagram shown in Figure 1 is the result. Here, σ_θ is as computed in the experiments and $\bar{u}$ is the anemometer wind speed. The $\sigma_\theta \bar{u}$ term includes direction meander over the length of the sample. Since the use of the lateral wind parameter alone results in more scatter than if a vertical parameter had also been used, the final diffusion diagram was not drawn from best-fit lines but was eye-estimated with a bias toward the more conservative data. Totally, about 90% of the experimental data fall within the proper group or one group lower on the diffusion diagram (61% and 29%). The remaining 10% fall in one group above. In other words, Figure 1 may be used to obtain correct or conservative estimates 90% of the time.

The scatter is, of course, due to the fact that the product of the vertical and horizontal turbulence does not bear a simple linear relation to the lateral turbulence through the entire range of possible values. I chose not to use reported vertical data because of, first, the variety of vertical predictors used in the experiments and the impossibility of evaluating one in terms of the others; and, second, the additional observational complexity inherent in the measurement of vertical stability

data. Had vertical fluctuation data been available for all of these experiments, a better estimation technique could almost certainly have been developed, based on rather simple bivariate measurements. The customers for this type of scheme need the simplest justifiable technique that gives defensible results.

For the evaluation of σ_θ for a climatological diffusion study, I would suggest that an estimate be made from the wind range divided by a suitable constant. This type of relationship has been established by a number of workers and is satisfactory in a climatological sense although certainly not for any individual realization. A number of advantages accrue from the use of direction range. First, simple analogue wind recording systems operating at slow speed can be used. These are widely available, comparatively inexpensive, and quite reliable, and furnish wind rose data as well as diffusion information. Then, the use of an analogue recording allows the recognition of periods during which

the wind direction is variable. If it is noted that variability is a common characteristic at a site, then this technique will not work, nor will any others based on turbulent diffusion, and some different means would have to be developed. The electronic processing of wind direction data is likely to obscure the occurrence of variability.

This, then, is one technique for the construction of a diffusion climatology. Its virtues are simplicity and conservativeness and the direct use of on-site data. Any drawbacks it may have are directly ascribable to its simplicity. I believe that about \$5000 should be sufficient to assemble a one-year diffusion climatology. This includes an Aerovane, manpower, charts, and some machine time.

I hope that I have not implied that there is no further need for diffusion experimentation. There is certainly a need for diffusion information over longer release times and for greater travel distances, in fact, a variety of release and sampling times in one experimental series. Release times from a few hours to a day or two are, with a few exceptions, experimentally unexplored. Almost certainly, the turbulent diffusion characteristics that are assumed, if not obtained, in the shorter experiments are likely to be grossly violated on these larger scales. Further, there is certainly a need, even at the shorter times, for vertical distribution data. Vertical wind fluctuation data are useful here. Another field that has not received too much experimental study is the instantaneous source configuration. Simple techniques for evaluating these problems will probably await the successful completion of future experiments.

DISCUSSION

SMITH: I am curious about your reasoning in drawing your curve in such a way that your results are conservative. This is a tendency in the whole atomic energy field, and I don't like it. I think it might be more useful simply to have the best-fit curve, with some idea of the variability to be expected with a given case.

SLADE: I agree that frequently we have urged people to construct for hazard reports estimates that were unrealistically conservative, e.g., constant wind direction for on the order of 3 days. However, this conservativeness is inherent in the data because of the scatter and because of the lack of vertical predictor. There were points that indicated higher concentration than the bulk of the other points, and I think this is not an inordinate amount of conservativeness to include in a prediction diagram.

SMITH: I vote for best fit.

ISLITZER: On Figure 1, was that all measured data?

Figure 1.

SLADE: All measured data at 1000 sec, except yours, which went only to 700.

ISLITZER: At only 100 sec travel time you show at least 5 orders of magnitude difference in concentration with different values of $\sigma_\theta \bar{u}$. This seems like a large range at a short distance and travel time. Does any one site show this wide a range?

SLADE: No.

ISLITZER: What is the range between sites?

SLADE: I think the difference comes in the plotting of χ/Q , which tends to elongate the concentration axis rather than $\chi u/Q$, which compresses it. Your Idaho points fall toward the upper left corner of this graph and are largely associated with high values of the wind speed, but your experiments were conducted under stronger conditions of wind than, say, Ocean Breeze.

ISLITZER: There were sites that had that concentration in that short distance with that travel time.

SLADE: At Hanford the experimenters took in 70 or 80% of the total range, but they did not have large values because the wind speed at those times was comparatively low, about half of those occurring during your experiment.

ISLITZER: When you make the complete jump from stability, can you sketch in stability classes, say, three or four, on that same plot?

SLADE: Yes. I don't think there could be any doubt about that. The only problem with stability is that you can't just pick one layer for everybody to measure stability in as a predictor; you can't nail down σ_θ quite that well.

CRAMER: I think we have a pretty good idea on a σ_θ climatology, related to stability and slight roughness, and these numbers perhaps aren't universally known, but there is a good basis for them. I think you've forgotten to measure the thermal stratification, simply because it may put a constraint on the diffusion in terms of an inversion layer, which obviously has to be taken into account. I think further that we are getting a notion now where we can relate the diffusion, the spreading of the cloud in the lateral and vertical direction, to the spectrum, which is also tied to the σ_α and in the vertical direction to σ_θ . We are dealing with ground-level sources, and I think it is very difficult to use an actual bivariate measurement of σ_α .

SLADE: For an actual measurement, yes.

CRAMER: We are dealing here with a vertical measurement, where we have a gradient in the --

SLADE: Under unstable conditions, σ_θ may not vary too much over these heights, but the plume showed that the bivariate vertical trace was not a good predictor.

CRAMER: I think you have to consider two cases here. In the case of elevated sources, it is advantageous to have bivariate measurements for several things, but it raises some problems. I think we do pretty well along the line that you have indicated, depending on an implicit relationship between the σ_α and the vertical spread, however complicated it might be. I have some remarks about the way you plotted your data. I agree that the $\bar{u}\sigma_\alpha$ plot is interesting. It seems to consolidate the data. I think there is no great mystery about why it works, and that *per se* there is nothing against using distance as a scale, either. We are here using Taylor's hypothesis to relate u to distance, and so we can't say there is any particular magic in time or distance. Yet it is a fact that over a given range of stability the product of $\bar{u}$ times σ_α is nearly constant. This is the instability case, and it just so happens that in stable situations both $\bar{u}$ and σ_α change in the same way as the stability increases: they both become smaller. Thus, this product tends to emphasize the effect of stability on the patterns.

SLADE: Not really, at low heights at least. As the stability increases, it can be shown that there is wide dispersion of pollutant.

CRAMER: This is really meandering. As long as you study a region where the spectrum is continuous, as in instability, you will find no difference, in regression analysis or any test you want to make, between the prediction value of a distance plot against $\bar{u}\sigma_\alpha$ and a plot in time. They are practically identical. At night the distance plot starts to fail, because the spectrum is not continuous, and you get into a region where there is a gap and meandering, and you should not then follow along parallel logs or curves that are established for instability. I think each type of plot has its advantage, particularly if you are trying to relate the plot to the initial dimensions of the source and to a climatological classification of σ_α , and there is a lot to recommend the use of σ_α .

SLADE: Concerning your last point, it is interesting that the data fit in quite well under unstable conditions, although the major portion of the plot is during stable conditions.

Diffusion Studies in Canada

R.E. MUNN

I am going to review briefly our program in Canada and then ask some questions of a more general nature, which may not be entirely germane to this morning's discussion. We have four nuclear establishments in Canada and a fifth is contemplated. The A.E.C.L. experimental station at Chalk River is well known. It is being subdivided and personnel are now moving to Whiteshell, Manitoba. In addition there are two power reactors, one at Rolphton, Ontario, in the Ottawa valley, and another at Douglas Point on Lake Huron. The Meteorology Service of Canada corresponds to the U.S. Weather Bureau. We provide a meteorological consulting service for Atomic Energy of Canada Limited as well as for Federal and Provincial Health Departments. I find myself involved in community air pollution problems and single-stack problems as well as the A.E.C.L. requirements. In this broad general area we feel that there is a very important place for tracer studies, and we want to participate but on a limited budget. We simply do not have the kind of money that seems to be available in the United States.

Our concept, therefore, is to develop some kind of task or strike force, a mobile unit that can undertake diffusion trials anywhere, set up instruments in a day and commence measurements the following day. The meteorological part of this system is a portable 80-ft tower, with an associated trailer. There are profiles of wind and temperature at three levels. There is also a bivane on the top of the tower with a University of Michigan type of bivane integrator, which gives us averaging times from 6 sec to 1 hr. These integrators permit development of a diffusion climatology. For the actual diffusion trials themselves, we record the data on fast-speed charts and analyze by hand, using computers at a later stage. This is because one really doesn't know in advance which smoothing time to use. One may think one does, but a little while later wish the original records were available.

In addition, the Department of National Health and Welfare has developed a capability for ura-

nine tracer techniques, and our philosophy is to have portable samplers. This gives us much more mobility and reduces costs. We have done three sets of trials under varying conditions using 6 portable samplers. We are enlarging the sampling network to 12 or 18 this summer. Using a walkie-talkie or some other kind of radio communication, the meteorologist at the point of release decides where the samplers should be put. The ground crew is 3 to 5 miles away, ready to locate and activate the samplers. The forecaster has to predict where the centerline will be in the hope of intersecting it, and this is a very useful exercise in meso-scale circulations. It gives a real feeling for local flows when one must predict the wind direction for the next hour. At Douglas Point two years ago, there were some 39 trials, for which 19 successfully intercepted the centerline. Our predictions improved as the season went on because we got a better feeling for the local meso-scale wind flows connected with the lake breeze.

We have also done diffusion trials in the rear of a bluff building. We think this is an important area of research, and we intend to study other downwash situations.

We also had another kind of problem, to determine the source strength of a large manufacturing plant. This particular source emitted a reddish plume of iron oxide, which was visible, and by using the same battery-operated portable samplers, we were able to intersect the centerline of this plume. From measurements of wind and turbulence we were then able to deduce the source strength, which in this case averaged about 19 tons of iron a day for the particle size range of the filters.

Another set of diffusion trials is starting on the shore of Lake Huron next Monday and will be continuing until the end of July.

I have some questions of a more general nature. I was surprised that the first question didn't arise yesterday in the discussion of meso-scale transport. This is what might be called organized turbulence in the sense of standing waves, but on the micro-

or meso-scales rather than of the dimensions of mountain waves. There are very minor features of the ground or different types of radiating surfaces that maintain semipermanent conditions for a few hours, and convection tends to form over certain areas; also, the surface roughness patterns tend to create certain types of standing waves. Dr. Clodman, our clear-air turbulence expert, has been studying clear-air turbulence occurrences at 30,000 to 40,000 ft, relating the frequency of occurrences to ground topography. He finds that for relatively smooth terrain, with maybe 500 to 1000-ft differences in elevation, he can have a difference in clear-air turbulence occurrences of an order of magnitude. If this kind of thing is important at 30,000 ft, I can imagine what small terrain features could do to the trajectory of a plume at, say, 1000 ft.

Also, in a recent Soviet paper on shelter belts, it was mentioned that in a field surrounded by trees, the trees couldn't be sprayed successfully with insecticides during the daytime. There was a continuous downdraft over the trees with an updraft over the field. However, the trees could be sprayed at night because the circulation was then reversed.

This raises the question: Is $\bar{w}$ equal to zero? I don't think it often is. I think that sometimes on the micro-scale, frequently on the meso-scale, there are areas where $\bar{w}$ isn't zero, and this kind of organized turbulence needs to be considered. I haven't any suggestions of how to go about it; I am just suggesting it for discussion.

Another subject that hasn't been discussed, possibly because Professor Panofsky didn't arrive until today, is the internal boundary layer. When there is a sudden discontinuity in surface conditions (a change in roughness, moisture conditions, or something else), an internal boundary layer develops. As an illustrative example, suppose that a tall chimney is situated on a lake shore and the wind is blowing from lake to land. What happens

to the plume geometry after the plume intersects the upper surface of the internal boundary some distance inland?

