Argonne National Laboratory
P. O. Box 299
Lemont, Illinois

**REPORT ON MEETING OF AD HOC COMMITTEE ON SCALING
MESOMETEOROLOGICAL PHENOMENA**

Held at Argonne National Laboratory
May 15-17, 1957

Edited by

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I. GENERAL ORGANIZATION OF MEETING

In considering future requirements arising from expansion of both atomic energy and other types of industry, it has become evident that the need for information on mesometeorological processes, especially those related to atmospheric dispersion, would, in the next few years, become so demanding that short cuts or auxiliary tools would be most welcome. In this connection the use of modeling techniques offers an attractive possibility. As a first step in an effort to judge the potentialities of modeling, it was decided to assemble a group of individuals who either have had wide experience with problems of modeling meteorological phenomena or have made outstanding contributions in the field of micrometeorology, and to allow them to hold broad general discussion on the mesometeorological problem.

It came to the attention of personnel of both the Community Air Pollution Program of the Robert A. Taft Sanitary Engineering Center and the Argonne National Laboratory that there were thoughts along the same lines and plans to hold similar meetings. It was therefore decided to sponsor jointly a single meeting which would serve the purposes of both organizations.

The number appointed to the committee was small in order to allow maximum freedom of discussion. For this reason, it was impossible to invite many individuals who have done outstanding work in the fields bearing on the discussions.

The meetings were held at the Argonne National Laboratory, Lemont, Illinois, on May 15-17, 1957. There were four formal half-day sessions beginning on the afternoon of May 15th and ending at noon on May 17th.

Social functions were arranged for the early part of the evenings of May 15th and 16th. These were followed by informal discussions which lasted past midnight. The schedule followed by the participants was quite strenuous.

Chairmen, rapporteurs, and themes were selected for each of the sessions. These were:

- I. APPRAISAL OF EXPERIENCE - May 15th, Afternoon
Chairman, Prof. Horace R. Byers
Rapporteur, Prof. Robert R. Long
- II. SCALING: QUANTITATIVE AND ANALYTICAL - May 16th, Morning
Chairman, Dr. Charles H. B. Priestley
Rapporteur, Dr. Franklin A. Gifford, Jr.
- III. SCALING: PRACTICE AND METHODOLOGY - May 16th, Afternoon
Chairman, Prof. Hunter Rouse
Rapporteur, Prof. Maurice L. Albertson
- IV. NEEDS AND EXPECTATIONS - May 17th, Morning
Chairman, Prof. E. Wendell Hewson
Rapporteur, Mr. Joshua Z. Holland

Each rapporteur was requested to submit a summary of about a thousand words covering the salient points of his session. After the summaries were received, they were reproduced and sent to all of the participants for comment. These were incorporated into second drafts and resubmitted to the rapporteurs for final approval. This final version of the rapporteurs' summaries is found in Part II.

The cooperation and aid of many individuals were instrumental in achieving the objectives of the meeting. Thanks are due to Dr. Norman Hilberry and Dr. John E. Rose of the Argonne National Laboratory, to Mr. Arthur C. Stern of the Robert A. Taft Sanitary Engineering Center, and Prof. Garrett Birkhoff of the Harvard University for encouragement and early suggestions concerning the desirability of holding a meeting of this type. Help in planning the meeting was rendered by Prof. Gordon H. Strom of New York University; Prof. Franklin I. Badgley of the University of Washington; Prof. Horace R. Byers and Prof. Dave Fultz of the University of Chicago; and Mr. Paul Frenzen and Mr. Peter E. Wasko of the Argonne National Laboratory. The cooperation given by Argonne administrative and service personnel went far toward making the meeting successful.

A. List of Participants

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II. RAPPORTEURS' SUMMARIES

Session I - APPRAISAL OF EXPERIENCE

by
Robert R. Long

The conference opened with a session devoted to an "Appraisal of Experience." Remarks by Dr. Norman Hilberry, Director of Argonne National Laboratory, in a welcoming address to the gathering, emphasized the basic importance of meteorology to activities of the Atomic Energy Commission, in particular the necessity for safeguarding residents within a wide area against the dangers of explosions and leaks of atomic wastes. This importance fully justifies modeling or any other promising approach to the meteorological problems.

Professor Horace R. Byers, as Chairman of this session, asked the representatives of the sponsors, Mr. R. C. Wanta and Mr. Harry Moses, to outline the broad nature of the problem to be discussed. Wanta conveyed the greetings of Mr. Arthur C. Stern, Chief of the Community Air Pollution Program of the Public Health Service, and expressed the wish that it were practicable to assemble in Cincinnati at the Robert A. Taft Sanitary Engineering Center for part of the conference. Wanta and Moses expressed the hope that much of the discussion would be devoted to the many questions related to modeling of meso-scale atmospheric phenomena. They suggested that the more experienced participants might survey the specific nature of the modeling technique, including personal, informal opinions about its future as a meteorological tool. The possibility of using this approach on problems of importance to the sponsors suggests also the practical question of facilities and staff required for a modeling laboratory.

The informal discussions began with attempts to gain a common understanding of the term "meso-scale." Complete agreement was not achieved, but it seemed the consensus that an upper limit of 100 miles or so was a reasonable one. The lower limit was less definite, but it was felt that it should be of the order of the size of a building. Various persons mentioned particular phenomena that could be included: tornadoes, dust devils, flow over hills and mountains, diffusion of smoke and other contaminants over distances of miles, cumulus scale convection, thunderstorms, etc.

