On Atmospheric Simulation: A Colloquium

G. M. HIDY

November 1966
The National Center for Atmospheric Research (NCAR) is dedicated to the advancement of the atmospheric sciences for the benefit of mankind. It is operated by the University Corporation for Atmospheric Research (UCAR), a private, university-controlled, non-profit organization, and is sponsored and principally funded by the National Science Foundation.

NCAR shares with other atmospheric research groups four interrelated, long-range objectives that provide justification for major expenditures of public and private funds:

- To ascertain the feasibility of controlling weather and climate, to develop the techniques for control, and to bring about the beneficial application of this knowledge;
- To bring about improved description and prediction of astrophysical influences on the atmosphere and the space environment of our planet;
- To bring about improved description and prediction of atmospheric processes and the forecasting of weather and climate;
- To improve our understanding of the sources of air contamination and to bring about the application of better practices of air conservation.

The research and facilities operations of NCAR are conducted in four organizational entities:

The Laboratory of Atmospheric Sciences
The High Altitude Observatory
The Facilities Laboratory
The Advanced Study Program

All visiting scientist programs and joint-use facilities of NCAR are available to scientists from UCAR-member and non-member institutions (including private and government laboratories in the United States and abroad) on an equal basis. The member universities of UCAR are:

University of Alaska  Florida State University  University of Oklahoma
University of Arizona  University of Hawaii  Pennsylvania State University
University of California  The Johns Hopkins University  Saint Louis University
University of Chicago  University of Maryland  Texas A & M University
Colorado State University  Massachusetts Institute of Technology  University of Texas
University of Colorado  University of Michigan  University of Utah
Cornell University  University of Minnesota  University of Washington
University of Denver  New York University  University of Wisconsin
On Atmospheric Simulation: A Colloquium

November 1966

G. M. HIDY

November 1966

atmospheric simulation
atmospheric processes
NCAR
colloquium

NATIONAL CENTER FOR ATMOSPHERIC RESEARCH
Boulder, Colorado
FOREWORD

Some years ago, the original projections for the National Center for Atmospheric Research (NCAR) included plans for several facilities to simulate atmospheric processes. Among the planned facilities were cloud chambers and a large low-speed wind tunnel. Many questions and controversial opinions arose about the value of simulation, and as a result elaborate facilities of this kind have not yet been constructed. After the early planning for NCAR's activities it became evident to the directors of NCAR that it would not be possible to make a quick and simple decision on the national needs, if any, for simulation equipment. The requirements for such service facilities as balloon launch sites, instrumented airplanes, and a large computer were easier to define and document. However, the problems associated with the prospects of laboratory simulation are so diffuse that there is great difficulty in determining how a general, multi-purpose facility or a specific simulation laboratory should be effectively designed and built.

The responsibility for generating new national facilities customarily has been assigned to groups of scientists, or has originated with the ingenuity and persuasiveness of an individual. Either method imposes certain inherent limitations when one faces the problems of clearly defining the requirements for a general purpose simulation facility. In the case of a panel or a committee, a judgment on new efforts must be passed during meetings over a few days. Then there is a danger that decisions will be based on somewhat superficial information, or perhaps will tend to rubber stamp new ideas in too positive a way. Reliance on the opinion of one person or a few investigators, on the other hand, places too much emphasis on the judgment of even well qualified individuals. The potential of a group for coping with all facets of complicated questions involved in large facilities or
programs is greater than that of an individual. However, the biggest problem in committee studies centers upon the lack of time to make a thorough survey of the problem in question.

We felt, at NCAR, that it should be possible for an individual, given a long enough period of time, to make a definitive study of relevant problems in a proposed program, especially if he is systematically and well advised by other workers who are familiar with various aspects of the program. The directors of NCAR decided to try this method to seek the answers to questions bearing on the overall university community, or broader national needs for major programs in laboratory simulation of atmospheric phenomena. In January 1965 I was asked to organize this study. After conversations with the directors of NCAR, we decided to set up a Colloquium on Atmospheric Simulation under the auspices of the NCAR Advanced Study Program. A number of scientists from various institutions were invited to NCAR to discuss aspects of laboratory experiments on atmospheric phenomena. The visitors gave several formal seminars on different subjects. Their lectures were supplemented by informal discussions and other seminars by members of the NCAR staff.

Over a year was devoted to surveying laboratory experimentation on atmospheric processes with the aid of the results of the Simulation Colloquium. Where possible, topics were chosen to be investigated in depth by calling on at least two workers having differing opinions on, or different approaches to, the same class of experiments. The Colloquium was designed mainly to place emphasis on the following points:

a) interesting results from laboratory work directly related to atmospheric phenomena,
b) the possibilities for, and potential importance of, future studies in the laboratory,
c) the needs for laboratory equipment or facilities which would exceed those capabilities of any particular research group or institution, and
d) projections for the possible location, design, and staffing of a project for laboratory simulation.

This report presents the results of the survey in the light of these four main points.

It is a pleasure to acknowledge the aid of so many of my colleagues in this venture. Without their contributions a comprehensive survey of this kind would not have been possible. Special thanks go to Drs. P. Squires and J. Telford for their critical remarks about the preliminary draft of this report. The helpful discussions with the directors of NCAR, especially P. D. Thompson and W. O. Roberts, are gratefully acknowledged.

The visiting participants of the Colloquium on Atmospheric Simulation included:

Prof. W. Bennett
Prof. J. Cermak
Dr. H. F. Eden
Prof. A. Faller
Dr. P. Frenzen
Prof. D. Fultz
Prof. R. Hide
Prof. R. List
Prof. R. Long
Prof. E. J. Plate
Prof. G. Strom
Prof. R. W. Truitt
Dr. J. S. Turner
Dr. B. Vonnegut

North Carolina State University
Colorado State University
A. D. Little, Inc.
University of Maryland
Argonne National Laboratory
University of Chicago
Massachusetts Institute of Technology
University of Toronto
The Johns Hopkins University
Colorado State University
New York University
North Carolina State University
Woods Hole Oceanographic Institution
A. D. Little, Inc.

The NCAR participants of the Colloquium included:

Dr. R. Bushnell
Dr. R. D. Cadle
Dr. J. W. Deardorff
Dr. W. Jones
Mr. G. Langer

Dr. D. K. Lilly
Dr. Y. Nakagawa
Mr. C. Palmer
Mr. J. D. Sartor
Dr. P. Squires
I am indebted to Mrs. Mary Allen, and Mrs. Jane Stroh for their efforts in searching the literature. The extensive work of Mrs. Stroh on the many drafts of this report is also greatly appreciated.

G. M. Hidy, Chairman
Colloquium on Atmospheric Simulation

November 1966
## CONTENTS

**FOREWORD** .................................. iii

**ON ATMOSPHERIC SIMULATION: A Colloquium Report** 1

I. **INTRODUCTION AND RECOMMENDATIONS** ............. 3

II. **BASIC CONSIDERATIONS IN SIMULATING ATMOSPHERIC PROCESSES**. 5

   Classifying Techniques for Simulation ............... 9

III. **ATMOSPHERIC SIMULATION: PROGRESS AND PROSPECTS** .......... 13

   Molecular and Near-Molecular Scale Phenomena ........ 13
   Atmospheric Chemistry .................................. 13
   Cloud Physics and Chemistry ........................... 15
   Plans and Speculations for New Cloud Chambers ....... 20

   Microscale Processes ................................ 22
   Mechanics of Turbulent Boundary Layers .............. 22

   Mesoscale Phenomena ................................ 28
   Wind Tunnel Modeling ................................ 28
   Penetrative Convection ............................... 30
   Buoyant Convection and Concentrated Vortices ...... 32
   Boundary Layers in Rotating Fluids .................. 34

   Continental and Planetary Scale Phenomena .......... 35
   Flow Patterns like the Planetary Circulation ...... 36
   Basic Experiments in Rotating Fluids ............... 37
   Hydromagnetic Analogies ............................... 39

IV. **ON THE QUESTION OF A NATIONAL PROGRAM IN SIMULATION** .... 41

   An Aircraft Instrumentation Test Chamber ........... 42
   A Research Program for Laboratory Cloud Physics ... 45
   A Program for Studying the Aerodynamics over Mountainous Terrain ....... 50
   Data Processing Equipment ........................... 53
APPENDIXES

A. LECTURES AND DISCUSSIONS OF THE PARTICIPANTS IN THE COLLOQUIUM.

1. W. H. Bennett: Charged Particle Streams in the Earth's Magnetic Field.

2. R. D. Cadle: Indirect Simulation in Atmospheric Chemistry.


5. H. F. Eden: Two Laboratory Experiments with Convection; Their Relationship with Numerical and Field Experiments.


13. Y. Nakagawa: Laboratory Simulation of Magneto-
hydrodynamic Phenomena .......................... 165

14. E. J. Plate: Modeling of Flow in a Disturbed
Boundary Layer ........................................ 173

15. J. D. Sartor: The Simulated Behavior of Cloud
Particles .................................................. 181


17. G. H. Strom: Simulating Atmospheric Processes in
a Wind Tunnel ........................................... 193

18. R. W. Truitt: Quasi-One-Dimensional Flow of an
r-Times Ionized Monatomic Gas ..................... 205

Systems .................................................. 209

20. B. Vonnegut: Some Simulation Experiments Pri-
marily in Atmospheric Electricity ................... 219

References .................................................. 229

B. DISCUSSION OF A TEST CHAMBER FOR AIRCRAFT INSTRUMENTATION 239

C. BIBLIOGRAPHY ............................................. 247

  Environmental Chambers ............................. 249
  Cloud Chamber Experiments .......................... 250
  Fluid Dynamics Analogues ............................. 256
  Laboratory and Modeling Experiments ............... 261
  Wind Tunnel Studies .................................. 263
ON ATMOSPHERIC SIMULATION:

A COLLOQUIUM REPORT
I. INTRODUCTION AND RECOMMENDATIONS

The laboratory simulation of the atmosphere and its phenomena is an old and established method for extending the horizons of understanding of the science and technology of the earth and its environment. In a traditional sense, simulating devices perhaps are most commonly known for their extensive use as tools to aid in designing and testing vehicles or equipment that travel through the atmosphere. However, considerable efforts also have been made to build in the laboratory realistic and useful analogies to atmospheric processes. Such experiments have led to a variety of new conceptual theories for explaining the atmosphere's behavior from microscale phenomena to processes on a planetary scale. These studies are quite distinct from the aeronautical problems solved by environmental testing.

Both classes of simulation experiments will always play a key part in technological developments. The services provided by environmental testing provide essential information for improving our ability to measure and to control the effects of the atmosphere on equipment. The use of controlled and reproducible laboratory experiments gives the atmospheric scientist great hopes for better understanding the peculiarities of natural phenomena. Indeed, the primary feature that laboratory studies have of reducing complicated processes to their barest essentials gives new challenges to both the novice and the experienced simulator.

The general principles of construction and use of equipment for environmental simulation are well known, and are well documented. However, the many ideas for conceiving basic experiments bearing on atmospheric phenomena are somewhat more subtle, and are not so well understood. Consequently the bulk of the discussions in this Colloquium concentrate on the development of laboratory experimentation as a direct basic, scientific tool, rather than as a procedure for
environmental simulation. Despite this emphasis, one of the major new projects to be recommended in this study falls into the latter category, and involves the construction of a new device for evaluating instrumentation for meteorological research vehicles.

From the sampling of various aspects of simulation surveyed, it is evident that the use of laboratory experimentation is continuing to expand its role in the study of the atmosphere. Regardless of the success or failure of particular experiments in clarifying the mechanisms involved in natural processes, the net yield over many years of laboratory study, integrated with new field observations and theoretical analyses, will prove to be very valuable to meteorology and geophysics. One should expect, however, that laboratory investigations will find their continuing success in contributing to advancing basic knowledge rather than to specific applications in modeling particular atmospheric processes.

The results of this Colloquium have suggested several avenues for new work in atmospheric simulation. In addition, the study has clarified much of the haziness surrounding certain questions about the construction of new facilities. The findings of this investigation indicate that:

A. There appears to be no pressing need at this time for a multipurpose facility for simulation of atmospheric processes. Since various techniques and devices for simulation are so closely tied to the special and unique ideas of individual scientists, a generalized facility concept will not be flexible enough to meet the optimum requirements of any given series of experiments. Major efforts in laboratory research related to atmospheric phenomena should proceed from the beginning through interested scientific staff as a step-by-step sequence of planned experiments leading to definite goals. Because of their potential value to the atmospheric sciences, such evolutionary programs should be encouraged and given strong support on an equal basis to ventures using numerical analysis alone, or relying on well conceived observational programs in the field.
B. Certain specific projects for atmospheric simulation stand out as particularly important possibilities for long-term ventures which possibly cannot be undertaken by individuals or very small groups with limited resources. In order of priority, according to my opinion, these are:

1. A special cloud chamber for calibrating and testing instrumentation to be used on meteorological research aircraft. To aid in establishing uniform and accurate standards for instrumentation of meteorological research aircraft, more reliable methods of laboratory testing such instruments are badly needed for conditions of both dry air and cloudy air. Therefore, the development of such a device should be undertaken as soon as possible at NCAR to support its own planned research program using aircraft observations, as well as those of other flight facilities.

2. A research program for laboratory cloud physics. This project should be centered around definite experiments to explore: (a) questions related to the influences of heterogeneities in gas properties, of electrostatic perturbations, and of turbulent motion in air-water vapor mixtures on the growth of collections of particles, and (b) the contribution or role of the ice phase (and ice nuclei) to the stability of clouds of growing water droplets. This particular series of investigations should be initiated at NCAR with the close co-operation for example, of ESSA's research groups. Such a study should help to provide criteria for truly large-scale ventures in laboratory cloud physics by step-by-step development through planned experimentation in relatively small, inexpensive vessels.

3. A program to study the aerodynamics over mountainous terrain. The theory of stratified flow over barriers is relatively well understood. Therefore, this project seems ideal for testing the feasibility of extensive uses of modeling combined with field observations and studies by computers as a tool to develop improved forecasting techniques for mesoscale processes. For convenience, this program should be located near the eastern side of a mountain range.
Because of the advantages of the complex of institutions near Boulder (ESSA, NCAR, Colorado State University (CSU), and the University of Colorado) it seems natural that this study would be undertaken in this geographical area, possibly under the direction of ESSA's atmospheric physics group.

4. **Flexible, high speed data processing equipment.** These units should be designed to satisfy the needs of laboratory research, as well as observational efforts in the field. This project should begin with a careful feasibility study initiated by NCAR's Facilities division.

5. **Expanded programs of basic research in geophysical fluid dynamics, and plasma physics.** Two dynamics problems of a long-term nature perhaps should be undertaken by national laboratories such as NCAR or ESSA. These are: (a) an extensive study of the development of front-like discontinuities in density in rotating stratified fluids, and (b) a comparative study based on numerical integrations and laboratory experiments of the flow in the irregular wave regime of a stratified fluid placed in a rotating annulus or pan. A variety of experimental studies largely should evolve out of existing efforts in universities and national laboratories. They should be based on the needs of definite experimental programs initiated by interested and qualified workers.

Because of the complexities and expense of equipment needed for plasma research, efforts perhaps should be made to support the recent NAS ideas for organizing a new national laboratory program for fundamental research in the dynamics of ionized gases.

6. **A new wind tunnel program to investigate the dynamics of turbulent shearing flow in stratified media.** The results of a number of investigations of turbulent flow in wind tunnels has demonstrated the value of such experiments in better understanding small-scale processes in the atmosphere (see also recommendation 3). Continued research in this field will be fruitful and should be undertaken in the future. To augment present facilities, which already
are working nearly at full capacity, new meteorological wind tunnels will eventually be needed. The decisions for the requirements of the design of new equipment should be based on specific experimental programs such as the ones indicated in recommendation 3, and therefore should await the results of a new feasibility study of wind tunnel research just begun by workers at the Argonne National Laboratory.

II. BASIC CONSIDERATIONS IN SIMULATING ATMOSPHERIC PROCESSES

In studying the mysterious workings of the atmosphere, scientists have often used laboratory devices to test and calibrate their observational instruments. They also have dreamed of solving some problems through basic experiments on a laboratory scale. The use of test chambers for evaluating the performance of devices involves the application of well known principles, and represents a rather conventional approach to the general idea of simulation. However, the laboratory investigation of fundamental processes related to the behavior of the atmosphere implies much more subtle and less well understood considerations of atmospheric simulation. Although it is easy to distinguish between the two general modes of simulation, it sometimes is necessary to mold the two methods together to treat new programs in a comprehensive way. This study deals in the main with simulation in the sense of basic laboratory investigations. However, the requirements of certain aspects of environmental testing also will come from these discussions.

Because of the complexities in theoretical models of atmospheric processes, and the difficulties in interpreting unregulated natural phenomena, many investigators have been attracted to laboratory simulation under controlled conditions as a possible source of supplementary conceptual ideas. Reports of liquid-phase analogies to tornado-

* An old but comprehensive account of methods of designing wind tunnels is described by Durand (1935). A summary of existing environmental chambers is provided in the supplemental bibliography in Appendix C.
like vortices have appeared as early as 1780 (Wilcke, 1780). Perhaps better known are F. Vettin's mid-nineteenth century experiments which attempted to duplicate the atmosphere's thermally driven circulation (Fultz, 1951). After Vettin's work, enthusiasm for laboratory analogies waned somewhat. However, by the early twentieth century, the far-reaching success of devices for simulation of hydraulic and aerodynamic processes gave atmospheric scientists renewed interest in laboratory study of geophysical questions. Indeed, in the last twenty years the application of small-scale experimentation has yielded a wealth of new ideas about possible mechanisms involved in atmospheric phenomena.

The simulation of atmospheric processes using small models has proved to be an exceedingly rewarding but sometimes frustrating means of studying behavior related to planetary atmospheres. Although it is quite feasible either to reproduce or to model, in the strictest sense, phenomena on a microscale (for example, Bridgman, 1931), it is very difficult to build quantitative experiments directly related to medium-scale and large-scale phenomena in the atmosphere. Simulation, or more precisely modeling, on these scales naturally must involve the application of the principles of dynamic similarity. Even though partial similarity may be demonstrated between the laboratory experiment and an atmospheric process, the fact that the two systems behave in similar ways may be fortuitous. In many cases one finds, for example, that fluid motion in a laboratory model depends strongly on microscale influences involving viscous effects and surface tension, while "similar" atmospheric motion is dominated by entirely different controlling influences acting over a wide range in scale.

Despite the inherent pitfalls of laboratory simulation of interacting phenomena which occur over many orders of magnitude in distance and time, the advantages of controlled experimentation still lead geophysicists back to the laboratory. It might be said that the enthusiastic ideas generated by an atmospheric analogy provide sufficient justification for such activity to override the drawbacks.
The recent successes in certain kinds of atmospheric simulation have led some to believe that much larger, systematic, and longer term efforts in this area should be considered. In this Colloquium we have attempted to survey some of the experience with a wide variety of basic experiments that have bearing on atmospheric processes.* We have tried to determine some of the drawbacks in this type of work, and we have looked for some particularly fruitful areas for further efforts. Some suggestions are made for a new series of major laboratory studies using sophisticated equipment and extending over long periods of time. These programs may be useful for a research organization with national responsibilities like NCAR's to consider in its planning for the future.

CLASSIFYING TECHNIQUES FOR SIMULATION

Investigations that can be applied in a very general way to simulating atmospheric phenomena may be separated arbitrarily into laboratory experiments and numerical experiments on large computers. Numerical studies of the behavior of the mathematical equations governing atmospheric motion have proved to be effective as simulation techniques. However, such studies will not be considered in this survey unless they pertain to laboratory experimentation.

In the wide variety of laboratory work that has application to the atmospheric processes, one may conceivably distinguish between indirect and direct classes of investigation. Almost any fundamental study in physics or chemistry eventually may find indirect application to the atmosphere. However, in the act of "simulation" one usually defines an "indirect" experiment as one that at least involves an attempt to seek fundamental information applicable to a particular geophysical process, without actually trying to reproduce the process. Often in such cases, extrapolation of the laboratory data to conditions in the atmosphere requires careful use of well known basic

* Lectures and discussions of participants are given in Appendix A, arranged alphabetically and sequentially numbered. Text references to these contributions, therefore, appear, for example, as A-6, A-12, etc.
theoretical ideas. Extrapolation is involved, for example, in obtaining thermodynamic data for components of air, and in estimating rates of certain chemical reactions of trace gases in the atmosphere.

Direct simulation, on the other hand, normally implies an effort on the part of the investigator either to reproduce exactly atmospheric conditions, or to "model" atmospheric processes. Reproduction necessarily involves only microscale phenomena while modeling has far wider applicability in its use for constructing "scaled" analogies by application of the principles of dynamic similarity.

The concepts of similarity have deep roots in theoretical physics. Similarity in behavior of systems stems from certain principles of invariance in laws of nature, leading, for example, to notions like dimensional homogeneity. A recent article of Houtappel, et al. (1965) reviews the newer abstract ideas on invariance principles in theoretical physics. The indirect application of these underlying notions, and of group theory to dynamic systems has resulted in the general theory of dynamic similarity (for example, Birkhoff, 1950). Corrsin (1953) has discussed the role of similarity and modeling in geophysical fluid dynamics.

In general, the construction of models of dynamic phenomena requires similarity in geometry, and equality in certain dimensionless numbers characterizing the system. For modeling atmospheric motion, one seeks laboratory analogies in which as many as possible of the following sample groups are the same.*

*A more complete outline of dimensional analysis of dynamic equations applied to the atmosphere is given, for example, by Fultz (1961), by Cermak, et al. (1966), by Long (A-12), and by Hide (A-9).
### Dimensionless Parameter

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Characteristic Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>1. Aerodynamics:</strong></td>
<td></td>
</tr>
<tr>
<td>Reynolds Number</td>
<td>( UL/\nu )</td>
</tr>
<tr>
<td>Froude Number</td>
<td>( U^2/gL(\Delta \rho/\rho) )</td>
</tr>
<tr>
<td>Richardson Number</td>
<td>( g(\Delta \rho/\rho)/L(\partial U/\partial z)^2 )</td>
</tr>
<tr>
<td>Rossby Number</td>
<td>( U/\Omega L )</td>
</tr>
<tr>
<td>Strouhal Number</td>
<td>( U/fL )</td>
</tr>
<tr>
<td>Rayleigh Number</td>
<td>( gL^3(\Delta \rho/\rho)/\alpha \nu )</td>
</tr>
<tr>
<td><strong>2. Transport Processes:</strong></td>
<td></td>
</tr>
<tr>
<td>Prandtl Number</td>
<td>( \nu/\alpha )</td>
</tr>
<tr>
<td>Schmidt Number</td>
<td>( \nu/D )</td>
</tr>
</tbody>
</table>

In atmospheric motion the Reynolds number is always very large, and consequently the flow is turbulent. The Froude number is important in dealing with gravity waves in the atmosphere, and in studying the relative stability of stratified media. The Rossby number serves as a criterion for the importance of the effects of the earth's rotation on large-scale flow of air. In unsteady flow, the Strouhal number gives an indication of the importance of the two components of local acceleration of a fluid element.

When considering the dispersion of heat or matter in the atmosphere, one uses the diffusional parameters such as the Prandtl number or the Schmidt number. In situations where one attempts to model the

* In the groups, \( U \) is the characteristic velocity of the fluid, \( L \) the characteristic length, \( \nu \) the kinematic viscosity, \( g \) the gravitational acceleration, \( \rho \) the mass density, \( \Delta \rho \) the vertical change in density between layers of air, \( \Omega \) the earth's rotation rate, \( \alpha \) the thermal diffusivity, \( D \) the mass diffusivity, \( z \) the vertical coordinate, and \( f \) a characteristic frequency.
behavior of hydrometeors, other parameters have to be considered (List, A-11).

Atmospheric phenomena occur over extremely wide ranges of scale of distance and time. Therefore, except in certain limited cases, complete dynamic models of atmospheric processes cannot be devised. One then looks for examples where partial similarity exists in certain key parameters believed to control the behavior of the system. The trick to building "successful" laboratory analogies is to delineate the key parameters. Some of the "models" are found by accident, and others develop rationally, for example, from the theory of fluid dynamics.

Whether or not laboratory experiments reproduce or exactly model atmospheric conditions, controlled basic studies that are conceptually related to geophysical processes have consistently proved their value through the years as a valued research tool for earth scientists.

For the purposes of this study it seems convenient to examine many different laboratory experiments within a framework of ranges in scale of geophysical length. Already we have subdivided work rather naturally into microscale and larger-scale phenomena. Let us then further divide this survey arbitrarily into the following groups:

<table>
<thead>
<tr>
<th>Group</th>
<th>Scale (km)</th>
<th>Key Phenomenology</th>
</tr>
</thead>
<tbody>
<tr>
<td>A. Molecular, or near-molecular scale</td>
<td>$\leq 10^{-6}$</td>
<td>Molecular phenomena, aero-colloidal particle-gas interaction involved.</td>
</tr>
<tr>
<td>B. Microscale</td>
<td>$10^{-6} - 10^{-1}$</td>
<td>Turbulence, rotating system unimportant, role of stratification.</td>
</tr>
<tr>
<td>C. Mesoscale</td>
<td>$10^{-1} - 100$</td>
<td>Rotating system becomes important; thermal convection, turbulent shearing, flow, compressibility, existence of concentrated vortices, and discontinuities between masses of air involved.</td>
</tr>
<tr>
<td>D. Continental or planetary scale</td>
<td>$&gt; 100$</td>
<td>Effects of rotating system and thermally driven quasi-horizontal circulation dominant in dynamics.</td>
</tr>
</tbody>
</table>
Within this framework a wide variety of laboratory experiments can be classified and discussed as a part of the overall picture of atmospheric phenomena. The lectures and discussions, summarized in Appendix A, cover a multitude of studies in these four groups. To illustrate how laboratory experiments have progressed, a liberal sample of various methods has been taken, and they are described briefly below.

III. ATMOSPHERIC SIMULATION: PROGRESS AND PROSPECTS

MOLECULAR AND NEAR-MOLECULAR SCALE PHENOMENA

Atmospheric Chemistry

The chemistry of traces of reactive gases in the atmosphere is important to several problems, ranging from air pollution to the heat balance of the atmosphere. Not only is there concern for the behavior of the reactive materials in the atmosphere, but also there is regard for the consequences of systematic increases in relatively non-reactive components like carbon dioxide. Because all the numerous traces have very low concentrations in the atmosphere, it is exceedingly difficult to study their properties and their behavior under natural conditions. Much of the laboratory work of chemists has been concerned with developing analytical devices for measuring minute quantities of organic and inorganic materials in air. Having determined what some of these reactive gases are, it has been possible to begin systematic laboratory investigations to determine if specific chemical reactions involving known components of the atmosphere control the formation and decomposition of important gases like ozone and sulfur dioxide.

Nearly all of the studies of chemical reactions related to atmospheric processes necessarily fall into the category of indirect simulation. Cadle (A-2) has cited a number of examples of this kind of research, particularly with reference to determining rate constants of reactions of different species. The papers given in the 1961 International Symposium on Chemical Reactions in the Atmosphere (Stanford Research Institute, 1961) provide other illustrations of this type of laboratory work.
Although considerable information has evolved from various laboratory studies of chemical reactions, difficulties sometimes arise in interpreting data as they apply to the atmosphere. By nature, reactions of atmospheric gases are primarily homogeneous in the gas phase. In mechanism they are largely photochemical, and many of the reactions involve species having excited non-equilibrium states. Besides taking into account non-Maxwellian behavior of molecules (a formidable theoretical task) the experimenter must be careful of spurious effects of surfaces in laboratory equipment.

Most of the experiments related to atmospheric chemistry are best carried out in small devices or vessels made of glass. Many schemes have been worked out to try to avoid surface catalysis spoiling reactions in small systems. However, these are not always successful in fulfilling their purpose. One way to see if surface catalysis is involved consists of decreasing markedly the surface to volume ratio of the reactor. To conveniently do this, it might be desirable to have a large chamber, say 1000 m$^3$, which has temperature and pressure control, and possibly radiation sources.

Another facet of atmospheric chemistry involves the study of the role of aerosols in chemical reactions. Although the surface catalytic effect of small particles suspended in the air is believed to be small, chemical reactions directly with aerosols may be important, say in the formation of gaseous chlorides from sea salt. Studies of aerosol chemistry also can be undertaken in small containers. However, a large chamber would be useful for these investigations, especially if one wants to keep particles in the reacting gas for relatively long periods of time.

There are a number of other aspects of aerosol behavior which are of interest to atmospheric science. For example, some efforts have been made to study the rate of agglomeration of aerosols in the laboratory and their relation to the structure of the size distribution of atmospheric aerosols. The role of trace gases has also been examined in the light of the mechanism of production of aerosol clouds by condensation.
With better ideas of the part played by tiny particles in radiative energy transfer, more interest has been generated recently in laboratory study of the scattering of incident electromagnetic radiation by dilute aerosol clouds. To undertake such experiments, the Russians apparently have constructed a chamber for aerosol scattering studies over 50 m long at the Institute for Applied Geophysics at Obninsk, USSR.

Cloud Physics and Chemistry

Since the pioneering work of groups headed by Findeisen and Langmuir during World War II and before, laboratory analogies to various aspects of cloud particle behavior have played an important role in cloud physics. Recent texts on this subject by Mason (1957) and Fletcher (1962) provide comprehensive case studies of indirect and direct attempts to investigate in the laboratory the microphysical processes of cloud formation and development. These books amply demonstrate how laboratory work has provided successfully numerous clues to possible mechanisms of droplet and ice crystal generation and growth in natural clouds.

Despite the years of work on problems of cloud physics, a large number of questions about the behavior of droplets and ice particles remain unanswered. Left unsaid in most books are the dreams of simulation turned to nightmares when the realization dawns that the undependable behavior of cloud particles even in "controlled" laboratory experiments can lead to contradictory, non-reproducible results, depending on the kinds of devices and instrumentation used. Recently there has been a tendency to return to basic experiments to improve existing analytical devices as well as to gain a better picture of the general behavior of small particles suspended in a gas.

In discussing the physics and chemistry of clouds, workers generally attempt to reproduce in one way or another the environmental conditions applicable to the atmosphere. The principles of similarity have not been used to a great extent in studying hydrometeors, or aerosols, except for single-particle behavior or two-body interactions
(e.g., List, A-11, and Sartor, A-15). The applications of these principles to clouds of particles is not at all straightforward. In fact, virtually no theoretical investigations have been reported which discuss dynamic similarity for dispersed systems. To aid in establishing the necessary criteria for new studies in cloud chambers, such theoretical analyses will be crucially important, and they should be started in connection with new laboratory experiments.

In simulating in the laboratory the chains of events leading to precipitation, the formation and growth of particles to about 10 to 20 μ diameter often have been separated from the growth to larger sizes. Based on the present knowledge of particle growth, this division seems to be a natural one. Condensation from supersaturated vapor tends to play a dominant role in growth of particles below about 10 μ in diameter, while collision of particles of different size resulting in coalescence appears to control growth of particles larger than about 10 μ.

Since the early work of Aitken and Wilson, many attempts have been made to simulate the initial and late stages of particle development in natural clouds, in cloud chambers or other devices of various sizes. Some examples of this class of work are described by Langer (A-10), and recently by Podzimek (1964).

The whole question of the observed polydispersity of cloud droplet spectra, compared with predicted monodisperse distributions based on condensation theory, is an open one. Coagulation alone cannot be responsible for the polydispersity because this mechanism does not act rapidly enough for natural clouds of water droplets. There has been some speculation that the droplet size distribution is broadened by the effect of temperature and concentration gradients in supersaturated vapor, or by turbulence in the suspending gas. New experiments in cloud chambers or columns may verify this hypothesis.

Droplet concentrations in clouds vary over 2 to 3 orders of magnitude, while updrafts change by factors of 2 to 3. Since the number of droplets depends roughly on the fourth root of the updraft speed,
these ranges indicate that natural aerosols must play a key role in the nucleation process (Squires, A-16). The size spectrum and the solubility of natural aerosols are natural parameters to associate with observed differences in nucleation rate. However, the exact details of the link between the formation of cloud particles and the physicochemical properties of the natural aerosols are not well understood. Squires (A-16) has described the importance of laboratory work in connection with careful field observations in resolving many of the features of aerosol behavior and the process of nucleation of water vapor in nature.

After cloud particles start to grow by condensation, they interact with the suspending gas by transferring heat, mass and momentum, and they interact with each other by collisions. List (A-11) has begun extensive efforts to study the aerodynamics of hydrometeors in a wind tunnel at Davos, Switzerland. Comparison of his laboratory results with the structures of natural hydrometeors has led to better understanding of the formation of hailstones in clouds. List has continued his work in this area, and has begun new experiments to examine the relation between properties of hydrometeors and mechanisms of interaction between the suspending gas and other particles. Other workers also have taken this route towards attempting to understand the behavior of relatively large water particles. For example, Neiburger and his colleagues at UCLA have designed a new cloud column for hydrometeor investigations.

Attempts have been made to construct hydrodynamic analogies of particle collision processes. These experiments have provided helpful information about the applicability of aerodynamic theories of collisions such as Hocking's to cloud droplet collision and capture. Some examples of these attempts to verify theoretical calculations have been described by Sartor (A-15), and recently by Horguani (1965). Unfortunately, some confusion still exists due to inconsistent results among different investigations of gravitationally induced collisions. Though potentially useful, analogies of droplet dynamics using bubbles passing up through a liquid apparently have not been examined. Perhaps
such experiments might help to elucidate the collision process between accelerating particles.

The role of electrification in clouds and its relation to droplet or particle behavior remains poorly understood despite many attempts at field observation and certain efforts to simulate the dynamics of charged particles (Vonnegut, A-20). Since it is difficult at this point even to guess what one should be simulating in electrodynamic systems containing gas and particles, progress in this area must come from especially close co-ordination between field observations and controlled laboratory experiments.

Progress in the study of cloud physics suggests strongly that new information about precipitation and particle growth should come not only from considering one or two particles at a time, but also from observations of the complete population of particles examined in a statistical sense, in much the same way as has been done in the kinetic theory of gases, or in the theory of turbulence. The collection of cloud particles has to be considered an active participant in the dynamics of the gaseous surroundings on scales of a meter or so, and the "micro" statistics have to be tied more clearly to the buoyant convection of the cloudy air on scales larger than 10 m.

Some years ago, initial efforts were recorded in which the dynamics of large numbers of particles and their interaction with the suspending gas were observed in relatively large devices. Notable among these investigations were those of Gunn and colleagues in the 1950s (Langer, A-10; see also: Gunn, 1952; Gunn and Phillips, 1959), who studied certain aspects of cloud droplet formation in a mineshaft, about 2 by 2 m cross section by 210 m deep, and in a 20 m diameter Horton sphere. These experiments were designed to produce clouds within the chambers by expansion of moist gas. Although these devices yielded some interesting results, they do not seem to have been fully exploited as research tools. The reasons may have been inadequate funding, lack of sufficient theory for the statistical mechanics of swarms of particles, or limitations in operating capability of the units.
Phillips, for example, has mentioned that Gunn's spherical gas holder was seriously limited in its capabilities because no provisions were made for supercooling the artificial cloud, and, in addition, relatively large spurious convective currents developed during cloud formation. Langer (A-10) has described certain other limitations of these large chambers.

Large diffusion chambers seem to have been used infrequently as cloud chambers. Squires (A-16) has described some work of his group in designing diffusion chambers for continuous flow nuclei counters. Some intriguing qualitative demonstrations of cloud formation in a diffusion chamber 1 by 1 m by 0.3 m high have been described by Luger at Seattle University. This equipment was built largely after the prototype called the Geneva Cloud Chamber (Powell and Oswald, 1956). Luger's device provided scientifically provocative entertainment for many observers at the 1963 World's Fair in Seattle.

Recently, the theory of the statistical behavior of clouds of particles has advanced somewhat. Typical examples of newer work dealing with time behavior of particle size distributions are the theories of "self-preserving" behavior of Friedlander (e.g., Friedlander and Pasceri, 1965) and Wang (1966), the related numerical studies, for example, of Hidy (1965) and Berry and Mordy (e.g., Berry, 1965), and the analytical work of Golovin (1963) and Scott (1965). Stochastic models for the dynamics of a condensing-coagulating cloud of particles have been presented recently by Levin and Sedunov (1966a, 1966b). And Lilly and his colleagues at NCAR have begun preliminary work on a comprehensive numerical experiment to study the dynamics of a convective cloud. Ideally, this new model will provide the first detailed connection between the microphysical processes and larger-scale convection in the cloud. The consequences of this new theoretical work have provided some background for beginning several laboratory experiments dealing with large collections of particles.

In addition to progress in statistics of clouds, several workers have advanced ideas for fruitful laboratory study of clouds, which are

The enormous potential for uncovering new information by controlled experiments on swarms of particles, combined with the natural outgrowth of theoretical work, has led the Russian geophysicists to build a giant new laboratory for cloud physics at Obninsk. This facility includes a very large expansion chamber of over 3000 m$^3$ volume, two smaller expansion chambers, a cloud column, and a wind tunnel. These devices are all relatively well instrumented, and are controlled to some degree for temperature, pressure, and humidity (Volkovitskii, 1965). The Soviet laboratory for cloud physics is unique. It represents a pioneering commitment to long-term, systematic laboratory studies of aerosol behavior, a commitment on a scale unmatched in the West. Aside from Steele's 1.3 m diameter by 1.5 m high nucleation chamber at CSU, no cloud chambers of any large size are known to be in operation in the United States at this time.

Plans and Speculations for New Cloud Chambers

Based on the requirements of several new theoretical ideas, some preliminary suggestions for the design of modern laboratory devices for cloud physics have come from List (A-11). Surprisingly enough, List's requirements for chambers and tunnels, their dimensions, and their capacity (pressure, temperature, and humidity control) closely resemble the Russian facility at Obninsk. List's conclusions were obtained independently of information about the Soviet laboratory. Others, from time-to-time, have indicated possible configurations for a cloud physics complex. Some information has come from Kinzer's correspondence with H. Wexler, and later from some ideas of F. Bates. Langer (A-10) also has discussed the possibilities for new work in this area, based on remarks of Gunn's colleagues, B. Phillips and P. Allée.

There has been some recent speculation about the construction of a gigantic cloud chamber with a volume near to that of the Houston baseball stadium. Such extremely large vessels do not appear useful at this time because they present enormous problems of instrumentation
and control. However, the possibility of undertaking exploratory experiments with the cooperation of the Houston authorities should be considered, since small clouds have been observed to form just under the stadium dome.

Ideas for new cloud columns also have come forward recently. For example, A. E. Carte has suggested that certain very deep shafts in South African mines may provide very useful columns for studying droplet growth. In contrast to Gunn's experiments using adiabatic expansion in a mineshaft, Carte would utilize these columns as continuous flow devices. The South African mines are about 1000 m deep and are 4 m in cross section. Air is continuously pulled up the shaft from the bottom by large fans. The air enters the bottom nearly saturated with water vapor at about 90°F. During expansion with lifting, the air becomes saturated, and water droplets continuously form and grow in the air stream of rising air coming up the mineshaft. Since Carte's idea involves continuous flow, the problems of loss of droplets to the walls found in Gunn's experiments may be avoided.

Recently Schleusener has proposed using for cloud studies air rising in shafts of the Homestake Mine in South Dakota. Homestake depths are similar to those in South Africa, but the shafts are stepped. This might prove to be a problem in interpreting results of experimental investigations.

By way of another example, Weickmann has suggested that a large unit like a 3 by 3 m cross section by 20 m high cloud column would have use in experimenting with hailstone softening. Weickmann has observed on occasions a close relation between the appearance of graupel and snowflakes in large cumulonimbus storms. If, by experimenting in a cloud column, one could find a method to seed clouds to change graupel to snowflakes, some ideas about the practical aspects of hailstorm modification would be obtained.

In cloud physics, the constant interplay between theorists, laboratory workers, and field observers should continue to provide the firm foundations for future progress. The role of laboratory work for
instrumentation development is now relatively well understood (Squires, A-16), but the possible "primary" role of studying fundamental processes involved in cloud dynamics on a scale larger than the traditional Wilson cloud chamber remains to be worked out. Perhaps such large devices as those built by the Russians eventually will demonstrate a new dimension in laboratory simulation of physical processes in natural clouds. However, it seems likely that the path towards the most successful design and utilization of such large devices has to come from an evolution through specific experiments in much smaller equipment.

MICROSCALE PROCESSES

On scales larger than molecular scale, experiments related to atmospheric processes have largely concentrated on the various aspects of natural aerodynamics. Up to lengths of about 100 m, aerodynamic processes primarily center around the consequences of turbulent flow in air. At the small end of this scale (1 cm or less) one deals with the range of turbulence in which a large part of the energy dissipation of air motion occurs. A particularly important area of study in this category has dealt with the micrometeorology of the lowest 10 m of the atmosphere near the earth's surface. In this zone, turbulence is maintained by the combined action of shearing flow and thermal convection.

Mechanics of Turbulent Boundary Layers

Using the principles of similarity, it has been possible, at least in principle, to construct in the laboratory complete accurate dynamic models of the behavior of atmospheric surface layers. In this class of problems the earth's rotation does not play an important role in the aerodynamics, and need not be considered in scaling the model to prototype. Similarity then depends primarily on matching the appropriate Reynolds number and, in stratified flow, the Froude number.