Another example is a city, in which a sudden increase in surface roughness must cause an internal boundary layer to develop. Suppose a chimney is located on the upwind side of a city. What happens to the smoke plume when it intersects the upper surface of the internal boundary layer? If there is an inversion over the country at night and a weak lapse condition in the city heat island, does a fumigation take place? It has been shown that these boundary layers do exist. Dr. Peter Summer in his doctoral thesis at McGill University mentions that the smog layer over Montreal is tilted towards the upwind side.

DISCUSSION

SMITH: With regard to your last question, we have had two interesting examples here of precisely this case, in which the plume from our reactor has traveled down Long Island some 55 miles or so during a deep temperature inversion and approached New York City. The interesting thing is that the radiation from the plume was detected on two extremely sensitive devices built by the NYU Physics Department: we have observed these same types of traces here in the general vicinity of the Laboratory. So we have a very good idea, for example, what a fumigation case looks like on the radiation traces, and our tentative answer to your question would be that the fumigation you expect actually does occur when you approach the city and the inversion disappears. In other words, you come into a turbulent regime over the heat island.

MUNN: There are a number of references, particularly in the British literature, to the fact that over a city a tall stack is associated in the public mind with urban pollution, whereas poor air quality is really caused by short chimneys and ground-based sources. I have discussed this with Dr. Summer. We think it quite conceivable that the tall stack, if it is on the upwind side of the city, may be above the internal boundary layer, but the plume may intersect the boundary layer farther downstream causing a fumigation over the center of the city. At the moment this is speculation, but the subject must be treated with great caution, I think.

Dispersion in Urban Areas

DONALD B. TURNER

My remarks will be restricted to the measurement and prediction of dispersion as it affects urban areas. You have already heard that measurements are being done in St. Louis and in Fort Wayne sponsored by Dugway. I hope Dr. Munn's strike force will soon be operating in the urban areas in Canada to provide more concrete data. It is important to make measurements in urban areas and not rely on the dispersion measurements already made over level terrain out to distances of, in some cases, on the order of 30 km.

The difference in roughness over urban areas compared with Hanford with its 1½-m sagebrush has repercussions in a number of ways. It changes the wind profile considerably. It also changes the stability, distributing the flow over this roughness and consequently changing the diffusion and dispersion conditions over urban areas. Also important is the fact that over urban areas there is frequently diffusion in transitional states, as Dr. Munn has shown, where there is an inversion moving over the city which is heated from below, and the roughness also causes weak lapse conditions below, so that there is not the same stability in the lower layers as aloft.

Most studies, even over very level locations, have been concerned with sampling times on the order of 15 min to 1 hr, and I think the importance of meander with increasing time period must be investigated. The particular scale used is of considerable importance to what sort of dispersion models are applied to the particular situation. Dr. Cramer mentioned the application of dilution and mixing height concepts to urban areas, which work extremely well, particularly in calculating concentration at some distance downwind from the city rather than within the city itself, and also work in cases of stagnation, where the pollutants are restricted in movement and continue to build up. But naturally one needs information about the residence times of the pollutants, and there is very little.

We are working on the problem of diffusion for travel distances on the order of tens of kilometers, which are somewhat greater than even the over-level-terrain distances that have been considered. Some of the problems we will soon face in trying to estimate concentrations for the longer travel times and distances are the following. We will be increasingly concerned about trajectories rather than mean flows; some of the observational techniques for determining trajectories have been discussed at this meeting. Also, the budget of the pollutants in the atmosphere will be of considerable importance: over these longer travel times of 10 or 12 hr we will not have a simple situation of a continuous emission of a quantity Q , and therefore we will have to consider in estimating concentrations not only the emission that took place one hour's travel time upwind from this location, but also that which occurred 4, 6, 10, and 12 hr before. Rather than having sources and receptors, we will have the problem of keeping track of the pollutants in the atmosphere as they are transported, complicated by removal processes that change the characteristics of the pollutants, including in addition to deposition, the removal of gases by photochemical and catalytic reactions and even the formation of other pollutants by these reactions. This is an area of atmospheric chemistry that we meteorologists will be forced to consider in greater detail.

In this session not much has yet been said about the prediction of dispersion. I think that in the not too distant future real time estimates will be made of pollution concentrations based on certain models and on the source inventories available to put into these, which will be used for control and for special studies; and this will require the forecasting of meso-meteorological parameters. Dr. Munn might agree that the most important meso-meteorological parameter to forecast is wind direction. Much more information will be needed about diffusion, but I think this will be more con-

servative than the variations caused by differences in wind direction, which will be extremely difficult to forecast accurately, particularly over urban areas. I think we will be involved in the next decade in forecasting meso-meteorological parameters.

DISCUSSION

PACK: I have been eagerly awaiting a statement such as Mr. Turner's about the prediction of trajectories. I have heard no comment at this session on the probable use of numerical techniques on the meso-scale. It would appear that what little we know, from hand calculations with the prediction techniques for trajectories, is true of instantaneous and even single-point sources. We are in a state of abysmal ignorance in trying to predict the target of a diffusing plume 100 miles downwind. A difference in plume concentration of an order of magnitude or two makes very little difference if the question is whether an area is in the path of the plume or not, and I feel very strongly that one of our big lacks is in the ability to answer this. Fortunately, Mr. Allen has mentioned some initial attempts along these lines. It appears that statistical techniques recommend themselves for initial utilization, but modeling techniques on the meso-scale for trajectories and use of high-speed computers certainly will be a fruitful field of research over the next 5 or 10 years.

CRAMER: Maybe we will never be very successful in modeling the meso-scale, but I think an operational scheme involving tetroons might easily be used. In connection with very large transport problems between ur-

ban areas it might be possible to use an orbiting satellite to survey more specifically. It is difficult to get an over-all knowledge of patterns on this scale without using some kind of gross technique.

ENGELMANN: I agree with Dr. Pack's remarks. I would like to emphasize the need for knowledge of mixing depths in the meso transport. At 10 miles from the source, and in daylight hours or nighttime hours, what does one use for the wind level and for the trajectory level? This is perhaps no problem in the Great Lakes, where there is not too much difference in shear and in speeds, but it can be very critical in rough terrain or if there are wind shifts downstream or in time. One may have an accurate trajectory in terms of direction, but the trajectory also requires a knowledge of wind speeds so that one can determine what portion of the plume is under consideration.

BIERLY: I am not quite as pessimistic as Dr. Cramer about tetroons. Mr. Turner mentioned physical meteorology coming into chemistry, but when chemistry comes into meteorology, we will all benefit. I think Mr. List and Mr. Holland are proponents of tracers, natural or otherwise, and I think techniques will be developed within the next 5 years or less that we can all use to get observations at meso-scales.

BADGLEY: Mr. Pack's suggestion of using numerical techniques on the meso-scale is basically a good idea, but people tend to think that, because it is on a smaller scale, it will be easier than using numerical techniques on a global scale or in a forecasting method. I would say it is a good deal harder, perhaps by a factor of 10.

DAVIDSON: I would guess two orders of magnitude harder.

Diffusion Studies at Hanford

CHARLES L. SIMPSON

Figure 1 (which I have shown before) is a summary of the exposure data from 46 experiments at Hanford. It is a plot against time of the peak exposure multiplied by the wind speed at the release height and divided by the source strength. The data were grouped according to the Richardson number and the lateral growth parameter $\sigma_\theta \bar{u}$ (which Mr. Slade discussed). The solid lines represent the average normalized exposure for each group and the dotted lines, the outliers observed in each group. We obtained a fair degree of separation between the groups, particularly in the stable case. We have never had much difficulty in unstable and neutral atmospheres, but in predict-

ing diffusion in stable atmospheres there is considerably more variance. What the technique has done is to reduce considerably the variance about the mean in stable plumes. For example, the two groups at the upper right both have a Richardson number >0.08 , but they differ in the lateral growth, the upper group of very small plumes having $\sigma_\theta \bar{u} < 0.15$ and the second group having $\sigma_\theta \bar{u}$ between 0.15 and 0.5. An interesting feature of this diagram is that the slightly stable conditions represented by Richardson numbers from 0 to 0.08 are very close to unstable, so that the slightly stable plumes are close to the unstable plumes which have a narrow growth; the group to the extreme left represents the unstable plumes which have the wide growth.

This scheme is the best we have been able to devise for making a prediction with some kind of confidence limits, and we feel reasonably sure that the accuracy is within a factor of 2. We were discussing yesterday that we are generally pretty good at prediction. We know a great deal about diffusion; we have run a lot of tests and have a lot of data. Therefore I agree in the case of ground sources; with elevated sources some complexities may enter, and there is not yet enough experimental evidence.

Figures 2 to 4 are summaries of ground source data. At the time they were drawn we did not have complete data to calculate $\sigma_\theta \bar{u}$ and therefore used estimates based on the data we did have. The exact values will not change the graphs much. In Figure 2 the Hanford experiments are compared with Prairie Grass experiments. The curves to the right represent the very stable case. The Prairie Grass curve was plotted by averaging 17 of their most stable experiments as represented by smallest values of $\sigma_\theta \bar{u}$ and Richardson numbers >0.08 . In general it agrees very well with the curve for the Hanford most stable experiments. It is interesting that there is curvature in both of these, which is not particularly evident in the unstable curves. This curvature for stable cases appears in most of these graphs. Separating the curves in terms of

Figure 1. Normalized peak exposure, $E_p \bar{u} / Q_i$, versus time.

stability gives a wide range of values, but use of $\sigma_y \bar{u}$ results in the formation of groups, so that, rather than a factor of 5 to 7, I think we can consider the precision to be within a factor of ± 2 . The Prairie Grass study of 14 experiments runs pretty close to the Hanford model in the slightly stable case, and, finally, for the unstable cases the agreement again is as good as one can hope for.

In Figures 3 and 4, the Hanford curves are the same as those in Figure 2. These figures show the excellent agreement between the Hanford data and those from NRTS and Dugway, and from Cape Kennedy and Vandenberg.

Figure 2. Variation of normalized exposure with time for Prairie Grass and Hanford.

I think these figures verify yesterday's comment that the agreement between experimental runs has been good, and therefore we should be able to make predictions within a factor of 2 or so. Any further improvement will be very difficult and will require diligent searching for parameters and refinements in counting techniques and experimental design.

DISCUSSION

DAVIDSON: Are these data all from ground sources?

SIMPSON: Yes, released at about 1½ m height. One of the encouraging things I didn't mention was the variety of tracers used: SO₂, 2210 zinc sulfide, fission products, iodine. We get good agreement with these various kinds of sources. I am not implying that we have it made. In the Cape Kennedy and Vandenberg data there were some very unusual runs. In some runs at Vandenberg the exposure increased with distance because of channeling; in another very interesting run the plume was actually entrained into a heavy cumulus cloud: at ¾ mile there were evident high concentrations but at 1½ miles there was no evidence of a plume at all, as a cumulus passed over and entrained all of it. Thus, there are circumstances that cause complexities, particularly rugged regions and regions where there are strange circulations.

DAVIDSON: The data you presented earlier showed quite significant loss of whatever tracer you used at Hanford. Do you think the loss process for SO₂ is the same? It is a little hard to believe.

SIMPSON: There are two things that we are not specifying, deposition and σ_z , both being mechanisms that go on in the vertical. I do not know whether SO₂ is being deposited or not.

SINGER: We have summarized about 10 years of our data and attempted to use this scheme to separate the data, and we got absolutely nowhere. The old-fashioned system of $\chi \bar{u} / Q$ vs distance separated the data nicely into "gustiness" classifications. One effect not considered in the equation is the variation of $\bar{u}$ with height.

SIMPSON: I believe this, but Mr. Glade and I have plotted all these data versus distance, versus $\chi \bar{u}$, and the results I showed are the best that we have achieved. This is not necessarily to say that these are the most significant, but the method does work well.

DICKSON: The results in Figure 1 were from tests run over the same type of terrain - sagebrush - and now you say you could not associate them with the tests that were run over sagebrush. Is there any relationship with the difference in terrain?

SIMPSON: There are some tests in the Ocean Breeze (Vandenberg) series that do not agree, although the averages are pretty much in agreement with the other results. The tests were not run over sagebrush. Prairie Grass was run over a flat area of mowed grass. There are effects due to terrain features, but so far, from the data produced, these are exceptions rather than the general rule.

Figure 3. Variation of normalized exposure with time for NRTS and Dugway, Utah.

Figure 4. Variation of normalized exposure with time for Cape Kennedy and Vandenberg Air Force Base.

