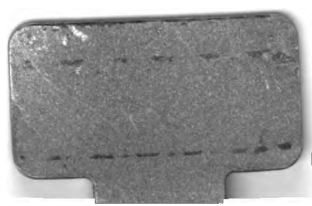# WEATHER and CLIMATE MODIFICATION
Problems and Prospects

VOLUME II

Research and Development

WEATHER and CLIMATE MODIFICATION

PROBLEMS and PROSPECTS

VOLUME II

Research and Development

Dr. Frederick Seitz, President
National Academy of Sciences
Washington, D. C.

Dear Dr. Seitz:

It is my pleasure, on behalf of the Committee on Atmospheric Sciences, to transmit to you the report of the Committee's Panel on Weather and Climate Modification. This report has been reviewed and unanimously approved by our Committee.

The Panel was established two years ago because the Committee was convinced that recent advances in mathematical modeling of atmospheric processes, computer technology and data communications, and foreseeable improvements in meteorological instrumentation held promise that a rational exploration of weather and climate modification could be one of the important developments in the atmospheric sciences during the present decade. The deliberations of the Panel over the past two years have confirmed this conviction. Moreover, the analysis of the results of field experimentation to augment precipitation, while still not definitive, clearly suggests that this aspect of the problem has now reached a stage at which it deserves more conclusive evaluation.
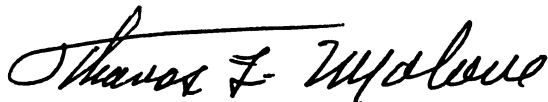
The Committee was not unmindful of the close relationship between weather prediction and weather control. It was confident that, should efforts directed at understanding the physical basis of weather and climate modification demonstrate that deliberate intervention is impractical, the research that led to such a conclusion would nevertheless have broad application to the important problem of weather prediction.

We believe that the findings of the Panel deserve careful and thoughtful consideration by the government and the nation at large. To be successful, the national program recommended by the Panel must represent a fine balance between unwarranted optimism and undue skepticism. There is an unparalleled opportunity for our scientific community and our federal government to demonstrate imagination, perception, and wisdom in the management of a program having both intrinsic scientific interest and potentially far-ranging socio-economic and political consequences.

The Panel properly emphasizes the role of international cooperation in this endeavor. In the research stages, and before meaningful control of the atmosphere might become an accomplished fact, it is important that the foundations be laid for what may be a new dimension of international cooperation. It is singularly appropriate that this report has been developed during the International Cooperation Year, dedicated to a re-examination of international cooperation and identification of new opportunities for extending cooperative efforts among nations.

A long and winding road lies ahead, with the outcome still uncertain. However, our Committee commends this problem as one deserving the further thoughtful study and attention we trust it will receive during the years ahead.

Respectfully,

Thomas F. Malone
Chairman
Committee on Atmospheric Sciences
National Academy of Sciences—
National Research Council

Committee on Atmospheric Sciences

*Deceased December 2, 1965

Panel on Weather and Climate Modification

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*Through 1964

Preface

In November 1963, the Committee on Atmospheric Sciences of the National Academy of Sciences appointed a Panel on Weather and Climate Modification "to undertake a deliberate and thoughtful review of the present status and activities in this field, and of its potential and limitations for the future." The complexion of the field had changed subtly since the appearance in 1957 of the final report of President Eisenhower's Advisory Committee on Weather Control. It was time for a new and broader evaluation.

Public interest in weather modification led the National Science Foundation to establish, in June 1964, a Commission on Weather Modification. The Academy Panel has worked closely with this Commission on the relevant scientific problems. The primary responsibility of the Commission is to advise the National Science Foundation on the Foundation's present and future activities in the field of weather modification and to respond to a request by the Interdepartmental Committee for Atmospheric Sciences for an analysis of the problems and potentialities of the field. Since the Foundation has broad statutory responsibility in weather modification, the Commission report will treat legal, economic, and social questions that have been of interest to the Academy Panel but are outside its specific competence and responsibilities.

The Panel resolved that it should attempt a fully comprehensive scientific review and complete it within two years. It also decided that its usefulness would be greatly diminished if it declined to discuss controversial issues or avoided critical areas of conflict and confusion. Consequently, the process leading to this final report has involved several steps. First, a series of meetings with many interested scientists during 1964 culminated in the limited publication, in October of that year, of a preliminary report.* Second, the report was distributed, with an invitation for criti-

* *Scientific Problems of Weather Modification*, NAS—NRC Publication 1236 (October 1964). The present report supersedes Publication 1236.

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cism, to a broad selection of atmospheric scientists, in particular, to those directly engaged in weather modification research and operations. Responses to that report were stimulating and valuable in the further work of the Panel, and led to the disclosure of data not previously available. A subsequent series of meetings culminated in the preparation of working papers for discussion among panelists and invited experts during a final two weeks of study sessions in Woods Hole, Massachusetts, in August 1965.

In carrying out its mission, the Panel undertook to examine all the relevant technical and scientific aspects of weather modification. It sought to take advantage of the experience accumulated by those engaged in operational weather modification both through examination of their writings and through personal contacts and discussion. The Panel did not go deeply into the complex legal, sociological, or economic aspects of weather modification but, throughout its work, was keenly aware of these nontechnical factors.

The meeting time of the Panel totaled 34 days during which panelists heard 56 invited experts. In addition, Panel members devoted a great deal of time to preparation of position papers and reports. They also undertook research to amplify and support prior investigations. Special mention should be made of the work of James E. McDonald, who through the spring and summer of 1965 devoted a major fraction of his time to a study of reports of operational cloud seeders, and of Joseph Smagorinsky, who instigated a study of the effects of H_2O and CO_2 on the radiation balance in the stratosphere, carried out in his laboratory by Syukuro Manabe. The Panel also gratefully acknowledges the contribution of two additional special studies on cloud seeding effects, one conducted in The Environmental Science Services Administration by Glenn W. Brier and Dwight B. Kline, the other in the RAND Corporation by Theodore E. Harris, Albert Madansky, R. Robert Rapp, and Charles Schutz. These special reports are included here as appendixes to Volume II.

The present report, then, is the result of a full two-year examination and study by the Panel. Volume I contains a summary of the status of weather and climate modification, suggestions for essential research, and recommendations for actions that appear mandatory to ensure orderly and rapid progress in the future. Volume II presents a general assessment, on which the Panel has based its conclusions and recommendations. Certain conclusions reached in this report are at variance with conclusions stated in the Panel's earlier preliminary report. The present report reflects the use of data not available to the Panel at the time of issuance of the preliminary report. *The two volumes of this final report completely supersede the Panel's preliminary report.*

The Panel expresses its gratitude to Ralph E. Huschke of the RAND Corporation for devoted assistance throughout the several months this final report was in preparation. Thomas F. Malone, Chairman of the NAS Committee on Atmospheric Sciences, was a constant source of valued guidance to the Panel; and John R. Sievers, that Committee's Executive Secretary, very capably provided for the many important details of administrative support. The Panel gratefully acknowledges the opportunity to undertake this study on behalf of the National Academy of Sciences—National Research Council, with support from the Atmospheric Sciences Section of the National Science Foundation and the Environmental Science Services Administration, under Task Order No. 83 of NSF-C310.

GORDON J. F. MacDONALD
Chairman
PANEL ON WEATHER AND
CLIMATE MODIFICATION

November 1965

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WEATHER and CLIMATE MODIFICATION
PROBLEMS and PROSPECTS

Introduction

All atmospheric processes and weather phenomena are ultimately induced by the solar energy reaching the earth. At present, we cannot modify that energy at its source, nor can we intervene between the sun and the earth's atmosphere. Clearly, in the foreseeable future, if any aspect of the atmosphere is to be altered by man, it will be done by interference within the atmosphere or at its lower boundary. It is the purpose of this volume to present an assessment of possible modes of interference, both deliberate and inadvertent, and to discuss the principal theoretical and technological problems that are now apparent.

Only a decade ago, major elements of the scientific community were reluctant to regard weather and climate modification as a legitimate scientific pursuit. Perhaps the first public statement of a general shift in attitude, a statement that also presaged a more comprehensive view of the subject, was made by Professor H. G. Houghton in October 1957: *

. . . our recent progress in meteorological research is such that it no longer appears visionary to talk about weather control.

And, concerning the risks of experimenting with the atmosphere on a very large scale:

It would be unthinkable to embark on . . . a vast experiment before we are able to predict with some certainty what the effects would be. Without such knowledge the effects might be catastrophic or, as a lesser evil, there might be no noticeable effect after the expenditure of large sums. A much more reasonable approach lies in continuation of basic research. For reasons quite aside from weather control we are now attempting to construct ever-more-realistic mathematical models of the atmosphere. When a model has been developed that seems to incorporate all the important features of the real atmosphere and operates much like the real atmosphere we can rather easily explore the effects of artificial modifications. . . .

* Speech presented at the First National Conference on Applied Meteorology, Hartford, Connecticut, October 29, 1957.

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Professor Houghton's conclusion:

Basic research in meteorology can be justified solely on the economic importance of improved weather forecasting but the possibility of weather control makes it mandatory.

The many different problems of weather-modification research can and should be viewed together as a cohesive subject. As stated by a RAND Corporation study group (*Greenfield et al.*, 1962), the essential ingredients of this viewpoint are as follows. Energy is transferred and transformed to and within the atmosphere by properties and processes that span a spatial range of about 17 orders of magnitude, from the size of a molecule to that of the earth. Within this enormous spectrum there appear to be preferred bands of concentrated energy exchange that are associated with recognized atmospheric phenomena. The major portion of atmospheric energy exchange is due to the release of instabilities inherent in the preferred phenomena; and these dynamically unstable situations are looked upon as "levers" or "soft spots" in the system where man's efforts might be able to trigger a chain of natural reactions. Four of these unstable soft spots are: (1) the phase instability of water substance as a result of supersaturation in the vapor phase and supercooling in the liquid phase, the triggering of which produces a local heat source; (2) colloidal instability of cloud particles, which must be released for clouds to precipitate and thereby finally transfer the latent energy of water-vapor phase change to the atmosphere from the originating points of evaporation at the earth's surface; (3) convective (or vertical) instability of the local atmosphere, the release of which relieves the accumulation of heat commonly near its source at the earth's surface by injecting it into the higher atmosphere and, in the process, often produces kinetic and electrical energies of violent consequences; and (4) baroclinic (or horizontal) instability in the large-scale circulation, which serves to relieve the imbalance of heat between equator and poles through the creation of large extratropical cyclones.

The only hints of any real success in weather modification to date have come from attempts to exploit our meager understanding of the first three of the instabilities mentioned above. This has been possible because the sizes and energies of the triggering mechanisms have been economical to produce. Man has exploited these instabilities on a limited scale in the belief that the effects would be as short-lived as the phenomena themselves and that the energies released would not escalate to the level of baroclinic instability.

Energy can progress upward through the preferred scales of exchange. The release of phase instability in an aggregation of supercooled cloud

drops can simultaneously colloidally destabilize the cloud into precipitation (through the Bergeron-Findeisen mechanism) and, through the introduction of latent heat of fusion, convectively destabilize the volume of air within which the phase change is occurring. All of this has been observed, on a very small scale, in the seeding of stratocumulus clouds. It is not hard to imagine that induced convection or induced snowfall on a much larger scale could sufficiently alter the horizontal temperature distribution to trigger or subdue baroclinic instability, changing the natural development of large cyclonic storms. This, in turn, might alter the global radiation balance and thus influence a fifth scale of instability about which we can only conjecture—the possible instability of global climate.

The great variability of ancient climates is accepted as fact, yet the cause of climatic change is far from being a settled issue. It is obvious that the earth-atmosphere system can support radically different climatic regimes, some of which could be disastrous to civilization. We do not yet know what can cause a shift from one climatic regime to another, whether change can occur in an "instant" of geologic time or only as a secular cyclic process; our few theories still hang on the most tenuous of evidence. Growing data from a large variety of scientific research is placing our understanding on a firmer ground; and it must continue to do so.

Numerically integrated mathematical models of the atmosphere have come to be regarded as necessary tools for research in atmospheric modification. Indeed, the application of numerical techniques to theoretical models has in the past 15 years encompassed a great variety of scales, phenomena, and processes of interest. Perhaps the greatest advances to date have been made in simulating the general circulation. Because of its implications relating to climate modification, this is treated at length in a later section of this report. More recently, similar models, not yet as fully developed, have been constructed to investigate large-scale ocean-atmosphere coupling. There have also been theoretical-numerical simulations of planetary boundary-layer processes, and, on a larger scale, buoyant convection, sea-breeze circulations, and hurricane propagation. Some of the earliest work has been on elements of the hydrological cycle. On the other end of the spectral scale are studies on modeling the physics of rain drops and the molecular processes of radiation transfer.

As the contents of this volume suggest, the Panel takes a very broad view of the field of atmospheric modification. Problems in modifying clouds and storm systems are recognized as having an immediacy of interest not presently shared by other areas within the field. However, this Panel does not hold that the other areas are either unrelated to the problems of cloud modification or of less potential practical importance.

Modifying Clouds and Storm Systems

INTRODUCTION—SOME PERTINENT ASPECTS OF CLOUD PHYSICS

Weather-modification research has, in the past, focused on clouds and has thereby exposed our ignorance of them. Until the seeding experiments in the late 1940's, and with the exception of a few devoted students, meteorologists regarded clouds in little more than a morphological or aggregative sense. Their respect for the complex individuality of clouds and for the dynamic importance of cloud systems is now immense.

A cloud can embody three of the potentially triggerable instabilities described in the introduction to this volume—the phase instability of supercooled water drops, colloidal instability of the aggregate of cloud particles, and convective instability of the cloud's vertical thermal structure. In addition, cloud systems probably have important influence on large-scale baroclinic instability through their roles in atmospheric energy exchange, both as releasers of latent heat and as absorbers and transformers of radiant energy. Furthermore, clouds are the means by which the waters of the oceans are converted into a useful fresh-water supply on the land. In cloud physics, especially in the context of weather modification, the study of precipitation processes has therefore attained a predominant importance. It may be useful, through the example of precipitation physics, to summarize some pertinent scientific aspects of clouds and storm systems before proceeding with discussions of specific applications in weather modification.

The evolution of precipitation from water vapor requires a number of physical steps. Briefly, (1) the air must be cooled, or vapor must be added, until the air becomes saturated; (2) the air must contain particulate nuclei to enable the phase transition from vapor to water or ice to take place without great supersaturation; and (3) some of the cloud particles thus formed must grow large enough to fall out of the cloud.

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The first step may be accomplished by a gradual lifting of the air during the horizontal transport (advection) of large air masses, by which process it cools adiabatically and finally reaches saturation. Advective lifting, even though it may be very slow (of the order of centimeters per second), is usually connected with the expenditure of large amounts of kinetic energy in form of eddies (large-scale cyclones and anticyclones) in the general circulation of the atmosphere. Convective lifting, on the other hand, is a local atmospheric process in which the energy, available initially in the form of excess heat at a low elevation, drives the updraft mechanism through buoyancy. Here, also, great energies can be expended, but normally it requires the subsequent release of latent heat within the convective cloud to drive the truly intense updrafts, which may in thunderstorms reach speeds of tens of meters per second. The required cooling is also frequently produced by the forced lifting of air over elevated terrain—orographic lifting. Orographic updraft speeds are a function of the horizontal wind speed and direction and the steepness of the slope. All three of the above lifting mechanisms can, and often do, act together. Not all clouds form as results of a lifting process. Extended stratiform cloud layers and ground fog may form by radiative cooling; and persistent fog over coastal areas is usually a mixing phenomenon due to the passage of moist maritime air over cold coastal waters.

For the process of condensation to occur without great supersaturation, nature must provide particulate condensation nuclei. The number of nuclei available is an important factor in determining the concentration of cloud droplets. While we have a fair theoretical understanding of the basic condensation mechanisms at the base of convective clouds (e.g., *Howell*, 1949; *Mordy*, 1959; *Neiburger and Chien*, 1960), observational data on the nature and concentration of natural condensation nuclei in continental and maritime clouds are lacking. Such evidence as we now have indicates that nucleus distributions cause a concentration of cloud droplets of less than 100/cc in maritime air, and greater than 100/cc in continental air. Under special conditions the air may be so pure that it contains only a few condensation nuclei per cubic centimeter. This has been observed on mountain tops and in the higher tropospheric layers, on the surface in Greenland, and above the trade-wind inversion near Hawaii. It has been found that true condensation nuclei generally must lie in the diameter range from 0.05 to 1.0 μ. Aside from the size requirement, the only condition required if a particle is to act as an effective condensation nucleus is wettability, that is, the contact angle between the material of the nucleus and water must be close to zero. Nonwettable surfaces would require a degree of supersaturation that may not be reached in the cloud; therefore such particles would not be involved in nucleation. Hygroscopicity (the ability, such as that of salt, to condense

water out of subsaturated air) is not an indispensable condition, though it certainly plays an important role in natural nuclei.

Our understanding of the formation of minute ice particles, such as those comprising cirrus clouds, is much less certain than our understanding of the condensation process. Apparently the ice crystals can be obtained in two different ways: (1) through the crystallization of supercooled water droplets stimulated by a "freezing nucleus"; and (2) through the direct sublimation of water vapor to ice, perhaps stimulated by a "sublimation nucleus." In nature the most frequent process is probably the first one, but we should add that the two processes can be so intimately intertwined that it is extremely difficult to know which process is active in a given instance. *Fletcher* (1962) makes this clear with two examples: (1) A nucleating particle may act as a condensation nucleus, and the very thin initial water film may immediately crystallize without ever reaching a true droplet stage. (2) Condensation can occur on the soluble part of a composite nucleus forming a small water droplet that can in turn be crystallized by contact with the insoluble part of the nucleus. In both examples one could incorrectly assume that a sublimation nucleus had been active because no appreciable liquid phase had been involved. The basic requirement of ice-forming nuclei (whether for freezing or sublimation) is a similarity in the crystal structures of the nucleus and of ice; it has therefore been suggested that the bulk of natural ice nuclei are made of silicates and clay minerals (*Kumai*, 1951, 1957, 1961; *Kumai and Francis*, 1962; *aufm Kampe et al.*, 1952; *Isono*, 1955; *Mason*, 1956; *Mason and Maybank*, 1958; *Mossop*, 1963; *Byers*, 1965). This similarity is not required of the total crystal structure but only for the two planes that first contact the water. Therefore, substances whose crystal structures belong to different crystallographic groups will also act as ice-forming nuclei, particularly at low temperatures. Studies of the ice nuclei in ice fogs of Alaska at low temperatures have indicated an abundance of hydrocarbons (*Kumai*, 1964).

The natural occurrence of freezing nuclei has been measured. Their concentration appears to be related to the air temperature by an exponential law that increases their number by a factor of 10 for each 4°C decrease of temperature (*Fletcher*, 1962). It is well established that the natural abundance of ice-forming nuclei is many orders of magnitude smaller than the abundance of condensation nuclei. The temperature range in which ice nuclei are of practical importance is limited because, at temperatures below about -40°C, ice forms from supercooled water by the process of homogeneous nucleation, i.e., without the requirement of a foreign nucleus.

Clouds (thus obtained by water condensation or crystallization from saturated air) may contain a variety of forms of the minute cloud

particles. An entire cloud may be "warm," consisting exclusively of liquid droplets at above-freezing temperatures. If made up of liquid droplets at below-freezing temperatures, it is a "supercooled" or "cold" cloud. An "ice" cloud is one composed entirely of ice crystals, e.g., a cirrus cloud. When a cloud contains two or more particle forms, it may be referred to as a "mixed" cloud. To get precipitation out of any of these clouds, it is necessary to cause some of the minute, suspended particles to grow very large, possibly at the expense of others.

The first tenable hypothesis for the growth of precipitation particles that did not invoke mechanical agglomeration of droplets or crystals was presented by *Bergeron* (1933). He proposed that in an admixture of ice crystals and supercooled water droplets the ice crystals would grow at the expense of the droplets. The basis for this is the difference between the saturation vapor pressures over ice and over water. Air that is just saturated relative to water is supersaturated relative to ice. Hence, condensation will continue on the ice crystals, thereby removing water vapor from the air, which then becomes subsaturated relative to the water droplets; the droplets then begin to evaporate and in so doing replenish the vapor supply demanded by the ice crystals; the process continues until all liquid droplets vanish. The mixed supercooled and ice-crystal cloud systems required by this process seem to occur frequently and in a variety of forms. In all the principal precipitating systems (cyclonic storms, orographic storms, and convective shower clouds), the vertical extent of the cloud mass is usually high enough to reach sufficiently cold temperatures for the two types of particles to exist together.

However, many precipitating clouds remain warm throughout, particularly in the oceanic tropics where the most frequent precipitation is from cumulus clouds that rarely reach higher than 10,000 ft, a fact noted, incidentally, by Bergeron in his 1933 paper. With a few notable exceptions (e.g., *Houghton*, 1938; *Simpson*, 1941), the reports of rain being observed to fall from warm clouds in the tropics were at first received with skepticism from those meteorologists whose experience had been confined to the middle latitudes. The problem of explaining "warm rain" became a matter of general interest when *Langmuir* (1948b) suggested how coalescence alone might produce large raindrops. Workers in other countries soon thereafter came to similar conclusions (*Bowen*, 1950; *Ludlam*, 1951). In all these investigations one difficult problem, clearly expressed first by *Houghton* (1950), presented a bottleneck: How can cloud droplets initially get large enough to collide with others and effectively start the coalescence mechanism? *Hocking* (1959) suggested, on the basis of theoretical calculations, that the droplets must have a radius of at least 19 microns to do so. To grow from known drop-size distributions measured

in continental cumulus clouds to this size would require longer than the average lifetime of most cloud elements (*Mason,* 1952). It was then suggested that the microturbulence in those clouds might exceed the accelerations due to gravity, and thus affect materially the rate of collisions between cloud droplets (*East and Marshall,* 1954). After it was found (*Squires,* 1956) that the warm tradewind cumulus clouds exhibited very different droplet concentrations and size spectra than had been measured in continental cumuli (fewer and larger droplets), recomputations of coalescence efficiencies (*East,* 1957) revealed that growth to precipitable size could occur within a reasonable length of time relative to the life cycle of cumulus clouds (*Blanchard,* 1957). Further support is provided by the hypothesis (*Woodcock,* 1952, 1957) that the existence of giant sea-salt nuclei favors the rapid early growth of some of the cloud droplets.

Much, if not most, cloud formation and precipitation in nature defies such simple explanation as the foregoing. For example, it is increasingly apparent that convective precipitation often results from complex combinations of ice-crystal and coalescence processes. Air circulations within cloud systems can dominate the precipitation process, as they certainly do in the case of hailstorms. An influence of cloud electrification on coalescence is suspected. Furthermore, cloud systems interact dynamically with both the local and large-scale features of the atmosphere, and they are strongly influenced by physical properties of the underlying earth's surface. Both of the latter influences act to produce wide variations in cloud structure and storm types that are related to geographical location, time of day or year, and the over-all synoptic weather situation.

A thorough review of cloud physics and dynamics is not intended here. The discussions that follow point out in greater detail more of the specific concepts and problems in cloud physics related to various goals of weather modification.

PRECIPITATION STIMULATION AND REDISTRIBUTION

While the total worldwide precipitation may remain adequate to support civilization for centuries to come, the uneven geographical distribution of rainfall and its large and unpredictable natural variability will impose increasing economic hardships. Population growth and centralization and the increasing concentration and productivity of agriculture will not only require more good-quality water than is needed today but also will force heavier reliance on elaborate water-supply systems. Most of such systems will be limited, on economic grounds, to watershed areas so small

that they will remain, as they are today, intrinsically vulnerable to fre-
quent short-term droughts. Drought alleviation, then, will continue to be
an objective of artificial rainfall stimulation. Furthermore, if the amount
of precipitation supplying a water system can be maintained by artificial
means, at a higher-than-natural level, then there will be less need to
expand the watershed area.

The latter objectives have underlain essentially all the many attempted
experiments and operations in rainmaking over the nearly 20 years that
have elapsed since V. J. Schaefer's initial discovery of a technique for
inducing the freezing of supercooled cloud droplets (*Schaefer*, 1946).
This vigorous activity has produced a number of interesting revelations,
the most pervasive of which is the astounding complexity, not previously
fully appreciated, of natural physical processes in the atmosphere. Natu-
ral variability is the big obstacle to obtaining quick answers to any
questions of weather modification. Each time some new phenomenon has
come under scrutiny in modification studies (rain, hail, fog, hurricanes,
tornadoes) the principal effect has been a greater awareness of the extent
and nature of variability of that phenomenon.

In the following discussions of precipitation stimulation we distin-
guish "experimental" from "operational" efforts. The former are guided
by objectives and procedures of research, the latter by objectives and
procedures of business. A few organizations have gone in both directions.
Experiments are undertaken and designed so that they may add directly
to the general store of knowledge. Operations have not been thus moti-
vated, and the general knowledge they have contributed has been
extracted only with great difficulty. The first three of the discussions
deal with experimental precipitation stimulation from three categories of
clouds and storm systems—orographic, cumulus, and cyclonic. The fourth
discussion concerns operational experience, which has involved a mixture
of all the storm categories.

Orographic cloud systems

Orographic cloud systems are those produced primarily by the forced
ascent of air passing over mountain barriers. Cyclonic and convective
factors are often superimposed on the forced lifting, so that separation
into distinct types is seldom clear-cut. Some of the world's highest mean
precipitation amounts are found where moist winds flow against mountain
barriers, so it is not surprising that, early in the history of weather
modification, attention was drawn to the possibility that precipitation
augmentation might be particularly favored in orographic cloud systems.
This point was emphasized by *Bergeron* (1949) and later stressed by

Ludlam (1955), *Thom* (1957b), and *Houghton* (1957), and has continued to occupy a prominent place in the thinking of many other meteorologists concerned with the prospects of weather modification.

The 1957 report of the President's Advisory Committee on Weather Control placed special emphasis on the likelihood that west coast winter orographic storms are amenable to precipitation enhancement by cloud seeding. That committee concluded that "The statistical procedures employed indicated that the seeding of winter-type storm clouds in mountainous areas in western United States produced an average increase in precipitation of 10 to 15 percent from seeded storms, with heavy odds that this increase was not the result of natural variations in the amount of rainfall." This conclusion was challenged on statistical grounds of possible hidden bias (*Brownlee,* 1960; *Neyman and Scott,* 1961); further comment on this question has been given by *Gilman et al.* (1965) and by this panel (in a later section of this report). The Advisory Committee strongly recommended that randomized seeding trials be conducted in such orographic storms, and the Santa Barbara project was organized as a step in that direction. Unfortunately, the results (*Neyman et al.,* 1960) were rendered somewhat equivocal due mainly to probable contamination from nearby seeding operations of another project. One season's operations indicated large positive effects of seeding, about a 100 percent increase in 1957, versus nearly zero in 1958. Until quite recently, no other randomized trials have been conducted on winter orographic storms of the western United States, but at least two are in progress now (one in Colorado and one in Nevada). The findings resulting from these trials are awaited with interest. The initial indications for the Colorado trials are positive, but sample size is not yet large enough to draw any statistically significant conclusions. In the Nevada trials near Elko, no results have yet been reported; a second randomized trial is currently being organized near Reno.

The only summer orographic seeding trials done in the United States on a randomized basis are those of *Battan and Kassander* (1962) in Arizona. Their treated clouds were so distinctly convective in character that one might logically classify them as cumuli, but since their treated clouds were formed with strong orographic uplift due to the Catalina Mountains, they will be discussed here. During 1957–1960, silver iodide (AgI) was released from aircraft flying at about the $-6°C$ level (threshold of nucleation for AgI) about five miles upwind of the nearest foothills. After four summers, the average precipitation for all seeded days was found to be lower than for the unseeded comparison days; but the significance level was a mere 0.40, so the only allowable conclusion was that no significant difference existed between seeded and unseeded populations. Subsequently, seeding at flight altitudes about 2,000 ft below

cloud-base level along a seeding track about 5 miles upwind of the mountain foothills was carried out for three additional summers. Results have not been published, but preliminary indications are that this method also failed to show significant differences between seeded and nonseeded periods. *Battan* (1963) has discussed radar data suggesting that the summer orographic cumuli that were treated in the Arizona seeding trials develop precipitation primarily by the coalescence process, which may account for the lack of response to AgI seeding. The possibility that some other explanation is involved cannot be rejected, however. Whether AgI nuclei reach the active cores of cumuli when released well above cloud-base level at points upwind is a question that might be raised concerning the first portion of these Arizona trials. The strongest basis for concluding that entrained AgI does get to the centers of the cumuli is the fairly extensive evidence that the liquid water contents of such cumuli are found to average well below the ideal adiabatic value, even near the cloud cores. Presumably, then, AgI nuclei entrained into the sides of the clouds mix deeply into the cores at some altitude. The seeding technique used in the later years of the Arizona trials gave essentially similar effects (statistically nonsignificant deficits of rain on seeded days), yet involved quite different injection conditions. Those latter conditions were essentially the same as the seeding conditions employed in most of the Australian orographic trials. The very fact that optimal seeding techniques are not yet known is one more measure of the still-rudimentary status of cloud modification.

A number of experimental trials of orographic seeding with silver iodide have been conducted in other parts of the world. Results have only very recently been reported on several of these; others are still under evaluation. Although difficulties of interpretation are present in certain of these trials, it can be said that, taken as a group, they tend to support the conclusion that positive seeding effects may be obtained in many orographic regions.

Orographic-seeding trials in the Okutama region of Japan during a 3-month period (January–March, 1960) have been reported by *Isono* (1961) as giving indicated increases of approximately 50 percent, with a probability of 0.005 that the result could have come by chance. No detailed account of this project and its evaluation appears, as yet, to have been reported in the scientific or technical literature.