*For a discussion of a generalized dimensional analysis of atmospheric surface layers, see Bernstein (1965).
or its close relative, the Richardson number (Cermak, A-3; Plate, A-14; Strom, A-17), as well as matching the geometrical boundary conditions, and matching the Rayleigh number and the Prandtl number for unstable conditions of density. It has been found that modeling atmospheric turbulence in surface layers depends on proper scaling of the profile of average horizontal velocity, the Reynolds number based on turbulent intensity and length scale, and perhaps the rate of dissipation of turbulent energy (Cermak, et al., 1966).

In constructing the similarity principles for turbulent surface layers, the Monin-Obukhov theory and Priestley's arguments have proved to be particularly useful. In the Monin-Obukhov development, for example, the relation for the mean velocity profile has a general form which contains the well known logarithmic form for neutrally stable conditions, and accounts for the effect of stability by a length which can be related to the Richardson number. Similarity in average flow over a smooth surface is largely insured by equality in the Monin-Obukhov profile between the model and the prototype. In flow over varying topography, recent developments in modeling criteria suggest that obstacles like hills or structures should have model heights less than the boundary layer thickness. Furthermore, the ratio of the height of the obstacle to the aerodynamic roughness length should be the same in model and prototype.

In practice, experiments on turbulent surface layers in larger wind tunnels have indicated that it is possible to realize sufficiently large (turbulent) Reynolds numbers to model turbulence for quasi-steady atmospheric flow over a boundary of uniform roughness, provided the natural flow conditions upstream can be reconstructed. The development of similarity in upstream flow seems to be one of the central problems in modeling atmospheric surface layers. Another problem involves the viscous effects near boundaries, and the influence of different kinds of surface roughness (Cermak, et al., 1966; Strom A-17). The details of accounting for these effects have not yet been worked out.
Some difficulties have also arisen in attempting to model cases of unsteady flow, and for conditions where the boundary roughness changes sharply. Again, in these cases many practical aspects of modeling still have to be worked out (Cermak, A-3; Plate, A-12; Strom, A-17).

Aside from direct attempts to model atmospheric motion in surface layers, the low-speed wind tunnel or similar devices provide a key tool for fundamental experiments in turbulent media. Though there exists an extensive background of aerodynamics of turbulent flow, there remain questions that must be answered about general properties of turbulence to understand this phenomenon in detail. Some have said, in fact, that the nature of turbulence constitutes the last great unsolved problem in physics. Since turbulence is always present in the atmosphere, it seems essential to know as much as possible about this subject to understand the atmosphere. Long-term, systematic experimental studies in well equipped facilities, combined with new theoretical work, will be essential for solving the problems of turbulent motion.

Fundamental studies of turbulent convection find their way into atmospheric science in many different ways. In one example, certain recent experiments of thermal convection in a chamber have been undertaken by Deardorff and Willis (1966). To model this case, the Rayleigh number and the Prandtl number must be maintained as in microscale atmospheric convection without mean horizontal motion. Their results in combination with numerical integrations of the equations governing the convective flow have shed light on several facets of the theories of turbulent convection as applied to the atmosphere. In addition, these experiments have given some helpful information leading to the crucial problems of how to deal with the effects of motion on scales smaller than grid sizes in numerical computations of air motion (Deardorff, A-4).

Other basic studies of flow in boundary layers over bodies of water in a small wind-water tunnel (e.g., Hidy and Plate, 1966) have provided certain insights into the important questions of the small-
scale interaction between the atmospheric surface layer and the oceans. The fact that the aerodynamic roughness length measured over ocean waves is of the same magnitude as the values obtained in the laboratory, may give an essential ingredient for establishing possible relations between the field observations and the laboratory experiments.

Another important aspect of turbulent flow in atmospheric surface layers involves the diffusion of heat and contaminants such as traces of gas or aerosol. The first principles of the theory of turbulent diffusion are well known and have been reviewed recently, for example, by Corrsin (1964). One of the major difficulties in connecting the theory to observations in the atmosphere centers on relating the theoretical statistics in the Lagrangian frame of reference to statistical observations taken in the Eulerian framework. Studies in wind tunnels have given some insight into this problem. Several examples of direct and indirect attempts to link theory and experiment have been discussed by Corrsin (1964). Other studies using the Lagrangian similarity hypothesis (Cermak, A-3) have displayed some success, particularly in establishing principles for modeling diffusion in the atmosphere.

A variety of practical aspects of dealing with the modeling of diffusion of material in turbulent media are outlined by Cermak (A-3) and, with particular reference to the behavior of stack plumes, by Strom (A-17). Strom has described in some detail certain aspects of modeling of dispersion of smokes from large stacks. Requiring partial similarity of the (unmodified) Froude number ($U^2/gL$) in model and prototype seems to work well experimentally in this class of problems, even in stratified fluids. This is particularly interesting in view of the fact that one might expect the Froude number containing the density ratio to be the key similarity parameter as in the case of the modeling of lee waves (see p. 11; also Long, A-12).

Frenzen (A-7) has shown that one can construct hydrodynamic analogies to diffusion processes in the atmosphere. Using a quasi-homogeneous turbulence generated by moving a grid through a tank of water layered with different distributions of density, Frenzen was able
to deduce from measurements of tracks of colloidal particles, Lagrangian coefficients of correlation for this system. With this technique some quantitative information was obtained about the relation between stratification and the diffusion of matter in a field of turbulent motion.

Research on some aspects of the micrometeorology of atmospheric surface layers as well as fundamental experiments on the structure of turbulence can be undertaken with relatively low investment in time and money in small wind or water tunnels, provided homogeneity in fluid density is the only requirement. However, if the desired research involves flows at very high Reynolds number, or if conditions of stratification are of interest, larger wind tunnels or vessels may be needed. The careful control of temperature, humidity, and other environmental factors places rather strong requirements on the development of adequate facilities. Therefore, even low speed wind tunnels for modern meteorological research can involve rather complex and sophisticated equipment.

The discussions of this Colloquium suggest that large wind tunnels for meteorological research should be constructed with capabilities for thermal and humidity control as well as dust removal capabilities. There are several moderately large low-speed tunnels available, for example, at the University of Michigan, New York University (NYU), and The Johns Hopkins University and the University of Western Ontario. In addition, the U. S. Forest Service has fairly large tunnels for micrometeorological studies in operation at Macon, Georgia and Missoula, Montana. Some of these units have capabilities for stratifying the air flow. The NYU tunnel has floor and ceiling heating combined with an elaborate grid system for heating the air entering the test section. The recirculating CSU tunnel has both heating and cooling units along the floor of its very long test section. However, it has no thermal control at the inlet sections. As yet, no tunnels have been built especially to control the viscous influences associated with the walls and ceiling. Such a unit, possibly contained in the proposed tunnel design at Argonne National Laboratory, would be particularly useful for micrometeorological research.
The question of tunnel length for meteorological modeling seems to have raised divided opinions. Workers at CSU and Western Ontario believe one has to depend on natural growth of a boundary layer for development of equilibrium velocity and temperature profiles, and turbulence properties. Such growth requires very long test sections. However, others like Strom (A-17) feel that much shorter test sections may be used if one "tailor-makes" the profiles and turbulence properties artificially at the inlet section. Halitsky at New York University is working with Strom on developing this possibility.

Plans have appeared for the construction of a large new meteorological tunnel, 3 m high by 6 m wide by 20 m long, at the Argonne National Laboratory. This unit would be designed partly for use at lower speeds than can be achieved in the CSU tunnel. The major stumbling block to this facility has been a well justified concern that, to date, too few specific experiments have been outlined for the tunnel's use. However, a feasibility study, currently being undertaken by the Cornell Aeronautical Laboratories, may give a clearer picture of both the needs for, and the capabilities of, such a new installation.

In addition to investigations dealing directly with micrometeorology, wind tunnels and other environmental devices have occupied an important seat in simulating the aerodynamic behavior of buildings and structures, as well as aircraft and other vehicles. Large environmental chambers have been built to reproduce different kinds of climatic conditions on vehicles and other mobile equipment. Evidence of the continuing widespread practice of environmental simulation is well illustrated in the discussions of Cermak (A-3) and Strom (A-17), and in the bibliography of large facilities listed in Appendix C. Some problems in this kind of modeling also have been reviewed by Scorer (1963).

An interesting example of the approaches to new problems of engineering test devices has been outlined by Truitt (A-17), particularly for ionized gas effects in supersonic flows in an idealized atmosphere. Wind tunnel models to microscale flow can provide useful and important information for developing aerodynamically sound procedures for building
construction and for maximizing wind action for dispersal of atmospheric pollution. Needless to say, the famous disaster of the collapse of the Tacoma Narrows Bridge built in the late 1930s might have been averted by a preliminary wind tunnel study of the structure's aerodynamic stability. And the windy location of Candlestick park near San Francisco might well have been abandoned, or at least the structure might have been improved if a careful preliminary aerodynamic study of the surrounding terrain had been undertaken before construction.

**MESOSCALE PHENOMENA**

When atmospheric motion is considered on scales greater than about one order of magnitude larger than the laboratory, the theoretical problems of developing dynamic models become much more difficult. In general, it has been possible to find only partial similarity in this range of scale between model and prototype.

As the geophysical scale in the horizontal taken in by the laboratory analogy increases, the effect of the earth's rotation becomes more and more important. The influence of rotation has to be considered when the Rossby number becomes less than unity. It is feasible in a given laboratory experiment on atmospheric processes to match the Froude number and the Rossby number. However, the Reynolds number characteristic of these medium-scale and larger-scale flows has much larger values than ever can be realized in the laboratory.

As long as the horizontal scale of atmospheric behavior stays less than a few kilometers, the effect of the rotation of the earth remains small. Under these conditions, some aerodynamic analogies can be devised at least for the average flow on a medium scale in non-rotating devices.

**Wind Tunnel Modeling**

A few experimental analogies to air flow over large obstacles and over topography covering several kilometers, but small enough for the effect of rotation to be small, have been obtained in wind tunnels. Encouraged by the successful use of partial similarity in hydraulic models, workers have examined at least qualitatively the flow around
large structures, or even over topography with ratios of length scales of up to 1:10,000. One of the earliest analogies of this kind involved Abe's (1941) study in a small wind tunnel of the formation of crescent shaped clouds in the lee of Mt. Fuji. More recently Cermak's group (Cermak, et al., 1966) has used wind tunnel analogies to successfully examine stratified flow over two-dimensional obstacles in order to study the gross features of flow around models of the projected World Trade Center Building in New York City, and to picture the dispersion of smoke around topographical models of San Nicolas Island, off the coast of Southern California. Workers in the U. S. Forest Service have also conducted experiments in wind tunnels on flow over timbered topography to try to study, at least qualitatively, how forest fires propagate under given aerodynamic conditions.

There are certain limitations to wind tunnel analogies of air flow extending over distances of kilometers. Some early problems that arose in this class of simulation have been described by Rouse (1951). Cermak, et al. (1966) have described other difficulties in such efforts, particularly in obtaining similarity in the details of the patterns of flow.

The fact that certain features of air flow over terrain extending for many kilometers cannot be duplicated exactly in the wind tunnel evidently results from two factors: first, the Reynolds number based on molecular viscosity generally cannot be made equal in these models; and secondly, the shapes of the topographical features often may have a primary effect on the flow. Workers have argued, however, that dynamic similarity can still be maintained by matching the Reynolds number based on the eddy viscosity rather than the molecular viscosity. This criterion seems to work satisfactorily provided one only looks for gross features of the flow patterns. If the structures or the terrain have sufficiently sharp edges or crests, the gross patterns of average air flow may be duplicated closely, even for conditions where the Reynolds number of the prototype is $10^4$ times greater than that of the model. This effect results from the fact that the sharp edges of the model cause separation in the lee of the model and the viscous effects near
the surface become less important. It is known from aerodynamic studies that, at very large Reynolds numbers, separated flows around bodies display configurations that are largely independent of Reynolds number (Corrsin, 1953).

Some of the more interesting analogies to medium-scale flows include the groups of hydrodynamic experiments designed to study lee waves generated in stably stratified air behind mountain ranges. Some experiments of Long (A-12) in the 1950s showed close quantitative agreement between a hydrodynamic model of waves in the lee of the Sierra Nevada Range and the geophysical prototype. This close analogy was anticipated because of the correspondence in the Froude number, the key similarity parameter in this case. Long has found that the problem of development of mountain waves constitutes one of the few known cases where "exact" dynamic models can be devised for atmospheric behavior, based on certain simplifications of the equations governing the air motion.*

It is believed that theoretical knowledge of stratified flow over mountains is sufficiently advanced to provide the basis for a fairly extensive, systematic modeling program in connection with continuing theoretical studies and carefully coordinated field observations. Such a program should have particular interest for meteorologists because of the difficulties in predicting weather near mountain systems.

Penetrative Convection**

Another class of hydrodynamic experiments related to medium-scale motion, and involving thermal convection in unstably stratified fluids,

*There is also a direct analogy between baroclinic flow and viscous flow of a homogeneous fluid undergoing rotation (Long, A-12).

**Analogies to processes in stratified fluids have not been confined to wave phenomena or penetrative overturning. Turner (A-19), for example, has examined some consequences of the behavior of mixing along surfaces of a discontinuity in density. And recently, Magono at the University of Hokkaido has revived some old techniques of using smoke or dry ice clouds in air to follow qualitatively the development of frontal discontinuities.
has received some attention in the last twenty years. Some of the best known work covers aspects of the rising and spreading of individual buoyant elements. These studies were undertaken in the 1950s by Scorer's group at Imperial College, and have been extended considerably by Turner and others. The original impetus of these experiments involved questions of the rate of entrainment of dry air into the rising buoyant elements, modeled either as plumes or as bubbles. Other experiments have included analogies to the gross effects of condensation and evaporation of clouds, by using combinations of fluids of different density. A recent study begun by Hay (1965) is designed to measure in detail certain properties of an overturning circulation in a liquid. Although, to date, these investigations have largely been qualitative in nature, a great deal of information from them has been applied to conceptual pictures of development of cumulus clouds (e.g., Turner, A-19). Of special interest have been the clarification of ideas about the rate of entrainment into cumulus elements, the scale of the disturbance in the atmosphere, and the balance of forces acting on the element of buoyant fluid.

Other experiments in unstably stratified media have centered attention on the behavior of cellular convection analogous to the classical Bénard problem. Perhaps the earliest attempt to duplicate in the laboratory cellular structures observed in clouds was carried out by Vettin (1882)* in an electrostatic chamber filled with smoke. Later interpretations of cellular convections in liquids as related to atmospheric behavior have appeared in Brunt's (1951) article. Since then, several other articles have described more quantitative aspects of the fundamental features of Bénard convection.

Strictly speaking, cellular convection having exactly the same mechanism as the Bénard problem cannot exist in the atmosphere because

*Interestingly enough, Magono's students have begun to work with thermally driven circulations marked by smoke, in a manner similar to Vettin. Vettin's circulation, however, was driven by electrostatic forces acting on the smoke particles and not by thermal effects.
the Rayleigh number in the atmosphere is much too large. Nevertheless, cellular patterns often do form, particularly in cumulus clouds observed in tropical regions. Therefore, meteorologists have sought to interpret these cells in the light of the knowledge of Bénard-like overturning, using a turbulent Rayleigh number. Some interesting work of Frenzen (A-7) has combined experience with laboratory experiment on convective circulation with geophysical considerations. This work has resulted in interpreting observations of widespread regularities in clusters of clouds as direct consequences of the cellular structure of thermal circulation.

Vonnegut and Eden (A-20) have reported an interesting analogy to the behavior of electrical sheathing layers above large thunderstorms. Disturbances were observed in a sheathing layer over a dense cloud of water droplets formed from dry ice in a box after a convective penetration of the top of the cloud. This perturbation seems to have its counterpart in field observations of electrical disturbances above the anvils of large cumulonimbus clouds.

Weickmann also has suggested that it may be possible to investigate the interaction between vortices associated with penetrative convection in squall lines and medium-scale wind patterns in a shallow tank containing water. Such experiments necessarily would produce mainly qualitative results, but new modeling principles conceivably could be developed in connection with the mountain wave problem.

**Buoyant Convection and Concentrated Vortices**

The behavior of concentrated vortices generated by buoyant convection has also been investigated recently. Qualitative simulation of tornado-like vortices generated in spinning liquids has come from studies of Turner and Lilly (Turner, A-19). The buoyancy along the axis of rotation in these experiments was produced either by nucleation of bubbles in soda water, or by bubbling air into water through capillary tubes placed vertically at the axis of rotation. Hydrodynamic investigations of vortex formation accompanied by heat transfer, simi-
lar to processes in hurricanes, are being conducted by Hess' group at Florida State University.

Similar experiments on a vortex produced in swirling air, either under conditions of homogeneity in air, or by use of heating near the bottom of the column of air, by C. C. Chang and colleagues has had certain popular appeal (*Time*, 87, 52-53, 1966). The news reports of Chang's unpublished work on these basic flows were somewhat sketchy, but evidently he is seeking to measure distributions of mean and turbulent velocity particularly near the boundaries confining the bottom of the vortex. Frenzen has noted another example of a study of mechanically driven vortices in air reported by Dessens (1963). As in the work of Chang, Dessens attempted to measure distributions of velocity and pressure in Weyher's (air) tornado model.

Vonnegut (A-20) also has described some unorthodox but intriguing ideas on the electrodynamic origins of tornado vortices. Some of his simple experiments, for example, have shown that a vortex will stabilize electrical discharges passing through the center of the vortex. Since lightning discharge is often seen in tornadoes, it may be possible that the current flow through the vortex can produce sufficient heating to drive the tornado.

Many of the experiments in stratified media consist of studies that can be extended or repeated with relatively simple, inexpensive equipment. However, it seems that a long-term, systematic program may be desirable to investigate certain aspects of convection in stratified media, and its turbulent transport processes in stratified surroundings. Turner (A-19) has pointed out that knowledge of flow in stratified fluids is very important to better understanding of a wide variety of geophysical phenomena. It might be particularly interesting from the standpoint of applied meteorology eventually to examine the interaction between penetrative elements and the wind field, for example. Such a systematic program could well be integrated into wind tunnel studies of shearing flows in stratified fluids.
Boundary Layers in Rotating Fluids

Because of the action of the Coriolis force the earth's rotation becomes critically involved in fluid motion when the Rossby number characteristic of the horizontal flow becomes sufficiently small, say less than one-tenth. Some consequences of the primary influences of a rotating system are discussed below in connection with laboratory analogies to patterns of large-scale motion. In the groups of studies dealing with medium-scale phenomena, interest in the effects of rotation has centered around the behavior of boundary layers in rotating fluids.

At about the turn of the century, V. W. Ekman showed how the Coriolis force affects shearing flow of a homogeneous fluid near a spinning surface. Later, other workers discussed the possible relationships of flow in Ekman layers to geophysical problems.

Recently, in reviewing the possible significance of Ekman layers for geophysical fluid dynamics, Hide (A-9) has noted the need for distinguishing between "secondary" viscous layers in non-rotating systems and "primary" layers in rotating fluids. Primary layers like Ekman layers exert a direct influence on the geostrophic flow far from the boundary (except in some limited cases of source-sink flow), while secondary layers do not. Since planetary boundary layers are like Ekman layers, these regions of shearing flow may exert a strong influence on the general behavior of tropospheric motion.

During sequences of rotating-pan experiments related to the circulation of deep water in the oceans, Stommel and colleagues (Faller, A-6) accidently discovered certain significant instabilities in laminar Ekman layers. After these initial results, Faller explored the stability question of laminar Ekman layers in some detail. He has used the analogy of the turbulent Reynolds number to relate some of the instabilities found in laboratory experiments to possible mechanisms for development of wind-driven, helical roll vortices observed in planetary boundary layers, which are believed to exist also in the upper portion of thermocline layers in the oceans (Faller, 1966). It appears that the
rolls in the atmosphere are more closely related to thermal convection than to shearing flow instability. Nevertheless, Faller's ideas may well provide the explanation of the wind's role in the formation of the helical vortices in surface layers in large bodies of water.

Like the non-rotating cases, hydrodynamic experiments in rotating fluids can be undertaken easily with relatively small, inexpensive equipment. Instrumentation becomes somewhat complicated, especially if measurements of local properties of the moving fluid are of interest. Perhaps a principal bottleneck in this general class of experiments lies in the elaborate requirements for equipment and techniques to carry out systematic, quantitative measurements on these rotating systems. A large multi-channel data processing system would be welcome to workers in this field.

CONTINENTAL AND PLANETARY SCALE PHENOMENA

The large-scale tropospheric motion is largely dominated, in the mid-latitudes at least, by the influences of the earth's rotation and by baroclinic instability. In the large-scale motions in the upper atmosphere, hydromagnetic phenomena seem to be of central importance. Laboratory experiments related to large-scale processes in these two regions of the atmosphere necessarily involve distinctly different techniques. Analogies to tropospheric motion merely involve further studies of flow in rotating vessels while hydromagnetic processes are associated with plasmas.

It has been possible in a limited way to produce effects in flowing, stratified liquids contained in rotating vessels which are analogous to the mechanisms governing tropospheric motion on a large scale. Of course, one cannot construct exact dynamic models of large-scale motion because the spherical geometry and the action of a central gravitational field cannot be achieved in the laboratory using pure fluids.* Never-

*Rosensweig (1966) has suggested that colloidal suspensions of iron particles could be held to the outside of a spinning sphere by a strong magnet. Such a scheme might provide a direct geometrical analogy to atmospheric motion on a planetary scale.
theless, the initial successes of the classical dishpan experiments at the University of Chicago in qualitatively duplicating certain features of atmospheric flow have stimulated a great deal of interesting hydrodynamic research on rotating fluids since the late 1940s.

These experiments have proceeded along two different philosophical paths. One mainstream revolving around Fultz's (A-8) work has examined in considerable detail the multitude of phenomena in the dishpan experiments in the light of their application and significance to atmospheric behavior. The second avenue has been followed principally by Hide's group (A-9). Their approach has been to study the hydrodynamics of rapidly rotating fluids for its own sake. The application of their results to the flow in planetary atmospheres has been secondary to attempts to better understand the general properties of the dynamics of rotating fluids.

Flow Patterns Like the Planetary Circulation

The experiments carried out by Fultz's group at the University of Chicago and by others have revealed a wide variety of interesting phenomena in rotating fluids. The results have been closely linked to various atmospheric phenomena, from inertial oscillations and Rossby waves in homogeneous fluids, to jet streams and baroclinic waves in symmetrically heated, thermally driven circulations (for a comprehensive review, see Fultz, 1961, and A-8; and Frenzen, A-7). Perhaps less well known are some results of Faller (A-6), who carried out a few dishpan experiments with unsymmetrical heating. The resultant patterns of flow seem to be qualitatively similar to certain nearly permanent troughs observed eastward of continents. Frenzen, however, has remarked that flows similar to Faller's can be found from orographic effects. Long's work on inertial oscillations induced by obstacles in a hemispheric vessel is typical of such results (Frenzen, A-7).

Although the earth's rotation is probably critically involved in the formation of fronts in the atmosphere, relatively little work has been done in the laboratory on this class of problems. However, Faller
(A-6) and Fultz' group (A-8) have carried out some preliminary experiments on frontal disturbances in a rotating vessel. The initial results showed that it is indeed possible to observe fronts in stratified rotating liquids which appear qualitatively to be very similar to fronts observed in the atmosphere. Although these experiments could help to verify conclusions of numerical studies of frontal development, such as those of Kasahara, et al. (1965), no quantitative laboratory work of this kind has been reported.

**Basic Experiments in Rotating Fluids**

Much of the work exemplified by Hide's group has concentrated on attempting to place on firm grounds a few more-or-less general "theorems" about the behavior of rotating fluids (Hide, A-9; Eden, A-5). In some of Hide's recent studies, he has found that the Proudman-Taylor theorem combined with the ideas of suction of fluid into Ekman layers can give very general principles for dealing with barotropic fluids in rotation. For example, the Proudman-Taylor theorem has direct bearing on the nature of the idealized columnar vortices called "Taylor columns," and on detached shearing layers such as the jet streams in the atmosphere. Hide has used the behavior of Taylor columns to suggest an explanation for the red spot in Jupiter's atmosphere, and Warren has used similar ideas to interpret the steering of the Gulf Stream by bottom topography (Hide, A-9).

The research of Hide's group on rotating baroclinic fluids has concentrated on the various stability regimes of flow in a rotating annulus. Perhaps best known of the four stability regimes are the axisymmetric case, sometimes called the Hadley regime, and the unstable wave case, analogous to planetary flow of the atmosphere in the mid-latitudes.

After some effort, Hide (A-9) and his colleagues have shown that a range of instabilities in the rotating annulus flow is dominated by mechanisms of baroclinic instability. Hide finds that Eady's (1949) theory for baroclinic instability contains the essential ingredients of the behavior of this controlled hydrodynamic experiment over the range where the effects of viscosity and thermal diffusivity are small.
With rising interest in these kinds of experiments, new work on rotating fluids is becoming available. For example, Ibbetson at Woods Hole Oceanographic Institution has carried out some careful experiments on inertial oscillations in a homogeneous rotating fluid. He and Phillips (Ibbetson and Phillips, 1966; see also Phillips, 1966) have related the results in an interesting way to inertial oscillations in ocean basins. Recently Pfeffer, et al. (1965) has published some ideas on available potential energy in the atmosphere, incorporating some results of laboratory work on rotating fluids at Florida State University.

Hide (A-9) believes that the annulus experiments involving thermally driven circulations are sufficiently well understood that one can proceed with experiments of more complicated geometry and more complicated heating configurations. Fultz (A-8), Hide (A-9), and Faller (A-6) have indicated several avenues for rotating systems. One important suggestion is the need to establish stronger ties between numerical studies of circulation and laboratory experiments. One of the better ways for testing the principles used in the numerical models of general circulation is to apply these techniques to the laboratory experiments. Certain detailed statistical quantities can be measured in flow in rotating vessels, and the behavior should be predicted reasonably by a numerical integration similar to a general circulation scheme. Climatological calculations could be checked in a similar way using a dishpan or an annulus experiment. Fultz feels that it is possible, in principle, to realize about 4000 years of "elapsed time" in a laboratory experiment conducted over four months. Perhaps an experiment of this kind would have value only after initial sequences of shorter experiments were undertaken to check numerical calculations.

One of Hide's students, Piacsek (A-9), has already had some success in constructing a computer solution to the symmetrical flow case in a rotating annulus. This procedure is expected to be extended farther to more difficult patterns of flow like the cases involving symmetrical baroclinic waves.
The technology seems to be well established for devising suitably accurate turntables and other machinery-like slip rings for the experiments in rotating systems. Relatively large rotating vessels may be built from existing designs with few major design improvements. However, as with mesoscale effects, further detailed study of the hydrodynamics of rotating systems almost certainly will require more elaborate data processing techniques. Fultz, for example, has recommended that, ideally, a very sophisticated multi-channel recording system should be developed, incorporating high-speed record chart readers, and streamline photograph tracers. The system should also include a fast analog-to-digital data conversion system. The expenditure for new rotating platforms, even large-diameter ones (2 m), is relatively small compared with the expense of developing new, elaborate instrumentation.

**Hydromagnetic Analogies**

In contrast to the laboratory analogies of tropospheric motion, the rarified gas dynamics of the highest reaches of the atmosphere, involving hydromagnetic phenomena, have challenged the laboratory experimentalist in a different way. The direct modeling of hydromagnetic processes on a geophysical scale or an astrophysical scale appears impossible (Nakagawa, A-13). However, a number of basic laboratory experiments have been devised that shed some light on limited aspects of these problems.

Birkeland (1908, 1913) was perhaps the first worker to experiment on electromagnetic phenomena as related to the atmosphere. He studied glow discharges about a spherical magnetized body, and tried to relate his observations to the aurora. The principles of these experiments were largely ignored by geophysicists until after World War II. The

*Certain similarities in the dynamic equations of hydrodynamics and electromagnetic theory have suggested possible analogies between phenomena governed by these relations (see also Fultz, 1951, and papers listed by Nakagawa, A-13). Further examination of this aspect of "modeling" indicates that such analogies are quite limited, especially in geophysical situations.*
strong interest in interactions between streamers of charged particles from the sun and the earth's magnetic field stimulated workers at the U.S. Naval Research Laboratory to try to construct new analogies to the aurora in terms of trajectories of proton beams impinging on a spherical magnet (Bennett, A-1). These experiments were quite successful in matching observations of time and location of the aurora. However, the proposed mechanism of interaction between the earth's magnetic field and beams of charged particles has turned out to be too simple. Charged particles from the sun are now believed to approach the earth in large-diameter streams (the solar wind) rather than in narrow pencil-like beams. The impingement of the solar wind on the planet gives rise to complicated effects like a hydromagnetic bow shock, and a cusped, wake-shaped magnetic field to the lee of the earth.

Despite revisions in the theory of streaming of particles from the sun, it may be possible to incorporate the older beam model into the newer theory. Bennett (A-1) has cited some new experiments of Baker and Hammel (1965) on plasmas that largely reopen the whole question of the origins of auroral phenomena. In view of these experiments and the earlier beam investigations, the time may be ripe to proceed to new theoretical and experimental work on this class of problems.

Several other basic experiments on plasma behavior bearing on the sun and its effect on the earth have been described by Nakagawa (A-13), and a recent experiment dealing with the geophysical significance of laboratory work on plasmas trapped in a magnetic field has been reported by Quinn and Chang (1966).

It is clear from the remarks of Nakagawa, Bennett and Chang that considerable basic work has to be done on the dynamics of plasmas before they are well understood. The idea of modeling geophysical and astrophysical phenomena involving plasmas is very attractive. However, it seems desirable to press forward in the basic physics of hydromagnetics as a primary objective instead of concentrating on new "look alike" laboratory demonstrations.
IV. ON THE QUESTION OF A NATIONAL FACILITY FOR SIMULATION

One of the primary objectives of this Colloquium was to find out if there is a need for certain groups of atmospheric problems to be attacked on a laboratory scale corresponding to a national facility. During our discussions it became obvious from the beginning that one large multi-purpose facility for simulation would never meet the requirements of a wide variety of possible future experiments. Even the concept of a "general" laboratory stocked with various devices, "plug-in" machinery for controlling temperature and pressure, and general purpose instrumentation, would be unsatisfactory. Such a configuration could never be designed sufficiently flexibly to accommodate the needs of all experimenters and, at the same time, provide optimum utility for a particular study.

The ability to control and reproduce phenomena related to atmospheric processes in laboratory experiments should help to accelerate progress in the atmospheric sciences, and should aid in realizing promptly new practical applications of basic ideas to understanding of the weather. Therefore, careful research in the laboratory should be considered as a primary tool in modern atmospheric research, side-by-side with computers and observational equipment like ground stations, balloons, and aircraft.

In general, atmospheric simulation is too closely tied to the ingenuity and creativity of individual workers to rely on a heavy commitment to the drudgery of routine data recording for its own sake. Thus, even major laboratory investigations should be placed in the structure of specific programs of planned experiments with definite goals. Therefore, if qualified scientists become interested in carrying out certain classes of experiments that require sophisticated devices and long-term ventures, every effort should be made to support their needs both in manpower and materiel.

Many relatively small-sized simulation programs of considerable importance already exist in which experienced scientists are studying a variety of different aspects of atmospheric processes. Some of these
have been discussed in this Colloquium. Based on our discussions, however, there appear to be at least three promising major programs of a long-term nature that may fit into the category of laboratory investigations requiring effort greater than any one group could muster at the present time. Perhaps the most significant are the following: (a) an aircraft instrumentation test chamber, (b) a program for research related to the behavior of dispersed systems in the atmosphere, (c) a study of the dynamics of mesoscale flow near mountain ranges, and (d) an extended study in a wind tunnel of turbulent shearing flow in stratified media. In addition, there are several worthwhile projects of a miscellaneous nature that bear on atmospheric simulation. This class of problems includes studies of the dynamics of stratified fluids, both in non-rotating and rotating systems (often delineated as the major part of geophysical fluid dynamics), and new basic experimentation in plasma physics.

Linked with virtually all of the projects involving laboratory simulation of atmospheric phenomena, there is a definite need for major but flexible units for data processing.

In the sections below are discussed the more promising schemes ordered as to my view of priority.

PRIORITY A: An Aircraft Instrumentation Test Chamber

A traditional part of atmospheric simulation involves environmental test facilities. These devices have the advantage over pure research projects that they can be designed around definite needs to test instruments or vehicles. To explore small-scale turbulence away from the ground, and the nature of clouds, observers are faced with using an aircraft as an instrument platform. In working with airplanes, one automatically is faced with the problem of adopting instrumentation for the measurement of atmospheric properties at speeds of 100 to 200 knots. An exceedingly important step towards obtaining reliable and consistent measurements by airplane is the calibration of the instrumentation in a system actually traveling at aircraft speed through relatively quiescent air. Up to the present, no completely satisfactory methods have been
devised to provide accurate calibration standards for such conditions in dry air, let alone in wet, cloudy air. However, the importance of this kind of basic standardization to the future work of groups throughout the world is all too clear from remarks, for example, contained in the recent NAS-NRC report on weather modification (Panel on Weather Modification, 1966).

In connection with this Colloquium, P. Squires and J. Telford have given a plan for a new cloud chamber that will provide, in principle, a suitable facility for calibrating aircraft instruments, including pitot tubes, wet and dry bulb thermometers, and devices for measuring spectra of cloud droplets. The details of this facility are outlined in Appendix B. In essence, the unit consists of a tunnel about 400 m long, in the middle of which is located a thermally well insulated cloud chamber about 60 m long. Clouds of water droplets of controlled size distribution are produced in the chamber by a series of atomizers. Instruments are passed through the stagnant air in the test section by means of a sled-like vehicle traveling at speeds of about 200 knots. The aircraft instruments are then calibrated against a series of stationary instruments placed at various distances down the test section. The overall length of the chamber has to meet the requirements of acceleration and deceleration of an instrument carriage, while the length of the test section is based on the "time of flight" needed for measurements with instruments having 0.1-sec time constants. For versatility, this test facility should be able to attain the range of -20 to 30°C temperature and 300- to 1000-mb pressure.

The total cost of development and construction of such a test chamber might be nearly $1 to $2 million not including staff (see also Appendix B).

The need for the aircraft instrumentation facility is particularly acute in respect to the efforts of workers with programs like those of Squires, Telford, Sartor and others. In my opinion, the design
and development of this chamber should be placed within the scientific programs of the interested groups of experimenters to insure proper evolution and growth of this project. I believe that efforts might be initiated towards plans for construction of this unit as a part of the NCAR-LAS program or other major programs for meteorological research using an instrumented aircraft. To implement this, I recommend the following steps:

**Step 1.** Contact scientists at leading organizations in aircraft meteorology such as the following, to enlist co-operation and seek suggestions for complete criteria for design of the test chamber:

- a) U.S. Naval Research Laboratory
- b) Commonwealth Scientific and Industrial Research Organization (Australia)
- c) Air Force Cambridge Research Laboratory
- d) ESSA-USWB Flight Facility
- e) Meteorological Research Office (Great Britain)
- f) U.S. Meteorological Research Institute
- g) Institute for Atmospheric Science, South Dakota Institute of Mines and Technology
- h) Department of Meteorology, University of Chicago
- i) Department of Atmospheric Sciences, Pennsylvania State University
- j) Desert Research Institute, University of Nevada
- k) Department of Meteorology, University of Wisconsin
- l) Department of Atmospheric Sciences, Colorado State University.

**Step 2.** Begin locating and hiring staff, as recommended in Appendix B, and formulate definite plans for design and construction of the test chamber.

**Step 3.** After construction, begin experiments by calibrating thermometers in dry air in test section. With this well defined test completed, begin program of calibration of instruments in cloudy air.

**Step 4.** After satisfactory completion of development stages in Steps 1 through 3, turn over the facility to NCAR Facilities division for routine standardization programs for all users.
PRIORITY B: A Research Program for Laboratory Cloud Physics

For some years, workers have recognized that basic research on particles suspended in gases can have many applications to the atmosphere from the standpoint of understanding the mechanisms governing formation of clouds, and from the view of developing an understanding of aerosol behavior in smogs. Both of these cases are closely tied to important work in weather modification, and the development of measures for controlling air pollution (see also Panel on Weather Modification, 1966).

Although there exist substantial quantities of literature dealing with the behavior of single particles in gases, the laboratory study of collections of particles, their mechanical interactions with surrounding gas, and their associated electrodynamic phenomena, has been left largely untouched. In undertaking such research it is not at all obvious, in general, what kind of vessels and devices are needed, not to mention their dimensions and ranges of control. Evidently the Russians have felt that a complex of rather large vessels is needed to investigate the dynamics of particles in clouds. Their explicit reasons for such an extensive commitment to large facilities only can be surmised at this time. It appears, however, that the only real advantage in devices of large volume lies in satisfying the requirements of stabilizing a cloud for relatively long periods of time, by delaying the effect of diffusional removal of particles at the walls of the vessel. The increase in size of the chambers to satisfy this requirement, of course, has to be balanced by considerations for the ability to thermally control these units within very close tolerances.

If one chooses to attempt to reproduce conditions in the atmosphere for simulation of the development of even a small cumulus cloud, including small-scale circulation of air, length scales realistically have to be in the range of 10 to 100 m. The characteristic length easily realizable in the laboratory is at least an order of magnitude lower than these figures. To complicate matters, the guidelines for constructing dynamic models of interacting systems of colloidal particles as yet have not been worked out in any satisfactory way. Therefore, no complete
quantitative criteria are available for scaling down (building models of) systems of individual clouds for laboratory study.

The published results from experiments carried out in large vessels are disappointingly few. Although the work of Gunn and colleagues produced some interesting and promising preliminary results, it failed to demonstrate a truly critical need for continued study of model clouds in large chambers. Furthermore, the Russians have not given any indications so far in their scientific literature of significant new experimental results from their Obninsk facility despite the fact that this complex is over three years old (see also, Battan, 1965; Atlas, 1965). In all fairness, however, one should bear in mind that major projects of this kind often have time delays of five to ten years before fruitful experimental results are obtained.

The available information hints rather strongly to me that even though an enormous potential may exist for investigations of microphysical processes related to natural clouds in large vessels, the giant step to very large devices may not be the best way to proceed. Instead, the evolution of such a research program might best come from stepwise progress through a planned expansion based on increasing knowledge about the behavior of dispersed systems. To initiate such an evolutionary project, I recommend that a new long-term program of five years or more in laboratory aerosol-cloud physics begin as soon as possible. The course of this project should be set initially by the requirements of three important sets of questions about the behavior of groups of particles suspended in gases:

a) It is known on theoretical grounds that vapor condensation on atmospheric aerosols in an assumed stagnant, homogeneous medium cannot explain the broadness of the size spectrum of cloud droplets. What role do microscale heterogeneities in the supersaturated vapor, and (associated) microscale turbulence play in causing this spectral polydispersity?

b) Although the aerodynamics of capture of a small particle by a larger one falling in a stagnant medium has been studied, virtually no
Experimental investigations of the growth of a large droplet falling through a collection of smaller particles have been undertaken. Does this complication affect the polydispersity of a cloud? What is the effect of mechanical disturbances (turbulence), or electrostatic perturbations on the growth of such large particles as they fall through swarms of small particles?

c) The contribution of the ice phase to the stability of clouds of growing water droplets remains a mystery. What is the effect of sudden freezing of certain supercooled droplets in a collection on the growth of the entire group of particles? Is there an apparent triggering mechanism leading to a chain reaction of ice crystal formation in the cloud which is analogous to crystal growth in supersaturated liquids? Can the questions surrounding the nature of ice nuclei be elucidated by observations of the behavior of a supercooled cloud under controlled conditions? Can the efficiency of the mechanism of cloud seeding be better understood by introducing various nuclei like silver iodide into a stable supercooled cloud contained in a chamber?

Experimental work in all three of these categories should be preceded and accompanied by careful theoretical studies.

To meet the needs of sets (a) and (c), either an expansion chamber or a diffusion chamber (or both, or a combination of the two) should be constructed of a size sufficiently large to allow mechanical or thermal disturbances to be induced conveniently (possibly in analogy to the device of Deardorff and Willis, 1966), and to allow direct sampling in parts of the cloud without disturbing the entire volume of dispersed phase. This implies that the devices should have volumes of the order 1 to 3 m$^3$. To study the effects of ice nucleation, the chambers should be controlled to $\pm 0.1^\circC$ over the range of -50 to 50$^\circC$. The expansion chamber should have an operating range of pressure from 200 to 1500 mb. Injection systems for introducing water droplets and aerosols, plus filtered air supplies, and means of inducing controlled air circulation and electrical properties, will be needed.
The investigations noted in (b) can best be implemented in a cloud column. Requirements on temperature, pressure, injection systems, etc., will have to be met in designing this device. In addition, a cross-sectional area of about \(1 \text{ m}^2\) will be useful to insure spatial uniformity across the column. A test section length of 2 m, with an overall column length of 6 to 8 m, seems reasonable as a preliminary estimate for maintaining proper control of the flow in the column.

A key factor in carrying out experiments in all three categories is the requirements of proper instrumentation for a cloud sampling. For example, the size spectrum of aerosols put into the chamber will have to be monitored and the changes in size distribution will have to be measured either by sampling directly or by optical techniques. The structure of air turbulence in the devices will have to be measured, possibly by known methods using hot-wire anemometry. Also, in principle, the temperature field in cloudy air will have to be measured. Instrumentation development during these projects will be a natural outgrowth of the experiments.