Turbulence and Low-Level Structure

BEN DAVIDSON

The current state of knowledge concerning low-level wind structure has been best summarized in the book *The Structure of Atmospheric Turbulence* by J.L. Lumley and H.A. Panofsky (Interscience, 1964).

The information available about low-level wind structure is the result of careful preselection of data. We inspect our runs and decide which particular stretch of data looks steady enough for us to analyze. Then we compute the turbulence statistics and the spectrum. I think perhaps less than half of the hours in the course of a year are suitable for this kind of analysis. Thus, we know about the structure of turbulence at certain places in the United States for maybe half the time. The point is, if we put out sources randomly and run them for 20-min intervals, hour after hour, how often will we get bimodal and trimodal distributions of concentrations in the atmosphere? Again, I would guess about half the time.

At Brookhaven, for example, all of them are bimodal despite some careful preselection in determining when an experiment would be run. So perhaps it is the normal state of the atmosphere to give not normal Gaussian distributions but the sort of distribution one might expect to get by adding up a set of Gaussian directions under conditions that vary abruptly during the course of the experiment. We know almost nothing about that kind of statistics, and I think it is time we devoted some attention to studying this aspect of the turbulence structure problem.

Also, in many of the practical problems of the AEC, such as that of the maximum credible accident, I think the time scale does not really involve turbulence. It involves the probable sequence of wind directions for a period of, say, a week. There have been minor studies, but there has been no mass applied climatological approach to this kind of problem, which is certainly solvable with conventional weather records, which are available in enormous quantities.

Professor Panofsky thinks the differential equations of turbulent diffusion are not applicable but that I advocate their use; he is half right. For

small t , the differential equations will never yield a result correct according to Taylor's theory for the homogeneous turbulence case. For large t , there is no inconsistency between the two predictions. On the other hand, there is no reason to expect the Taylor assumptions to be valid for diffusion near the ground, since the flow there is inhomogeneous. Particularly in the vertical direction, there is a marked change of scale with height. I do not believe there are available exact solutions of the Taylor approach which incorporate the effect of varying scale of turbulence. This problem should be worked on because there is a contradiction here. We assume that the Taylor expression is valid for the atmosphere and if its prediction differs from that of the differential equation then the differential equation must be wrong, but on the other hand it is not valid to apply Taylor's simple diffusion theory near the ground.

There is another point about the diffusion equation. Since the vertical and horizontal atmospheric scales are greatly different, the appropriate diffusion equation for this necessarily anisotropic system in two dimensions is

$$\frac{\partial q}{\partial t} + \mathbf{V} \cdot \nabla q = \frac{\partial}{\partial x} \left(K_{xx} \frac{\partial q}{\partial x} \right) + \frac{\partial}{\partial y} \left(K_{yy} \frac{\partial q}{\partial y} \right) + \frac{\partial}{\partial x} \left(K_{xy} \frac{\partial q}{\partial y} \right) + \frac{\partial}{\partial y} \left(K_{yx} \frac{\partial q}{\partial x} \right).$$

We always assume that in the atmosphere near the ground the principal axis of diffusion is in the direction of the mean wind and is horizontal. I think this assumption cannot be correct for the case with strong momentum flux, so that if one studied this problem and decided that the principal axis ought to be one along which the diffusion tensor is diagonalized then the principal axis should be one in which the shearing stress would disappear. I suggest study of the results of this type of equation with the assumption that the principal axis of the diffusion tensor is not along a horizontal line but along a sloping line.

I am particularly interested in this because I have tried the same technique for stratospheric

diffusion. By assuming that the principal axes are always along isentropic surfaces and then resolving the values of the cross-diffusion terms, I have obtained patterns of concentration in the stratosphere that are more reasonable than those obtained with any other model without recourse to general circulation terms.

DISCUSSION

PANOFSKY: I don't really understand the K theory. For the diffusion problem it may not be possible to say that K depends just on the position. If you have a fluid and you know all about its turbulence, fluctuation statistics, correlation, etc., you can't say that K_x or K_z has a certain value if you have an area source with no variation in the transfers and you are interested in momentum flux or heat flux or evaporation. In an expanding plume you may have to consider the width of the plume at the point as well, and I assume that although you maintain that everything is nicely homogeneous, you find different values of K here and there. This is the kind of argument that Richardson makes and the sort of thing one tends to get in a wind tunnel, and this is why I think other fluid mechanics people are wary about using this theory at all. The K value is not a function of the variables.

DAVIDSON: The test of any pudding is in the eating, and if in two-dimension theory the theory, at least under adiabatic conditions, gives without recourse to fudge factors very decent coincidence with observation, then I don't see the objection to its employment.

PANOFSKY: It may work for the wrong reason. It works very well for a continuous line source at right angles to the wind on the ground, and it may work because you assume that K varies with height. As you go away from the source, downstream, you get wider plumes and variation with height, and you get a larger K . According to the theory, the reason for the larger K is the larger height, but the actual physical basis may be the larger plume. With an elevated source, you could probably tell the difference, because then, according to the theory, K would not increase in the x direction; but if you let K depend on the width of the plume, then K would increase in the x direction.

DAVIDSON: There are some elevated source data, for instance Hage's study of the deposition of microspheres, and I think the data are in accordance with theory for the adiabatic case. The principal objections to the K theory are perhaps cogent, but they are not any more serious than the principal objections to the applications of the homogeneous theory or of Gaussian models in the atmosphere, where clearly the scales are changing with distance from the ground. But I would not before the fact throw

out the K theory. If it fails to yield agreement with observations, then it is clearly not valid, but it is at the same time a theory which is perfectly specific, has no fudge factors in it.

ARNOLD: What Dr. Davidson wrote in the equation is essentially what Prof. Corrsin* expressed a few years ago for momentum transport. The implications of this when applied to K theory show that the K theory can be true only if the theory is severely restricted. It has been stated that the K theory works. A better statement would be that the working of the K theory depends on how fundamental is the question asked of it. Here, Dr. Davidson considers the principal axis of the stress and of the strain rate tensors, and in this consideration K theory does not work in general.

DAVIDSON: I don't feel rich enough to reject a theory on the ground that it may explain something exactly for the wrong reason – at the present time. Maybe 10 years from now we will be in a much better position to be choosy.

BADGLEY: One can do what you suggest, surely, and even diagonalize in three dimensions rather than two, but it is not clear to me why this would be of a great deal of help, because you would be diagonalizing the stress matrix (if that's what you did) where you have a distributed source (if you will, an infinite source, the ground) and if you try to apply this to a diffusing plume, you have an entirely different set of boundary conditions, and it is not obvious that what would diagonalize it for momentum would diagonalize it for a diffusion problem.

DAVIDSON: True, but why not examine the consequences of such assumptions? Some numerical experiments we have been making indicate that such an approach might be helpful to explain the Prairie Grass unstable runs, which cannot be explained by the Taylor theory in any case. But it is too early to tell.

BADGLEY: That may be. In some cases where I have tried to diagonalize the stress tensor (just for the fun of it, not for that purpose), the angles were not small – they might be on the order of 45° . Another question is whether or not, after having done this, one can depend on having the same orientation for the axis everywhere in space, or where you are going to have to coordinate the system as you go along, as you do it in two dimensions.

DAVIDSON: When you get above the constant stress change, you are in trouble, in any case.

VAUGHN: This brings up a point I mentioned yesterday in favor of making direct measurements of turbulent fluxes. The whole idea behind the K theory is the gradient transport of matter or whatever other property of the atmosphere you are measuring, and if you can measure this flux directly and, in addition, measure the gradient, you can at least see whether the idea of the K theory applies here.

*Corrsin, S. in *Naval Hydrodynamics*, pp. 373-99, Publ. 515, Natl. Acad. Sci. Natl. Res. Council, 1957.

Wind Profile

ABRAHAM ARNOLD

My remarks will be directed toward the question: Where are we today?

There comes a time when workers in any particular field, such as turbulence and low level structure, should face up to a dispassionate assessment of the state of true knowledge in the particular area. The different speakers at this session have their individual ways of arriving at a statement of where we are today. I would like to start with a brief review of background. However, in the time allotted it is impossible to do more than choose a portion of the session topic, and I have picked "wind profile," more particularly the turbulent case. But what I want to say may, in a broad sense, be applied to other aspects of the low level structure and to turbulence.

In treatments of wind profile the objective is to obtain some functional relation between wind speed and height. In the early days (1915-1935) workers in fluid mechanics and in meteorology ran a somewhat parallel course. In both cases a velocity which varied with the height raised to some power was measured - the power profile. Also, there were data which showed a variation with the logarithm of height. There seemed to have been an awareness between experimenters in the laboratory and their counterparts in the atmosphere, and a fair amount of exchange. Thus, it was Prandtl, writing in a meteorological journal, who suggested the logarithmic profile for the adiabatic atmosphere. In the later years there seems to be less of this, and I believe we have been the losers.

There are explanations for this pursuit of separate paths. In the fluid mechanics laboratories the more typical experiments are for neutral (i.e., adiabatic) conditions. In the atmosphere, the non-neutral case is more typical. Further, in fluid mechanics, the velocity profile was moving into a secondary role, while in our case the profile is still of primary importance. The non-neutral case, where there is interaction between the thermal field and the dynamics of the problem, has been handled by each discipline in a separate way. In fluid mechanics, there has been some theory

for the simplest of situations, and this has yielded some interesting results. The mathematics is very difficult and involved, and to my knowledge no one has published any resulting wind profiles. In meteorology, approaches to this problem retain one or the other of the two profiles (log or power) and modify them. (Thus there is a log plus linear profile, and a Deacon profile, each of which reduces to the log profile in the neutral case.) Theory, in meteorology, features a fair amount of dimensional reasoning using parameters identified by the particular investigator as being pertinent to the phenomena involved. Note that "theory" in this case does not in general get down to fundamental equations. But then, fundamental equations such as the Reynolds averaging of the Navier-Stokes equation form an intermediate set with more unknowns than equations, so that, pending a formulation of some new physics in the problem which will fill this gap, there can be no general solution. The theory which is based on dimensional reasoning tends to be asymptotic. Some sort of final, idealized state is being described. But is the atmosphere in this state?

To get back to the neutral case, in the laboratory, one finds in the simple turbulent flow situation that at the lower Reynolds numbers there is a power profile with an exponent of $\frac{1}{2}$, and with increasing Reynolds numbers the exponent progresses to $\frac{1}{4}$, $\frac{1}{3}$, $\frac{1}{2}$, $\frac{1}{10}$. The logarithmic profile is the asymptotic limit for high Reynolds numbers. It is not easy to distinguish on a semilog plot between a logarithmic profile and a power profile with an exponent of, say, $\frac{1}{10}$. Figure 1 shows this. The points were computed from a $\frac{1}{10}$ power profile, while the straight line is the logarithmic profile.

For the individual who is interested in an engineering answer, either a log or power profile may be suitable. We, at Brookhaven, are looking into this question. That is, given a minimum of information, what are the relative merits of the several profiles for estimating the wind speed at some altitude, and what are the merits of these profiles when prediction in time at some altitude is re-

Figure 1.

quired, and what may that minimum of information be? For an engineering use, then, it is expected that with a reasonable amount of effort answers can be obtained to questions of this type. A presentation is planned for the A.M.S. meeting at Salt Lake City in October 1964 on the merits of several profile equations in the prediction of winds.

The situation as far as theory goes is different. While there is theory that has yielded a log profile, no theory, dimensional reasoning, or what have you, has produced a power profile. We note that there exist situations in which a power profile is a very good description. If there ever evolves theory which includes the power profile, such theory will have to be for a state that is not yet at the asymptotic limit.

The question of "where we are today" may be considered in terms of what we really know. One way of answering this is to make a series of statements and check them. Is this or that true or not? – regardless of what theory or the consensus of opinion may say. I would now like to make such a statement which I believe most people would agree to. It is: "In an adiabatic atmosphere the wind profile is logarithmic." To test this I selected

Figure 2.

data from a few different studies. Of these I picked a handful that were nearest to adiabatic. Mind you, the temperature field is not very cooperative, and I never found that nice linear adiabatic lapse rate. Nevertheless, I tried my best, going to hundredths of a degree to select those few nearest to adiabatic. This stringent requirement left only a handful of profiles. There were too few profiles to permit a generalization. In most of the cases, the plot of the profile on semilog paper showed a small but nevertheless ever-present convex bow instead of the anticipated straight line. Figure 2 is an example. A remaining small minority showed a random scatter. Thus, on this small sampling of data, I must conclude that the statement that "in an adiabatic atmosphere the wind profile is logarithmic" may have to be qualified. To get back to the opening question: "Where are we today?" – I'd say, we are where our measurements and our ability to organize these measurements have brought us.