In Australia, a 5-year experiment using aircraft to seed winter orographic clouds over the Snowy Mountains on a randomized basis has been reported by *Smith et al.* (1963). An indicated 19 percent increase, significant at about the 0.05 level, was rendered somewhat equivocal by the observation that recording precipitation gauges in the target area failed to show marked effects within the first 2 to 4 hours after onset of

seeding. For the latter reason, the investigators declined to draw final conclusions from this experiment. It is of interest that the investigators noted the most positive differences between seeded and unseeded storms in the spring, when there was the highest frequency of occurrence of the cumuliform-type cloud system regarded as likely to be most amenable to seeding. Four other randomized trials have been conducted in Australia, all with orographic effects playing at least a small role (*Adderley*, 1961). One of these (Darling Downs) gave an apparent decrease of 13 percent at the 0.03 significance level; two gave nonsignificant effects. A fourth Australian randomized aircraft AgI-seeding project was conducted under orographic conditions in the Darling Downs area of the Great Dividing Range from 1958 to 1963 (*Smith et al.*, 1965). Although the first year's results indicated an increase of about 30 percent, significant at about the 0.01 level, subsequent years were variable. The over-all 6-year period exhibited about 4 percent more rain for seeded periods than for unseeded periods, when adjusted by the double-ratio method used by the investigators. The over-all significance level was estimated by several techniques that led to probabilities ranging from 0.02 to 0.20. In some years (1961, 1963), substantially less precipitation fell under seeding conditions than was statistically anticipated, a result tentatively ascribed to a preponderance of clouds with tops warmer than $-10°C$. Since still other workers have, in the past, suggested that it would be just such weakly supercooled clouds from which one might hope to obtain the largest relative precipitation enhancement, one has here further indication of the still very unsettled state of the seeding art. In still another Australian orographic experiment, carried out in the Warragamba catchment area between Sydney and Canberra, 4-year results show slightly less precipitation (few percent) for seeded periods, but the probability of this being due solely to chance is high, about 0.6 (*Bowen*, 1965). The possibility that randomized trials of this type may be vitiated by the so-called "carry-over effect" has been suggested by Bowen and others. It is briefly discussed later in the section on Cloud Seeding Technology. The investigators have expressed the suspicion that they got weak or negative seeding effects with stratiform clouds and positive effects with cumuliform clouds. Further randomized trials are currently under way in Australia.

Siliceo et al. (1963), in an experiment of partly randomized design, carried out both aircraft and ground-generator AgI seeding of mixed orographic-convective clouds on the east slopes of the Mexican cordillera (Necaxa basin). Their results were analyzed in a variety of ways, and quite different effects were indicated for synoptic situations giving generally light and heavy rains, respectively. For days on which the control-area precipitation was less than 20 mm/day, approximately a 20 percent increase within the target area was inferred; but for days with control

amounts in excess of 20 mm/day, a target-area precipitation decrease of 8 percent was indicated. The investigators emphasized that the over-all seeding effects appeared to be due to a relatively small number of cases in which seeding was effective.

Many other studies have seemed to indicate similar variability of seeding potentiality with respect to certain subclasses of clouds or certain times in the life cycles of the clouds. It is important to recognize both the plausibility of such inferences and, at the same time, a certain danger that they may be merely *ad hoc* explanations of patterns or trends that appear only as sampling fluctuations due to the notorious variability of all cloud and precipitation processes. Only after one has seen the same pattern or trend manifest itself in a number of rather similar experiments of well-designed types will it be safe to draw firm conclusions. This built-in danger of being misled by natural variability must not be overstressed to such a point that we close our eyes to first indications of such important patterns and trends, for it will be mainly from carefully studying such patterns that we may hope to exploit more fully the potentialities of cloud seeding.

The foregoing examples of orographic seeding experiments may suffice to indicate the generally encouraging if not yet conclusive evidence suggesting that, under certain orographic conditions (probably, chiefly in winter), silver iodide cloud seeding may yield positive effects in stimulating increased precipitation. The magnitude of those effects would seem to be similar to that reported in the work of the President's Advisory Committee on Weather Control (*Thom*, 1957*b*). In a subsequent section on operational seeding experience, further indications of positive effects of about that same magnitude (order of 10 percent) are summarized. Although it is not within the scope of the present study to assess economic significance, it seems relevant to note that some estimates have suggested that seeding costs in many orographic operations would be repaid if a small percentage increase could be secured. For this practical reason, the experiments and the operational results cited above may justify acceleration of a broad spectrum of research investigations designed to achieve three ends: (1) much more conclusive evidence, based on carefully planned and executed randomized trials, for the validity of the now-available indications that positive seeding effect may be obtained in certain orographic situations; (2) more detailed information than is now available concerning the many features of orographic cloud microphysics and structure, water budget, and cloud dynamics that must be adequately understood if seeding techniques are to be optimized; and (3) improved measurement techniques and seeding technology arrived at by applying engineering methods of research and development.

Of these three objectives, the second may warrant some elaboration

here. We do not at present have more than rough notions of the size-distributions or spatial and temporal variability of condensation nuclei and ice nuclei for the areas of the western United States in which orographic seeding may be the most promising. (Nothing has been said here about orographic seeding in the highlands of the eastern United States, but our ignorance of relevant parameters is at least equally great there.) Many details of both cloud dynamics and cloud microphysics hinge critically upon cloud updraft speeds, yet we have virtually no data on these for typical orographic situations. We need to know much more about the frequency and levels of occurrence of supercooled water in orographic clouds, for the ice-nucleation process operates only where supercooled water is found. Broader questions of storm water budgets and storm hydrometeorology call for very close attention. *Elliott and Hovind* (1964*a*) have found that much of the precipitation falling from cyclonic storms moving across the western orographic barriers in the United States originates in rather narrow convective bands separated by several tens of miles; and similar indications were reported by *Hall* (1957) for coastal storms in the Pacific Northwest. A study of hourly precipitation data gathered during the Santa Barbara randomized trials has led *Elliott* (1962) to the conclusion that the bulk of the seeding effect may occur within short time intervals of particularly high precipitation rate, presumably corresponding to passages of convective bands. *Elliott and Shaffer* (1962), and also *Smith* (1962), find rather strong evidence that variations in stability of the moist marine layer, out of which the coastal orographic stormclouds grow, play a dominant role in governing both natural precipitation and seedability. These are questions demanding much more study, as are detailed questions concerning trajectories and turbulent diffusion patterns of seeding plumes released in orographic situations. In addition to the need for better answers to such cloud-microphysical and cloud-dynamical questions, there is urgent need for climatological and hydrometeorological studies bearing on orographic seeding. We need much better information on precipitation climatology (correlation distances, data on variability, analyses of secular trends, analyses of storm types and their historic frequencies) and on storm hydrometeorology (data on percentage rainout of condensate for various storm types and barriers, analyses of precipitation rates and relation to storm types, and synoptic-scale parameters, precipitation and runoff relationship, investigations bearing on seeding operations and analyses). The basic problem of measurement of orographic precipitation is by no means satisfactorily solved. Very large differences in raingauge catch are known to occur over astonishingly small distances in mountainous terrain (e.g., *Court*, 1960; *Hovind*, 1965). Sampling errors are uncomfortably magni-

fied by such small-scale spatial variability, so great care is required in gauge-site selection in all orographic projects.

Many of the above problems are equally important in relation to nonorographic seeding, but many are peculiarly influenced by orographic factors and must be studied in that specific context. It is to be hoped that equally vigorous research will be carried out on similar problems in various orographic regimes elsewhere in the world. Transfer of knowledge and experience concerning seeding methods gained in one area to some other portion of the globe can be done on a rational basis only if we secure considerably more information on similarities and differences as to storm structure and hydrometeorology for various geographical regions.

In summary, recent experimental evidence suggests that stimulating increased precipitation in certain orographic cloud systems is one of the most promising of the presently available applications of cloud modification. But, in order to move out of the present empirical and intuitive stage of orographic cloud modification, and to attain a sophisticated level of physical understanding of the cloud and precipitation processes involved, we shall need substantial developments in both scientific knowledge and engineering technology.

Cumulus Clouds

In the present context, we shall understand "cumulus clouds" to mean convective clouds existing in relative isolation, as distinguished from (a) convective cells embedded in cyclonic systems or (b) convective cells stimulated by extensive orographic lifting of broad currents of air. There is, in fact, no clear-cut line of demarcation between cumulus clouds, so defined, and cumuliform clouds formed under other circumstances; so the artificial distinction that we use will become rather blurred at times.

The natural precipitation efficiency (fraction of condensate that reaches the ground as precipitation) tends to be quite low for isolated convective clouds over flat terrain. Such cumuli do not enjoy the advantages that accrue to deep cumuliform elements embedded in widespread areas of cloud—advantages that result chiefly from being completely surrounded by saturated air. In such wet environments, entrainment of ambient air does not exact the heavy price levied by turbulent intermixing of the relatively dry environments through which isolated cumuli must penetrate. Hence, one finds that even the largest cumuli of the class we are considering here—those that reach the thunderstorm stage—exhibit precipitation efficiencies of the order of only 10 percent (*Braham*, 1952). By contrast, orographic systems may have efficiencies about two or three times larger (*Elliott and Hovind*, 1964*b*), while extratropical cyclonic

systems may exhibit over-all efficiencies about four to eight times larger than those of the thunderstorm (*Wexler and Atlas*, 1958). It is to be emphasized that the concept of "precipitation efficiency" is intrinsically difficult to define in universally applicable terms, and, in storm systems as complex as extratropical cyclones, it is quite difficult to define precisely and quantitatively what amounts of vapor undergo condensation within the small-scale elements of the entire storm.

The important question, then, is whether isolated cumuli constitute promising targets for artificial nucleation by virtue of their comparatively low natural precipitation efficiency. An optimistic view might be that, starting from such a low initial efficiency, one may expect rather good chances of producing, say, a 10 or 20 percent relative increase in precipitation release rate in these clouds. On the other hand, a pessimistic view would be that such clouds suffer so many adverse environmental interactions that there is only marginal hope of stimulating appreciable increases in precipitation. The fact is that, today, we simply do not know enough about the microphysics and dynamics of this class of clouds to permit reliable *a priori* judgments. Hence, the sensible course of action is to carry out nucleation experiments at the same time that we seek to increase our basic knowledge about such clouds, watching carefully all the while for clues as to vulnerable spots that may be attacked in an effort to achieve useful modification of the water budgets and the dynamics of these cumuli.

In examining seeding potentialities in the general class of isolated cumuli growing over low-lying terrain, we might try to subdivide the entire class into several subclasses on the basis of some presumed differences in seedability. Neat subdivision proves difficult at the present stage of understanding; nevertheless, it seems moderately useful to try to consider maritime and continental cumuli separately, following a distinction that has been stressed particularly by *Squires* (1952) and *Twomey* (1959*a*). There is evidence (e.g., *Squires and Twomey*, 1960; *Battan and Reitan,* 1957) that cloud-droplet concentrations in most maritime cumuli run in the neighborhood of 50/cc, seldom being over 100/cc, whereas, in cumuli growing over continental interiors in air having a long overland trajectory, droplet concentrations usually run from several hundred to as many as a thousand per cubic centimeter, with typical values of about 400/cc. The reason for this characteristic difference is chiefly related to certain differences in the populations of condensation nuclei found over sea and land, though differences in typical cloud updraft speeds may play a minor role here (*Twomey*, 1959*b*). If we take 50 and 400/cc as representative droplet concentrations, and note that cloud liquid-water contents are not greatly different for maritime and continental cumuli (at least for those of similar size and cloud-base temperature), then we see

that the average droplet radius must be about twice as great in maritime as in continental cumuli. This implies that in maritime cumuli the coalescence growth process of large precipitation particles can probably proceed rather rapidly due to a high proportion of initially large droplets; whereas in cumuli containing a continental-type condensation-nucleus population, the coalescence process alone would have to operate for a much longer time to result in precipitation. This conceptual picture appears to fit the known facts fairly well; but it must not be regarded as definitive. On the basis of this tentative picture, one might conclude that ice nucleants probably offer little potentiality for stimulating precipitation in small maritime cumuli of the "tradewind cumulus" class, for those can, and evidently do, rapidly develop precipitation by the coalescence process. On the other hand, the same conceptual picture would suggest that cumuli growing on continental-type condensation-nucleus distributions might be artificially modified if one could accelerate particle growth by stimulating, by seeding with silver iodide or some other ice-nucleating agent, the Bergeron process of diffusional growth of ice crystals. The latter assumes, of course, that (1) we are talking about continental cumuli with tops colder than 0°C, and (2) that natural ice-forming nuclei are so deficient that a substantial portion of the cloud water exists in the supercooled state.

This rationale is helpful as a first rough guide; but closer inspection of assumptions and a broader survey of existing observations on relevant parameters disclose discrepancies in this neat picture. The present summary will not be served by attempting further elaboration. Rather, the above will suffice to sketch the kind of crude inferences we can now make as to seeding potentialities, and a few further provisos will have to serve as warnings against extrapolating the above inferences too far. (1) It is possible that "continental" nuclei may develop during rather short over-land passages of maritime air. (2) There is danger in overemphasizing the high rainout rate and the consequent low ice-nucleant seedability of tradewind cumuli, for we still know so little about global patterns of condensation nuclei, and about their mode of formation, that even by restricting our attention to small cumuli we may underestimate the seeding potential of maritime cumuli as a whole. (3) Cumulus clouds do develop over the oceans and grow to great heights, even in the tropics, and frequently appear to contain substantial amounts of supercooled water. For all these and other reasons, a maritime-continental dichotomy is probably only a rough first approximation to a rational scheme for subdividing cumulus clouds on the basis of seeding potentiality. The problem has fascinating subtleties, and work is progressing on a number of fronts.

Turning to the empirical approach of cumulus seeding trials, let us

examine briefly where we stand today. In Australia, *Kraus and Squires* (1947) seeded cumulus clouds with dry ice and observed precipitation reaching the ground shortly after seeding. In the United States, Project Cirrus efforts of the General Electric Research Laboratory group in the late 1940's involved a number of seeding experiments on individual cumuli, sometimes yielding visual evidence of seeding effects on precipitation and cloud growth. Continental cumulus and stratocumulus clouds over southeastern Australia have been seeded internally by flying through them with airborne AgI generators (*Warner and Twomey,* 1956). Of 35 seeded clouds, 22 were observed to yield rain or drizzle within times averaging about 20 min. In all such experiments on individual cumuli, the investigator must guard against possibility of biased selection of clouds ripe for natural precipitation, so Warner and Twomey sought to select comparison clouds of similar size near their treated cumuli whenever possible. That possibility could be realized in only eight out of the 35 cases. In one of these eight, both the treated cloud and the control cloud dissipated. Of the remaining seven, the control cloud also rained in three cases, while in the other four the treated cloud rained but the comparison cloud dissipated. This experiment has been summarized in some detail to illustrate the serious difficulties one encounters in trying to secure adequate comparison cases in flight-seeding of this type, and to point out that, even when one patiently works to secure comparisons, the background of natural variability acts to obscure seeding effects. Without experimental designs incorporating comparison cases chosen in an unbiased manner, the danger of drawing false inferences is exceedingly great. For the latter reason it is now almost universally accepted that the difficult step of introducing randomized design into seeding experiments must be taken, regardless of cost in experimental time, effort, and dollars.

A randomized experiment involving dry-ice seeding of cumulus congestus clouds in the central United States was undertaken by the University of Chicago Cloud Physics Laboratory (*Braham et al.,* 1957). Seeding was carried out at flight levels at which ambient temperatures averaged about $-3°C$ (an acceptable temperature when using dry ice, which functions at any temperature colder than $0°C$), in the hope that early release of latent heat of fusion might add buoyancy in such a manner as to stimulate cloud growth and favor development of precipitation. The results did not indicate that this seeding produced detectable differences in the formation of radar precipitation echoes. *Squires and Smith* (1950), using similar techniques, concluded that positive results had been observed in about 50 percent of seeded clouds having tops warmer than $-7°C$, and in 100 percent of clouds colder than $-7°C$; but lack of ran-

domized design and comparison cases in the latter experiment precludes drawing firm conclusions.

Silver iodide seeding of populations of cumuli on a randomized basis has been attempted in a few instances. In Australia, several projects have involved at least some cumulus seeding (*Adderley*, 1961), but since all the Australian trials have been over high terrain, they are summarized in the preceding discussion of orographic seeding. In other trials, similar difficulty is encountered in properly categorizing the projects, since cumuliform elements may be seeded over orographic barriers in synoptic situations dominated by passing cyclonic disturbances! However, the University of Chicago's "Project Whitetop," carried out in southern Missouri during the five summers of 1960-64, is one in which the treated population fell rather clearly into the class of shower-producing cumulus clouds. The Whitetop studies incorporated both a carefully planned randomized seeding design and a variety of auxiliary physical measurements on important cloud and precipitation parameters (*Hoffer and Braham*, 1962; *Braham*, 1963, 1964; *Bourquard*, 1963; *Koenig*, 1963). Aircraft AgI seeding was done along a 30-mile arc about 45 miles upwind of a central ground radar site. A network of precipitation measurements, plus the radar data, are being used to assess the seeding effects. Although initial inspection of the data suggested no marked effects of seeding, more recent analyses of radar precipitation echoes (*Braham*, 1965) reveal some evidence for positive seeding effects (order of 5 to 10 percent increase of radar echo frequency) in the region lying just downwind of the seeding arc, changing over to negative effects of about the same order of magnitude beyond a downwind distance of around 40 to 50 miles, and returning to positive effects still farther downwind. Braham tentatively attributes the apparent downwind "echo-shadow" effect to changes in cloud-layer stability or other more subtle dynamical changes accompanying the upwind seeding effects. At present, he discounts the likelihood that any rainshadow effects could result from decreases of precipitable water vapor due to upwind rainout. Because further analysis of these results is still under way at the time of this writing, final conclusions cannot be drawn. However, on the basis of the preliminary results, it would appear that modest precipitation increases in summer cumuli in at least one portion of the continental interior of the United States can be stimulated by silver iodide seeding. It would also appear, from the same preliminary analysis, that downwind rainshadow effects may accompany the seeding; so a host of scientifically interesting and practically important questions are immediately triggered by these preliminary results. If these results are confirmed in subsequent analyses of the Whitetop data, then important evidence of two kinds will argue for further field trials: (1) seed-

ability of clouds of a type that have sometimes been regarded as unsuitable to seeding with ice nucleants; and (2) interesting evidence that seeding may, even with isolated cumuli, accomplish a spatial redistribution of precipitation that obviously demands study in utmost detail.

The Whitetop experiments raise intriguing questions not only with respect to the matters of seeding potentiality mentioned above, but also with respect to cloud microphysics. Quite unexpected evidence was found (*Koenig*, 1963; *Braham*, 1964) for the presence of natural ice crystals at surprisingly warm temperatures: about one third of all clouds with tops colder than −10°C were found to contain ice when penetrated by sampling aircraft, and in some instances ice was found at the startlingly warm temperature of −5°C. Ice-particle concentrations as high as 10,000 per cubic meter with cloud tops warmer than −10°C were sometimes observed (*Brown and Braham*, 1963; *Koenig*, 1963). Because concurrent measurements of natural ice-nucleus concentrations (*Bourquard*, 1963) showed no evidence that such high ice-particle concentrations could possibly grow by Bergeron-type diffusion on individually nucleated centers, the inference has been drawn (*Koenig*, 1963; *Braham*, 1964) that perhaps a very small fraction of large water drops undergo heterogeneous nucleation on especially effective nuclei, and that these drops produce large numbers of ice splinters (*Mason and Maybank*, 1960) upon freezing. That hypothesis seems, at present, to be the only means of accounting for the appearance of ice in the rather warm Whitetop clouds, but the investigators emphasize that not all details of the hypothesis fit together satisfactorily.

The Whitetop ice measurements and radar observations gave initial indications that ice processes were not dominant in the Missouri summer cumuli, and suggested (*Braham*, 1964) that these clouds might be "much less attractive for rain inducement by silver iodide seeding than we had previously believed." But the later analyses seem to show a small seeding effect, so perhaps this seeding action operates on that fraction of all clouds that happen not to develop ice naturally at warm temperatures (warmer than −10°C). The related question of whether a large or a small fraction of summer precipitation is contributed by the clouds that tend to develop ice at warm temperatures now needs further study. Also demanding careful study is the question of whether the strongest positive AgI seeding effects in the Missouri trials might have come from a relatively small fraction of the total cumuli characterized by particularly strong updrafts. The existence of hail in severe storms suggests that natural ice-nucleating processes are unable to eliminate all supercooled water; and AgI seeding can only produce modifications in the presence of supercooled water. That question is of obvious importance in assessing seeding prospects in such a geographical region.

Whether the characteristics found in the Whitetop cumuli are also found in other regions is not now known. The University of Chicago group has (in summer 1965) moved its field operations to northern Minnesota in a direct effort to examine just such differences. Preliminary results of their Minnesota observations suggest a number of marked cloud differences between summer cumuli there and those in Missouri. Several carefully executed comparison studies in other geographic areas are needed. It might be apposite to note that the fact that Project Whitetop confronts us with many presently unanswerable questions reveals the rudimentary state of our knowledge of many facets of cloud physics bearing on seeding prospects. Perhaps this very ignorance warrants the optimistic prediction that with better knowledge we may be able to improve substantially our abilities to produce useful modifications of cumuli. Certainly it argues the need for much more research on all the cloud parameters we seek to modify.

Turning next to cumulus clouds in the tropics, we find scattered experiments that have given indications of some positive effects under certain conditions (e.g., *Davies,* 1954; *Howell,* 1960). Evidence gathered in the Caribbean by the University of Chicago team suggests strongly that ice nucleants probably are of little effect in initiating precipitation in tradewind cumuli over the open oceans. They found (*Braham et al.,* 1957), that 100 percent of all over-water tradewind cumuli had precipitation echoes by the time their tops extended above approximately the 4 km level, a result that seems consistent with previously cited information on droplet populations that develop from the maritime condensation nuclei typical of that area. The cumuli growing over the island of Puerto Rico, on the other hand, exhibited distinctly less tendency to rain out with warm tops. Only about 40 percent of clouds with tops at approximately 4 km showed radar echoes indicative of the presence of precipitation-size particles. It is possible that dynamical factors are in control here rather than (or along with) a rapid transformation from maritime to continental nucleus populations. The Chicago group's observations would seem to suggest that ice-seedability of island cumuli in the tropics may be more promising than that of over-water tradewind cumuli. *Howell* (1960) has discussed briefly the question of the efficacy of using silver iodide to seed such island cumuli in the tropics, suggesting that buoyancy alteration may be the principal desideratum of such seeding.

Although ice nucleants may or may not be useful in the tropics, there appears to be satisfactory evidence that water-spray treatment can effect release of precipitation in certain tropical clouds. *Bowen* (1952) obtained some evidence of positive response to injection of spray drops into the bases of convective clouds in Australia, but lack of randomized design precluded firm deductions. *Braham et al.* (1957) seeded tropical

cumuli in the Caribbean by flying above the clouds and releasing large quantities of water from the bomb bay of an aircraft in order to permit large drops to fall through the updraft. Their randomized scheme disclosed significant evidence of artificial increase in the frequency of formation of radar precipitation echoes as a result of this technique. Their technique must be regarded as having more scientific than practical significance, since they found it necessary to seed at rates as high as 450 gallons of water per mile to produce observable effects, and the cost of transporting such masses of water to cloud-top altitude probably precludes favorable economic returns. More work must be done on the opposite type of water seeding, introduced by Bowen, in which smaller drops are introduced into cloud bases to make efficient use of the clouds' updrafts to take the drops through the growth cycle; less tonnage of water and lower flight altitudes are thus made possible.

Similarly, better experimentation is needed for seeding warm cumuli with large salt particles. If the natural condensation-nucleus distribution is deficient in giant nuclei of the type that may be important in initiating the coalescence process, one can show that seeding with dry salt nuclei might produce large cloud droplets using only about one tenth the weight of seeding materials required for drop-spray seeding under cloud bases. A few experiments involving both dry-salt seeding and salt-solution-spray seeding from the ground have been carried out (e.g., *Fournier d'Albe,* 1957), with indications of modest positive effects; but no consistent set of results has emerged. No well-designed randomized trials have been conducted anywhere in the world on the salt technique. In view of the appreciable evidence that coalescence processes are very important in natural precipitation in many parts of the world, and in view of the evidence suggesting that a small number of large drops may dominate in the coalescence process, it appears desirable that some well-designed randomized trials of both salt seeding and water-spray seeding techniques be undertaken. There is, of course, just as much need for elaborate auxiliary measurement of cloud parameters in such seeding trials as there is in ice-nucleating trials. This distinctly calls for a joint effort by a team of investigators. Here, as in many other demanding areas of research, one meets the need for better-integrated efforts by larger groups.

The question of whether ice nucleants can be used to release latent heat of fusion in cumuli containing supercooled droplets is discussed in another section of this report. It should be noted here that it is quite conceivable that such buoyancy enhancement could exert favorable influence on release of precipitation, though this influence still awaits adequate statistical documentation.

In summary, very interesting experimental evidence for usefully modifying cumulus clouds is coming from the Whitetop data, which suggest

not only the possibility of precipitation increases of the o
percent from silver iodide seeding, but also the premature
decreases may occur. These results must not distinctly differe
lated to other geographic regions or to other dependent
logical conditions. It is important to conduct similar care in
experimental trials in many other areas. The role of condent
in affecting coalescence processes demands far more invest
ticularly using the techniques for studying the supersaturat
that Twomey and others have developed in recent years.
seeding and salt seeding probably hold less promise of effect
than AgI seeding, yet they certainly deserve more scientific
than they have received to date. Tropical cumuli over land
susceptible to ice nucleation, and the limited evidence
suggest this. Here again, more basic information must
before any firm conclusions may be drawn.

Extratropical cyclonic cloud systems

Modification of precipitation processes in extratropical
interest on at least two grounds: (1) A fairly large f
precipitation falling in middle and high latitudes results
cal cyclones; thus there is direct interest in the possibili
increasing or at least redistributing their precipitation. (
storms depend in part on release of latent heat as one
and it may become possible to use cloud-modificati
modify the dynamics of these storms and thereby to n
to larger-scale weather modification. The second possil
difficulty that the total kinetic energy of a typical ext
(having a diameter of the order of 1,000 km) is very l
ergs (equivalent to several hundred megatons of TNT
the circulation or the path of such a huge dynamica
turbations introduced via disturbances of the precipit
thus require either cloud-seeding on a heroic scale
unknown trigger mechanisms or circulation instabilitie

In 1953–1954, a field experiment, Project Scud (S
ducted "to determine the effects of artificial cloud n
development in the east coastal region of the Unite
(1951, 1953) had suggested that latent heat of fusion
freezing of supercooled water within large systems t
effects on large storm systems. Project Scud was s
seeding could produce perceptible influences in that
life cycle regarded as likely to be most vulnerable
stage of cyclogenesis. A region of frequent cyclogen

seaboard was chosen for the trial; and two winters of seeding (with 17 silver iodide ground generators deployed from Florida to New York, augmented by dry-ice seeding from U.S. Navy aircraft) was devoted to a randomized test of this hypothesis. The results gave no indication of any effect on either the precipitation or the dynamical development of the treated storms. No other experiment has been performed in any part of the world to test the dynamical effects of seeding such large systems, and the early claims and hopes for large-scale influence from small-scale seeding operations are now viewed with reservation. Whether more massive seeding could lead to different conclusions cannot now be predicted.

Let us consider next what the present evidence indicates as to the likelihood of influencing precipitation of cyclones. One view of the outlook was well put by *Houghton* (1957), who stressed that the large size and relatively long duration of cyclones probably tends to raise their precipitation efficiency to values appreciably higher than the efficiencies of either orographic clouds or shower clouds. Houghton suggested that although cloud-seeding may not greatly change the total precipitation released by cyclones, it might permit beneficial redistribution of precipitation (in time or space, or both).

Houghton's view that over-all precipitation efficiency is rather high in cyclones is shared by others. *Wexler and Atlas* (1958), and *Wexler* (1960) have suggested that it may approach 100 percent in some cases, and similar suggestions of nearly 100 percent efficiency have been made by Ludlam, Mason, Sutcliffe, and others. However, no definitive aerological studies have ever been carried out to confirm these suspicions, and some question has been raised as to whether imbedded convective cells or the outer edges of cyclonic storms might be characterized by precipitation efficiency as low as that of isolated shower clouds. If the efficiencies of some precipitating elements of cyclones are often as low as 40–80 percent, it remains possible that localized seeding may do somewhat more than merely redistribute precipitation that would have fallen naturally at some other time or place. These are possibilities only. It is quite certain, however, that firm answers to such extremely important questions cannot be given without considerable increases over present-day knowledge about water-vapor budgets in extratropical cyclones.

Some recent experimental trials bearing on the question of seeding restricted portions of cyclonic storms in the absence of orographic factors illustrate the still-uncertain nature of field evidence. A 4-year aircraft-seeding experiment using silver iodide to seed cyclonic and frontal precipitation during the summer rainy season in Canada (*Godson et al.,* 1965) indicated no significant precipitation effect. The actual 4-year result was a 2 percent decrease, with a probability of about 0.8 that this was due solely to natural variability.

In recent trials dry-ice seeding of wintertime precipitating storm systems by aircraft in the Ukraine, *Leonov and Nerobeeva* (1965) report an average increase of approximately 10 to 15 percent over the small amounts of natural precipitation observed in adjacent control areas in this semiarid region. Seventeen seeding experiments were conducted, using dry-ice seeding rates of up to 500 g/km, released at temperature levels colder than −5°C. The Russian experiments gave indications of downwind rainshadow effects, that is, decreases of rainfall in localities downwind of the target area, whose length corresponded to approximately 40 to 60 min of drift of the seeded clouds. It is not possible to infer the thicknesses of the treated cloud systems, but this is probably a crucial factor in the rainshadow question, as is also the nature and intensity of vertical motion. The Ukrainian trials incorporated no statistical design other than a very simple target and control without use of any historic regression analysis or randomization scheme.