The first specific modeling topic was the impossibility of obtaining equality of the Reynolds number in atmospheric models and prototypes. This difficulty has long been understood, and meteorologists who conduct modeling research are constantly aware of it. There are at least two viewpoints that can be adopted: first, that friction, both molecular and turbulent, is negligible in both model and prototype; second, that turbulent friction acts in the same manner in the atmosphere as molecular or turbulent friction in the model. The first viewpoint is reasonable in some instances, in particular in those cases where the direct effect of friction can be detected and noted. Prof. Robert R. Long cited an example: there is evidence that in airflow over mountains, boundary layer separation may occur and may be the cause of the common "rotor-cloud" type of flow patterns. If friction is simply ignored, no effort would be made to model this boundary layer separation phenomenon, and attention would be confined to modeling the gravity wave aspects. If it is supposed, on the other hand, that we can adopt the second viewpoint, we would attempt to obtain the same ratio of inertial and viscous forces and hope thereby to model the "rotor-cloud."

The modeling of phenomena such as heat convection and diffusion of contaminants, which depend essentially on the nature of eddy diffusion of momentum, heat, etc., may be possible if these have a Fickian character. Such is not the case, however, near the ground, as pointed out by Dr. C. H. B. Priestley. Priestley believes, however, that this

may approximate the state of affairs higher up in the atmosphere. For example, Bénard-cell type of convection could conceivably occur at levels of alto-cumulus clouds but perhaps not in connection with low-level cumulus. Indeed, Priestley's measurements show no trace of the organized type of convection resembling Bénard cells in the lowest 100 meters.

The remarks concerning thermal phenomena introduced a discussion of atmospheric convection on the scale of cumulus clouds. Mr. Paul Frenzen, for example, reported on his observations of the soaring of hawks on what he would regard as steady "thermals" of an organized nature. The general controversy centered on the actual nature of atmospheric convection, whether it is of the bubble type as believed by Scorer, or of a cellular nature.

The Chairman called on Prof. Hunter Rouse to contribute to the general discussion on experience with modeling. Rouse presented a motion picture of some wartime experiments connected with diffusion of contaminants. Smoke was released in a moving stream of air which was occasionally heated or cooled to produce instability or stability. Turbulence was caused by a grid, artificial roughness, and heating, and the spread of the plume was noticed, in some cases as influenced by passage over bluff barriers. With such barriers, one might expect to model turbulence due to boundary layer separation, since separation on angular bodies does not depend on the value of the Reynolds number.

Rouse also discussed briefly more recent work of his group, in particular the effect of mixing on the thermocline in the oceans, stratification in fluids due to the presence of foreign materials and temperature differences.

The rest of the session was devoted to a discussion of diffusion and its relationship to convection caused by heat inequalities. It was generally agreed that heating effects were among the most difficult to model.

Session II - SCALING: QUANTITATIVE AND ANALYTICAL

by

Franklin A. Gifford Jr.

Dr. C. H. B. Priestley first introduced Dr. Donald A. Flanders, Director of the Argonne National Laboratory Applied Mathematics Division, to the group and then briefly reviewed the previous afternoon's topic. In his review, Priestley touched very generally on the acknowledged difficulty of scaling the complex orographic and convective phenomena that are characteristic of the mesometeorological scale of events, and called upon Prof. E. Wendell Hewson to start the discussion.

Hewson referred briefly to his experience in the Columbia River Valley, where a detailed observational study of the valley wind and temperature structure, and analysis of the pattern of diffusion of SO_2 from the Trail Smelter, were carried on. Because of the complex nature of air flow patterns that are under strong orographic control, Hewson suggested that it would be desirable to attempt at first a still air model, i.e., a closed room rather than a wind tunnel. Terrain features such, for example, as a wide canyon or valley, could be modeled; and study of the development of flow patterns in these from air initially at rest would form a logical starting point for the modeling approach.

In the discussion on this point, various advantages and disadvantages were mentioned. With such a model, one could start with simple, idealized terrain and atmospheric configurations, adding complications gradually and approaching the complexity of a real atmospheric situation by stages. Prof. Hunter Rouse, referring to the practice in hydraulics, emphasized the ability of a model to isolate a particular phenomenon and to keep out random, or unwanted, influences, thus increasing the capacity to study significant effects separately. Density stratification, ground roughness, air turbulence, and convective heating (as examples) could be introduced and varied separately or perhaps jointly in a suitable model.

Professor Robert R. Long referred to the difficulty of modeling complex terrain and convective heating effects. It appeared to be generally agreed by those experienced in modeling techniques that the effect of heat convection is the most difficult problem to be expected in dealing with the mesometeorological scale. Priestley again suggested, apropos of this difficulty, that perhaps a model of atmospheric convection might be sought in the atmosphere itself, at a fairly small scale. He pointed out that detailed study of convective atmospheric motions on a scale, say, within tens of feet of the surface, is comparatively easy to do. Such study could be expected to provide at least some insight into the larger convective motions characteristic of the meso-scale. Although (as was emphasized in the previous afternoon's discussion) strict similarity of the small-scale convection with convection on the cumulus cloud scale cannot be expected, useful modeling might be obtained. In this connection the distinction between quantitative and qualitative modeling was drawn. A qualitative model, i.e., one in which strict similarity of significant characteristic parameters is at least partially absent, can still be of considerable use in illustrating aspects of a phenomenon, a case in point being the previous day's motion pictures of model smoke plumes. Mr. Joshua Z. Holland observed that, though intended as an instructional aid only, their similarity to real atmospheric smoke patterns at Oak Ridge was striking.