DISCUSSION

DAVIDSON: Will you state the hypothesis: that under adiabatic conditions and with the present state of fluid anemometry the wind profile may be logarithmic? I wonder

what would happen if you used the Johns Hopkins anemometers rather than those at MIT.

ARNOLD: There were no Johns Hopkins data in the cases examined. I asked people if there were adiabatic, log profile cases and they said there must be many. I haven't been able to find them. But then I haven't made an exhaustive search, either.

PANOFSKY: The other thing you should probably put in your hypothesis is how high the profile is logarithmic under neutral conditions. I point this out because, at the 100-m Brookhaven tower, they have a model for a neutral profile such that between the top levels, certainly around 300 ft (and I think even from 150 to 300 ft), some rather significant deviations are expected theoretically from the logarithmic profile.

ARNOLD: I purposely didn't bring in the Brookhaven data, knowing your comments in the past. In Figure 2 O'Neill data were used.

PANOFSKY: But I think you should say how high you expect the logarithmic profile.

ARNOLD: There is a need for an answer to that question. Laboratory observations may help. There is a point I would like to make. Every once in a while, the atmosphere is not in the asymptotic state we assume. Some people, for instance Scrase and Frost in England, have come up with a $1/2$ power profile in the neutral case; there is nothing magic about this number – there may be other exponents. At Brookhaven we have an even larger exponent for neutral cases. My general impression (without studying too many cases) is that if we are to find a log profile, it is for the higher wind, which again suggests a Reynolds number dependence – a Reynolds number which we haven't spelled out as yet.

CHAMBERLAIN: I think most people select, consciously or subconsciously, the smoothest part of the site to set up a tower. This tends to be troublesome because some distance away there is a rougher terrain, and therefore the problem is different.

ARNOLD: I believe there were some 500 yards of smooth ground around the O'Neill observations.

FRENZEN: That's the point: the height to which the logarithmic profile holds depends upon the uniformity of the site. Therefore, the O'Neill curve may really be a criticism of the site.

PANOFSKY: I don't think you can make that conclusion.

BARAD: You may be right, but you may not have the sole reason, or not the reason at all. Have you examined the 8 or 9 cases you mentioned to determine – in whatever subjective or objective way – what effect an error of a certain size in any one anemometer or collection of them would have on the profile with the slight bulge? We have in the past tested what accuracy is required to stay away from the $1/2$ power law and the log law. Probably no one can state at this date that any set of instrumentation in 1953 or 1956 had an accuracy which would have enabled one to determine the difference, in the range from <1 cm/sec to 5 m/sec.

ARNOLD: The error you are talking about would have to be a bias type of error. In general, there wasn't just a random plus or minus. The curves generally had a convex bulge.

BARAD: I am talking about all the inherent difficulties with cup anemometers and matching.

BADGLEY: I agree with all the statements made so far. Very often the time when you find an adiabatic lapse rate is actually a change-over period between a perfectly stable case and an unstable one, or vice versa, and it is quite possible that in this period the entire wind profile hasn't yet come to equilibrium with any one condition, either the old or the new.

MOSES: I would like to point out that, even though your site may be smooth physically, if you have cumulus clouds overhead you may have hot spots and cold spots, and there you will have a thermal roughness which can cause a deviation in your log profile.

ARNOLD: Our concept of using thermal structure alone to determine what the curvature will be on a semilog plot may not be enough; there may be other factors.

MOSES: My point is that a site may not be as smooth as it looks even though it is physically smooth.

ARNOLD: In my search for straight-line plots I found in a book a nice straight line on a semilog plot. The authors had taken two adjacent periods in the O'Neill data, one highly stable and another highly unstable, and by averaging them together had gotten a straight line. Are we going to extend our definition of adiabatic that far?

BADGLEY: No.

Temperature Profiles

ARTHUR B. BERNSTEIN

Rather than discuss temperature profiles in themselves, I want to talk about one problem that arises in studying turbulent structure and to use the temperature profile as an example.

The two previous speakers mentioned problems in choosing the period and locations to study. In actual practice, the periods we study not only represent but a small portion of the total time and space in which we are interested, but also represent some selective combination of the meteorological variables in which we are interested. For example, one generally expects the wind speeds associated with neutral conditions to be higher than those associated with either stable or unstable conditions. In any given experiment, it may make a difference whether unstable conditions have low wind speeds and large heat fluxes while neutral conditions have high wind speeds and small heat fluxes, or whether the winds remain the same at all times and only the heat flux varies. When a stability index such as a Richardson number is used, wind speed and heat flux are combined and one cannot distinguish between these two possibilities. Thus the results may be less general than we think, if the experimental conditions did not include a wide range of both wind speed and stability. This problem is compounded by the fact that we generally select certain conditions under which to measure; for example, we may decide to measure only under clear skies in order to insure steadiness in incoming radiation. If, however, strong winds are associated with overcast skies, we are deliberately excluding all cases of strong winds from consideration and thus again detracting from the generality of the results.

Another problem is that we have a somewhat limited range of variables here on earth. Truly fundamental laws of atmospheric turbulence would apply on any planet and in any atmosphere. Yet such quantities as specific heat, gravitational force, mean temperature, mean density, etc., which appear in our equations as variables, are virtually constant on this planet; thus, although we may in-

clude them in our expressions, we really know nothing about how they affect the problem in any general sense.

As an example of a problem that may occur, I will refer to some of the work done by Swinbank and Dyer in Australia, who have examined the temperature profile from a dimensional point of view. They have followed the reasoning of Monin and Obukhov, and describe the temperature profile by means of two dimensionless products,

$$\frac{kc_p \rho u_* z}{H} \frac{\partial \theta}{\partial z} \quad \text{and} \quad \frac{kgzH}{\rho c_p T u_*^3}$$

The latter is recognized as Monin and Obukhov's $-z/L$, and the former they call ϕ_H . They have performed a number of experiments with great care, and they have found that ϕ_H varies with z/L as shown in Figure 1. There is a great deal of scatter, but the mean trend is clearly indicated. Examination of the expressions for ϕ_H and z/L shows that for practical purposes the only variables are u_* , z , H , and $\partial\theta/\partial z$. In these experiments, u_* and H were constant with height, and $\partial\theta/\partial z$ was determined at a number of different heights. Consequently, the observations plotted at the right of the graph do not represent very unstable conditions; rather they represent the same moderately unstable conditions appearing further to the left, but are determined from measurements made at a greater height. Such measurements cannot really be used to test a similarity theory that equates an increase in instability with an increase in height because sufficiently large heat fluxes are not observed.

Figure 1.

Another problem is that the expression for ϕ_H contains u_*/H , while that for z/L contains H/u_* ³. There is thus a "built-in" relationship between ϕ_H and z/L . Deacon and Taylor were rather concerned about this, and wondered whether this built-in bias would overshadow the variation shown by the observations; therefore, they took the same gradient measurements, took u_* and H from tables of random numbers, and plotted a graph; and they found that, although the scatter was much greater, the mean trend was essentially the same as that shown here. I wonder if this may mean that in this problem the experiment was not really necessary – that the correct result could have been deduced in advance. Of course, this "correct result" might not hold true if we had observations from other planets and other atmospheres, making density, specific heat, etc., true variables.

DISCUSSION

BADGLEY: It is obvious from your expressions that there are not enough dimensionless parameters to describe the problem. You have 8 dimensionless properties, in 4 dimensions, so there should be at least 4 dimensionless groups of numbers needed to describe the problem completely, of which you have considered only two.

BERNSTEIN: I didn't go into detail regarding the dimensional analysis. I think I can show that the reasoning is legitimate. One may argue that H and c_p are the only variables containing a heat dimension, and therefore must occur in a ratio as a single variable, and that g/T enters as a buoyancy coefficient and is therefore also a single variable, thus reducing the number of variables to 6 and the number of dimensionless products to 2. Alternatively, one can analyze the data in terms of 4 dimensionless products – which I have done – and it turns out that two of them drop out. The point is that we are hampered in our search for truly fundamental laws by the limited range of variables here on earth, and even a well executed experiment can yield little information beyond what we could have found *a priori* without needing the experiment at all. At least, this may be the case as far as the temperature profile is concerned.

PANOFSKY: Would you say there is the same difficulty with the wind profile? Or is this better because there is not quite the same correspondence of variables in ϕ and z/L so that there is not the same random number effect?

BERNSTEIN: You are right. H does not occur in ϕ for the wind profile. I mean to try random numbers in the wind profile case, but I haven't yet.

ARNOLD: In the case you discussed, were these stable Gaussian variables, or do they have some other characteristics?

BERNSTEIN: I believe they were taken from a rectangular, not a Gaussian, distribution of random numbers.

ARNOLD: This indicates the variation of u_* and H , doesn't it?

BERNSTEIN: Yes. I don't know how different the result would be if one used Gaussian random numbers.

Diffusion Experiments

PAUL FRENZEN

No one at this meeting has yet written this equation:

$$\overline{x^2} = 2\overline{u^2} \int_0^T \int_0^t R(\xi) d\xi dt.$$

Obviously this expression is written in terms of time as the independent variable; and this is relevant to a point that has been discussed: the natural coordinates for this admittedly idealized specification of the *mechanism* of diffusion are, indeed, time; that is, the natural coordinates for a Lagrangian description of dispersion in a moving fluid are time. Perhaps the confusion arises in that the natural coordinates for describing the *symptoms* of diffusion (as expressed, say, in the form of pollutant concentrations recorded at fixed points along the ground) are space. Thus, in the analysis of diffusion, there is no question whether we are dealing with space or time; for we are using either

and/or both, depending upon the particular way an observer chooses to view a given situation.

I will describe two different experiments, each of which had the above equation as a starting point. The first goes to the heart of the idealized Taylor diffusion problem and is an investigation of the influence of gravitational stability upon the Lagrangian autocorrelation function by means of hydrodynamical-model studies. The second departs from the more familiar applications of the Taylor equation and does not deal with the diffusion problem at all.

The truly homogeneous, stationary fields of turbulence required by the Taylor theory do not (as pointed out at this meeting) ordinarily exist in nature; but in the laboratory we can obtain data closely resembling those observed in such ideal, stationary fields, provided we carefully correct for the effects of turbulent decay. Figure 1 shows the

Figure 1. Argonne meteorological model towing tank.

apparatus used in these experiments, a towing-tank 30 ft long, 2 ft wide, and about 18 in. deep. At the top is the moving carriage which can be pulled along the tank by an endless cable driven by the mechanism below. In operation, the tank is filled with water to a depth of 12 in., and 95% of the free surface is covered by a semirigid, floating lid which both damps undesired wave motions and serves as an upper heat exchanger. The entire bottom of the tank constitutes a second, lower heat exchanger. By circulating water at the desired temperatures through these devices, vertical temperature gradients of the desired direction and magnitude can be created and maintained for days at a time.

The experiments were carried out in fields of decaying, homogeneous turbulence generated in the wake of a rectangular grid (Figure 2) moved

through the tank by the carriage. Figure 3 is a positive reversal of a photograph of a set of turbulent trajectories traced by a number of particles moving through a 6-in. cube within one such field. Note that successive positions of each particle on its particular trajectory are marked by brighter flashes at 1-sec intervals. Simply by measuring these 1-sec displacements, we obtained successive 1-sec average velocities for as many dispersing particles as desired. Five such trajectories were selected from each run, each complete experiment consisting of 10 runs carried out under virtually identical initial conditions. The resulting matrix of up to 20 successive velocity measurements taken along 50 trajectories made up the raw data, uncorrected for decay.

The method of decay correction used was originally suggested by G.K. Batchelor (*Proc. Cam-*

Figure 2. View of the interior of the tank test section showing grid approaching from the right; arrows locate center of camera field.