In Israel, randomized aircraft seeding of winter cyclonic storms has been going on for several years (*Gabriel,* 1965). After four and a half winters of seeding, a 15 percent increase is indicated (personal communication to the Panel from K. R. Gabriel). Further statistical analysis will be given in a forthcoming fourth annual report on the Israel experiments. A nonparametric test of significance on the pooled results of five classes of rainy days has indicated a significance level of about 0.12; other modes of analysis suggest still stronger significance of the increase. When analysis was confined to only the interior portions of the two crossover target areas, positive anomalies as large as 23 percent were found; but since this analysis was not part of the original design, its implications are not stressed by the investigators. The cloud types believed responsible for these positive effects are cumuliform elements within the passing cyclonic systems. Only weak orographic effects are present in the Israel experiment.

Of the five randomized seeding experiments that have been conducted in Australia, only the South Australia project near Adelaide involved the seeding of cyclonic storms in essentially nonorographic conditions (*Smith et al.,* 1963). This 3-year, randomized, AgI, aircraft seeding project showed an over-all decrease of precipitation of about 5 percent, but this decrease was not statistically significant (probability of chance occurrence about 0.5). It was felt that the clouds were dominated by coalescence processes, due, perhaps, to strong maritime influences.

It might be noted that the Japanese orographic seeding trials (*Isono,* 1961) that gave indications of 50 percent increases involved cloud systems in which the orographic effects were superimposed on cyclonic effects. The principal cloud types were nimbostratus decks in warm sectors or over warm fronts of passing wave cyclones.

The positive implications of the Russian, Israeli, and Japanese results just cited, even though partially offset by the negative implications of the Canadian and Australian results, provides a basis for some optimism with respect to seeding of cyclonic systems. Despite *a priori* reasons for doubting that large net increases can be achieved, redistributions may be possible. Why certain trials have yielded discernible increases whereas others have not remains a significant question. We need to learn more about the structure of cyclonic cloud systems and also to learn more about their water budgets. We need to examine more thoroughly the possibility that downwind rainshadow effects may accompany positive effects in restricted areas. Study should be directed toward the question of whether *any* seeding techniques might be found capable of suppressing precipitation in migratory cyclonic storms while they are still offshore where their precipitation serves no direct purpose. Perhaps some way might be found to induce temporary storage of water just before such storms come on land, with net benefit in the form of increased overland precipitation.

Clearly, the present scientific basis for predicting the seedability of cyclonic cloud systems over lowland areas is very limited, and even an empirical basis can scarcely be said to exist. Only when we have gained better understanding of water budgets, storm structure, and natural precipitation efficiency of this important class of storms will it be possible to make a sound assessment of ultimate seeding potentialities. Well-planned randomized trials coupled with a maximum of physical observations and meteorological analysis are needed as a first step toward better insights into this important problem.

OPERATIONAL EXPERIENCE IN STIMULATING PRECIPITATION

We have summarized, in the previous section, the *experimental* programs of cloud modification. Since the 1946 beginnings of modern cloud-modification activities, many *operational* programs have been carried out by commercial cloud-seeding operators for clients who sought additional precipitation on their agricultural lands, their forests, or their watersheds. The open scientific literature contains very little information on these operational programs; hence, many scientists working in related fields have had little opportunity to assess adequately the merits and the results of many of these programs. The total amount of cloud-seeding that has been carried out by such operational groups is, however, rather large.

Evaluation of operational seeding programs is seriously hampered by the fact that clients, with few exceptions, demand that the operators seed at every possible opportunity. Thus, it has never been feasible to incorpo-

rate randomized designs into these projects, and most statisticians have therefore despaired of extracting useful information from them. Without randomization, various subtle but significant sources of bias may enter, especially in the presence of the notoriously large natural variability of precipitation. In the course of the work of the Advisory Committee on Weather Control, *Thom* (1957b) used target-control regression analysis and attempted to derive some scientifically useful information from selected operational programs. For west coast orographic storms, he concluded that increases averaging 14 percent could be detected with statistical significance ($P=0.01$). Subsequently, Thom's conclusions were challenged (*Brownlee,* 1960; *Neyman and Scott,* 1961) on grounds of possible hidden bias resulting from seeder-selection of treated storms and possible hidden bias due to the "storm-types phenomenon." A recent short summary of certain aspects of this interesting controversy has been given by *Gilman et al.* (1965).

Hidden bias resulting from selection of storms by seeders can enter a nonrandomized test if the seeder's performance is rated only on the particular storms he selects for seeding. For, if he is a skillful forecaster, he may be able to choose as his seeded storms only those that tend to put more precipitation on the target area than on the control area. However, such a *modus operandi* is virtually never used in projects carried out by commercial cloud-seeding operators. Rather, the client rates the seeder on the evidence for increase in total precipitation in the target area over some contract period. The precise basis for estimating that increase has differed widely over the past two decades, but, by and large, the target-control regression method has come to be the principal tool in estimating such increases.

Of the objections so far raised against the use of regression analysis in seeding evaluations, the most plausible concerns a possible "storm-types" error. Suppose the historic regression sample of data for target and control area is based on a period of years in which general circulation patterns produce a dominance of one type of storm that produces a characteristic bias towards, say, low precipitation in the target area and high precipitation in the control area. Suppose, further, that near the beginning of the operational project a shift in the general circulation results in a new pattern of storm types that characteristically tends to favor high amounts in the target area and low in the control area. Under such hypothetical conditions, regression analysis would indicate an apparent positive effect of seeding even in absence of any real effect. This is the storm-types difficulty (*Jeeves et al.,* 1954; *Neyman and Scott,* 1961). *Thom* (1957b) discusses the question in some detail and indicates that a search for marked effects of this type of error in his study did not disclose any. It should be remembered, also, that the storm-types error can work in both

directions. No further analyses of this problem appear to have been carried out subsequently, which is regrettable since it is of basic interest in seeding-evaluation methodology and is also of intrinsic synoptic-climatological interest.

With regard to the operational programs whose results we have studied, there is no question of the first type of bias (seeder-selection bias), since none of the projects was based on contractual arrangements in which the seeder had only to show increases in storms of his own selection. Rather, the total precipitation during one or more months or a season formed the basis of a client's estimate of the seeder's effectiveness; hence the seeder had nothing to gain by seeding only those storms that he might have thought to be storms naturally favoring the client's target area. The second main type of bias, the storm-types error, may enter subtly into individual projects, but we feel that the rather large variety of separate projects (18 projects) and separate geographical areas that are involved (New England and middle Atlantic states, Sierras, Cascades, and Great Basin) tends to reduce the likelihood that the storm-types objection is critically important. There appears to be only slight chance that shifts of storm-types can be the sole factor influencing patterns of general circulation changes that might account for all or for the bulk of the observed seeding effects in these operational projects.

Each of the seeding projects that we analyzed was conducted as a commercial operation, not as a scientific experiment; therefore, certain privileged information was involved. This circumstance precludes full reporting here. We selected for analysis 14 short-term projects conducted in the eastern United States and four long-duration winter orographic projects in the mountain regions of the western United States. All statistical analyses were done with data extracted by us from original data compilations, to permit the cross-checking of all steps of the operators' own analyses and to permit a variety of other checks bearing on questions of possible bias. We must emphasize that we have concluded from this sampling of operators' reports that there is need for considerable improvement with respect to completeness of reporting of relevant statistical data, clarification of evaluation methods employed, and basic editorial checking of a broad variety of report-preparation errors that mark some of the operators' reports. However, when we had completed all our checking procedures, we concluded that the results of these projects strongly imply that statistically significant increases in precipitation, of the order of 10 percent, can result from seeding.

Table 1 shows, for the eastern projects, the locale, season, and year of operation, approximate duration, apparent percent increase over the amount expected on the basis of regression, and significance level. All the tabulated percentage increases and significance levels are those arrived

TABLE 1

Locale, Season, Year	Approx. Duration	Percent Increase [a]	Significance Level
(1) Pennsylvania, summer, 1954	1 mo	17	0.20
(2) Pennsylvania, summer, 1955	2 mo	33	0.07
(3) South Carolina, winter, 1957	2 mo	19	0.04
(4) New Hampshire, winter, 1957	2 mo	21	0.06
(5) Massachusetts, summer, 1957	1 mo	30	0.15
(6) Pennsylvania, fall and winter, 1957–58	5 mo	6	0.30
(7) New York, summer, 1962	1 mo	57	0.04
(8) Pennsylvania, fall and winter, 1963	3 mo	5	—
(9) Connecticut, summer, 1964	1 mo	29	0.17
(10) New York, summer, 1964	1 mo	37	0.12
(11) Maryland, summer, 1964	3 mo	14	0.22
(12) Massachusetts, fall, 1964	1 mo	8	0.34
(13) New Hampshire, fall, 1964	19 days	14	0.21
(14) New Jersey, fall and winter, 1964	3 mo	0	—

[a] Based on cube-root-transformed precipitation data.

at in our own check-evaluation. (The figures labeled "Percent Increase" are expressed in units of the cube-root-transformed variates. There are possibilities of both upward and downward bias inherent in computing treatment effects in units of a nonlinear transform of a basic variate, as Scott and other statisticians have noted. However, some separate runs based on the untransformed data for this same set of projects gave essentially similar over-all results, showing that the nonlinear bias is not serious, *in toto*, here.)

The results of these fourteen short-duration ground-generator AgI seeding projects, totaling about 23 project-months of operations, cannot by themselves be regarded as conclusive evidence of the efficacy of seeding; yet, taken together, they seem to us to be a new indication of positive effects, warranting optimism.

Looking for an independent assessment of the above findings, the Panel asked the RAND Corporation and the Environmental Science Services Administration (ESSA) to undertake brief additional studies of the projects summarized in Table 1. It was felt that such additional scrutiny of those projects might disclose hidden sources of bias or other implications missed in the Panel's analyses. Detailed summaries of the RAND group's studies are contained in Appendixes 2 through 4; the ESSA study is reported on in Appendix 5. Here, only brief remarks on the highlights of those studies will be given.

Concern over possible bias associated with the storm-types phenomenon was expressed by the RAND group, initial attention being given to a

coarse stratification of the data on the basis of upper-wind directions. Although discernibly different monthly precipitation regressions were found to result from a breakdown into prevailingly southwesterly versus prevailingly northwesterly flow aloft, no basis for any systematic bias on this score could be recognized in the small sample of projects thus analyzed. The stratification analysis does suggest that stronger evaluation techniques than the simple target-control regression are possible, even when randomization is impractical. Specifically, climatological target-and-control-area relationships could be supplemented by synoptic-meteorological factors to increase the statistical predictability of target-area precipitation.

Little credence could be given to the apparent positive seeding effects if the seeded months were preceded and followed by non-seeded months in which the target area received precipitation excesses over regression comparable to excesses indicated for the seeded months. The results of a cursory analysis of this possibility by the RAND group tended to indicate the converse; that is, to support the reality of the seeding effects.

Since commercial cloud-seeders, or their clients, exercise some control over the duration of their seeding projects, and because their services might be solicited only after an abnormal series of dry months, two additional kinds of statistical bias are possible. For example, consider what we may call the "optional stopping-time bias." If there were a general tendency for seeding contracts to be terminated once some good rains fell on the target area, thus satisfying the client's need, then the seeded population would become a not-quite-random sample, with a possibility of positive bias. This is a matter of particular concern in judging the results of short-term "drought-alleviation" projects. RAND analyses of the probabilistic aspects of this source of possible bias, based again on monthly precipitation units, indicated that it might result in fictitious increases of the order of 5 to 10 percent, depending upon the precise model used and upon the stopping rule postulated. A bias of the order of 10 percent tends to result if the stopping rule is couched in terms of occurrence of positive departures from regression; but since the clients in these projects are not likely to be basing contractual decisions on any statistical basis as sophisticated as regression analysis, the estimate of 5 percent bias associated with stopping rules based only upon rainfall amount in the target (rather than residual from regression) seems more relevant. Whether termination of seeding was, in fact, frequently based upon receipt of heavy precipitation in the client's target area could not be ascertained through surveys within the scope of the Panel's efforts; but we feel that RAND's demonstration that optional stopping-time bias is only of the order of 5 percent may be taken as an indication that the

Table 1 results are not explainable solely in terms of that type of bias. For further details see Appendix 3.

Unusually dry conditions may often be associated with the starting time of operational seeding. Contracts may be written after a drought has already prevailed for a number of months, or even for several years, and such a selective criterion for starting may imply that the unstratified historic sample of past data may not be the proper sample for evaluating seeding effects. Because of such a possible "starting-time bias," it might be argued that one should be using a regression based only on some subsample of the total historic period, selected to match the circumstances governing the start-up of new seeding projects. No quantitative analyses were carried out on this question, but discussions of the matter both within the Panel and within the RAND group led to two conclusions: (1) that it could not now be shown to impose very serious bias on the results of Table 1; and (2) that the kind of climatological analysis required to determine the reality of a starting-time bias is conceptually simple, yet has never been done in the important context of cloud-seeding evaluations.

Due to the fact that 6 of the 14 projects summarized in Table 1 were conducted in the latter half of 1964 within a fairly small geographic area (northeastern United States), the RAND meteorologists and statisticians felt it very important to examine the extent to which those six projects were independent in the statistical sense. One means of getting at that question is to study the historical correlation among the *residuals* from regression for pairs of the nearby target-control-area combinations involved in the concurrent seeding projects. The fact that several such sets of paired residuals exhibited correlation coefficients as large as 0.5 to 0.6 does indeed argue caution and certainly reveals that enough interdependence exists among some of those six 1964 projects that one cannot treat their results as wholly independent in the statistical sense.

Special attention must be given the last point when judging Table 1 as a whole. What significance can be attached to the lumped results of that table? A. Madansky of RAND looked into this important question and obtained results set out in detail in Appendix 4. When the eight projects that he reevaluated, which included five nearby projects in late 1964, are lumped together (ignoring, for the moment, the knowledge that they are not truly independent results), the Fisher method of combining the significance probabilities yielded a value of 0.05. When the partial dependence was taken into consideration, using a covariance matrix method, an improved estimate of the over-all significance of those eight projects resulted in the value 0.16.

Examination of other aspects of the Table 1 results was carried out by

TABLE 2

Locale	Duration, Years	Percent Increase	Significance Level
San Joaquin River, California	14	7	0.04
Kings River, California	10	6	0.04
Bear River, Utah	9	14	0.01
Rogue River, Oregon	8	18	0.002

G. W. Brier and D. B. Kline of ESSA (Appendix 5). In an exploratory analysis of 16 operations over 11 areas (62 months), they undertook to determine whether any downwind rainshadow zones could be detected. No evidence was found of consistent or regular negative anomalies over distances up to approximately 150 miles beyond the target. Over all, the evidence could be construed as indicating the opposite; that is, as indicating a consistent tendency for positive anomalies during the seeded months. The investigators stress that since these data were not from scientifically designed or randomized experiments, a more conclusive statement on the causal relationships between seeding operations and precipitation could not be made. To the point of our more immediate concern, in the opinion of the Panel the ESSA analysis of the eastern United States projects tends to confirm the reality of positive seeding effects in that group of projects. We stress that we are unable to understand how seeding effects could appear so far downwind of the AgI generators. Both photolytic decay and simple dispersion of the AgI nuclei would seem to preclude such remote effects.

Extended operational programs are found mainly in mountainous areas of the western United States, where silver iodide ground generators have been used in the same areas continuously for many seasons to seed winter orographic storms with the goal of increasing winter snowpack. We checked four such projects, totaling 41 projects-seasons. In all evaluations the variates were seasonal watershed runoff values, obtained from U.S. Geological Survey records, with regressions based on nearby control watersheds for which runoff values correlated strongly (order of $r = 0.9$) with the target runoff. Watershed runoff as a test variate has both advantages and disadvantages. Although a watershed represents an enormous raingauge, it has its own peculiarities and sources of error, such as sensitivity to watershed-management practices that may change with time in such a manner as to yield effects that could be confounded with precipitation-enhancement effects. Table 2 summarizes the Panel's independent evaluations of four orographic projects in which runoff was employed as the test variate. Project durations range from 8 to 14 years.

The duration-weighted average runoff increase of these four projects is about 12 percent, and it is apparent that their combined significance level is such that natural variability is not a likely explanation for these increases.

Despite the impossibility of rigorous quantitative evaluation of all these operational results, when taken together and supported by the tentative results of some experimental projects, they appear to us to suggest that precipitation increases of the order of 10 percent may be stimulated, under certain meteorological conditions, by silver iodide seeding.

In every attempt to draw firm conclusions from operational seeding experience, one is confronted by the many unanswered questions of possible bias. Most of the questions arise directly from the use of historical target-versus-control regression lines as the standard to which the seeded period results are compared. First, what bias could be imposed by arbitrary selection of the historical regression period? Might the seeder be benefiting from a change in storm-types from those that characterized the historical regression—either fortuitously, or selectively by virtue of drought conditions having prevailed for several months or years? How often does seeding cease upon receipt of sufficient rainfall, thereby increasing the apparent probability of success? Can results be biased appreciably by indefinite specification of target areas, by *a posteriori* selection of rain or streamflow gauges, or by arbitrary area-weighting of raingauge readings? These questions are most cogent with respect to short-duration, opportunistic (drought-alleviation) projects, but some of them apply to *all nonrandomized projects* even of many years' duration.

Nonrandomized operational programs obviously are not an ideal source of evidence for or against the efficacy of cloud seeding. Nevertheless, we maintain that the operational indications cited above, plus the results of a number of recent and past experimental projects discussed earlier in this report, argue strongly for more vigorous prosecution of research on all aspects of cloud modification. We see no single piece of cloud-modification work, no single project, that has marked any distinct turning point; but the cumulative body of information now at hand argues the conclusion that cloud-modification techniques may offer more promise in enhancement of precipitation than has been evident in the past. This conclusion is still tentative enough that we strongly urge randomized trials of precipitation-enhancement techniques, carefully planned to secure pertinent auxiliary information on the many cloud-physics and cloud-dynamics questions that we have stressed.

Great improvement is needed in the procedures for the detailed reporting of operational seeding programs. The considerable effort to secure an adequate sample of operational reports for the analyses we have summarized above was due to the fact that the operators have not made

their results available through the open scientific literature. Some have indicated that their clients preferred that they not disclose any measures of seeding effectiveness. Inasmuch as atmospheric water vapor constitutes a natural resource in the public domain, steps must be taken to assure complete reporting of all cloud-seeding programs, both experimental and operational. Perhaps federal funds should be made available to support more complete reporting than is now customary. More detailed registry and regulation of operational programs are required. This requirement will become urgent if further experimental trials show still more conclusively that seeding offers economically significant means of enhancing precipitation. Neither the regulatory nor the economic issues lie within our Panel's purview, so we urge that suitable studies concerning these implications of the growing evidence for efficacy of cloud modification be made by other groups.

HAIL MITIGATION

The nature of hailstorms

Hail formation is intimately linked to the dynamical organization and life cycle of the hail-producing cumulonimbus clouds. The pertinent parameters are still not well understood and are just now emerging from rather vague concepts of the life history of hailstorms. Rather comprehensive review articles are available, and it is relevant to call attention to some of them: *Ludlam* (1958, 1959); *Douglas and Hitschfeld* (1959); *Donaldson et al.* (1960). The discovery of the influence of an upper jet stream on the storm dynamics (*Fawbush and Miller,* 1953; *Dessens,* 1960b; *Sourbier and Gentry,* 1961), the numerous studies of the low-level and high-level fields of divergence and convergence (*Fujita and Byers,* 1962; *Newton and Newton,* 1959; *Newton and Fankhauser,* 1964), and studies of hail-damage areas (*Stout et al.,* 1960) have all added substantial insight into the dynamics of these storms. What evolves, basically, is the concept of a hail factory in the form of a huge updraft that may sometimes become several kilometers wide.

Efforts to study the structure of hailstones in order to learn more about their life history have met with only limited success, due mainly to the need to store the stones in refrigerators prior to analysis. In so doing, they become frozen solid, while in nature they may contain much liquid water incorporated in the ice structure either as "spongy hail" or as snow slush. Therefore, the analysis of their structure using polarized light, microphotography, or Formvar replication is compromised by many unknown factors. These factors include aerodynamic uncertainties, since

soft stones may have a different drag than solid ones. Among the parameters being used to study the life history of a hailstone are the shape, the type of ice (clear or opaque), the structure and orientation of enclosed air bubbles, the orientation and size of crystallites when viewed in polarized light, and the growth boundaries. Usually one may recognize a few gross features. A center kernel is often conical in form, indicating its origin as a "snow pellet," which, in turn, may have its origin on either a large frozen cloud droplet or an ice crystal. The structure around this particle may change abruptly into another growth regime, frequently characterized by small crystallites. This may be due to individual crystallization of the accreted cloud droplets and thus indicate growth in a very cold environment. A subsequent structural layer may comprise larger crystallites, indicating growth during descent into a warmer environment where crystal growth was slower and permitted fusion prior to complete solidification. The larger crystallites may also indicate an increase of liquid-water content without temperature change, a condition in which the heat of fusion cannot be transferred fast enough and coalescence occurs prior to complete crystallization. It is difficult to explain large hailstones that do not have distinct growth boundaries; apparently, these stones grow in a relatively uniform cloud environment. Among the possible explanations for such homogeneous growth are: (1) that they are permitted to remain in the same part of the cloud during their entire growth phase; (2) that the cloud's thermal and hydrometeoric structure is uniform over a large vertical extent; and (3) that, while it is usually assumed that hailstones grow in supercooled water clouds, a particularly fast growth is possible in a mixed cloud near the glaciation level where the liquid water just acts as a binder.

What distinguishes a hailstorm from a normal thunderstorm? A study of thunderstorms indicates that three conditions must be fulfilled for a hailstorm: (1) It must have sufficiently high updraft velocities to support the stones during their growth. (2) Liquid water must accumulate in a supercooled state in the upper parts of the storm. (3) The updrafts must be persistent in order to permit the stones a sufficiently long time to grow in size.

It is difficult to compute updraft velocities, because of the unknown parameters of drag, friction, and the dimensions of the updraft; it is also difficult to measure such speeds during aircraft penetrations. From the flight recorder of an airliner that crashed in a thunderstorm, the updraft speed in the storm was computed to be 55 m/sec. On the basis of modern convective theory such speeds can be explained only if the updraft was several thousand meters wide and was enhanced by a ring-vortex circulation. Such high speeds are not required for hailstorms. The updrafts necessary for efficient formation of large damaging hailstones

must be greater than the fall velocities of large raindrops, that is, greater than roughly 13 m/sec at 10 km elevation.

For such updraft speeds, supercooled droplets will accumulate high in the cloud since all droplets smaller than raindrops are lifted, but none will return. If this process is sufficiently strong and persistent to accumulate 15 g of supercooled water per cubic meter, hailstones larger than 1-in. diameter could grow in about 10 min from initial drizzle droplet size. There can be no question that such conditions are extreme, since the weight of 15 g of water represents a great drain on the available buoyancy; yet hailstones much larger than 1-in. diameter occur whose formation cannot be explained without the assumption of either greater water storage or greater storm persistence.

Factors affecting updraft speed and supercooled water content seem fairly well understood. Factors affecting the persistence of a storm have not been thoroughly investigated. For instance, in cases where an upper-level jet stream is present at the proper altitude and proper speed, storms tend to be longer lasting. It is postulated that the jet stream carries the top of the storm away from the body of the cloud, thus preventing downdrafts from developing due to the accumulation of liquid water. Such a convective storm may assume a semisteady state: moist air is entrained at the base and exhausted at the top after releasing heat of condensation used to drive the storm engine. As the storm gets organized, other factors may enter to make it both persistent and more intense. Another possible cause of updraft longevity is the formation of the ice-crystal "anvil top." Glaciation liberates heat of fusion, which adds to the buoyancy and leads to an intensification of the updraft velocity aloft. Simultaneously, the cloud base lowers due to increased moisture in the entrained air, which may pass through the storm's own precipitation area. This lowering of the cloud base increases the instability and consequently the updraft speed. The updraft may persist or may continuously regenerate for over 1 hr, and it may accumulate liquid water of the order of 20 g/m^3 in its upper levels over a cloud depth of perhaps 1,000 to 2,000 m. Almost any size of hailstone can develop in such a storm. The storms just described could be called the continuous hail producers (*Donaldson et al.*, 1960), as they may produce hail "roads" of 100 miles and longer. They are the most intense of an extensive variety of hail-producing storms. The smaller storms are no less complex.

Hail usually falls along the "leading edge" of the storm, where the precipitation is heaviest. It is often difficult, however, to determine which edge of a storm is the leading one. There are storms that grow from "behind" with respect to their migration direction, and for these the "trailing edge" would be the "leading edge." Others grow from the side, usually from the right side with respect to their migration direction,

and their heaviest precipitation is discharged along the right side. This storm type is typical of tornadoes. Others have their leading edge out front. In determining the leading edge, radar can be a powerful tool since the radar echo frequently has very sharp boundaries along the leading edge of the storm and somewhat fuzzy boundaries elsewhere. Furthermore, contour radar mapping permits identification of regions with maximum precipitation. There appears to be little question that we can not only determine but also forecast within due time, for certain storm types, where the heaviest precipitation and hail will fall. It is more difficult to determine, let alone predict, just when and where hail will develop within a cloud.

Among all hail-prevention projects, only the Russian approach is based on the prediction of the precise hail-producing portion of the cloud. The prediction method is based on the concept that within a cloud a great accumulation of liquid water in a supercooled state is required. Through observations of radar targets in hailstorms, updraft speeds of 20 to 30 m/sec have been measured, and updraft distribution in the cloud has been observed to be maximum in middle levels and decrease upward. This distribution is favorable to the storage of large amounts of supercooled water in the vicinity of the maximum updraft. By identifying such regions of extremely high supercooled water content with radar, Russian scientists claim 97 percent accuracy in detecting those clouds that are actively producing hailstones.

Hail-suppression concepts and experiments

The present approach to hail modification is to add freezing nuclei so as to produce more minute ice particles, and thus to promote the growth of more hailstones of smaller size than would naturally occur. *Ludlam* (1958) and *Appleman* (1959), however, have suggested that hailstones evolve from large liquid droplets that form by coalescence in the warm part of the updraft. If this were the only hailstone-originating mechanism, seeding with AgI would not add additional hail embryos; it is difficult to say what the effects of seeding would be. Also, *Weickmann* (1953) has called attention to the fact that the continuous rate of condensation in a hailstorm could be so great that it would be possible artificially to increase the number of hailstones without reducing their size. For zero rate of condensation (*Iribarne and de Pena*, 1962), the concept of hail suppression seems much more tenable. From considerations such as the above, it is not completely clear why the total precipitation should increase in hail-prevention projects (e.g., *Schleusener*, 1962; *Dessens*, 1963), while the hail size or amount decreases.

Another suggestion for hail suppression is to overseed as much as pos-

sible of the supercooled part of the cloud in order to prevent the growth of hail by accretion of supercooled droplets (*Ludlam*, 1958; *Weickmann*, 1953, 1964). Ludlam suggests the formation of 100 ice nuclei per cubic centimeter, while Weickmann suggests one ice nucleus per cubic centimeter. The overseeding method is by no means certain, either, since it is quite possible that large hail can form as long as some supercooled water (perhaps a very small amount) remains and acts as a binder for accreted ice crystals.

If the suggestion of Appleman and Ludlam is correct (that hailstones grow from large liquid drops that have formed in the lower part of the cloud), then it appears possible that by adding great amounts of condensation nuclei at the base (*Weickmann*, 1964) the growth of large droplets due to coalescence could be retarded sufficiently to fully exploit the effects of overseeding with ice nuclei in the supercooled region of the cloud.

In all the hail-suppression activity around the world, the greatest enthusiasm emanates from Russia. As reported by *Battan* (1965), an apparently large program in hail-suppression research is under way in the Caucasus and Transcaucasus. Their approach is to try to identify precisely the region in a convective cloud where hail is beginning to form, then to try to place silver iodide crystals in that spot by means of artillery shells. Their experimental design does not allow for adequate statistical evaluation. Nevertheless, empirical evidence has convinced the Russian experimenters that their technique is highly successful in reducing hail damage.

With the exception of the approach of Russian scientists, all other projects of hail suppression use an indirect method of silver iodide seeding from ground generators or else use small rockets that are dispatched against the storm in general. Projects are being (or have been) undertaken in the United States (*Schleusener*, 1962), France, Switzerland, Bavaria (*Muller*, 1964), Argentina, (*Grandoso and Iribarne*, 1961), and Africa. All these projects except the last one (but including the Russian project) are based upon the philosophy mentioned in the beginning; the attempt is to increase the number of hailstones in order to decrease their size. The method of increasing the number of hailstones is by seeding with silver iodide, i.e., by increasing the number of potential ice embryos. All these projects suffer from a common handicap, namely, that we do not know just how much we have to increase the number of freezing nuclei in order to get the desired result. In Table 3, we have listed the available operational data on these activities.

The interesting results of the five-year Argentine project were reported on by H. N. Grandoso at the First National Symposium on Hail Suppression, Dillon, Colorado, October 14–15, 1965. The randomization

TABLE 3. *1963 Hail Seeding Data*

	Target Area	Number of Generators	Target Area per Generator	Single Generator Output	1963 Total Output [a]
Switzerland	3,000 km²	23	130 km²	7.5 g/hr	80 kg
Bavaria	1,500 km²	30[b]	50 km²	12 g/hr	20 kg
France	27,000 km²	162	150 km²	27–93 g/hr	2,300 kg
Argentina	4,000 km²	100	40 km²	3.5 g/hr	—

[a] The artillery-shell technique used in Russia places 400–1,000 grams of silver iodide into each storm.