In developing the discussion on these various points, Hewson emphasized that a model may permit one to see fairly clearly what is very difficult to observe in the real atmosphere. In studying only the atmosphere, there is always the risk of completely missing some important phenomenon because of the sheer difficulty of the observing job.

He cited as an example the fact that, despite a vigorous and comprehensive observational program at Trail, not all details of the air flows in the valley could be observed satisfactorily, due to purely practical limitations on personnel and equipment. It would appear to be more efficient to study models first and to use the results as a guide of the real atmosphere.

In view of the expected difficulties involved in modeling complex terrain and heating effects, Holland suggested, as a possible alternative, that it might be just as efficient and perhaps more educational to look, for example, for a particularly simple valley that could be instrumented completely, in order to obtain very detailed, quantitative mesometeorological data. He pointed out that at Oak Ridge it was possible to get only an averaged, probabilistic description of the mesometeorological situation, because of the complexity of the hilly terrain there. But if ideally simple terrain features were sought out and completely instrumented, perhaps a body of experience based on real atmospheric prototypes could be assembled and then extrapolated to more complex situations. Rouse advocated running models and field installations simultaneously until confidence in mesometeorological model techniques is well developed. Holland suggested the possibility of a direct combination of the modeling and atmospheric prototype approaches in connection with the problem of handling insolation in a model. He pointed out the possibility of using a model chamber that is screened at the sides but open on top to the sky radiation. Such an arrangement would permit controlled modeling of terrain features together with heating and cooling by natural conditions.

A general discussion of dimensionless hydrodynamical numbers and their relation to the scaling problem concluded the first half of the session.

The second half of this session was largely devoted to a description by Long of some of his analytical and experimental modeling work. Although it was emphasized that the particular problem he has worked on is not directly associated with atmospheric diffusion or with the mesometeorological phenomena previously discussed, it was agreed a description of the general approach he used would be of value as an example.

Long has been doing experimental and theoretical work on simple types of stratified fluid flows for about 5 years. In the latest phases of this work, he has modeled the air flow over the Owens Valley, east of the Sierra Nevada Mountains in the vicinity of Bishop, California, a region for which the real atmospheric flow has been studied intensively.

Geometric similarity of the boundary form was the first modeling problem. The model barrier was about 1 meter long and about 4 cm in height, having the cross-sectional form of the Sierra Nevada ridge near Bishop. The flow was assumed to be two dimensional. This barrier was towed through a channel 20 feet long, 6 inches wide, and 2 feet high to produce a model of the flow. The working fluid was a salt water mixture.

A second modeling problem is friction. It is generally impossible to model Reynolds numbers since these differ by six orders of magnitude between model and prototype. The Navier-Stokes equations describe the model motions, whereas the atmospheric motions are described by Reynolds' equations of the mean-mean motion. One possibility is to neglect all friction in both systems; another is to assume that the Reynolds stresses have the same form in the atmosphere as do the molecular friction terms in the model. Having both experiments and theory, it was possible to pick out what effect friction was actually having in the model, and it was concluded that friction does not have a controlling effect on the significant model motion and can be neglected.

This is very likely also true for the prototype atmosphere, because the ratio of inertia terms to Reynolds stresses is about the same order of magnitude as the experimental Reynolds number.

If, then, modeling on the basis of perfect fluid flow is acceptable, the steady-state two-dimensional equations for both systems may be written, neglecting the earth's rotation and making certain simplifying assumptions, chiefly that of incompressibility. With respect to the steady-state assumption, two types of unsteadiness are observed in the model experiments: upstream propagation of waves, and formation of large turbulent lee eddies. The first does not significantly affect conditions near the obstacle. The second results in considerable energy dissipation in the vicinity of the eddies without, however, altering the general pattern of the perfect fluid flow very much. It is then shown that modeling can be produced if equality of a certain function of the quantity $F^2 = \bar{U}^2/gL$, i.e., of Froude's number, where $\bar{U}$ and L are representative velocity and length, is obtained. A difficulty is that in the atmosphere the relative depth of the troposphere, which is the region of interest, is very small. Thus either (a) the troposphere only is modeled, assuming the free fluid surface corresponds to the tropopause, or (b) the lower 10 to 20 km of the stratosphere is also modeled, which is more difficult. The actual similarity criterion used in the modeling is called by Long a modified Froude number, F_i , defined as $F_i = \bar{U} / \left(\frac{gh\Delta\rho}{\rho} \right)^{1/2}$, where h is the depth of the troposphere.