The cost of the design and construction of the two chambers and the cloud column with adequate instrumentation and staff is estimated to be under $1 million. By a rough appraisal, at least one staff scientist and one technician would be involved initially in the program for each device. The total cost then can be estimated arbitrarily on the basis that the three devices and their instrumentation would require roughly equal cost for development and construction. For one unit, the following items are considered: (a) insulated vacuum-tight vessel ($10^4$); (b) refrigeration and vacuum ($2 \times 10^4$), control of pressure, temperature and humidity ($10^4$), instrumentation (including multi-channel tape recorder, strip chart recorders, dual channel hot-wire anemometer, temperature and humidity probes, multipliers, electrical signal spectrum analyzer, aerosol size spectrum analyzer, photographic devices, aerosol generators, and optical scattering measurement equipment ($6 \times 10^4$); (c) design and engineering ($1.5 \times 10^4$); (d) staff of one scientist ($1.2 \times 10^4$) and one technician ($0.8 \times 10^4$) (times five years) ($10 \times 10^4$), and (e) miscellaneous and overhead 15% of total
above (\$32 \times 10^4\)). For three equivalent units, the total cost is 3 \times \$2.47 \times 10^5, or \$7.41 \times 10^5.

Systematic experimental programs like those outlined in categories (a) through (c) will lead to other studies with similar questions to be answered. With the research will come more knowledge about the need for larger devices, and better understanding of the technology involved in handling dispersed systems. An evolutionary process of this kind should lead to definite specifications for a new generation of larger cloud chambers, columns and tunnels, if they are really required for future efforts of a long-term nature.

A program in laboratory cloud physics as outlined above would have value in itself. However, its value will increase considerably if the work is carried out in such a way that is closely linked to theoretical programs and programs of careful, systematic field observations. Such an extensive cooperative investigation is best located within the structure of a national laboratory like NCAR or ESSA's atmospheric physics laboratory. Since NCAR has existing programs of a theoretical and laboratory nature, as well as capabilities for implementing research aircraft and ground stations, I believe that the particular equipment discussed above should be undertaken at this laboratory to serve the NCAR programs. Concurrently, ESSA is beginning to formulate plans for a major program of laboratory study of processes related to clouds. Co-operation between these groups and others, such as workers at the RAND Corporation and the Desert Research Institute, will be essential.

Furthermore, certain parts of the Public Health Service (PHS) support for research in aerosol physics and chemistry is closely tied to experiments (a) and (c) above. Therefore, co-ordination with PHS planning also should be sought.

To implement the development of a major program for laboratory aerosol and cloud physics, I recommend the following initial steps:

**Step 1.** In co-operation with ESSA and PHS, begin planning for programs based on series of specific experiments to provide for a step-by-step evolution of experimentation on the dynamics of dispersed sys-
tems. Such planning should lead to better delineation of requirements ultimately for devices possibly much larger than the ones described above for an aerosol-cloud physics laboratory complex.

**Step 2.** Undertake a theoretical study to attempt to determine criteria for modeling the dynamics of collections of particles suspended in a moving gas.

**Step 3.** Incorporate the program outlined above in present LAS groups with the full support of NCAR. For example, the work under part (a) could be initiated under the efforts in cloud physics of Squires and, in aerosols, of Hidy. The research on the cloud column (b) fits into the present commitments of Sartor's group and into Rosinski's plans. Studies involving the role of ice particles (c) are linked, for example, to the LAS programs of Knight, Rosinski, and Goyer.

**Step 4.** The activity in Step 3 should be carefully co-ordinated with field observations and instrumentation development proposed, for example, by Squires and Telford (see also Priority A).

**Step 5.** The experimental program outlined above should be viewed partly as a vehicle for answering within the next five years the questions involved in the possible construction of a "next stage" of larger facilities. In connection with this, it is essential that communication on a personal level be established with the Russian workers at Obninsk for evaluating the progress and usefulness of their laboratory complex. In addition, close liaison should be maintained with Carte's group in South Africa to assimilate the results of his proposed work in the deep mineshaft study.

**PRIORITY C: A Program for Studying the Aerodynamics over Mountainous Terrain**

The theoretical principles of the flow of a stratified fluid over a two-dimensional barrier are fairly well understood, and the theory has been applied successfully to hydrodynamic modeling of internal waves forming to the lee of mountain ranges. Therefore, a major study seems in order for examining in detail many aspects of orographic effects in the atmosphere for one particular topography. Such a program would
employ a three-pronged attack on the problem of forecasting weather in
mountainous regions, which would include (a) numerical studies extending
existing theory, (b) modeling in a water channel or a wind tunnel, and
(c) systematic field observations on the ground, by balloon, and by
aircraft at key locations and times. Such a project could very well
demonstrate just how modeling of dynamic processes can fit in a defini-
tive way into developing better techniques for forecasting mesoscale
weather.

Initially, the modeling study should concentrate on (a) experiments
looking for better specification of the consequences of nonlinear
effects in flow over two-dimensional barriers, and their relation to
air motion over a particular topography, (b) answering the questions
about the role of viscous effects near the boundary in limiting complete
similarity between the model and the geophysical prototype, and (c)
the advantages and disadvantages of wind tunnel analogies compared to
hydrodynamic models for this class of mesoscale problems. Part (a)
should help to establish the key locations for making efficient and
critical field observations under given atmospheric conditions to
verify predicted patterns of flow. Parts (b) and (c) will contribute
information to better plan for the possible role of modeling in future
research of atmospheric dynamics.

For convenience, it makes sense to provide for a mountain flow
project in a region just east of a large mountain range. A location
near the Sierra Nevadas, the Rockies, or the Central or Northern Appa-
lachians would be suitable.

The cost of the mountain flow project would largely depend on the
elaborateness of the field observation network and the type of simu-
lation equipment to be used. For a complete, and well instrumented
program over a five-year period with field observations, and employing
both a water channel and a wind tunnel, up to $10 million including
staff might be realistic. The size of the technical staff will largely
depend on the magnitude of the program once it becomes organized.
The highest estimated cost of this program is based on the following: (a) ground based network of instruments, an instrument package every 10 mi (100 instruments) to measure pressure, temperature, humidity, wind speed and deviation, condensation and freezing nuclei counts, and atmospheric electrical fields; 10 of these units would include masts and sets of instruments for measuring vertical profiles of wind speed and direction, temperature and humidity ($4 \times 10^6$); (b) aircraft and instrumentation (including operation) ($2 \times 10^6$); (c) wind tunnel ($10^6$); (d) water channel ($0.1 \times 10^6$); (e) computer time 100 hr x $500$ ($5 \times 10^4$); (f) data processing and communications ($1 \times 10^6$); (g) staff of 20 people ($10,000/yr) ($10^6$); (h) overhead and miscellaneous at 15% of sum (a) through (g) ($10^6$). The total is about $10^7$.

Because of the possibilities for co-ordinated planning and activity, as well as access to computers, aircraft and instrumentation, it seems logical to recommend that the mountain flow work be undertaken by ESSA's research laboratories in Boulder with co-operation from NCAR's dynamics groups and with links to the CSU efforts in the meteorological wind tunnel, and to Colorado University's programs in experimental fluid dynamics. Because of ESSA's close ties with forecasting problems, perhaps this project should be headed by this organization's research groups.

To implement such a project, I recommend the following steps:

**Step 1.** Organize the mountain flow project around the structure mentioned above. Begin outlining the realms of responsibility in a co-ordinated program between various divisions of ESSA, NCAR and CSU.

**Step 2.** Examine the existing two-dimensional theory of orographic effects on stratified fluids, especially in the light of possible non-linear effects, and attempt to extrapolate the possible role of three-dimensional influences of terrain. Continue aerodynamic experiments

*Much of this work could be an extension of the existing NCAR field study of Lilly, the numerical integrations of D. Houghton, and the present wind tunnel investigations of Binder in the CSU tunnel.*
on two-dimensional barriers like those at CSU to better define design criteria for a special wind tunnel to study lee waves. Begin experiments for two-dimensional barriers in a water channel to explore possible influences of Froude number and the Reynolds numbers. Compare results of wind tunnel work with the hydrodynamic models. Continue NCAR's present program of field observations of weather in the Rocky Mountains near Boulder.

Step 3. Begin modeling experiments on a particular topography, and begin laying out programs for systematic field observations based on the results of the model flows.

Step 4. After the usefulness of such a project is demonstrated, expand the program if desirable.

**PRIORITY D: Data Processing Equipment**

In the many quantitative investigations of the laboratory, there is an ever pressing need for easy-to-use, accurate, high-speed equipment for data processing. As a part of this Colloquium, it is important to add a recommendation for a general program for developing new research-oriented recording and analyzing equipment. Such equipment, once available, would provide incentive to devise more elaborate experimental studies, and, of course, would have wide use in the scientific community over and beyond the handful of laboratory experimenters for whom it was created. One easily can see that data processing for geophysical operations could cover a vast range of activity from laboratory investigations to field observations.

As an example of the magnitude of development required for such data processing systems, Fultz (A-8) and Eden (A-5) have given some suggestions of the needs for experiments in rotating fluids. In Fultz's view there are two possible systems of interest to the experimenter, both of which give great encouragement to undertake more sophisticated and detailed studies than presently foreseeable.

The first unit would be a small package to be used at individual laboratories in connection with large existing computers. Such a
device might consist of special strip charts or photograph readers connected to a high-speed multi-channel analog-to-digital converter with capabilities for (digital) magnetic tape storage. The second unit would be a very large, but flexible multi-channel analog-to-digital converter which could handle all types of input data from photographs and strip charts to analog magnetic tape recordings. This kind of equipment could be located centrally and attached as an input device to a large national computing facility. Individual workers could submit their laboratory data in several different forms routinely to the central facility for various kinds of analysis.

Fultz has estimated that either one of these possible configurations for data processing might require up to $10 million for development and purchasing. However, Frenzen has suggested that similar equipment might be developed for specific objectives for at least an order of magnitude less cost.

In view of the widespread interest in the development of data processing units, I recommend that the following steps be taken to provide the initial impetus to such a project:

**Step 1.** A feasibility study of a unit flexible enough to combine the needs of laboratory workers and field observers should be undertaken by the NCAR Facilities division in connection with its computer efforts. Possibly the help of one of Fultz' associates could be enlisted for one or two summers to implement this study.

**Step 2.** Based on the findings of the Facilities study, planning for the design and construction of the data processing units should be started with the use of a reliable and interested electronics company as subcontractor.

**Priority E: Geophysical Fluid Dynamics and Plasma Physics**

The general field of basic experiments on dynamics of geophysical fluids is an expanding one in which a number of investigators are contributing interesting and important results. Particularly helpful to the meteorologist are the studies of the behavior of stably stratified
fluids in non-rotating and rotating systems. Since experiments in geophysical fluid dynamics provide a great wealth of fertile areas for the investigations of graduate students at relatively small expense, universities should be encouraged to support and expand existing programs, and to begin new efforts in this area if staff become interested.

The philosophy of Hide, for example, in seeking fundamental theorems for the behavior of rotating fluids appears to be a particularly fruitful approach. This sort of view may lead to generalizations that will be much more aid to the study of the atmosphere than attempts to model special effects based on ideas of partial similarity.

The idea of Rosensweig (1966) of applying the peculiarities of liquid suspensions of ferromagnetic particles to geophysical fluids problems should be explored further to see if the behavior of these colloidal suspensions can tell us more about the dynamics of the atmosphere.

**Major Projects in Geophysical Fluid Dynamics.** Three major problems in the hydrodynamics of rotating fluids may be sufficiently time consuming and extensive in equipment requirements to rule out student efforts in university groups. These are (a) extensive study of the development of fronts in a rotating, stably stratified fluid, (b) comparison of the average properties of flow in irregular wave regimes of a rotating annulus with the statistics generated by a numerical integration of the equations of motion governing such a system, and (c) as a sequel to (b), if (b) proves to be an interesting and fruitful study--a comparison between the climatology generated in a rotating annulus or a dishpan and those generated by a numerical solution of the equations governing motion in the laboratory experiment. If interest in such projects develops on the part of qualified workers, these experiments possibly should be undertaken in connection with atmospheric dynamics programs at NCAR and ESSA.

Over a period of years, many individual scientists may wish to try out certain ideas about different kinds of geophysical fluid flows in the laboratory. These workers may have no immediate access to proper,
specialized equipment like rotating tables, water channels or wind
tunnels. In view of this potential need, it may be desirable to work
out an association of individual university laboratories presently
having strong resources of equipment for various classes of simulation
experiments. Such a co-operative program (upon agreement of the scien-
tists involved) would require a certain amount of support from the
National Science Foundation, for example, to keep equipment maintained,
and to provide for the overhead of short-term visitors at the insti-
tutions.

One of the flaws in the organization of the present Colloquium was
the fact that no allowance was made for the visitors to NCAR to explore
with each other plans for future work in geophysical fluid dynamics.
Therefore, I recommend that a symposium be organized in the summer of
1968 as a part of the NCAR summer program to give opportunities for
wider discussions about various aspects of dynamic models of the behav-
ior of geophysical fluids. Support for this symposium perhaps should
be sought through the American Meteorology Society, the American Geo-
physical Union, and the Fluid Dynamics Division of the American Physical
Society.

**Basic Research in Plasma Physics.** The state of knowledge of the
behavior of plasmas, or more generally, hydromagnetic systems, is very
limited. The failures of many groups to resolve the practical prob-
lems of controlled thermonuclear devices has driven scientists back to
basic studies of plasma physics. Many processes involving plasmas are
believed to be relevant to the behavior of the sun and its interaction
with the atmospheres of other planets. However, the basic dynamics
of ionized media are not sufficiently well understood at this time to
warrant a strong program specifically directed towards geophysical and
astrophysical applications. Because of the expense of undertaking a
first-magnitude operation for studying plasma dynamics, the National
Academy of Sciences in a recent report of recommendations for future
effort in physics (e.g., *Science* **151**, 1363-1366, 1966) proposed that
a stronger program in advanced research for plasma physics should be
organized. Until the behavior of plasmas is better understood, those interested in the application of the theory of plasma dynamics to the upper atmosphere perhaps should lend complete support to such a centralized, basic program.

**PRIORITY F: A New Wind Tunnel Program for Investigating Features of Turbulent Shearing Flow in Stratified Media**

Many fundamental questions about the structure of turbulent motion in stratified fluids remain to be fully answered. For example, (a) what is the relation between the effects of stable stratification and the degree of anisotropy in the turbulence? (b) How does stratification influence the spectrum of turbulent energy? (c) How do the effects of time variations in turbulent energy influence the development of boundary layers? (d) What are the details of the "turbulent" exchange processes near an inversion layer? Answers to such questions as these will help investigators to better understand many aspects of the micro-scale behavior of the atmosphere.

In addition to their use as tools for basic studies of turbulence, wind tunnels will play an expanding role in examining a wide variety of problems of an applied nature including: investigations of dispersions of pollutants from smoke stacks and from release of toxic gases by an explosion or other sources; engineering studies of the aerodynamics of structures such as bridges and buildings; and the scattering or distortion of the propagation of electromagnetic waves in a turbulent medium.

Some of this type of research can be undertaken in existing tunnels. However, most of these units are not equipped to operate under conditions of stratified air flow. Facilities specifically designed as meteorological tunnels (like the one at CSU) are already being used nearly full time. With the growth of programs in wind tunnel research on meteorological problems, new facilities having design requirements for specific experiments will certainly be needed.

The work at CSU, and the studies at New York University and other institutions, have just begun to fully exploit the potential usefulness
of wind tunnel modeling. Based partly on the initial success of these and other investigations, H. Moses at the Argonne National Laboratory recently submitted a proposal to the Atomic Energy Commission for a new meteorological tunnel. Because of reservations about the future directions of wind tunnel modeling, the Argonne people have been asked to make a thorough feasibility study of such new facilities based on present knowledge of this field. The decision to build a major new wind tunnel for meteorological research should be held in abeyance until the Argonne study, presently undertaken by Cornell Aeronautical Laboratory, has been completed.
REFERENCES


Int. Symposium on Small-Scale Phenomena in the Atmosphere,
Moscow, September.

particles in Brownian motion," J. Colloid Sci. 20, 123-144.

particles in Brownian motion," J. Colloid Sci. 20, 123-144.

Hidy, G. M., and E. J. Plate, 1966: "Wind action on water standing in a
laboratory channel," NCAR MS No. 66, to be published in J. Fluid
Mech.

particles in Brownian motion," J. Colloid Sci. 20, 123-144.

Horgani, V. G., 1965: "Determination of the trapping coefficient of
cloud particles of comparable size by a model experiment,"

Houtappel, R. M. F., H. Van Dam, E. P. Wigner, 1965: "The conceptual
basis and the use of the geometric invariance principles," Rev.
Mod. Phys. 37, 595-632

Houtappel, R. M. F., H. Van Dam, E. P. Wigner, 1965: "The conceptual
basis and the use of the geometric invariance principles," Rev.
Mod. Phys. 37, 595-632

on Rossby waves in a rotating annulus," to be published in Tellus.

on Rossby waves in a rotating annulus," to be published in Tellus.


Levin, L. M., and Y. S. Sedunov, 1966a: "The theoretical model of
condensation nuclei; The mechanism of drop formation in clouds,"
paper presented at the IUGG Conference on Condensation Nuclei,
May.

Levin, L. M., and Y. S. Sedunov, 1966a: "The theoretical model of
condensation nuclei; The mechanism of drop formation in clouds,"
paper presented at the IUGG Conference on Condensation Nuclei,
May.

Levin, L. M., and Y. S. Sedunov, 1966a: "The theoretical model of
condensation nuclei; The mechanism of drop formation in clouds,"
paper presented at the IUGG Conference on Condensation Nuclei,
May.

Panel on Weather and Climate Modification, 1966: Weather and Climate
1350, National Academy of Sciences-National Research Council,
Washington, D. C.

Panel on Weather and Climate Modification, 1966: Weather and Climate
1350, National Academy of Sciences-National Research Council,
Washington, D. C.

481 pp.

481 pp.

concept of available potential energy," Proc. of the Int. Symposium
on the Dynamics of Large-Scale Processes in the Atmosphere, Moscow,
June. (See also Report No. 66-1, Department of Meteorology, Florida
State University.)

concept of available potential energy," Proc. of the Int. Symposium
on the Dynamics of Large-Scale Processes in the Atmosphere, Moscow,
June. (See also Report No. 66-1, Department of Meteorology, Florida
State University.)


Appendix A

LECTURES AND DISCUSSIONS OF THE PARTICIPANTS IN THE COLLOQUIUM
1. CHARGED PARTICLE STREAMS IN THE EARTH'S MAGNETIC FIELD

(Lecture of W. H. Bennett)

The aurora is one of the more intriguing phenomena that one can observe in the earth's atmosphere. Although the exact causes of the aurora are not fully understood, it is believed that this phenomenon is closely related to the effects of charged particles from extraterrestrial sources impinging on the earth, and interacting with the planet's magnetic field. A particularly interesting possibility for laboratory study of atmospheric physics lies in the simulation of the aurora. This discussion outlines briefly one of the more successful attempts to do this kind of simulation. In addition, certain more recent fundamental work in physics is examined in the light of possible mechanisms for auroral phenomena.

In 1953, four years before Sputnik, we at the Naval Research Laboratory became interested in the possibility of checking in a laboratory device the idea that beams of protons of 100 to 1000 km width, coming from the sun and interacting with the earth's magnetic field might be responsible for the aurora. It was determined that one could simulate solar beams of protons impinging on the earth using electron beams directed towards a small spherical magnet (Bennett, 1959a).

Prior to our work, many orbits of protons had been calculated, notably by Störmer (e.g., Bennett, 1959b), but many others had not been calculated. In none of that prior work had studies been made on beams of finite diameter.

The principles of similitude were applied to the governing equations for the dynamics of charged particles to scale the laboratory experiment to the geophysical conditions. The similitude relations
applied equally well to both non-relativistic and relativistic particles because the interaction between the particles and the magnetic field does not involve an energy change of the particles.

One process which could not be simulated precisely in the laboratory was the pinch effect in the beam. However, there is an analogy between electrically pinched beams and magnetic pinched beams and since our streams in the Störmertron were electrically pinched, at least qualitatively, we were indeed modeling pinch effects in the beams. In any event, we really modeled the orbits of charged particles in a magnetic field similar to the earth's.

After we had completed the study of orbits around a magnetic sphere, it was shown that a simple model of proton streams entering the earth's magnetic field is inadequate to explain the aurora. The electromagnetic processes in the atmosphere are more complicated than the older simple ideas. Current theory leads, for example, to concepts of electromagnetic shock fronts resulting from the impact of neutral charged clouds with the earth's magnetic field, and observations suggest the existence of "shadow cones" and other effects around the earth. These kinds of interaction are not observed in the simple beam model. Nevertheless, some recent advances had been made which would make it appear that the old beam-orbital model for streams should be reconsidered.

The laboratory device for studying the interaction between electron beams and a magnetic sphere, called the Störmertron, was arranged so that the beam could sweep the sphere, simulating the change in orientation of the beam due to the rotation of the sun. The orbits of the electrons and the stream forms were traceable in full detail because the glass enclosure around the sphere was filled with mercury vapor.

The best records of the orbits of the charged particles were obtained by time lapse photography of the Störmertron. A set of slides
and movies show the striking effects that were observed when a "pencil" of electrons in a beam encountered the magnetic field of the sphere.

When the electrons were beamed towards the sphere, the particles spiraled inwards and around the sphere until the beam made contact. Contact was made first in the auroral altitudes on the dawn side of one hemisphere. Later the beam would back away and recontact the ball at "midnight" at auroral latitudes on the opposite hemisphere.

Many of the details of the particle orbits were not found in the theoretical predictions. Some features of the detailed behavior of the particles, especially at contact, were a surprise even to Störmer himself, who spent most of his life working on the theoretical calculations.

In addition to the primary orbits, we found that particles could be trapped in belts around the sphere. These Störmer "ring currents" which developed were identical with the Van Allen radiation belts as they are now known. At first, we thought the observed trapped orbits meant that the device was not behaving properly. Later, however, we realized that the Störmertron was actually giving us a display of the effects of scattering as well as the primary interaction. This detailed structure was not suggested at all by the theory which existed at the time of the experiments.

The Störmertron showed how a beam of charged particles could interact with a magnetic field around a sphere and make contact with the earth only at the right locations, and at the right times of day as observed for the aurora. This generally has been ignored by recent studies of the mechanism of the auroral phenomenon. The newer theories based on electrically neutral clouds of particles approaching the earth inherently require that the aurora should occur predominantly on the daytime side of the earth. Some new experiments by Baker and Hammel (1965) have opened up a quite new approach to the development of a theory of the aurora.
Baker and Hammel have recently observed the penetration of an electrically neutral plasma beam into a transverse magnetic field and found that the magnetic field would not stop the beam at magnetic field intensities which if previously had been supplied would have been sufficient. However, if a small bit of conductor was placed off to one side of the plasma jet the part of the plasma encountering lines of force passing through the piece of conductor stopped dead while the rest of the plasma passed by. The observations may be explained as follows. As the electrically neutral plasma passes the mirror coils, it becomes polarized. This in turn produces an electrical field that just neutralizes the magnetic force of the mirror field. Thus the electrically neutral plasma may pass through the magnetic field at mass densities of particles $n$ given by

$$n \gg \frac{B^2}{4\pi mc^2} \quad (1)$$

where $B$ is the field strength, $m$ is the particle mass, and $c$ is the velocity of light.

The second observation of Baker and Hammel involves the generation of electrical currents spiraling along lines of magnetic force. This short circuiting currently eliminates the electrical field produced by charge separation in the plasma, allowing the $\vec{v} \times \vec{B}$ force to have full effect on the plasmas. In this case the plasma is stopped by much smaller magnetic fields:

$$n \gg \frac{B^2}{4\pi mv^2}$$

where $v$ is the velocity of the plasma. This suggests that the criterion (1) currently used for stopping a plasma may be in error by as much as a factor of 500.

These fundamental observations have some rather important
implications in understanding the processes which produce the aurora. Suppose we consider only the upper regions of the atmosphere where ionized particle streams interact with the earth's magnetic field.* Streams of electrically charged, neutral (equal density of positive and negative charged particles) clouds encounter the earth's magnetic field. Because of the arguments above, the particles first could penetrate to much lower altitudes than predicted by Eq. (1). During penetration, there is separation of charge in the stream, possibly causing short circuiting currents to be set up in the conducting part of the atmosphere. Under this condition, penetration may be stopped at higher altitudes than anticipated by the action of the magnetic field alone. The existence of the short circuiting currents in the atmosphere could return us to the older "beam" model.

Because the short circuiting currents are indeed currents moving in the earth's field, then it follows that the Störmer considerations, leading to the result that all latitudes and longitudes are forbidden except those at which the aurora occur, will apply.

I believe that it is now time to re-evaluate many aspects of auroral theory in the light of the old Störmertron work, and the new results of Baker and Hammel. Perhaps the best route to proceed on this class of geophysical problems lies in new, good theoretical work combined with basic laboratory studies of steadily generated plasma beams at low mass densities, and relatively low electrical currents. After the basic work, there may be new clues to better methods for simulating in the laboratory electromagnetic processes on a geophysical scale.

*The question of the auroral phenomena at lower altitudes, where ionization and excitation of neutral particles occurs, is much more difficult. It is unlikely that this region can be modeled because of limitations in the range of excitation levels in real gases.
In one of the first seminars of this series it was proposed that simulation experiments could be divided conveniently into direct and indirect studies. Direct simulation involves an attempt to reproduce some atmospheric behavior in the laboratory, and to apply the results directly back to the atmosphere. Indirect studies, on the other hand, assume that a worker knows certain processes occur in the atmosphere, and he designs experiments to obtain as accurately as possible classes of data needed to specify quantitatively the details of these processes. Since most of the talks in this series so far have been concerned with the direct methods, I want to concentrate on indirect simulation with particular reference to atmospheric chemistry in this discussion.

There is a variety of chemical reactions that occurs in the atmosphere. Although many reactions are quite important to different aspects of atmospheric science, relatively little is known about them in a fundamental sense. Fortunately, it is possible in many cases to study atmospheric reactions under laboratory conditions, and to extend these results to atmospheric conditions by well known methods of thermodynamics and chemical kinetics.

Interest in groups of chemical systems that may be studied in the laboratory can come up for several reasons. First, knowledge of reactions can provide information for estimating the concentration of reactive species in the atmosphere where no other means for obtaining their concentration exists. Second, if one wants to estimate certain reactions like those involved in production of photochemical smog, studies of various reactants and reactions give clues to controlling mechanisms of smog components. And third, concentrations of certain
trace materials even obtained indirectly can lead to information about circulation of the atmosphere.

Nearly all reactions in the atmosphere are either primary or secondary photochemical reactions. Primary reactions involve direct absorption of photons, while secondary reactions proceed through interaction between various interacting ions, atoms or molecules.

To illustrate the kind of information needed for specifying the kinetics of a chemical reaction, let us consider a general secondary reaction,

\[ aA + bB \rightarrow C + D \]

where \( a \) and \( b \) are stoichiometric coefficients, \( A \) and \( B \) are reactants, and \( C \) and \( D \) are products. The rate of disappearance of \( A \) can be written

\[ \frac{d[A]}{dt} = k [A]^a [B]^b, \]

where \( k \) is the rate constant and the bracketed quantities denote concentration of species. The rate constant is generally a function of temperature given by

\[ k = P(T)e^{-E/RT}, \]

where \( R \) is the gas constant, \( E \) is the activation energy, and \( P(T) \), weakly dependent on temperature, contains the effective probability of reaction once species \( A \) and \( B \) collide. Thus in general, one needs to know the concentration of species, and the rate constant as a function of temperature to describe reacting systems.

If the rate constant is determined in the laboratory, it is possible to find a concentration of a particular species, say an excited one, by applying a classical technique of chemical kinetics. To illustrate this, consider the reactions

\[ A + B \rightarrow C + D, \]
C + E → products,

where C is the excited species. The rate of disappearance of \([A]\), then is:

\[ \frac{-d[A]}{dt} = k_1 [A][B], \] (1)

and the rate of disappearance of \([C]\) is:

\[ \frac{-d[C]}{dt} = k_2 [C][D] + k_3 [C][E] \] (2)

Assuming that steady-state conditions exist, \(-d[A]/dt = d[C]/dt\), then from Eqs. (1) and (2),

\[ [C] = \frac{k_1 [A][B]}{k_2 [D] + k_3 [E]} \]

Thus, if concentrations of stable species are known, the concentration of the unstable body may be determined, provided the reaction constants have been obtained independently.

The assumption of steady state in the atmosphere is sometimes partly true, and sometimes not true at all. Nevertheless, this technique has been used effectively for approximately estimating concentrations of atomic oxygen, atomic nitrogen, ozone, and several other unstable materials.

The question of the applicability of the steady-state assumption and the use of \(k\) values to atmospheric chemistry is of particular importance. Information about the rate constants has to be obtained in the laboratory under conditions remote from the atmosphere. A number of key factors have to be taken into account in using the laboratory data. For example, the steady-state solution implicitly requires that the energy in reacting species have an equilibrium or Maxwell-Boltzmann (MB) distribution. Under conditions where "hot" atoms or molecules exist in the atmosphere, this requirement is not fulfilled. In the absence of detailed information of atmospheric conditions, the
Appendix A-2

(MB) assumption constitutes a reasonable starting point for studying chemical reactions.

There are other pitfalls involved in using laboratory data. For example, the extension of rate constants to wide ranges of temperature assumes that the activation energy is constant. Under extreme temperature ranges, $E$ may change somewhat with temperature. However, this difference usually introduces a small error under normal application. It is also observed that $k$ may be sensitive to pressure changes. Careful laboratory experimentation is vital in these cases to determine the region where $k$ may be insensitive to pressure. Pressure effects often come into cases where a foreign body $M$ is involved in a reaction. For example,

$$M + A + B \rightarrow C + D + M.$$  

Here the nature of $M$ comes in despite the fact that $M$ is on both sides of the reaction equation.

Laboratory studies are usually carried out under conditions where the surface to volume ratio of the reaction vessel, $S/V$, is relatively large. This means that one has to be careful about obtaining data for a heterogeneous reaction when you think you are studying a homogeneous reaction. Atmospheric reactions, of course, are virtually all homogeneous in nature. Results for heterogeneous reactions can only give the maximum rate of change in concentrations to be found for analogous systems in the atmosphere.

In the study of primary reactions, say

$$h\nu + A \rightarrow \text{products},$$

some difficulties arise. For example, the rate of disappearance of $[A]$ is given by

$$- \frac{d[A]}{dt} = f\phi [A],$$

where $\phi$ is the photochemical yield, the fraction of photons absorbed
that are actually involved in producing a reaction. And $f$ is the specific rate constant, equal to the rate of absorption of photons per molecule or atom over the range of wavelength in which the species A is absorbing. Both $f$ and $\phi$ are nearly independent of temperature, fortunately. However, $\phi$ may be slightly dependent on pressure. Worst of all, $f$ is difficult to measure and calculate for atmospheric cases because this function involves the spectral distribution of radiant energy at each level in the atmosphere.

Our studies in the laboratory deal primarily with secondary photochemical reactions, and the determination of rate constants. We also have interests in certain classes of primary reactions. Some theoretical studies are being undertaken to apply our laboratory data to atmospheric conditions, and try to determine where the (MB) distribution and steady-state assumptions may be applied to the atmosphere. Illustrations of results of several studies are given by Allen and Cadle (1965), Cadle and Ledford (1966), Cadle and Allen (1965), and Cadle (1962, 1963).

Several examples of rate studies, primarily in laminar flow reactors, serve to illustrate some of our experimental techniques, as well as some consequences of our results (for details, see references listed above).

From samples of these investigations it is clear that our experiments generally are small in size. These indirect studies do not require a long-term use of a large facility. However, we could use a large, environmentally controlled room from time to time. Such a temperature controlled box, with gas sampling outlets, would provide us with a means of reaching much lower values of $S/V$. A large vessel has drawbacks, of course, in that it is difficult to vary $S/V$ much. A large tank also would be useful for making mixtures of gases at very low partial pressure for one species. There are some interesting spectroscopic investigations that could be done on a large volume.
The use of a large chamber for such cases is needed primarily because greater intensity of light can be attained in some cases in the larger volumes.

DISCUSSION

Although aerosols are always present in the atmosphere, it is unlikely that these particles have a catalytic effect on most atmospheric reactions. These concentrations are generally too low to give enough surface area to be significant.

There is another aspect to the aerosol problem. One also has to consider the actual reaction of an atmospheric gas with the particles, as contrasted with the catalytic effect mentioned above. For example, the reaction of nitrogen dioxide with sea-salt particles liberates hydrogen chloride into the atmosphere.
3. WIND TUNNEL SIMULATION OF ATMOSPHERIC PHENOMENA

(Lectures of J. E. Cermak)

Wind tunnels have proved to be very useful tools for modeling certain kinds of atmospheric phenomena. The effective utilization of wind tunnels for modeling purposes depends largely on the successful application of the principles of geometric and dynamic similarity. In the two discussions to follow, several concepts and experiments will be described which deal primarily with modeling of atmospheric surface layers. The first talk concerns the mean motion that develops in turbulent shear layers, while the second talk covers certain aspects of the structure of turbulence, and the diffusion of heat and matter in shear layers having thermal stratification.

LECTURE A

Recently a large meteorological wind tunnel has been built at the Fluid Dynamics and Diffusion Laboratory, CSU. This facility has been designed to simulate the behavior of atmospheric surface layers with various degrees of thermal stratification. The floor of this wind tunnel can be heated or cooled to give flows different stability. The test section is square in cross section (1.8 by 1.8 m), and is about 25 m long to allow the turbulent boundary layer developing along the floor to grow naturally, and to approach an equilibrium configuration where the flow properties remain nearly invariant in the streamwise direction.

The limitations for modeling fluid motion of surface layers in the laboratory center around the magnitudes of dimensionless parameters, the Rossby number $R_o$, the Reynolds number $Re$, and the Froude number $Fr$ (which is analogous to the Richardson number $Ri$). Since the wind tunnel has no horizontal curvature and does not rotate, the scale of
motion to be modeled must be confined to the regime where $Ro > 1$ or larger. This means that only small-scale to microscale atmospheric motion can be modeled. The difference in $Re$ between model and prototype sometimes may be as high as $10^3$. Using the heating and cooling in the tunnel, equality of the $Fr$ between model and prototype can be taken care of within an order of magnitude.

In modeling flow around small structures in the lowest meters of the atmosphere, the Reynolds criterion usually can be met. Motion involving larger length scales can be studied when looking at flow over or around sharp-edged bodies. In this case, the viscous effects are small away from a region very near the boundary. If it is assumed that the molecular viscosity can be replaced by a turbulent eddy viscosity in defining $Re$, the motion on still larger length scales can evidently be modeled, at least in appearance of the gross features.

Some examples of a reasonably successful modeling of flow around sharp-edged structures include our work in Candlestick Park near San Francisco (Cermak, et al., 1963) and the World Trade Center Building to be constructed in New York. Here, it was found that the basic flow patterns were the same even though the ratio of Reynolds number, prototype $Re_p$ to Reynolds number, model, $Re_m \approx 10^2 - 10^3$.

For a model of flow on a larger scale such as our studies of Pt. Arguello, California, of San Nicolas Island, and for lee wave formation over simple two-dimensional triangular "mountains," the ratio $Re_p/Re_m \approx 10^4$. However, the gross features of the model flow near these kinds of topography appear to be similar to the natural flow, provided the Froude criterion is met. Even gravity waves are observed to the lee of the mountains, and secondary flow up valleys is found. This seems to provide evidence for the applicability of a turbulent Reynolds number criterion since the turbulent Reynolds number ratio between prototype and model in these cases is about unity.

There remain problems in the use of an analogy between Reynolds
criteria based turbulent and molecular viscosity. For example, the eddy viscosity varies spatially. This feature cannot be modeled properly in the wind tunnel. Furthermore, care must be taken to insure that the viscous effects near the surface of the model are well understood before this kind of technique will be completely reliable. Gross features may be consistent between model and prototype distortions in horizontal and vertical length scales involved here.

In dealing with modeling in boundary layers, two additional criteria have been proposed, particularly with reference to a quasi-geometrical similarity. First, an analogy of atmospheric flow around a body requires that the object must be well submerged in the surface layer. That is, the thickness of the boundary layer must be greater than the height of the object \( h \). Second, Jensen (1954) has proposed that the ratio \( h/z_0 \), where \( z_0 \) is the length characterizing the roughness of the boundary, must be the same in the model and the prototype. Jensen has tested this hypothesis with some success. The first criterion involves the generation of thick boundary layers in the tunnel. This can be done either by using a long tunnel like ours, or the layer can be artificially thickened at the inlet by using grids, and screens. The latter cases are being studied by Strom and colleagues at NYU. The second criterion can often be met by artificially roughening the floor of the tunnel to match the \( h/z_0 \) ratio properly.

The problem of modeling unsteady flows has not been studied in any detail. However, the people at the University of Michigan evidently have done some preliminary work in this area, and there have been some experiments reported by Karlsson (1959).

LECTURE B

In this session, we shall discuss the application of wind tunnel experiments to the study of turbulence in atmospheric surface layers.

Modeling turbulent flow so far has centered attention on the similarity theories for shearing flows. These theories generally require
the turbulence to be homogeneous in the direction of flow. Hence, the long test section is necessary again to insure boundary layer development to a point where there is little variation in turbulence properties in the downstream direction.

Basically, the modeling of atmospheric turbulence appears to require similarity between the mean velocity profile, a Reynolds number based on turbulent intensity and a length scale, and perhaps the dissipation rate $\epsilon$.

In our studies, most of the effort has been concentrated on cases of "level ground" where the surface roughness height is much less than the boundary layer thickness. It has been useful to apply the theoretical development of Monin and Obukhov (1954), who noted that essentially three quantities, which are roughly independent of height, should control turbulence near ground. These are: the ratio of gravity to temperature $g/T$, the shear velocity $u^*$, and a heat transfer parameter $H/\rho_c p$, where $H$ is the heat flux, and $\rho_c p$ is the product of density and heat capacity. The mean velocity and temperature distribution can be written in similarity form. For example, the mean velocity reads:

$$U = \frac{U^*}{K} \left[ f \left( \frac{Z}{L} \right) + f \left( \frac{Z^2}{L} \right) \right],$$

where $L$ is the Monin-Obukhov length scale. The length $L$ contains the three parameters above. Unfortunately $L$ is difficult to measure directly, so another parameter, $R_i$, often is used to characterize the turbulent flow. Values of $R_i$ depend on the height so that this parameter is not completely satisfactory either.

Using either $L$ or $R_i$ as characteristic parameters, suitably defined for flow in the wind tunnel, the turbulence in thermally
stratified motion for boundary layers can be studied, and the laboratory results can be compared with corresponding field measurements. Our program has just begun in this area, but we have some preliminary results that are of interest.

Data for the vertical component of motion are presented for various heights above the boundary, for Ri ≈ 0.02 and 0.5. These results are compared with the field work of Gurvich (1960), who used a sonic anemometer to measure vertical velocity fluctuations. Frequency spectra for the velocity are also presented and are shown with field data taken under field conditions. Agreement between the wind tunnel results and the field studies is moderately good. The lack of agreement may be related to differences in Reynolds numbers between these studies. These results are discussed in more detail by Cermak and Chuang (1965).

Another important problem associated with turbulent flow in shear layers is the diffusion of heat and mass. Our studies of diffusion center attention on the application of the Lagrangian similarity hypothesis to modeling of particle dispersion in atmospheric layers. This hypothesis again deals with properties of turbulence which are homogeneous in the streamwise direction, and the theory leads to a statement that two parameters h_s/L and z_0/L are of primary importance for diffusion (h_s is the height of the source). The key then to modeling diffusion using this approach lies in the ratio h/z_0. This, of course, is analogous to the hypothesis of Jensen. Some of our data from wind tunnel tests, and some results from the Prairie Grass studies have been correlated within the framework of the Lagrangian similarity hypothesis. These data are discussed briefly. The agreement between theoretical predictions, model studies, and the available field results appears to be satisfactory. For the details, see Cermak (1963).
4. MODELING THE EFFECTS OF SMALL-SCALE EDDIES
IN NUMERICAL STUDIES OF TURBULENT FLOW

(Lecture of J. W. Deardorff)

Most of our efforts in this Colloquium have been directed towards laboratory experiments and modeling. However, it is of interest to cover briefly a few special problems in numerical modeling, particularly with a view of the interplay between basic laboratory studies and progress in numerical calculation of fluid motion.

In one of their greater contributions to the understanding of turbulent flow, laboratory studies have shed light on the behavior of the fine-scale eddies of motion. One of the key problems in numerical modeling of turbulent flow in the atmosphere has involved the question of dealing with the effect of small eddies whose scale is less than the grid scale of the finite differencing scheme. Accurate computation of the behavior of the fine structure of motion is essential to this kind of motion because this region usually contains the major contribution to the dissipation of kinetic energy of the flow. A combination of theoretical reasoning, numerical testing, and laboratory experiments is providing some interesting new directions to pursue for solving the problem of accounting for the effects of the small eddies.