[b] In addition, about 72 rocket stations, 652 Italian type rockets.

scheme produced a total of 96 control (nonseeded) days and 102 seeded days. Taking all data together, the results are inconclusive: an indicated 10 percent decrease in hail damage at a 0.3 to 0.4 significance level. However, by stratifying the data according to whether or not a cold front was a feature of the test-area weather situation, much more significant and interesting results are obtained. For the cold-front category (27 control and 28 seeded days), an apparent hail-damage *decrease* of 70 percent due to seeding is significant at the 0.10 to 0.15 level. For the non-cold-front, or "air-mass," category (69 control and 74 seeded days), the results show an *increase* of 100 percent in hail damage on seeded days, with an associated significance level of 0.10. (In this study, the Mann-Whitney nonparametric test for significance was used.) The hail-damage parameter is essentially the product of hail area and intensity, derived from the reports of a dense network of observers throughout the test area.

In the Swiss project (*Saenger,* 1960), seeding was also governed by strict randomization: every day at 1630h a weather forecast for the following day was received. If there was a possibility of thunderstorms, this day was considered a test day. A randomization experiment then determined whether seeding was to be done. Seeding always took place from 0730h to 2130h with the generators pulsed 10 min off and 5 min on. This seeding rate would result in a consumption of about 800 g of AgI per day. Hailfalls occurring between 0800h and 2200h were used for the statistical evaluation. This well-designed experiment continued following the death of the director, Prof. Dr. R. Saenger. Recent evaluations, after 7 years of operation, indicate a 68 percent greater frequency of hail-days in the seeded population than in the unseeded population, at a significance level of 0.04.

The French effort is under the scientific direction of Prof. H. Dessens. The over-all effort is well organized and apparently well recognized in the rural districts of France, since the number of generator stations has steadily increased. The stations are distributed over the southwestern

part of France where most of the damaging hailstorms occur, and Dessens makes the point that one of the most important conditions is a homogeneous distribution of generators over a large area. The project is non-randomized and has been conducted since 1951. The experimental data during this time period is, according to Dessens, not entirely homogeneous due to a fundamental change in procedure in 1959 and due to uncertainties in the boundaries of the target area. In the evaluation of the seeding effect, he computes an "effectiveness factor" that is defined as the ratio of paid hail insurance to total insured capital. This factor is given for each year since 1944 and compared with a mean value computed from the number of years starting with 1944 up to the year considered. This procedure is carried out for the experimental region as well as for the region outside of it. Dessens points out that hail damage in the target area since 1959 has been below average (except for 1963); and he sees success of the mission in this statistic. From these same data Weickmann has computed the ratios of the "effectiveness factors" for the target area to those for the external region, and this statistic indicates an almost continuous increase of the *relative* damage in the target area. There can be no question that both evaluations are subject to criticism: Dessens', because a mean hail-damage factor as defined by him lacks physical reality; and Weickmann's, for the same reason and also because target and control areas are not well defined.

The principal point to be made in summary of the projects cited is that, so far, with the possible exception of the Argentine and Swiss projects, none of them leads to conclusive results, and none of them disproves the possibility that hail damage might be increased as well as decreased by hailstorm seeding. Indeed, the Argentine trials seem to indicate that it might well be possible to increase hail damage. This may seem discouraging, but it should not cause pessimism as to the ultimate potential of hail suppression. The fact remains that the hailstorm is little understood; and the process of hailstone growth might well be delicate and localized in time and space. Therefore, the reported Russian and Argentine results should be taken seriously as indications that a seeding hypothesis based on a detailed hailstorm model and a better understanding of synoptic, hail-producing situations might very well produce success. The primary need is for a large, well-conceived research effort on the hail mechanism.

LIGHTNING SUPPRESSION AND ELECTRICAL MODIFICATION OF CLOUDS

Lightning is recognized as the outstanding cause of forest fires in the forested areas of the western and midwestern United States. In the region around Flagstaff, Arizona, for example, the number of reported

TABLE 4

Type of Discharge	Percent Less on Seeded Days than on Unseeded Days
Cloud-to-ground	38
Intracloud	6
Total	21

forest fires per season is about 400, of which only 10 percent can be attributed to human activities. In all the western United States, more than 10,000 lightning fires occur annually. These fires, aside from being a threat to human life and property, damage the resources of the forest including timber, forage, water, wildlife, and recreation. Also, with the increase of volume of commercial air traffic, lightning becomes an increasing hazard to aircraft. For these and other reasons, methods of lightning prevention or lightning evasion are being explored.

Presently, two similar approaches to lightning suppression are under investigation: (1) modification of the electrical structure of thunderstorms by means of silver iodide seeding, such as the work being carried out under "Project Skyfire" by the U.S. Forest Service; and (2) modification of a lightning discharge into a corona discharge by dispensing chaff dipoles into the thundercloud. The latter approach is being explored by the U.S. Army Atmospheric Sciences Laboratory, Fort Monmouth, New Jersey.

In the first method, silver iodide seeding is employed to produce an abundance of ice crystals in that supercooled part of the thundercloud in which most ground strokes are thought to originate, i.e., in the -10 to $-15°C$ region of the cloud. It is postulated that the artificial ice crystals furnish additional corona points, which increase the leakage current between the charge centers in the form of a corona current and consequently suppress the formation of a stepped leader. This physical concept is open to question when viewed in the light of theories of thunderstorm charge generation based on collisions between ice crystals and soft hail particles or on the freezing of water drops. According to these theories, an increase in the quantity of ice particles could be expected to increase the effectiveness of charge generation and consequently increase lightning activity.

Results obtained in Project Skyfire so far appear to be favorable. A two-year field study was conducted in Missoula, Montana, during the 1960 and 1961 seasons, using ground-based AgI generators having a combined output of about 10 kg of AgI per hour (*Fuquay and Baughman*, 1962). The results of the pilot project are shown in Table 4. The investigators emphasize: (1) that the results were not statistically significant

in their opinion, the significance level being 0.12; and (2) that the effect of seeding on fires was not evaluated, the implication here being that seeding might produce fewer but more intense ground strokes that could result in no decrease in the number of fires. Results of further Skyfire field trials are expected to be published early in 1966.

Statistics of the same kind were calculated in connection with airborne seeding experiments of cumulonimbus clouds in Arizona (*Battan and Kassander,* 1962). The Arizona results could not demonstrate that the over-all frequency of cloud-to-ground lightning was significantly affected by the seeding procedures employed.

The chaff-seeding experiment conducted by the U.S. Army is based on a similar concept, with the exception that corona discharge is initiated by chaff particles, following the corona-discharge theory of *Chapman* (1958). The corona-discharge current, initiated by 10^6 to 10^8 chaff dipoles 10 cm long, will amount to several amperes in a thunderstorm field of 600 V/cm. With such current intensities the storm could effectively be discharged. Tests so far have been limited to flights underneath the cloud bases where maximum fields of 1,000 V/cm were measured. Strong radio noise following the chaff seeding indicated corona activity. For actual field-testing, thunderstorm penetrations must be made with delivery of the chaff into the high electric fields inside the storm. The possibility that the chaff may quickly acquire an ice coating and thereby lose effectiveness should be tested as an integral part of the experiment.

An interesting set of questions arise concerning the possible effects of cloud-electrical modification on precipitation processes, since (in addition to effects of ice-nucleation by silver iodide) a sustained corona discharge may affect the coalescence efficiency of the cloud droplets. *Vonnegut and Moore* (1960) have proposed a possible precipitation process associated with lightning discharges in thunderclouds. Namely, it is possible that greatly increased droplet coalescence occurs in the high electric fields around lightning discharge channels, and that, therefore, rain gushes formed by sudden coalescence of cloud droplets follow lightning discharges. Sartor has demonstrated this basic effect in a laboratory cloud chamber, and later theoretically computed enhanced collision efficiencies due to electrostatic forces (*Sartor,* 1954, 1960). *Plumlee* (1964) has investigated the influence of a strong electric field on drop collision efficiencies and has found that horizontal electric fields produce larger collision efficiencies than do vertical fields. A horizontal electric field of 3,500 V/cm increases the collision efficiency of a 30- and 5-μ pair by a factor of 34.5, compared with a factor of 5.6 for a 40- and 5-μ pair and a factor of 5.0 for a 50- and 5-μ pair. It is known that R. Gunn measured a field of 3,500 V/cm in an aircraft shortly before a lightning discharge; thus, collision efficiencies as cited above may be very realistic in charged clouds.

Vonnegut and Moore (1958), *Moore et al.* (1962), and *Vonnegut et al.*

(1962) have studied the influence of ions released from a wire several kilometers long and charged to 30,000 V to produce corona. The ions became attached to aerosol particles and were carried downwind and up to cumulus clouds. No effect on coalescence was observed. *Fletcher* (1962) reports on experiments performed by Telford, who trailed a long wire from an aircraft while flying through a cloud. The wire was fed by high-voltage alternating current so that regions of positive and negative space charge separated by about 1m were produced along the flight path. The ions, it was hypothesized, should have become attached to the cloud droplets and, as a result of the opposite charges between neighboring regions, coalescence should have been accelerated. While the concept behind this experiment is interesting, positive results were not observed.

The effects of thunder on cloud and precipitation particles have recently been scrutinized by *Goyer* (1965*a*, 1965*b*). He considers the effects of the supersonic shock wave, which is emitted from the channel of a return stroke, on the processes of coalescence, nucleation, and drop breakup. Attention is called to the possible "pop-gun effect" (*Vonnegut,* 1948) of the expansion wave on cloud nucleation: the adiabatic cooling may cause homogeneous nucleation of the water droplets and form many ice crystals, which in turn may influence the development of precipitation. Goyer calls this process "shock seeding." It is likely that a number of processes caused by the electrical effects of lightning, as well as the mechanical effects of thunder, combine and cause the precipitation gushes that have repeatedly been observed to occur after a lightning discharge (*Vonnegut and Moore,* 1960).

The natural electrification process in convective clouds remains a mystery. It is possible that electric-charge distribution is a controlling influence not only, obviously, upon lightning, but also subtly upon precipitation and convective cloud dynamics. Vigorous research in cloud electrification has been urged in the past. The need is still urgent, mainly for the development of a more complete and tenable theory for the total cloud-electrification process. In the interim, all cumulus-cloud-seeding experiments, for whatever primary purpose, should provide, insofar as practical, for the detection of possible electrical effects of the seeding. Also, it is very clear that trials of cloud seeding to suppress lightning should be examined for possible effects upon precipitation.

MODIFICATION OF FOG AND STABLE CLOUD LAYERS

Dissipation of supercooled fog and stratus clouds

It was upon supercooled stratiform clouds that Schaefer, Langmuir, and Vonnegut first demonstrated the efficacy of cloud seeding with dry ice

and silver iodide. They found, as predicted by Bergeron, that ice crystals in such clouds or fogs will grow at the expense of water drops, and that if a great enough number of ice crystals is generated, all water drops will diffuse over to the ice crystals and the latter will fall out. Thus, it has been accepted for nearly two decades that this type of cloud could be dissipated by seeding, and that no important unsolved scientific problems were involved.

The physical basis of supercooled cloud dissipation was thoroughly studied by a research team of the U.S. Army Signal Corps during 1953-1955 within the framework of Artificial Cloud Nucleation (ACN) Project (*aufm Kampe et al.*, 1957). The investigators showed that the effectiveness of dry-ice seeding was quite insensitive to the rate at which dry ice was dispensed, and that a stable stratocumulus deck could be dissipated with a seeding rate of 1 lb of dry ice per mile just as well as with 10 or more lbs per mile. They observed that it required 15 to 50 min. for a cloud deck to break up, depending partly on its thickness. A seeded line would spread laterally at a speed of about 1 m/sec, apparently in agreement with the turbulent diffusion in the cloud layer. Sometimes snow would reach the ground, but usually it evaporated before reaching the ground. Under special conditions, not only light but also heavy snow showers were observed to develop. When the cloud deck was more convective in nature or when the temperature was warmer than $-5°C$, the cloud-forming process was quite as efficient as the cloud-destroying process, and no areal cloud dissipation occurred. Also, when seeding was done in early spring in March or April, the solar radiation was strong enough to start convection frequently as soon as the seeded area was cleared, and the clearing filled up quickly with cumulus clouds. Seeding using an airborne silver iodide generator was also successful, releasing the plume inside the cloud and at cloud-top level. A seeding rate of 16 g of AgI per mile was found to be equivalent to a rate of about 4 lbs of dry ice per mile.

Despite the impressive early experimental foundations, not until recently have there been any serious attempts in the United States to develop appropriate operational procedures and equipment for the obvious application—in clearing wintertime supercooled fogs from airports. By contrast, it is reported that supercooled fog-clearing has been operationally practiced in the U.S.S.R. since the late 1950's. *Beckwith* (1965) has reviewed a two-winter operational fog-dispersal program, using airborne dry-ice seeding, involving eight airports in the northwestern United States, and financed by six airlines. In summarizing the results, all cases in which airport visibility improved to ¾ mile or better following the seeding, and that were not accompanied by natural dissipation, were considered operational "successes." Ninety seeding flights were made, of which 74 (or

approximately 80 percent) were successful by these criteria. Beckwith concludes that further practical improvement is mainly a matter of developing ground-based seeding devices and refining aircraft dispensing systems and techniques. Based on a long-term research project at the Air Force Cambridge Research Laboratories, the U.S. Air Force is well along in perfecting its routine "Cloudbuster" system in which dry-ice pellets are produced directly from liquid CO_2 and dispensed by means of standard airborne equipment (*Vickers*, 1964). An operational procedure has also been worked out for Paris Airport (*Serpolay*, 1965) in which cooling by the expansion of propane gas is used for the production of ice crystals. Sixty propane-gas sprayers and their supply tanks are set up along the periphery of the air field at distances not less than 1.5 km upwind of the group of principal runways.

Dissipation of warm fog and stratus clouds

"Warm" (nonsupercooled) fog and stratus cloud decks are much more difficult to dissipate than supercooled clouds, for such warm clouds are thermodynamically stable. They contain no apparent instabilities subject to artificial release. There is little doubt, however, that troublesome warm fogs and stratus layers are much more widespread and frequent than the supercooled variety, and consequently there should be considerable economic justification for seeking to develop methods of warm-fog dispersal.

This particular quest has been going on for a long time, and probably comprises the most durable of all active scientific problems in weather modification. *Houghton and Radford* (1938), for example, recorded in detail their well-planned and successful project in dissipating small volumes of warm advection fog on Cape Cod by two techniques exploiting the extreme hygroscopicity of calcium chloride. (Incidentally, their report contains summaries of *prior* work in the field and of the various possible approaches to warm-fog dispersal; the latter in particular makes enlightening reading 27 years later.) The concept behind their experiments is, simply, that the removal of water vapor from the foggy air will cause the fog droplets to evaporate. They designed two systems to accomplish this: (1) an apparatus to spray a highly concentrated calcium-chloride solution into the fog as it was blowing by, taking care to control the drop-size of the spray so that the CaCl solution was as finely divided as possible, yet did not remain suspended and thus, perhaps, turn into a secondary fog; (2) the use of calcium-chloride powder as the active (and reclaimable) agent in a large dehumidifying unit from which relatively dry air would be blown and mixed into the fog. Houghton and Radford report that typical clearings using the spray technique

were 600 m long, 30 to 50 m wide, and at least 15 or 20 m high. While no full-scale dehumidifier was constructed, theoretical computations and model experiments predicted comparable success with a relatively small unit. Recently, Houghton (in a personal communication to the Panel) reiterated that the experiments did work and, further, that no adequate tests of this approach or similar ones have been made since 1938. He also emphasized some of the drawbacks, namely, that the technique is rather expensive and cumbersome and that corrosive material is involved.

Two approaches have been tried recently in attempting to precipitate fog in an electric field. In one experiment (*Vonnegut*, 1956), a highly charged oil fog was blown downwind by an aircraft propeller and was supposed to form the upper condenser plate, the lower plate being the earth's surface. Fog droplets in the strong field in between would, it was proposed, coalesce and precipitate due to the electrical charges on the droplets. The other approach was to have the fog drift across a highly charged wire in the hope of stimulating coalescence in the strong field and thereby to induce precipitation (*Hagen*, 1961). One of the serious side effects encountered in tests with both methods was that corona and arcing to ground occurred before the potential necessary for precipitation could be reached.

The efficiency of seeding with carbon-black, to promote evaporation of the warm fog or stratus by heating due to radiant absorption, was theoretically explored by *Fenn and Oser* (1962), and *Korb and Moeller* (1962). Fenn and Oser determined the optimum size of spherical carbon particles for maximum absorption of solar radiation and minimum infrared reradiation, and arrived at $0.1\,\mu$. Korb and Moeller computed the radiation budget of different types of clouds at the 1,000-m level (such as pure water cloud, water cloud with droplets containing a soot kernel, and pure water droplets and soot side by side). For cloud types with the extremely small water content of $0.01\,g/m^3$, 1,000-m thickness, and an assumed seeding rate of 40 kg of carbon-black per square kilometer, appreciable values for computed dissipation rates were only obtained for the last type of cloud and soot combination. The time calculated for evaporation of the cloud droplets was 10 min.

Water seeding of warm stratus clouds has been tested by Japanese scientists. This method is based strictly on mechanical collision and rain-out. In cases of a very thin cloud deck or fog, temporary clearing was obtained. The Japanese (*Hori*, 1953) have also investigated thoroughly the efficacy of forests in scavenging the particles of advection fogs that plague the coastal areas of temperate land masses adjacent to cold coastal waters.

It appears that the most reliable dispersal techniques for warm fog are still based upon massive, direct heating of the foggy air from ground

heat sources. Certainly the "FIDO" installations in England during World War II were operationally successful in spite of their excessive cost, largely because the expense of those oil-burning installations was easily justified as a military necessity. Postwar installations in the United States, at the Landing Aids Experiment Station in Arcata, California, and at Los Angeles International Airport, proved impractical for several reasons, including cost and the psychological effect on airline passengers. Two somewhat controversial and yet unproven modern versions of "FIDO" are: (1) heating by jet engine exhaust (*Downie and Smith*, 1958; *Dubois*, 1965); and (2) a proposal to burn anthracite coal instead of fuel oil to minimize the addition of water vapor to the air along with heat (*Myers and Hosler*, 1965). The latter report contains a very useful and up-to-date survey of past and present attempts to dissipate warm fog.

An almost inevitable conclusion to be drawn from the history of attempts to dissipate warm fog and stratus is that the problem has not, at least in the past 20 years, been taken quite as seriously as its practical potential warrants. Certainly there are a number of very practical civil and military problems in transportation and logistics that have persisted for decades because of this very common meteorological phenomenon. It is suggested that one should turn to the Houghton and Radford report of 1938, not necessarily to find a panacea for warm-fog dispersal, but definitely to find an example of the type of scientific and engineering planning necessary to conquer that problem, and, as well, many of the other problems that confront us in all phases of weather modification.

MODIFICATION OF CLOUD DYNAMICS

It is important not only to consider whether clouds of various types may be artificially modified with respect to microphysical factors influencing the growth of cloud and precipitation particles or development of electrification, but also whether any means can be found to influence directly or indirectly the dynamics and energetics of clouds. If such means can be discovered, an interesting avenue for study of cloud dynamics may be opened, and, conceivably, it may become possible to develop new trigger mechanisms for manipulating precipitation processes on the microphysical scale and air circulations on larger scales. Understandably, attention has been given to possible use of existing cloud nucleants as a first approach to this interesting goal of modification of cloud dynamics.

How might artificial cloud nucleants possibly be used to affect cloud dynamics? If vapor supersaturation were common within natural clouds, then addition of condensation nuclei (soluble salts or other hygroscopic

materials) would afford a possible technique for inducing release of the rather large latent heat of condensation of water vapor. However, it can be stated emphatically that one of the most firmly established facts of cloud physics, well supported by observation and theory, is that vapor supersaturation in natural clouds is rarely greater than about 1 percent, and almost certainly never in excess of a few percent. Furthermore, even these modest supersaturations are confined to the shallow activation zone just above the visible cloud base. The reason for the low percentages of vapor supersaturation in natural clouds is simply the abundance of natural condensation nuclei almost invariably present in the atmosphere. That abundance is so well documented that proposals to influence cloud dynamics through release of latent heat of condensation of super-saturated vapor are doomed to failure in all but extremely exceptional cases. (Very pure air of abnormally low nucleus content has been found in certain snow-covered areas in continental interiors, and affords opportunities for certain informative experiments that deserve scientific attention, but such cases do not afford obvious opportunities for producing marked modifications of cloud dynamics.)

Turning from consideration of the normally low degree of vapor super-saturation to the frequently strong degree of liquid-water supercooling, we find that possibilities of influencing cloud dynamics by triggering release of latent heat of fusion through introduction of artificial ice nuclei deserve serious and imaginative attention. Because of the frequent lack of effective natural ice nuclei, supercooling of all or part of natural clouds existing at temperatures below 0°C is known to be common, and this circumstance affords the entire basis for silver iodide and dry-ice seeding to modify precipitation. Here we examine the question of whether that same type of seeding might offer means of affecting cloud dynamics by the controlled release of latent heat of fusion. Such controlled release may permit some degree of controlled increase of cloud buoyancy at some critical stage or level of cloud growth. At present, it is this route that seems to offer interesting possibilities for modification of convective cloud dynamics.

Kraus and Squires (1947) were apparently the first to observe, in the course of some early seeding trials in Australia, an apparent surge of cloud growth following seeding. Some of the cumulus clouds that they seeded with dry ice were observed to grow upward in a spectacular manner. Kraus and Squires attributed these effects to buoyancy increases induced by release of latent heat of fusion, though lack of randomized design precludes any decision as to the chance that these were merely random natural growth changes. Isolated cases of similar effects have been reported by others, and *Langmuir* (1951) suggested that this process might offer means of influencing the dynamics of large-scale systems.

Seeding trials designed to test this hypothesis (*Spar*, 1957), however, gave negative results. Although one cannot rule out large-scale modification possibilities at some future time when our knowledge of many factors has improved, it appears wise to focus research attention on the greater likelihood that artificial ice-nucleation methods might permit influencing the dynamics of individual clouds.

Experiments designed to test the latter hypothesis are currently under way and, although only preliminary results are at hand, these experiments appear to hold some promise for cloud-dynamical modification. *Malkus and Simpson* (1964a) have described initial tests in which tropical cumuli were seeded with pyrotechnic devices, loaded with silver iodide, dropped from aircraft flying at high altitudes. Instances in which cloud growth was observed to occur shortly after treatment were frequent enough that a much more elaborate statistically randomized test, part of Project Stormfury, was planned for the summer of 1965. Only preliminary results are available on the 1965 trials at the time of this writing. However, the 1965 experimental design represents a laudable step forward toward incorporating adequate randomization features in a relatively difficult context.

Another related experiment, being conducted by workers at The Pennsylvania State University (*Davis et al.*, 1965), involves introduction of ice nucleants into cumuli that develop in flow disturbances over mountain ridges in central Pennsylvania. Here, again, interesting and promising effects appear to be present.

Although it is still too early to draw any firm conclusions about ultimate prospects for useful cloud-dynamical modification, we find it encouraging that such experimentation is under way. Further work is clearly justified, and may develop fruitful techniques of learning more about cloud dynamics and perhaps also some practical methods for artificial modification of motion fields on a scale an order of magnitude larger than that of the clouds themselves. This goal is of central importance in long-range efforts directed toward weather and climate modification, for the clouds may be our best trigger mechanism in trying to induce mesoscale and large-scale dynamical modifications. Equally important, as many workers have stressed, may be certain effects on the mechanisms of precipitation growth in cumulus clouds.

TORNADO SUPPRESSION

The steady growth of communities and attendant buildup of industry in the midwestern tornado belt has inevitably led to a steady increase in tornado deaths and tornado damage. Clearly, any practicable method

for reducing the frequency or severity of these most intense of all atmospheric storms would be of great significance. Present knowledge of tornado dynamics is still so primitive, however, that it must be stressed that there is at present no scientifically sound technique for altering appreciably the genesis of tornadoes. Predicting the development and behavior of individual tornadoes, a skill that will have to be well advanced before tornado control can be seriously considered, is at present highly empirical and dependent chiefly on extrapolative procedures applied to already-detected funnels. Here, as in so many other areas of weather modification, the close relationship between prediction and control argues that the principal need is for increased scientific study directed toward the prior goal of improved prediction.

The practical difficulties of tornado control can be put into perspective by examining rough estimates of certain relevant energies. The rate of generation of kinetic energy in a typical tornado funnel has been crudely but plausibly estimated by *Vonnegut* (1960) to be of the order of 10^{18} ergs/sec, or 10^8 kW. By way of comparison, the total electric-power-generating capacity of the United States today is about 3×10^8 kW. To look at it another way, 10^{18} ergs/sec is equivalent to detonating high explosives at an average rate of about 20 tons/sec. Both thermal and explosive techniques of destroying tornadoes have been loosely proposed in the past; the foregoing comparisons indicate the implausibility of such brute-force approaches.

Some evidence suggests that tornadoes develop under conditions characterized by a strong temperature inversion in the neighborhood of 800-mb (near 2 km altitude). This has tempted some persons to propose the use of fuel oil or electrical heaters to destroy the inversion by direct heating of the subinversion layer. *Newton* (1963) estimates the thermal-energy density required to eliminate the inversion to be about 450 cal/cm², equivalent to 2×10^{10} ergs/cm². Tornado-forecasting experience makes it clear that the inversion would have to be burned off over areas of many thousands of square kilometers to be fairly sure of suppressing tornadoes in a squall-line synoptic situation typical of tornado genesis. But let us consider an area of only 100 km² (about one midwestern township). Suppose electrical energy were used in some hypothesized array of resistance heaters covering this area. About 10^9 kW-hr of heat would have to be added to the atmosphere over that area to eliminate the inversion. This would be equivalent to diverting into the array of heaters all the nation's electrical generating capacity for a heating period of about 3 hr.

These energies, when expressed in the above manner, may seem so large as to raise doubt that they are compatible with other meteorological power densities. But they are not incompatible with the enormous

power densities typical of many ordinary atmospheric disturbances. *Braham* (1952) has estimated the total energy exchange in a typical thunderstorm cell to be of the order of 10^{22} ergs. A single tornado funnel may have a lifetime of about 15 min (*Battan*, 1959); so, if we accept Vonnegut's tornado power estimate of 10^{18} ergs/sec, the total energy expended in a single funnel during such a typical lifetime is perhaps 10^{21} ergs. Since this comes to only about 10 percent of Braham's estimate of total thunderstorm cell energy exchange, we see that all of these huge energies and powers are roughly comparable.

In summary, one must say that tornadoes are energetically far beyond direct control in the foreseeable future. However, improved understanding of the embryonic stages of tornado development, coupled with increases in understanding of the microphysics and dynamics of the convective clouds that spawn these destructive storms, could conceivably open presently unsuspected avenues to tornado modification. If, as has been proposed by at least one investigator (*Vonnegut*, 1960), lightning discharges play any important role in initiating tornado vortices, it is possible that some alteration of the electrical balance of the parent storms might delay or suppress tornadoes. Also, it is possible that improved knowledge of the life cycle of tornadoes might disclose some early stage at which a small perturbation of the convective circulation of the parent thunderstorm could forestall development of the funnel circulation. If such a vulnerable stage does exist, it might somehow be possible to use cloud-nucleation techniques to supply just the right convective perturbation to prevent funnel formation. All such hopeful speculations are today unfounded, but we know so little about tornado dynamics that we must not rule out the possibility that vigorous research in this area might lead to currently unsuspected vulnerabilities that we can turn to advantage in mitigating the destructiveness of these awesome storms.

HURRICANE MODIFICATION

Hurricane energetics

Despite the most strenuous and hazardous efforts by man to examine and comprehend it, the hurricane remains an unsolved problem. Not even the most successful aircraft reconnaissance and radar surveillance have yet come close to completely revealing the structure and dynamics of a hurricane from birth to death, nor are we able to forecast hurricane motion and intensity with satisfactory accuracy. Although hurricane research is being vigorously pursued in the United States, it is severely limited by our observational capabilities. Until we can observe the com-

plete life history and structure of a hurricane, including the structure of the disturbed ocean below the vortex and of the complete atmospheric environment of the storm, we cannot expect any significant advance in our understanding of the hurricane, or in our ability to predict or deliberately modify it.

The hurricane is a giant heat engine in which direct transfer of sensible and latent heat from sea to air is the fundamental energy source. Quantitative information on this heat flow under hurricane conditions is essential for an understanding of storm genesis, intensification, and decay, and is fundamental to any scheme for hurricane control. Such information has never been obtained by direct measurement. The ocean is a sink for atmospheric kinetic energy, which the hurricane transfers by wind stress to the sea, where it is manifested in waves and currents. In a steady hurricane a sensitive balance must exist between this frictional dissipation and the conversion of heat to kinetic energy.

To investigate the energy transfers between the sea and the air, it is necessary to probe directly into the disturbed boundary layer of the ocean below the hurricane, as well as into the atmospheric boundary layer. Since the heat stored in the surface waters is a primary source of energy for the storm, it is important to know whether the wind action mixes this warm water with colder water from below, or whether converging winds transport warm surface water into the storm core. We know that hurricanes tend to weaken over cold water and apparently are revitalized over warm water. Despite the obvious importance of the sea as an energy source for hurricanes, however, we know almost nothing about what goes on in the water under the hurricane. Possibly this is a soft and sensitive spot that might be used to modify or prevent these storms.