In the actual experiments, the obstacle was moved along the channel bottom at a known speed, and photographs were made of the motion as traced by reflection from suspended aluminum particles. At high flow velocities a single wave crest was observed over the valley, and its location moved downstream as speed was increased. This is the case of Froude number nearest that of the atmospheric prototype, and there is a strong resemblance. If F_i is reduced ($0.09 < F_i < 0.17$) two wave crests appear, and for $0.05 < F_i < 0.09$ there are three. For lower values, the flow is confused. A qualitative model of the stratosphere was introduced by putting several lighter fluid layers on top of the salt water mixture for several of the experiments.

It seems that the relevance to the committee's problem of Long's work lies particularly in his demonstration that a physical model and a mathematical model can be so investigated jointly as to provide a greater insight into the real atmospheric problem than could have been obtained from either separately.

Session III - SCALING: PRACTICE AND METHODOLOGY

by
Maurice L. Albertson

Prof. Hunter Rouse opened the meeting with the comments that the emphasis for the afternoon might be placed upon the mechanical phases of modeling - stressing what has not been done, but needs to be done, in this field. He urged that each person comment without any inhibitions and that the various items be threshed out at the time, in somewhat a "brainstorming" manner.

As an initial comment to begin discussion, Rouse complimented Prof. Robert R. Long on the elegant way in which he conducted his Sierra Wave model study in contrast to the way in which a hydraulician would have done it.

A. Effects of Viscosity and Turbulence

Throughout the conference, considerable emphasis was placed on the marked difference between the very wide spectrum of turbulence in the atmosphere as compared with the relatively narrow spectrum of turbulence in the usual research laboratory. In connection with this general discussion, Prof. Dave Fultz presented the idea that it might be possible to use the basic idea of creating the Reynolds number from the Reynolds stresses and resulting eddy viscosity in the atmosphere for the prototype, and from the molecular viscosity for the model. Rouse (as a purist) voiced his strong opposition to the basic idea, but hastened to say that he hoped thought in this direction would be continued so that if (by pure luck) such an artificial tool could be developed, then the idea would not have been lost. Even if this direction of thinking did not develop just as originally expected, there is also the possibility of such thought stimulating a new line of attack which would produce an answer.

One important consideration in connection with this idea is that in each case the problem is one of momentum transfer. In the small model, the momentum transfer is principally by molecular diffusion as reflected in the molecular viscosity which is homogeneous but varies with temperature, while in the atmosphere, the momentum transfer is by molar diffusion as reflected in the molar or eddy viscosity which is non-homogeneous (at least near an interface or a boundary) and varies considerably less with temperature. If this momentum transfer can be modeled by the selection of a particular viscosity of fluid in the model (by using various fluids such as glycerine), then it probably will be applicable only for a very limited range of conditions - which, nevertheless, might well serve the purpose desired.

Rouse stated that rather gross approximations sometimes do not introduce appreciable errors, despite the fact they are not fundamentally sound to a purist. He cited the example of the development of the equation for transport of suspended sediment in which, to obtain a simple solution, it was necessary to assume that the eddy viscosity was homogeneous. Satisfactory results were obtained.

It might be well to point out that Fultz made his suggestion regarding Reynolds number as a result of observations in the dishpan experiments which pertain to phenomena which are much larger than mesometeorological in scale, and Rouse based his comments on the fundamental aspects of fluid mechanics and wind tunnel experiments where the broad spectrum of turbulence is not ordinarily encountered, since the usual wind tunnel technique is to eliminate as nearly as possible all of the ambient turbulence.

B. Influence of Rotation

Although rotation was definitely not to be considered a part of mesometeorological phenomena, some consideration was given to rotation in helping to define the problem. Long stated that in the flow over the Sierra Nevada mountains some rotation could be seen but that it could be ignored successfully. Mr. Paul Frenzen pointed out that if the cumulus scale is included in the model, then rotation must not be ignored in connection with convective cells. Mr. Joshua Z. Holland, on the other hand, expressed doubt that this conference is particularly interested in convective cells. Although the idea of rotating the wind tunnel was suggested, it may be possible to introduce rotational effects to some extent artificially if such effects are deemed necessary.

C. Phenomena Which Might Be Studied

Because of the preliminary nature of the meeting, only indirect suggestions regarding specific subjects for study were forthcoming. However, repeated references were made to the possibility of studying Bénard cells, puffs, leaks, fallout, precipitation scavenging, and influence of topographical features. The problem of determining concentration of pollutants at various points in the immediate proximity of buildings and downwind from such sources as leaks and puffs came up repeatedly throughout the meeting. Dr. Franklin A. Gifford, Jr. raised the question again - just what are the essential phenomena of primary interest.

The suggestion was made that specific reactor sites be studied from the viewpoint of determining the best location in order to minimize potential danger downwind. The value of such studies was recognized, despite the fact that the results might be only qualitative rather than quantitative in nature.

Holland pointed out that it is necessary to distinguish between diffusion caused by the flow pattern in the immediate vicinity of the plant or other source areas and the flow pattern outside the control of these areas. In connection with this, he pointed out that an unusual accident might completely change the spectrum of the turbulence which normally was found in the vicinity of the area. Furthermore, it was pointed out that a distinction should be made between the instantaneous concentration at a point and the average concentration over a period of time - the instantaneous concentration being appreciably greater than the average.

Considerable and repeated mention was made of the fact that the turbulence in the atmosphere has a much broader spectrum than the turbulence which is usually associated with laboratory experiments. Holland pointed out that further research is needed in the atmosphere to determine more completely the statistical characteristics of the turbulence, i.e., the transfer properties with regard to heat, material, and contaminants.