In this talk, the general scheme for integrating numerically the small-scale averaged Navier-Stokes equations for turbulent flow is reviewed with reference especially to the problems of representing the small-scale Reynolds stresses. With some misgivings, the usual procedure is adopted here of relating these Reynolds stresses to the larger-scale motions.

Using the concept of an eddy viscosity and Kolmogoroff's hypothesis for the dissipation of turbulent kinetic energy, the semi-
empirical scheme of Lilly (1962) is presented for dealing with the effects of small eddies in a numerical integration.

When an inertial subrange exists, the eddy viscosity $K_i$ in the $x$, $y$ or $z$ coordinate is given by the relation:

$$K_i = \left( k \Delta x \right)^{4/3} \varepsilon^{4/3}$$

where $k$ is a constant, $\Delta x$ is the grid spacing in the $x$, $y$ or $z$ direction, and $\varepsilon$ is the dissipation rate of turbulent kinetic energy. Once values of $k$ are known or assumed, $K_i$ can be calculated from the numerical model by implicitly evaluating $\varepsilon$.

Recent numerical studies indicate that $k \approx 0.23$ if an inertial subrange exists. In the absence of an inertial subrange laboratory studies of small scale turbulent convection suggest a value of $k \approx 0.3$.

The use of Eq. (1) may be extended to cases of shearing flow near a boundary if we assume the distribution of mean velocity near the boundary to be logarithmic. Under these conditions, $K_z$ increases with distance $z$ from the boundary, and exceeds the value from (1) when $z = \Delta z$, the grid interval. The problem centers around the merging of these values of $K_z$ with those of Eq. (1) at greater distances from the boundary. Possibly $K_x$ and $K_y$ can still be given from Eq. (1) even when $K_z$ is obtained by this different method.

The scheme using the principles involved in Eq. (1) has been applied to a three-dimensional numerical model of plane Poiseuille flow. Preliminary results of this model are discussed for cases above and below the critical Reynolds number for this type of flow. The numerical integration gives amplification of disturbances suggesting a quasi-turbulent motion for a Reynolds number above the critical value. The results for the mean motion, the turbulence intensities, and the Reynolds stresses are examined and compared with some experiments of channel flow by Laufer. For the details, see Deardorff (1965).
AN ELECTRIFIED CLOUD SIMULATION

Convective processes in clouds have been simulated in a number of hydrodynamic models, and many interesting results have been obtained from these studies. It appears, however, that certain features of cloud behavior also may be investigated in clouds of water droplets on a laboratory scale (particularly the effects of electrification, and light scattering or transmission). One interesting experiment of this kind has shed some light on the perturbations in electrical fields associated with thermal penetrations through large thunderstorm anvils (Eden and Vonnegut, 1965).

It is easy to generate clouds of water droplets in several ways. One method with which we have had success consists of dropping dry ice (solid $\text{CO}_2$) into warm water. A dense cloud of droplets then is generated by condensation of water vapor that has been released by heating of the water (from heat of solution of the dry ice). This cloud will persist for several minutes. The droplet distribution in such clouds is relatively narrow, in ranges from 3 to 8 $\mu$m radius, while the liquid water content is about 20 gm/m$^3$. This yields a cloud whose concentration of droplets is $\sim 10^6$ cm$^{-3}$.

The observations of the laboratory clouds were made in a tank 1.5 m square by 1 m high. The clouds, 0.3 m high, were located in the bottom of the tank. The droplets were charged by point discharge from a high tension probe. To simulate the ionosphere, a charged polonium strip was suspended above the charged cloud. We then made measurements of the current flow between the suspended strip and the cloud.
Initially a small current flowed which decayed to zero, presumably as the result of the induction of a sheathing layer at the top of the charged cloud.

If, after the decay of the initial current flow, a turret or a thermal is induced to penetrate and to perturb the sheathing layer, a new transient current flow is measured from the polonium strip to the cloud. After the sheathing layer becomes re-established the current again reduced to zero. This behavior is analogous to changes in atmospheric potential gradient near turrets penetrating through thunderstorm anvils as measured from recent U-2 aircraft flights in Florida (Vonnegut, et al., 1966).

It may be possible to extend these results to more quantitative analogies, including, for example, studying the thickness of the sheathing layer.

**PROPERTIES OF CONVECTION IN ROTATING SYSTEMS**

A number of experiments have been undertaken recently to try to understand better the dynamics of heated fluids in a rotating annulus. The work of Fultz and his colleagues has indicated that the fluid motion in such systems appears to be closely related to the large scale circulation in the atmosphere, provided that the Ro numbers are the same for the model and the prototype.

In parallel with the work of Fultz' group, Hide and co-workers have carried out similar experiments in rotating systems to map out the properties of (laminar) flow in a rotating annulus over the regions of steady, axially symmetric Hadley circulation, and steady Rossby-like wave development. Several general features of the convective fluid motion and the associated temperature fields are described for these regimes. The behavior of the boundary layers which develop near the hot and cold walls of the annulus are also discussed. For the details, see Bowden and Eden (1965).
Perhaps the studies of greatest meteorological interest in use of the rotating hydrodynamic models came several years ago. The early results were largely qualitative, but provided considerable insight into the behavior of fluids at different Rossby numbers. There are many experiments of this kind which remain to be done. However, they will probably be of primary interest to fluid dynamicists, and experimenters who want to check results of numerical models with simple experiments. It would certainly be worthwhile to extend the type of studies described for example in Bowden and Eden (1965) to the unsteady wave regimes and to rotating systems in turbulent flow.
LECTURE A: EKMAN BOUNDARY LAYERS AS ANALOGS OF PLANETARY BOUNDARY LAYERS

Analogs or models, if you want to make the distinction between these concepts, may arise in two different ways: by design or by chance. Experiments evolving by design usually fall between two extremes. On one side, laboratory study is undertaken to test conceptual theories or simplified mathematical models, and on the other side, experiments are carried out, perhaps naively without theoretical basis, to try and construct analogs to certain kinds of atmospheric motion. During some investigations, a worker may recognize a "new" circulation by chance which looks very similar to a geophysical prototype. Since many of the previous discussions have dealt with experiments by design, I want to place emphasis in this lecture on the second category, chance discoveries.

To illustrate how an analog may develop by chance, the case of instabilities in an Ekman boundary layer will be treated in relation to the roll vortex structure in the planetary boundary layers in the atmosphere, and in the oceans.

Our study of (laminar) Ekman layers arose from some work of Stommel, et al. (1958), in which a model was developed to test Stommel's conceptual theory of general ocean circulation. In this experiment, a sector of a rotating tank was used to simulate an ocean basin. The rotation of the tank caused a variation in water depth with tank radius, which essentially simulates the $\beta$-effect in homogeneous fluid motion over a plane surface. When a source and a sink of fluid are established in such a sector, a western boundary current is
observed, which is analogous to similar currents in the oceans. These boundary currents thus are believed to be the direct consequence of the $\beta$-effect.

During this study, it became apparent that the Ekman layer at the bottom of the tank was very important to the behavior of the circulatory structure in some cases. Therefore, we continued these experiments using the entire rotating tank with a sink at the axis of rotation, and a source distributed uniformly about the rim of the tank. These studies indicated that the Ekman layer at the bottom dominated the circulation in the tank. That is, the primary horizontal flow from the source was a spiraling motion in towards the sink, all within the boundary layer.

The Reynolds number for the Ekman layer of depth ($D = (\sqrt{\Omega r})^2$), where $\nu$ is the kinematic viscosity of the fluid and $\Omega$ is the rotation rate, is proportional to $1/r$. Therefore, a critical Reynolds number, $Re_{cr}$, is reached towards the axis of rotation, in the laminar Ekman layer where there is a transition to unstable modes of flow. Later, of course, there may be a second transition to turbulence. The instabilities were observed in the bottom layer of the tank by dye traces. Spiral bands of dye were found to develop at an angle to the "geostrophic" flow. The bands are related to rows of vortices superimposed on a steady, horizontal flow. The first class of unstable motion to be observed, Type I, consisted of band spiraling inward towards the center of the tank at angles of $\sim 14^\circ$ to the geostrophic flow, with wave lengths of $\sim 11D$. These turned out to be related to regions of inflection in the mean vertical profiles of horizontal velocity. A second independent class of unstable modes, Type II, also was found. These bands were oriented about $15^\circ$ to the right of the geostrophic flow with wave lengths of about 24 to 30D and traveled rapidly to the left of the geostrophic direction.

* This class has been called the parallel mode of instability by Lilly (1966).
Two similar analogies by chance came out of these basic studies to understand the behavior of the unstable Ekman layer. First, the banded structure observed in the rotating tank experiments suggested an analogy to the hurricane vortex. The observed cloud bands in hurricanes look quite similar to the dye structure in the rotating tank. To be sure the hurricane system is rather complex and is driven by a different mechanism than the experimental vortex. However, it is very interesting that the separation of cloud bands in the hurricane is $\sim 11D_{\text{hurr}}$, remarkably close to that found in the experiments. The connection between model and prototype here depends, of course, on the use of eddy viscosity and a turbulent Reynolds number compared to the "laminar" Re in the model.

The second analogy to be suggested is the relation between roll vortices in the atmosphere and in the mixed layer on the ocean, which tend to be oriented at a small angle to the wind direction, and change direction with the wind direction. The roll vortices in the ocean were first observed by Langmuir some years ago in connection with regularly spaced rows of seaweed on the surface of the water. The spacing of the wind rows on the ocean, combined with their angular orientation to the wind suggests a close relation to the instabilities associated with an Ekman layer in the water. Similar arguments can be proposed for the structure of the roll vortices in a "quasi-Ekman" planetary boundary layer in the atmosphere.

We also note that the use of the criterion, $(\text{turb})Re_{\text{prototype}} \approx (\text{lam})Re_{\text{model}}$, fits the geophysical observations and the model experiments satisfactorily. That is, with reasonable assumptions about the eddy viscosity, $Re_{\text{cr}}$ for the (turbulent) atmospheric layer is $\sim 500$ to 1000, and for the ocean $Re_{\text{cr}} \approx 100$. These ranges are the same as the laminar $Re_{\text{cr}}$ for the model.

To better understand the role of vortices produced by shearing flow instabilities, the connection between cells of thermal convection
and the behavior of Ekman layers has to be investigated. It has been suggested, for example by Kuo, that roll vortices in the atmosphere can be of thermal origin. We do not know at present the relative importance of the two mechanisms in establishing the observed helical cellular structure of circulation in the planetary boundary layer. However, one can guess that because of the greatly reduced importance of baroclinic effects in the oceans, the mechanism of shearing flow instability controls the wind row pattern of circulation. For further details, see Faller (1964).

One of the interesting features of our investigation of the instabilities of the laminar Ekman layer is the result of numerical studies which have accompanied the laboratory work. Our numerical computations for the modes of Type I and Type II instability, e.g., Faller and Kaylor (1966), and those of Lilly (1966), show agreement which is quite satisfactory with the experimental results.

We incidentally have found indications of a third independent mode of instability. Although relatively little has been done on this aspect of the Ekman layer, it appears that, for Type III, $\lambda \approx 3D$, and the angle is about 20 to 40° to the left of the geostrophic flow.

Our work on these problems is continuing. For example, we are investigating the roll vortices in the atmosphere and in the oceans through a field observation program off the coast of Bermuda to see to what extent the laboratory analog actually is similar to atmospheric and oceanic convection. New laboratory experiments of instabilities that develop in the Ekman layer near a free surface also have been undertaken. These studies are of particular interest because they have shed some light on the role of surface films in changing the direction of the surface stress, and in affecting the development of small surface waves.

Our experience with laboratory modeling resulting from "chance" discoveries has been quite illuminating as basic fluid dynamical
A. J. Faller studies, and, I believe, has certainly been at least partially suc-
cessful in pointing towards unexpected mechanisms for explaining geophys-
ical phenomena. I would recommend therefore that those who want to do
these kinds of experiments should go ahead and try, and see what may
be found. Sometimes qualitative results from these investigations can
be just as useful as the quantitative results.

LECTURE B: SOME UNPUBLISHED ATMOSPHERIC MODELS

Before going into some different experiments, there are one or
two additional comments to make about the Ekman layer study.

Reviewing the previous discussion, the modes of instability that
we have found number three. The second mode appears at a lower Re_{crit}
than the Type I mode. However, the Type II class travels very rapidly
as it grows so that unless an initial finite disturbance occurs in the
fluid, these bands of Type II will not be observed. Indeed, Gregory,
Stuart, and Walker (1955) failed to find the Type II mode for flow due
to a rotating disc because of this fact.

The transition to turbulence in an unstable Ekman layer occurs in
an interesting manner. Evidently, the onset of turbulence is related
almost entirely to the Type II band structure. As these bands develop
and travel outwards, waves appear on these bands which break down
quickly into turbulence. This breakdown is related to an interaction
of the Type II mode with the (distorted) mean flow rather than to the
Type III mode.

Cold Fronts

The next group of experimental analogies again arose more or less
by chance. These began in 1953 with the study of thermal convection
in a rotating tank, a dishpan experiment. During the investigation of
heat transfer under transient conditions, where the tank was heated
continuously at the rim, we found at low rotation rates, that very
reproducible systems of frontal disturbances appeared. These
propagating frontal discontinuities seem to be somewhat analogous to fronts in the atmosphere (see also Faller, 1956).

The lessons to be learned in these experiments are: that it is not necessary to have clouds and precipitation for fronts, and that cold continental air masses are not needed for fronts. However, friction in the bottom layer may well be required for forming fronts in the dishpan experiment. The interesting question comes up, of course, as to how closely the laboratory fronts are related to the atmospheric case. This can only be answered by a more detailed laboratory study of these transient experiments than I have been able to do so far. Needless to say these experiments are quite suggestive and worthy of further study.

General Circulation Analogies

The well known dishpan experiments of Fultz and Hide may be extended to at least qualitatively explore some features of atmospheric motion. The early experiments involved symmetric and constant sources of energy, and constant rotation rate. But there are many experiments that can be done using simple non-steady boundary conditions. For example, one might examine the seasonal variations and certain zonal asymmetries, or orographic influences. Such external conditions would superimpose irregularities on the circulation as developed by symmetric heating. Perhaps orographic effects will introduce favored locations for trough formation in the long Rossby wave, but irregularities in the shorter waves. It also should be possible eventually to introduce other modifications to account for feedback mechanisms involved in snow cover, clouds, ocean circulation, etc. Some time ago, I built an open centered rotating tank that was designed to explore some of these features, especially in the realm of sectorial heating and cooling. The preliminary results that were obtained are quite interesting and suggest some qualitative analogies to atmospheric circulations.

Under conditions of symmetrical heating at the rim of the
rotating tank without center cooling, we studied the temperature changes with time in the fluid. From the mean temperature and the perturbations in temperature it was possible to deduce some information about the potential energy cycle in the tank, in a manner similar to the circulation studies of E. Lorenz, N. Phillips, and others.

When the cold region at the center of the tank was added, the mean temperature distributions suggested conditions analogous to the stable temperature distribution associated with air over the cold Antarctic continent. Without cooling at the center, but with complete insulation including the top and bottom of the tank, a temperature distribution developed that was qualitatively analogous to an equivalent radiative loss from the entire body of the fluid. For the same rotation rate, an irregular five-wave pattern appeared for the insulated case while a nearly symmetrical one-wave pattern formed with the centered cold source in the experiment.

When there was evaporation at the "free" surface, a completely different small-scale cellular-convective pattern of flow developed. This particular case may be somewhat analogous to conditions of high cloud level and radiation from the upper troposphere.

The exploratory experiments using sector heating also yielded some interesting results. Two geometries were used: one consisting of two heated sectors and two insulated sectors, and the other, three heated sectors and three insulated sectors. In both cases, an analysis of streamline patterns indicated a behavior very similar to the circulation in winter as observed over an eastern side of a cold continent adjacent to a warm ocean. Examination of the zonal component of flow indicated zonal maxima near the east coasts of continents, as observed in the northern hemisphere. The variations in temperature also oscillated periodically in a frequency corresponding to the time required for short waves to travel from warm regions to cool regions. Similar periodic variations were found in the kinetic energy change, and the transform function for kinetic energy.
Although the meridional component of flow in these studies compared favorably with the atmosphere, the average zonal component in the experiment was considerably less than that in the atmosphere. This discrepancy may well be related to the missing feature in these pan experiments -- the $\beta$-effect.

In general, these dishpan experiments have revealed some very interesting features. Although the experimental results are complicated, they are sufficiently intriguing to do further, more detailed studies in this class of hydrodynamic systems. Particularly important in future work of this kind must be the quantitative checking of numerical models of the laboratory experiment with results from the laboratory experiment. One would be hard pressed to defend many of these experiments on grounds of dynamic similarity between the model and the atmosphere. Nevertheless, hydrodynamic analogies can be very useful for testing and extending conceptual theories as well as for suggesting new or different mechanisms for geophysical phenomena.

DISCUSSION

Rotating Tank Experiments

One of the important possibilities for the hydrodynamic experiments is the more effective use of the combination of numerical computation and laboratory experiment. Numerical models specifically designed to correspond to the hydrodynamic experiment can lead to insight into the observed motion on the one hand. On the other hand, this combination may give insight into the reliability of numerical integration schemes which later may be applied to the atmosphere. There are two classes of numerical-laboratory studies that can be done: (a) the transient problem where some "end" circulation is followed after given initial conditions, and (b) the steady-state problem,
where one examines the statistics of a fluctuating system after the initial conditions no longer influence the flow. Both kinds of studies should be undertaken. However, it does not seem necessary at this stage to go to very detailed comparisons between experimental flow patterns and numerical calculations. A careful comparison of statistical properties like the mean temperature distribution should be a sufficient "test."

I have begun combining numerical models without laboratory experiments, of course, in the Ekman layer work. In Stockholm, Sundquist and I worked together on a numerical model to predict flow patterns in a simple disturbed, barotropic, rotational flow in the laboratory. This case worked satisfactorily, with a predictive "skill" of ~90%. We have hopes of successively complicating such models to incorporate baroclinic effects, including irregular boundary conditions.

In connection with the general circulation analogies discussed in Lecture 6-B, we also have tried introducing orographic effects. The addition of a ridge in the tank, oriented with crest radially, produced circulations surprisingly similar to the large-scale atmospheric motion over North America. That is, a semi-permanent cyclonic low developed upstream of the mountains "near" the region equivalent to the Alention region, and over and over again disturbances would break away from a zone off the "west" coast and travel across the continent towards the "north-east."

It is worth noting that there has been a substantial increase of interest in rotating tank experiments. In addition to our program, there are groups working on such projects at the University of Chicago, Woods Hole, Harvard, Amherst, Columbia (Lamont Geological Observatory), University of Washington, Florida State and UCLA. I believe that the really large turntables exist only at Woods Hole, and at the University of Maryland.
The Facility Question

A versatile new facility might be useful to consider. Such a piece of equipment might consist of a turntable about 8 ft in diameter, accompanied by a good set of slip rings, and standard fluid dynamical instrumentation. The turntable and slip rings could be built for about $10,000 to $15,000. An apparatus of this kind would not represent a large expenditure, and could be operated in a way that would be useful to outsiders as well as to the NCAR staff. To use the turntable efficiently, one would require that different experiments be designed as units in such a way that they could be lifted on and off the turntable very rapidly. The change-over for a given experimenter should not require more than a few days.

A user could either design and build his own equipment to be placed on the turntable, or the central operator could make shop facilities available to users who supply a design on paper.

A general, flexible rotating platform of this type would certainly be welcome to workers who want to do certain experiments rather quickly. This kind of facility should see wiser usage as more groups get interested in these hydrodynamic studies and provide a supply of students, and interested personnel. The fact that the turntable might sit idle from time to time should not necessarily be a detracting factor in considering the equipment.

It is possible that a "co-op" system might work out as a national facility. The resources of individual groups specializing in certain equipment could be utilized effectively in this way, provided that certain precautions are taken to insure proper management of experimental programs. One of the main drawbacks to such a co-op system is that people may want to come to a central location like NCAR to work with certain people as well as a particular piece of equipment.
7. SOME METEOROLOGICAL MODEL EXPERIMENTS OF SEVERAL SCALES

(Lecture and Discussion of P. Frenzen)

C. G. Rossby, who can be considered one of the fathers of modern meteorological modeling (cf. Rossby, 1926), once wrote of his teacher, V. Bjerknes, that he combined an exceptional ability to reduce complicated physical problems to their essentials with a remarkable capacity to generate enthusiasm among those around him. Those who knew Rossby will recognize these qualities to be his own as well, while in the present context, it can be noted they also represent the attributes of a good meteorological model. Successful simulation of complicated geophysical phenomena in uncluttered laboratory experiments usually leads to clarification of analysis and understanding, while the ability of working meteorological models to generate enthusiasm (notably, the "dishpan experiment") makes these devices very effective teaching aids.

This presentation will outline three different meteorological model experiments related to as many different scales of atmospheric motion, the planetary, meso-, and microscales, respectively. Emphasis will be placed upon a few aspects of each model, these selected in order to illustrate some similarity (or lack thereof) with certain features of prototype circulations in the atmosphere.

WESTERLY FLOW PAST A BARRIER IN A ROTATING HEMISPHERICAL SHELL

Experiments in rotating shells at Chicago led to studies of relative flow past barriers mounted within these systems (Fultz and Long, 1951). It was found that although easterly flow remained relatively undisturbed, westerly flow produced meteorologically realistic patterns of long waves with periodicities in close agreement with a frequency equation given by Haurwitz (Long, 1952):
Here $m$ is the planetary wave number, while the kinematic Rossby number $\text{Ro}$ (the ratio between the angular velocity of the fluid relative to the obstacle, and the absolute angular velocity of the obstacle itself) represents a single similarity criterion for the model.

A motion-picture study of the westerly-wave case (Frenzen, 1955) found that wave amplitude varied considerably with the latitude of the barrier. A $20^\circ$ diameter obstacle at latitude $30^\circ$ produced strong, V-shaped troughs with a closed anticyclone in at least the first ridge downstream; at $45^\circ$ latitude, the same barrier produced weaker, U-shaped troughs and no closed anticyclones. With the obstacle at latitude $60^\circ$, no steady pattern appeared at all; but slow-motion pictures revealed that here a series of cyclonic vortices periodically formed and moved off in the wake, an observation which was qualitatively compared with Exener's barrier cyclone theory.

Time-lapse, "streak" motion pictures of increasing westerly flow showed that reduction in wave number occurs discontinuously, the last wave downstream gradually weakening while the rest of the pattern remained relatively undisturbed. When $\text{Ro}$ attains a value about midway between that appropriate to the old and the new stationary patterns, the length of the remaining waves begins to increase, eventually relocating the reduced pattern in a stationary position symmetrically located relative to the obstacle. This behavior permits a one-wave single vortex to first appear only about $100^\circ$ of longitude downstream from the obstacle, a pattern reminiscent of the eccentric circumpolar vortex found to occur with strong westerlies in the northern hemisphere. When it pulls out, the vortex centers in the Gulf of Alaska, very nearly $100^\circ$ downstream from the Himalayas (Frenzen, 1956; LaSeur, 1956). Since the model suggests low latitude barriers should be even more effective,
it was suggested that the Andes might produce similar eccentric vortex circulations in the southern hemisphere. Recent analyses (Van Loon, 1965) do show a tendency for a single wave at 500 mb, but the vortex is centered 150 rather than 100° from the Andes. Conceivably this could result from the superposition of effects of both the Andes and the lesser mountains of South Africa. Interestingly, the strongest phase of this pattern sets in during periods of maximum zonal index. However, Van Loon maintains the effect is largely of thermal origin.

The point of discussing this apparent model-prototype analogy is not to argue for a purely barotropic mechanism for eccentric circum-polar circulations in either hemisphere, but rather to illustrate the model experiment's ability to isolate dynamic effects. It would, for example, be useful to repeat the obstacle experiments, using heated sectors in place of physical barriers. After isolating large-scale thermal effects in this way, the barrier could be reintroduced and a study of combined orographic and thermal effects could be carried out.

In their enthusiasm, visitors often suggest that literal analogues of continents and oceans be included in the laboratory models. Some form of this procedure will certainly come in time (alternate warm and cold sectors have been used in the dishpan); but, in consideration of the state of our knowledge of the atmospheric prototype, the most useful virtue of meteorological models today is their simplicity. By continuing to isolate individual effects in well designed models, one can contribute to the understanding of the prototype; but, in consequence, only limited, direct comparisons can be drawn between the simplified experiments and the complex atmosphere.

CELLULAR CONVECTION IN FLUIDS UNIFORMLY HEATED FROM BELOW

Since high-altitude photographs confirm that several forms of quasi-cellular convection are prominent mesoscale features, laboratory models of these atmospheric modes could be quite useful. The most obvious small-scale candidate, laminar cellular convection, has been
known for some time. Following James Thomson’s famous identification of convection cells in a tub of warm, soapy water in 1882, Henri Bénard carried out a systematic study of the cellular mode in thin layers of liquid in 1900. But as early as 1882, Friedrich Vettin, a Berlin physician and amateur meteorologist, carried out some unusual cellular experiments with the specific intent of modelling cirrocumulus clouds. After introducing smoke between two horizontal, electrically charged plates, he observed that "the smoke organized itself in bands divided into many small cloudlets which climbed toward the upper plate and individually surrounded themselves with vortex rings" (Vettin, 1882). Note that, even though the instability mechanism was wrong (Vettin associated this form of convection with strong thunderstorm activity), this early attempt to model organized convection did succeed in "simulating" the circulation desired, a circumstance illustrating an important distinction between "simulation" and "dynamic modelling."

Beginning with the basic treatment by Rayleigh in 1916, theoretical analysis of cellular convection has been extended to include a variety of boundary conditions (summarized by Stommel, 1947), rotation (Chandrasekhar, 1953; Nakagawa and Frenzen, 1955) and non-linear effects (e.g., Stommel and Veronis, 1958; Kuo, 1961). More recently, efforts have been made to include in both laboratory and numerical models the additional instability contributed by the release of latent heat (c.f., A-19, J.S. Turner).

Besides the usual qualitative comparisons drawn between regular cells in the model and uniformly distributed fields of altocumulus or cirrocumulus in the atmosphere, it has been suggested that the simultaneous excitation of two cellular modes of slightly different wave length could generate two-dimensional, "beat-frequency" patterns of organized convection on the mesoscale (Frenzen, 1962). Two superimposed hexagonal cellular distributions could form patterns of regularly spaced cloud groups, each group with its strongest members in the center and the groups themselves uniformly located relative to one another on the
vertices of equilateral triangles. Individual cloud groups of this general appearance have been observed by Plank (1960) on days with insufficient vertical shear to generate rolls; however, the anticipated larger scale distribution of the groups themselves was not detected. Another "bimodal" cloud distribution could perhaps result from the modulation of a rectangular cellular pattern (associated with significant shear) by a train of gravity waves, a combination which conceivably could form large-scale convective bands similar to those seen to spiral around cyclones in satellite photos. Again, such bands would resemble the cloud groups in that the tallest cloud rows would lie along the bands' central axes with convective activity tapering off on either side.

Early criticisms of the lack of similarity between the cellular models observed in the laboratory and proposed convective prototypes in the atmosphere called attention to two purely qualitative defects:

1) realistic circulations with ascending central cores had only been observed in liquids; cores in gaseous cells always descended;
2) cells in the laboratory exhibited depth: diameter ratios of 1:3, whereas convective clouds suggested ratios of 1:1 or more.

An early suggestion that the opposite vertical circulations observed were due to the opposite temperature dependence of viscosity in liquids and gases was later confirmed by Tippelskirch (1956) in experiments with molten sulphur, a liquid whose viscosity variation reverses at 153°C. On the other hand, results of rotating experiments appeared to satisfy both qualitative objections. With the additional radial constraints imposed by rotation, it was found that steady cells with very large depth: diameter ratios could be created, only very weak rotational effects being required for atmosphere-like ratios of 1:1. Moreover, in both water and air, cell cores were observed to move either way, the direction only depending upon the position of the boundary
where instability first set in; cores always ascended when heat was added from below, and always descended when heat was removed from the top (Nakagawa and Frenzen, 1955).

Of course, the fundamental difficulty precluding true dynamical similarity between these laboratory cellular experiments and atmospheric convection arises from the fact that small-scale flow in the model is laminar whereas full-scale flow in the prototype is turbulent; thus the molecular transfer coefficients appropriate to the model (\( \nu \) and \( \kappa \)) are properties of the fluid, whereas the eddy transfer coefficients appropriate to the prototype (\( K_m \) and \( K_h \)) are properties of the flow. On occasion this difficulty is ignored by suggesting that an experiment might be considered an adequate model insofar as molecular transfer can be taken to represent a scaled version of eddy transfer in the prototype; but there remain second order problems such as:

1) \( \nu \) varies with mean temperature, whereas \( K_m \) does not;
2) \( K_m \) varies with the scale of the circulation, whereas \( \nu \) does not;
3) relative transfer ratios characterized in the atmosphere by and "eddy Prandtl number" \( \text{Pr} \equiv \frac{K_m}{K_h} \) exhibit a variation with fluid stability, whereas the laminar ratio, \( \text{Pr} \equiv \frac{\nu}{\kappa} \), does not.

In short, small-scale laminar convective experiments may be useful as simulators suitable for heuristic demonstration and teaching purposes, but they are not dynamically similar to full-scale phenomena in the atmosphere. To successfully model atmospheric convection, larger apparatus capable of sustaining quasi-steady, turbulent convection would be required. It might be noted in passing that the use of dense, cold gases in such experiments would grant significant advantages in the magnitude of the Reynolds numbers which the flow might attain. Even at normal temperatures, the value of \( \nu \) for carbon dioxide is only half that of air, while values for certain forms of butane approach one fifth that of air; operating at lower temperatures would
of course reduce these values further. Decreasing the scale requirements of a turbulent convective experiment by such exotic means could be quite useful, especially since it might well prove desirable to rotate the entire apparatus.

LAGRANGIAN TURBULENCE STUDIES IN A STRATIFIED FLUID

The dispersion of material particles in turbulent flow, an important practical problem in micrometeorology, is described analytically for the special case of stationary, homogeneous turbulence by the well known Taylor equation for "diffusion by continuous movements":

\[
x^2(T) = 2 \int_0^T \int_0^t R(\tau) \, d\tau \, dt
\]

Here the mean square displacement of an ensemble of dispersing particles \(x^2(T)\) at time \(T\) is expressed solely in terms of the (Lagrangian) intensity of turbulence \(u^2\) (assumed constant) and the Lagrangian autocorrelation function \(R(\xi)\), the average correlation of particle velocities with their own values after lag time \(\xi\). A series of experiments were carried out in the lee of a moving grid in a 30-ft towing tank at Argonne to measure the key factor \(R(\xi)\) over a range of initial turbulence intensities and fluid stabilities (Frenzen, 1963). Data taken in Lagrangian coordinates (successive, 1-sec displacements exhibited by small droplets of a neutral-density oil mixture suspended in the flow) were corrected for decay by a method suggested by G. K. Batchelor (1952). Since the experiments were restricted to the so-called "first period," the grid-produced turbulence was characterized by a velocity scale (rms velocity fluctuation) which decayed as \(t^{-1/2}\) and a length scale (representative eddy size) which increased as \(t^{1/3}\). It follows that one can define a time scale which is itself proportional to \(t\):
Batchelor's corrections then consist of
1) multiplying all observed velocities (1-sec particle displacements) by \( t^{\frac{1}{2}} \), and
2) dividing all time intervals by \( t \).
Here the time of observation \( t \) is measured from the "virtual origin" of the decay process. Note that integration of a consecutive series of corrected, infinitesimal time intervals defines a new, decay-corrected time for the experiment:

\[ \int_{t_1}^{t_2} \frac{dt}{t} = \ln\left(\frac{t_2}{t_1}\right) \]

With these adjustments, correlation estimates with identical lags do not use velocities separated by equal time intervals, but rather those whose central times are in equal ratios. In the experiment, velocity-decay corrected data collated according to this scheme did succeed in producing statistically stationary correlation estimates for constant values of \( \xi = \frac{t_1}{t_0} \). Actually, this experiment utilized data derived from a stationary, homogeneous turbulence field that never existed, except briefly as a decay-corrected data matrix in a computer.

Results obtained showed that, although the vertical component of \( R(\xi) \) was strongly affected by increasing stability (falling to zero more rapidly and changing its shape from exponential to Gaussian), the longitudinal and transverse components remained relatively unchanged. These effects were expressed quantitatively in terms of the variation of virtual diffusion coefficients as well as the integral Lagrangian...
The range of prototype eddy circulations to which this model might be considered dynamically similar was determined by appealing to Batchelor's (1953) demonstration that the Richardson number affords a single similarity criterion for atmospheric motions on the scale of 100 m and less. A "Richardson number of turbulence" characteristic of the turbulent field was defined as follows:

\[ \text{Ri}^* = \left[ \frac{g}{\rho} \frac{d\rho}{dz} \left( \frac{1}{\sqrt{\frac{u_z}{\ell_z}}} \right) \right]^2. \]

Here, the denominator represents a typical eddy shear, written as a ratio of the rms vertical velocity fluctuation and the vertical eddy scale. Note that the numerator of this expression is identical with \( N^2 \), while the denominator is equivalent to \( \frac{1}{T_{L_z}^2} \) in which \( T_{L_z} \) represents the vertical Lagrangian scale time; hence the ratio reduces to

\[ \text{Ri}^* = N^2 T_{L_z}^2. \]
Values of this parameter derived from the experiments were compared with those extracted from available field observations. The model was found to represent $Ri^*$ values of the order $3 \times 10^{-3}$, the field data indicating that this magnitude characterized turbulence at stack height ($\sim 50$ m) in comparatively weak, stable lapse rates (between $-0.6$ and $1.0^\circ C/100$ m) where vertical Lagrangian scales from 2 to 5 sec have been observed. However, these similarity considerations are necessarily tentative since truly adequate observations of the prototype have never been made.

DISCUSSION

In conducting atmospheric model experiments, continuing comparisons must be made between laboratory results and observations of the full-scale prototype. Without this interchange, there is the danger to the implied purpose of meteorological modeling that the experimental studies could assume a sterile, ivory-tower aspect. The real value of modeling lies in its ability to reduce a phenomenon to its essentials; its most rewarding applications are analytical rather than synthetic. But because over-simplification can lead an experiment astray, continuous contact with reality in the form of observations of nature should be maintained.

Incidentally, the word "simulations" should perhaps be used with care; its emphasis on the synthetic aspects of modeling implies qualitative "look-alikes," rather than true dynamical models definitely related to prototypes through similarity principles. A "simulator" may afford a restricted analogy, but a dynamically similar relationship between model and prototype is expected to be sufficiently homologous to allow extrapolation of results to a wider range of experimental variables than that actually observed.
Should some group undertake atmospheric modeling on a large scale, success will basically lie with the scientists involved, and not with the sophistication of the facility. A permanent cadre of technicians would be required, this to include at least one scientist who actively wants the facility from the beginning and who is both sufficiently able and interested to work on model experiments for an extended period of time. Research for answers is relatively easy, usually needing only time and money; but formulating valid questions for such research is difficult, since this requires people who combine training with talent. Specific questions must be asked of an experiment in advance, since these will largely determine a particular model's design. Construction and operation of a complicated facility more or less to see what will happen, or what almost amounts to the same thing, in hopes of attracting outside users, could be a waste of time and money.

Given the promise of an adequate skeleton staff, the facility itself probably should not be committed to a single, large apparatus, especially in view of the present state of the art. Rather it should resemble a generalized physical research laboratory, well equipped with basic instrumentation and tools; some idea of the range of techniques involved can be gained from Section C of Frenzen, et al., (1956). The laboratory should be attached to a large, open structure with a firm floor, adequate utilities, and a controlled environment, within which a variety of apparatus can be built and operated. To cite one example of work that might be done, thermally stratified flow could be investigated in a large room with insulated walls and uniformly heated and/or cooled ceiling and floor. To reduce wall effects, the room should be as much as twenty times as wide as it is high; but, to complicate matters, it might again eventually prove desirable to rotate the entire volume.

Finally, with regard to the future prospects of atmospheric simulation in the laboratory, the writer is neither overly enthusiastic nor bleakly pessimistic; rather he remains hopefully realistic,
sharing an attitude held by most of those who have carried out meteorological model experiments.
8. CURRENT AND FUTURE PROSPECTS FOR LARGE-SCALE AND MEDIUM-SCALE CIRCULATION MODELING

(Lectures and Discussions of D. Fultz)

One of the more striking examples of basic hydrodynamic experiments which shed light on atmospheric motion is the well known dishpan experiment. These studies depended on partial similarity, primarily of appropriate Rossby numbers and vertical stability parameters, and provided considerable insight into the behavior of large-scale motions of the atmosphere, particularly in the polar and middle latitudes. The following lectures and discussions deal with a selection of interesting features of the basic phenomena of a barotropic and baroclinic nature which develop in rotating annuli and disks. The potential for exploiting the experimental systems for better understanding of the processes occurring in the atmosphere also is discussed.

LECTURE A

Successful modeling of atmospheric processes depends to a large extent on the equal partnership of careful experiment in the laboratory, geophysical observation, and theoretical developments. I will try to illustrate some of the interactions in this triangle from the point of view of the experimental vertex of the triangle.

Basic experiments on stratified fluids in rotating systems may contribute to a better understanding of atmospheric motion in several ways. These include: (a) one may obtain relatively simple and qualitative results that lead to the discovery of new fluid dynamical phenomena, (b) experiments may be able to provide critical tests of certain existing quantitative theories, (c) one may in properly designed experiments be able to separate complicated phenomena into simpler processes for study under controlled, reproducible conditions, and (d) one may obtain rather vague and general insights which neverthe-
Appendix A-8

less suggest new conceptual aids to understanding geophysical phenomena, and which, in turn, allow improved theoretical models to be developed. Each of these aspects of laboratory experiments can be illustrated by examples of current experimental studies.

Several cases of new phenomena have appeared in careful study of the astounding variety of modes of instability in rotating systems. For example, inertial oscillations of the sort calculated by Kelvin have been observed in a barotropic fluid rotating in a pan. The axisymmetric types have given certain specific eigen-frequencies in units of $\Omega$ that depend only on the radial and axial mode numbers. For the higher radial mode numbers, the zonal motions at the top reverse in alternate rings. This motion has been found to become unstable at a definite finite amplitude to disturbances consisting of rows of pulsing vortices located on the nodal cylinders. These kinds of instability are three dimensional, and involve simultaneous vertical and horizontal shear. Since this type of oscillation instability is independent of Reynolds number over wide ranges, it appears to be primarily an inviscid instability. Other oscillations and circulatory patterns have been observed in the boundary layer at the bottom of the fluid. Some of these phenomena are analogous to the streaks in Ekman layers, as observed by Faller. They evidently are strongly dependent on viscous forces. None of these oscillations has been predicted in existing theory of fluid motion.

An example of a critical test has arisen with Kelvin's theory for gravity waves on a rotating homogeneous fluid in a circular cylinder. According to the theory, the ratio

$$A = \frac{f_\text{rot}^2 - f_\text{non-rot}^2}{4 \Omega^2} \to 1, \text{ as } \Omega \to 0.$$
Kelvin does not say but implies the result is independent of mode. Here \( f \) is frequency, and \( \Omega \) is the angular velocity of the cylinder. Although this theoretical conclusion was never tested for restrictions of validity, it has been used extensively, for example by Sverdrup, in connection with the properties of gravity waves on a rotating fluid. We found experimentally that \( A \rightarrow a \) finite value as \( \Omega \rightarrow 0 \) only for axisymmetric modes, the limiting value is not 1 and depending on the mode may be negative. Azimuthal modes do not behave according to this type of relation at all.

An interesting example of a qualitative test of a very old theory occurs in an idea of Halley concerning the subtropical trade easterlies. Halley, in 1686, thought that the low-level trades were to be ascribed to an average tendency to move in the direction of the zone of maximum heating with the sun. This idea was largely forgotten until J. Thomson discussed it again in 1892. Thomson suggested that there was no reason to expect that the sun-driven winds should go either with the sun or opposing it. We tried this experiment by rotating a burner under the rim of a fixed pan in the laboratory. We found that indeed a circulation could develop as a result of the rotating heat source. A slow circulation near the bottom developed around the rim in the same direction as the burner rotation, while a much stronger circulation developed in the center of the pan at high levels which rotated in a direction opposing the burner rotation thus corresponding to upper westerlies.

Among the class of experiments of primary geophysical interest are the experiments on thermal convection in rotating cylinders or annuli. These studies, undertaken in pans with heated walls and cooled centers, or in annuli with heating at the rim and cooling at the center (or vice versa), have been described in detail in a recent review (Fultz, 1961). However, as a refresher, several features of flow in these geometries are considered over three ranges of Rossby number:
(a) Hadley regime, where the zonal flow is essentially axially symmetric, (b) regular Rossby wave regime, where regular waves form in the
zonal motion, and (c) the vacillating and irregular Rossby wave regimes, where the waves on the zonal motion tend either to vacillate at regular periods, or (at lower Ro, of a few per cent) the zonal flow tends to break down into irregular eddy-wave motion combined with the development of strong jets between eddies.

The structure of the irregular Rossby regime closely resembles many features of atmospheric motion in the middle latitudes. These similarities have been discussed in the light of dynamic modeling (e.g., Fultz, et al., 1959; Fultz, 1961).