Figure 3. Positive reversal of 17-sec-long turbulent trajectories photographed from above. Droplets are moving in direction of the arrow; intensified images on trajectories are 1 sec apart.

bridge Phil. Soc. **48**, 345-62, 1952). It is based upon the well established properties of the so-called first period of decay known to characterize the earlier history of fields of grid-produced turbulence. In this method, each 1-sec mean velocity is multiplied by the square root of the time of observation, this compensating for the observed $t^{-1/2}$ decay of the turbulent velocity field. In addition,

all real time intervals in the experiment are re-defined in such a way that the effects of steadily increasing turbulent scale during decay are eliminated (for details, see ANL-6794). These numerical operations were performed upon the raw data by a computer, creating at one stage a decay-corrected matrix of Lagrangian turbulent fluctuations. Thus, although the ideal of a stationary,

Figure 4. Examples of 4 autocorrelation curves (vertical component) derived from experiments carried out in neutral, weak stable, moderately stable, and very stable stratifications; note decreasing area under curve and change of functional form from simple negative exponential to Gaussian.

Figure 5. Examples of integral parameters computed from autocorrelation curves; note that the horizontal components of the Lagrangian scale time τ_k remain more or less unchanged under conditions of increasing stability while the vertical component steadily decreases; similarly, the frequency of the principal peak of the horizontal component of the Lagrangian spectrum n_k remains approximately constant while that of the vertical component increases; in each graph, the abscissa N^2 is the Brunt-Vaisala stability parameter.

homogeneous turbulence field was never actually attained in the laboratory, the traces of such a field were made to exist, briefly, in the high-speed memory of the computer.

The computer used the corrected data to compute the Lagrangian autocorrelation function for each experiment. Figure 4 shows the functional forms for the vertical component of the Lagrangian autocorrelation function observed in 4 experiments whose stratifications ranged from neutral to very stable; the lapse rates were $< +0.01^\circ/\text{cm}$ in the neutral case shown in the upper left, $+0.03^\circ/\text{cm}$ in the weak stable case in the lower left, $+0.12^\circ/\text{cm}$ in the moderately stable case in the upper right, and $+0.30^\circ/\text{cm}$ in the very stable case shown in the lower right. All I would like to point out here is the marked change in the behavior of the vertical component of the autocorrelation function with increasing stability. It falls off more rapidly as the stability increases, as one would expect; but note also that there appears to be a systematic change in the general functional shape. Whereas a version of a simple negative exponential adequately fits the neutral case, the increasing stability gradually introduces Gaussian functions as the more appropriate form.

Significant changes in the form of the correlation function could make the use of simple scale factors between the independent variables inadequate for Lagrangian-Eulerian comparisons. The possibility remains that similar changes in shape may also occur with increasing stability in the vertical component of the Eulerian correlation, but one cannot count on such a fortuitous circumstance. It may be a small point, but it would be interesting to see if this behavior can be found in either or both types of correlation functions in the real atmosphere.

Figure 5 shows some of the turbulence parameters that can be derived from the correlation functions. Here the general term ϵ_k represents the Lagrangian scale time, the first integral of the correlation function, or, more simply, the area under the correlation curve; ϵ_{xy} then designates the scale of the horizontal components while ϵ_z represents that of the vertical component. These are shown plotted against N^2 , the Brunt-Vaisala stability parameter. Note that, over-all, the horizontal scales remain essentially unaffected by the changing stability on the fluid. Here, where the ϵ_{xy} curves briefly rise abruptly in near-neutral conditions, there is a peculiarity which may be

due to the unsymmetrical tank cross section; however, the point is not fully understood. On the other hand, the other curve for the vertical component ϵ_z shows a steady decrease in Lagrangian scale with increasing stability, this reflecting the steady decrease in area under the vertical autocorrelation curves seen in Figure 4.

Through the cosine transform the Lagrangian scale time can be shown to provide a measure of the frequency of a principal peak in the Lagrangian turbulence spectrum. Angell showed some results of this kind obtained from tetron flight data. In the towing-tank results shown here, we see that the principal frequency of the vertical oscillation of the tracer droplets n_z steadily increases with increasing stability, whereas the principal frequency of the horizontal fluctuations n_{xy} remains essentially unaffected. In reviewing this diagram (as a result of conversation with Dr. Angell) I realized that this n_z vs N^2 line lies very nearly at a 45° angle; since the abscissa is laid out in such a way that it is linear in the absolute value of N , this indicates a one-to-one correspondence between the principal vertical oscillation frequency and the Brunt-Vaisala frequency. Indeed, this should have been expected in view of the common, text-book explanation of the Brunt-Vaisala frequency in terms of the vertical oscillation that results when a constant-density balloon suspended in a stably stratified environment is released from some height other than its equilibrium level. It responds like a simple harmonic oscillator, with vertical oscillations resembling those of a weight suspended by a spring. Thus the observed increase in n_z with increasing stability is readily seen to be analogous to the increasing frequency of oscillation of a suspended weight with increasing stiffness of the spring.

Finally, it may be remarked that, since these Brunt-Vaisala type motions evidently represent the principal oscillations (hence principal vertical velocity fluctuations) present in turbulence fields in stably stratified media, and since, from the experiment, the two appropriate functional forms for the vertical component of the Lagrangian autocorrelation function are known for these same conditions, it seems possible that one might write fairly good versions of the vertical correlation functions for a particular situation solely from the vertical density gradient (merely by identifying N with n_z and reversing the procedure by which n_z was obtained from the correlation curves).

The other experiment to be described uses the Taylor equation as a point of departure. Here we are once more concerned with stationary time series data, but not with fluctuating velocities of dispersing particles. From a purely formal standpoint, the Taylor equation can be used to analyze any stationary time series, as Taylor implied in his paper (*Proc. London Math. Soc.* 20, 196-212, 1921) when he illustrated his discussion with a continuous record supplied by a fluctuating pressure signal. The present application uses Eulerian records of turbulent velocity fluctuations as seen by a fixed anemometer.

Figure 6 schematically illustrates the derivation of the Taylor expression from an Eulerian time series. On the left is shown the familiar dispersing particle concept: one computes the displacement of the i th particle at time t in terms of the integral of its instantaneous, fluctuating velocity over the period, then squares and averages these lengths over an ensemble of particles, and finally arrives at the familiar Taylor expression for the mean-square dispersion. The right-hand sketch illustrates that one can base a completely analogous derivation upon a continuous record of Eulerian fluctuations: here we compute an unspecified "Eulerian length" for the i th sample of the record by integration, square and average over an ensemble of record samples of similar duration, and again arrive at the Taylor equation. Of course, $\overline{u^2}$ now refers to the variance of the Eulerian record, and $R(\xi)$ represents an Eulerian time correlation derived from the anemometer record.

Figure 7 illustrates (both for the Lagrangian particle case on the left and for the Eulerian record case on the right) that one can substitute mean values for the integrals in the previous representation. In this way one obtains the alternative expression for the first integral (Lagrangian displacement or Eulerian length, as the case may be) shown in the middle of Figure 7. By squaring and computing an ensemble average one thus obtains the alternative expression for the variance of the first integral of the time series shown at the bottom.

One might ask what the significance of $\overline{x^2}$ might be in the Eulerian case. In Lagrangian examples it represents, of course, a measure of the dispersion attained by time T ; but it has no obvious, physically significant definition in the Eulerian application. But that need not bother us, for in the next step we eliminate it by substituting the right-hand side of the expression (2) for the right-hand

From

$$x_i(T) = \int_0^T u_i(t) dt \quad \text{or} \quad x_i(T) = \int_{t_i}^{t_i+T} u(t) dt$$

it can be shown

$$\frac{1}{N} \sum_1^N x_i^2(T) \equiv \overline{x^2(T)} = 2 \overline{u^2} \int_0^T \int_0^t R(\xi) d\xi dt \quad (1)$$

Figure 6. Schematic representation of the analogy between a true Lagrangian displacement [where $u_i(t)$ represents the velocity of a particular dispersing particle] and the so-called Eulerian length [where $u_i(t)$ represents a particular segment selected from a wind speed record supplied by an anemometer].

Alternatively:

$$x_i(T) = u_{Ti} \cdot T$$

hence:

$$\frac{1}{N} \sum_1^N x_i^2(T) \equiv \overline{x^2(T)} = \overline{u_T^2} \cdot T^2 \quad (2)$$

Figure 7. Schematic representation of the analogy of Figure 6 with mean values (u_{Ti}) substituted for integrals over the period T .

side of the Taylor equation. A number of people have followed a similar procedure, usually concerning themselves with diffusion applications; these include Pasquill, Ogura, Smith, and MacCready, among others. Many of these analyses involve the equation shown at the top of Figure 8. Termed "the variance function," this expression represents (in the language of the present application) the percentage of the total variance ($\overline{u^2}$) retained after the Eulerian record has been subjected to an averaging or running-mean procedure over an interval T , with $\overline{u_T^2}$ designating the magnitude

From (1) = (2):

$$\beta(T) \equiv \frac{\overline{u_T^2}}{\overline{u^2}} = \frac{2}{T^2} \int_0^T \int_0^t R(\xi) d\xi dt \quad \left\{ \begin{array}{l} \text{Variance} \\ \text{retained,} \\ \text{any } u(t). \end{array} \right.$$

But, in inertial subrange,

$$R_E(\xi) = 1 - \frac{c}{\overline{u^2}} \overline{u}^{2/3} \epsilon^{2/3} \xi^{2/3} \quad \left\{ \begin{array}{l} \text{For Eulerian} \\ \text{fluctuations,} \\ u(t) = U(t) - \overline{U} \end{array} \right.$$

Therefore:

$$\overline{u^2} - \overline{u_T^2} = \frac{9c}{20} \overline{u}^{2/3} \epsilon^{2/3} T^{2/3} \quad \left\{ \begin{array}{l} \text{Variance} \\ \text{lost.} \end{array} \right.$$

and

$$\epsilon = \frac{A}{\overline{U}} \left[\frac{\overline{u_T^2} - \overline{u_{T_2}^2}}{T_2^{2/3} - T_1^{2/3}} \right]^{3/2}, \quad A \approx 3.3 \quad \left\{ \begin{array}{l} \text{"EVA"} \\ \text{relation.} \end{array} \right.$$

Figure 8. Basic equations leading to the EVA relation for the rate of turbulence dissipation.

of the reduced variance that remains after this smoothing has been carried out. As noted on Figure 8, this relation holds for any stationary time series $u(t)$.

The second line on Figure 8 presents the known functional form of the Eulerian time correlation for the specific case when the time series $u(t)$ represents turbulent fluctuations seen by a fixed anemometer which has been filtered in such a way that it sees only the inertial subrange of the turbulence spectrum. By substituting this function in the top line and rewriting the expression in terms of the absolute instead of the relative variance reduction, (i.e., as a difference instead of a quotient), one obtains the third equation for the actual amount of variance lost in a high-frequency (inertial subrange) Eulerian record of turbulent fluctuations after it has been smoothed by running means. Note that if one associates the limited response times of real instruments with such averaging procedures, it becomes possible to compute the *total* variance of a real wind from a record taken by an imperfect instrument which has failed to respond to the higher frequencies. F.K. Ball has, in fact, done this.

The final expression shown at the bottom of Figure 8 follows from the fact that the third expression for variance lost contains the dissipation function ϵ . By writing this expression for two different averaging times, T_1 and T_2 , and eliminating the perfect variance $\overline{u^2}$ between them, we can solve for ϵ to obtain the last expression, which

might be termed EVA for Eulerian variance analysis. The value of the constant A follows from the known value of the universal constant c in the expression for $R(\xi)$, which is, in turn, related to the constant in the $k^{-5/3}$ spectral law for the subrange.

The EVA relation has been tested by applying it to wind records obtained with 3-cup anemometers mounted at several heights on a radio mast, the data being a portion of that obtained by E.L. Deacon some years ago in a study of the variation of gust intensity with height. The instruments were ordinary Casella anemometers with no especially fast response characteristics. However, since they were located relatively high above the ground and were operated only in fairly high wind velocities, their response times were actually rapid enough to detect a significant portion of the low frequency end of the inertial subrange.

If one particular anemometer record gives a certain value of the turbulence dissipation when the EVA relation is computed with two particular averaging times T_1 and T_2 (the times being short enough to be appropriate to the high frequencies of the inertial subrange), then the relation must always give the same value of ϵ for any pair of sufficiently short averaging times. This follows because the rate of turbulence dissipation is a property of the turbulence field and not of the details of its measurement. Further, the foregoing implies that the terms within brackets in the EVA relation must preserve the same value for all appropriate averaging times. Therefore, if we plot the numerator of this ratio versus its denominator, the region of validity for the EVA relation will be designated by a straight line of constant slope. What is more, the line must pass through the origin, for however small T_1 and T_2 are chosen to be, the slope of the line must remain the same, even though both numerator and denominator become vanishingly small.