The question was asked whether modes of heat transfer could be studied in a model. This stimulated the comment that the atmosphere is not a place to study modes because ordered motion is not found in the atmosphere. Instead, the spectrum of the turbulence should be investigated. Gifford believes that, on the average, over the scale of mesometeorological phenomena this spectrum is broad and smooth with no significant gaps. In his opinion, there are plenty of data available for the time being which have been taken in the field.

D. Techniques of Study

The techniques of study to be employed in modeling mesometeorological phenomena depend upon the purpose of the study. Upon the basis of the comments which were

made, the purposes could be broken down in one of the following two ways:

1. Demonstration for visual observation, qualitative research, and quantitative research, or
2. Fundamental research and applied research.

Considerable argument developed because of different viewpoints on the part of those in attendance with respect to which of these categories a given research project might fit. Rouse pointed out that there are very marked differences between the techniques of research when generalized and fundamental information is desired as compared with the techniques for conducting specific and applied research. Prof. Maurice L. Albertson mentioned the importance of conducting investigations concurrently wherever possible on a three-fold front which includes mathematical theory, laboratory research, and field investigation.

Other aspects of research techniques which were mentioned included the discussion by Prof. Franklin I. Badgley of the tidal model which was constructed for Puget Sound. In this case a study was made, by means of a scale model, of the tidal oscillations and flows in Puget Sound. Density gradients were introduced to show that they influence the flow and deposition of waste material. The currents induced by periodic fluctuations of the tides at the mouth of the Sound were successfully reproduced. The influence of fluctuations having periods of several hours was successfully demonstrated by showing how the fluctuations affected various islands and other features of the Sound.

Although the Puget Sound model was not successful in reproducing exactly the concentrations at a given point or a given time, the general flow patterns and the location of waste material, as demonstrated in the model, were found to be confirmed in the prototype.

Another study which was mentioned only briefly was that conducted by Prof. E. Wendell Hewson on the diffusion and behavior of pollen.

Gifford pointed out that it may not always be necessary to reproduce the complete turbulence spectrum for important flow phenomena. In other words, in some cases, the diffusion and flow pattern might well be relatively independent of the spectrum.

Holland pointed out that, despite the difficulty regarding viscosity, the flow pattern in the model of a Sierra Wave looked very much like that seen in the prototype. He added further that simple problems should be tackled first, in order to develop the techniques.

Some discussion emphasized the need for developing new techniques for conducting model studies of the type proposed at this conference.

The need for field investigations was repeatedly mentioned. Rouse pointed out that such field investigations are time-consuming, expensive and frustrating. He cited the example of the study conducted at Dugway during the War. It was intended to obtain data which could be checked with laboratory findings on diffusion downstream from a puff or a point source. Extensive preparation was made and the wind was finally just right for the test to begin. After beginning the test, the wind shifted and the field data were not obtained. Such repeated difficulties (which are not at all uncommon) make field investigations very expensive.

It was mentioned that the atmosphere is the best model of the atmosphere and, naturally, field studies should be used wherever possible. Topography - even hills only 100 feet in height - is known to have, under certain stable conditions, very large effects.

E. Value of Model Investigations

Despite the fact that considerable question was raised regarding the value of model studies, repeated mention was made of the fact that it has already been demonstrated that models can have value from the viewpoint of demonstration to show individuals the problems involved and possible solutions. Models also have proven to have value in a qualitative sense where they have shown whether one thing or another would be the better solution to a problem. Finally, models have proven in some cases to be able to predict quantitatively the phenomena involved.

F. Facilities Needed and Available

The facilities needed to conduct laboratory investigations of mesometeorological phenomena are not at all extensive in the United States. Therefore, Rouse suggested that a special laboratory be set up for this specific type of study. He cited the experience associated with the Vicksburg Laboratory of the Corps of Engineers of the U. S. Army. In this case, considerable difficulty was experienced in connection with the various hydraulic features of the rivers. Initially, designs were made and construction was carried out entirely on the basis of engineering judgment. Despite the efforts of the designers, hydraulic structures were continually being washed out. This went on for 80 or 90 years and became more and more expensive. Finally, in the late 1920's, a hydraulics laboratory was built at Vicksburg in order to study the hydraulic aspects of specific problems. Throughout its history, the Vicksburg Laboratory has demonstrated that the cost of modeling has been very small compared with the value of the knowledge gained and the savings in structures resulting from improved designs. Furthermore, the staff of the laboratory has gained increasing competence and a close liaison is carried on between the laboratory and the designer. Rouse expressed the belief that a similar type of laboratory should be developed for studies of mesometeorological phenomena.

Supplemental to, but also somewhat in contrast to, Rouse's suggestion, Albertson pointed out the value of depending upon educational institutions for fundamental investigations and the various agencies for the application of these fundamental principles to field problems. He stated that another approach is one like that of the Bureau of Public Roads, which is trying to get all existing laboratories in the United States interested in the problems of hydraulic structures associated with highway design. It is the belief of this agency that much is gained by having as many specialists in the field of hydraulics as possible interested in seeking solutions to the problems of the Bureau of Public Roads. Therefore, they have no laboratory of their own but depend entirely upon existing laboratories to conduct their research. Perhaps a combination of the systems suggested by Rouse and Albertson would be most successful in the study of mesometeorological phenomena.