Generally, the annulus experiments, first attempted by Hide, produce much more regular, and well defined Rossby wave regimes than the cylindrical, open centered experiments. Therefore, much of the study of vacillation and transition in the wave number spectra has been carried out in annuli. The effectiveness of stabilization by annulus geometry of waves in the Rossby regime evidently involves the strong interaction of the wall boundary layers with the meridional and zonal flow in the rotating fluid. We have here an example of separating relatively clean phenomena in the annulus from the noise and irregularity of more realistic Rossby regime convection experiments.

Quite frequently, we have observed modes of instability superimposed on the normal baroclinic waves that develop in rotating pans or annuli. Some of our recent work has been involved in trying to define these modes, as well as to straighten out some of the inconsistencies in the principal regions of vacillating wave motion and in the zones of transition between wave number regimes.

LECTURE B

In this discussion, I shall concentrate primarily on newer studies, particularly with reference to detailed measurements of velocity and temperature fields, and on some prospects for future work.
In the examples of experiments mentioned previously, the results were largely qualitative, based on dye traces, tracks of aluminum particles on the rotating fluid surface, and limited measurements of the temperature field. We are beginning to approach a new stage of development now where we can realize the measurement of the local velocity field at the fluid surface at least, and the local three-dimensional temperature field in regular and periodic experiments. In irregular experiments, long-term statistics of the temperatures can be obtained at least for a few positions. In these classes of experiments, much can be learned about the details of interaction of motion on various scales, but the price must be paid of evaluating enormous amounts of data. It is only possible to undertake such experiments if the capacity and speed of data acquisition or processing systems are pushed to their present limit. With such detailed measurements, it is just beginning to be feasible to consider the "critical" testing of numerical models by laboratory experiments.

Three examples of temperature measurements are described to illustrate what is involved in these cases. In the first example, the temperature field along a meridian in a non-rotating flow is mapped. The second corresponds to the meridional temperature field of a rotating axially symmetric regime, and the third shows the temperature distribution for a vacillating, five-wave Rossby regime. The last case covers averages of some 30,000 data points. All three cases exhibit a stable temperature distribution with height in the interior, and minima in the layers near the hot and cold wall. Since all three cases display similar temperature fields, the wall boundary layers must be largely independent of rotation. These results do not follow recent theories for similar configurations.

Careful study of the flow patterns which develop in a non-rotating system indicates that boundary layers form at the walls, at the bottom and along the free surface of the liquid. The boundary condition at the top surface becomes quite complicated. That is, the radial com-
Appendix A-8

The component of velocity tends to follow a no-slip condition, while the azimuthal component is something between no-slip and no-stress. These conditions must be taken into account in attempting to compare critically the laboratory observations with a numerical model.

At present, there is no way of obtaining data for the local velocity field within the fluid. However, after several years, we have developed a photographic technique for obtaining horizontal velocities at the top surface. This method is very tedious, and requires considerable effort to analyze even a small fraction of the data presently on film. For example, the films for a single experiment consist of a sequence of 10,000 frames. With our present methods, reduction of this data to results for computers would require about thirty years of time.

Clearly, the detailed study of field measurements in these experiments requires data processing of the same magnitude as the worldwide network for gathering meteorological observations. Of course, the ultimate results of such a detailed study should yield much more valuable information than any of our previous qualitative studies. The ultimate limitation in these experiments on rotating systems comes from the limits of reasonable data handling. The better the technology for this type of data acquisition and processing, the more sophisticated the experiments can become, and hopefully the more valuable the information gained from these studies.

In addition to the problems of data handling, it is vital in those experiments involving thermal convection in rotating vessels to maintain a very well controlled environment. Temperature and humidity must be controlled. For example, the sources in the experiment should be maintained within tolerances of $\pm 0.01^\circ C$ for the thermal gradients which develop in the vessels. And the environment should be maintained to $\pm 0.01$ to $0.05^\circ C$. Furthermore, the axis of rotation of the vessel must be held ideally to within 4 sec. of arc from the vertical direction. Otherwise, it is possible to get coupling between tidal motions and the baroclinic waves which can ruin an experiment completely. An
example is shown in the case of a five-wave vacillation experiment which would not develop properly when the axis deviated from the vertical by 25 sec. of arc.

As for the prospects of future work on fluid dynamics in rotating systems, there are at least five classes of experiments which show promise for yielding valuable information.

The first group consists of further experiments on surface gravity waves on a rotating fluid. This work could be extended to the internal wave modes. In particular, there seems to be a close relation between the waves that appear in a two-layer rotating fluid and the behavior of the polar front. It may be possible to compare the experimental results with numerical computations similar to those of Kasahara, et al. (1965).

Second, further study of inertial oscillations including barotropic waves in the $\beta$-regime, such as the experiments of N. A. Phillips and A. Ibbetson at Woods Hole, should be of interest, especially in a two-layer fluid in connection with the polar front model.

The third area would explore the tilt of the axis of rotation of the turntable, and the non-linear interaction between simple gravity modes excited in this way with vortex modes of motion.

In the fourth area, a continuation of more detailed measurements in rotating systems undergoing thermal convection should prove useful. In particular, the investigation of transient responses in the experiments along with mapping out the properties of the steady wave regimes in comparison to a corresponding numerical calculation should give some information about the stability of existing procedures for numerical integration. The vacillating wave case involves certain mechanical processes which are closely related to the processes in the long waves at mid-latitudes. However, the experiments are much better defined. Therefore, a reproducible, controlled system is available for examining these mechanical interactions.
The fifth area of interest is the irregular Rossby regime. For detailed measurements, this class of experiments presents very difficult problems. It may never be possible to explore these systems in any detail of the magnitude required to check a general circulation model. However, it should be possible to measure certain statistical properties which could compare with statistics generated in a numerical model. One of the more intriguing possibilities in this class of experiments is the possibility of investigating in hydrodynamic systems climactic changes, and large-scale weather modification. It is now feasible to undertake experiments involving 4000 equivalent years of time. With one month's running time we can get 1000 equivalent years of temperature and surface velocity data. The computer simulators cannot give this kind of information yet, but it appears feasible to check sections in time of a steady state behavior for statistics generated by computers with the experimental results. One can also look at the effects of changing the heating, say of the Arctic over a long period of equivalent time. If the effects of modification are marked, knowledge of the qualitative changes would certainly help. However, even if the results of modification on climatology are more subtle, it should be possible to isolate these effects by detailed field measurements. And these results could be matched with computer results as they become available. For climatological studies where long time integrations are involved, it appears vital to provide critical tests of numerical integration schemes. I believe that this can be done, in principle, using the laboratory models even though the dynamical processes are not fully similar. It seems to me that this might well be the most critical way of establishing the credibility of long-term numerical integrations.
DISCUSSION

Experiments in Rotating Vessels

The point is well taken that numerical integration schemes could be checked by means of the model experiments. However, it should be noted that this sort of check has not been ignored by numerical experimentalists. There have been a number of numerical studies of laminar flows of simple geometry which have been compared with laboratory studies. The strong question comes up among workers as to the relevance of many of the interesting fluid dynamic experiments in rotating vessels to atmospheric processes. In the experiments, the flow is largely laminar, or at best quasi-turbulent, but in the atmosphere the flow involves turbulence of high Reynolds number. The laboratory experiments say virtually nothing about this regime. In laminar flows, the behavior of the Navier-Stokes equations is relatively well understood, and satisfactory numerical models can be undertaken for these conditions. Unfortunately, this is not the case where the fluid flow is turbulent, and this is the regime where we need most of the information for dealing with the atmosphere. These arguments are entirely correct. However, it seems quite worthwhile to provide a truly critical check of numerical results with experiments. This has not been done yet. In particular for cases where these temperature gradients are large, the viscosity of liquids (water) vary greatly. Hence, the term $\nu \nabla^2 \mathbf{u}$ in the Navier-Stokes equations cannot be correct ($\nu$ = kinematic viscosity, $\mathbf{u}$ is the velocity vector). It is really the involvement of variation of physical properties locally in the equations of motion that should be examined along with the numerical schemes in such critical tests.

The rotating pan experiments historically provided considerable insight into the large-scale motion of the atmosphere. However, very little has been done in relating the fine structure of the laboratory experiments to observations of mesoscale behavior in the atmosphere. There is certainly potential for doing such studies, and we have observed a lot of fine structure behavior which look similar to
atmospheric phenomena. However, a detailed quantitative study of this fine-scale motion would be very difficult. One might have to go to a larger vessel, and go much farther in ability to process large columns of data.

The question of data conversion and reduction is a major key to future work on the laboratory experiments. Until automatic devices become available that will be able to convert large volumes of data on strip chart, on magnetic tape, and in photographs to more usable form for analysis by computer, we are severely limited in our studies. By way of illustrating the expense involved for development of equipment, we figure about $10^4/\text{channel}$ for at least 200 channels, or $\sim 10^6$ for analog-to-digital conversion above. For good photographic scanning, development will cost $\sim 10^7$ unless, for example, nuclear track scanning devices can be adapted and cover the development cost.

In examining the question of the usefulness of a large-scale, systematic program using rotating systems, it appears that, from the standpoint of geophysics, the climatology problem looks most attractive (see A-8, B). To do a really complete study, on say three different "climates" even in a 40-cm diameter model would require an effort on a larger scale than a university can undertake right now. Conceivably one might want statistics on several properties of the flow, and one would want to run about an equivalent of 4000 years ($\sim 4$ to 5 months in the laboratory). This would require several people to analyze the data and watch the equipment, and it would necessitate a redundancy of certain equipment, plus very extensive and fast data processing units. Needless to say, the environment of the laboratory would have to be controlled within the tolerances I mentioned earlier.

The Facility

The problems of data conversion and reduction might be resolved by a national facility of some kind. It might be useful for NCAR to consider, for example, the project of developing a medium-scale, in-
expensive data processing device (i.e., analog-to-digital conversion system) which could be made available to workers to use as a package. Either a centralized data processing system, or a "traveling" package would be helpful, but the package idea is better, especially in the early stages of work where trouble shooting and monitoring has to be done on experimental equipment. The notion of a centralized photographic processing system would be very worthwhile considering. A fast, elaborate system to scan photos and map out digitally velocity trajectories (need coordinate, direction and magnitude) would be very expensive, as mentioned above. A flexible national facility for our use and the use of other experimenters would be too expensive to operate individually. Therefore, the concept of a centralized facility here is much more attractive.

With our present arrangement of microscopes, we could tackle some of the photo reduction work for our own experiments, provided that enough money was available to hire about twelve operators. We could also increase our speed capabilities by about two or three times if funding were given to us to build servo drives to automatically scan coordinates on a microscope stage.

The idea of a loose co-operative plan between institutions presently having capabilities in certain areas of simulation or modeling may be useful. This would be an efficient way of using funds in this area since the present facilities could all be beefed up by people who know what new and better equipment is needed.

Simulation experiments depend largely on the researcher and not on the equipment. Therefore, it seems vital to center the strength of a simulation program in the hands of those who are interested and competent in particular phases of laboratory experimentation.

There are some problems that might require larger rotating vessels of sizes unsuitable to construct, for example, at a university. For example, one might want to avoid the viscous effects at the walls of
the vessel by going to a larger diameter disk. However, there are a number of troubles with larger systems. These are of course the obvious mechanical problems of construction and maintenance and the data handling. However, perhaps the most important reason is that there is not enough known about the capabilities of small systems now to build reliably a larger system. People at NCAR, for example, should undertake such experiments only if they have a specific problem in mind and know exactly what to do with the experimental procedure.

In using large facilities, one should be prepared for years of trouble shooting, and many hours of operating time. Such "vague" programs might tie up a given facility so that its use by many workers for different studies would be very inefficient, if not impossible.

We reach the conclusion here, as in previous discussion, that at this stage modeling in rotating systems must be left to the individual workers rather than within a large centralized facility. At present, one really cannot plan reliably for a large program; it will be all too easy to devise a "real lemon" of a simulation laboratory for hydrodynamic modeling.
9. ON THE DYNAMICS OF ROTATING FLUIDS

(Lectures and Discussion of R. Hide)*

Certain hydrodynamical processes that are thought to occur in large-scale natural systems (e.g., the earth's atmosphere) can be produced on the much smaller scale of the terrestrial laboratory. To the intrinsic value, therefore, of crucial laboratory experiments on such processes must be added their geophysical significance.

The use of the term "crucial" is deliberate. It implies that in carrying out the laboratory investigation the usual precautions are taken to eliminate accidental and systematic errors and to minimize random errors, and that whatever theory is necessary and appropriate is brought to bear on the investigation. It is useful to draw a clear distinction between crucial experiments, on the one hand, and haphazard investigations -- whether they be carried out or the textbooks of mathematical analysis -- on the other hand. Too few published studies fall into the first category. The importance of studies in the second category has occasionally been overemphasized. Though often impressive, such studies have only been useful in the few cases that they have led to crucial experiments.

Though certain phenomena in the earth's atmosphere, in the oceans, and in the atmospheres of the other planets may have their counterparts in laboratory investigations of rapidly rotating fluids, this is the only sense in which the laboratory apparatus should be considered a "model" of the geophysical prototype. The indiscriminate use of the terms "model" and "simulation" has, unfortunately, led to confusion in geophysical fluid dynamics.

The purpose of my two lectures is to discuss a selection of laboratory experiments with rapidly rotating fluids and to comment, tentatively,

* See also Hide, R., 1966d.
on their geophysical significance. It will be convenient to discuss barotropic fluids in the first lecture and baroclinic fluids in the second. I shall restrict attention to incompressible fluids; though the dynamical effects of compressibility may be very important in the atmospheres of Jupiter and Saturn, I know of no relevant laboratory work. (For general references, see Chandrasekhar, 1961; Fultz, 1961; Phillips, 1963; and Squire, 1956.)

LECTURE A: BAROTROPIC FLOW IN ROTATING FLUIDS

The Proudman-Taylor theorem (Hide, 1960; Proudman, 1916; Taylor, 1923) and the theory of the Ekman boundary layer offer immediate insight into the hydrodynamics of rotating fluids (Faller, 1963; Faller and Kaylor, 1966; Greenspan and Howard, 1963; Gregory, Stuart and Walker, 1955; Hide, 1964, 1965; Prandtl, 1957). Surprisingly, it is only very recently that meteorologists and oceanographers began to grasp the relevance of the Proudman-Taylor theorem to their studies, and it is noteworthy that Taylor's laboratory work on rotating fluids was not included in the meteorology section of his collected works!

The Proudman-Taylor Theorem

The Proudman-Taylor theorem states that "slow, steady motions of a homogeneous (i.e., barotropic) inviscid fluid that otherwise rotates uniformly will be two-dimensional, in planes perpendicular to the axis of rotation." In terms of \( \mathbf{u} \), the Eulerian velocity vector of the fluid relative to a frame rotating at the basic angular velocity of the system, \( \Omega \), the Proudman-Taylor theorem can be stated as follows:

\[
\frac{\partial u}{\partial z} = 0 \tag{1}
\]

where \( z \) is parallel to the direction of \( \Omega \). Equation (1) can be obtained directly from the equation of motion by taking the curl of that equation.
and neglecting small terms. Motion satisfying Eq. (1) is "geostrophic": in geostrophic motion Coriolis force exactly balances the pressure gradient.

**Ekman Boundary Layers**

Boundary layers in which viscous stresses are concentrated arise at the walls of the container of the fluid. The thickness of an Ekman layer near a rigid surface perpendicular to the z axis is \((\nu / \Omega)^{\frac{1}{2}}\), where \(\nu\) is the kinematic viscosity of the fluid. If the flow outside the boundary layer (the "interior flow") has vorticity (relative to the rotating frame of reference) there will be a flow into the boundary layer given by

\[
w = \pm \frac{1}{2} (\nu / \Omega)^{\frac{1}{2}} (u_g) \frac{z}{u_g}
\]  

(2)

where \(u_g\) is the fluid velocity evaluated at the edge of the boundary layer remote from the rigid surface, and the upper or lower sign is taken according as \(z\) increases or decreases from the rigid surface towards the flow.

One immediate consequence of Eqs. (1) and (2) is a definite relationship between the motion of rigid surfaces in contact with the fluid and the vorticity of the interior flow. In particular,

"Steady geostrophic flow of a barotropic fluid in contact with rigid walls that are stationary in the rotating frame will have zero relative vorticity."  

(3)

**Stewartson Boundary Layers**

On rigid surfaces that are inclined at an angle \(\theta\) not too close to \(90^\circ\) to the z axis, Ekman layers of thickness \((\nu / \Omega \sin \theta)^{\frac{1}{2}}\) occur. On rigid surfaces parallel or nearly parallel to the z axis, boundary layers more complicated in structure than Ekman layers occur.
Appendix A-9

Stewartson was the first to discuss these layers in detail in the case when inertial forces are negligible compared with viscous forces (i.e., small Reynolds number). Unlike Ekman layers, which have been termed "primary" boundary layers because in most cases (though not quite all; see below) their presence profoundly affects the interior flow, Stewartson boundary layers (Stewartson, 1957) are "secondary" in the sense that if the walls on which they occur are removed the interior flow would not change. The thickness of Stewartson boundary layers is usually proportional to $\sqrt[3]{D}$, with sub-structure on a scale proportional to $D^{1/3}$, and is typically greater than the thickness of Ekman boundary layers.

**Dimensionless Parameters**

The accuracy to which the flow can be regarded as steady and slow and the fluid as inviscid is measured by the dimensionless parameters

$$G = (\Omega \tau)^{-1}$$  \hspace{1cm} (4)

$$F = U/L \Omega$$  \hspace{1cm} (5)

and

$$E = \sqrt{D} L^2 \Omega$$  \hspace{1cm} (6)

where $\tau$ is a typical time-scale associated with changes in the pattern of fluid flow, $U$ is a typical relative flow speed, and $L$ is a typical length scale. The Proudman-Taylor theorem is strictly correct when $G = F = E = 0$. Deviations from geostrophy are of the order of max $(G,F,E)$.

Another important parameter is a Reynolds number

$$R \equiv UL/\nu = F/E$$  \hspace{1cm} (7)
In most geophysical systems, \( R \) is very large, and this is even true of many laboratory systems. In many mathematical contributions to the theory of rotating fluids attention is restricted to the case of \( R \ll 1 \), since non-linear terms can then be ignored.

It is known from laboratory studies that instabilities arise in Ekman boundary layers when the Reynolds number of the system exceeds about 50. Dr. Faller, in a lecture in this series (A-6), has given a detailed description of this work and its applications in meteorology and oceanography, to which he himself has made important contributions.

**Taylor Columns**

According to the Proudman-Taylor theorem, the hydrodynamical effects of bumps and corrugations on bounding surfaces of the fluid, and of obstacles within the fluid, are not localized to the regions of their origin. Such effects propagate in the \( z \) direction throughout the fluid, forming "Taylor columns," to use a term which I coined in a subsequent extension of Taylor's work. (See also Hide, 1961; Hide and Ibbetson, 1966; Jacobs, 1964; Long, 1953; Stewartson, 1954; Taylor, 1923.) Laboratory experiments have been and are still being carried out on Taylor columns. The suggestion that a Taylor column may underlie Jupiter's Great Red Spot has proved useful in the study of Jupiter's atmospheric motions, and has led to several unexpected results. The steering of the Gulf Stream by bottom topography may be understood in terms of processes akin to those responsible for Taylor columns.

**Detached Shear Layers**

The constraints of the Proudman-Taylor theorem often lead to the occurrence of detached shear layers. Such layers occur, for example, when a rigid disk is slowly rotated in a rapidly rotating fluid. Titman and I have carried out laboratory experiments on the structure and stability of detached shear layers. Fultz and Moore have carried out related, but different laboratory experiments.
Among the most remarkable features of the atmosphere and oceans are the strong shearing motions that occur in regions remote from bounding surfaces.

**Flows Due to Sources and Sinks**

There is a theorem due to Taylor which states that two-dimensional flow of a rotating fluid will be the same as if the fluid were not rotating if the boundary conditions are independent of $\Omega$. In some recent experiments on flows due to sources and sinks I have shown that this theorem holds in practice with remarkable accuracy, and it is in these circumstances that Ekman layers lose their primary character (see above; see also Hide, 1966c; Stommel, et al., 1958).

I have also shown that the boundary conditions will not be independent of $\Omega$ when it is possible to find a closed curve within the fluid across which the total flux does not vanish. In these circumstances, when $R \gg 1$, an inertial boundary layer in which the relative vorticity rises to $-2\Omega$ in magnitude occurs on the source. A geostrophic interior region occurs in which the relative vorticity vanishes, and a Stewartson boundary layer occurs on the sink. When $R \ll 1$, both boundary layers are of the Stewartson type. In both cases, Ekman layers connect the boundary layers on the source with that on the sink. No transport of fluid occurs in the geostrophic interior region.

These simple experiments throw light on more complicated investigations in which three-dimensional flow occurs. The inertial boundary layers exhibit instabilities which will be examined in detail in the near future.

**Inertial Oscillations and Rossby Waves**

When the bounding surfaces of the fluid are not everywhere perpendicular to $z$, filaments of fluid stretch and contract as they move

---

* See also Aldridge and Toomre, 1966; Hide, 1962, 1966a; Ibbetson and Phillips, 1966.
about. The concomitant vorticity changes can give rise to inertial oscillations. Rossby waves (i.e., tidal oscillations of the second class) are one well known example of inertial oscillations. Dr. Long, in a lecture in this series (A-12) has described some of this work, to which he has made original contributions (Long, 1952).

LECTURE B: BAROCLINIC FLUIDS

Having discussed very briefly some aspects of barotropic flow in rotating systems, now consider the motion of rotating baroclinic liquids of thermal coefficient of cubical expansion $\alpha$.

In general there are two types of density configuration, one which is unstable, $\partial \rho / \partial s > 0$ ($s =$ vertical coordinate), and stable $\partial \rho / \partial s < 0$. The first case refers to systems in which Bénard-like overturning develops. The stability theory for such motion in a rotating system has been described in Chandrasekhar's book on the subject. A number of related experiments have been carried out. Certain aspects of overstability, oscillations and heat transfer in such systems have also been investigated in the laboratory; Chandrasekhar also gives details of some of this work.

The second case is potentially of greater interest in the study of large-scale motion in the atmosphere than is the first case. In both the earth's atmosphere and the oceans the Brunt-Väisälä frequency, $\omega_B$, divided by $\Omega$, where

$$\omega_B = \sqrt{-g \alpha \partial T / \partial s} \quad (8)$$

is relatively large. Therefore, stratification is important to motion in these fluid systems. It is significant for geomagnetism that $\omega_B / \Omega$ is so small for the earth's core that to a first approximation the Proudman-Taylor theorem should hold.
According to a theorem due to Jeffreys and Bjerknes (von Zeipel's theorem to the astrophysicist) a fluid subject to a horizontal temperature gradient will always be in hydrodynamical motion. A so-called theorem due to Sandstrom has confused this part of the subject for some years, in spite of Jeffreys' lucid discussion of the error in Sandstrom's work.

Since you have already heard from previous speakers about the historical development of experiments on rotating baroclinic fluids, I will restrict the subsequent remarks only to the annulus experiments which were initiated in 1950 in Cambridge and which were subsequently taken up in other laboratories. These experiments bear directly on the theory of baroclinic instability, a process discovered independently by Charney and Eady in an attempt to understand the dynamics of the energy-producing eddies in the atmosphere. Details of these experiments can be found in various publications (Fowlis and Hide, 1965; Fultz, 1961; Hide, 1953, 1966b; Piacsek, 1966).

Two general classes of theoretical problem arise in connection with the annulus experiments. The first is the mathematical description of fully developed flow in the four different regimes of flow; the second consists of trying to understand the stability of the flow to different kinds of small perturbations. Problems in the second class are usually easier for theoreticians to handle because they can be linearized. Of several theoretical studies, perhaps Eady's work on baroclinic instability is the most relevant to the experiments. Provided that \( \nu \) and \( \kappa \) (thermometric conductivity) are small, Eady's theory accounts for the transition between axisymmetric and non-axisymmetric flow, and for the number of waves occurring in the "steady wave" regime. The important theoretical implications of this result have been discussed elsewhere.

Attempts to predict the character of the flow when \( \nu \) and \( \kappa \) are not unimportant have not yet been successful. The main regime
transition curve probably depends on rather subtle effects due to the side wall boundary layers on the profile of the interior flow. These effects have not yet been examined theoretically.

It has been possible to construct a simple theory to account for the measured heat transfer and mean temperature structure in the axisymmetric flow regime. For small values of thermal conductivity in the fluid, a relation between \( \Delta T_s \), the vertical temperature contrast set up by the fluid motion itself, and \( \Delta T \), the impressed temperature difference, is given by

\[
\frac{\Delta T_s}{\Delta T} \approx \frac{2}{3} \left( 1 + \varepsilon \right)
\]

where \( \varepsilon \) is a small correction term. This relation and a related expression for the Nusselt number, agree quite well with experiment.

The recent numerical studies of Piacsek have elucidated the details of the flow in the axisymmetric regime.

Since the earth's surface is curved, baroclinic processes in the atmosphere are complicated by other effects (e.g., inertial oscillations). Now that the simplest annulus flows are fairly well understood, it will be possible to carry out experiments in vessels of more complicated shapes. Such experiments could bring the laboratory work closer to the problems of dynamical meteorology.

DISCUSSION

Since the process of baroclinic instability occurs in the atmosphere, further laboratory experiments on this process will be of obvious
theoretical importance in dynamical meteorology. A program at NCAR along these lines might be able to bring to bear greater strength than would normally be possible at a university. A long-term program, perhaps lasting several years and involving a number of experienced scientists and engineers, could produce results of great and permanent value. Collaboration of NCAR members with university workers in this field could be mutually beneficial.

Specifically, the laboratory experiments can make their most important contribution in the study of non-linear effects (e.g., jet streams). The excitation of inertial oscillations by baroclinic waves is also an important study for the future. The possibility of investigating the interaction between baroclinic waves and obstacles always seems to fascinate the meteorologists I meet. Vacillation -- one of the most arresting phenomena discovered in the annulus experiments -- has also attracted the attention of theoretical meteorologists.

One advantage of a large, well conceived project is that it can act as a focus of research and may spawn useful smaller-scale projects.

A turntable-annulus set-up of perhaps 1 to 2 m in diameter may cost about $10^4 to build. I must emphasize that only standard mechanical engineering techniques are involved. Transparent turntables have proved useful in certain recent experiments.

Adequate data processing techniques and good instrumentation will, of course, be essential. It may be desirable for NCAR to consider the design and construction of a fast, reliable, multi-channel data processing system as a general facility.

It goes without saying that fundamental studies in fluid dynamics are essential to progress in atmospheric sciences. To ask a national laboratory whose long-term goal is the design of controlled thermonuclear reactors not to study plasma physics in a basic way would be absurd. Similarly, a national laboratory charged with understanding the atmosphere must be pre-eminent in fluid dynamics.
There are, of course, many important areas of fluid dynamics to be studied. In deciding on the expansion of facilities and projects, I suppose that one should consider seriously the likelihood of substantial scientific advances being made, but this is always a hazardous business, and risks have to be taken.

The emphasis I have placed on baroclinic instability is partially the result of my comparative ignorance of other areas of geophysical fluid dynamics. Nevertheless, I believe that a combined laboratory experiment-theoretical program on baroclinic instability would be of scientific value and find direct application to meteorology.

It seems feasible to obtain a scale height equivalent to the atmosphere's in an apparatus of a meter or so in size for rotation rates of say $10^3$ to $10^4$ rad/sec. A lot of information about thermal convection in centrifuges is probably contained in classified AEC documents dealing with the separation of isotopes. If it were in the national interest, it would be useful to have this material de-classified.

I am optimistic about the role that laboratory experiments can play in NCAR's research program, and would be willing to help as much as possible.
10. AN ENGINEERING STUDY OF A LARGE CLOUD SIMULATION FACILITY

(Discussion of G. Langer)

INTRODUCTION

The subject of this study is the possible need for a large cloud simulation facility. It should be noted that the author's experience is in the field of supercooled cloud nucleation, scavenging of aerosols by clouds, and icing effects and has involved studies with up to 0.6 m diameter icing and nucleation wind tunnels. Recommendations are based on a review of pertinent literature, and correspondence with scientists who have direct experience in the field.

REVIEW OF CLOUD CHAMBER FACILITIES

We are concerned only with large cloud chamber facilities, and not with those set up by individual research teams and abandoned after serving a particular need. A number of cloud chamber facilities are discussed below. The list is probably incomplete because time was not available for an exhaustive review.

The best known effort in the United States is the work done by the U. S. Weather Bureau. It involved the vertical shaft in Miami, Arizona (Gunn, 1952) and the larger sphere in Hitchcock, Texas (Gunn, 1954). These studies were under the direction of Ross Gunn. Other principals in this research were Byron Phillips and Paul Allé. Fortunately the latter two were able to visit NCAR and discuss their work. Dr. Gunn, who is no longer with the Weather Bureau, was contacted regarding his recommendations, but was not in a position to contribute.

The mine shaft facility was 2.2 by 2.2 m in diameter and 210 m deep and was an expansion-type chamber. Considerable troubles were encountered in sealing the shaft. Limited control over the
temperature was achieved with water sprays, and steam was used to humidify the shaft. The evaporation and growth of free-falling drops was studied in the chamber, as well as the effect of electric charge. The maximum lifetime of a cloud was 5 to 7 min and was usually only 1.5 min. Operational problems made much of the experimental work difficult and the facility was abandoned.

The Weather Bureau then used a 3000 m$^3$ Horton Sphere from the Navy to continue the study of cloud drops. This sphere was impractical for the study of supercooled clouds because it was located near the Gulf Coast and could not be refrigerated. Also, there was no insulation on the sphere, which, because of uneven heating on the surface, led to interfering convection currents in the chamber.

The studies dramatically showed the advantages of going to a large diameter (15 m) and a minimum wall area design. The clouds now lasted a minimum of 30 min. Efforts to produce a continuous cloud by continuous blowdown and placing plastic sheets on the walls for insulation were unsuccessful. Reduced pressure tests were not possible because of the construction of the sphere. Interesting electrification tests were made. In the end, the unit was abandoned because it was not practical for quantitative work unless a major rebuilding job was undertaken. Phillips and Allee hope to take up the design of an advanced facility of this type in two years.

Recently an advanced cloud and aerosol study facility was completed at Obninsk, near Moscow (Volkovitskii, 1965). Of particular interest is the aerosol chamber which is of cylindrical shape and 3200-m$^3$ capacity. It is 15 m in diameter and 18 m high. Internal scaffolding makes it possible to place sampling equipment at various points. Clouds can be formed by steam, sprayed water, or adiabatic expansion. The chamber can be pressurized to 1.5 atm. From preliminary tests, fog lasting 1.5 hr was reported. Low temperature tests must be carried out in the winter because the chamber does not have
refrigeration. It is well instrumented -- particle size collectors and humidity, pressure, and temperature sensors are installed.

Dr. Volkovitskii was contacted regarding the present operation of this facility; that is, whether or not routine operation has been achieved. His answer was that a paper is being written covering the operation of the facility, but it is not yet available. According to Dr. Podzimek, a cloud physicist from Prague who recently visited NCAR, engineering problems are still being encountered. These difficulties are normal for a chamber of this size.

The Obninsk facility is of considerable interest because it was deliberately planned as a center for cloud simulation work and aerosol studies. It also has smaller, so-called thermal pressure chambers of 100-m$^3$ capacity. These are refrigerated and a low temperature of -45°C is feasible. The pressure can be varied from 0.1 to 2 atms, absolute. Clouds are formed by steam, atomization, or expansion of moist air.

The Obninsk center also has an optical tunnel (50 m long) and horizontal and vertical wind tunnels. The wind tunnels are for aerosol studies using liquid and solid particles. The vertical tunnel is 20 m high and 2 m in diameter. The horizontal unit is about 1 m across and has a special working section, 1.5 m long. Flow controls, as well as temperature and humidity controls, are built in. A horizontal tunnel for studying icing phenomena is also available.

The author's research uncovered only one relatively large cloud chamber in operation in this country. This chamber is installed at CSU (Steele, 1966) and is used to calibrate cloud seeding generators. It is refrigerated to a temperature as low as -25°C under favorable conditions. The cloud is formed by admission of steam. Instrumentation for counting ice crystals by microscopic means is available. The size of the chamber is 1.4 m diameter and 1.5 m high.
In Davos, Switzerland, two hail tunnels are in operation. According to List (1966) these have proven quite useful.

RECOMMENDATIONS FOR A CLOUD PHYSICS SIMULATION FACILITY IN THE UNITED STATES

The question now is whether or not past research with large chambers has been worthwhile; and, if so, do we need a facility in the United States. The answer to the first question is not an unequivocal yes. The Russian facility is the first fully planned activity of this type and the results will not be available for some time.

The question then is whether or not we need a chamber in the United States. It is recommended that an engineering study of a large, central cloud physics simulation facility be initiated now for the following reasons. Past efforts in the U. S. in the field of meteorology have shown the need for large cloud chambers, even though our weather research has been limited. With our present stepped-up efforts in weather modification and more to come, one can foresee the need for some type of large cloud test facility. There is a possibility that field experimentation may be more profitable. However, it takes several years to produce a cloud simulation facility and until a definite choice has been made, we must allow ourselves both options by planning for it now in an intelligent manner instead of a potential crash effort.

The only remaining item to be discussed in this report is the areas that should be covered in the proposed engineering study of a cloud physics simulation facility and the organization of such an effort.

From available information, it is apparent that the proposed facility can eventually cost many millions of dollars. Therefore, a system engineering study is necessary. It should include the geographic location and the organization which is to direct the facility.
The central part of the facility will presumably be a large cloud and aerosol chamber. This part should be studied first. The design of the large chamber will influence the design of supporting research facilities; therefore, its main features should be defined first. There are two basic options. The first option is a static facility, as all past and present chambers have been. The second option is a large dynamic arrangement; that is, a type of wind tunnel. Possibly there is a need for both types of facilities. It is recommended that the scientists involved state the ideal type of system they would like, regardless of feasibility and cost. Then, engineering studies should consider the ideas without bias. Imaginative use of new structural materials, insulation, use of surplus tunnels, aircraft hangars, inflatable structures, orbiting chambers, etc., should be considered. For structural strength and ease of insulation, a larger chamber may be built into a mountain.

If convective cooling is used, the defrosting of a large chamber must be studied for meaningful studies of supercooled clouds. A foam plastic lining that wicks glycol may prove adequate. Facilities for electrical studies must also be considered. Control of air cleanliness and deliberate addition of nuclei is of interest. There is no point listing all the factors involved. These suggestions indicate the scope of the effort involved.

The question of organization of this undertaking is an important matter. The engineering study may fall into the scope of NCAR's responsibility. International co-operation in such an undertaking is also a possibility. Presumably, the engineering study can be effectively handled by subcontracting various aspects of it to experts in the particular field. A listing of possible experiments by various groups should be obtained and contracts should be considered for the interesting ones. This is a natural way of involving various groups in the central facility and evolving a permanent staff. It is suggested that a pilot study be made amongst organizations involved in
weather modification for their desires for such a facility. Their needs are probably most pressing.

It is hoped that this report will stimulate a more definitive study of a central cloud simulation facility.
11. STUDIES OF HAILSTONES IN A WIND_TUNNEL

(Lectures and Discussions of R. List)

This summary constitutes a brief account of Prof. List's remarks during the course of two lectures held on succeeding mornings, and of the discussion between Prof. List and members of the NCAR staff during the two afternoon sessions (see also List, 1966).

LECTURE A

During the last ten years, new information about the growth of ice particles and hailstones has been found by studying the behavior of artificial hydrometeors in wind tunnels. Perhaps the most extensive investigations were undertaken in the hail tunnel at Davos, Switzerland. The origin of this facility is traced to the interests of the Swiss farmers in preventing hail damage to crops. The Swiss government was asked to provide research support for studying hail formation. Hence, a committee was initiated under the direction of the late Prof. Sanger. The committee decided that it was desirable to obtain a better understanding of the physics of hail formation before determining the "best" preventive measures. The hail tunnel at Davos was then set up to help provide new information about hydrometeor growth in the atmosphere. For a description of the Davos facility see List's papers.

A number of features of the growth of individual hailstones were studied in the wind tunnel including: (a) the aerodynamic drag and fall speed of stones of various shapes, (b) the structure of the ice-water system in hail, (c) the density of hydrometeors, (d) scattering of electromagnetic waves from different kinds of particles, and (e) the accretion of small supercooled droplets on single hydrometeors.
The experiments in the hail tunnel yielded important information about the physics of hailstones which could not be obtained from observations under field conditions. One rather obvious significant result showed that the growth of hydrometeors under natural icing conditions in the atmosphere is much more complicated than anyone had ever imagined. For example, the growth of hailstones is intimately tied to the complex freezing process of water held onto the stone in spongy ice. The ice content of stones depends on the air speed, the liquid water content of the cloud as well as the air and ice temperature. The ultimate size of hailstones depends on their residence time in regions of severe icing conditions.

Hailstones are generally irregularly shaped ellipsoids. Depending on the shape of particles, stones may obtain the same fall speed in spite of differences of a factor of nine in mass! This means that stones may be retained in a cloud for relatively long periods of time with smaller updrafts than expected from calculations of the motion of spherical particles.

The crystallographic structure of artificial stones along with density measurements of shells have yielded some information about the growth of particles in uniform icing conditions. The growth of ice in artificial stones, for example, develops with the C-axis of the ice crystals oriented either more or less randomly or at 90° to the radius vector of the stone. However, natural stones frequently show maximum growth with the C-axis parallel to the radius vector. This has not been observed in the artificial stones. We therefore need to know more about the effects of heterogeneous icing conditions which may be encountered under natural conditions.

Tests of the reflection of radar signals from drops and compact ice indicate that the scattered waves are too complicated to give unambiguous results about the nature of particles in natural clouds.

Investigations of the collision of small droplets (70μm) on ice spheres (2 cm diameter) has shown that although all droplets appear
to stick to the surface, as much as 20% of the accreting mass appears to be lost. The loss of mass may be associated with the formation of very small secondary drops leaving the surface after impact of the primary particles of the effect of vapor diffusion. When ice particles of about 70 \( \mu \) diameter were injected, they primarily bounced off the larger ice sphere.

The previous investigations are being extended in a second tunnel at Davos which can be run at different pressures. The new equipment has just been placed in operation.

New work in hail tunnel experiments should include, for example, more investigations of the aerodynamics of irregular hailstones, simulation of heat and mass transfer to stones. Along with these experiments, basic studies are needed to develop better techniques for crystallographic study of stones, to determine the solubility of air in supercooled water, etc.

LECTURE B

One of the more important areas where simulation of precipitation processes should be useful is the study of the interaction between the particles and the surrounding air. Most investigations of cloud particles center attention on the growth of droplets or ice particles in a passive, quasi-uniform environment. Actually, the particles themselves should have considerable effect on the development of the cloud. For example, it is not known how the drag of a collection of particles changes the air motion in a cloud. Also, the action of cloud particles as heat sources and heat sinks must certainly exert an important influence on the dynamics of the cloud.

To find out what collections of particles do to the motion of air in clouds, the problem of interaction between single particles and air first must be solved. This will be one of our first concerns in future hail tunnel studies. Let us then outline briefly how one might go about such simulation experiments.
For the purposes of discussion we shall divide cloud particles into two classes: (1) particles having no motion relative to air aerosols and nuclei, cloud droplets, and ice particles (mean size $< 20 \mu m$), and (2) particles having motion relative to the air (size $> 20 \mu m$), with (a) drops, snow pellets, and snow flakes, and (b) hailstones. In Table I, the possible interactions between air and the particles of both classes are shown schematically.

\begin{table}
\centering
\begin{tabular}{|l|}
\hline
Interaction & Simulation Equipment \\
\hline
Air $\leftrightarrow$ class (1) & Cloud Chamber \\
Air, 1 $\leftrightarrow$ (2a) & Wind Tunnel or Cloud Column \\
Air (1) $\leftrightarrow$ (2b) & Cloud Tunnel \\
\hline
\end{tabular}
\end{table}

Before discussing the problems of simulation of precipitation, let us first clarify a definition of simulation. By atmospheric simulation, I mean the modeling of a physicochemical process occurring in the atmosphere in such a way that the laboratory results can be used quantitatively and directly to explain the phenomenon in question. This interpretation implies, of course, the use of the well known principles of geometric and dynamic similarity.

In contrast to simulation, one generally can refer to reproduction of processes as the conscious effort to duplicate the specific conditions observed for the process occurring in the atmosphere.

Modeling the aerodynamics of free falling bodies involves the parameter called the Best number, and perhaps the ratio of the density of the particle to the density of the medium. Care must be taken when simulation is carried out in a liquid system. The effects of pressure changes should be taken into account, and the generation of
eddies behind large particles must be observed. This means the aero-
dynamic simulation of free falling hydrometeors should really be done in air.

To model the simultaneous heat and mass transfer to falling bodies, new scaling parameters are needed which include the Nusselt number for forced convection, the Sherwood number for transfer of latent heat, and the Efficiency of Catch for taking into account the heat transfer by accretion of droplets. Correlations for the heat and mass transfer to hydrometeors of various shapes can be done for varying pressures in a hail tunnel.