Figure 9 shows 8 plots of this kind, the 4 on the left representing data collected at a height of 64 m and those on the right, at 153 m. Consider first the upper, dashed line in the family of 64-m curves shown in the upper right. The first point on this line is located by variance reduction factors (i.e., numerator and denominator of the bracketed terms) based on the 2-sec means minus the 4-sec means, while the next point is designated by factors derived from the 2-sec means minus the 6-sec means, and so on. Below the dashed curve, the solid line marked by triangular points represents the 4-sec means minus the 6, minus the 8, and so on.

Figure 9. Variance reduction factors (numerator vs denominator of terms within brackets, last equation Figure 3) computed from 3-cup anemometer data recorded at heights of (a) 64 m and (b) 153 m.

In virtually all cases, curves based on the 2-sec means (dashed lines) do not pass through the origin; this is considered to indicate the presence of high-frequency noise, probably introduced by the method used to collect the 2-sec mean winds from the original, rather coarse graphical record of anemometer switch closings. But note that the initial portions of all other curves do, indeed, tend to pass

through the origin; thus, averaging over 4 sec or more successfully smooths out the spurious contributions to the high-frequency variance. Sooner or later (specifically, when the larger of the two averaging times locating a point exceeds the value designated $T_K \text{ max}$), the curves fall away from the line through the origin; this behavior designates the point where the averaging times have become too large for the variance reduction effect to be controlled solely by the characteristics of the inertial subrange.

Finally, note that a value for the slope of the lower, straight portions of the curves is shown on

Figure 10. Observed rates of turbulence dissipation plotted against mean wind speed showing close adherence to $\epsilon \propto \bar{U}^3$ (solid line) on part of 64-m results; dashed line represents trend expected at 153 m by translating solid line in accordance with $\epsilon \propto z^{-4/3}$.

each diagram; these correspond to the values of the bracketed terms in the EVA relation which enable one to calculate a rate of turbulence dissipation for each of the cases. Figure 10 shows values of ϵ determined in this way.

The plots show that at 64 m the dissipation values both follow the expected trend with the cube of the mean wind speed and also exhibit an average, normalized value (i.e., an average of the individual values reduced to a standard wind of 5 m/sec) that is of entirely reasonable magnitude, namely about $10 \text{ cm}^2/\text{sec}^3$. At 153 m, however, the values are less well organized, which perhaps indicates that, because of relatively limited uniform terrain, the turbulent structure at this relatively great height was not fully adjusted to conditions in the layers below; but the normalized mean was still a reasonable $3 \text{ cm}^2/\text{sec}^3$.

At Argonne we have built what we term the LIHT (low inertia, high torque) anemometer in an attempt to employ the EVA method at more convenient heights and in winds of only average speed. The rotors of this instrument are made of expanded polystyrene, the entire rotor assembly weighing about 6.4 g, only about $\frac{1}{10}$ the weight of the cups and arms of a Casella, even though the LIHT cups present twice the frontal area to the windstream. The base is a modified version of an old Beckman & Whitley photochopper model; as

much metal as possible was machined from all rotating parts in order to reduce the inertia, and a 6-hole chopper cylinder was installed for the initial trials (we have since increased this to 21, but this has necessitated a new, smaller photocell). Acceleration tests in a wind tunnel indicate that the LIHT anemometer has a response length of only 0.5 m. Since this indicates that the instrument will adjust to $1 - (1/3\epsilon)$ or 95% of a sudden velocity change in 0.3 sec when the average wind is 5 m/sec, the LIHT anemometer is expected to be able to detect the low frequency end of the inertial subrange at heights as low as 5 m, where $\bar{U}/z = 5/5 = 1 \text{ cycle/sec}$.

One of the goals of this program is the measurement of the surface stress from only single point measurements, the ultimate hope being to develop a method for use on board ships at sea. Given the dissipation function, one can obtain the stress τ through the relation

$$\epsilon = \frac{\tau}{\rho} \frac{\partial \bar{u}}{\partial z} + \phi_1(H) + \phi_2(\text{div})$$

where ϕ_1 represents the rate of production of eddy energy through the mechanism of the vertical heat flux and ϕ_2 represents the divergence of the vertical diffusion of turbulent kinetic energy. When the stratification is neutral, both ϕ_1 and ϕ_2 are zero, and one may use the logarithmic profile for $\partial \bar{u}/\partial z$. But there are some indications (a point which may be amplified by Professor Panofsky) that in unstable conditions, the third term ϕ_2 tends to cancel the second, ϕ_1 ; that is to say, as fast as additional energy is created by the turbulent heat flux mechanism, it tends to be carried away by the divergence. Thus, one may be able to use the first term alone to connect the stress and the dissipation under unstable conditions as well.

DISCUSSION

CRAMER: This kind of study has been pretty well carried out. We also have the advantage that we have made direct measurements of the heat flux and the momentum flux. It works out quite nicely, and we find that, although we could not evaluate the divergence of the value of kinetic energy terms as well as we would like, Professor Panofsky is right that in instability there is almost equivalence between the heat flux term and the divergence term. There is some suspicion that the divergence term is slightly larger, but in any event there is great promise for this particular technique in estimating dissipation or the stress. Concerning the use of this technique over water,

it seems to us that it is very critical with regard to where you choose your functions that you make sure your observations actually fall within a suitable range. We suspect that over water the spectrum is essentially a high-frequency spectrum, and it will take some checking to be sure the measurements are in the right range as far as response is concerned. Aside from that difficulty, I don't see why it shouldn't work nicely.

FRENZEN: Are you using hot wire anemometers?

CRAMER: No. We use a thermistor anemometer. It is probably good up to 5 cycles. Because of our sampling rate, we are not taking it out that far.

FRENZEN: Deacon gave up hot wires because they have to be exactly matched.

CRAMER: Yes, but these particular thermistors work very nicely, and we have had no difficulty. Because of the type of structure and function, the transmission is loaded in such a way that it projects towards low frequencies very heavily, so that this relationship has been observed to hold at lengths of frequencies which you would not expect in terms of the inertial subrange. That is a favorable feature. The Russians, I think, have the right idea when they plot the structure functions; the place they choose to evaluate it is along the longest leg before it starts to break down, so that they avoid all the response troubles and holding troubles, and after the averaging can take a very short lag.

DAVIDSON: In the analysis of your first experiment, why do you throw away the negative part of the Lagrangian?

FRENZEN: The shapes of the Lagrangian autocorrelation functions tend to be distorted by mean flow effects when the correlation lags are long, and this effect is most pronounced in the stable cases. The distortion in the "tails" is the principal reason analytical functions were

fitted to the observed $R(\xi)$ distributions, in order that integral determinations of scales, etc., could be based primarily upon correlations computed over shorter lags. What it amounts to is that our estimates of $R(\xi)$ are good so long as ξ is not too long; but, as ξ increases, estimates of $R(\xi)$ begin to scatter widely, both because the confidence limits on R widen drastically as R approaches zero, and because what in the experiment were initially small errors in mean flow allowances, become increasingly serious as the decay corrections multiply them by larger and larger time factors. Thus, we really are confident only of the shapes of the initial and intermediate portions of the autocorrelation functions; the behavior of the "tail," where $R(\xi)$ sometimes goes negative, is quite uncertain.

CRAMER: Your negative case has a large stable area?

FRENZEN: Well, that is something else.

CRAMER: Isn't it real?

FRENZEN: One has to be very careful in working in stable cases. In diffusion studies, I don't think we understand the effects of extreme stability in either the atmosphere or the tank. This is one area I intend to look into when I put the towing-tank back into service next fall. Briefly, the effect in the experiment to which you refer is caused by the fact that any disturbance in a strongly stratified, stable fluid tends eventually to create a broad, vertically narrow current, a two-dimensional jet. When one of these forms in a field of turbulent trajectories, it stretches some parts of them in preference to others. Since the passage of the grid does not create the jet immediately, its full effects are felt only by the latter parts of the trajectories; as a result, estimates of $R(\xi)$ computed from such trajectories over long lags tend to show negative correlations, while those for small ξ remain largely unaffected. In the true context of the experimental model, however, the effect is spurious.

Micrometeorology

MORTON L. BARAD

I would like to bring up a subject that has not yet been discussed at this meeting. I would like to ask a question and see what the response is from this group, which represents 90% or more of the competence in micrometeorology in this country. I think – and I am curious whether others agree – that there has been a tendency for the micrometeorologist to associate himself with the time series analyst and with sophisticated statistical methods. On occasion there have been some contacts – not too fruitful, I am afraid – with the fluid dynamicist.

But I think we have, or will have in the years ahead, something to contribute to the broader field of meteorology. Where else but to a group such as this can people working on more sophisticated models of atmospheric circulations and subsequent weather processes and who are concerned with the transfer of heat, momentum, and water vapor turn for assistance? Maybe I have been so isolated from it that I am not aware of the great contact which now exists between the micrometeorologist and the large group of meteorologists engaged in the field of numerical weather prediction. As we begin to do the things that are on the horizon now, as we get to the point where we can measure fluxes, where we can provide an input on such terms as $\partial\tau/\rho\partial z$, the time is ripe for us to begin to cooperate, formally or informally, to try to provide more to these people. They seem to feel that the boundary layer is a sort of never-never land, that it is best to take a gross look at it and to treat it as lightly and as quickly as possible. They look upon us as being in a deep morass from which we will never emerge. Maybe they are right.

This is my question (or plea): since groups such as this often have conferences, and since some of us here are in a position to prompt these by virtue of membership on committees of the American Meteorological Society and other groups, might it not be a good idea if we planned a workshop to bring together some of the people who sit on the outside of the boundary layer and tell us about the

success they have at 700 or 500 millibars, but bemoan the fact that they do miserably at 1000 millibars, and some of us who sit in the boundary layer, a little more concerned with whether the situation is more likely to give cumulus clouds or remain completely clear?

DISCUSSION

FRENZEN: I believe there are some moves in this direction in connection with studies at the air-sea interface. One hope I have for the LIHT 3-cup anemometer is to develop it as a device to obtain synoptic measurements of surface stress from ships at sea.

PANOFKY: There is a working group which is supposed to be looking into just this. It deals with boundary problems in large-scale meteorology, and it is supposed to bring out a report summarizing what the micrometeorologists know. Then Dr. Cressman can implement any recommendation in this area. There will be a meeting on micrometeorology in October in Salt Lake City, and I wonder whether it might be a good idea to extend particularly invitations to macrometeorologists who normally would not attend in order to start some informal discussions on this.

HOLLAND: Some members of the AMS Committee on Turbulence and Diffusion are here. Maybe we could get a quorum to vote on this. No special efforts have been made to invite macrometeorologists to the micrometeorological conference as yet, but there are some interface people like Mr. Thompson on the program, and there may be others. Mr. Carlson may know of other people prominent in the large-scale business who plan to attend the Salt Lake City conference.

CARLSON: You can make arrangements right now.

HOLLAND: We certainly should consider this. It would be a good opportunity because the large-scale meteorologists probably are not aware of how much is known about the boundary layer and have not perhaps taken advantage of what science we have been able to develop over the years in these studies.

BARAD: That is one part of what I am talking about, but there is also another part. I am afraid that not only they don't know what we have done, but also we know far less about what they have done. Unless there is real cross-feed in this area, we will continue to have a separation for years.

DAVIDSON: I am not a macrometeorologist, but I know hardly anything about the boundary layer although I know a little about the surface layer.

BARAD: I agree. I don't think we are now in a position to present ourselves to a group of numerical weather prediction people and say that what we know about the boundary layer and about the surface layer is probably sufficient for their needs. But we are on the verge of providing direct measurements. For years we have been dis-

cussing ways of getting around the problem of direct flux measurement, and we have devised all sorts of artifices for doing this. Perhaps now, as we get into the area where it is possible to do direct measurement, we will be in a position to help them, and at the same time open up new vistas all around.