It was pointed out that there is a shortage not only of existing laboratories to conduct the research in question but there is also an unfortunate shortage of personnel.

The problem of equipment needed for conducting research was discussed. In connection with this, Rouse pointed out that when working with air relatively simple and inexpensive models can be built because the forces involved are very small. Therefore, a large building - such as a hangar or large quonset - could probably be most valuable, and the equipment and models could be built so that they could be dismantled easily.

Rouse also pointed out that simply because wind tunnels had originated in the field of aerodynamics was no reason for wind tunnels intended for study of mesometeorological phenomena to be designed in the same way. In other words, the research for investigation of mesometeorological phenomena should be designed specifically for the needs of such investigation.

G. Summary

In summary, the rapporteur would like to point out that before specific plans can be made for the conduct of laboratory investigations of mesometeorological phenomena, it is necessary first to determine the principal areas of interest and the specific factors which need to be measured. Furthermore, it should be decided whether the study is to be purely for demonstration, for qualitative information, or for quantitative information. And, finally, it should be established whether the investigation is to be of a fundamental nature or an applied nature. Once these matters have been decided, the problem can be analyzed with a better understanding of the minds than under the conditions existing at the time of the conference.

The research program should be broken down into three parts and consideration be given to each of these parts in order to determine the role that each might play in helping to solve the problem under consideration. These three parts should be:

1. Theoretical or analytical research,
2. Laboratory research,
 - a. Fundamental or generalized research,
 - b. Applied or specific research,
3. Field Research,
 - a. On a small scale,
 - b. On a large scale.

Whenever possible, these three ways of approaching a problem should be carried on simultaneously, so that each can help the other to progress with maximum efficiency.

The problems which might be studied are puffs and continuous point sources and their diffusion downstream. The following factors should be studied with respect to their influence on the flow pattern and downstream concentration:

1. Free convection,
2. Forced convection,
3. Overhead or ambient turbulence which originates from a source far removed from the boundary,
4. Stability with respect to density gradients,
5. Boundary roughness,
6. Topographic features,
7. Rotation.

A systematic program of research should include the control, isolation, or elimination of most of these variables so that the influence of one alone can be studied. As the fundamental principles are better understood, additional variables can be added. While the foregoing fundamental investigations are under way, specific model-prototype comparisons can be conducted in order to help to hold the fundamental investigations on a more useful plane.

Session IV - NEEDS AND EXPECTATIONS

by
Joshua Z. Holland

The topics agreed upon for discussion were:

1. A review of current work and available facilities,
2. Needs for additional facilities and projects,
3. A delineation of specific problems in sufficient detail to indicate possible analytical and experimental approaches.

Wind tunnels and controlled atmosphere chambers at the University of Michigan, New York University, State University of Iowa, and Colorado A & M College were described, and additional facilities at Pittsburgh under Hemeon and at Worcester Polytechnic Institute were mentioned. Liquid tank facilities at the University of Chicago, John Hopkins University, State University of Iowa, and Argonne National Laboratory were referred to in other sessions, and the group later had an opportunity to inspect the last named.

A need for corroborative field studies was strongly emphasized by several participants. In particular, it was agreed that a combined field and model attack would be necessary in order to establish the range of applicability of model techniques. Prof. E. Wendell Hewson and Prof. Gordon H. Strom, having experience in industrial air pollution studies, brought out the difficulty of obtaining access to data from industrial plants. They observed, however, that hard-headed industrial executives seem to be satisfied with the engineering solutions to stack disposal problems obtained by means of wind tunnel model tests, indicating though not proving that these experiments are giving realistic results.

Other participants (notably Badgley, Long, Rouse, and Wanta) deplored the lack of scientific method exemplified by some engineering studies. They advocated (1) a more logical attack on idealized problems which could be formulated analytically and modeled relatively completely, or (2) a critical comparison of field and laboratory data on realistic and complex problems to test the conditions for similitude.

By this means, the theory for the model or for the similitude conditions could be confirmed. Having been confirmed in the region of exact modeling, the theory could be extended to prototype situations not susceptible to exact modeling, or the model technique could be extended to situations not susceptible to direct prototype observation.

There appeared to be good agreement that for long range development of modeling techniques it would be desirable that a laboratory be equipped and staffed to carry on (1) analytical studies, (2) laboratory test of theory, (3) model experiments on realistic situations, and (4) prototype field studies. It was recognized that the success of such a venture might depend upon whether one well-qualified and enthusiastic individual can be found to direct it.

Prof. Maurice L. Albertson outlined an experimental study of momentum transfer, mass transfer and heat transfer in the boundary layer. Using Lake Hefner, evaporation data (mass transfer) for full-scale verification, the wind tunnel data are found to be on the same curve, when plotted in suitably chosen coordinates, although the model parameters do not scale the prototype parameters in the strict sense. In order to study heat and momentum transfer in the 80-ft Colorado tunnel, heating, cooling and roughening of the boundary will be introduced. A major difficulty arises from the interference

of secondary circulations induced by side and top boundary effects in the tunnel. It is planned to bleed off the side boundary layers.