The modeling using similarity parameters provides the key to the effective simulation of the developments of cloud and precipitation particles. By understanding the physics of growth through the use of the laws of similarity, we can reduce the number of necessary laboratory experiments by simulation considerably in contrast to the use of a reproducer. Of course, the number of experiments can also be minimized by continuously feeding information from natural observations into the simulator system. For example, specific limits can be placed on the number of drops, on the shape of the size distribution of drops, and on parameters such as pressure and temperature.

To carry out a comprehensive study of the physics of clouds, and particularly the study of particle-air interaction, one needs at least three different kinds of simulators: the cloud chamber, the cloud column, and the cloud tunnel. The application of these pieces of equipment to the interaction problem is shown in Table I. The air-small particle interaction can be undertaken in cloud chambers of moderate size (say $\sim 1 \text{ m}^3$ in volume or less, initially). Much of the drop-air interaction should be carried out in a cloud column where a cloud is suspended at a quasi-stationary location and particles are dropped through this cloud. These experiments cannot be done too well in a wind tunnel because of uncertainties in the velocity, temperature
Appendix A-11 ............................

and supersaturation distributions. The cloud tunnel is most useful for studying the third set of interactions.

Because of wall effects, desirability of uniform environmental conditions, etc., the ideal simulators should have a moderately large size. For example, the study of droplets in a cloud chamber column should require a cross-section of say 10 to 15 m, with about 16 to 25 m height. The cloud tunnel should be about 1 m$^2$ in cross-section in the region of measurement, and should be about 12 m high. For equipment of this type, a building of about 1800 m$^2$ should be necessary.

At the same time the simulation work is being done, reproduction experiments and basic studies must be undertaken. The whole program might require a staff of approximately 24 scientists, including perhaps 12 Ph.D's.

In conclusion, one should bear in mind that the judgment of the success of a simulation laboratory or institute should not be made on its degree of atmospheric simulation specifically, but on the basis of its contribution to the state of knowledge of physical and chemical processes.

DISCUSSION A

Usefulness of Laboratory Experimentation

As a beginning, the discussion group centered attention on the problems associated with tying together the microphysics of clouds with the larger scale dynamics of cloud development. For the dynamicist, the microphysics enter into the cloud problem largely as initial and boundary conditions, or the microphysics are associated with the transfer functions driving the cloud system. At least two key items connected with the behavior of cloud particles really must be
known for a "complete" dynamic picture. These are (1) the effect of
the drag of the collection of particles on the moving air, and (2)
the rate of release of latent heat resulting from condensation. The
first item is involved with the aerodynamics of individual particles
and collections of particles. What is the effect of the turbulence
induced in the wakes of precipitation particles, for example? The
second point involves the growth of particles from nuclei (submicron
size) up to sizes of about 10 μ.

It was soon brought out that microphysics of clouds is far from
being well understood. And there are a variety of important features
of droplet growth and behavior that really should be studied in the
laboratory as well as in nature.

A cursory examination of several stages in the development of
cloud particles indicated that although much is known, there remains
much to be learned. It is perhaps obvious to say that our present
state of knowledge, in some respects, depends on sophistication of
the questions one asks about atmospheric processes.

For the purposes of this discussion, the microphysics of cloud
particles was divided into the following classes: (a) nuclei and
condensation (up to ~ 20 μ particle size), (b) the behavior of
large single particles (> 20 μ) in a cloudy environment, and (c)
the behavior of collections of particles.

The physics of nucleation and growth of small particles under
controlled laboratory and atmospheric conditions is not well under-
stood. For example, the growth rate of particles in cloud chambers
is more rapid than expected on theoretical grounds. Therefore, some-
thing is wrong with our diffusion models for the condensation process
for small particles. Present theories generally assume nucleation
from a (macroscopically) homogeneous supersaturated medium. A more
realistic model might consider a heterogeneous system containing hot
and cold spots.
The role of natural nuclei (aerosols) in the condensation, sublimation, and freezing processes is not clear. The present theory of nucleation works reasonably well for spherical, chemically "inert" particles. However, the mechanisms of nucleation on rough, chemically reactive particles is not at all clear. The "artificial" nucleation by seeding with materials such as silver iodide remains a mystery. The relative importance of particle size of nuclei is not clear. More laboratory work should really be done in these areas.

Another facet of the whole field of nucleation is associated with proper instrumentation. One often is not sure just what is being measured with present techniques for determining nuclei concentration. For example, some instruments now indicate that the maximum in freezing nuclei concentrations lie at the -8 to -10°C level. Surely there are higher concentrations of freezing nuclei in sizes corresponding to -15°C and below.

The behavior of single particles in their cloudy environment from droplet sizes to precipitation size ranges is a vast and complex subject which must include a knowledge of aerodynamics, heat and mass transfer, particle structure (ice), and surface chemistry. For examples of some of the problem areas, at least for hailstones, see List's lectures (A-11, A and B) and his papers.

Perhaps the most important problem associated with collections of cloud particles lies in the uncertainties about the efficiency of collection. With the aerodynamic calculations of Hocking came the notion of a cut-off limit for agglomeration of spherical particles at about 20 μ. From this it has been inferred that growth of cloud droplets up to this size must occur only by condensation. Recent calculations of Davis (Rand), however, have suggested that there is in fact no cutoff at sizes around 20 μ. The collection efficiency is small, but finite for sizes of droplets smaller than 20 μ. The new theoretical work should be checked by laboratory experiments. Further-
more, much more work should be done on the collision and agglomeration of particles of non-spherical shape.

**Concepts of a Simulation Program or a Facility**

In defining the concept of a simulator, one should distinguish between the ideas of reproduction and simulation. Reproducing conditions in a cloud involve duplicating the environment in terms of changes of pressure, temperature and liquid water content. However, the notion of simulation involves an effort to simulate certain parts (or a whole) phenomenon on a different scale using the principles of dynamic and geometrical similarity. Hence, the basic physics of processes must be known specifically here. Reproduction and simulation then must go hand-in-hand with basic experiments in the physics of fluid-cloud particle interaction.

Simulation of atmospheric processes, of course, can involve both liquid phase and gas phase models. Liquid phase systems have only limited usefulness for modeling cloud particle behavior because of the effects of size irregularities, Reynolds number, and the density ratio between the medium and the particle.

In determining NCAR’s role in atmospheric simulation, we should distinguish between the needs for indirect simulation or possible direct simulation through conventional laboratory experiments of small size and low investment, both in time and in money. As far as the usefulness of a program of a facility for simulation is concerned, NCAR really should concentrate on large, more sophisticated equipment and experiments. Our services as a group should consider simulation in this light regardless of the requirements for staff, time and money.

Criteria for the usefulness of a facility include (1) how well it will serve the needs of atmospheric scientists in performing their experimental programs, (2) its ability to provide answers to certain key questions about atmospheric processes in contrast to well defined
observational programs in the atmosphere. Within the context of the second point, it should be noted that an elaborate experimental program and a facility would probably require five to ten years to build and should provide a base for perhaps 10 to 20 years of sequential experiments. Furthermore, the facility should be designed and constructed by the (NCAR) scientific staff to insure its capability of simulation is met, and, of course, to learn more about the problems of simulation in "large" chambers.

There are at least two directions for consideration of a facility. One possibility is the design of an elaborate, well instrumented and sophisticated unit (or units) to meet the needs of a specific program and a particular set of experiments. Another possibility is to construct a laboratory which essentially consists of a "plug in" patch board of equipment in which scientists could save time and expense by coming to this laboratory to set up and perform small, special experiments. The concepts of experiments being undertaken in series or in parallel should be evaluated.

The question of management of such a facility naturally grows out of these considerations. Should the administration only be limited to logistical problems, or should a "committee" be established to coordinate experiments and to assign specific spheres of influence to interested scientists? How should a visitor program fit in? Obviously one cannot expect to develop a simulation program by coming to the laboratory for a week or two. Perhaps a minimum of one to two years residence is needed.

The size of simulation equipment can only be determined for specific experiments. Certain experiments can be done in small chambers, while it may be necessary to have rather large facilities for other studies. Prof. List has given us some estimates for an "ideal" precipitation physics facility (see A-ll, B).

To illustrate how the conception of a facility might develop, let us consider the problem of studying condensation in a cloud cham-
Previous work in this area can be traced to the preliminary efforts of the U. S. Weather Bureau, to the large Russian chamber, or to the 1 m² x 20 cm high chamber at Seattle University. So far, none of these chambers have provided much quantitative data. There seems to be a problem of stabilizing clouds in a cloud container. The cloud droplets of the water cloud are removed, for example, by diffusion to the walls. There is a characteristic time scale for existence of clouds in a chamber. This is the order of seconds to minutes depending on the concentration of droplets. Clouds in the atmosphere remain in existence for longer times. How does the atmosphere do this? There also is the problem of the control of chambers. The larger the apparatus the (exponentially) more difficult it is to control.

Because of the uncertainties in size and time scales for designing cloud chambers, perhaps the best way to approach such a simulation program would be to use the standard engineering technique of proceeding through a series of experiments with successively larger chambers. In this way a better understanding of all the problems associated with the technology of larger chambers could be solved.

DISCUSSION B

Relation of Scientists to Simulation Laboratory

One central theme continues to come to the surface in the discussions of atmospheric simulation. This deals with the relationship between scientists, their interests, and a centralized laboratory for simulation. It seems that the concept of a facility as a platform for carrying out experiments is not enough for a simulation laboratory. The success or failure of a long term program in simulation will largely depend on the scientist in charge and his associates, and not on the hardware of a laboratory.
The promotion, organization and operation of the simulation laboratory must come from inside an organization such as NCAR. The burden of directing such a program cannot be left to outside visitors coming in for one or two year periods. Visitors should be invited to join the simulation program to operate the equipment or perform experiments under direction of the permanent scientific staff. A visitor policy similar to that of the scientific programs in NCAR would probably be best for a simulation laboratory.

Prof. List was asked, for example, if he would be interested in visiting a central facility with his colleagues for a period of one to two years to do experiments on an "existing" tunnel. In answer to this question, it was brought out that this would make List's own research program too complicated. He would prefer to build his own equipment and do his own experiments at a "permanent" home base.

Although an active participation for visitors might be unlikely in many cases, most scientists would like to know what is being undertaken in a simulation laboratory and might want to help informally in the design stages of the equipment. It was brought up, incidentally, that no formal overtures are known to have been made to NCAR by cloud physicists from the university community for use of a large facility for cloud simulation.

It was suggested that perhaps no centralized facility is desirable, but a decentralized group having a particular kind of simulator could be organized. This committee of scientists could serve as a pool of talent in various universities so that interested parties could visit these scientists' laboratories for training and undertaking short-term simulation experiments. This suggestion was not considered very favorably because of the problems of coordinating this activity and of maintaining specific pieces of equipment in universities over long periods of time.
The point was brought out that we as a colloquium should consider what NSF, NCAR or any other organization should be willing to do for support of a simulation program. For example, should we provide a facility for simulation in being, or should a scientist be enlisted and completely supported to begin a new simulation laboratory or "institute"?

The conclusion of this part of the discussion essentially was that a simulation effort should be undertaken. However, a man or group of people with this specific interest in carrying out a systematic program of simulation experiments should be given the support in money and manpower to complete this task.

The Merits of a Simulation Facility for Precipitation Physics

After some discussion, it was generally concluded that a platform such as a balloon for launching special experiments in simulating precipitation processes is not desirable. There must be a long-term direction and continuity to the simulation studies.

From the reports of Prof. List, it seems clear that a combination of fairly large units including a cloud chamber, a cloud column, and a cloud tunnel would be very useful for future studies of precipitation physics. The dimensions of these units given by List, are rather strikingly similar to the dimensions used by the Russians for their equipment at Obninsk. Prof. List elaborated on his determination of the scales of dimensions for the chamber, column and tunnel. For example, a new cloud tunnel should be designed to study particles as large as 10 to 15 cm in diameter, with the potential of reaching Reynolds numbers based on particle size of about $5 \times 10^5$. This means that the wind speeds must be the order of 160 m-sec$^{-1}$ or higher. To avoid wall effects, the walls must be spaced at about six times the particle diameter. The test section of the tunnel should be sufficiently long to insure uniform conditions in velocity, water content and temperature over several meters. Therefore, an ideal cloud tunnel
would be about 1 m$^2$ in cross-section, and say 16 to 25 m high. A blower for such a recirculating tunnel would be the order of about 200 horsepower. Recirculation is needed, of course, to operate at reduced pressures, and to maintain steady controlled conditions in water vapor and in temperature.

It would be useful in the light of this discussion to learn more about the philosophy behind the cloud physics laboratory at Obninsk.

List pointed out that NCAR should move forward in the field of cloud simulation and begin pilot studies in this area. This would be more useful than another preliminary paper and pencil study. Once the program was begun and results became available, the future of large simulators would be much more certain.

Based on the discussion this week, we have concluded that there is no real need for a facility for simulation in precipitation physics in the sense of a "plug in" platform. However, there is very definitely a need for a program (emphasis here on the scientific staff) for simulation of precipitation processes and cloud formation.
LECTURE A: GENERAL ASPECTS OF THE MODELING PROBLEM

In this lecture, certain general principles of modeling will be discussed in the light of some recent thoughts (Long, 1963). These principles will be applied to an example of air flow over mountains on a scale where the earth's rotation is important.

The use of the term model is defined for this discussion as follows: we say we have a model and a prototype if, by mathematical transformation, by non-dimensionalization, and by physical approximation, the mathematical equations describing the prototype can be made to correspond exactly to those of the model. Although modeling appears to be feasible on paper, the experience of workers so far has indicated that, in general, it is almost impossible to create a complete dynamic model of atmospheric motion, except in some particular cases. These special situations may, of course, be of considerable interest and importance.

Heat Transfer to a Sphere

To illustrate how one may attack the problem of modeling, let us first consider Rayleigh's classical example of heat transfer from a sphere to a flowing fluid (Rayleigh, 1915). Rayleigh argued that the transfer of heat from the sphere to the fluid should depend on the heat flux h, the spherical radius a, the temperature difference between the body and the fluid $\Theta$, the thermal conductivity and the heat capacity of the fluid k, and c, and the fluid velocity v. Using the $\Pi$ theorem with these six variables, Rayleigh found that only two dimensionless parameters appear, and the heat flux parameter $(h/k\Theta a)$ is a function only of the dimensionless parameter $(cva/k)$. 
Later, Rayleigh's result was criticized because he used temperature as an independent dimensional parameter. Bridgman (1931) gave an explanation for Rayleigh's intuition, but one can also explain the use of temperature as an independent dimension by applying the mathematical equations governing this heat transfer problem.

The use of the governing equations assumes that these equations are known to a satisfactory approximation. In other words, one must be convinced that "physical behavior" can be determined by a well formulated boundary value problem (this includes, of course, proper boundary conditions).

In the case of heat transfer from a sphere, the mathematical formulation can be carried out, but the solution of the governing equations does not appear to be possible. However, by using just the differential equations of conservation of mass, momentum and energy, we find first of all that Rayleigh left out, for example, the properties viscosity \( \mu \) and density \( \rho \), and time \( t \). Proceeding with ordinary use of the \( \Pi \) theorem, or preferably by generalized dimensional analysis (see, for example, Long, 1963), the heat flux is determined in general by three dimensionless parameters in addition to Rayleigh's two:

\[
\left( \frac{h}{k \theta a} \right) = f \left[ \left( \frac{cv_a}{k} \right), \left( \frac{va}{\mu / \rho} \right), \left( \frac{\mu v^2}{k \theta} \right), \left( \frac{tv}{a} \right) \right].
\]  

Thus, we see that Rayleigh's result is a special case where three of the groups on the right of Eq. (1) do not affect the heat transfer. That is, (a) under steady-state conditions, (b) when the velocity is low enough for the ratio of the dissipation of energy \( (\mu v^2) \) to the heat conduction \( k \theta \) is small, and (c) under conditions of high Reynolds number \( Re = \rho v a / \mu \), where \( Re \) does not affect the heat transfer (this is known by experiment).
The same result can be obtained from the governing equations by placing them in dimensionless form. Here, in effect, we determine a "model" by seeking an affine transformation of the governing equations which leave the equations invariant (see also Birkhoff, 1950).

In Rayleigh's problem, the application of generalized techniques of dimensional analysis essentially do no better than Rayleigh's intuition, but the generalized method is safer in that it does not leave out parameters, and sometimes it will yield more information.

**Stratified Flow over Large-Scale Mountains**

As an example of the use of generalized dimensional considerations for modeling atmospheric motion, consider the case of stratified flow over a ridge whose scale is large enough that the Coriolis force affects the air flow. In general it is not possible to model large-scale atmospheric motion because the \( \beta \)-effect cannot be modeled. However, it is possible within certain restrictions to model mountain flow, and to take into account the earth's rotation, at least locally.

The equations of motion for this problem are written neglecting viscous effects and the influences of diffusion of heat. We make use of the potential density, and an equation of state corresponding to an adiabatic, perfect gas is applied to account for the influences of the compressibility of air. The Coriolis parameter is expanded in terms of the \( \beta \)-factor \( (f = f_0 + \beta y + ...) \), and a number \( B \), equal to \( L/a \), where \( a \) is the radius of the earth, is introduced. Lengths are scaled by a characteristic horizontal length \( L \), and a characteristic vertical length \( h \). Horizontal velocities are scaled to a characteristic speed \( c \). Vertical velocities are scaled by \( \delta c \), where the factor \( \delta \ (<1) \) accounts for vertical velocities being much less than horizontal velocities.

The model has the limitations that \( B \) is much less than the Rossby number \( R_o \), and the vertical difference in potential density over the troposphere, \( kh \), is much less than \( R_o (= c/f_0L \) which is not assumed small yet). This amounts to saying that the ratio \( L/a << R_o \), and
\( \zeta \ll \text{Ro} \). Assuming that the parameter \( \alpha \) (= \( \frac{cL_f \alpha}{ghk} \)) is the order of Ro, the equations of motion may be simplified with the above (tentative) limitations. The mathematical problem then is specified completely by giving the velocity conditions upstream from the mountain, and the kinematic conditions at the obstacle.

By inspection of the "acceptable" mathematical formulation we note that similarity between model and prototype requires that Ro be the same for the two systems. To satisfy the various other limitations, and the dimensional requirements for an atmospheric prototype, \( h \) is determined by the analysis, but \( L_{\text{prot}} \ll 10^7 \) cm. We cannot surpass \( L \approx 100 \) km without having to deal with the \( \beta \)-effect. For other conditions, based on a typical atmosphere \( \alpha \) and \( \zeta \) are \( \sim 10^{-3} \) and \( 10^{-2} \) respectively, and \( B \) is determined essentially by \( \alpha \) and \( \zeta \).

It turns out that no laboratory model realistically can be created using an equation of continuity in the form which incorporates the compressibility of the air. There are two possibilities for getting around this. First, one may ignore the compressibility term and assume that this parameter will not affect the model for qualitative comparisons. The other possibility is to insist on a dynamic model, and examine the limitations of a small Rossby number. This condition gives vorticity equations of the usual form, but one is still faced with the problem of the compressibility, which is finite in the atmosphere, but in a hydrodynamic model approaches infinity.

For \( \text{Ro} < 0.1 \), one can introduce a characteristic length scale \( L' \), which is a function of height, and one can explicitly remove the compressibility term. Then a strict model may be obtained. However, one now must deal with a non-linear transformation in vertical height. Nevertheless, the fact that the compressibility may be transformed out means that it is not introducing new effects into the model. Therefore, this technique is equivalent to introducing pressure as the vertical coordinate, where \( \text{Ro} \ll 1 \).
For a realistic hydrodynamic model in the laboratory, there are severe restrictions on \( L \) even for \( \text{Ro} << 1 \). However, it appears that the case \( \text{Ro} \sim 0.25 \), and \( L \sim 300 \text{ km} \) may work out to give at least a crude approximation to a complete dynamic model of the baroclinic flow over large-scale mountains.

**LECTURE B: MODELING SMALL-SCALE GEOPHYSICAL PHENOMENA**

In the last lecture, we found that under certain restrictions it should be possible to construct a strict dynamical model of mountain flow where the earth's rotation is important. For air motion on smaller scales, where the earth's rotation does not affect the flow, it is less difficult to develop models. As an illustration, let us consider a case where the winds are typical of the mid-latitudes. This range of air flow in turn requires that horizontal scale \( L \) of the mountains is less than about 200 km. Flow over mountains of this size corresponds to flow over a portion of the Sierra Nevada range, but not over the entire range. As an example, the flow over the Owens Valley in California is substantially unaffected by the Coriolis force.

As before, we can develop the criteria for a dynamic model using simplified equations of motion for the atmosphere. Again in this case, the governing equations are taken as the inviscid approximation without rotation, and without heat conduction or radiation. Potential density \( \mathcal{P} \) is introduced, but the Boussinesq approximation \( (\Delta \mathcal{P} / \mathcal{P} << 1) \) is avoided using Yih's velocity transformation \( u_1 = u_2 (\mathcal{P} / \mathcal{P})^{\frac{1}{2}} \), where \( \mathcal{P} \) is the air density at the surface, and \( u_2 \) is the actual horizontal velocity. The velocity \( u_1 \) is given in terms of a scale \( \bar{u} \) plus a perturbation \( u' \). Other perturbation variables for pressure and density are introduced. As before, the conditions far upstream of the obstacle, and the kinematic conditions at the obstacle are specified. And the formulation of the problem is made complete by requiring the top of the model (the tropopause) to be rigid.

To obtain the criteria for modeling, the equations of fluid motion now are made dimensionless in such a way that the non-dimensional
quantities in these expressions are the order of unity. After scaling
the equations for typical atmospheric conditions, one finds that, on
the small scale, the compressibility terms are small in contrast to
the larger scale case discussed in Lecture 12-A. For long mountains
of height, \( h \approx 2 \) km, and length, \( L \approx 100 \) km, the ratio \( \frac{u^2}{g \beta L^2} \) (\( \beta \) =
thermal stability parameter, with dimensions of length\(^{-1}\)) is small,
and the pressure in the fluid is essentially hydrostatic. However,
for mountain heights of \( h \approx 10 \) km, the vertical acceleration terms
are the same order as the hydrostatic terms. This is reasonable since
mountain waves essentially are a non-hydrostatic phenomenon, and they
consequently develop in the lee of larger obstacles.

Another scaling parameter \( R = a(g \beta)^{\frac{3}{2}}/\bar{u} \) gives us an idea of the
influence of the non-linear terms. For \( L = 2 \) km, \( R \approx 1.3 \), but for
\( L \approx 1 \) km, \( R \approx 0.6 \). Therefore, the non-linear terms in the equations
of motion are always important in mountain flows, and the early
linearized theories cannot give a realistic picture of this phenom-
ennon, except for very small hills. This conclusion was substantially
verified in the hydrodynamic model experiments (e.g., Long, 1959).

For scaling the hydrodynamic model to the atmospheric prototype,
we find that two parameters have to be the same: the Froude number

\[
Fr = \frac{\bar{u}}{(g \beta)^{\frac{1}{2}} h},
\]

based on the height of the fluid, \( h \), and \( R \). But \( Fr \cdot R = a/h \), so that
the model may be scaled just as easily by \( Fr \) and \( a/h \). It also is
necessary, of course, to provide geometrical similarity, and to provide
a velocity distribution upstream of the model mountain similar to the
atmospheric case.

We note in passing that the study of dimensionless equations and
the model experiments give information about the nature of the solu-
tions to the equations of motion. For example, we find that the
Froude number \( Fr_1 \), based on a vertical length scale inside the fluid,
always is adjusted automatically to the order of unity for mountain flows. If Fr is large, the disturbances induced by the mountain are small and the flow is like potential flow. As Fr becomes smaller and smaller, the disturbed velocity gets larger. For a given Fr, the larger the obstacle, the larger the disturbance. When $Fr \approx a/h$, one gets closed eddy flow which is evidently turbulent. For equivalent cases where $R \approx 1$, one obtains cases which are related closely to atmospheric motion over mountains.

An example of flow over the Sierra Nevadas towards the Owens Valley is described (see also Long, 1959). This experiment involved the towing of a model of a cross-section of the geographical region containing the mountains and the valley through water stratified with salt solutions. The upstream velocity in the model is uniform, but the prototype shows a strong increase in wind with altitude upstream from the mountains. Nevertheless, the flow in the model duplicates the Owens Valley wave phenomena fairly faithfully. At low Fr, wavy jets are observed and the mountain flow is rather complicated. The jet phenomena has not been observed in the Owens Valley probably because the vertical motions are too small to produce clouds. At $Fr \approx 0.1$, three lee waves form as observed in the atmosphere, and at $Fr \approx 0.2$, the well known one-wave case can develop with a rotor forming under the crest of the wave, as found at the same value of Fr in the atmospheric prototype.

Even though friction was disregarded in establishing the modeling criteria, both the model and the prototype in the Owens Valley study suggested a frictional effect of the rotor formation, which is believed to be related to boundary layer separation in the lee of the mountain range. Perhaps the model and the prototype behave in the same way with respect to this frictional effect because the turbulent Reynolds number of the prototype is effectively the same as the Reynolds number of the model, based on the molecular viscosity. The correspondence also may result in the "automatic" scaling of the larger energy containing eddies developing in the two systems.
In conclusion, I want to mention one last possibility for strict models between a stratified flow and a rotating system. It is well known that there is an analogy between baroclinic fluid motion and motion of a homogeneous fluid undergoing rotation with friction. If we neglect conduction of heat (or salt in a liquid), and use the Boussinesq approximation, $\Delta \rho / \rho << 1$, a strict model, based on the same simplified equations of motion, can be made. Thus for the case of a uniform current upstream, one can investigate the flow of a stratified fluid over a long flat plate, say, and relate this directly to analogs of flow on a rotating earth. We are presently undertaking some experiments of this kind in our laboratory.

DISCUSSION

In general, it is not possible to construct strict dynamic models of geophysical phenomena, except in special cases. However, continued basic experiments using hydrodynamic or aerodynamic analogies to atmospheric phenomena are still certainly worthwhile. These experiments are useful tools towards understanding fluid motion under given boundary conditions, and thus will provide stimulation for new theoretical work. These kinds of experiments should be undertaken only using the simplest geometries. There is no point in duplicating topography at this stage in these fundamental studies.

As I have indicated, there are a few cases, say in mountain flows, where "complete" similarity is possible, and one can do modeling with a fair degree of assurance. If a group of people were working on these kinds of models, one might come up with other cases. It also is conceivable that such a group could make a truly basic discovery or find a strict model of the general circulation; that is, a model at least locally of large-scale flow stratified fluid rotating on a sphere with a central gravitational field. The trouble with the
dishpan experiments is that the fluid flow develops in a uniform gravity field rather than a central field of body force. Therefore, you cannot get the same equations of motion between model and prototype. Robinson's group at Harvard is looking into this problem with particular interest in the ocean circulations.

It may be possible to devise a basic shearing flow which corresponds to the basic variable vorticity field in the atmosphere. By doing this, one may model locally large-scale (∼ 1000-km horizontal length) atmospheric motion. It does not appear to be possible to model the entire spherical field of flows.

The dishpan experiments certainly were valuable in increasing our understanding about large-scale motion in the atmosphere. But the "cream was skimmed off" very quickly in these studies, leaving the big unanswered question in the dishpan experiments: why is atmospheric motion, which we feel sure is controlled largely by the $\beta$-effect, so similar to the motion of stratified fluid in rotating dishpans where there is no $\beta$-effect? In answering this question, someone can make a real contribution to geophysical fluid mechanics. This question is even more critical for the model of ocean circulation since the $\beta$-effect is more predominant in the oceans. We really need to devise good models for the $\beta$-effect now. However, this is largely a theoretical problem which cannot be attacked by a "theory aware" experimenter alone. If a group of workers from technicians to theory aware experimenters to theoreticians could be brought together for this problem, perhaps we might get somewhere.

There are conceivably some modeling projects that one might want to undertake on a large scale. For example, it appears that the theoretical problem of modeling flow over mountains is well enough understood to begin routine modeling studies in this area. The meteorology in mountainous regions is often strongly influenced by the vagaries of mountain flow, which make forecasting difficult. It seems that a pilot program of modeling could be worthwhile to start
in connection with Lilly's observational program. If modeling proves
to be a successful tool here, one might extend the program gradually
to larger models which duplicate substantial regions of topography.
I believe the simultaneous modeling attack on the mountain flow prob-
lem, even if qualitative, could help an observational program. The
trouble with taking observations alone is that it can lead to frus-
tration quickly. Realistic geographic sampling is usually too small
to get a good picture of what is going on in a region of, say 100 km².
However, the use of models could give you some direction and feedback
in pinpointing classes of flow patterns, and it might even suggest the
best network of observational stations to be used in the field.

This kind of project is a big operation, and represents a sys-
tematic effort for a large group. Some universities might do this,
but under our present structure, we at Hopkins would not undertake
this kind of project.
There is a wide variety of astrophysical and geophysical problems that can be classified as hydromagnetic phenomena. For the laboratory experimentation of such phenomena, it may be worthwhile to look into the possibility of dynamical similarity between the actual phenomena and laboratory experiments in the manner similar to that which has been employed in hydrodynamic modeling.

In hydrodynamics certain relevant, non-dimensional parameters of the problem are usually selected, and by attaining the same values for these parameters, the modeling is achieved. A set of such non-dimensional parameters for hydromagnetic problems has been discussed by Beiser and Raab (1961) for a general class of unstratified flows in a non-rotating coordinate system. One important distinction in hydromagnetics is that the macroscopic transport coefficients, such as the coefficients of viscosity, thermal as well as electrical conductivity, become tensor quantities in most of the problems. Though this is true in the large-scale atmospheric dynamics where eddy diffusivity in the horizontal direction is commonly assumed different from that of the vertical direction, this effect appears at the molecular scale in hydromagnetics. In particular, some of the hydromagnetic phenomena in astrophysics and geophysics can be explained only in terms of such a spatially inhomogeneous transfer effect. In the latter case, it is necessary to examine the scaling parameters from the equations governing the macroscopic transfer effect, i.e., the Boltzmann equation or the generalized Vlassov equation.

* Though the total number of independent non-dimensional parameters are given by the $\Pi$-theorem, the choice of a particular set depends strongly on the problem (see for example Birkhoff, 1950).
Consider in the first case a fluid having a constant electrical conductivity \( \sigma \), permittivity \( \varepsilon \), permeability \( \mu \), and specific (kinematic) viscosity \( \nu \), and its motion described by the velocity field \( \vec{v} \). Then the hydromagnetic behavior of the fluid is governed by Maxwell's equations, with Ohm's law for a moving media, and the Navier-Stokes equation which includes the Lorentz force terms \( \frac{1}{\mu} (\nabla \times \vec{B}) \times \vec{B} \). These equations are in MKS units:

\[
\nabla \times \vec{E} = -\frac{\partial \vec{B}}{\partial t}, \quad \nabla \cdot \vec{B} = 0,
\]

\[
\nabla \times \vec{B} = \mu \vec{J} + \mu \varepsilon \left( \frac{\partial \vec{E}}{\partial t} \right), \quad \nabla \cdot \vec{E} = \eta \varepsilon
\]

\[
\vec{J} = \sigma (\vec{E} + \vec{v} \times \vec{B}) + \eta \vec{v},
\]

\[
\frac{\partial \vec{v}}{\partial t} + (\vec{v} \cdot \nabla) \vec{v} = -\nabla p + \frac{1}{\mu \rho} (\nabla \times \vec{B}) \times \vec{B}
\]

\[
+ \nu \nabla^2 \vec{v} + \frac{\nu}{3} \nabla (\nabla \cdot \vec{v})
\]

where \( \vec{E} \) is the electric field strength, \( \vec{B} \) the magnetic field induction, \( \vec{J} \) the current density, \( \eta \) the charge density, \( p \) the static pressure, and \( \rho \) the density, respectively.

In all we have 13 equations, \( \sigma, \varepsilon, \mu, \nu, v, E, B, \eta, J, p, \rho, t \) (time) and \( \ell \) (length) with four fundamental dimensions \( M \) (mass), \( L \) (length), \( T \) (time) and \( Q \) (electric charge). However, out of these 13 quantities only 10 quantities are non-redundant; thus by applying
the \( \Pi \)-theorem, we can conclude that 6 independent non-dimensional parameters \([ 6 = 10 \text{ (non-redundant quantities)} - 4 \text{ (basic dimensions)} ]\) exist and these parameters must be scaled for dynamical similarity between the prototype and the model. The parameters include the usual hydrodynamic numbers, the Reynolds number and the Mach number, plus others which denote the effects of the electric and magnetic fields as well as the electromagnetic properties of the fluid (see Beiser and Raab, 1961).

In the second case, we start from the Boltzmann equation as the relevant governing equation. In this situation, we find 8 non-redundant quantities with the same 4 fundamental dimensions, giving 4 independent non-dimensional parameters for the scaling. These quantities, in essence, represent the effect of an electromagnetic field during the time between the successive collisions of the particles and they contain ratios of the well known parameters such as the Larmor radius, the electric drift speed, the mean collision frequency, together with the characteristic scale length and time of the phenomena.

One important effect which must be stressed here is the Hall effect, which represents an enhanced diffusion in the direction perpendicular both to the magnetic field and gradient of the macroscopic properties such as temperature, density, and velocity (somewhat like the geostrophic wind). In the presence of a strong magnetic field, the Hall diffusion is increased by the factor \((\omega/\nu)\) (where \(\omega\) is the cyclotron frequency and \(\nu\) is the mean collision frequency), while the diffusion in the direction of gradient of macroscopic field perpendicular to the direction of magnetic field is reduced by the factor \(1 + (\omega/\nu)^2\). The diffusion along the direction of magnetic field remains unaffected (see Chapman and Cowling, 1953).

When we construct a complete dynamical model of hydromagnetic phenomena in the geophysical or astrophysical context, we must further take into account the action of gravity and rotation as well as the temperature field in the governing equations. This implies that in
addition to the 10 non-dimensional parameters discussed by Beiser and Raab, we must consider non-dimensional parameters such as the Froude number, the Rossby number and the Prandtl number in the modeling. Further inclusion of the effect of radiative transfer can add at least two additional non-dimensional parameters to such a list, i.e., the ratio of photo to collisional cross section as well as the ratio of the emission to the absorption profiles of the specific radiation.

Furthermore, the hydromagnetic effect arises from the interaction between the electric current carried by the fluid and a magnetic field (the Lorentz force term in the equation of motion), while the current denotes the net differential flow of positive and negative charge particles, thus the question of thermodynamic equilibrium between these particles enters into the problem.

This question arises from the fact that usually the negatively charged particles are electrons and the positively charged are ions. Consequently, in the collision between these two particles, due to the large mass ratio (1,860), the electrons are scattered readily by the ions while the inertia motion of ions remains virtually unaffected. In other words, the temperature of electrons and ions can remain different until a sufficient number of collisions between these two particles occur;* the conditions prevail in high temperature rarefied gases such as solar corona, chromosphere and solar wind.

With these considerations in mind, the limitations of the various macroscopic transfer coefficients and the macroscopic description of the problem have been discussed in part by Kantrowitz and Petscheck (1957). They have classified various regions in terms of the strength of electric or magnetic field effect referring to a characteristic macroscopic

* We might call this situation the "macroscopic local thermodynamic non-equilibrium" in contrast to the common "non-local thermodynamic equilibrium" used by the astrophysicist. The latter refers to the microscopic nature, i.e., the excitation or de-excitation of the atom or ions in different quantum states.
scale length of 1 cm. Since the limit of present solar observation is at best about one second of arc corresponding to the linear dimension of 700 km, one can relax considerably one's classifications and apply a macroscopic analysis over a wider range than suggested by Kantrowitz and Petscheck. However, one has to be careful about such cases because under such circumstances the large-scale transport effects are represented by the tensor eddy diffusivity as in the case of atmospheric large-scale motions, and the details of the continuum analysis are likely to be lost completely.

In any case, if we place reasonable numbers into typical parameters governing a hydromagnetic system, it is readily apparent that no complete similarity between geophysical or astrophysical phenomena and laboratory studies can be attained. Therefore, experimentalists must look for the "indirect" experiments which simulate certain particular aspects of the natural hydromagnetic phenomena.

In reviewing briefly various experiments that have been performed so far in hydromagnetics, we may group these experiments into the fundamental experiment and the simulation. The first group of experiments which has provided the basic understandings and informations on geophysical and astrophysical phenomena include:

1.a) The examination of the exact magnitude of the Lorentz force term by measuring its effect on the inhibition of the onset of convection (Nakagawa, 1957) and of turbulence (Murgatroyd, 1955).


1.c) The study of hydromagnetic shock waves (Gross, 1965).

1.d) The enhancement of the magnetic field by fluid motion (achieved only by means of ionized gases in thermal fusion works).
The second group of experiments which has been undertaken to simulate specifically certain geophysical or astrophysical phenomena are:

2.a) The studies of the aurora and the auroral zone (Block, 1955).
2.b) The studies of the interaction between the solar wind and the earth magnetic field (Bostick, et al., 1962; Osborne, et al., 1966).
2.c) The examination of the whistler mode propagation (NBS, Boulder).

A number of experiments on the subjects (2.a) (2.b) and (2.c) are currently being developed and pursued at various laboratories.

Our current effort in the laboratory experiments are:

1) The examination of the mutual effect of macroscopic flow and radiation under non-local thermodynamic equilibrium conditions (the conditions prevail in most of the solar atmosphere). By using an electromagnetic shock tube, we produce such conditions behind a self-ionizing shock wave and measure the radiation and fluid motions. Then we compare the results with theoretical computations based on the appropriate macroscopic flow equations including the effect of radiative transfer.

2) Study of the interaction between plasma flow and magnetic field. Our particular interest in this study is the examination of the stability of magnetic neutral line which bears basic importance in the understanding of the possible physical mechanism of solar flares as well as magnetosphere circulation of the earth as proposed by Levy, Petscheck and Siscoe (1963).

We are also devoting part of our effort to developing the theoretical interpretation of the results of laboratory experiments.
In concluding this lecture, I would like to stress the importance of the laboratory experiments in this field. Though the scale modeling is impossible, simulations of particular aspects of the hydromagnetic phenomena in nature provide us with the only means to examine the soundness of our understanding of the physical mechanisms of the problem.

**DISCUSSION**

In some cases, direct hydrodynamic analogies to hydromagnetic systems are possible (Frenkiel and Sears, 1960). For example, for a non-dissipative flow, the governing equation of the magnetic field is identical in the form with that of the vorticity equation and direct correspondence between the vorticity and magnetic field exists. Also, the possible analogies between electrohydrodynamics and hydromagnetics exists which has been discussed by Stuetzer (1962).
14. MODELING OF FLOW IN A DISTURBED BOUNDARY LAYER

(Lecture of E. J. Plate)

In general, modeling of fluid flow requires application of the principles of geometric and dynamic similarity. For modeling atmospheric boundary layers in steady flow, one may consider essentially two different cases where the lower boundary is homogeneous, or non-homogeneous in surface properties such as roughness. When the lower boundary is homogeneous or uniform, modeling requires simple geometrical scaling, and correspondence between the Rossby number, the Reynolds number, the Froude or Richardson number, and the diffusional parameters, the Schmidt number and the Prandtl number. If the lower boundary is non-homogeneous, modeling of the flow becomes more complicated.

The Rossby number, of course, sets an upper limit on modeling in a straight wind tunnel. To operate in a regime where the Coriolis force is unimportant, the Rossby number must be greater than unity.

When there should be a balance between pressure and momentum only, and we look for gross features developing away from the boundaries, geometric scaling leads to reasonably successful modeling, as for example, in our study of flow around Candlestick Park (Cermak, et al., 1963). On the other hand, if the effects of viscosity need to be taken into account and the Reynolds number Re comes in, application of the principles of modeling becomes more involved. For example, the proper choice of Reynolds number frequently depends on the particular problem and the nature of flow to be studied. There are at least three different forms of Re that one may use in modeling, including the one based on a gross length scale and a free stream or "geostrophic" velocity; the one based on the wall layer properties, using the friction velocity \( u^* \) for a characteristic velocity and the roughness length \( z_0 \) as a characteristic
length; and the one based on a velocity of turbulence; i.e., a mean square fluctuation, and scale like Taylor's microscale or an integral scale. Under homogeneous boundary conditions, one hopes that all three of these characteristic numbers are proportional, or at least simply related.

In dealing with the Richardson number, we must consider this parameter in its original sense, as a local measure of the ability of a thermally stratified fluid to damp out its turbulent motion, and perhaps in a more useful sense, also a number characteristic of the behavior of an entire layer in the atmosphere.

For diffusional modeling in a turbulent medium, the Schmidt and Prandtl parameters based on (assumed) constant eddy coefficients seem to be of central importance. Normally these numbers are (wrongly) assumed to be unity. The use of these parameters and their proper values remains an open question.

When modeling criteria are applied to non-homogeneous or non-uniform boundary conditions, more uncertainties arise. In fact, we cannot really be sure at this stage just what the complete modeling criteria are. This can be illustrated by considering flow from a nearly smooth surface like a lake onto a very rough surface like a forest. Two Reynolds numbers based on $u^*$ and $z_0$ are clearly involved. In some cases, it does not appear that dynamic similarity for these configurations is possible. In addition to the problem of correspondence in $Re$, there is the question of the geometrical distortions in scaling of vertical and horizontal lengths. These cannot be treated in any simple manner.