Structure of Turbulence

HANS A. PANOFSKY

One problem in micrometeorology is that various people who are interested in the same sort of things never talk to each other. This group is essentially oriented towards diffusion, and there are very few structure people here. There are people interested in the effect of structure turbulence on high buildings, and people interested in turbulence affecting airplanes which, strangely enough, may obey the same laws as turbulence close to the ground. There are people interested in missiles. The propagation people are getting more interested in turbulence, and the various groups seem to meet each other very rarely; I hope that there will be another colloquium where representatives from the various groups can find out what they need and perhaps cooperate on getting some of the information. Everyone has some kind of tower for measuring wind, temperature, and humidity, but nobody has enough money to put into the systems to do the kind of thing the Russians do. The Russians have a solid 300-m tower with 14 levels, with instruments in all four corners of each level, so that anybody who wants to measure anything can put instruments on the tower. I hope it may be possible to bring together in a colloquium the different groups interested in wind structure, both mean and fluctuation.

What do we know about the wind direction and the temperature direction close to the ground? A lot of progress has been made both in fluctuation statistics and in the mean. The profile has come from similarity theory, which is in this sense a kind of dimensional analysis. Certainly one does not try to go back to the fundamental equations. However, similarity theory is not perfect. For example, in the standard deviation of azimuth, which is important for diffusion, or the standard deviation of longitudinal velocity, there is very little change with height in unstable conditions. But if the atmosphere is more unstable, the fluctuations of azimuth become very much larger at the same height, so that similarity theory cannot apply to this particular set of statistics.

Where does similarity theory apply? It doesn't seem to apply usually under stable conditions. In fact, under very stable conditions, the different layers are essentially decoupled, and one can't really define any wind profile or simple distribution of anything. The similarity theory does apply under near neutral, neutral, and unstable conditions. The wind profile can be represented by an equation derivable from similarity theory:

$$\bar{u} = \frac{u_*}{k} \log \left(\frac{z}{z_0} - \psi(R_i) \right).$$

This equation, where ψ is a universal function, is compatible within the error of measurements with most of the data that have been taken over reasonably good terrain.

The Australians have cleared up the question of what the ideal terrain would be; it remains to be seen whether any real terrain would give as good answers, and we will have to study the effect of the change of terrain. We understand something about this now, and many kinds of experiments with observations close to the ground, say up to 8 m at O'Neill or at Round Hill, satisfy well enough the conditions of uniform terrain. In these cases (including those in Australia, where the terrain question was particularly considered) the same universal function fits under unstable conditions. If a plot is made for the middle of the day when there is equilibrium, of $\log(z - \psi)$ vs wind speed, with the data from Argonne, Round Hill, Australia, and O'Neill, it gives nice straight lines. Thus, we understand the wind profile under proper conditions with enough fetch; it is somewhat controversial what is enough fetch, but we are approaching an understanding of this also.

The equation fits the data only if it is written in terms of the Richardson number, which is given by the temperature profile and the wind profile. In other words, given the temperature profile, we can explain the wind profile. However, the original similarity theory had another function here, in terms of z/L ; with this as the universal function,

everybody gets a different function. Particularly, there is a discrepancy between Swinbank's last paper and his earlier work over the past 5 years.

We know the gradient Richardson number but not the flux Richardson number, which means that we don't know much about K_h/K_m . This is one of the most critical discrepancies. For example, in the O'Neill data there are indications that the wind profile and temperature profile are similar with height – that K_h/K_m does not vary much with height. The Australians might say that the terrain was not flat enough or not big enough. In the Australian data there is a tremendous dissimilarity of profiles. There are many possible reasons why the wind profiles and the temperature profiles may be different in one place and similar in another. There may be irregularities in heat distribution even when the terrain is uniform. One of the things we know relatively little about which is causing trouble is the relation of the wind profile not to temperature but to heat flux, through K_h/K_m .

I would like to say a little about the structure of turbulence. With regard to spectra and fluctuation statistics, we know a lot about spectra at a point as a function of time; we know which obey similarity theory and which don't. We still have trouble with the relationship of Eulerian and Lagrangian statistics, and here the question is whether Pasquill's β is a constant factor. Pasquill has shown that even though the shapes of the Eulerian and Lagrangian correlograms may not be the same, as long as one knows the scale factor properly, one can predict diffusion, and quite a few measurements have been made of this kind of data.

There is theoretical and observational evidence (for example, observations by Hansen and by Angell) that β actually is proportional to a constant divided by I , the relative intensity of turbulence. This is not completely clear, but the evidence is overwhelmingly in favor of such a relation. It is interesting that for diffusion from an instantaneous source the often used Smith-Hay formula is

$$\sigma_x = \frac{2}{3} \beta I^2.$$

If β is proportional to a constant divided by I , then σ_x , the spread of a cluster with distance, is proportional to I , which is the standard deviation of a velocity divided by a mean velocity. If I is moved to the other side of the equation, it is clear that this formula means that the expansion of a

cluster with time is proportional to the fluctuation velocity. This sort of equation seems to work fairly well, even in Pasquill's diagram, which is supposed to be drawn with a constant β . This is one of the things that need to be studied, since many engineering formulas involve β .

Finally, it is very important that we get together with larger-scale meteorologists, but we must realize that there is a big unknown territory, roughly between 1000 and 5000 ft, where an enormous amount of research is needed, mainly observations. There are hardly any data for this region. There are 6 or 7 theories about how the mean wind and the temperature vary, and this is the region the large-scale people are interested in. They want not only to know the surface flux, which we can measure, but also to be able to predict or estimate the surface flux momentum, given the large-scale condition. We can't do that yet; we need a lot more of that kind of observation.

DISCUSSION

FRENZEN: There is some evidence that the Lagrangian-Eulerian relationship is also stability dependent, independent of changes in turbulence intensity. I think the work was done at Johns Hopkins.

PANOFSKY: Angell found that β depended on both stability and level of turbulence, but these are not independent variables: the more unstable the conditions, the larger the level of turbulence. So if you expect that this kind of relation is satisfied, which follows from the theory, and then make an observation seeking a relation between β and instability, you will certainly find one. As a matter of fact, we found it in the O'Neill data. It is one of those cases in which it is hard to study the dependence on two variables which are very well correlated with each other.

DAVIDSON: In the case of diffusion, is it true that the σ 's vary with x , as the formula predicts? I don't think so.

PANOFSKY: I'm not a diffusion person. Pasquill has some data in his book according to which diffusion of a cluster seems to start out with various tiny linear expansions and then goes to the Kolmogorov subrange, and then there seems to be a rather lengthy range of linear variations.

MARKEE: I am a diffusion person, and I said yesterday that perhaps they aren't too valuable. We studied β using all the data available, including our own from Idaho Falls, and our conclusion is that if β departs from 1, which it may, it doesn't do so sufficiently to affect the results if one is working directly through the spectrum, and that β must depend on the spectrum. If one knows part of the spectrum, one can predict what β will do in terms of predicting diffusion patterns. Concerning the formula for the rate of expansion of the cloud, we are looking at expansions from bottom exposures. We have no evidence for any dependence of σ_x on distance to a power >0.9 .

PANOFSKY: The formula indicates a linear function of time or distance, which you say is not quite right (you

give an exponent of 0.9), whereas the original implication is that the rate of spread depends on L^2 ; but I claim that it is a function of L . Can you decide among the alternatives on the basis of your observations?

Q: I think we can relate the initial dimensions of a cloud, the short-distance effects, to a gradient measure of wind fluctuation, which is an intensity, and that relationship holds very nicely. What happens beyond that point I'm not so sure, but it looks as though it is not greatly different from the kind of pattern we get with continuous sources.

DAVIDSON: Did you ever find a figure of $\frac{3}{2}$?

Q: Probably not. I have to hedge a little because out at 1 km or so some of the measurements are not too reliable. There is some evidence that the rate may increase, but I don't know how much; I expect the increase is very small.

DAVIDSON: That is contrary to theory.

Q: I know it is.

IZLITZER: I think the formula specifically represents the maximum growth and is supposed to be a pretty good

approximation for some distance. The main problem is in computing L . Just what length of record do you take from a Eulerian measurement that is appropriate to a puff? There is a length of record for which this equation is a sufficient approximation, but there is another length of record for which L to the first power will apply.

PANOFKY: Pasquill shows in his book a plot of L vs $\sigma_z^{1/2}$ which is a straight line. But a parabola can be fitted to the data as well.

VAUGHAN: This is a special case of the Smith-Hay formula. In the more general case it predicts expansion as $\sigma^{1/2}$ after the length scale of turbulence has been exceeded by a factor of 3 or 4, that is, at the dimension of the cloud itself. As for β , I agree with Dr. Panofsky in feeling intuitively that it should vary inversely as the turbulence intensity. Regarding what Dr. Cramer said, I don't know what scale diffusion experiments you used to test the β hypothesis, but Pasquill states that over the initial linear rate of expansion, y , diffusion is completely insensitive to β . Thus it almost seems necessary to exceed the length scale of turbulence in order to determine the value of β .

The Application of Meteorology to the Regulatory Program of the AEC

JOHN F. NEWELL

A more appropriate title for this talk would be "The application of meteorology to the solution of engineering problems." The regulatory group of the AEC is responsible for evaluating the safety of nuclear facilities to determine the adequacy of the safeguards provided to prevent the release of radiation or radioactive materials which would have hazardous effects upon the people or the environment. One important part of the program is the study of the effect of a potential accidental release of substantial quantities of radioactivity to the atmosphere. Another important part is the evaluation of the effects of routine releases to the atmosphere of radioactive materials during operation. The AEC has regulations which cover both these aspects. As a member of the AEC Division of Reactor Licensing, I will discuss the problems of a reactor facility.

Two basic situations with meteorological implications are considered in the licensing of a reactor facility: safety in connection with a potential accident and safety in connection with routine operations. *Part 100, Title 10, of the Code of Federal Regulations* describes the reactor site criteria in terms of performance required in order to prevent serious consequences to the public in the event of a serious accident to a reactor, in which a large amount of fission products might be released from the fuel. The criteria are in terms of dosages to the public of either 25 rem for whole-body exposure or 300 rem to the thyroid.

For siting a reactor facility, zones are described in which a reactor site should have an area under control of the operator with the boundary far enough from the reactor so that in the event of a maximum credible accident the dosage at the boundary accumulated in 2 hr should not exceed 25 rem whole-body exposure or 300 rem to the thyroid from inhalation of iodines. Another zone is described in which these dosage values should not be exceeded over the duration of the accident. Within this second zone the population should be low enough so that perhaps the people in the path

of the cloud during the accident could be evacuated, but at the boundary of this second zone no one should receive more than 25 rem whole-body or 300 rem to the thyroid for the entire duration of the accident even if they were in the cloud 100% of the time.

Many types of situations can be encountered, and we have seen quite a number of them already. In the first place, an accident can extend over a short or a long period of time, that is, over a few hours to a day or a week to a month. Also, the various types of terrain conditions must be considered in an analysis of the possible consequences of an accident. The coastal terrains, like those along the East Coast, are fairly flat and have on-shore or offshore winds; they probably have fairly good diffusion conditions most of the time. A reactor facility may be located in a valley on the bank of a river, with the terrain on either side of the river rising to a height of several hundred feet, and there may be cities upstream or downstream from it. The situation typical of the Midwest is rolling prairie land; and in the Rocky Mountain area a facility might be located on a plateau in a mountainous region. On the West Coast the reactor could be located along the coastline but with a mountain range not far inland which would cause certain types of air flow.

After a reactor is in operation, *Part 20* of the Commission's regulations prescribes standards for protection against radiation in terms of dosages which should not be exceeded by any individual over a period of time, usually a year, and also lists concentrations of radioactive materials to which it would be permissible for an individual to be exposed indefinitely. In operation, some reactors release radioactive effluents on a relatively continuous basis, and others on an intermittent basis, over a series of short periods of time. In applying the intent of the regulations, *Part 20* provides that a licensee can either meet the requirements of the regulations at the point of release (the stack) or he can request AEC authorization to meet the con-

centration requirements at a point where the activity would come in contact with people, perhaps even off-site.

This is directly related to the problems of what kind of diffusion there will be and how much, and of what dilution factor can be authorized for a particular location that can be applied with confidence that no one will be exposed to concentrations greater than permissible. Here again the various types of terrain conditions are important. In the case of smaller reactors such as research reactors, which are usually located in built-up areas, another set of conditions has to be dealt with. Such a reactor usually has a short stack, on the roof of a building, and there may be buildings just 100 ft or so away with people in them. What is the diffusion picture in this kind of a situation?