An interesting but inconclusive discussion of the validity of substituting eddy coefficients for molecular coefficients in the non-dimensional scaling parameters and in the dynamic equations took place, continuing the discussions on this topic which had occurred in earlier sessions of the conference. The fundamental question seems to be whether the eddy coefficients, being properties of the flow rather than of the fluid, can be used as independent variables in the analysis of the flow itself (Rouse). From experience in the analysis of a wide range of model experiments (Fultz, Long) there appear to be situations in which the role of eddy viscosity in the atmosphere is well represented in the model by the molecular viscosity.

Dr. Dave Fultz gave the group an enlightening review of the significance and interrelations of some of the non-dimensional parameters used in the study of free convection. On the mesometeorological scale, the possibility of modeling this phenomenon appears to be discouragingly remote. However, with exaggeration of the vertical scale, there is hope that vertical velocities induced by topography, either through thermal or mechanical perturbation, can be amplified sufficiently to permit analysis. Exaggerated surface roughness would be important in such studies. The possibility was suggested (Holland) that potential temperature, rather than geometrical height, is the significant vertical coordinate in stable temperature stratifications (inversions). This might at least ease some of the difficulties of instrumentation in modeling shallow, strong inversions by permitting exaggeration of the geometrical height.

The participants were unanimous in their expression of appreciation to the sponsors and, in particular, to Mr. Moses and Mr. Wanta for arranging this most stimulating and pleasant meeting.

III. REVIEW AND COMMENTARY

The group invited to this meeting represented one of the more informed groups in this country on the problems of modeling meteorological phenomena; hence it was able to discuss modeling of the difficult meso-scale with both grace and advantage. Some off-beat or unconventional ideas arose in the non-directed discussions of the group. Some differences of opinion also came to light. The atmosphere of the conference was nevertheless marked by an unexpectedly high degree of accord on many of the points discussed. Whether or not this was salutary, the reader must judge.

This section reviews some of the agreements and disagreements and some of the more unusual ideas, and closes with a general statement relating to the need for meso-meteorological model experiments.

Areas of Agreement

One of the first points of agreement was the definition of the term meso-scale, which was defined as the horizontal size range extending from somewhat larger than the size of a building up to about 100 miles. Participants also agreed on a number of areas of ignorance, and on what action should be taken to increase the effectiveness of modeling as a tool.

Work with models was accepted as falling into one or more of the following categories:

1. Visual aids for training and demonstration;
2. Study of correspondence between prototype and model over particular ranges of interest and feasibility;
3. Empirical learning from model when prototype is relatively inaccessible;
4. Experimentation which seeks to control in the model essentially uncontrollable parameters in the prototype;
5. Study of the model on its own scale for its own sake, i.e., to explain observed phenomena.

There was disagreement, though, as to which of these categories best fit specific cases.

Admittedly there are many gaps in our knowledge and understanding of the processes which take place in the lower layers of the atmosphere. One of these gaps concerns the nature of the convective process on the scale of cumulus clouds. Whether atmospheric convection takes place in the form of bubbles or organized cells or columns still remains unsettled. It was acknowledged that modeling atmospheric convection and complex orographic flow is most difficult particularly with differential and variable surface heating.

There was general agreement that the significance of turbulence spectra for mesometeorological processes needs clarification. For example, it is well known that the turbulence spectra in a modeling facility such as a tunnel and in the atmosphere are quite different. What role does this difference play in problems of modeling atmospheric dispersion? Given a variety of initial distributions and physical conditions of atmospheric contaminants, for example, soon after an unusual reactor accident, dispersal on a meso-scale and larger is still comparatively unknown. One would like to know how atmospheric turbulence spectra are altered by such unusual incidents.

The group readily accepted as well known that it is impossible to model Reynolds numbers strictly, since these differ greatly in the model and prototype. The point was made that strict Reynolds modeling may not be necessary.

As a course of action for increasing the effectiveness of modeling, it was frequently urged that serious effort be devoted to research on the techniques of modeling. For example, more ways need to be sought or developed for effectively distorting models through appropriate scaling parameters to overcome inherent limitations of modeling techniques. It was acknowledged that we can learn a good deal about the techniques of modeling from a program which more or less simultaneously carries out the following: (1) analytical studies; (2) laboratory studies to test theory and to establish principles; (3) model experiments on realistic situations; and (4) prototype field studies.

In model studies of realistic situations, it is imperative that adequate measurements such as flow velocities, temperatures, etc. be made. As indicated by Fultz⁽¹⁾ considerable early work on models has yielded sterile results, mainly because of the lack of quantitative observations.

By taking full advantage of modern technological advances in instrumentation, it was agreed that it is now possible to obtain measurements which were beyond reach a few years ago. Both prototype and model require study to establish the range of validity of particular modeling techniques. Some field studies should be more than merely corroborative; there can now be critical comparison between model and prototype. There was broad agreement that efforts to model meso-scale meteorological phenomena should be considerably multiplied.

Areas of Disagreement

The controversies which arose as might be expected stemmed mainly from lack of information in two areas: (1) meteorology, and (2) modeling technique.*

Not all participants felt that the Coriolis force had a significant effect at horizontal scales under the agreed upper limit of the meso-scale (100 miles). Some thought that rotational effects played an important part in the establishment of convective cells like the Bénard cells. However, the existence of ordered convective cells in the lower atmosphere was challenged. There was little dissent on the dearth of evidence that ordered convective motion occurs in the lower atmosphere.