While the lower portion of the storm contains the heat source, the upper portion acts as a heat sink. In any thermodynamic engine the difference in the state of the working gas from heat source to heat sink is the main concern. Thus, we need to know more about the export of heat from the hurricane's *upper* levels into the general circulation of the atmosphere and also the role that long-wave radiation to space may play in the energy budget of the storm. Observations of the upper reaches of the storms by high-altitude aircraft and satellites can contribute to this knowledge.

Progress toward further understanding of the hurricane rests squarely on our ability to examine the presently inaccessible portions of the atmosphere. Hurricanes that approach the highly populated eastern and southern seaboards of the United States are under constant surveillance by aircraft and radar. However, they originate in regions of the tropical Atlantic in which the density of data is extremely low. The surface and

upper-air observations necessary to learn how hurricanes are born are almost nonexistent. Even ship reports are few and far between, and those received are concerned mainly with elementary surface observations. By the time a storm is off the coast, within the range of our reconnaissance aircraft, it is already a roaring monster. The TIROS satellites have given us some early views of young storms, but these have been limited to one look per day. One look per day serves fairly well for storm *location* only. Since a storm's internal motions have a much shorter time scale than this, one look per day is quite inadequate for learning how the storm works. Thus, even with the help of the present near-earth satellites, we simply do not have a set of hurricane observations that allows us to see its full life cycle.

Paradoxically, the midaltitude portions of a storm, those portions accessible to reconnaissance aircraft and radar, and hence the layers we know most about, turn out to be the least important in the energy-conversion process. However, this is the layer where most of the kinetic energy of the swirling air mass is stored. Just how this swirling mass responds to, and in turn disturbs, the general weather environment by feedback is of course a central forecasting problem. Here, too, a better understanding of the storm's energy budget is important to the goal of improving prediction of the storm's direction and rate of travel.

Hurricane-modification experiments

Simpson et al. (1963) advanced a hypothesis relating the effects of the heat of fusion released by seeding to alterations in the circulation dynamics of a storm. This hypothesis emphasizes the possibility of wind reduction in hurricanes rather than "steering" the entire storms. The authors postulate that added heat of fusion due to seeding would result in a modification of the hydrostatic-pressure field, leading to an upset in the balance of forces that would become manifested in a perceptible wind reduction and outward migration of the eyewall cloud.

Under Project Stormfury, two seeding experiments on actual hurricanes have been executed (on Esther in 1961, and on Beulah in 1963). High-output pyrotechnic silver iodide sources were dropped into portions of the eyewall cloud. Cloud behavior was observed by radar, and winds measured by airborne Doppler radar. The apparent results are consistent with Simpson's hypothesis (*Simpson and Malkus*, 1963). However 10 percent changes in wind speed and 10-mile outward migrations of an eyewall can readily occur in the storm's natural fluctuations. The verification of the experiment is made difficult by the fact that the natural fluctuations in the storm are expected to be large.

While a continuation of such seeding experiments could provide inter-

esting insights into limited aspects of hurricane behavior, the possibility seems rather remote that an effective hurricane-modification technique can emerge from this approach alone. A very comprehensive attack is required on the many scientific unknowns of hurricane behavior. From all points of view—socio-economic, logistic, and engineering, as well as scientific—this is clearly a special problem in meteorological research bearing urgently upon a recognized goal of weather modification. Therefore, the requirements of a hurricane-research program are discussed in greater detail in a later section of this volume.

Modifying the Weather and Climate
of Large Areas

Introduction—The Question of Climatic Stability

Whatever our discontent with local characteristics of the present world climate, we should bear in mind that things could be much worse, as they have been in the distant past. No thoughts of environmental tampering on any large scale or for any long term can ignore the possibility of major unforeseen consequences. It is in this area that the difference between controlled and inadvertent or premature modification assumes its greatest significance.

Any consideration of the possibility of modifying the weather or climate of large areas inevitably raises the question of how constant the climate is under natural conditions. Firm geologic evidence for a long sequence of glacial ages indicates that, at the very least, the world's climate has been in a state of slow evolution. There is also good geologic, archaeologic, and historic evidence that there is a pattern of smaller, more rapid fluctuations superimposed on the slower evolutionary change. The changes of climate since the last maximum of the Wisconsin Glaciation (*Flint*, 1953) are important for the theory of climatic change, because during that period the changes in land and sea distribution and in the elements of the earth's orbit were small. The following brief description of the postglacial European climate (adapted from *Brooks*, 1951) serves to illustrate the nature and rapidity of the fluctuations that may naturally occur.

The climate of the early postglacial period in Europe was continental, with hot summers and cold winters. In the sixth millenium B.C. there was a change to a warm humid climate, with a mean temperature up to 5°F higher than at present and a heavy rainfall that caused a considerable growth of peat. This period is known as the "Climatic Optimum." In Scandinavia it was accentuated by subsidence of the land, which permitted a greater influx of warm Atlantic water into the enlarged Baltic

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known as the Littorina Sea, but the Climatic Optimum was so widespread —probably worldwide, but not everywhere the same—that this cannot have been the only cause. Judging by the flora of Spitzbergen, the Arctic Ocean was free of ice.

On the European continent the climate following the Climatic Optimum was peculiar. On the whole, there was a very gradual decrease of temperature and rainfall, but this was interrupted by long droughts in which the surface of the peat dried out, followed by returns to more rainy conditions. This fluctuation occurred several times, the main dry periods falling about 2200–1900, 1200–1000, and 700–500 B.C. The last was the best developed and caused a widespread interruption in the growth of peat in Europe. It has been described as a "dry heat wave," lasting for perhaps 200 years. The drought was not sufficiently intense to interrupt the steady development of forests, but it caused extensive migrations of peoples from drier to wetter regions.

About 500 B.C. there was a great and rapid change to a colder and wetter climate. Over large areas of Europe the forests were killed by a rapid growth of peat. The levels of the Alpine lakes rose suddenly, flooding many of the lake dwellings, and most of the mountain area became uninhabitable, the few settlements being limited to the driest valleys. Traffic across the Alpine passes, which had continued steadily since 1800 B.C., came to an end. As many of the peat bogs formed during this period are now drying out, it seems that the rainfall for this Sub-Atlantic Period must have been greater than at present. This change of climate was by far the greatest and most abrupt since the end of the ice age, and its effect on the civilization of Europe was catastrophic. It did not last long, however, for by the beginning of the Christian Era, conditions did not differ much from those of the present.

Since the beginning of the Christian Era, climatic variations have continued to occur, although none has been as catastrophic as that of 500 B.C. One more recent perturbation, however, the Little Ice Age of the 17th century (1650–1850)—is worthy of mention. Although the climatic extremes of the Little Ice Age were less than in the earlier examples, many scientists feel that there is good evidence that the speed with which changes occurred was just as great. These changes in climate arose from natural causes but, especially for the more recent variations, our inability to identify these causes positively indicates that they were probably quite subtle.

Today's need for improved hypotheses of climatic change stems from the fact that human activities have reached the stage at which they can effect significant changes in global environmental parameters. Because of the spectacular rate of growth of human influence on the environment, for good or bad, a new urgency is attached to climatic history and

theory. However, geological and paleontological considerations and simple intuition may not yield enough in themselves, for ancient evidence is incomplete and ambiguous. Questions concerning the general circulation of the atmosphere, land–ocean–atmosphere interactions, and details of the earth-system's use and disposal of solar energy all have to be collectively answered in the course of developing a comprehensive geodynamical model with which we can perfect theories and test hypotheses of climatic change. The following sections describe some of the approaches that, it is believed, are most promising for progress toward understanding the central meteorological aspects of the problem.

THE GENERAL CIRCULATION AND THE PROBLEM OF CLIMATE

The low-level global wind systems—the familiar trade winds in the tropics and the prevailing westerlies in middle latitudes—have been observed for centuries. More recently the motions at higher elevations, such as the strong westerlies near the middle-latitude tropopause, have been systematically observed; and, in the past decade or so, the circulation of the lower and middle stratosphere in the Northern Hemisphere has been observed with some regularity. Less is known about the upper-level winds in the tropics and in the Southern Hemisphere. These global systems of motions and their accompanying fields of pressure, temperature, and density constitute what is ordinarily called the general circulation of the atmosphere.

Proposed explanations for the global wind systems date back to 1735, with the publication of Hadley's famous paper on the cause of the trade winds. An explanation for the circulation in one region involves the nature of the circulation in other regions, and, one after another, past explanations have had to be discarded as they were found to conflict with newer, more extensive observations. At present, we still lack an acceptable complete theory of the general circulation.

By far the most ambitious schemes for weather and climate modification have been those aimed at altering the general circulation of the atmosphere, rather than simply changing conditions in restricted localities. The more familiar schemes of this sort are typified by such grandiosities as the spreading of coal dust over Greenland, with the intention of melting away the Greenland ice cap. This project is not proposed with the primary intent of making Greenland more habitable, but with the idea that removal of the ice cap would somehow bring a milder climate to much of the Northern Hemisphere.

However impractical or irresponsible such proposals may be, they clearly recognize the dependence of weather and climate in one region

upon conditions in other regions. Plainly, a modification of the general circulation, if feasible, could bring about improved conditions in specific locations, although not necessarily without harmful effects elsewhere.

Whereas our ideas concerning weather and climate modification on a local scale have been gained partly from the results of experiments actually performed, no serious attempts to modify the general circulation have yet been carried through, largely because of the incomparably greater effort and expense required. Our ideas concerning modification of the general circulation must, therefore, in the foreseeable future rely totally upon theory, and the incompleteness of theories of the general circulation thus becomes more serious than any similar lack in theories for smaller-scale phenomena. In the following paragraphs we discuss some of the observational aspects of the general circulation, the extent to which they have been accounted for theoretically, and their implications for weather and climate modification. We then discuss the simulation of the general circulation by numerical models and laboratory experiments.

Theoretical aspects of large-scale modification

Two properties of the general circulation of the atmosphere that distinguish it from the circulations of many other fluid systems are its close approximations to two forms of equilibria, hydrostatic and geostrophic. Hydrostatic equilibrium occurs when the vertical pressure forces balance the force of gravity. Geostrophic equilibrium, which is a less accurate approximation than hydrostatic equilibrium and does not prevail in equatorial latitudes, occurs when the horizontal pressure forces balance the Coriolis force. Associated with these forms of equilibrium is a tendency for the motion to be quasi-horizontal and quasi-nondivergent.

As a result of hydrostatic and geostrophic equilibria, the principal features of the wind field are generally reflected in the pressure and temperature fields. Indeed, if these forms of equilibrium are accepted as explained phenomena, an explanation for a feature of the temperature field often constitutes an explanation for a feature of the wind field, and vice versa. Although no rigorous explanations of hydrostatic and geostrophic equilibria seem to have been given, the following qualitative arguments appear to contain the essence of the physical cause.

It can be shown that geostrophic hydrostatic motions tend to oscillate in rather long periods, while nongeostrophic motions oscillate in shorter periods, and nonhydrostatic motions oscillate in extremely short periods. The general circulation is driven primarily by forces that oscillate in long periods and produce long-period motions. For various reasons the nonlinear interactions among the motions do not generate new short-

FIGURE 1. The energy cycle of the atmosphere. Values (at arrows) for generation, conversion, and dissipation of energy are fractions of the rate at which solar energy reaches the earth. Values (in circles) for energy are fractions of the amount of solar energy reaching the earth in one day. Diagonal arrows indicate gains or losses through interactions with the environment. Values have been compiled by *Oort* (1964).
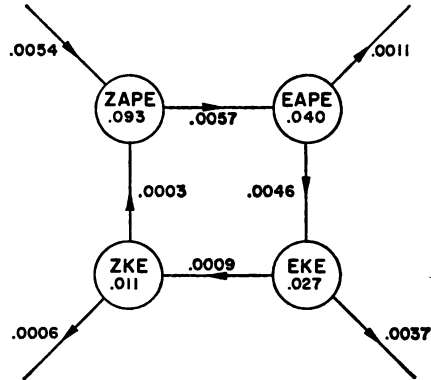
period motions having appreciable energy. It is interesting to observe that one important shorter period in the thermal forcing is the diurnal period; it leads to the well-known diurnal and semidiurnal pressure variations, which are nongeostrophic but which, except in the tropics, do not account for a major fraction of the low-level pressure variability.

With regard to weather and climate modification it is difficult to imagine how any feasible scheme can appreciably upset hydrostatic or geostrophic equilibrium. Consequently, one need not consider modifying either the temperature field or the wind field separately; any scheme aimed primarily at modifying one of these fields will, if it succeeds, inevitably modify the other.

The prominent observed features of the general circulation include the low-level trade winds and prevailing westerlies and the strong upper-level westerlies already mentioned. They also include the presence of large-scale "eddies" superimposed upon these currents. These large-scale eddies include the long wavelike meanders in the westerlies and the somewhat smaller cyclones and anticyclones. An appreciation of the importance of large-scale eddies may be gained by considering the energetics of the general circulation. Figure 1, based upon figures compiled by *Oort* (1964), shows the amounts of four forms of energy—zonal and eddy available potential energy, denoted by ZAPE and EAPE, and zonal and eddy kinetic energy, denoted by ZKE and EKE—and it also shows their rates of generation, conversion, and dissipation. Evidently, the eddies of the general circulation contain more than two thirds of the kinetic energy and nearly one third of the available potential energy.

It was once supposed that the eddies were a sink for the energy of the zonal circulation. The recent figures reveal that, although there is a net transfer of total energy from the zonal flow to the eddies, the net transfer of kinetic energy is from the eddies to the zonal flow. Therefore, any theory that treats the eddies as a form of large-scale turbulence, trans-

ferring angular momentum from zones of high to zones of low angular velocity, is at odds with the observations and must fail. A proper explanation for the maintenance of the zonal circulation must involve the non-randomness of the eddies.

It may also be incorrect to say that the cyclone-scale perturbations *must* give up kinetic energy to the zonal motion. In January 1963, for example, a quasi-stable flow pattern developed, in which the eddies actually received large amounts of kinetic energy from the zonal flow (*Wiin-Nielsen et al.*, 1964). This particular situation was only temporary, but a situation in which the eddies *permanently* receive kinetic energy seems to be mathematically possible, albeit improbable. The more frequently observed state in which the eddies give kinetic energy to the zonal flow is evidently the highly preferred situation, but not the only possible one. These considerations indicate the complexity of general-circulation theory, and the relative ease of drawing incorrect conclusions.

We might attempt to modify the general circulation by altering the intensity of any one of the processes of energy generation, conversion, or dissipation. The generation and dissipation processes seem to be the most likely targets, since they can, in principle, be altered by changing some thermal or mechanical properties of the earth's surface.

As shown in more detail in the later discussion of local and regional climate modification, there are several classes of physical properties of the natural earth–air interface that can be manipulated by artificial means. For applications on a truly large scale, there are obvious technical and logistic difficulties. For example, if one wished to create a surface coating of as little as 1 μ (0.001 mm) thickness to cover a square 1,000 km on a side, the total material for this extremely thin coating would fill a volume of 1 million cu m, and might weigh several million tons, depending on the specific gravity of the substance. Proposals to dust by aircraft some of our globe's extended ice sheets (like the arctic pack ice, or inland ice in Greenland or Antarctica) with a coating of carbon black to decrease the albedo, and to utilize additional absorption of insolation for snow-melt, are, for logistic reasons, of academic interest only. The costs and technical difficulties of such schemes are truly tremendous, as was demonstrated quite convincingly by *Wexler* (1958).

The generation of available potential energy depends upon the covariance of heating and temperature. In theory, the large-scale heating field can be altered by modifying the distribution of albedo. It should be remembered, then, that the distribution of albedo is determined largely by the distribution of clouds, and is therefore being continually altered on large scales by natural mechanisms. Any attempted artificial surface modification would likely be small by comparison. Moreover, a surface alteration might well affect the cloud distribution; thus, for example, a

darkening of the earth's surface would not decrease the albedo if it should lead to the development of additional cloudiness.

Nevertheless, if the albedo should be changed either intentionally or inadvertently over a large enough area, the over-all temperature gradient and, hence, through geostrophic equilibrium, the intensity of the upper-level winds, could be altered. This altered circulation pattern could possess an altered degree of baroclinic instability; i.e., the migratory cyclones and anticyclones would grow or decay at an altered rate. In short, the entire general circulation, including the major eddies, would be modified.

Evidently the generation of available potential energy can arise from the covariance of infrared cooling of the atmosphere and temperature also. Satellite and balloon measurements show that the presence of cirrus clouds in the upper troposphere near the tropopause strongly reduces the heat loss of the atmosphere by infrared radiation. Thus, a cirrus cloud cover over warm northward-moving tropical air will prevent it from cooling as rapidly as does southward-moving cooler air, which has no cirrus cover. On the average, this mechanism yields only a small amount of potential energy, but on the scale of the traveling cyclones it can reach high positive values locally. It is interesting that this phenomenon has the basic ingredients of a possible control mechanism, i.e., a small amount of cloud, such as a vapor trail, released at the optimum altitude has a large influence, will last a long time, or may even grow. However, before we can hope to test either this or changing surface albedo as a means of control, we must wait for mathematical models of the atmosphere that are somewhat more sophisticated than those presently available.

Proposals for modifying climate, as opposed to day-to-day weather variations, by changing the albedo include the already-mentioned proposal to spread coal dust over Greenland. The much-discussed proposal to dam the Bering Strait and then pump cold water out of the Arctic Ocean would be aimed mainly at changing the temperature and thermal capacity of the underlying surface. Possible climatic changes consequent to a hypothetical removal of the arctic ice pack have been repeatedly discussed in past and present literature. For example, *Budyko* (1962) concludes that, if this ice cover were removed, the central Artic might enjoy summer temperatures of +10 to +20°C and winter temperatures of +5 to +10°C, which obviously would imply decisive changes in the climate of Northern Siberia and Canada. Budyko also suggests that, once removed, the arctic ice cover would not form again. *Badgley* (1961), however, arrived at the conclusion that it is not likely that the ice on the northpolar ocean will be permanently dissipated. The problem is far from being solved. It has attained some fundamental importance in view of a hypothesis by *Ewing and Donn* (1956) explaining the ice ages in North

America and Europe as results of atmospheric circulations caused by open water in the Arctic Ocean. A very comprehensive and detailed survey of the arctic heat budget, its relation to climate, and possible modification techniques was reported recently by *Fletcher* (1965).

Schemes to alter the dissipation of kinetic energy by changing the coefficient of surface friction include proposals to spread a thin film of oil over much of the ocean surface. It has even been suggested, not entirely facetiously, that a dense network of billboards be erected over the Great Plains to change the effective coefficient of friction, the cost of the billboards hopefully to be borne by the advertisers.

One might argue that such ideas, if acted upon, would modify the climate. The important question, however, is not *whether* the climate would be altered, but *how*. Whatever else one may say about these schemes, they share the failing that we cannot anticipate their effects with any confidence. The general circulation is too highly complex and nonlinear to enable one to draw conclusions from step-by-step qualitative reasoning.

It seems plausible, for example, that increasing the effective coefficient of friction over an extensive region will decrease the surface winds over the same region, but, beyond this, we are in the dark. A larger coefficient of friction coupled with weaker winds could mean either more or less dissipation of kinetic energy. Detailed theoretical computations seem essential. The fact that different oversimplified numerical models of the atmosphere can yield different results emphasizes the importance of taking as many aspects of the general circulation as possible into account quantitatively.

Several other phenomena are particularly worthy of mention although the extent of their influence upon the general circulation is not yet known. One of these is the jet stream. It is not known how greatly the general circulation would be altered if the maximum upper-level westerly winds in middle latitudes occupied a broad band rather than a narrow meandering stream. A narrow jet appears to offer an efficient means of transferring momentum from one latitude to another. Another phenomenon is the front. Again, it is not known how the general circulation would differ if contrasting air masses were typically separated by broad transition regions rather than narrow frontal surfaces. Still another such phenomenon is the tropical hurricane. If ordinary hurricanes could be eliminated, would the necessary transports of energy and moisture in low latitudes be accomplished by smaller systems, or would energy continue to build up in certain regions until a super-hurricane developed? Until such time as the importance of these phenomena can be properly assessed, the theory of the general circulation will remain incomplete,

and our ability to evaluate any proposals for modifying the general circulation that are based on modifications of the jet stream, fronts, or tropical hurricanes will remain inadequate.

A final noteworthy feature of the general circulation is the irregularity of its oscillations. Despite the obvious annual and diurnal cycles and their overtones, a large residual oscillates nonperiodically. As a consequence, the circulation is unstable in the sense that two slightly different initial states would ultimately evolve into considerably different states (*Lorenz,* 1963).

The possibility of altering the course of the weather by some triggering impulse is immediately suggested. It must be noted, then, that there are large uncertainties in our knowledge of the existing state of the atmosphere at any given time, particularly in regions of sparse observations. With the current observational network, any triggering modification will initially be small as compared to these uncertainties. The effects of the modification will not grow to appreciable size until after the effects of the uncertainties have also reached appreciable size; i.e., the effects of the impulse will not yet be felt at those times for which acceptable weather forecasts can be made. Thus, we cannot recognize in advance those times when triggering impulses are desirable; we cannot profitably use an impulse to modify a coming bad-weather situation when we do not know whether bad or good weather is actually coming. Likewise, if an impulse is introduced anyway, its true effect after some time will never be discovered, but will be masked by the uncertainties of the state upon which it is superimposed. Ability to forecast the natural course of the weather is, therefore, a prerequisite to controlling the course of the weather in a predictable fashion, rather than modifying it recklessly.

With the hoped-for improvement in the present observational network, we are looking forward to an extension of the range of acceptable forecasting. This, considered together with our ever-increasing ability to release large amounts of energy, suggests that we may some day introduce modifications whose initial amplitudes exceed the uncertainties in specifying the initial state of the atmosphere. Until that day, the effect of a small triggering mechanism will be no more predictable than the effect of giving an additional shuffle to an already well-shuffled deck of cards.

It is less discouraging to consider the eventual possibility of modifying the climate by continued, periodic application of triggering impulses. If it can be demonstrated that certain specific triggering actions will, statistically, show a tendency to alter the circulation in certain directions, the statistical effect of a large number of triggering impulses may be predicted and detected even when the effect of each individual impulse is below the threshold of detectability.

Numerical simulation of the general circulation

Phillips (1956) first attempted numerical simulation of the general circulation. His study followed after the successful application of numerical techniques to simple quasi-geostrophic baroclinic models at the Institute for Advanced Study. His work showed that certain gross properties of the general circulation involving eddy transfer of energy and momentum can be simulated in simple quasi-geostrophic models. Phillips' study suggested that if the atmosphere is disturbed it will return to a state having the space, time, and speed scales that are characteristic of the atmosphere.

Phillips' work prompted the construction of a general circulation model (*Smagorinsky,* 1963) using the primitive equations of motion.* This model permitted the study of motions in a spherical zonal strip and admitted nongeostrophic motions, such as internal gravity waves, but was thermodynamically similar to that of Phillips. The long-term integrations of the primitive-equation model revealed an energy cycle of 12 to 16 days. This cycle could be identified with the real atmospheric energy cycle, more particularly the life cycle of an extratropical cyclone. Also, the analysis of the flow of energy in the system and the accompanying poleward heat and momentum transfer showed close agreement with observation.

Despite such encouraging early results obtained with these models, there remained many physical shortcomings. Many physical factors of importance in the atmosphere were omitted from the models. Thus, the static stability was prescribed parametrically.† In reality, the static stability must be determined by the motion itself. One may ask what determines the vertical temperature structure, particularly the existence of a tropopause and its variation with height and season. There are additional questions to be asked. What parametric formulation can be assigned to the hydrologic cycle in calculations concerning the general circulation? To what extent do the thermal asymmetries of the lower boundary (resulting from the distribution of sea and land, ice and snow) determine the earth's climate? Must the atmosphere and ocean be treated as a single fully-coupled fluid system for periods longer than a week or month? What are the influences of large mountain masses on the general circulation? How rapidly do those and other influences propagate? How much do the circulations of the two hemispheres interact? What is the role of tropical disturbances in maintaining the general circulation?

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* The primitive equations are the Eulerian equations for fluid motion, in which the primary dependent variables are the fluid's velocity components.

† "Parameterization" is a simplification introduced into a dynamical model by preassigning the magnitude of a physical effect rather than allowing the effect to be realistically determined internally as a consequence of the dynamics of the system.

The answers to these questions require models with fewer approximations than have so far been used. There must be greater resolution in the vertical, the entire globe must be considered, parametric controls must be relaxed, and it must be possible to introduce degrees of freedom heretofore ignored.

Table 5 lists the efforts of three principal groups in the United States engaged in model construction and experimentation. In addition, a number of related efforts are being made in other countries.

In this country, substantial progress has been made in constructing sophisticated numerical models. Mintz's two-level model atmosphere (*Mintz*, 1965), whose lower boundary consists of land (including mountains) with zero heat capacity and a surrounding sea with fixed surface temperatures, verifies and extends the results of earlier linear analyses. This shows the influence of the lower boundary in forcing the quasi-stationary disturbances one observes on climatological mean charts. His global calculations and those of *Leith* (1965) yield intertropical convergence zones that have some resemblance to observation but whose details obviously suffer from inadequately formulated convective mechanisms. The Weather Bureau group has also produced some results with a nine-level model (*Smagorinsky et al.*, 1965; and *Manabe et al.*, 1965). This model, subjected to the annual mean solar radiation, forms and maintains a tropopause and stratosphere that are in good agreement with annual mean observations except in the polar regions. This deficiency is presumably in part a result of having ignored the seasonal variation of the real radiation cycle. Meridional profiles of the poleward heat and momentum flux agree quantitatively with observations. The upward transition from hemispheric wavenumbers 5 and 6 in the troposphere and lower stratosphere to a wavenumber of 3 at 10 mb is also in agreement with observation and with theoretical analysis. The loss of kinetic energy by dissipation and by transformation to potential energy is maintained in the stratosphere by a vertical flow of energy from the troposphere. Many of the most important climatological properties of the water balance of the atmosphere have been reproduced: the latitudinal and vertical variation of relative humidity, including the very low values in the stratosphere; the precipitation-evaporation distribution with latitude, particularly the desert-producing conditions in subtropical latitudes; and the latitudinal variation of the ratio of sensible to latent heat exchange between the earth's surface and the atmosphere (the Bowen ratio). (Discussed later in this report are studies of the water budget as an element of the general circulation.)

It is important to point out that the ability to simulate the observed properties of the real atmosphere is sterile unless accompanied by the ability to explain the simulation in terms of the detailed mechanics of

TABLE 5

Model Features	U.S. Weather Bureau (Mark I) (Smagorinsky, 1963)	University of California Los Angeles (Mintz, 1965)	Lawrence Radiation Laboratory (Leith, 1965)	U.S. Weather Bureau (Mark II[a] and Mark V[b])
Nongeostrophic modes	internal gravity	internal-external gravity	internal-external gravity	internal-external gravity
Vertical structure	two-level (no stratosphere); one parametric static stability	two-level (no stratosphere); one static stability	five-level (one in stratosphere); five static stabilities	nine-level (two to three in stratosphere); nine static stabilities
Spatial domain	zonal strip on sphere	global	global	hemispheric
Radiation	parametric	parametric	terrestrial radiation prescribed	explicit function of astronomy; and climatologically prescribed water vapor, clouds, CO$_2$, ozone
Thermal property of lower boundary	land: thermal equilibrium	sea and ice: fixed temperature; land: thermal equilibrium	sea and land: fixed temperature	land: thermal equilibrium
Snow-cover budget	none	none	none	none
Boundary layer	Ekman and Prandtl approximation	Ekman and Prandtl approximation	Ekman and Prandtl approximation	Prandtl approximation
Convective transfer	parametric momentum	parametrically noninteractive heat and momentum	parametrically noninteractive heat and momentum	moist adjustment, acting on heat (Mark II and V), water vapor (Mark V)
Hydrologic cycle, including large-scale condensation	none	none	yes, but no cloud stage	yes (Mark V) none (Mark II)
Land-water budget	none	none	none	none
Orography	none	yes	none	none
Differencing scheme	central, nonlinear viscosity	vorticity and quasi-energy-conserving (Arakawa); linear viscosity, time-smoothing	semi-implicit (modified Marchuk); linear viscosity	energy-conserving (Lilly); nonlinear viscosity, occasional time-smoothing

[a] Smagorinsky et al. (1965).   [b] Manabe et al. (1965).

operation of the model. It is in this respect that numerical-model experiments can provide their greatest power as a scientific tool. Some measure of the continuous growth of understanding represented by such modeling research is the level and scope of predictability attained in the application of real initial data to such general-circulation models. (*Smagorinsky et al.*, 1965).

Future numerical models will provide useful means for testing the feasibility of weather- and climate-modification schemes. In particular, we can expect to construct numerical models that simulate natural climatic situations. The detailed analysis of such models may suggest the existence of instabilities that might be exploited in modification experiments. While great progress has been achieved, present models show important deficiencies when compared with observations, and discrepancies when compared with one another. The principal causes of these weaknesses are thought to be:

1. Incorrect formulation of the physical processes—in particular, the role of convection in relation to the large-scale motions.

2. Mathematical errors in transforming the partial differential equations of motion into finite-difference forms.

3. Uncertainties in establishing the initial conditions of the state variables (particularly in predictive as opposed to climatic studies).

4. Limitations imposed by existing computing systems.

While many simplifications are made out of ignorance, others are made to facilitate analysis at a particular phase of investigation or to conserve computation time. The latter types of simplification, therefore, are removable, having been made temporarily in the interest of expedience. The most damaging kind of uncertainty in formulating the physical problem relates to assignment of parameters for convective transfer, a separate scale process that is crucial in low latitudes and an important contributing process in the midlatitudes. The difficulties in defining parameters for convective processes are due to the small-scale turbulent transfers of heat, momentum, and water vapor. These processes constitute the turbulent link between the resolvable dynamics and the molecular dissipation. The parameterization of the convective process emphasizes the latent weakness of numerical models. Parameterization is derived from the particular observed properties of the atmosphere. Care must be exercised, therefore, in interpreting the numerical results, lest an "explanation" for a phenomenon in reality be a built-in climatological property.