If differences in thermal stratification over non-homogeneous terrain further complicate the picture, modeling is made even more difficult. Similarity in the development of thermal layers may not be at all compatible with similarity in the momentum boundary layers.
There is virtually no information available about the nature of turbulence upstream from bodies or obstacles. The structure of turbulence retains a memory of past disturbances for many lengths downstream from disturbances, and this information must be incorporated into more sophisticated models of atmospheric surface layers.

Very little is known about similarity in unsteady flows. The whole question of unsteadiness is partly a semantic problem. The regime between fluctuations in atmospheric turbulence and very slow diurnal changes, for example, is not well defined. The criteria for determining when planetary boundary layers can be treated as quasi-steady have not been determined. Perhaps the numerical integrations of the large-scale motion can provide some insight into this problem.

For non-uniform boundary and initial conditions, there are virtually no guidelines for modeling except possibly in the work of Karlsson (1959). This seems to be a vital area for future work by combining field studies with wind tunnel investigations.

As a beginning of studies leading to modeling principles for disturbed boundary layers, we have undertaken some experiments in the CSU tunnel. The first investigation consisted of an attempt to find out what the nature of the boundary layer flow is over a surface roughness whose height is the order of the boundary layer thickness. We found that the air motion could be divided into two layers, one inside the roughness elements, and the other in the region above the elements. The logarithmic profile of velocity is established in the outer layer, while the inner region, through displaying similarity profiles, depends on the nature of the roughness. Dynamic similarity seems to require only correspondence of parameters based on $u^*$ and $z_0$ for neutral conditions of stability. Of course, problems still lie in properly defining both $u^*$ and $z_0$. These results are discussed in more detail by Plate and Quaraishi (1965).
So far our work on modeling of the mean flow in thermally stratified layers indicates that, provided the heat flux across the lower boundary is estimated in a reasonably way, the Monin-Obukhov length seems to scale both the laboratory data and the available field data satisfactorily (see Plate and Lin, 1966).

Some recent experiments on a simple analog of a hill have indicated a number of interesting features for flow in disturbed layers. For example, the recent semi-empirical theories for multi-layered regimes in flow (e.g., Townsend, 1965) do not correlate our data very well. Since these theories represent an oversimplified picture of the flow behind obstacles, more experiments are needed to elucidate the details of this type of motion. So far, the scaling of these types of flows may be accomplished best by using an overall momentum balance analogous to the classical momentum integral of von Karman (e.g., Schlichting, 1960). Lin and I (Plate and Lin, 1965) have found that the scaling depends essentially on the drag coefficient of the obstacle and the length ratio $h/z_0$. This, of course, is consistent with Jensen's (1954) criterion. Jensen required correspondence in geometry and in the ratio $h/z_0$. It appears, however, that the correspondence in drag coefficient of the body may be more important than strict geometrical similarity.

It is interesting to note that, in the cases we have studied, there is so much mechanical turbulence generated in the lee of obstacles that this effect overshadows effectively the influence of stratification.

DISCUSSION

Wind Tunnel Modeling

Wind tunnels play a twofold role in laboratory studies of atmospheric motion. In the first use, tunnels are essential for carrying
out fundamental studies of turbulence. These types of investigations must be pursued to forward the basic knowledge of the behavior of turbulent shearing flows. The second role lies in applied meteorology, and in the modeling of specific flow configurations. Just as the Froude models in water tanks have proved useful to hydraulic engineering, wind tunnel modeling will provide the guide lines for environmental engineering studies, and for design of aerodynamically efficient structures. Within this framework, the wind tunnel can be viewed as a basic research tool, and as a highly useful service facility for routine studies of an applied nature. The latter aspect should certainly be borne in mind in NCAR's future plans for environmental studies.

The state of modeling theory in wind tunnels is relatively advanced in cases where geometrical similarity only is necessary, and to a lesser degree where Froude similarity is a requirement. The theory is hazier for Reynolds number criteria, and is very uncertain for combined Reynolds, unsteady and thermally stratified flows.

At the present time, there are several groups of scientists using a variety of moderately large subsonic wind tunnels for study of turbulence and simulation of atmospheric boundary layer flow. Some of these include groups at NYU, the University of Marseilles, Cambridge, and The Johns Hopkins University not to mention the tunnels used by the Forest Service, etc. These groups are progressing slowly on many problems. Manpower for carrying out the experiments and analyzing results is quite limited in this field, so that opening a new program, though perhaps desirable in principle, would spread existing talent even thinner. In view of this, it is likely that a new wind tunnel will not be necessary for continued progress in this area for about 5 to 10 years. After this time, many of the current problems about modeling and about the nature of turbulence will become more clear. Then a tunnel can be designed which will allow a much greater step forward in continuing to investigate these problems.
If a new subsonic wind tunnel should be built in the future, some features might be incorporated that have turned up after using the CSU tunnel. The tunnel itself should be in a sealed environment to reduce the effects of fluctuations in ambient, atmospheric pressure; the inlet section should be designed for very low speed flow in the test section if thermally stratified motion is to be investigated over a wide range of Fr and Re; the tunnel should contain a dust separator, and the cooling system as well as the dehumidifiers could be redesigned for attainment of wider ranges of stratification.

To undertake three-dimensional studies of disturbed surface layers, a wider tunnel would be useful to reduce the interference of the wall layers with lateral secondary flows. An alternative to a wider tunnel might be to artificially thicken the floor layer in a narrow tunnel. This would allow a thick layer on the bottom of the tunnel with much thinner wall layers. There seems to be considerable uncertainty at present about the exact way of artificially inducing development of a boundary layer. Some work of Klebanoff and Diehl (1952) at the National Bureau of Standards has provided preliminary criteria, but these results are not conclusive.

To reduce the effects of secondary circulation in a tunnel of rectangular cross section, an oval shaped design for the test section might be used. The plate could be suspended in the rounded section by thin supports. The suspended plate should be as wide as possible to avoid edge effects interfering with the primary flow along the plate.

A completely sealed tunnel would be useful for studying turbulent motion with various atmosphere from, say, helium to freon gas. If the modeled motion were largely independent of molecular viscosity, for example, the turbulent Reynolds number criterion would be placed on much firmer ground.

Even though a new tunnel cannot be justified completely based on present needs, it would certainly be utilized easily once it was in
existence to augment present facilities. There is a great deal of vital exploratory work to be done in modeling of surface layers, and wind tunnel experiments are sophisticated and time consuming. Only a limited number of experiments can be done in existing tunnels so a new facility would always be welcome. It should be mentioned here that the Argonne National Laboratory has proposed that a cooperative tunnel with several midwestern universities be constructed at Argonne. This facility would add one more tunnel to the existing equipment.

If a new facility were to become available with a central operating staff, many students and research workers could use the equipment on a short term basis (e.g., summers or a few months at a time). The driving force for the program, and the scientific coordination and continuity, should naturally come from within the organization making the most use of the capability so that the program and the equipment will be effective. A full time operating staff of at least one scientist, one electronics engineer, and two to three technicians would be desirable.

To best utilize present facilities there may be some merit in considering a plan for loosely organizing a number of facilities in the country under unified funding and direction, especially for encouraging interchange of independent ideas, and different workers. NCAR probably should consider obtaining at least a small, flexibly designed wind tunnel anyway so that certain limited experiments can be done conveniently "in house."

Rotating Systems and Planetary Boundary Layers

Continued attempts to model tornado vortices in either rotating tanks of gas or water do not appear to be very attractive at the moment. Although the work of Turner and Lilly (1963) indicated that these models are feasible and are "look alikes," further work along this line is not fruitful until the energy source for maintaining a tornado is better understood. We need some temperature measurements inside the vortex. Hydrodynamic estimates of energy sources seem insufficient to maintain
a tornado. The temperature within the vortex may be as much as 100°C higher than we anticipate from hydrodynamics to maintain an inner pressure sufficiently low to keep the tornado going. This kind of heating may come from electrical effects, as postulated by some recent speculations of Vonnegut, for example.

The planetary boundary layer, that is, the Ekman layer, has been modeled by applying the turbulent Reynolds number correspondence to the results of a laminar Ekman layer in water (e.g., Faller, 1964). Unfortunately, the question of comparing laminar flow results to turbulent flow cases through this kind of Reynolds analogy is unanswered. It would be desirable, therefore, to repeat Faller's experiments in a turbulent medium. The possibilities for undertaking these experiments in a tank larger than Faller's 3-m diameter facility were discussed. Probably the experiments could be done with a rotating pan containing air (for structural reasons).

The construction of a rotating tank facility of a large size would be expensive and should be justified by a series of experiments more extensive than the Ekman layer work alone. Perhaps the flow of layers disturbed by obstacles or by differential heating would be of interest. Much of this kind of work could be done by computer analogy. These experiments would certainly be valuable from the standpoint of extending the fundamental knowledge of fluid dynamics; however it is not clear at this point how applicable such work would be to modeling the motion in planetary layers. On the basis of atmospheric simulation alone, a tank facility of this kind probably could not be justified by these experiments unless there was a strong interest to undertake this sort of study within the NCAR staff. It seems that the first steps in clarifying the role of steady Ekman layers in atmospheric phenomena may be to repeat Faller's study in his equipment using a roughened bottom. This would induce turbulence in the lower layers at much lower Reynolds numbers, and might indicate some features of the turbulent layer which have not shown up as yet, and which would be worth studying in a larger, more elaborate system.
15. THE SIMULATED BEHAVIOR OF CLOUD PARTICLES

(Discussion of J. D. Sartor)

INTRODUCTION

At present, it is virtually impossible to simulate in all complexity the interaction of particle collection and charging. Even if it were possible, it is highly likely that it would not be desirable, in that to obtain all ranges of interactions it would be necessary to have a simulation chamber that essentially reproduced the atmosphere. Since the atmosphere itself is freely available, this can be used. However, since we see mainly the integrated result of many particular motions when we look at a natural cloud, it is very desirable to try to bring particular stages of this action into the laboratory for understanding. Then, in order to complete our research, the full range of interactions is put together by computer models and by returning to the natural cloud for final testing. There is a fairly large number of these particular interactions that we can study piecemeal in the laboratory, and together with suitable theories, get a reasonable idea of what to expect in nature.

THE USE OF HYDRODYNAMIC MODELS TO CHECK THE HYDRODYNAMICS USED IN CALCULATIONS OF COLLISION EFFICIENCIES

At the present stage of understanding, it is generally easier to consider the interactions between liquid drops because of their relatively regular sphericity or deformation from sphericity. In some cases it is easy to make the extrapolation to solid particles and in other cases it is quite difficult. We are considering laboratory simulation of atmospheric problems, and the simplicity of working with drops makes this restricted approach acceptable at the present stage of study. Both laboratory and numerical studies on cloud and rain
drops are concerned with two quite separate aerodynamic flow regimes. For the smallest cloud drops with radii less than 30 \( \mu \) one can consider mainly the viscous forces, and for the larger drops the viscous forces are neglected and the inertial forces considered. From the standpoint of hydrodynamics and the understanding of droplet reaction to collision and near collision, the transition region where both viscous and inertial forces are important is poorly understood. However, the theoretical studies of collision efficiencies in this regime do not seem to be very sensitive to the various approaches to the hydrodynamics.

In the viscous flow regime, the theory is in pretty good shape. It can now be shown that it is possible to check the hydrodynamic theory with experiments on the spheres in viscous media in the laboratory. For example, by including an induced mass term in the equations of motion without specifying its form in any greater detail than by adding roughly 50\% to the mass of the drop, it can be shown that it does not affect the trajectory of two drops to a measurable degree. This is taken as confirming evidence that, when the Reynolds number is small with respect to unity, the ratio of viscous to inertial forces is small. One is then free to choose spheres and suitable media for laboratory use as long as the free-fall Reynolds number remains small. The trajectories of two spheres obtained in the laboratory in this way can be used to check the hydrodynamics.

The interaction of raindrops with cloud drops can be accomplished with water drops in air as has been done a number of times without resorting to indirect modeling. This is also true, to some extent, in the transition region between 30 \( \mu \) and 100 \( \mu \) radius.

**COALESCEENCE**

There are two main problems in the theory of coalescence: the deformability of the drops themselves and the removal of the cushion of air between them (which is a function of this deformability). For the larger drops of precipitation size, this can be done directly in the
laboratory by studying pairs of drops. However, it is not as easy to
do this with freely falling drops, although it seems well within the
realm of possibility to try. The second major problem in droplet
growth by coalescence is the understanding of the stability of a cloud
of droplets. This involves the growth of the drops by diffusion and
coaalescence as a function of the saturation of the air, and the role
played by the complete absence of electric charges and fields. These
problems can be studied under controlled conditions of free suspension
for the droplets by using a vertical wind tunnel with precision flow
control.

PROBLEMS INVOLVING THE ADDITIONAL FORCES OF ELECTROSTATICS

The small fields and charges in the early stages of cloud growth
have a small effect on the direct collision efficiency of cloud drops.
However, as the cloud matures, the electrostatic forces cause an in-
crease in the probability of collision by a hundred-fold or more for
drops in electric fields of the order of those of thunderstorms. In
the overall problem this cannot be ignored, since there are many in-
stances in the atmosphere in which fields of this order do build up and
where the precipitation-growth process must continue for a considerable
period of time. In some clouds, these forces may be necessary to ex-
plain the duration of the precipitation and its intensity. For those
convective clouds which grow up in families, the successive turrets
develop in regions of high fields; therefore, the precipitation growth
process can be expected to grow in a much more rapid growth regime.

A problem of some difficulty now enters the picture. There is a
matter of definition of collision efficiency when cloud particles are
both charged and in an electric field. It seems that in many cases the
only way an integrated result might be possible is to follow the motion
and interactions of individual drops. This is not feasible on the com-
puters of the present, because of the great capacity and computation
time required. It is possible to model the hydrodynamics and
electrostatics in models of different materials for the drops and media. The scaling is different for the electrostatic forces and the hydrodynamic forces but can be accounted for in the modeling.

It has recently been shown that charged drops emit electromagnetic radiation on close approach or collision. This offers a possible diagnostic tool for natural clouds. It is necessary, however, to understand the form of the electromagnetic emission. This must be done with highly regular, repetitious encounters between drops when they are charged and in electric fields. The time "jitter" between drop encounters can now be controlled to within $\pm 1 \mu$sec. Laboratory extensions of pairs of drops interacting can be made by use of swarms of drops. This requires a large vertical wind tunnel because of their considerable relative fall velocity.

**PRECIPITATION GROWTH**

The computer models of the growth of precipitation from cloud drops are far from satisfactory, but at the present time, do offer a reasonable and useful means of proceeding with the problem. The computer models can be applied to the results of laboratory studies piece-meal by following the interaction of two mono-dispersed clouds of droplets in a vertical wind tunnel.

**SUMMARY**

In summary, it seems possible to simulate piecemeal, many of the interactions between droplets and raindrops and eventually frozen particles by laboratory models or studies of the particles themselves in the laboratory. Some of these require special equipment that almost any laboratory can develop. The interactions between swarms of droplets or clouds of large mono-dispersed drops with a cloud of mono-dispersed smaller droplets require a specially designed vertical wind tunnel for this purpose. On the average, a $10 \mu$ cloud droplet must fall 6 m before having an equal probability of encountering another cloud droplet. For droplets of this size, the time required to fall
this far is 10 min. To ensure that enough interactions for study occur, the vertical dimensions of the wind tunnel must be stretched by upward moving air. The wind velocity must be carefully controlled with wall effects and electrostatic effects controlled. The vertical wind tunnel of dimensions comparable to that planned for the new building, around 9 m high and 9 m² or so of horizontal working space on each of three levels, seems to be adequate for the first stage of this research. Let me emphasize that this is a project that appears to be entirely feasible but which must be considered a research endeavor that will require a number of years of experimentation to be successful in producing the desired results. Considering the dimensions of the problem, it is not an expensive endeavor either.

Finally, after the particular parts of the problem have been solved, it will be necessary to put these together in mathematical models and then go to the natural clouds for confirmation. Such field trips will undoubtedly send the investigator scurrying back to the laboratory repeatedly. But at this time, there seem to be very promising ways of approaching the problem. This can be considered as encouraging to the future work.
Workers have demonstrated that microphysical processes have an important influence on the performance of clouds as precipitators. And, of course, it is well known that the formation of clouds and subsequent rainout varies strongly in different geographical locations where the nuclei population and composition are different. Microscale processes can be studied comparatively easily under controlled conditions of the laboratory. However, it also is necessary to know about the behavior of a variety of different nuclei produced in nature. Thus, the interaction between careful field observations and laboratory experimentation has to play a vital role in the study of the microphysics of clouds.

To see how some of our laboratory experiments relate to cloud study, let us first review some current ideas as to how atmospheric aerosols contribute to development of cloud droplets.

The simple theory of droplet birth by condensation begins with the properties of a pure, soluble, spherical nucleus at equilibrium with its environment. Thermodynamic calculations for the relation between vapor pressure of water and size indicate that a maximum in the curve for supersaturation vs. size exists around nuclei of radius $\sim 10^{-5}$ cm. The maximum in this curve is related to the competing effects of vapor pressure depression by the solubility of the nucleus in water, and the increase in vapor pressure with decrease in size resulting from the effect of surface tension (Kelvin's equation). The region of size of $10^{-5}$ cm is considered the critical size of nuclei for unstable growth to larger droplets. Particles in this size range do not affect the mass of water vapor in the system. However, particles of $10^{-4}$ cm
in size or larger represent a range when the mass of the droplets begins to interact significantly with the mass of water vapor.

It is clear that this simple picture is complicated by variations in the chemical structure of the nuclei. Atmospheric aerosols are rather well mixed species whose material may only be 5 to 10% soluble in water. It appears, however, that variation due to solubility effects probably has relatively little effect on the behavior of nuclei in condensation. Furthermore, we note that from a mass standpoint the molecular weight of a particle can vary effectively by only 3, but its mass can vary by $10^3$. Therefore, one expects that the nucleus mass is the critical parameter for initial droplet growth, and the simple theory may be adequate.

The next step in the ideal theory of cloud formation centers attention on the increase in supersaturation with time in a rising element of air. As a convective element rises, saturation will be attained. Continued rising of air will cause a degree of supersaturation in the air. As droplets begin to form increasing numbers of nuclei the supersaturation reaches a maximum then drops off to approach an equilibrium saturation level. For typical updraft speeds, 10 to 20 sec are required to reach a maximum supersaturation of order 1%.

In the simple model, the nuclei essentially provide the "door opening" for further growth under supersaturated conditions. To have a precise idea of the rate of growth, however, it may be necessary to take account of the composition of aerosol particles as well as the distribution in size. Furthermore, the chemical structure of nuclei could change the accommodation coefficient $\alpha$, which accounts for the number of water molecules colliding with the nucleus that do not stick. The rate of growth of droplets can change greatly with $\alpha$, a coefficient varying from 0 to 1.

Setting the problems of composition aside for the present, cloud formation within the classical theory will depend primarily on the
updraft speed, and the mass spectrum of aerosols entering the cloud at its base. Hence as a first approximation one needs to specify the vertical velocity at cloud base and a curve correlating supersaturation with time for a given aerosol distribution.

The total number of droplets $N$ in convective orographic clouds may vary from 10 to 3000 cc$^{-1}$. If the only two independent parameters of interest are the nucleus size spectrum and the updraft velocity $V$, one can estimate the importance of these two. Very crudely, $N \propto V^4$ on theoretical grounds. $V$ can range over about $10^2$. Therefore, the updraft velocity can affect $N$ by only about 3. However, $N$ varies in clouds over $10^3$. This implies the nature of the aerosol entering cloud base plays the primary role in explaining the variation in effectiveness of clouds as precipitators.

If the atmospheric aerosol is assumed to be the key to determining cloud growth and rain formation, we can construct an "organization" chart for avenues of study in cloud microphysics (Fig. 1).

![Fig. 1](image)

From this drawing, one observes that there are many possible ways to examine the nature of natural aerosols in relation to cloud formation. All of these approaches are certainly valid. However, it is
possible to divide roughly the branches into field programs and laboratory investigations. That is, the questions of birth and removal of natural aerosols and the verification of the applicability of the simple classical theories must largely depend on field study. On the other hand, detailed analysis of the properties of aerosols in the condensation and growth processes have to be examined in the laboratory, primarily by "indirect simulation."

Much of our work has been concerned with the development of field devices that will measure accurately the spectrum of the concentration of nuclei activated at various supersaturations. Since the range of supersaturation $S$ from 1 to 3% may be sufficient to provide information about cloud formation, we have concentrated on equipment for measurement in this range of $S$. To determine the "supersaturation spectrum," an instrument should be able to establish a degree of supersaturation around the particles and maintain long enough for particles to grow -- one needs to distinguish clearly visually between the grow-no grow cases. The well known thermal diffusion chamber seems to hold promise for such an instrument. This device consists of a vessel in which the ceiling is warmer than the bottom and both are maintained wet. A zone for maximum supersaturation develops in the center of the vessel resulting from mixing of saturated air at different temperatures (e.g., Taylor, 1917).

Droplet growth can be observed visually in a thermal diffusion chamber containing stagnant air after a sample of aerosol is injected. Our results indicated that such devices worked fairly well qualitatively, but it was difficult to quantitatively follow growth on nuclei because of depletion in $S$, and fallout of droplets. Depletion could be taken care of by diluting the sample sufficiently, but the question of fall out still remained.

To check the performance of the thermal diffusion chamber, a vertical column was designed. This unit works as follows: clean air
moves up a wet-walled tube in which the temperature increases upwards. As the diffusivity of water vapor exceeds that of heat, the central region becomes supersaturated. The solutions for the equations of convective motion, heat transfer, and diffusion for this configuration are known (Morton, 1960). The aerosols were carried upwards by the laminar flow of clean air.

Particles injected into the center section of the column would grow, the faster growing ones falling out first, and the slower growing ones falling out later. All particles can be counted in this device, and the problems of keeping a particle in constant supersaturation for a certain time are eliminated. This device turned out to be rather tempermental, but it provided a check on our results from the thermal diffusion chamber to about 30%.

Our most recent studies have involved a new design using steady laminar flow of clean air through a thermal diffusion chamber. Again the supersaturation develops in a parabolic distribution with a maximum near the center of the spacing between the moist plates. The aerosol is injected into the center of the stream of air and the entire air flow is taken out of the chamber in a jet past a photocell counter through a nozzle. In this design, the faster growing particles tend to fall out to the floor before being counted for a given air velocity. By increasing the air flow to high enough values, all of the entering nuclei eventually can be counted. The curve of number counted versus flow rate can be related, in principle, to the growth rate of the nuclei.

The new steady flow chamber is quite versatile in that it counts particles accurately and very rapidly and supersaturation levels can be maintained easily. This device shows promise (a) as a counter for field studies to determine the relation of critical supersaturation to the size distribution of aerosols, and (b) as a laboratory tool for studying some of the details of the growth process including the rate
of growth of particles and their accommodation coefficients in the condensation process.

We hope to use this new instrument in both types of studies.

In connection with the applicability of the simple "adiabatic" rise theories, it seems that more use should be made of wave clouds. Mordy's group in Nevada is beginning to look at orographic cloud properties in the Sierra Wave. It also might be useful to look carefully at our clouds near Boulder.

In other lectures of this series, emphasis has been placed on the laboratory investigation of atmospheric phenomena. Laboratory studies also can be pointed to support of field programs by developing analytical tools. These same devices can be used effectively for continued "indirect" research in parallel with atmospheric observations.
INTRODUCTION

The establishment of criteria for scale models of physical phenomena is a process which seems to go on indefinitely. As the demands on scale models increase the criteria must be improved, refined or revised to meet the more demanding requirements as the knowledge of the field increases. Rarely are the criteria in simple forms. More commonly there are conflicting requirements which lead to compromises and empirical corrections. Scale models of the lower atmosphere, the subject of this discussion, are no exceptions to this process.

Scale model experiments on wind effects of the lower atmosphere have been conducted for many decades. While some have been made with liquids, the bulk have been with air in wind tunnels. The early experiments dealt primarily with wind forces on various man-made objects. The greatest effort by far has been given to problems of aircraft flight. Over the same period various experiments have been conducted for wind loads and air flow patterns on ground based structures. These have generally been conducted in wind tunnels designed for aircraft model experiments. One of the significant features of such wind tunnels is the uniform airstream having nearly constant velocity throughout the test section and extremely low turbulence except for the boundary layer at the boundaries, which is small in thickness for smooth surfaces. While this is the best representation of the atmosphere for objects the size of aircraft for elevations removed from ground surface effects, i.e., above the shear layer, it is far from a good representation of the non-uniform lower atmosphere near the ground where most of man's activities occur. Many useful experimental studies have, nevertheless, been conducted on ground-based problems. These include smoke and gas
diffusion which have been performed largely in the last three decades. The success of these various experiments on ground based problems is due to the fact that the local air motions are dominated by the particular structures involved whose presence produces its own field of motion.

The non-uniform properties of the lower atmosphere are expressed in terms of the vertical profiles of mean (temporal) velocity and turbulence. The region in which these occur is often termed as the shear or boundary layer. These have yet to be modeled to a precise degree. The future of atmospheric modeling lies in the successful production of these properties to a suitable scale.

It is evident that a wind tunnel with its limited size and confinement of walls and ceiling in the test section is going to have inherent limitations in its representation of the unlimited atmosphere. Two general methods have been used to produce profiles of velocity and turbulence which, in the atmosphere, are the result of air passage over unlimited upwind regions as well as the influence of the overlying airstream. A common method is to allow the boundary layer to develop to the desired depth by using a sufficient length of model ground surface. This may be a geometrically scaled version of the actual prototype as is often done with models of regions with marked topographical features or a surface of selected texture of roughness to give the desired profile characteristics. Excellent examples of this method, particularly for the more nearly level surfaces, are in the wind tunnels at CSU and, more recently, at the University of Western Ontario. The development of temperature profiles may be obtained in a similar manner by control of surface temperature as is done in the CSU wind tunnel.

The second method of producing profile characteristics is the use of special apparatus at the beginning of the test section to force the desired velocity, turbulence and temperature profiles appropriate to the subsequent ground-surface boundary conditions, mechanical and thermal. This method is being developed at the New York University wind
tunnel. There are reports that it is being experimented with in equipment located in France and England.

The first method has the advantage of being more natural, but it still differs from the atmosphere in that it is a growing boundary layer with a small or negligible thickness at the beginning of the test section. It has the further advantage of a large body of background knowledge developed from numerous boundary layer investigations of the past, experimental and theoretical. One obvious disadvantage is the long test section needed. Conversely, the second method needs a much shorter test section, but it needs much development and lacks background experience. It offers the potential of greater flexibility in control of profile characteristics including transient effects. Development of both methods should continue. It may be found that each has its own area of superiority.

The following sections are directed primarily to the second method and its development at the NYU wind tunnel and results obtained.

MODELING CRITERIA

The development and selection of modeling criteria are obviously of major importance to the design and accuracy of scale model experiments. They may also play a major role in the design of experimental equipment.

One approach to the formation of modeling criteria or scale factors is the application of dimensional analysis. The accuracy of the result will depend on the selection of the variables. The following presentation is based on past experience in the NYU wind tunnel and projections into the future.

The atmospheric motions will be analyzed in terms of their effects on a gas or smoke plume emanating from a stack. Experiments on stack gas diffusion have been a major activity in the NYU wind tunnel. While various properties of the plume may be selected as the dependent
variables, all leading to similar results, the plume height and width or thickness at some downwind distance will be used in the following presentation. The independent variables are shown in the following equation:

\[
\frac{z_p}{b} = f \left[ h, \delta, x, v_s, v, v_0, \rho_s, \rho, A \frac{d \rho}{dz}, g \right]
\]

where

- \( z_p \) = height of plume axis at downwind distance \( x \)
- \( b \) = plume width or thickness at downwind distance \( x \)
- \( h \) = height of stack
- \( \delta \) = height of velocity boundary layer
- \( v_s \) = stack gas ejection speed at stack top
- \( v \) = air velocity at elevation \( z \) above the ground
- \( v_0 \) = a reference velocity or velocity above boundary layer
- \( \rho_s \) = stack gas mass density at stack top
- \( \rho \) = mass density of ambient airstream
- \( A \frac{d \rho}{dz} \) = difference between actual density gradient and adiabatic gradient
- \( g \) = acceleration due to gravity.

By dimensional analysis the following result is obtained:

\[
\frac{z_p}{b} = f \left[ \frac{h}{\delta}, \frac{x}{\delta}, \frac{v_s}{v}, \frac{\rho_s}{\rho}, \frac{v_0}{v}, \frac{\delta A}{\rho} \frac{d \rho}{dz}, \frac{v^2}{\delta g} \right]
\]

where the dimensionless ratios on the right are the scale factors which should be maintained at the same values in model and prototype. The
ratios of dependent variables on the left and the first two on the right indicate that geometric similarity should exist throughout the physical model (on its rigid boundaries) and the flow field. The third and fourth factors on the right set the plume characteristics at stack top. The fifth factor implies that similarity of velocity profiles must be preserved. The sixth factor determines the density profile. Alternately it can be expressed as

\[- \dot{\xi} \left[ (d\theta/dz) + \Gamma \right] / \theta\]

or

\[- \dot{\xi} (d\theta/dz) / \theta\]

where

- \(T\) = air temperature in absolute units
- \(\Gamma\) = adiabatic gradient
- \(\Theta\) = potential temperature.

The last factor is Froude number which determines the velocity scale.

An obvious omission in the above selected variables is the fluid viscosity which, if included, will lead to Reynolds number \((\rho v \delta / \mu)\) as a scale factor. It is impossible to satisfy Froude and Reynolds number simultaneously. The NYU experiments on buoyant gas plumes show that if Froude number is not followed, the resultant plume path is unrealistic. The experiments must, therefore, be confined to situations in which Reynolds number is not important or can be satisfied in some other way such as distorted roughness. There are, fortunately, many atmospheric problems in which Reynolds number can be neglected. It is well known that flow over irregular surfaces such as marked topography and angular objects with sharp edges is largely independent of Reynolds number except at extremely low values.
The scale factor \( v_o/v \) (or more commonly its reciprocal \( v/v_o \)) may seem impossible of satisfying in view of the extensive experimentation over the years on boundary layer flows and their dependence on Reynolds number. Here, again, cases must be sought which do not have such dependence. The possibility that this can be achieved is suggested by simple application of some well known equations for turbulent flows. The logarithmic equation for boundary layer profile for adiabatic conditions is

\[
v/v_* = (1/K) \ln (z/z_o)
\]

where \( k \) is the Karman constant (\( K \approx 0.4 \)), \( z_o \) is the roughness length and \( v_* \) is

\[
v_* = \sqrt{\tau_o/\rho}
\]

where \( \tau_o \) is surface shear stress which can be expressed in terms of a surface friction coefficient by

\[
\tau_o = C_f \rho v_o^2/2.
\]

Substitution yields another form of the profile equation:

\[
v/v_o = (\sqrt{C_f^2/2}/K) \ln (z/z_o).
\]

The roughness length \( z_o \) tends to be proportional to the size of the surface roughness elements, in which case \( z/z_o \) will be the same for model and prototype when geometric similarity of roughness elements is preserved. The roughness coefficient \( C_f \) tends to be constant for a given rough surface. The result is at least a possibility of obtaining similarity in velocity profiles.

A form of Froude number which includes fluid density difference
has been used. It may be expressed in terms of fluid density as
\[ \frac{\rho v^2}{\Delta \rho g S}. \]
This allows freedom in selection of velocity scale by adjustment of density variation, an advantage because of the difficulty of obtaining low test section velocities free of undesirable convective motions. For buoyant plumes there are experimental difficulties in obtaining substantial increase in density difference. Furthermore, in the regions near the stack there are momentum interactions which require the ratio \[ \frac{\rho v^2}{\rho v^2} \] to be preserved which is not accomplished with the modified Froude number. For flows where density profiles in the airstream are modeled, the modified Froude number may be applicable. Some cases of waves in stably stratified flows have been modeled. The modified Froude number would replace the last two factors of the above group. It is also a form of Richardson number.

There is no inclusion of turbulence variables in the above analysis. They are implicit in the velocity profile requirement \( \frac{v}{v_o} \) since the profile is dependent on turbulence shear stress. This is a subject which requires much exploration.

There are many useful experiments which can be performed without satisfying all the above scale factors as demonstrated by many past programs. An example is the effect of wind speed on the importance of atmospheric density or temperature gradient. As wind speeds increase, the effect of temperature gradient is reduced due to the greater mechanical turbulence. For higher wind speed, the effect of non-adiabatic gradients is negligible. A discussion of the various cases in which certain scale factors can be neglected is beyond the scope of this presentation.

**DESIGN FEATURES OF THE NYU WIND TUNNEL**

The NYU wind tunnel has undergone extensive development and modification over the years. It began as a conventional low speed wind tunnel with a long test section (compared with the aircraft type) for viewing smoke plumes. The test section had a uniform airstream with no means of controlling airstream or stack gas densities. Only the scale factor
was satisfied. With the introduction of stack gas density control, accomplished with helium-air mixtures, extremely low speed capability had to be developed to meet the Froude number velocity scale. This required the introduction of various temperature control equipment for those parts upstream of the test section.

The wind tunnel is of the open circuit type with intake air coming from within the laboratory and exhaust air ducted through the roof to eliminate contamination of the laboratory atmosphere by smoke and gases used in experiments. The test section is rectangular in shape, 2.1 m wide, 10.5 m high and 12 m long in the direction of air movement. The wall on one side is equipped with large windows for viewing and photographic purposes.

Stretched across the upstream end of the test section is an array of horizontal electric heating wires placed in a vertical plane. Individual control of electric heat input produces a temperature gradient of desired form and magnitude. The grid of heating wires is used only at low test speeds where non-adiabatic temperature gradients produce convective (thermally induced) turbulence or suppress mechanical turbulence.

Test section floor and ceiling are provided with adjustable heat for surface temperature control. The forward section of the walls have surface temperature control to prevent downward flow of convective air currents near the walls which continue across the floor. The various parts of the wind tunnel upstream of the test section are in an insulated enclosure which has thermostatically controlled heaters for maintaining constant temperature. Intake air is heated with thermostatically controlled heaters as it passes into the enclosure. These raise air temperature to the level set in the enclosure. The enclosure temperature is kept constant within $1^\circ\text{F}$ on a 24-hr basis during a test series.

An exhaust fan located downstream of the test section draws the air through the wind tunnel. The maximum air speed is about 6.3 m-sec$^{-1}$ in the test section. Depending on model scale, the maximum needed for
modeling atmospheric winds by Froude number is approximately 1.2 to 1.5 m-sec\(^{-1}\).

When modeling gas plume diffusion with marked topography, about one-half of the test upstream of the source is covered with the topographical model. This produces a boundary layer sufficient to envelop the plume.

Several series of experiments have been performed in which the ground was flat and level with limited surface roughness insufficient to produce a substantial boundary layer. There the concept of forcing an initial velocity and turbulence profile was applied with vertical stack of horizontally oriented roughness plates which spanned the airstream. Roughness elements were placed on the plates. Velocity and turbulence profiles of various forms were produced by selection of the size and number of elements on each plate and plate spacing.

The various available instruments for experimental measurements are conventional. They include capabilities for measurement of gas concentration.

Plans for the future include refinements and additions to the temperature controls and instrumentation. The roughness plates provide adequate control of initial velocity profile but are deficient in turbulence magnitude. Whether sufficient turbulence can be produced with static (non-moving) roughness elements remains to be proved. Some dynamic means may be needed such as oscillating vanes or air jets. Experience to date does not suggest that this is not feasible.

With proper selection of roughness plate arrangement in relation to subsequent flow roughness downstream, variation of velocity profile is maintained to a low degree but not eliminated. Ideally, as a model of the atmosphere, the profile should remain constant. One of the reasons, perhaps the most important, is the adverse effect of the walls and the ceiling. It is planned to install a moving ceiling surface by which the shear stress can be maintained in the same direction as in the atmosphere.
EXPERIMENTAL RESULTS

While the NYU wind tunnel has had various sponsorship, private and governmental, it was started with support of a private electric power utility. Over the years much of the effort has been on industrial air pollution problems, particularly for the electric power industry. These are conducted in accordance with Froude number for velocity scale but not with airstream temperature gradient since the problems usually involve the higher wind speed range. There is, unfortunately, a lack of field data for correlation with scale model results. Indirect evidence on accurate results in the form of satisfactory solutions to pollution problems is abundant. When useful field observations were available, satisfactory agreement was obtained. In one case of a topographical model, excellent agreement was obtained for velocity profiles.

With the sponsorship of the Argonne National Laboratory several series of experiments were conducted on smoke plume diffusion with initial velocity and turbulence profiles produced by the roughness plates. The heating grid was used to produce various temperature profiles in both the stable and unstable ranges. The stack was a model of the Argonne experimental smoke stack for which field data had been obtained in the form of plume spread with distance. Similar type data was obtained in the model experiments. These were compared with the Argonne data and some published field of summary forms for other field installations. The comparisons were made in two forms to bring out various features. The more significant aspects of the comparisons showed that the spread of the model plumes due to convective turbulence or suppression under stable conditions was close to that of the prototype. Model spread due to mechanical turbulence was quite deficient. This is interpreted to show that the roughness plates, at least in the form used in these experiments, were inadequate. Use of a heating grid appears to be a satisfactory method for producing thermally induced effects.
FUTURE POSSIBILITIES

Past experience with the upstream cross-section control methods of producing profiles suggests that with further development and refinement of equipment and techniques an accurate model of the atmospheric boundary layer will be produced for the steady movement of the atmosphere. With the development of the dynamic type of cross-section control a greater degree of flexibility will be achieved which will lead to models of time variable profiles to represent variation in air mass movement. Its action may be programmed on the basis of time dependent measurements of the atmosphere. Extension of the concept of dynamic control of the test section walls and ceiling will yield the possibility of controlling the cross stream component of motion. This can be used to produce such movements as convergence, divergence and cross stream shear. A further improvement in dealing with spurious boundary influences in the model behavior might be achieved by representing the atmosphere surrounding the prototype with an electrical analog representing the equations of motion of the air, whose solution at the test section boundaries would be used as an electrical signal to actuate the boundary controls. This kind of control would give continuity between the wind tunnel airstream and the atmosphere surrounding the prototype. Such a system might provide a more realistic representation of the time dependent influence of atmospheric processes.

Some elementary demonstration experiments conducted in the NYU wind tunnel showed the possibility of producing convective circulations such as one caused by differential heating of valley walls or the thermally induced drainage winds. The effect on air movement by heat from urban regions could be modeled. The modeling of inversions of various thicknesses and elevations are well within the realm of possibility. With such capability various solutions to urban air pollution will be investigated.

With imaginative thinking, serious and persistent effort, modeling
Appendix A-17

of the lower atmosphere will be advanced to a level not dreamed of a decade ago.
18. QUASI-ONE-DIMENSIONAL FLOW OF AN r-TIMES IONIZED MONATOMIC GAS

(Lecture of R. W. Truitt)

One of the more interesting fields of modern gas dynamics is concerned with the problem of the behavior of a flowing, chemically reacting gas. The primary interest of geophysicists in this class of problems centers on the behavior of partially or fully ionized gases at very low mass density, corresponding to conditions at very high altitudes in the earth's atmosphere. However, there also are other cases, particularly in flow around bodies, in which the gas densities are sufficiently high that the medium can be treated approximately in terms of well known thermodynamic equations. To illustrate some features of the nature of flowing ionized gases, we shall consider some calculations of the thermodynamic equations for an idealized flow of a multi-ionized gas through a nozzle. The case of nozzle flow is of particular interest to aerodynamicists since this is precisely the configuration that one needs to examine in the design of high speed wind tunnels for simulating flow around models of vehicles of various types.

In studying the flow of gas through nozzles, one begins with the classical calculation for the "cold" flow of gas passing through a nozzle. If the gas is in some ionized state during passage at very high speed through a converging-diverging system, the cold flow estimates may be inadequate to describe the properties of the flow. However, the actual behavior of a real, reacting gas in high speed flow is generally assumed to lie between the limiting cases of the classical "frozen" or "cold" conditions and "equilibrium" behavior. Frozen flow essentially means that if the gas contains 50% singly ionized species at the entrance of the accelerating region, it will
remain at this ionization level throughout the nozzle flow. In contrast, an equilibrium calculation takes into account the law of mass action and the thermodynamics of chemical reaction in the flowing system. The non-equilibrium case, in between the limits of frozen and equilibrium flow, involves the rates of reaction of various species of the gas. Realistic non-equilibrium cases generally are too complicated to solve even numerically at present even if the key reaction rates were known.

These questions are of considerable practical interest in designing facilities for simulating high speed, high temperature flow of gases. As an illustration, it is possible using frozen flow calculations to design for a Mach number of say 15 and realize an actual flow of Mach number of 3 to 4 under some conditions. Such differences serve to stimulate our engineering research on the aerodynamics of real gases considerably.

Since the theory for the flow of a multicomponent reacting gas is quite complex, there have been efforts to simplify the computations by assuming certain kinds of idealized gas behavior. Lighthill (1957) was perhaps the first to formulate a theory for an idealized dissociating gas flowing under equilibrium conditions. Later, Freeman (1958) considered a non-equilibrium flow of a dissociating system, and subsequent computations for nozzles flows, and a theory for ideal singly ionized gases of Bray (1959), Bray and Wilson (1960) contributed to the understanding of reacting systems. Recently, Truitt and Perkins (1961) generalized the theory for multi-ionized or r-times ionized ideal gas flows. Some interesting features of such multi-ionized systems will be outlined below.