The modeling techniques used by some were questioned by others. For example, the propriety of using the eddy viscosity derived from the Reynolds stresses for the prototype, and the molecular viscosity for the model, was argued; the former is a property of the flow, while the latter is a property of the fluid. The group which questioned this practice was willing to tolerate it in the case of modeling large scale or global phenomena, as having practical value.

*A third category, administrative considerations, may be added. The size and nature of modeling facilities, qualifications of investigators, relationships with other laboratories, etc., were discussed. Besides disagreement concerning the classification of specific research projects into categories of basic or applied research, some controversy arose concerning the type of organization most suited for research on mesometeorological modeling. One group favored a rather centralized laboratory, and another felt that the research projects should be distributed among the universities and small individual projects. The importance of the individual investigator was mutually recognized.

Another point of debate was whether studies should start with simple or with complex models. One group would begin as simply as possible, add complications gradually, and approach actual prototype complexity by stages. The other group favored starting with rather complicated models immediately and varying the atmospheric parameters singly to learn the effect upon some other variable of interest.

There was disagreement over a procedure to study a model first and then to use results as a guide in study of the prototype. Atmospheric dispersion in a complex valley may be taken as an example. Some participants would prefer to study first air movements in a simple valley by means of a dense meteorological network, thus using the atmosphere itself as a model. Some felt that time and money could be saved by studying a model of the complex valley first and then using the results as a guide for later full-scale investigations.

Unusual Modeling Techniques

Thinking was quite unfettered as to the form which a modeling enclosure should take for mesometeorological experiments. Proposed model facilities ranged all the way from no enclosure at all (using the atmosphere itself) to partial enclosures (open top), to hangars, still-air rooms, quonset huts, prefabricated structures which are easily constructed and dismantled, and rotating wind tunnels. Means for controlling the velocity and thermal structure of a model enclosure in an effort to provide effective simulation of the atmospheric boundary layer were also discussed.

A still-air (volume mean speed zero) room may be used for the study of the development of flow due to differential terrain heating, while the rotating wind tunnel or one with curved axis of flow may be used for the study of Coriolis effects. The gravity term in certain non-dimensional modeling numbers might be experimentally varied through use of techniques developed in studies of space medicine.

It was tacitly agreed that neither studies in gases nor studies in liquids should be discouraged. The check on sensitivity of experimental results to variations in viscosity of the test medium was advised, and may be accomplished through the use of liquids. Also with liquids it is possible to experiment with sedimentation as a substitute for buoyancy, i.e., model "upside down."

Another unusual idea was the suggestion that small-scale concentrated sources of energy (explosives) be used to model some part of the physical processes of dispersion which may be significant in the vicinity of a nuclear reactor building after a nuclear accident. Generally, a "no holds barred" attitude toward modeling technique was favored, even to modeling, for example, in x, y, θ (potential temperature) instead of x, y, z space.

Why Resort to Modeling ?

At the present time, meteorological modeling represents no substitute for field observations. Our knowledge about modeling is too inadequate to accept its use at present as a valid short cut in the solution of any but the simplest mesometeorological problems. Much arduous and imaginative work at a substantial cost will be required before adequate development of modeling techniques is achieved. Efforts to sample or test fairly the potential of this tool will take considerable time, and will also require better prototype measurements for comparison and other purposes than are now commonly available.

As stated by Fultz,⁽¹⁾ it has long been the dream of meteorologists to solve atmospheric problems by means of experimental work on a small scale. In recent years

there have been far reaching successes in other fields, such as aerodynamics and hydraulics. These have given renewed impetus to efforts in modeling meteorological phenomena.

For some problems the modeling experiment provides a method of arriving at answers, perhave even qualitative answers, which are too difficult or too expensive to obtain in other ways. A modeling experiment may be compared to an analog computer in which the controlled meteorological parameters represent the input and the resultant effect upon the parameter of interest is the output. Information concerning the relation between variables which in the prototype cannot be controlled or which are too difficult to handle analytically may thus be obtained from this technique.

Since a modeling facility permits control of meteorological and other important parameters either singly or in combination and also reduction of sizes, it becomes feasible to obtain a more complete set of data at less cost and in less time than through a corresponding prototype experiment. Thus, successful modeling would represent a valuable shortcut.

In the future, many new industrial establishments will be built, progress will be made in planning land use, and many radioactive waste storage facilities, nuclear fuel processing plants, and nuclear reactors will be constructed. Demands will arise for the selection of suitable sites and for safeguarding life and property from the potential hazards associated with possible mishaps. To meet these demands, information on the diffusive capacity of the atmosphere under a variety of meteorological conditions in individual kinds of terrain will be essential. Extremely large expenditures will be necessary if field experiments are to be conducted at every site to provide this information. Theoretical hazard evaluations without field experiments can at best be less than satisfying. Success in perfecting modeling techniques to provide short-cuts in furnishing at least part of the needed reference or survey information would prove of inestimable value. The risk of partial or meager success seems not unduly great. Not only will this achievement meet the rising demands of our society, but as stated by Rouse⁽²⁾ it "will open many an avenue of fruitful study hardly conceivable in advance."

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