The violation of integral constraints in long-term numerical integrations provides a measure of the systematic errors resulting from the finite-difference approximation to the partial differential equations. The problems of finite-difference approximation and of internal dissipation

are not independent. Internal dissipation occurs in connection with large deformation fields, such as the jet stream and frontal zones, and an adequate physical description of these features will require new methods of obtaining high resolution in regions where they occur.

Observational inadequacy is of particular importance in prediction experiments, and thus will have a major bearing on simulated-modification studies. Moreover, a climatic model cannot be expected to be accurate unless it is capable of correctly predicting, say, the life cycle of an individual storm system. Hence, in the long run, predictive studies will also be necessary for the development of climate models. The large gap in the world weather net is such that the results of large-scale experiments cannot now be verified adequately. The primary requirement for adequate simulation of large-scale modification experiments is a considerable enhancement of the observational net, particularly in oceanic, tropical, and southern-hemisphere regions.

Numerical simulations are also severely limited by the capacities of available computer systems. Estimates based on known mathematical models suggest that a computing system 10 to 100 times more powerful than the fastest now available (CDC 6600) is required in many of the general circulation studies that could be undertaken immediately. Such a computing system would permit comparative studies of models, construction of more sophisticated models, and the development of methods for determining transport properties by the calculation of ensemble evaluations, i.e., essentially by Monte Carlo methods.

(The observation and computation problems mentioned in the preceding two paragraphs are discussed at greater length in later sections of this report.)

The four previously listed causes of discrepancies between model evolution and the real atmosphere are not without their interactions. There are, indeed, instances in which limitations in one will obviate expected advances in the others. An excellent case in point is the interaction of the limitations due to observational and computer inadequacies. The future computer needs will be tempered by the degree to which, and where, we can satisfy the requirements for global data. On the other hand, similar arguments may be raised about the combined effect of limitations due to mathematical and physical errors, etc.

It is becoming apparent that our understanding is now virtually good enough to permit us to predict reliably the qualitative influence of certain kinds of massive tampering—for example, removal of the Rocky Mountains, or a twofold alteration in the earth's roughness, or a 20 percent change in the atmosphere's CO_2. However, if we look for the effect of subtle changes, more accessible to human intervention, then our understanding and our ability to simulate natural phenomena will have to be

much greater. An indicator of our present inadequacy is that the degree to which the precipitation rate of widespread stratiform clouds might be altered by seeding techniques (as determined statistically) lies well within the predictability error of current numerical-dynamical models. Model simulations may, however, still prove useful if they can give the direction of an effect, even if the estimate is quantitatively unreliable.

If it becomes evident that the atmosphere only marginally sustains a particular set of phenomena, we can hope that model experiments will divulge the types of instabilities that can be seized upon to swing a climatic regime in a particular direction. For example, if it appears that the poleward heat flux due to hurricanes can be produced entirely by weakly organized tropical convection, such as in stable easterly waves, then it would appear that the hurricanes are not essential elements of the general circulation, and steps could be taken to eliminate them with less fear of disturbing other elements of large-scale weather and climate. Speculating on a larger scale: Is the occurrence of an ice age or the formation of a large desert an inexorable necessity, or is each a consequence of weak but systematic interactions that may easily be disrupted if we learn what the critical participating processes are?

Laboratory simulation of the general circulation

Laboratory experiments use the flow of fluid in a cylindrical vessel to simulate the general circulation. The vessel rotates on its axis, which is vertical. The horizontal cross section may be either the interior of a circle or an annulus. The vessel is systematically heated; ordinarily heat is added at or near the outer radius and removed at or near the axis of rotation. The resulting thermally forced flow within the vessel is studied. The most comprehensive of these experiments are those of *Fultz et al.* (1959) and *Hide* (1958). Only gross features of the atmosphere and its surroundings are introduced into the apparatus in simulated form. In the laboratory general-circulation models, no attempt is made to model the evaporation and condensation processes, the distribution of oceans, continents, lowlands and mountains, or the details of the field of heating. Moreover, the spherical shape of the earth is lacking.

The precise flow pattern, if it is steady or periodic, or the set of statistics of the flow, if it is nonperiodic, depends on several dimensionless ratios. The experiments indicate that two of these are of primary importance, the remainder being more or less redundant. If the size and shape of the apparatus and the nature and amount of the fluid are fixed, these dimensionless ratios may be taken as the thermal Rossby number and the Taylor number. These, in turn, are determined by the intensity of the heating contrast and the rate of rotation.

The possible resulting flow patterns fall into several regimes or sub-regimes. Within a subregime the differences between flow patterns are mainly quantitative, while flow patterns in different regimes differ qualitatively. The simplest is the Hadley regime, in which the flow is symmetric with respect to the axis of rotation. This regime is favored by slow rotation. The energy cycle differs considerably from that of the atmosphere since there is no eddy energy.

The Rossby regime, in which large-scale traveling waves occur, is favored by faster rotation, but is inhibited by very strong or very weak heating. It sometimes possesses an energy cycle resembling that of the atmosphere. Within the Rossby regime there is a *steady* Rossby regime, where a chain of nearly identical waves progresses without changing its shape. The steady Rossby regime possesses further subregimes, each having its own number of waves. A pronounced jet stream generally occurs. In the annular experiments, the jet meanders from wall to wall and appears to be the principal mechanism for conveying heat from the outer to the inner wall. At higher rates of rotation, vacillation sometimes occurs. Here the waves do not maintain their shape but alter it in a regular periodic fashion. Forecasting in a periodically vacillating atmosphere would pose no problem. At the highest rates of rotation, the *unsteady* Rossby regime occurs. Here the configuration of the circulation oscillates nonperiodically.

A small modification of the dimensionless ratios may simply change the circulation pattern to another pattern within the same regime. If, however, the ratios pass certain critical values, the regime will change. The change may be a simple change of wave number, a change from a steady to an unsteady Rossby regime, or even a change from a Rossby to a Hadley regime. One of the difficult tasks of experimentalists is to determine the critical combinations of dimensionless ratios, some of the criteria for which are not sharply defined.

Possible applications of laboratory models to climate modification can be visualized. Long-term statistics could be compiled from two or more experiments that differ slightly in their fields of heating, or in some other characteristics such as different degrees of roughness of the bottom surface. Their value in weather-modification studies is much less obvious. If the experimental system is oscillating nonperiodically, prediction of its future state may pose all the problems encountered in predicting the real atmosphere, and the effect of a triggering impulse may be just as hard to identify. In a periodically varying system, exact forecasting is possible, and the effect of an impulse can readily be assessed. But periodic behavior is the result of stability; it is likely that, by studying the effect of impulses on systems that would otherwise vary periodically, we are restricting our attention to those systems that are least vulnerable to modification.

Modifying Local and Regional Climates

A systematic approach to the problem of local and regional climate modification can be based on thorough appraisal of all the constituents of the energy budget at the ground–air interface. We start out with the continuous energy-flux density emitted by the sun, which for our purposes can be considered constant. As a first step, let a hypothetical planet consisting only of a thin solid shell with zero heat capacity and zero heat conductivity intercept this energy flux. At any instant, the radiative energy received (the insolation), minus that reflected, must be balanced by the energy radiated out to space (planetary radiation). The two physical parameters involved here are the reflectivity (or albedo) and the emissivity. The climate of this hypothetical planet follows from the general radiation laws of physics, and furthermore will be determined by its orbital elements relative to the sun and the spatial orientation of the vector of rotation (that is, axis orientation) together with angular speed. This means that there will be annual and diurnal variations of surface temperature corresponding to cycles of revolution and rotation. Such periodic elements determine insolation as a time function at a given latitude of the planet, and constitute the true and fundamental forcing function of the planetary climate.

Second, we relax the requirements concerning the physical properties of the planetary body. This means that two additional parameters enter—heat conductivity and heat capacity of the outer crust of the planet. Diurnal and annual temperature variations will be modified so that, during periods with insolation, heat will be conducted downward from the surface and temporarily stored in the crust, to be released during periods when the surface area in question points away from the sun. Since such conditions correspond to conditions on the moon, we may speak of the "lunar-type" heat budget. Its parameters are represented by

the four physical quantities so far mentioned. The last two quantities sometimes are used in derived forms, with the ratio of heat conductivity over heat capacity referred to as thermal diffusivity, and their product as the square of thermal admittance. For harmonic oscillations of the forcing function of insolation, it can be said that the thermal diffusivity determines the phase lag, while the thermal admittance determines the amplitude of the response function as represented by harmonic oscillation of surface temperature.

In the third step toward realizing the complexity of the planetary heat budget, an atmospheric shell is added, excluding those gases that may undergo changes of phase (like H_2O) as a consequence of temperature changes. If this shell has a relatively small optical density (such as clear air on earth or on Mars), the absorption of insolation will still mainly occur at the ground–air interface. At least four new parameters enter at this stage. Two are due to the increased complexity of radiation-transfer conditions—the transmissivity (or turbidity), including scattering effects, of the air relative to insolation; and the emissivity (or absorptivity) of the atmosphere for long-wave planetary radiation, necessary to define the amount of "back radiation" to the surface. The other two are needed to describe heat conduction as well as heat storage in the atmosphere. Since the air is a fluid, and certain critical requirements concerning the stability of laminar flow processes are hardly ever satisfied (mainly due to the geometric extent of the atmosphere), heat conduction is invariably of the turbulent, or eddy, type. This means that semiempirical concepts must be considered, since a universally accepted turbulence theory is lacking. For the parameterization of actual heat budgets, it appears to be most practical to describe the diffusive power of the lower atmosphere in semiempirical terms of aerodynamic surface roughness, together with the over-all intensity of an ideal or frictionless air motion as related to the surface value of the large-scale horizontal pressure gradient (the geostrophic wind speed). In contrast to subsurface conditions, the specific heat of the air varies hardly at all, within tolerable limits. Nevertheless, it follows that a minimum number of eight parameters must be involved in an understanding of energy budgets of the "desert-type" or "Martian-type" climate, as this third version may be referred to.

The fourth step toward further sophistication will require the consideration of phase changes (ice–water–vapor), and utilization of fractions of insolation to increase the latent heat of the air. The upward transfer of vapor is generally assumed to be affected by the same mechanisms and processes that determine the eddy diffusion of heat. Thus, as long as the air remains clear, only one parameter need be added, which describes the availability of H_2O for evaporation or sublimation at the ground–air interface. Another empirical parameter will be needed to

describe absolute humidity aloft. In general, however, the evaporative flux will, of necessity, lead to cloud, fog, and ultimately to precipitation. The optical properties of cloud layers affect, in turn, the insolation distribution and intensity at ground level, as well as terrestrial radiation. The study of the actual heat budget on the ground–air interface on a worldwide or even continentwide scale thus becomes a highly complex problem. This is confirmed in the literature by reports of many studies that have produced no generally accepted results. (See, for example, *Budyko,* 1958).

Nevertheless, at least two more steps are needed to formulate an exhaustive description of natural energy budgets. One of these deals with the possibility not only that insolation at ground level is transformed to latent or sensible heat and used to maintain terrestrial radiation, but also, additionally, that photochemical reactions can be significantly involved. It has been estimated that certain field crops (like corn), at least in certain stages of development, may utilize about 5 percent of the daily net radiation in the photosynthetic process. The sum of such processes, integrated over millions of years, has produced the presently available supply of fossil fuels. At any given time, part of the biologically assimilated solar radiation is the source of all food intake of all animal life.

Finally, there is the possibility of an active supply of energy not derived directly from the sun. There are certain localities on earth where significant amounts of volcanic heat are liberated. In fact, there is heat from the radioactivity of the earth's interior crust continuously being conducted everywhere toward the surface at the rate of a few milliwatts per square meter. This intensity is, however, negligibly small in comparison with values of the solar constant (about 1,400 W/m²) or the global average flux density of latent heat into the atmosphere (about 70 W/m²). Only when such terrestrial energy release is concentrated on a small area can this item be significant.

In summary, discounting characteristics of artificial and nonsolar energy releases, we have defined 12 parameters that must be accounted for in a complete description of ground–air energy-exchange processes. Most of these parameters exhibit variations in space and time, and quite a few of them are based on semi-empirical formulations due to inadequate theory.

Insight into the magnitudes of the effects produced by any form of surface modification requires the execution of well-designed field experiments under well-defined external conditions, and evaluated with due regard for all changes in energy budgets. Numerical studies of boundary-layer exchanges are still primitive in form, though a beginning has been made by *Estoque* (1963) and *Estoque and Yee* (1963). These

calculations are beset by major computational problems as well as by difficulties in the parameterization of the relevant physical processes, especially the processes of turbulent exchange. Even for the relatively simple case of numerical experiments with the heat-budget variations of dry-soil surfaces, the minimum number of parameters that must be considered will be seven.

MODIFICATION OF THE AGRICULTURAL MICROCLIMATE

With the systematic and complete parameterization of the processes involved in energy transfer at the ground–air interface, we have established a rationale for any attempts at local climate modification. In general, three of the seven most important boundary-layer parameters (heat capacity, emissivity, and albedo) show relatively little natural variability among neighboring areas. The major differences in neighboring natural microclimates are due mainly to the appreciable variations in the conductivity and specific heat of the soil, the available soil-moisture supply, and the aerodynamic surface roughness and resulting eddy diffusivity. To illustrate the effects produced by altering one of these properties: if the surface roughness is made small by leveling, packing, and smoothing of the natural ground surface, both the heat conduction into the ground and the turbulent structure of the lower atmosphere are changed; the vertical wind shear near the ground may be greatly increased. The onset of convection is determined by the Richardson number, which, in turn, is directly proportional to the lapse rate of temperature and inversely proportional to the square of wind shear. Consequently, the stimulation of the convective action necessary to transfer heat upward from the surface requires stronger heating rates on smooth areas than above rough ground, other conditions remaining the same. The fact that surface roughness is a very important determinant of the thermal structure in the lowest layer of the atmosphere has long been known; for example, with a given insolation, inferior mirages appear most readily over relatively smooth surfaces and only rarely over rough ground. The micrometeorological effect of aerodynamic surface roughness is demonstrated spectacularly by the fact that air temperature at a height of a few feet can be *cooler* by 1 or 2°F over a hot airfield runway than over the cooler grassy surroundings.

A frequent practical aim in agriculture is to counteract the tendency to low night-time minima of surface temperature; that is, to prevent frost damage to crops in certain stages of development, either in spring or autumn, in temperate or high latitudes. This problem can be approached through a variety of techniques. A precautious farmer could try to ac-

cumulate more heat from the insolation of the preceding day by deceasing the albedo of his ground cover, possibly also reducing its emissivity. More directly, he could aim to increase nocturnal back-radiation to the ground from the atmosphere by smoke or fog screens; or he could employ artificial stirring of the air over the crop by power-driven fans to increase the night-time downward convection of sensible heat; or, in a "brute force" attempt, he could directly apply radiant energy. Another method, long practiced by cranberry growers, is flooding of crops with water. A large body of empirical knowledge exists, and a number of frost-prevention practices have been firmly established. [For a list of references, see *Geiger* (1965) or *Gilman et al.* (1965).] Individual techniques of frost-prevention have certain limitations. Sometimes, combined techniques are helpful; but in the case of a general freeze associated with a deep, cold, polar air mass, no success can be expected except at the very fringe of the freeze area.

Other problems of small-scale climate modification can be dealt with in similar ways. For suppression of over-heating, shading can be used. For reduction of water losses, it pays to remember that it takes about 60 cal/cm^2 to evaporate a water film of 1 mm thickness, and that possibilities of reduction of energy supply should always be considered first. *Waggoner et al.* (1960) have summarized results of systematic experiments using various types of mulches on natural ground. Black plastic used in this manner can cause 90 percent of the net radiation to be converted into sensible heat. The modification of surfaces by plastic films creates new distributions of the energy-budget constituents, changing the soil climate in a predictable direction. It is possible to bring spring noticeably earlier or later to perennial plants, to conserve soil moisture, and to stimulate general plant growth.

Perhaps the oldest and most widely propagated means of modifying microclimates is the planting of multiple rows of bushes or trees. *Gilman et al.* (1965), whose summary is followed in this account, provide an extensive listing of original literature including several critical reviews of the meteorological and biological consequences of such sheltering efforts. In semiarid regions, such as the central parts of the United States and the Soviet Union the primary aim is to prevent soil erosion due to the combined action of wind and evaporation. In northwestern Europe, on the other hand, the aim is to shelter livestock and wind-sensitive crops and to raise daytime maximum temperatures. Most of the knowledge of the effects of shelterbelts is empirical and rather closely tied to the locality where the measurements were taken, usually at or near ground level. The most important conclusion arising from this evidence is the unsuitability of very dense or solid screens, which generate considerable turbulence that destroys the sheltering effect only a few

screen-heights downwind. A screen density of 50 to 60 percent gives the most extensive protection. While some wind-tunnel simulations have been run, field measurements of vertical wind or temperature profiles or of heat and moisture fluxes are rare. Thus, the generalizing power of boundary-layer theory has been absent, and there have been no attempts at numerical modeling. Recent systematic and well-instrumented experiments with groups of removable obstacles on lake ice constitute a first important step beyond empiricism [see the work of *Lettau and Stearns* (1963)]. It is not certain, however, that even a fully developed theory and modeling capability will lead to great advances beyond the substantial sheltering effectiveness already achieved.

AMELIORATION OF DESERT CONDITIONS

Creation of artificial lakes

In all arid regions of the world, proposals have been made from time to time, for at least the past century, to alleviate regional aridity by constructing one or more large water bodies from which water would evaporate to augment the natural stock of atmospheric vapor, thereby hopefully stimulating added precipitation. Only a little reflection is needed to see that such schemes overlook the huge scale on which atmospheric water vapor participates in the global hydrologic cycle, yet the idea is a recurrent one and has even received serious consideration from professional meteorologists at times. Summaries of a number of such proposals and an analysis of their futility have been given by *McDonald* (1962). Illustrating the scale difficulties, McDonald estimated that to increase Arizona summer rainfall by as little as 10 percent would require that a body of water covering some 20,000 sq. miles would have to span the upwind border of Arizona. By way of rough comparison, the combined area of Lakes Erie and Ontario is 17,000 sq. miles.

   Underlying the popular misconceptions, and some professional misconceptions, about the potential of lake-building seems to be the notion that a given molecule of water is, on the average, taken through a rather large number of successive evaporation-precipitation cycles within the confines of a single continental area (*Bergeron*, 1960); but at least in middle-latitude areas, such studies as have been carried out (*Benton et al.*, 1950; *Budyko*, 1958) indicate that an average of only about one such cycle occurs within even rather extensive land areas. Hence, the hope that the vapor yielded by evaporation from a water body of modest dimensions would show up many times in not-too-distant places as local rain appears to be vain.

Usually such schemes also err in expecting the manifold return of precipitation to show up in the vicinity of the artificially created water body, yet rather simple computations based on the mean atmospheric residence time (10 days, roughly) for water vapor and on mean wind speeds (say a modest 10 mph or 240 miles per day) reveal that an evaporated molecule is not likely to be precipitated again until it has drifted for a distance of the order of 2,000 miles. Indeed, this mean drift is of just the order required to match the inference that only a single evaporation precipitation cycle occurs as a molecule crosses an average continental area. Since, even in low latitudes, estimated residence times and mean winds are not greatly different from the values just assumed, one must expect that even in tropical and subtropical areas, lake-building is no cure for regional aridity.

The "thermal mountain effect"

The "thermal mountain effect" has been theoretically derived from a mathematical treatment of the basic fluid-dynamics equations with boundary conditions of a continuous-area source of heat acting on a limited region of the earth-air interface. With a series of simplifying assumptions, *Malkus and Stern* (1953) arrived at the theoretical conclusion that such surface heating will produce an airflow pattern similar to that known to exist when the wind crosses a mountain range. Hence, it was suggested that the effects of surface heating in level country could be specified in terms of a hypothetical "thermal mountain." *Black* (1963) utilized this scheme and computed the equivalent thermal-mountain effect created by sunshine on a hypothetical surface of extremely low albedo surrounded by a surface of relatively high albedo. Black suggested that such conditions could be artificially created by asphalt coating of desert areas. When applied to coastal deserts in the tropics, the resulting thermal-mountain effect would stimulate thermal updrafts, and lead to an increase of a given diurnal seabreeze circulation that could produce rain showers inland. The feasibility of such a surface-modification method was supported by the known and documented effects of heat release by large fires in generating cloud and rain showers, as well as the production of cumulus rows by islands in the West Indies.

In the West Indies, the trade wind gets locally heated from below when the air passes over an island on a sunny day, with the effect that rain showers are observed to form downwind. *Malkus* (1963) carried out a preliminary observational evaluation of the asphalt hypothesis in a study of the "cloud street" produced by the small (approximately 20 sq. miles) tropical island of Anegada. On a weakly disturbed day in the dry season, the island exhibited a street of clouds 15 to 20 miles long

and up to 6,000 ft thick, accompanied by showers, over the ocean downstream. Longer and thicker island cloud streets are common in the wet season and have been well documented by satellite photographs. Malkus concludes that, since asphalt coatings have a temperature advantage about twice that of Anegada, it appears that the asphalt hypothesis requires serious investigation as a possible convection-modification technique.

An objection was raised to the asphalt-coating proposal because the technique could reduce substantially the aerodynamic roughness of the land area, and thus would permit relatively high surface temperatures on the asphalt coating without simultaneously increasing the intensity of convective heat release to the atmosphere, because the excess of absorbed heat may be partitioned entirely into increase of infrared radiation loss, plus conduction into the subsoil. (See also the earlier discussion of the significance of change in Richardson number.) Other questions concerned the possibilities (1) that semidiurnal rainfall cycles existing in the tropics could work against the forcing of rainfall during or shortly after periods of peak surface temperature, and (2) that adding heat without a simultaneous addition of moisture could raise the height of the cloud base and thus reduce cloud buoyancy and decrease the likelihood of rainfall.

Black (oral communication to the Panel, August 1965) reported on plans in progress to test the method by more sophisticated mathematical models of the possible effects of localized surface heating, as well as by a medium-scale experiment that is proposed to be conducted in the coastal desert of West Australia. In the experimental field test, asphalt-blackening is to be applied in the form of cross-wind strips, with interspersed rows of stones, debris, and vegetation remnants, bulldozed together to increase the aerodynamic roughness of the surface. The outcome of such tests should be exceptionally interesting and provide information necessary to clarify the potential of the method.

New speculations for ameliorating arid climates

The inherent complexity of the thermodynamics of atmospheric circulations has been often demonstrated. New examples are the two concepts to be discussed below. The first involves, in some respects, a reversal of the "thermal mountain effect."

A new possibility for fairly large-scale weather modification is suggested on the basis of meteorological observations during a micrometeorological field program in the desert region of South Peru in 1964. This general region is extremely dry; it includes climatic stations with world records for the duration of periods without measurable precipita-

tion (up to 10 years and more) and lowest mean annual rainfall. This desert stretches along the western slope of the Andes. Here, one is in view of snow-covered peaks (up to 20,000 ft and more) in one direction, and in view of the ocean or its cloud cover in the other direction. The air is not extremely dry, and in the desert region above the stratus-covered maritime layer (at altitudes from about 3,000 to 8,000 ft), the strong insolation produces intense daytime heating of the air at low levels. Yet, in the presence of strong surface heating and of impressive real mountains, there appears to be a reversal of the concept of a "thermal mountain" inasmuch as clouds, when observed to exist in the morning, disappear during the forenoon, which suggests a downslope motion rather than one following direct circulation accelerations directed upslope. *Lettau* (1964) has demonstrated that, for sufficiently extended mountain slopes, the diurnal cycle between daytime surface heating and nocturnal cooling sets up horizontal temperature gradients away from the sloping ground, which will produce a seesaw oscillation of the horizontal pressure gradient in the lower atmosphere, which in turn generates air motions. This affects an air layer of perhaps not more than 1 mile in depth; but, if this ground surface is inclined by a slope of 1:200 to 1:800, while the crestline has a relative elevation of one mile, this must correspond to horizontal temperature and pressure gradients extending over distances of 200 to 800 miles.

If, in addition to a slope width of 100 miles or more, the range has a length of more than several hundred miles, it can be expected that any air motion generated by the above-mentioned oscillating pressure gradients is affected by the Coriolis acceleration. *Lettau* (1964) has suggested that the nocturnal low-level jet stream of the central United States (known to exist from Oklahoma to the Dakotas) can be explained by its location on the general slope between the Mississippi River and the Rocky Mountains, involving a forced oscillation in response to the above-described seesaw oscillation of horizontal temperature gradient. In this manner, a strong component of motion parallel to the contour lines of the general slope is explained. This is borne out by measurements of the low-level jet stream in north-central Nebraska during the "Great Plains Turbulence Field Program" (see *Lettau and Davidson*, 1957), as well as by a special observational program of the U.S. Weather Bureau (*Hoecker*, 1963). Peak velocities in the nocturnal south wind of more than 50 knots at about 400 m above the ground are not exceptional, with practically no wind at 1,500 m. Hoecker's data demonstrate clearly that the maximum of low-level jet development occurs at about midslope position, which is also confirmed by the statistical analysis of U.S. pilot balloon records by *Bonner* (1965).

An important point is that frictional effects when combined with the

air motion of this "thermo-tidal" structure suggested by Lettau will result in a general up-slope component in the nocturnal low-level jet stream over the central United States. This would agree with the other interesting climatic feature of a broad coincidence between the regions of maximum occurrence of the low-level jet and that of an unusually strong nocturnal thunderstorm activity.

An application of the same concept to the Pacific slope of the Andes Mountains, for the latitudinal range between 30°S and the equator, will result in daytime low-level jet stream, which should be accompanied by a daytime tendency for frictionally induced down-slope motion. The dominance of southerly winds during daytime is well documented by the migration of numerous sand dunes in the pampas of southern Peru.

If the validity of this concept is accepted, it suggests a possible method for modification of this desert climate. The remedy would be not an increase but a suppression of the slope-heating. This may be achieved either by albedo increase or by moistening of the ground by irrigation. The reddish-brown desert floor of the Pampa de la Joya was found to have the relatively low albedo of 17 percent. However, the sand is extremely dry and it was observed that the lightest sprinkling of water on the surface reduced its temperature by more than 5°C even in the early morning hours. Evaporation of a few millimeters of irrigation water per day could possibly bind heat corresponding to a few hundred calories per square centimeter per day; and this might be sufficient to suppress the thermal wind effect, especially if irrigated areas were to break up the presently continuous pattern of slope heating. There would be enough heating left to let a direct sea-breeze and mountain-breeze circulation produce cloud and rain on the higher slopes, which would serve to moisten the lower slopes even more. This scheme is admittedly highly speculative; more detailed theoretical calculation and field investigations of actual circulations are needed. However, it merits consideration because of an inherent supportive feedback mechanism. Namely, once the mountain slopes are sufficiently moist to prevent the indirect circulation, the mountain breeze will provide more showers, and so on. It should be added that, in occasional years, rainy periods can occur—the so-called Niño-rains of Peru; explanation of these in terms of disturbances of the general circulation has thus far not been possible. Furthermore, the same hypothesis may explain the desert nature of several other coastal ranges in the tropics—for example, the Somali Coast in East Africa.

A second scheme to be briefly discussed concerns a possible method of reversing the advance of some inland deserts. *Bryson and Baerreis* (1965) propose surface stabilization with the aim of reducing the dust content of the lower 10,000 ft of atmosphere over the Rajputana Desert

(Pakistan and India). They show that infrared cooling of the air due to the dust contributes significantly to the average subsidence, which inhibits convection and rainfall in that region. They provide evidence that the Rajputana Desert is man-made and that the climate of the region would be different in the absence of dust in the lower atmosphere. At the present time, the desert is said to be advancing into arable lands at the rate of half a mile a year. History suggests that the influence of man has been important in the making of this desert. Stabilization by rebuilding an adequate grass cover may be sufficient to reclaim it.

Inadvertent Modification of Atmospheric Properties and Processes

INTRODUCTION—BASIS FOR CONCERN

Until recently the sheer massiveness of the physical environment prob-
ably has been sufficient to override the influences of man's activities upon
the environment itself. We cannot maintain with any degree of cer-
tainty, however, that such conditions still hold. We have seen many
technological innovations that increase our ability to influence the en-
vironment. This is particularly true with respect to pollution. The pro-
duction of synthetic materials and their disposal as waste is affecting
the environment and many essential biologic species, although the nature
of the effects is not well understand. Many governments are greatly con-
cerned over the rate at which the population of the world is increasing,
and over the capacity of the world's resources to sustain such growth
for more than a few generations. However, although much thought is
now given to the sufficiency of the presently known resources of the
world to support populations, relatively less thought is being given to
ways in which the activities of populations may affect the availability
of resources, both biological and physical, or the ways in which man's
cultural activities may inadvertently deplete resources by altering them
so that they are no longer useful to our species.

  The fact that large-scale man-made alterations of the physical en-
vironment can be seriously contemplated serves to emphasize the great
importance of developing an understanding of our physical environment
and of its interaction with biological species. This is clearly a social
problem of the first magnitude. The rate of population growth at the
present time and the rate at which the nature of man's cultural activities
is changing indicate that we have, at best, but one or two generations
in which to understand the environmental consequences of man's cultural
activities, and to take effective action on the basis of that understanding.