Let us consider the general problem of the flow of a multi-ionized gas around a curved body or through a nozzle. Conceptually one can make a perfectly general analogy between these configurations for a quasi-one-dimensional geometry by dealing with behavior of a streamtube near the surface of the bodies. For the one-dimensional
configuration one obtains "exact" equations for the thermodynamics of a multi-ionized gas flow. The equations include the conservation of mass, charge, nuclei, energy, and specific entropy. And for equilibrium flows, one adds the law of mass action, or a set of "Saha" equations, as a constraint on the gaseous species in the system. Computations of a flowing gas which is triply ionized can be made by a computer. However, such a mixture involves keeping track of five species of gas, and this begins to press the capacity of even the largest existing computers.

To avoid some of the large amounts of computation, it is possible to simplify the flow problem for a triply ionized gas somewhat by making the assumption of idealized behavior. Such a calculation would require bookkeeping on 2 to 3 species in contrast to 5 in the "exact" calculation. The idealized theory can be used with reasonable accuracy provided that the levels of ionization are separated sufficiently to allow the assumption that effectively all of the r-times ionized material recombines to r-1 times ionized material, etc., during passage through the nozzle.

We have carried some computations of the "exact" equations for flow of neon in a nozzle. Neon was chosen because its levels of ionization are sufficiently far apart to allow the idealized equations for an ionized gas also to be applied. Numerical calculations verified that such an assumption could be made for neon.

The numerical results also indicated that for cases where the motion of an idealized multi-ionized gas could be used, there exist plateaus in the relation between the degree of ionization $\mathcal{E}$ and temperature (at constant pressure) where $\partial \mathcal{E} / \partial T = 0$. That is, regions of quasi-frozen flow exist even in a gas assumed to be flowing under equilibrium conditions.

For the case of a nozzle flow, one finds that the profiles of temperature and pressure of triply ionized neon display abrupt changes
in regions along the nozzle where the plateaus $\frac{\partial E}{\partial t} = 0$ develop. These abrupt changes are quite significant; for example, for temperature a drop of $\sim 10^4 K$ can be observed. No kinks are observed in the curves for density and mass flow variation along the nozzle.

Comparison between the results from the "exact" computations for triply ionized neon down a diverging nozzle and computations with the idealized gas assumptions yield very similar results. This suggests that the idealized behavior contains the essential physics of the problem, and consequently can be used to alleviate many of the laborious computations involved in the "exact" solution.

The sudden changes in temperature and pressure found in the flow of ionized gases through nozzles may have certain practical applications in controlling the stability and growth of boundary layers in large, high speed wind tunnels. One might want to take advantage of these changes in designing new facilities.
19. HYDRODYNAMIC MODELS OF CONVECTIVE SYSTEMS

(Lectures and Discussions of J. S. Turner)

The study of convective motion in hydrodynamic models has provided some interesting information about the behavior of mesoscale systems in the atmosphere. The first lecture reviews a variety of simple experiments which have shed light on certain features of mixing and entrainment, and their relation to convective cloud development. The second lecture consists of a discussion of the role of hydrodynamic modeling in future studies of dynamic processes involving convective motion.

LECTURE A: LABORATORY MODELS OF CONVECTIVE CLOUDS

Some years ago, it became clear that the growth of cumulus clouds could not be explained without accounting for the mixing and entrainment of air from quiescent surroundings into the edges of the turbulent, cloudy element. The simplest models dealing with mixing in buoyant elements of fluid have developed along two paths: the plume, and the bubble (the thermal). Both of these models assume the validity of G. I. Taylor's hypothesis that the rate of entrainment into the buoyant element is proportional to a mean velocity in the vertical direction. Using this key assumption, analyses for both the plume and the bubble model yield similar forms for the net rate of dilution of a buoyant element, and for the elements' change in shape during growth.

The main difference between the two models of buoyant convection lies in the effective rate of entrainment. The bubble model tends to spread about three times faster with altitude than the plume. Application of the concepts of these two idealized systems has suggested that cumulus clouds may behave like a combined plume and
bubble. The recent work of the Simpsons suggests further that, in the main, clouds may be more closely related to plumes than bubbles.

A number of features of buoyant elements, in which mixing into the surroundings occurs, have been explored in hydrodynamic models. Several examples of this kind of simulation are discussed, including attempts to model separately the influences of condensation and evaporation on the behavior of a buoyant element, and the induction of tornado-like vortices in a column of buoyant fluid. For a brief review of several of these experiments, see Turner (1965b).

Perhaps the most important results obtained from laboratory modeling of convective clouds are qualitative. By watching the behavior of simple models, it has been possible to better understand the dynamics of turbulent buoyant convection, and the associated processes of mixing. As a result of these new qualitative ideas, new and better theoretical models have been conceived for the dynamics of cumulus clouds.

One of the great advantages to hydrodynamic simulation (and, of course, laboratory modeling in general) lies in the observer's ability to look at the same phenomenon repeatedly under controlled conditions. This is not possible in the atmosphere. A second advantage of laboratory experiments is the ability to add complications of physical processes in a well defined way one at a time to the original model. This technique has shown very well how the influence of phase change in a cloud affects the buoyancy of an individual element which, in turn, may introduce rather important changes in the dynamic behavior of the element.

Perhaps the most important quantitative results from the laboratory models of convective clouds consist of numbers for the rate of entrainment of surrounding fluid into the buoyant element. Thus, the effective entrainment rates are predicted in advance, and these values may be applied to clouds in the atmosphere as quantitative tests of the simple plume and bubble concepts.
Although considerable exploratory work has been carried out on hydrodynamic models of cumulus clouds, little effort has been expended so far on the details of the turbulent motion in these convective systems. However, it seems that most of the features of the behavior of these model experiments which are of interest to cloud dynamicists may have appeared already. Nevertheless, the details of the dynamics of the buoyant elements in liquids will still contribute important advances to our basic knowledge of fluid mechanics.

LECTURE B: FUTURE MODEL STUDIES OF MESOSCALE DYNAMICAL PROCESSES

Having discussed the "state of the art" in buoyant convection experiments, let us now speculate a bit on new experiments and new models in this field. In this lecture, I shall deal with the general problems of transport processes in a turbulent fluid in which density gradients exist. Some applications to simulating the dynamics of medium or mesoscale systems in the atmosphere involving convection will be discussed.

The first class of experiments might be designed to fill gaps in the existing results. There are several features of convective motion that should be looked at, including: (a) the study of the detailed distribution of fluid velocity, and properties such as density and temperature in buoyant elements, (b) the use of continuous sources of buoyant fluid which are ejected into fluids of varying density while allowing the buoyancy at the source to change with time, (c) a more detailed test of the entrainment assumption using experiments of buoyant plumes and bubbles which would develop in fluids having a wider range of density differences, (d) the modeling of evaporation and condensation occurring together in the same buoyant element.*

*In this class of experiments, one should beware of overcomplication. To model both condensation and evaporation a complicated physico-chemical process may be required which may lose its relation to the actual behavior of a convective cloud.
(e) the process of breaking through cloud base (also, the nature of convection below a cloud and its relation to the larger-scale, "regular" motion in the cloud and its relation to how a cloud cuts off its own motion), (f) the question of broadening of a cloud at a given height, (g) the effect of precipitation in a liquid system on changes in buoyancy, (h) unsteady models of a cloud whose top is oscillating with altitude (mechanisms for stopping convection within the cloud), and (i) the influence of shearing motion on the developing buoyant element. This represents a respectable list of potential experiments in the study of convective systems. However, all of these possibilities involve individual "try it and see" experiments. There does not seem to be a way of organizing a systematic, long-term program within this class of investigations.

On the other hand, there are some studies that offer more tractable problems for a group effort. Among this class of work fall two projects.

The first would involve a systematic search by a group of scientists, including chemists, for suitable physicochemical systems which would have potential utility in modeling of convective clouds. For example, it would be especially useful to have an analog of combined evaporation and condensation by a mechanism equivalent to a variation of pressure with height. The present methods do not give this kind of analog since the release of buoyancy in present models depends on the rate of mixing and hence, on time rather than height alone. One possibility for this analog which has not been fully explored is the process of releasing gas from water that is supersaturated with the gas (soda water). So far, it appears that the gas available in the soda water is sufficient to provide the desired changes if all the gas comes out of the liquid at once. However, the gas release is not instantaneous at a given height; it is limited by the nucleation of bubbles.

The second project for a group study might center around a large
tank facility for producing "programmed" density and chemical properties with height. However, I cannot really recommend this kind of equipment as a facility unless there are scientists at NCAR interested particularly in this kind of work.

There seems to be only one area of convective dynamics which, at present, is large enough in scope to require the combined efforts of several workers, and to provide a long-term program for a large, sophisticated simulation facility. This is the general field of turbulent transfer processes in stratified surroundings, particularly in stably stratified systems. This area is very important to all of geophysical fluid dynamics, and it is the least well understood.

Some examples of stratification that are pertinent to the atmosphere include: the lifting of an inversion layer by turbulent convection acting from below, and the behavior of cold continental air flowing out over a warm ocean. The changes in the seasonal thermocline layer is another example of this sort of geophysical problem.

Several experiments have been undertaken in stably stratified liquids without shear which have indicated some interesting effects involved with transport of heat, salinity and momentum. Some examples have been discussed by Cromwell (1960), Rouse and Dodu (1955), and Turner (1965a).

Since the problem of turbulent transfer in shearing flows with density gradients is poorly understood, there seems to be a definite place for an extensive program in this area. Experiments both in liquid systems, and in wind tunnels capable of heating or cooling air from the bottom or top and upstream of the test section, would provide fruitful results in a long-range program of the fundamental study of stratified flow.

Once again, it should be emphasized that NCAR itself should not try to build a facility for its own sake. The stimulation, planning,
and execution of a significant, long-term program in atmospheric simulation should come from within NCAR, and not as a response to the same limited potential needs, or desires of other workers.

DISCUSSION

Modeling the Dynamics and Physics of Convective Clouds

One of the alternatives to modeling convection in the liquid phase is the use of large vessels or towers containing gas inside which the pressure may be varied in analogy to the decrease in atmospheric pressure with altitude. Although large pressure vessels may have some usefulness in studying the microphysics of growth of collection particles, it does not seem to be worthwhile for trying dynamics experiments. In the first place, it is difficult to scale the convection at high enough Reynolds numbers in chambers. But more important, the variation in pressure in the vessel, or tower, is a function of time and not of height as it should be for modeling the dynamics of buoyant elements.

Some large vessels have been constructed for studying certain aspects of droplet growth and cloud seeding. However, the experiments have not been entirely successful probably because of wall effects. It will be of considerable interest to watch for the success of the experiments undertaken at the Russians' new facility in Obninsk.

The suggestion of using a centrifuge to induce larger density gradients in a fluid does not seem to be too worthwhile for modeling experiments because of the scale factors for modeling turbulence, and the spurious effects of a large component of angular momentum associated with the centrifugal motion.

One possible simple system discussed some time ago by Squires and Turner has not been exploited as yet. This experiment would simulate
the effect of latent heat release on buoyancy by allowing small bubbles to rise and expand with decreasing hydrostatic pressure in a moderately high tank. To get a wide enough range in pressure difference with a steady plume-like system, one might evacuate the top of the tank containing the liquid.

Another possibility which may be particularly useful for studying the dynamics and growth of droplets has been suggested by A. E. Carte. Carte has mentioned recently that very deep mineshafts up to 900 m are available in South Africa. These shafts effectively provide an analog for a rising volume of moist air in the atmosphere. Since moist air is pulled up the shafts from the bottom at nearly saturated moisture conditions with temperatures greater than 90°F, water droplets can form and grow as they are carried to the earth's surface by the air flow. Carte says that substantial "rains" can develop in these shafts, especially if the fans at the surface stop suddenly. This kind of cloud column would provide an ideal simulation chamber for studying the microphysics of clouds, provided one could withstand the working conditions.

Another possibility for controlled experiments consists of studies under particular field conditions. Certain naturally suitable places have been quite useful. Some examples are the Cloud Physics Laboratory at Hilo, Hawaii, and the air-sea interaction site at Aruba. A systematic study of geographical locations for special experiments might be worth considering.

Controlled atmospheric heating to induce convection below cloud base also could be tried. One might conceive of using the electrical output of a power plant at "off hours" to get enough heat into a particular region on the earth's surface to make a significant change in the local convection. This kind of experiment would extend and improve on Dessens' ideas. One of the major disadvantages of Dessens'
experiments, which can be avoided by the heating alone, is the injection of large numbers of nuclei during the burning process at the ground.

The use of large areas of black surfacing such as asphalt to change the local temperature distribution does not seem to be as useful as the heating experiment -- for no other reason than its tendency to be "anti-conservation" oriented with respect to wild life and plant life.

Another controlled field experiment has been proposed recently by S. Colgate at New Mexico Institute of Mines and Technology. He has suggested that a natural cloud chamber for studying coalescence and electrification of droplets could be set up in a deep natural bowl. Clouds would be produced by spraying large amounts of water into the bowl from the top.

One key problem in cloud dynamics involves the question of the manner in which fairly regular, organized cloud motions get started from the disorganized, small-scale turbulence below cloud base. The notion of "continuous" plume-like columns extending from particular heat sources at the ground now seems incorrect. Some careful laboratory and field studies might be organized along the lines of this problem.

Facility Program

Generally simulation experiments involving convective motion in fluids seem to be best suited to the simple equipment of small investment, under the direction of a single worker, or a university professor with a sequence of students. The one possible exception, of course, is a well conceived systematic study of thermally stratified shearing flows in a well equipped wind tunnel.

A loosely organized co-operative program between various institutions having existing special facilities for simulation might have some merit. Arrangements for exchange of ideas and people would
help some programs. The solution to the question of communication between people presently working in simulation seem to be more important than trying to establish a network of "available" facilities.

Education of scientists who will be interested in, and will undertake, simulation experiments has always been a problem. It is not possible to "legislate" for a necessary number of people to do modeling of cloud convection even if financial support is available. Some changes in the historical biases of physics departments in universities should certainly help to encourage more physics students to pursue studies in fluid dynamics and geophysics.
20. SOME SIMULATION EXPERIMENTS
PRIMARILY IN ATMOSPHERIC ELECTRICITY

(Lectures and Discussions of B. Vonnegut)

The laboratory investigation of problems associated with thunderstorm electrification largely has been left unexplored except for a few cases. There seems to be great potential for these kinds of experiments to yield new ideas about electricity in the atmosphere, but much exploratory work has yet to be done. The first lecture in this series deals with several different simple experiments which have suggested how charged particles interact with air motion. The second lecture centers attention on an example of a simple qualitative model of the interaction between the ionosphere and a cloud, and some unrelated studies of the hydrodynamics of thermal convection in a rotating system.

LABORATORY MODELING OF THE BEHAVIOR OF ELECTRIFIED CLOUD PARTICLES

The atmosphere frequently changes in a very complicated manner so that its behavior as a whole is very difficult if not impossible to explain. One traditional method for attacking the atmosphere is to break its phenomena into pieces and bring them into the laboratory where simple controlled experimentation can be undertaken to see how the fragments behave. This approach has had some success in problems of thunderstorm electrification.

As an example, consider the lightning spark: what is it, and what does it do in the cloud? If lightning discharges could be induced in the atmosphere in a controlled way, the electrical effects could be studied more reasonably. Some years ago, the Navy observed that plumes of spray from depth charge explosions under thunderstorm clouds could initiate lightning sparks in the cloud. It was then
assumed that one could initiate a spark by holding wires under clouds with a balloon, but this did not work. We found, however, in laboratory experiments that sparks could be triggered in the region of strong gradients in electrical potential induced by a van de Graaf generator by passing a wire near the generator rapidly rather than fixing a wire in the electric field. Evidently, to produce a spark, the trigger must be passed in the neighborhood of the "source" of lightning sufficiently rapidly to avoid formation of an electrical screening layer around the triggering device. Subsequently, we have tried shooting wires through clouds with rockets and projectiles from a mountain in New Mexico. These experiments have not been successful, but Dr. M. M. Newman of Lightning and Transients Research Laboratory has tried the same experiments over water and found that sparks from clouds could be induced much more readily. Apparently the nature of the topography has considerable influence on discharge in these electrical fields.

Another example of studies of lightning discharge in the laboratory has been carried out using sparks generated in a specially designed bank of capacitors. We used this equipment to investigate the effects of lightning in a supercooled cloud in a cold box. Just after a spark, we found that at \(-20^\circ C\) in the cold box, no ice crystals could be generated, while at \(-25^\circ C\), the air in the cold box could be filled with ice crystals. This result may be interpreted as the result of adiabatic cooling in the rarefaction wave behind the shock wave produced by the spark discharge. Unfortunately such simple explanations are complicated by the fact that all sparks in the laboratory give off very large numbers of aerosol particles which may act as ice nuclei.

One of the most intriguing results of important interactions between electrodynamic effects and aerodynamic effects may take place in tornadoes (see, for example, Vonnegut, 1960). At present, there is no way of explaining the way nature concentrates energy
in a region to maintain a tornado vortex. The intensification of energy in these rotating systems may result from heating of the core of the tornado by nearly continuous lightning discharge "down" through the center of the vortex. There are some cases of observations of luminous glow and lightning sparks in tornadoes. And we have been able to stabilize spark discharges in rotating cylinders in the laboratory.

Incidentally, the acoustic properties of tornado vortices may be studied in a laboratory model here in my pocket. This tube (or whistle) is constructed in such a way as to induce a vortex flow upon leaving the tube. The noise produced has a frequency proportional to the speed of the rotating air leaving the tube (Vonnegut, 1954). Tornadoes certainly make noise. And it is possible that their sound is related to their speed of rotation.

Controlled experiments of atmospheric processes often can be carried out in the field as well as in the laboratory. For example, we have studied some features of perturbations in atmospheric electrical potential associated with thunderstorms by simulating the behavior of a huge cloud with a little one (see Moore, et al., 1962; Vonnegut, et al., 1962). In connection with the total flow of current from the ionosphere to the earth through a thunderstorm, there are reasons to believe that positive charge from point discharge on the earth's surface is transported into clouds by updrafts under the cloud. Flow of charge is very difficult to measure near or in a large storm. However, flow of charge from the ground can be traced to some degree under fair weather conditions. Recently in Illinois, we simulated point discharge into the atmosphere from a line source consisting of a wire stretched for some distance on poles. We were able to measure the perturbation in atmospheric electrical field downwind from the sources of charge. Furthermore, it was found that measurable charge apparently was transported by air motion into fair weather cumulus clouds in the vicinity of the sources of charge while no
"random" charges could be detected in uncharged clouds farther away from the electrical sources on the ground.

The influences of electrical forces on charged particles easily may be investigated in the laboratory. We have examined certain features of the trajectories of small charged droplets under the influence of electrical fields. These experiments have demonstrated how electrical forces induced by electrical fields present in thunderstorms may completely overshadow the influences of gravity on (charged) particle motion.

Another interesting question that one may ask is: what happens to the electricity in an evaporating charged droplet? Our observations (Doyle, et al., 1964) of highly charged ~100 µ diameter droplets in a modified Millikan chamber have indicated that the charge remains essentially constant during evaporation until the droplet becomes unstable. Instability eventually results from the unbalance between surface forces and electrical forces associated with charge on the particle's surface. When particles become unstable, they apparently throw off small charged droplets to reduce the charge on the original droplet. Interestingly enough, these experiments also have suggested that the value of the dielectric strength of air near the particles may exceed by a factor of ten or more the value of 30 kv/cm normally given for air.

The experiments which have been described here illustrate the many possibilities for fruitful work in laboratory simulation and controlled experiments in the atmosphere on atmospheric electricity. This is an interesting and challenging field that has just begun to be explored. Our work is continuing in this area.
DISCUSSION (with H. F. Eden).

Atmospheric Electricity

It is difficult to pinpoint "the most important" problems or stumbling blocks that presently exist in atmospheric electricity. So little is known about electrification, that any number of areas can be attacked fruitfully in the laboratory, and in the field. An example of one particularly interesting, and relatively important problem is the question of the distribution of charge in a thunderstorm. In these storms particles range in size from molecular dimensions to several centimeters in diameter. We know very little about the details of the spatial and temporal distributions of charge in a cloud, and virtually no information is available about the distribution of charge with particle size. It has been thought that most of the charge in a cloud is concentrated on particles of precipitation size, but there appears to be no real experimental or theoretical foundation for this conclusion.

To measure the distribution charge in storms, one would ideally want a resolution to 1 cm$^3$ or below. This would be quite desirable to examine carefully the nature of the "dendritic" distribution of glow in a lightning discharge. However, for the present, even a resolution of 1 km$^3$ would be worth trying for.

In connection with measuring charge distribution, it should be mentioned that the commonly accepted view relates charge separation in thunderstorms to gravitational separation only. That is, rain and hail tend to fall to the bottom of a cloud. If these particles are charged most strongly by charges of one sign they can effectively separate charge from the top to the bottom of a "still" cloud by falling through the cloud. However, one also should take account of the relatively intense vertical motion in a cloud in distributing charged particles. These "local" updrafts or downdrafts can be many times the size of the terminal velocity of particles in a field of
Appendix A-20

... gravity. Perhaps the new generation of experts in cloud electrification will be the cloud dynamicists.

Given the choice at the present time, it appears that the development of sophisticated instruments and new field techniques can better answer our questions about thunderstorm electrification than alternative laboratory studies. At present, we do not know yet exactly what to simulate in this area.

In choosing between the laboratory and the atmosphere, one should bear in mind, of course, that many basic experiments related to atmospheric electrification ought to be done anyway in the next ten years. These studies should provide interesting, and "important" results particularly about the behavior of electrical discharges in dielectric media like a cloud, where relatively large particles are separated from each other by an effective insulator. Other problems should be looked at too. For example, how do particles get charged? And what happens to the charge on a particle? Lightning may play a very important role in the charge distribution in a cloud. There also are possibilities for laboratory simulation here.

Convective Experiments

The investigation of the development of thermals, and convection in rotating chambers has provided a great deal of qualitative information for the meteorologist. However, the most important experiments of interest to meteorology in these areas appear to have been carried out 5 to 10 years ago. More recently efforts to further explore these convective models have led to contributions to the general knowledge of fluid dynamics, but have provided relatively little new information for geophysical applications. Nevertheless, there are some fruitful areas to be examined as noted earlier by J. S. Turner.
For continuing fluid mechanical studies in rotating systems, it would be desirable to provide a good (200-channel, say) data acquisition system for "general" use (see also A-5, H. F. Eden).

There is an area of study which has remained largely untouched for modeling. This field might be called electro-hydrodynamics, or electrostatic convection. Hydrodynamic experiments in intense electrical fields may provide some bizarre but interesting exploration of fluid flow under an additional degree of freedom.

A Simulation Facility

Given infinite financial resources, it is still very difficult to conceive of a class AAA simulation facility for cloud physics. This is like extrapolating from zero upwards. Cloud physics generally is in a rather primitive scientific state. Therefore, it does not seem useful to consider at this time a huge, multimillion dollar facility in the "cyclotron" class. Perhaps a better way of approaching this problem is to let individual scientists clearly define the needs for the ultimate facility through a chain of good research in smaller chambers and equipment, gradually building up experience and knowledge in simulation techniques. Then, if these scientists find that their experiments cannot be continued without use of a large facility, the structure can be designed and built to fit well defined research requirements. There is a very great danger in building any facility without a real need. To try to construct a compromise which will fulfill the requirements of an infinite number of experiments is very difficult. It is too easy to build a complete "dud." A simulation chamber or laboratory should come from individuals vitally interested in laboratory simulation. "You do not want an orphan on your hands; someone needs to love, and to watch over a facility" to make it useful and productive.

In building a central facility or group of facilities, one also must consider very carefully its useful lifetime. Frequently, only
one or two experiments can be undertaken in even an expensive piece of equipment. Then it falls upon the owner to justify the facility's existence by conjuring up new problems to be studied in the "monster." It should also be noted, of course, that the existence of a good piece of equipment often generates new enthusiasm for a class of problems, and easily can lead to many new studies over many years.

We have discussed previously the alternate possibility of setting up a facility consisting of many instruments, and some basic equipment like refrigerators, combined with a limited technical staff such as an electronics engineer. This route might solve the specific facility question. This kind of general purpose facility might also consist of a horizontal wind tunnel, and a vertical wind tunnel, too. There are certainly a wide variety of cloud physics experiments that can be done in a vertical wind tunnel. And this kind of unit should be useful to many workers. However, the point still should be emphasized that the origin of such a facility should come from an interested scientist. We may make an analogy to a camera. When you buy a camera, you have something very specific in mind to use it for. You can not design everything you might want into one camera.

In spite of the emphasis on a specific problem, we know that the usage of a vertical wind tunnel, say, represents a very wide field of activity, and we may need a centralized unit which could give us a range of pressure, temperature, droplet concentration, etc. Is there a group of gadgets such as a van de Graaf generator which is useful whatever the camera is? Perhaps the answer to this is: one or more people who are experienced in simulation and modeling should start a program, and once they have gotten all that can be done out of small, relatively simple equipment, then these workers should direct the design and construction of (and buy equipment for) a larger facility.

If it is desirable to begin systematic studies of simulation using certain members of the NCAR, should we attempt to organize such
a facility or laboratory around a newly recruited staff? It does not seem desirable to hire new research staff on a Ph.D. level (or lower) by telling them what research projects must be done. It is dangerous to assign tasks to new people. Of course, certain controls can be exercised easily by hiring only people whose research interests fit a predetermined pattern established for the specific goals of the laboratory.

Although some servicing would be required of large sophisticated equipment, it definitely would not be desirable to operate a simulation laboratory only as a service facility for outsiders. The careful direction and motivation lies only in technical people closely and permanently tied to specific problems. The incentive to be watchful cannot lie with technicians, however competent, who are assigned a task of "routine" programmed measurements.

There may be a place for a kind of mechanistic research involving routine sequences of measurement (like measuring ad infinitum the drag on various shapes of bodies). However, there is a very real question whether this type of work can properly be called research.

Regarding the question of the usefulness of a central facility of training and education of people in modeling, this approach seems to be a kind of backwards thinking. Ability to train or the usefulness as aids to teaching alone should not be used to justify a multimillion dollar laboratory for atmospheric simulation.

When Dr. Vonnegut was asked whether or not he would use a facility if it existed, he answered, "Maybe, but do not design the equipment on my account."

After this discussion, we again reached the conclusion that there are many different simulation experiments that may be undertaken in a laboratory. However, the driving force for a large and sophisticated laboratory should come from the crying needs of individual scientists, and not from a recognized "need" for facilities in principle.
If NCAR wants to explore simulation in detail, a program should be organized and a chain of research may lead through small equipment to a large facility. If scientists feel after sufficient exploration of a field, the large facility is needed and justified for progress, then it should be built without question of utilization time.

This conclusion directly parallels the results of our discussions with Roland List last summer.
REFERENCES


Appendix A


References


Appendix A


References

____, 1964: "The viscous boundary layer at the free surface of a rotating baroclinic fluid," Tellus 16, 523-529.

____, 1965: "The viscous boundary layer at the free surface of a rotating baroclinic fluid: Effects due to the temperature dependence of surface tension," Tellus 17, 440-442.


Appendix A


Appendix A


Steele, R., 1966: "Characteristics of AgI ice nuclei origination from anhydrous NH₃-AgI complexes," Colorado State University, Atmospheric Science Technical Paper No. 73, Fort Collins.


References


_____, 1965b: "Laboratory models of evaporation and condensation," Weather 20, 124-128.


Appendix A. ..................................................

Appendix B

DISCUSSION OF A TEST CHAMBER FOR AIRCRAFT INSTRUMENTATION
DISCUSSION OF A TEST CHAMBER FOR AIRCRAFT INSTRUMENTATION*

To observe atmospheric properties, including exchange fluxes of heat, mass, and momentum at heights greater than about 100 m, workers will remain committed at least in part to aircraft for some time in the future. Therefore, it is vital that the best possible techniques become available in the near future to develop and calibrate aircraft instrumentation for meteorological research.

The development of instrumentation for measuring properties of cloudy air from aircraft up to this time has been paradoxically slow and chaotic. The determination of even the most basic properties like temperature and vertical velocity from an aircraft presents a much greater problem than corresponding determinations from a fixed station because of the large horizontal velocity of the aircraft. The majority of techniques for the measurement of size spectra of cloud droplets and liquid water content depend on the inertial impaction of particles, utilizing sampling by the aircraft traveling through relatively quiet air. The high speed of the aircraft essentially presents the crucial problem to developing suitable instrumentation. One cannot be sure that devices designed for use at a stationary position will provide useful information when used on a platform traveling at high speeds.

Experimental evidence from aircraft sampling has suggested that there are large differences between actual instrument performance and expectations based on reasonably sound theoretical predictions. It is vital to future observational programs using aircraft to provide for data acquisition to make standards of less than + 0.1°C in temperature, for example. Other problems have to be resolved which include better knowledge of droplet sampling under non-isokinetic conditions.

* This discussion is based on the ideas and proposals of P. Squires and J. Telford.
Furthermore, investigators have to know quantitative answers to such questions as: (a) what happens to a cloud droplet impacting at high speeds of 200 knots on a rapidly drying formvar surface, or an oil layer; (b) what is the collection efficiency of impacting devices using such exposed surfaces; (c) what does a wet bulb thermometer measure as it travels rapidly through cloudy air; and, (d) what are the comparative limitations of various kinds of impacting devices, thermometers and velocity sensors to be used for measurements at aircraft speeds?

Faced with such questions, workers often have resorted to testing of instruments in wind tunnels. This is a rather poor way to resolve problems associated with aircraft sampling since the experimenter is really in no better position to measure properties of cloudy air moving at aircraft speeds through a tunnel than he is in the field. The presence of microscale turbulence in the wind tunnel provides further complications to such a method of calibration. Other factors like the problem of residual oscillation of suspended drops and of the long path needed to bring drops close to the velocity and temperature of the gas in the tunnel limit the usefulness of wind tunnel testing.

For some purposes, a "whirling arm" in more or less stationary air has proved to be of some utility, but these kinds of devices are severely limited by the high centripetal acceleration that must exist during the measurements (e.g., about 100 g at 200 knots for a 10-m arm).

A realistic solution to all of these problems simply is to reproduce the situation which occurs when an instrument is used on an aircraft to sample a cloud; that is to create a cloud in which the drops are falling at terminal velocity as in a real cloud, and then move a sampling device through this cloud at aircraft speed. The cloud in this case can be examined and controlled by well known laboratory techniques. The generation and maintenance of the cloud are not complicated by the need to inject it into a rapidly moving...
stream of air. The price to be paid for these gains is the need to accelerate the sampling instrument smoothly enough to aircraft speed and then decelerate it to rest after passing through a test zone of cloudy air. During the steady velocity part of the instrument's trajectory, it must pass through a cloud chamber in which the properties of interest can be measured repeatedly both before and after the passage of the instrument. With permissible accelerations of approximately 10 g, and a required speed of 200 knots, a path length of about 100 m would be needed to provide for both acceleration and deceleration.

The project of building a test chamber lends itself to being developed in a stepwise fashion. For the calibration of some devices for measuring droplet spectra, for example, it is sufficient to have a cloud chamber a few centimeters long, filled with a monodisperse cloud of drops produced by a single spinning disk, and a "sampling instrument" with a mass of the order of one gram, propelled by some simple device such as a bow or a rocket sled. On the other hand, for the calibration of cloud thermometers, or liquid water content devices, which may have time constants of the order of 0.1 sec, it would be necessary to have a reasonably uniform cloud along a path corresponding to perhaps half a second's travel; i.e., some 150 ft at 200 knots. The problem of controlling the temperature could perhaps be mitigated by arranging to have the cloud chamber at a mid-point along the path of the instrument, and stabilizing the air by means of a temperature inversion.

One would at present envisage as a final objective an instrument carriage capable of carrying a payload of perhaps 100 lb at speeds up to about 200 knots, with a cloud chamber about 200 ft long, and hopefully with control of pressure from 300 to 1000 mb as well as temperature -30 to 30°C.

In a rather early stage of the development it would be possible
to use this facility for accurate determination of air speed corrections for dry bulb thermometers, without a cloud present at all. *

For purposes of discussion an idealized test facility is presented, which should meet the requirements stated above, and should provide relatively comfortable working conditions for the operators. In Fig. 1, the facility is sketched schematically. The chamber would consist of two parallel tunnels about 1200 ft long including 200 ft for the environmental test chamber. The two parallel tunnels would be necessary to minimize the noise in the control and test section on passage of the carriage sled. The noise of such a device would be equivalent to an express subway passing an observer in a New York station. The instrument carriage would consist of a horizontal arm sticking side-wise from the rocket sled through a narrow slot into the test tunnel. Construction of the rocket sled, with its equipment for good thermal insulation, should be underground. Both tunnels would have to be held under vacuum conditions.

In the cloud chamber, temperature and pressure should be monitored at 5-ft intervals. Cloud droplets could be introduced continuously by a series of spray devices on the ceiling of the chamber. The size spectra of droplets and the liquid water content of the cloud should be monitored at intervals of every 10 ft or less.

For meeting the requirements of instrument calibration, the chamber temperature should be controlled to $\pm 0.05^\circ$C, the pressure to $\pm 5$ mb, the droplet spectra should be measurable and reproducible to a fraction error $\pm 20\%$. The liquid water content should be controlled to $\pm 5\%$. And the speed of the carriage should range from 120 to 250 knots, with control to about 20 cm/sec.

* This is so serious a problem that it has already prompted serious consideration of "rolling" a DC-3 at 120 knots along an airstrip past calibrated stationary thermometers!
Fig. 1 Schematic drawing of proposed test chamber for aircraft instrumentation.
The instrumentation of the test chamber, carriage and peripheral equipment would include oscillograph recorders, tape recorders, and possibly capabilities for direct conversion of analog signals to digital form.

Based on a five-year development and operation program, the following preliminary cost estimate would be realistic for the facility discussed above:

<table>
<thead>
<tr>
<th>Item</th>
<th>$ Cost</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Structure (Design and Construction)</td>
<td>500,000</td>
</tr>
<tr>
<td>2. Refrigeration and Vacuum</td>
<td>500,000</td>
</tr>
<tr>
<td>3. Spray System for Cloud Generation</td>
<td>20,000</td>
</tr>
<tr>
<td>4. Carriage Sled and Acceleration-Deceleration Equipment</td>
<td>500,000</td>
</tr>
<tr>
<td>5. Control System</td>
<td>100,000</td>
</tr>
<tr>
<td>6. Instrumentation</td>
<td></td>
</tr>
<tr>
<td>a. Temperature (40 x $500)</td>
<td>20,000</td>
</tr>
<tr>
<td>b. Static Pressure (40 x $500)</td>
<td>20,000</td>
</tr>
<tr>
<td>c. Velocity of Carriage</td>
<td>2,000</td>
</tr>
<tr>
<td>d. Droplet Spectra (based on 20 Royco type optical analyzers)</td>
<td>200,000</td>
</tr>
<tr>
<td>e. Humidity (40 x $2000)</td>
<td>80,000</td>
</tr>
<tr>
<td>f. Data Processing and Recording</td>
<td>100,000</td>
</tr>
<tr>
<td>Total Design and Construction</td>
<td>$2,042,000</td>
</tr>
<tr>
<td>7. Staff</td>
<td></td>
</tr>
<tr>
<td>a. One Scientist (5 yr x $14,000)</td>
<td>70,000</td>
</tr>
<tr>
<td>b. Two Engineers (5 yr x $10,000)</td>
<td>50,000</td>
</tr>
<tr>
<td>c. Three Technicians (5 yr x $8000)</td>
<td>40,000</td>
</tr>
<tr>
<td>d. Secretarial and Miscellaneous Technological (5 yr x $4000)</td>
<td>20,000</td>
</tr>
<tr>
<td>Total Staff</td>
<td>180,000</td>
</tr>
<tr>
<td>8. Total Construction and Operation</td>
<td>2,222,000</td>
</tr>
<tr>
<td>9. Operating Overhead, 15% of Item 8</td>
<td>333,000</td>
</tr>
<tr>
<td>10. GRAND TOTAL</td>
<td>$2,555,000</td>
</tr>
</tbody>
</table>
BIBLIOGRAPHY

During the course of this survey, I found a number of references to certain phases of simulation. The bibliography that follows is certainly incomplete. However, in combination with references in the main body of this report, and in Appendix A, the reader may find this list a useful introduction to the wealth of literature dealing with simulation of atmospheric processes. References are listed below alphabetically under five major topic headings: Environmental Chambers, Cloud Chamber Experiments, Fluid Dynamics Analogues, Laboratory and Modeling Experiments (General), and Wind Tunnel Studies.

ENVIRONMENTAL CHAMBERS


Appendix C ........................................


CLOUD CHAMBER EXPERIMENTS

Cloud Chamber Experiments


Appendix C


Cloud Chamber Experiments


Appendix C


Cloud Chamber Experiments


Riezler, W. and R. Werz, 1963: "Über eine einfache kontinuerliche nebelkammer," (On a simple continuous cloud chamber) *Naturwissenschaften* 40, 139-139.


Appendix C

Seroplay, R., 1955: "Une chambre à nuage pour étudier l'activité
glacogène des aérosols," (Cloud chamber for the study of ice forming
activity of aerosols) Bull. de L'Observatoire du Puy de Dôme, No. 4
97-107.

_____, 1956: "Une technique pour l'analyse des brouillards aqueux ou
glacés," (A technique for analyzing aqueous fogs and ice fogs)

Shimada, T., K. Sekihara and K. Kawamura, 1955: "An effect of ultra-
 violet radiation on the action of silver iodide particles as sub-
limation nuclei," J. Meteorol. Soc. of Japan, Tokyo, Ser. 2, 33,
276-279.

Soulage, Guy, 1953: "Une cuve frigorifique pour l'étude des cristaux
de glace induits artificiellement dans un nuage surfondu," (Cold
chamber for the study of ice crystals artificially induced in a
supercooled cloud) Bull. de L'Observatoire du Puy de Dôme, Ser. 2,
No. 4, 81-90.

Vonnegut, B., 1949: "Nucleation of supercooled water clouds by silver

Wirth, Endre, 1960: "A természetes jegmagvak koncentrációjára és
eredetére vonatkozó viszgálatok," (Research on the concentration and
origin of natural ice nuclei) Irodjárs; Budapest, 64, 233-235.

Zhigalovskaia, T. N. and V. N. Balabanova, 1960: "Izuchenie raaseiania
dyma iodistogo serebra v zamknutom sosude," (Study of the dissipation
of AgI smoke in a cloud vessel) Izvestiya Akademiia Nauk SSSR,
Seriya Geofizicheskaya, No. 6, 903-905.

FLUID DYNAMICS ANALOGUES

observed instability of a laminar Ekman flow in a rotating basin,"
Tellus 13, 31-39.

83-103.
Fluid Dynamics Analogues


Chicago University, Department of Meteorology, 1951: "Research on experimental hydrodynamics in relation to large-scale meteorological phenomena," Contract Report, Nos. 6 and 7, 8 pp.


Appendix C


Fluid Dynamics Analogues


Long, R. R., 1951a: "Research on experimental hydrodynamics in relation to large scale meteorological phenomena," University of Chicago, Department of Meteorology, Progress Report No. 5.


Appendix C


Fluid Dynamics Analogues


LABORATORY AND MODELING EXPERIMENTS


Brun, Edmond, 1948: "Considerations sur la réalisation d'une soufflerie de givrage artificial," (Considerations on the construction of a wind tunnel for producing artificial rime) J. des Recherches, Centre National de la Recherche Scientifique, France, No. 4-5, 1-4.


Appendix C


Wind Tunnel Studies


Howe, J. W., 1952: "Wind pressure on elementary building forms evaluated by model tests," Civil Engineering 22, 42-46.


Appendix C


Laktionov, A. F., 1961: "Pervaia mexhduvedonestvennaia konferentsiia po voprosam modelirovania iavlenii v atmosfere i gidrosfere," (The first inter departmental conference on modeling of phenomena in the atmosphere and hydrosphere), Okeanologiya, Moscow 1, 924-926.


Wind Tunnel Studies


Appendix C


Rouse, H., 19--: "Air tunnel studies of diffusion in urban area," Meteorological Monographs 1, 39-41.


Sibu1, O., 1954: "Laboratory study of wind waves in shallow water," Wave Research Laboratory, University of California, Berkeley, 13 pp.

Wind Tunnel Studies

"Special cases of simulating the energy budget of the earth/air interface," USAEPG Micrometeorological Research Notes No. 1, Army Electronic Proving Ground, Fort Huachuca, Arizona, 162 pp.


Stroescu, M., 1948: "Realisation d'une maquette de soufflerie de givrage aux laboratoires de Bellevue," (Model of a wind tunnel producing mine constructed at the Bellevue Laboratories) J. des Recherches, Centre National de la Recherche Scientifique, France, No. 4-5, 5-13.


Sweeney, M. W., Jr., 1955: "Supersonic wind-tunnel measurements of surface pressures on a small rocket to be used for upper air research," Wind Tunnel Report No. 80, Contract AF 19(604)-1208, Massachusetts Institute of Technology, Naval Supersonic Laboratory, 74 pp.

Appendix C