Concerning the types of information that we have been receiving in our applications, 7 or 8 years ago everyone was just beginning to feel the way along. We would receive either no information at all or a conglomeration in which we had to find what we needed. But there was no attempt to direct or to apply the data that were presented to the problem at hand. Unfortunately, we are still receiving applications in which to some degree this is still true, and I would like to ask each of you, who will undoubtedly, as time goes on, be called upon for guidance by applicants for a license, to help them put our problems into perspective.

The reason I suggested changing the title of this talk is that the data that you are developing and publishing are being used by engineers as absolute fact in the design of safeguard systems for nuclear facilities. You are really determining the quality and quantity of the engineering that is being put into, say, a large reactor power plant today, and you are more or less setting the standards for it. I want to emphasize the importance of the work that you are doing in this regard.

For example, part of the routine release of radioactivity to the air may be due to imperfections in the fuel cladding which allow some minute part of the fission products to escape into the reactor coolant, the gaseous part going through the stack out to the atmosphere. There is an economic incentive for the reactor owner to get as much fuel burnup as possible, and he would rather not shut down the reactor to take out a defective fuel rod if he can utilize the atmosphere to dilute and disperse this radioactivity within safe limits. We have already begun to see this sort of thing, and as time

goes on we will see more of it, because it is economical and sensible to utilize the atmosphere insofar as we can; yet we have to be certain that we are doing it in a safe manner.

Here is one important aspect of this, in the form of a question that I am asking you: How accurately can we determine the diffusion so that we can perhaps reduce the margin of safety to some point? To what point can it be reduced? By a factor of 1000; 100; or 10? How much data and what kind would be required in order to achieve each of these factors?

DISCUSSION

DAVIDSON: How much safety margin is there in your figure for the maximum permissible dosage?

NEWELL: I can only say that, each time this has been considered, the concentration values are reduced.

DAVIDSON: How much consideration is given to the possibility of future growth in the neighborhood of any one reactor - if it is releasing the maximum permissible, what does the next client do?

NEWELL: At present there is no way to control the growth of an area around or in the so-called low population zone around a reactor. One can only speculate what would happen if 10 years later the area should be built up completely. Possibly the original engineering safeguards may no longer be adequate. It might be necessary for the licensee to provide more safeguards such as an additional barrier against possible accidental release. This problem undoubtedly will have to be faced eventually.

FUQUAY: In *Part 100* isn't there a discretionary criterion which you didn't mention? In other words, the operator essentially should keep the release to the environs to the lowest practical level, so that there is some judgment made as to the need for the release, even though a higher release would be acceptable at that site.

NEWELL: I don't believe that is in *Part 100*. I think it is in connection with the routine release problem in *Part 20*.

PACK: I think Mr. Newell's question about whether we could reduce the safety margin by a factor of 1000, 100, or 10 should be answered on the basis of economics. The reactor operator knows how much he will gain by getting another 5000-MW-day burnup on the fuel, and we know rather well the costs for a meteorological study. Within fairly close limits the precision of the knowledge about the diffusion of a particular site is a direct function of the amount of dollars spent on the meteorological effort, and, having just completed a study of weather benefits, I think this would be rather easy to determine. If you have to spend \$100,000 on meteorology per year to save \$10,000 worth of fuel burn-up, obviously you don't want to do it. On the other hand, if you can save \$100,000 by spending \$10,000, you will. I think there are enough people here with backgrounds in economics who can begin thinking about this. With our engineering hats on, we meteorologists think our primary purpose is to promote the economical utilization of fuels, and if we recommend something that isn't economical, we haven't done a good engineering job.

The Clean Air Act of 1963

POPE LAWRENCE

The key portion of the Clean Air Act of 1963 is Section 7(b),* but I would like to give a brief summary of the whole Act. I have brought copies to distribute, so that when you return to your installations, where you exercise influence on state, local, and industrial decisions, you will take into account the fact that the Congress by this Act and the President by his action have expressed a change in the national policy with respect to control of air pollution.

It is much too early for anyone to say what effect the Clean Air Act will have on the AEC regulatory program. Certainly it will have no adverse effects, and I think the way the Atomic Energy Commission, through implementation of the Atomic Energy Act, has set up and used its regulatory authority will undoubtedly guide the Public Health Service and the Department of Health, Education and Welfare to a considerable extent in the way they implement the Clean Air Act.

The Act does the following:

First, it changes significantly Federal policy with respect to the kind and extent of aid available to state and locally controlled programs. There has been no change in the view that primary responsibility for control of air pollution rests with state and local governments, but the Federal Government, with the passage of this Act, is equipped to give more effective aid and to stimulate the state, local, and regional air pollution programs to higher levels of activity than in the past.

There has been a change of policy concerning the Federal role in air pollution control, as set forth in the preamble of the Act. First, it cites the fact of high urbanization, including the development of our metropolises to include municipal, county, and state boundaries. It reiterates as fact

*"In order to control air pollution which may endanger the health or the welfare of any persons, the Secretary may establish classes of potential pollution sources for which any federal department or agency having jurisdiction over any building, installation or other property shall, before discharging any matter into the air of the United States, obtain a permit from the Secretary for such discharge."

that air pollution does endanger human health and welfare. It specifically mentions motor vehicles as one of the major contributors to the growing problem, and it states that Federal financial assistance is essential for the development of a program to control air pollution.

These are important points. The Clean Air Act for the first time authorizes the Federal Government to award financial grants directly to air pollution control agencies to aid them in initiating, developing, and improving their programs, and we are now working diligently to be ready as soon as moneys are appropriated. The objective is to stimulate air pollution programs rather than to underwrite them on a long-term basis. Federal program grants of funds will be made on a matching basis only. They will require new money to qualify for Federal funds. Furthermore, the Congress did not intend the Federal funds to be used for continuous maintenance of facilities for air pollution control. On the contrary, the Federal money specifically is to be used to aid in initiating programs.

You meteorologists are concerned with the air, and our objective, like that of the regulations discussed by Mr. Newell, is to keep the air as clean as we can; we will be working together on this. The Clean Air Act gives favored financial support to fairly wide interjurisdictional air pollution control programs. Regional programs can receive \$3 for every \$1 of state and local funds, compared with \$2 for every \$1 available for strictly local purposes. Big masses of air know no political boundaries, and it is desirable to control large portions of the emissions from the ground.

The Clean Air Act also includes for the first time limited legal regulatory authority on the Federal level for the abatement of the specific problem of air pollution. The AEC was given strong legal regulatory authority from the beginning because radiation is a specific human health hazard that people are afraid of, and this authority was generally supported. However, the growing problem of day to day tons of ordinary air pollution

is only now apparently reaching such levels that it is felt necessary that Congress act. It is clearly intended that the Federal authority supplement and not supplant abatement powers already held by the state and local governments in two kinds of situations, interstate and intrastate. The Secretary of HEW may, under the Clean Air Act, on his own initiative or, as specified, on official request, initiate formal proceedings for the abatement of air pollution, as found necessary in interstate problems. In intrastate air pollution problems, the Secretary may invoke formal abatement proceedings only upon official requests from designated officials in the states involved.

The regulatory abatement procedures authorized are similar to those that have been used for several years under the provisions of the Water Pollution Control Act, and they involve the steps of conferences of cognizant official agencies, public hearings, and court action, finally by the Attorney General, if necessary. It is profoundly hoped that the first two procedures will be sufficient.

Other provisions of the Clean Air Act reflect new and evolving Federal policy, that is, the Secretary of HEW is directed to develop, promulgate, and publicize the standards for air quality, as you have already done, to guide in establishing local standards for source emissions and for the ambient air.

The Secretary is directed to form a technical committee on motor vehicle pollution with representatives of HEW, the auto industry, and manufacturers of pollution control devices and motor fuels. This committee, now in the process of formation, is charged with the responsibility for reviewing progress towards control of motor vehicle pollution and for indicating special areas of needed research. The Secretary of HEW is required to report periodically to Congress on this aspect and to recommend new legislation he deems necessary or warranted. This committee has 1 year to come up with some answers before the first report to Congress, and then the Secretary is required to report every 6 months. This means that Congress itself has initiated a process of almost continuous review of the motor vehicle pollution problem. About 40% of our over-all air pollution load is said to be caused by motor vehicles; the problem is how to cope with it.

We in the public health program must look to you meteorologists for ways – theoretical, or what-

ever is necessary, or whatever you can come up with – to help us collectively cope with the growing load of air pollution logically associated with a rising social and economic standard and an ever climbing population increase. It is obvious that we have to apply thought, ingenuity, and money to avoid becoming overwhelmed with these air pollutions, which are reaching levels in some areas such that they are definitely adversely affecting the health and certainly the welfare of the people.

The Clean Air Act, in Section 7(a), reiterates the intent of Congress that all Federal departments and agencies and installations shall cooperate with HEW and with any air pollution control agencies in preventing and controlling pollution of the air insofar as the discharge of any matter from such building, installation, or other property may cause or contribute to pollution of the air in such area. This is not new; it has been the law since 1956, and before that it was an executive order by the President. It has been endorsed by the last three Presidents, and it has been very effective in getting abatement when it was brought to the attention of the administrators of Government agencies on the basis of complaints. However, Congress this time went a step further by adding Section (b) which authorizes the Secretary of HEW to establish classes of potential pollution sources for which any Federal department or agency must obtain a permit (see footnote, above), such permits to be used for specified terms, and so forth. In connection with the issuance of such permits, there shall be submitted to the Secretary such plans, specifications, and other information as he deems relevant and under such conditions as he may prescribe. Also, the Secretary shall report each January to the Congress the status of such permits and compliance therewith.

Thus, in two instances Congress does intend to ride herd on this Air Pollution Control Act, and the committees in both the House and the Senate have made it clear that they intend to keep a close watch on how we do to see whether additional legislation will be needed.

Section 7(b), which out of context seems rather severe, has not yet been implemented. We are working out the details for that. It is now contemplated (and this is not for general publication) that the first attack will be on the disposal of wastes by the method of open burning, which contributes heavily to the air pollution load of the

nation in many places. It is outlawed in many states, but still it goes on. The ever mounting waste material must either be burned properly in well designed incinerators or buried or disposed of some other way than in an open-burning dump. Obviously at certain times, in certain situations open fires are necessary. To control fire hazards, back fires must be set, and open fires are needed to train firemen. But the elimination of burning waste materials is the most effective place for implementation to start. This would have no bearing on the AEC's regulatory function, but AEC installations would certainly want to take steps to avoid complaints arising from open fires.

Since passage of the Act, it has already been announced that extensive field hearings have been held by the Special Subcommittee on Air and Water Pollution of the Senate Committee on Public Works, headed by Senator Muskie. He has announced that he will hold some more hearings of a technical nature. It is impossible to predict what future action will be taken in the direction of air pollution control, but there is no doubt that Congress as well as the Executive branch are firmly committed to a program that will lead to significant improvement in the national effort. There have been a number of actions by the Executive branch. The Division of Air Pollution within the Public Health Service is being reconstituted to carry out the provisions of the Act. The Executive branch, through the Bureau of the Budget, is now working on a new set of directions that will go to

every Federal agency for the preparation of budget estimates for 1966. This will instruct them to include, in all estimates for new construction, provisions for the control of water and air pollution. Sooner or later probably you meteorologists will be called upon for consultation in that area, as well as the Public Health Service.

The Federal Power Commission, which is concerned with the allocation of fuels, has given considerable attention to air pollution, particularly with respect to the allocation of natural gas in areas of tight population density; and the Office of Science and Technology is responsible for establishing a clearinghouse for technical information concerning air pollution.

All this has happened since the bill passed. The Federal Government is committed to a policy of decisive action. Its intent is to bring about a substantial improvement in the ability to maintain a satisfactory quality of air in our country. And should the policy prove inadequate, it seems clear that the Federal Government will evolve new policies through executive agencies, programs, and executive rulings. In summary, as a nation we have entered a new phase in the struggle to maintain our society and its atmosphere. We hope for rapid progress toward more control of air pollution. If passage of the Clean Air Act is but an intermediate step, it does mark a change in affairs, and we are prepared to take the next step, for air pollution is an environmental problem that can no longer be sloughed off with halfway measures.

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