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THE INCREASE OF CARBON DIOXIDE IN THE ATMOSPHERE

Observed changes in atmospheric CO₂

The importance of carbon dioxide for life aroused early interest in the cycle of carbon on earth. The most important part of this cycle is the exchange of carbon dioxide between the atmosphere, on the one hand, and the oceans and the terrestrial biosphere on the other. The oceans do not represent a uniformly mixed reservoir; they contain carbon in a variety of forms. For reasons of quantitative consideration, it has proved useful to subdivide the ocean into two layers, one a shallow layer above the thermocline and the other the deep sea. The upper layer has an average depth of 50 to 100 m, is well mixed by the action of wind and waves, and contains most of the marine life. The deep sea moves slowly, with an overturn time of the order of several hundred years. Carbon dioxide is exchanged rapidly between the upper layer and the atmosphere, whereas the rate of exchange between the upper and lower layers is much slower.

The terrestrial biosphere consists of the living material of plants and animals and the dead material of humus deposits. A schematic diagram of the size of the various reservoirs and of the carbon cycle is given in Figure 2; it was first presented in this form by *Craig* (1957) and is now widely accepted.

A slower exchange is superimposed on the rather fast transfer between the oceans and the atmosphere. It involves rock weathering and the dissolution of precipitation of carbonates in the ocean, and is estimated to result in an overturn of marine carbon once in 10,000 years (*Brown*, 1957). This slow cycle is of importance for geology and geochemistry but can be disregarded for our discussions here.

Based on a survey of available carbon dioxide measurements, *Callendar* (1940) came to the conclusion that the amount of CO₂ had increased by about 10 percent since the turn of the century, and he suggested that this

FIGURE 2. Carbon cycle of the atmosphere and the ocean, essentially according to Craig (1957). C denotes the global average carbon density (in grams per square centimeter of earth's surface) for the various reservoirs: atmosphere (a), mixed layer (m), deep sea (d), plants on land (b), and humus (h). τ is the residence time (in years) in exchange with the respective reservoirs. The height of the columns is proportional to the carbon density.

Atmosphere

$C_a = 0.13$ grams/cm²

Ocean   **Land**

$\tau_{am} = 5$ $\tau_{ma} = 6$ $\tau_{ab} = 33\,\tau_{ba} = ?$ $\tau_{ha} = ?$

Mixed Layer   **Plants** τ_{bh} **Humus**

$C_m = 0.15 = 1.2\,C_a$ $C_b = 0.06 = 0.5\,C_a$ $= ?$ $C_h = 0.21 = 1.7\,C_a$

$\tau_{md} = 4$ $\tau_{dm} = \sim 300$

Deep Sea

$C_d = 7.3 = 58\,C_a$

Carbonate in sediments $= 28500\,C_a$

Organic carbon in sediments $= 10600\,C_a$
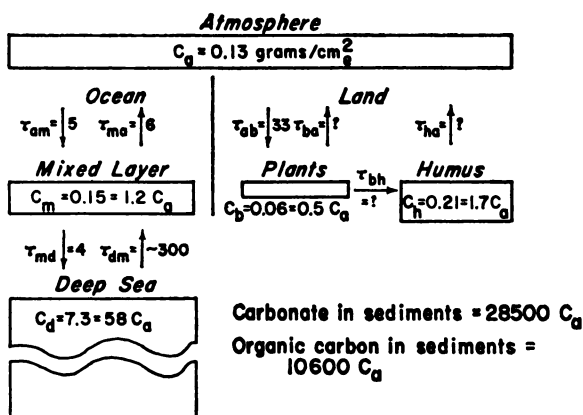
was due to the increased burning of fossil fuel by man. At the time of his original suggestion, the available data were not numerous and consistent enough to prove that an increase had really occurred. The data from the last century are scattered considerably over such a range that it appeared almost hopeless to draw any conclusions from them. Since that time, however, the monitoring of the CO_2 content of the atmosphere has received considerably more attention, and the data are much better. As of now, the general consensus is that the increase is real. In the latest of a series of papers on the subject, *Callendar* (1958) systematically selected the reliable sources and arrived at the conclusion that, prior to the year 1900, the average CO_2 concentration in the Northern Hemisphere was 290 parts per million (ppm) and that, since then, there is proof of a fairly constant increase up to the present value of 330 ppm. The data on the secular increase of carbon dioxide concentration in the atmosphere agrees quite well with the rate of its production by man's consumption of fossil fuels. The total observed increase represents a large fraction of the total CO_2 produced by burning of fossil fuels.

The ocean is a huge reservoir of carbon dioxide in exchange with the atmosphere, and a considerable portion of the artificially produced carbon dioxide should have been absorbed by the ocean, leaving only a fraction in the atmosphere. If this had proved to be the case, it would be necessary to invoke additional sources of carbon dioxide, such as changes in the amounts of carbon stored in the biosphere, a slight increase of ocean surface temperature, or similar possibilities. However, the oceans present a peculiar buffering mechanism by which a 1 percent increase in the carbon dioxide concentration of sea water is balanced by an approximate 10 percent increase of the carbon dioxide partial pressure in air. Thus it looks as if, because of this buffering mechanism, most of the carbon dioxide produced by man has remained in the atmosphere, and as if Callendar's original suggestion was correct.

The fair agreement between the observed increase of the total atmospheric carbon dioxide and the amount produced by fossil-fuel consumption seems to make it unnecessary to invoke other CO_2 sources, as suggested by several authors. *Revelle and Suess* (1957) pointed out and *Kanwisher* (1960) demonstrated that the CO_2 concentration of the atmosphere should be very sensitive to changes of the temperature of ocean surface waters. The same should be true for changes in the reservoir of carbon in the soil and in the amount of organic matter in the ocean. The estimates indicate that the increase in arable land since the middle of the nineteenth century might have resulted in a 4 percent increase of atmospheric carbon dioxide and that a 1 percent change in the concentration of organic material in the sea might have the same result. Based on measured variations of the ratio of carbon-14 to carbon-12 in trees of known age, there are indications of fluctuations in the

atmosphere prior to 1850 that certainly must have arisen from causes other than fossil-fuel consumption. (The carbon-14 in the atmosphere is produced in interactions between cosmic radiations and atmospheric nitrogen and oxygen nuclei. It is radioactive and, although it is found in plant material, its decay is rapid enough—half-lifetime equals 5.7×10^3 years—so that it is not found in fossil fuels.) Generally this ratio seems to have decreased since 1500 (*De Vries*, 1959), but it is difficult to give any explanation of these observations at the present time. These long-range fluctuations of the atmospheric CO_2 content may indicate how delicate and perhaps unstable the carbon dioxide budget of the earth is, and that it is worth every effort to pursue these questions further. (The preceding description of the carbon dioxide cycle and of the secular increase of its concentration has been adapted from *Junge*, 1963a.)

Possible effects of increased CO_2

Erickson (1963) has studied in detail the effects of variation of the properties of the sea on the atmospheric CO_2 concentration, using the equilibrium equations of the CO_2 system in sea water. His general conclusion is that neither temperature nor volume changes of the sea can have been large enough during the past few thousand years to cause any appreciable change in the atmospheric CO_2 concentration, at least not of any climatic importance. He does point out, however, that there is a considerable excess of carbon dioxide in the ocean, owing to a combination of biological and gravitational processes, which, if released, would increase the atmospheric concentration by a factor of 5. Thus, serious disturbances in the biological circulation of carbon in the sea in the distant past may have caused wide fluctuations in the atmospheric CO_2. However, this aspect of the picture is quite uncertain at the present time.

The energy input to the earth is derived mainly by the absorption of visible solar radiation at the earth's surface. The absorption of that energy tends to raise the temperature of the earth, and, in turn, the earth maintains its temperature balance by reradiating energy to space at longer wavelengths. The absorption of incoming solar radiation by carbon dioxide is so small that changes in its concentration will have no appreciable effect upon the rate of transmission of energy from the sun to the earth. On the other hand, carbon dioxide is quite opaque to certain bands of the long-wave radiation by which the earth loses energy to space. Therefore, variations in the carbon dioxide concentration in the air change the long-wave radiation characteristics of the atmosphere and, thereby, change the heat loss by radiation from the earth's surface. Increasing the CO_2 content increases the atmospheric radiation or diminishes the radiation loss from the surface (a "greenhouse" effect). The disturbed radiation budget will be compensated for, all other things being

equal, by an increased surface temperature. Considering that the eventual consumption of the world's reservoir of fossil fuels over the next few centuries could easily double the amount of carbon dioxide in the atmosphere, many authors have speculated on the possible changes in the atmosphere and in the global climate because of the resulting change in the radiation budget. Many of these calculations, especially those giving the greater increases of temperature, were very crude in that they did not sufficiently take into account the natural complexity of the earth's atmosphere.

Plass (1956) and other authors have discussed the older theory that a cause of climatic variation during geologic and recent times has been the variation of the CO_2 content, and they have estimated the resulting temperature changes. The supposed increase in atmospheric temperature and the climatic changes deduced from speculations based on this hypothesis have received much popular notice and have aroused concern from time to time. However, several authors, notably *Kaplan* (1960a, 1960b), have attacked calculations made by Plass and pointed out that Plass did not take into account the influence of cloudiness on the radiation. Recently, *Möller* (1963) has shown that the larger values of the temperature change calculated by Plass are valid only for a dry atmosphere. The overlapping of the infrared absorption bands of carbon dioxide and of water vapor near 15 μ wavelength substantially diminishes the temperature changes. Möller presents new calculations giving a rise in surface temperature of 1.5°C when the carbon dioxide content of the atmosphere increases from 300 to 600 ppm. In his model, cloudiness reduces the radiation effects but not the temperature changes because, under cloudy skies, larger temperature changes are needed in order to compensate an equal change in the downward long-wave radiation. The increase in water-vapor content of the atmosphere with rising temperature causes a self-amplification effect that results in almost arbitrary temperature changes; for example, for constant relative humidity (rather than constant absolute humidity) the temperature increase would be 10° in the above-mentioned case. Möller has shown, however, that the changed radiation conditions are not necessarily compensated for by a temperature change. The effect of an increase in carbon dioxide from 300 to 330 ppm can, in his model, be compensated for completely by a change in water-vapor content of 3 percent or by a change in the average cloudiness of 1 percent of its value without the occurrence of temperature changes at all. Thus, the theory that climatic variations are caused by variations in the carbon dioxide content becomes very questionable.

The extreme sensitivity of Möller's results to the assumptions with regard to humidity indicates that calculations of the effects of variations of CO_2 concentration based on such simplified models are probably

suspect. Computations on more sophisticated numerical models are now possible, however. For the purpose of this survey, *Manabe et al.* (1965), at the Geophysical Fluid Dynamics Laboratory of the Environmental Science Services Administration, examined the dependence of the earth's surface temperature on changes of the CO_2 content of the atmosphere, using a thermal equilibrium model of the atmosphere (*Manabe and Strickler*, 1964).

The result of their computation indicates that the larger the CO_2 concentration the colder is the stratosphere and the warmer is the troposphere. The magnitude of the effect is quite small. Table 6 shows the computed equilibrium temperature of the earth's surface for several concentrations of CO_2 and the temperature changes that would result from several conceivable changes in concentration. The corresponding atmospheric temperature changes are very nearly the same throughout the troposphere, while in the stratosphere (at, say, 40 km) the changes are of the opposite sign and an order of magnitude larger. These computations were based on the assumption of a fixed absolute humidity. If, on the other hand, the distribution of relative humidity rather than absolute humidity had been assumed constant, the computed temperature changes would have been twice those shown in Table 6. Manabe also points out that he found a much smaller dependence of the resulting temperature changes on the choice of the assumption as to the humidity than did Möller, since Möller discussed the heat balance of the earth's surface instead of that of the atmosphere as a whole. Although the recent

TABLE 6. *Relation between the CO_2 Content and the Equilibrium Temperature of the Earth's Surface*

CO₂ Concentration (ppm)	Equilibrium Temperature (°K)	
	50% Cloud Cover	No Clouds
600	288.2	301.7
300	287.0	300.4
100	285.2	298.4

Change of CO₂ Concentration (ppm)	Change of Equilibrium Temperature (°K)	
	50% Cloud Cover	No clouds
300–330	0.14	0.2
300–600	1.2	1.4
300–150	—1.2	—1.4
300–100	—1.8	—1.8

calculations provide better similitude to the atmosphere than Möller's model, allowance for interactions with the general circulation could materially alter the conclusions. Calculations incorporating the latter interactions will be forthcoming within the next year or two.

Möller comments that there may be two reasons why the CO_2 variations have so often been assumed to be causes of climatic variations. (1) The CO_2 content of the atmosphere is so remarkably uniform over space and time that it is possible to observe long-term variations in its mean value. This is impossible for almost any other factor that can influence the radiation process. Cloudiness, water vapor, and temperature show strong variations with day, season, latitude, and between oceans and continents. Observations are so scarce over about 60 percent of the earth's surface that a secular variation in the latter factors cannot be recognized. Carbon dioxide content is essentially the only factor whose secular variation we know. (2) The influence of carbon dioxide variation on the long-wave radiation seems to be evident because the relevant physical mechanism is quite clearly understood. It is, however, much more difficult to interpret the ultimate meteorological meaning and effect of CO_2 on the total radiation balance.

It is perhaps worth noting that, even in the more extreme estimates of the possible climatic consequences of increased atmospheric CO_2, the calculated temperature changes have been of the order of a few degrees, generally less than five or ten. From glacial-geologic data, it is known with some certainty that North America and Europe have, since the last maximum of the Wisconsin Glaciation, experienced climates that have averaged several degrees warmer than the present. As mentioned earlier, although some of the natural climatic changes have had locally catastrophic effects, they did not stop the steady evolution of civilization.

Although dire predictions of drastic climatic changes resulting from the increasing carbon dioxide content of the atmosphere as a result of human activities may well be unjustified, it is clearly important that the secular changes of carbon dioxide be followed with great care. First, it is, as Möller has pointed out, one of the few atmospheric parameters for which a secular variation can be measured. Additionally, *Bolin and Keeling* (1963) have pointed out that carbon dioxide is a useful tracer for the study of atmospheric mixing processes, so that the monitoring of atmospheric carbon dioxide should be a significant factor in our continued study of the atmosphere and, most importantly, in any study of the interchanges between the atmosphere, the biosphere, and the oceans.

The nature of the concern with which the secular increase of carbon dioxide in the atmosphere should be viewed was well expressed by *Revelle and Suess* (1957) who stated: "Human beings are now carrying out a large scale geophysical experiment of a kind that could not have

happened in the past nor be reproduced in the future. Within a few centuries we are returning to the atmosphere and oceans the concentrated organic carbon stored in the sedimentary rocks over hundreds of millions of years. This experiment, if adequately documented, may yield a far-reaching insight into the processes determining the weather and climate."

EFFECTS OF URBANIZATION

Air pollution and smog

The production of air pollution and smog is perhaps the most widely known way in which mankind, by his activities, inadvertently modifies his environment. The statement by Revelle and Suess on the secular increase of carbon dioxide, quoted in the preceding section, is perhaps even more applicable to this area because, with the introduction of trace substances into the atmosphere, particularly in urban areas where large quantities are released, mankind is engaged in another great geophysical experiment with no control, with inadequate gathering and evaluation of data, and with a nearly complete lack of understanding of the possible long- or short-range climatic consequences of the experiment.

It is not appropriate to go into detail on the subject of air pollution and smog in this report. The chemistry of air pollution has been treated comprehensively by *Leighton* (1961) and others. A concise description of the role of air pollution in atmospheric chemistry, from which portions of this section have been adapted, has also been given by *Junge* (1963a). The production and the control of the production of pollutants, the photochemistry of smogs, the role of meteorological factors in permitting or stimulating the build-up of pollutants on the micro- or meso-scale, and the role of weather systems in dispersing pollutants have been extensively studied. A comprehensive survey of the present situation has recently been completed by the Air Conservation Commission of the Committee on Science in the Promotion of Human Welfare of the American Association for the Advancement of Science (AAAS). The full report is soon to be published in book form by the AAAS, and it has been summarized by *Dixon and Lodge* (1965). Although the effects of pollutants and smog upon people, plants, and economic activities have been extensively studied, as has been meso-scale meteorology as it influences the accumulation or the dispersion of pollutants, relatively little attention has been paid to effects of pollution and smogs on a larger geographic scale or upon climatic patterns. Understanding of their influences upon long-term climatic trends is inadequate.

The modification of the air over urban areas, sometimes referred to as the "breathing of the city," has been described as comparable to the effects of an active volcano (*Kratzer*, 1956). The combustion output of gas, oil, gasoline, and refuse in a city like Los Angeles in the early 1950's has been estimated to amount to about 50,000 tons per day. In many of the larger West European cities, total consumptions are similar, usually also including a significant output of the combustion products of bituminous coal and lignite. A single large industrial plant, using fossil fuel, may emit several hundred tons of sulphur dioxide per day. Thus, transit of air across a city can increase the particulate loading from a rural value of several micrograms to nearly milligrams per cubic meters.

Many climatic elements are affected by pollution—radiation, cloudiness, fog, visibility, and the atmospheric electric field. Temperature and humidity are influenced indirectly, and effects on precipitation are also suspected. Pollution climatology is recognized as a basic problem of the climatology of modern industrial towns and cities. The extent of our knowledge of the effects of pollutants and smogs on the microclimatology of towns, as well as our knowledge of the temperature, precipitation, and radiation abnormalities that may be influenced by pollutants, has been thoroughly summarized by *Landsberg* (1956). As an immediate consequence of air pollution, the optical clarity of the air is affected. Solar-radiation intensity in three central European cities was found to be reduced by 11 to 36 percent (depending on the sun's elevation angle). Most obvious is the reduction of average visual range, which has been reported in nearly all growing cities and towns. Together with a conspicuous increase in the number of foggy days per season, this has increased the hazards to transportation on streets and airfields.

The definition of a polluted atmosphere is to a considerable degree arbitrary, and is usually based on standard levels of concentration set by air-pollution control agencies. These concentration standards are high compared with the background levels, and data on the chemical composition of the atmosphere in industrial and populated areas are generally not available unless these standards are approached or other nuisance factors occur. For practical reasons, the interest of pollution-control authorities is often concentrated on components that may not be the most important ones as far as effects on weather and climate are concerned, and only in a few cases—in Los Angeles, for example—do the studies consider a large variety of constituents. The selection of constituents to be studied in polluted areas is determined by their potential hazard to health and to plant life, or by other unpleasant properties. From a physical or chemical point of view, these constituents are sometimes poorly defined as, for example, "smoke," "dust," or "oxidants." For these reasons a direct comparison with natural trace substances is

possible only for a very limited number of compounds—SO_2, NO_2, and a few others.

The effects of air pollution depend to a considerable degree on secondary reaction products. The formation of such products is controlled by the relative composition of polluted atmospheres and by the climate. Broadly speaking, one can distinguish two types of pollution: the sulfur dioxide, sulfuric acid, sulfate, reducing-type pollution and the oxidation-type air pollution. Famous for the first type is London, where smoke and sulfur dioxide (SO_2) accumulate in moist, stagnant air masses during the fall season. Under these conditions sunshine is not able to destroy the ground-temperature inversion, and the conversion of sulfur dioxide to sulfuric acid (H_2SO_4) or sulfate ion (SO_4) on smoke particles is favored. The major, immediate effects of this smog are a reduction of visibility and an increase in mortality due mainly to respiratory complications.

The second type of smog is most serious in the Los Angeles Basin and similar areas. The accumulation of material in the ground layer is typically favored in these cases by the topography and by the persistence of pronounced temperature inversions at low elevations in the atmosphere. The relatively large amounts of nitrogen oxides and various hydrocarbons released by auto exhausts and other sources result, when exposed to the sunshine that is also typical of these areas, in the formation of ozone and a number of other oxidants by photochemical reactions. Eye irritation and plant damage appear to be the main manifestations of this type of smog. The variety of pollutants released in cities and industrial communities, and the variety of climatic and geographical conditions, cause a corresponding variety of smog characteristics that modify these two basic types.

Most of the material injected into the atmosphere by human activities is not deposited within the source area but is removed by air motion. Once this material has been thus dispersed from areas of high concentration, it is of little concern for air-pollution research. The ultimate fate of this material in the atmosphere, however, is a subject of special interest concerning which progress in understanding is only now beginning to be made. Essentially nothing is known of the role of these substances in the alteration of long-term climatic trends and of weather patterns. Also not known or understood is the extent to which, in the more-developed countries, the immediate problem has grown from a micro- and meso-climatic one, affecting cities or towns, to one affecting entire countries or subcontinental areas. Pollutant particles, after being blown away from areas of high concentration around cities and towns, are still available to participate as nuclei for the condensation or freezing of atmospheric water vapor or cloud droplets and, also, to take part in other atmospheric physical and/or chemical processes.

The natural distribution and concentration of condensation and ice-forming nuclei result from what might be called "natural pollutants." The more important of these are volcanic dust and gases, sea-salt particles, dust from wind erosion, pollen, spores, bacteria, and smoke from forest and brush fires. In an absolute sense, the atmosphere has never been free of pollution. Pollutants once injected into the atmosphere do not become permanent, but their residence times vary widely. The most important scavenging mechanisms are gravitational deposition and the removal of gases and particulates by rain and snow.

The natural pollutants constitute by far the greatest source of condensation and ice nuclei. It has been found that there are geographical and temporal variations in their concentration, induced by the characteristics of their sources, by the variations of the general circulation of the atmosphere, and by changes in the rate of scavenging. These variations undoubtedly have played a major role in establishing and maintaining the worldwide distributions of precipitation and climate to which mankind has adapted his culture. The marked difference between continental and maritime clouds attributable to the differences in the concentration of condensation nuclei has been previously mentioned.

The increasing production of man-made pollutants, especially in the temperate zones, is a cause for concern that the worldwide pattern of nuclei may be undergoing significant changes. Large areas of the developed nations, such as Europe and the United States with their exploding "megalopolises," now have extensive regions in which the concentrations of artificial pollutants are constantly high enough to be of possible meteorological significance. Having, by our own activities, changed the distribution and concentrations of nuclei over large areas, should we not now be concerned that we also may have changed the average frequencies and distribution of clouds, fogs, and precipitation? In some regions in which the normal concentration of natural ice nuclei is low, it is conceivable that by the addition of pollutants we may have increased the concentration of such nuclei and possibly, thereby, the rainfall. On the other hand, in regions in which the natural concentration of condensation nuclei is quite large, there is a danger that, by further increasing the available supply of nuclei, the occurrence of fine fog might be increased and the probability of rain decreased.

These are only samples of the kinds of questions relating to meteorological effects of air pollution that will become more insistent in coming years. If pollution becomes heavier and much more widespread, our attention *must* turn to the question of effects upon the atmospheric radiation balance and, in turn, upon the general circulation and resulting weather and climate. If we accept the latter possibility of continued

growth of pollution, the present is none too soon to begin the required detailed scientific examination.

Other effects of urbanization

In addition to the release of air-polluting agents, another direct consequence of urban fuel combustion is the release of considerable heat. Throughout the year, the natural supply of solar heat at the earth–air interface amounts to several hundred calories per square centimeter (langleys) per day, depending on season and geographic latitude. *Kratzer* (1956) quotes for Vienna a rate of 20 langleys per day from combustion of coal alone, which, at this latitude, approaches half of the insolation rate during wintertime. In addition, heat is released by the consumption of electric power. Comparative figures are readily obtained using the convenient relation that 2 langleys per day are approximately equal to 1 W/m^2; thus, 2 langleys per day would be released in consuming the output of a 100,000-kW power plant serving a city area of 10^8 m^2 (100 km^2). While the heat release in cities formerly reached a pronounced peak intensity only in the winter season, the growing use of indoor air conditioning has also created significant peak values in summertime. Body heat, generated at normal rates of a few thousand kilogram-calories per day per person, will amount to totals of meteorological significance only occasionally when large numbers of people are crowded closely together, as in stadiums or arenas during outdoor events.

The profound alteration in ground structure by urbanization must also be considered as an important influence for climatic change. In building a city, fields, grassland, or forests are replaced by vast expanses of rocklike formations such as concrete, brick, stone, roof coverings and asphalt. Of most direct importance for processes of heat conduction and convection are the albedo and the thermal properties of the materials. The albedo of pavements and walls is relatively high; that of roofs perhaps lower than that of the surroundings of a city. In the over-all picture, the built-up area tends to reject more sunlight (appears lighter from the air) than the surroundings. Thermal admittance and, consequently, subsurface heat storage during daytime will be increased. Quite definitely, the aerodynamic-roughness parameter of the town will be considerably larger than that of fields. Most important, however, will be the extreme dryness of pavements and roofs, since great care is usually taken to drain off, as efficiently and rapidly as possible, all local precipitation, and to clear snow from the pavements in winter time. The latter process is supplemented in many instances by the spreading of salt in considerable quantities. This may have an effect on microclimatological conditions.

Residential areas with lawns, scattered trees, extended gardens, and pools will not be as dry as downtown areas.

Climatic trends in cities

For the purpose of a precise study of climatic effects, it is difficult to define a "city," and a certain degree of arbitrariness appears unavoidable. It must be assumed that any slight change in natural ground cover will modify an existing microclimate (as discussed by *Geiger*, 1965), and types of human communities range from groups of huts to metropolitan complexes. Thus the assessment of "city climate," in a simply defined sense, is nearly impossible. The best practical approach is to investigate climatic trends for a large number of observing stations with continuous records over periods during which a city grew around them. The task is made difficult because many towns and cities have been founded and built in spots where the background climate, due to topographical and geographic preferences, was complicated and poorly documented.

Summarizing from the results of detailed studies (for example, those by Landsberg or Kratzer), it can be said that the climatic records show a persistent decrease of average wind speed and an increase in city temperature with the growth of urban areas. This concerns annual and seasonal means, as well as daily maximum and minimum temperatures. Also, urban areas have lower relative and absolute humidities than the countryside, even though the climatic records for most cities show gradual increases in cloudiness. This seeming paradox appears to be a result of air pollution (the excess of condensation nuclei), but, possibly, it is also a result of the direct warming of the air in the smoke pall of the city. Locally, increased heating of low-level air, especially in the late afternoon, stimulates convective activity, with resulting cumulus growth and possible rain showers. During periods of extensive glider flying in Central Europe in the early 1930's, there were many occasions when pilots, to gain altitude for soaring, searched for and benefited from regularly developing daytime "thermals" in fair summer weather over large cities.

In many growing towns, rain gauges have produced records showing systematic increases of the order of 10 percent, relative to the surrounding area, within two decades. Also, consistent with relatively high temperatures, precipitation in the form of snow tends to become less frequent over a city. On the other hand, there are occasional reports of snowfall occurring exclusively over heavily industrialized towns, with no simultaneous precipitation in the surrounding country. *Landsberg* (1956) discusses such reports, which appear to indicate the possible local nucleation of supercooled stratus clouds at temperatures of about −4°C.

Numerous reports in the literature document city effects in terms of

diurnal variations of weather elements during periods of generally fair weather. Such studies show that in extreme cases, during nocturnal calms, the air a few feet above street level between houses may be more than 10°C warmer than in open country. It has been found that increased general cloudiness, as well as generally stronger winds, tends to reduce the temperature difference between city and country. On the other hand, contrasts sharpen when the countryside is snow-covered and the city has no snow. Extended weather periods of low over-all air motion aggravate pollution conditions in urban areas and may lead—and have led—to truly catastrophic cases of smog.

The city effect on climate is admittedly highly complex. It can be reduced, however, to fundamental physical, meteorological, and climatological principles once the changes in boundary conditions and the nature and source strength of exhalations are known and surveyed. It can be said that the single meteorological factor that dominates the city effects is the degree of natural atmospheric ventilation. The important feature is this: with increasing area of metropolitan complex, natural ventilation will be reduced, and the pollutants and heat emission will be increased. The city effect provides the equivalent of a continuing experiment in climate modification, and study of it should be intensified in detail and scope.

Effects of Forestation, Deforestation, and Agriculture

Forests and the weather

There have long been popular notions that large-scale deforestation leads to decreased precipitation. A variety of nineteenth-century ideas along these lines were well summarized by *Marsh* (1874). *Webb* (1931) documents similar notions that arose repeatedly in the course of the opening of the semiarid western plains of the United States. This same viewpoint is all too easily inverted into schemes for increasing regional precipitation by planting trees; and an instance has been cited (*McDonald,* 1962) in which the argument was seriously made that the states of Oregon and Washington attested to the rain-producing influence of extensive forests. If there are any real effects of forests upon local precipitation, they are almost certainly much more subtle than the effects envisaged in popular plans to increase rain by tree-planting, and the real climatic effects (changes of surface roughness, albedo, and subsurface supply of moisture) seem likely to be relatively small on a continent-wide scale.

There are, however, a few direct hydrological benefits to be derived from tree planting or maintaining a certain density of forests on moun-

tain ranges. Winter snows are collected and maintained at a more uniform depth than on bare ground, and the snow-melt rate is more uniform. More important, however, a forest canopy provides protection against evaporation from the snow surface, thereby increasing spring and summer melt-water to feed rivers and reservoirs. Also, in some areas a major component of vegetation water supply is obtained through the collection of drifting fog by the branches and needlelike leaves of adapted trees.

Related to this process of relatively efficient collection or extraction, by vegetation, of properties of moving air is the extraction of horizontal momentum (the dissipation of the kinetic energy of the wind). In a University of Wisconsin thesis in 1963, E. C. Kung has investigated the climatology of the mechanical-energy dissipation in the lower atmosphere over the Northern Hemisphere. He makes use of boundary-layer concepts and empirical drag coefficients for various types of vegetation, employing ecological land-use charts for the continents. The drag effects of natural forests appear to cause substantial increases in boundary friction. A continuation of this work by Kung is presently under way at the Geophysical Fluid Dynamics Laboratory of the Environment Science Services Administration.

The albedo of evergreen forests is relatively low, and it contrasts significantly with that of surrounding fields, especially if the latter are snow-covered and if the tree tops have shed their snow mantle. Such conditions occur frequently. On clear days with good insolation, plenty of heat is absorbed and re-emitted by the forests, and light airplanes often encounter increased turbulence when flying over such sun-heated woods. It is possible that such conditions can stimulate local convective clouds if the horizontal extent is great enough. An experimental field study of such heating conditions would be valuable in support of weather-modification models based on localized surface-heating.

Agriculture and local climate

Another recurrent question is whether, particularly in arid regions, agricultural operations exert perceptible modifying effects on regional climate. When irrigation is carried out in arid regions, it becomes tempting to suspect that the enhanced evaporation alters the local climate. There is, to be sure, good micrometeorological evidence that humidities are increased within the crop stand, and for very short distances above and downwind from the irrigated area. But these effects appear to be diluted very rapidly as one moves away from the immediate vicinity of the irrigated areas. *Ohman and Pratt* (1956) have studied humidities near a 100,000-acre desert irrigation project near Yuma, Arizona. Within the crop stand, dew-point temperatures were elevated by 6 to 8°F above

ambient levels, but, at a mere 12 ft above the crop level, dew points were not measurably increased, even at locations lying entirely within the irrigated area. Even more strikingly, at points only 100 ft downwind from the edge of this large irrigation project (extending about 20 miles in the direction parallel to the wind at times of measurement), dew-point increases were too small to detect instrumentally. Repeated efforts to detect any marked difference in summer humidity conditions between Tucson and Phoenix, Arizona, have disclosed no indications of significant differences despite the fact that Phoenix lies in the midst of a very large area of irrigated fields whereas the Tucson area has very little irrigated agriculture. Such small differences as do show up between these two nearby cities amount only to a degree or two of dew point, and appear to be overwhelmed by naturally produced humidity differences accompanying onset of the summer thunderstorm season.

For an extremely interesting and penetrating comprehensive discussion of interrelationships between agriculture and climate, we can refer to the proceedings of an international symposium on "Man's Role in Changing the Face of the Earth." In this publication (*Thomas*, 1956), the processes of environmental change by man through increasing intensity of land use, stimulation or suppression of soil erosion, forestation and deforestation, and other activities, are thoroughly reported, notably by P. S. Sears. C. W. Thornthwaite deals specifically with modification of rural microclimates. If an over-all summary conclusion is possible, it can be said that statistical evidence on climatic effects is meager and controversial, especially concerning the effect beyond the immediate vicinity of the modified region. For example, nobody denies that a point formerly covered by the shallow waters of the Zuider Zee in Holland is radically changed in temperature and moisture by the successful reclamation of land and its use for field crops. But, beyond the immediate vicinity, climatic effects of the modification rapidly diminish.

EFFECTS OF SUPERSONIC TRANSPORT AIRCRAFT ON THE STRATOSPHERE

When supersonic transports come into widespread use, they will introduce larger quantities of water vapor into the stratosphere than has resulted from any earlier activities of man. If there is a clear expectation that the introduction of such quantities of water will affect climate, additional constraints might be placed on the aircraft design to avoid harmful effects in the atmosphere.

We can readily imagine two effects that might be important. One would be that persistent contrails might form to such an extent that there would be a significant increase in cirrus clouds. The other effect

would be a significant increase in the relative humidity of the stratosphere, even if there were no significant increase in the extent of cirrus cloudiness; both could affect the radiation balance and possibly, thereby, the general circulation.

It may be reasonable to expect that 400 supersonic transports will come into service; this is the number suggested by former Federal Aviation Administrator Halaby at the time when he was trying to persuade industry to make a substantial contribution to the program. Industry sources have suggested smaller numbers. The transports will fly in the altitude range 50,000 to 70,000 ft, or roughly 16 to 22 km; thus, they will normally fly in the stratosphere in both summer and winter. They will carry fuel loads of about 100 tons, about two thirds of which will be consumed at flight altitude; the weight of water vapor released is about 40 percent greater than the weight of the fuel consumed. If four flights per day can be scheduled for each aircraft, this amounts to 150,000 tons per day of water vapor introduced into the stratosphere (this is comparable to the injection of water vapor into the stratosphere by a single large cumulonimbus cloud in the tropics). The water-vapor content of the stratosphere is about two parts per million by mass. The total mass of the atmosphere in the 16 to 22 km altitude range in which the flights will take place is about 3×10^{20} g, and the total water vapor naturally present is therefore about 6×10^{14} g, or 6×10^{8} tons. Thus, the supersonic transports may release an amount of water vapor per day that is 0.025 percent of that naturally present in the altitude range in which the flights occur.

The contamination may be more significant locally. There should be a high concentration of flights over North America and the North Atlantic. If half of the activity is concentrated over 5 percent of the earth's surface, the local contamination should be 10 times larger than that calculated above on a global basis, or about 0.25 percent per day of the naturally present water vapor. However, as the horizontal mixing time in the stratosphere is of the order of a few weeks at most, the local concentration of water vapor from flights on crowded routes will probably be rapidly spread out and be of no real significance.

The possibility must be examined that the residence times for stratospheric contamination are so long that significant concentrations can build up from supersonic transport operations. If the contaminants introduced into the lower stratosphere remain there on the average of 10 years (about 10 times longer than is presently suspected), supersonic transports could double the concentration of water vapor naturally present. This would affect the radiation balance, but not in a very important degree, according to model calculations of *Manabe* (1965). Assuming fixed relative humidity, Manabe finds that a fivefold increase of

stratospheric water vapor would raise the temperature of the earth's surface by 1.6°C. Further, lifetimes as long as 10 years are unlikely, especially for water vapor. Manabe finds, using a numerical model, that large-scale quasi-horizontal eddies are very effective in removing moisture from the high- and middle-latitude stratosphere by freezing out near the cold equatorial tropopause. Even for constituents that do not freeze out, lifetimes as long as 10 years in the stratosphere do not appear likely, in view of the observed residence times of radioactive tracers in the stratosphere.

Increased cloudiness due to persistent condensation trails does not appear to be a hazard even if a large relative increase in stratospheric moisture should occur. This conclusion results from consideration of the extreme dryness of the unperturbed stratosphere.

CONTAMINATION OF THE VERY HIGH ATMOSPHERE

Meteorological aspects of the problem

The projected frequent use of large rockets has raised the question of the degree to which contamination and inadvertent modification of the tenuous regions of the upper atmosphere may take place. The possibility of contaminating or modifying the atmosphere between 50 and 120 km depends upon the rate at which impurities are introduced and the rates at which they are cleared from the atmosphere by natural processes. Small-scale contamination will, in general, be of fairly short duration, as winds or diffusion will carry the materials away from the point where they are introduced. The local and short-duration modification may be of military significance, but it is of doubtful significance in general atmospheric processes; the effect is similar to that of smoke screens near the ground.

The amount of contaminants that can be introduced into the upper atmosphere by rockets or bombs is substantial and may produce significant effects. Exhausts from the largest planned rocket motors will introduce amounts of water vapor above 100 km that are of the order of 0.3 percent of the worldwide total of atmospheric water vapor above that altitude (*Kellogg*, 1964). This will surely cause some local disturbance of the photochemistry or radiation balance of the upper atmosphere. Bomb explosions have introduced amounts of lithium into the upper atmosphere that completely overwhelm the naturally present lithium; the lithium twilight emissions have at times been almost entirely due to the bomb-induced impurities.

The magnitude of upper-atmosphere contamination is a function of several natural processes. Principal among them are: (1) mixing pro-

cesses; namely, small-scale isotropic or vertical eddy-mixing and large-scale circulation including large-scale quasi-horizontal eddy motion; and (2) processes that serve to remove the impurities altogether. In addition, the natural abundances and sources of minor (or exotic) atmospheric constituents must be considered as a basis for judging the relative effects of artificial contamination.

Mixing processes control the rate at which impurities are spread through the entire atmosphere, including the regions where impurities are removed (for example, by precipitation in the troposphere or by escape from the exosphere). If the factors enumerated above were fully understood, one could predict the contamination that would be produced by any given source. It would also be possible to compute the abundance and distribution of minor atmospheric constituents if the sources were known quantitatively. (For example, the source of atomic sodium is uncertain.) It is also necessary that the life cycles of minor constituents, such as lithium, be understood in order to specify whether or not a particular operation will produce a significant perturbation of those constituents.

The average values for vertical eddy-mixing are uncertain, even in the lower atmosphere where many data are available. In the lower atmosphere, however, mixing is relatively rapid, so that, in considering lifetimes, we will emphasize regions other than the troposphere.

Kellogg (1956) has pointed out the difficulty in obtaining diffusion rates from observations of the spreading of smoke puffs. The effect of wind shear in spreading the trail is a complicating factor; and even when this effect is removed, the spreading is not in agreement with the theory of isotropic turbulence. Consequently, it is necessary to talk in terms of average values for mixing and sometimes to arrive at values by rather indirect arguments.

In the stratosphere, and especially at higher altitudes, it is difficult to obtain data from which eddy-diffusion coefficients can be determined. From water-vapor measurements, *Brewer* (1949) found a value for the eddy-diffusion coefficient of 2×10^3 cm²/sec at the tropopause. From observations of tungsten-185 injected in the lower stratosphere by the "Hardtack" nuclear tests, a value of 10^3 cm²/sec was obtained at 20 kilometers altitude in tropical latitudes and 10^4 cm²/sec at temperate latitudes (*HASP*, 1960). Such values are probably typical of the stratosphere.

In the mesosphere and lower thermosphere, the principal source of information on vertical eddy-mixing has been the spreading of vapor trails, and these have not always been easy to interpret. Typical values are 2×10^6 to 10^7 cm²/sec at 75 km, and 10^7 to 10^8 cm²/sec at 100 km (*Zimmerman and Champion*, 1963). Layser (in a private communication

TABLE 7

Layer	Eddy-Diffusion Coefficient (cm²/sec)	Diffusion Time
Upper stratosphere (25–50 km)	10^4	10 years
Mesosphere (50–80 km)	4×10^5	6 months
Lower thermosphere (80–100 km).	3×10^5	1 week

to the Panel, 1964) has examined high-resolution photographs of vapor trails, and questions their interpretation in terms of a theory of isotropic turbulence, and, hence, also questions all diffusion coefficients so far derived from sodium trails. *Johnson and Wilkins* (1965) have shown that the thermal structure of the atmosphere and the amounts of heat that are available for eddy-conduction downward place an upper limit on the average value of the eddy-diffusion coefficient. This upper limit is about 3×10^6 cm²/sec in the 80 to 100 km region and 4×10^5 cm²/sec near 60 km. *Colegrove et al.*, (1965) have examined the effects of eddy transfer on the molecular and atomic-oxygen concentrations in the lower thermosphere; they find that the few measured values of these constituents, combined with photodissociation rates of molecular oxygen and the recombination rates for atomic oxygen, indicate that the eddy-diffusion coefficient in the 80 to 100 km region cannot differ greatly from 4×10^6 cm²/sec. The rates of vertical eddy-mixing are undoubtedly variable with time, depending upon the meteorological situation and the mechanisms that stimulate eddy-mixing.

The residence times for impurities introduced into the atmosphere are influenced by the rates of mixing by both small-scale eddy-diffusion processes and large-scale circulation. If eddy-diffusion were the sole mechanism by which impurities could spread, then the diffusion time through the atmosphere could be the controlling factor for residence time in the atmosphere, and the residence times for impurities in the atmosphere could be quite long. The variance due to diffusion of the positions of particles originally at a point is

$$\sigma^2 = 2Dt,$$

where D is the eddy-diffusion coefficient and t is the time. Through a layer of thickness d, in which D is constant, the diffusion time is

$$\tau = d^2/2D.$$

This expression indicates that the diffusion times through several arbitrarily selected atmospheric layers for specified diffusion coefficients are as shown in Table 7.

These figures make it clear that the upper stratosphere is the atmospheric region in which the diffusion times for impurities are greatest. The mesosphere from 50 to 80 km provides longer diffusion times than the lower thermosphere, in spite of the greater thermodynamic stability of the thermosphere. The residence times of impurities introduced at higher altitude can be shorter than the diffusion times only through the effect of large-scale circulation, which is undoubtedly the controlling factor in the stratosphere.

Large-scale features of atmospheric circulation are of great importance in mixing the atmosphere. Large-scale circulation must be the dominant effect for horizontal mixing at all levels of the atmosphere except for atomic hydrogen in the telluric hydrogen corona. When small-scale eddy-mixing (scale sizes no larger than those of large thunderstorms) is slow, the large-scale circulation may also be the dominant mechanism for vertical mixing. This is especially true in the stratosphere, where impurities would require decades to pass through the stratosphere if their vertical motion were not dominated by large-scale circulation. Large-scale circulation may also be the dominant effect in the mesosphere. In the troposphere and lower thermosphere, both the large-scale circulation and the small eddy motions can be expected to make significant contributions. Well above 105 km, molecular diffusion dominates over both large-scale and eddy-mixing, while near 105 km the combined effects are not quantitatively understood, but all three processes can be expected to produce significant effects. *Bolin and Keeling* (1963), from a study of seasonal and meridional variations of carbon dioxide, conclude that complete tropospheric mixing within a single hemisphere occurs within a few months. Tropospheric mixing between the hemispheres is much slower and requires a few years (*Junge,* 1963b).

Mixing between the stratosphere and the troposphere varies seasonally and with meteorological conditions. The principal evidence comes from observations of radioactive contaminants introduced into the atmosphere by nuclear explosions. According to *Junge* (1963b), the ensemble of data indicates a residence time in the lower stratosphere at high latitudes of a half to one year, and about twice as long at low latitudes. In the upper stratosphere, where the temperatures increase with altitude, the time is 1 to 2 years at high latitudes and perhaps 5 years at low latitudes. A downward flow at high latitudes in winter and early spring probably constitutes the main large-scale feature transferring stratospheric air into the troposphere.

The importance of large-scale circulation in mixing the mesosphere (50–80 km altitude) is difficult to assess. Unless the large-scale mixing is fairly rapid, the small-scale eddy-diffusional processes may dominate, since their time constant, as shown in Table 7, is of the order of 6

months. The relatively long mixing time in the upper stratosphere tends to reduce the importance of the mesosphere mixing time, since the mesosphere mixing time is much shorter than that of the stratosphere. Impurities introduced in the mesosphere or above will become well mixed down through the mesosphere before they can diffuse through the upper stratosphere. This emphasizes the importance of accurate knowledge of the mixing times in the upper stratosphere, since these describe the choke or bottleneck in the flow of impurities from the mesosphere and thermosphere downward into the troposphere.

Large-scale circulation patterns are probably important even in the thermosphere (i.e., above 80 km), although both molecular and eddy diffusion proceed relatively rapidly there. Whenever the distribution of a constituent is controlled by its diffusive flow through the atmosphere, any motion of the atmosphere at a speed comparable to the speed of the diffusive flow will markedly affect the distribution. The rates of diffusive flow (both molecular and eddy) in the lower thermosphere are such that the vertical motions of the order of a few centimeters per second near 100-km altitude will be important. Unfortunately, there is almost no information on the large-scale circulation in the thermosphere.

There must be many ways by which impurities may disappear from the atmosphere, and probably only a portion of these are known or even suspected. Some impurities that might be introduced, such as nitric oxide, will be decomposed by solar ultraviolet radiation, and ultimately will become indistinguishable from the oxygen and nitrogen of the atmosphere. Others will combine with atmospheric gases. Metals will, in general, form oxides in regions below the thermosphere; and the oxides will be removed from the atmosphere only in the troposphere or at the earth's surface. The processes by which atomic or molecular impurities are removed from the troposphere are not understood, but precipitation is probably the most effective agent. Atomic hydrogen is the only element light enough to be removed quickly from the atmosphere by escape upward, and there is a substantial continuous escape flux of natural atomic hydrogen. Addition of large amounts of hydrogen in the thermosphere would produce a perturbation lasting only a few days, since this is the time constant for escape from the lower thermosphere.

Relative effects of artificial contamination

There is an increasing body of information on the natural minor constituents of the atmosphere and their distribution, although the information is still only fragmentary. In most cases, their sources are not firmly established, although there are plausible suggestions for most of them.

The following two paragraphs summarize our present knowledge of the natural abundances of minor constituents.

Atomic sodium is present in the upper atmosphere, and the amount above 70 km is in the range of 10^9 to 10^{10} atoms/cm^2 (*Junge et al.*, 1962). Nearly all the atmospheric sodium below 85 km must be oxidized, possibly in the form of small particles containing other materials in addition, so that the total sodium content of the atmosphere, including the combined forms, is difficult to estimate. Junge *et al.* argue that the source of the atomic sodium must be meteoric, while *Sullivan and Hunten* (1964) believe that it comes from sea salt. The atomic sodium shows a seasonal variation that suggests that large-scale circulation is responsible for carrying the sodium up from lower levels. The maximum concentration of atomic sodium is at an altitude of about 90 km. *Atomic lithium* is present with a normal concentration above 70 km of 10^7 atoms/cm^2, with a peak concentration at 80 km (*Sullivan and Hunten*, 1964). There is a seasonal variation that shows an enhancement in November; this is entirely different from the variation for sodium, suggesting that the source is different (probably meteoric, if sodium comes from sea salt). Nuclear explosions have injected amounts of lithium that are larger than the normal abundance, and the present atmospheric lithium may still be under the influence of nuclear tests. *Atomic potassium* has been detected with a concentration above 70 km of 6×10^7 atoms/cm^2 (*Sullivan and Hunten*, 1964); its most probable source appears to be sea salt. *Magnesium* and *calcium ions* have been detected in the 100 to 110 km region (*Istomin*, 1961), and the source is presumably meteoric, although sea salt is again a possibility. Their concentration is about 10^8 ions/cm^2.

Several minor constituents play an active role in the chemistry of the upper atmosphere. To significantly perturb the thermosphere on a worldwide basis, amounts of these constituents would have to be introduced that are comparable to the amounts normally present. Some of these constituents and their approximate total masses above 100 km are (*Kellogg*, 1964): molecular oxygen, 10^8 tons; atomic oxygen, 2×10^8 tons; atomic hydrogen, 20 tons; water vapor, 10^4 tons; carbon dioxide, 4×10^5 tons; and nitric oxide, 10^4 tons. Atomic hydrogen is the least plentiful constituent of these; moreover, it has the shortest time-constant—on the order of a few days, because of its rapid escape from the "top" of the atmosphere.

The *COSPAR* (1964) Report of the Consultant Group on Potentially Harmful Effects of Space Experiments summarizes the situation as follows:

A worldwide long-term change of the background concentration of some atmospheric constituent that would be just detectable might be 10% or less for a relatively well mixed and permanent gas (e.g., CO_2, CH_4), and it might be by a factor of two or

three for constituents that vary a great deal naturally (e.g., H_2O, NO, Li). Considering what would be involved in causing a change of the composition of the upper atmosphere above 60 or 70 km, the region above the stratosphere, it is necessary to know what the rate of depletion of a given substance is due to mixing and dissociation (by sunlight and chemical reactions), and also what its natural concentration is. These are only known very approximately, but it seems that on the order of 10^5 to 10^6 *tons per year* of water vapor or nitric oxide would have to be injected above 60 km to double the amounts of these gases worldwide, and about the same tonnage would be required to add 10% to the carbon dioxide content. (This corresponds approximately to an annual launching of 10^5 to 10^6 Saturn-type rockets, or of the type of Soviet rocket used to put the second Soviet cosmonaut into orbit.) On the other hand, only 2×10^3 to 10^4 *kilograms per year* of atomic sodium would have to be injected above 60 km to double this constituent, and only a few tens of kilograms of lithium annually would be expected to double its background concentration. These last figures are within the capability of man now, and may (as mentioned above) have already been achieved temporarily in the case of lithium. The larger figures for nitric oxide, carbon dioxide, and water vapor, the main combustion products of rocket fuels, seem unattainable in the foreseeable future.

Major uncertainties exist at every step in our present understanding of removal of minor constituents from the upper atmosphere. Some aspects of the problem appear more hopeful than others, however, it appears probable that some real understanding of the mixing processes in the mesosphere and lower thermosphere will develop during the next few years. It also appears reasonable to expect that the pattern of general circulation throughout the atmosphere will become clear, but progress here may be less rapid. The understanding of small-scale eddy-mixing processes in the upper stratosphere may lag still farther, but, since general circulation probably constitutes the important mixing process in this atmospheric region, this is not likely to limit the understanding of the over-all problem. It seems reasonable to expect that improved knowledge of the chemistry of the mesosphere and lower thermosphere will develop rapidly in the next few years, but important problems will probably remain for decades.

Another important part of the problem relates to the troposphere and the removal of impurities from it. Again, this will probably not limit the prediction of effects in the mesosphere and lower thermosphere, since the upper stratosphere controls the rate of exchange between the mesosphere and troposphere.

METEOROLOGICAL SIDE EFFECTS OF WEATHER-MODIFICATION EXPERIMENTS

The objective of most weather-modification experiments, now and envisaged for the future, is to try to produce a desired "terminal" effect by initiating a chain of physical processes. It is to be expected that, in being

reasonably preoccupied with detecting a desired single result, we may miss evidence of some extremely interesting effects being initiated or manifested elsewhere along the chain. In addition, we might erroneously assume that the looked-for "terminal" result is in fact the last link in the chain. A few examples, admittedly containing considerable conjecture, should serve to illustrate the importance of being aware of side effects in all different scales of experimentation.

When silver iodide or dry ice is introduced into a supercooled stratiform cloud, it causes the seeded portion of the cloud to dissipate as its particles fall out in the form of very fine light snow. Consequently, the operational seeding to dissipate supercooled fogs and low stratus over airports will frequently produce, as a coincident side effect, a very light dusting of snow on the ground beneath the clearing area. Considering the generally low water content of such clouds (about 0.1 g/m^3), their normally shallow depth (50–500 ft) and the small areas (2–10 square miles) and short time periods ($<$2 hr) involved in a typical operation of this type, it is quite improbable that more than 1 or 2 mm of snow would accumulate from a single seeding operation on this scale. On the other hand, many continuous hours of seeding in the same area, or seeding so as to release conditional instability and thus promote convective snow showers, could produce enough local snowfall to warrant its prior quantitative prediction.

The potential side effects of convective cloud seeding, on the other hand, are many times more numerous and less understood than the previously cited example. When a cumulus cloud is seeded with silver iodide, any or all of the following are possible unnatural reactions to that single act. Seeding may:

(1) increase or decrease rain;
(2) increase or decrease hail;
(3) increase or decrease lightning;
(4) increase buoyancy and cloud height;
(5) increase or decrease the downdraft and surface gusts;
(6) alter the dynamics and thus the phenomena of adjacent cumuli; and
(7) alter the precipitation downwind from the target area. Since few, if any, seeding experiments or operations have been *designed* to detect and reliably evaluate more than just a single "terminal" effect, we know very little about the probabilities of a coincidence or sequence of different effects or about their relationships to environmental and experimental circumstances. Experience to date permits us to state only the following two tentative opinions concerning side effects of cumulus seeding.

(1) There is a strong possibility that AgI seeding to mitigate hail causes a small increase in rainfall whether or not hail is affected.

(2) It is improbable that drought conditions can be appreciably strengthened or prolonged by small-scale cloud-seeding operations.
The first opinion is based on the few available evaluations of the rainfall consequences of hail-suppression projects (e.g., by Dessens in France, and a Panel evaluation of a very short-term operational hail-suppression project in western Maryland—both of which give evidence of positive rainfall anomalies on the order of 15 percent). The second opinion is largely intuitive, founded more upon the lack of statistically significant evidence of rainfall decrease due to seeding (for whatever purpose) than upon the slowly mounting evidence of modest increases. In this regard however, the question of a possible rainshadow effect downwind of a cloud-seeding target area must be borne in mind. This question is discussed at greater length in earlier sections of this report.

In general, as weather-modification experiments increase in size or begin to produce long-lasting direct effects (such as affecting an abnormal delay in seasonal snow-melt over an extensive area), the potential side effects take on a very different degree of social significance. To specify the threshold size of such a "critically large" operation is impossible at this time, but we would become seriously concerned, for example, about the downstream and larger-scale circulation effects of an extended cloud-seeding operation whose effective target area is greater than about 100,000 sq. miles. An illustration of close to the ultimate in large-scale experimentation is presented by the Soviet meteorologist, *Budyko* (1962). In his article on "Climatic Change and Climate Control" he states:

Modification of the Arctic ice cover is a matter of considerable interest. Its destruction could lead to the re-establishment of the climatic conditions of the pre-glacial epochs, when the air temperature in the high and low latitudes of the northern hemisphere differed comparatively little.

And he continues with the caveat:

The question of the expediency of such a measure is very complex. One must keep in mind that the atmospheric circulation pattern would change drastically if the ice were destroyed. The decrease of temperature contrast between the pole and the equator would weaken the circulation, which could decrease the amount of precipitation in some continental regions. All possible results of the destruction of the Arctic ice cover must be investigated thoroughly before such a step is taken.

We underscore the last sentence of the quotation, and add that our present understanding of the general circulation and climatic change does not rule out *many more* possible consequences, one of which is the reglaciation of northern continents (e.g., *Ewing and Donn*, 1958). This problem and others of similar magnitude are no longer solely academic.

Ridding the Arctic Ocean of ice may be technically feasible today (e.g., *Fletcher*, 1965) and may already be under way through inadvertence. The difficulties in detecting slow changes and in evaluating global consequences are great; but the observational and analysis technologies are at hand, waiting only to be expanded so that these necessary but very complex and time-consuming investigations can be started.

Special Problem Areas in Weather Modification

EVALUATION OF WEATHER-MODIFICATION PROJECTS

In this discussion we shall consider the contributions that can be made to weather modification by statistical ideas and techniques. It is proposed to outline the nature and potential value of some of these statistical tools, indicating their strengths and weaknesses, and considering whether, in the light of what is known today about weather modification, it is likely that such tools can play a key role in future progress or should serve merely as accessories. Several related questions are considered, such as whether existing statistical tools are being applied to weather-modification problems as skillfully as possible, and whether they are adequate for the purpose in standard forms or need to be tailored or extended to cope with special situations arising in this field.

Background on the evaluation problem

The historical narrative of weather modification spans about a half-century, during which many radical changes took place in human knowledge and affairs; it can instructively be interpreted from several particular points of view. For the purpose at hand, the events can be classified into four phases of activity relating to the understanding of precipitation: (phase A, 1911–1945) theories assigning a critical role to ice nucleation; (phase B, 1947) implementation—discovery of very effective ice-nucleation agents; (phase C, 1947–1954) early experiments of an exploratory nature; and (phase D, 1955–1965) recent experiments—test and commercial efforts.

Of course, phase A refers to the work of *Wegener* (1911), *Veraart* (1931), *Bergeron* (1933), and *Findeisen* (1938), while B refers to the discovery of the action of dry ice and silver iodide by *Langmuir* (1948a), *Schaefer* (1946), and *Vonnegut* (1947). Phase C refers to "Project Cirrus"

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(1947–1952), the "Cloud Physics Project" (1948–1949), the "Artificial Cloud Nucleation (ACN) Project" (1953), and to certain others. Phase D refers to the "Santa Barbara Project" and to many later projects such as those of CSIRO (Canberra) Australia, the U.S. Weather Bureau, and many others carried out by university research teams and other contractors supported by the National Science Foundation.

Clearly, each phase contributed to the evolution of weather modification, but the scale of activity jumped to an unprecedented new high level upon the occurrence of event B following A: that is, upon the discovery of silver iodide as a seeding agent promising to implement the ice-nucleation theory of precipitation. Furthermore, though weather-modification activity has since had its ups and downs during the last two decades spanned by phases C and D, as seemingly positive and negative experimental results have emerged, it is clear that a level of interest and activity radically new and different from anything prior to 1947 has since been sustained. Also, it seems clear that this level is unlikely to subside greatly in the near future.

It seems, also, that interest and effort in weather modification has derived major support from a number of other trends and developments of great scientific, technological, and political importance, including: (a) increased technical capability to deliver "treatments" to clouds; (b) increased technical capability to observe effects in clouds, as by getting close with aircraft, radiosondes, dropsondes, radar, and other improved measuring instruments; (c) improved capability for data interpretation, establishing mathematical models and calculationally exploring them; (d) improved understanding of meteorology as a science: on the global scale, such as the general circulation; and on smaller scales as in cloud physics, phase change, particle accretion; (e) changes in attitude among people, in government, the public, and within the scientific community regarding large-scale scientific team efforts and objectives; and also an increased awareness of the value of limited success in the modification of storms and precipitation.

With these supportive factors, a remarkable and viable change has taken place within meteorology. A field that might previously have been called primarily descriptive* evolved into one oriented, at least in part, toward manipulation and utility as a consequence of the cautious acceptance of the possibility that weather modification might actually be possible. The change in attitude had many important side effects, such as the recognition that many other types of man-made disturbances of nature should be classed as aspects of weather modification. But within

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* As used here, "descriptive" is intended to include, prominently, mathematical description.

atmospheric science itself, one of the most disquieting side effects of the changed attitude was the gradual recognition of the profoundly difficult problem of making an adequate test of any modification procedure.

It is not appropriate to enter into a detailed discussion of the difficulties and frustrations encountered by both weather scientists and statisticians in attempting to test weather-modification attempts during the past two decades, although much might be learned that is relevant to future courses of action by so doing. It seems essential, however, to point out that the abrupt change from a passive to an active mode in meteorology focused attention upon a large package of unsolved problems, of which the competent atmospheric scientist was already well aware, but which had not been expected suddenly to require confrontation *en masse*. In this package of problems are many sources of natural variation, restrictions on experimental execution, gaps in knowledge about the specific action of treatments and possible observables, and, indeed, in the nature and interrelationships of physical properties being stored, converted, and exchanged, and which collectively constitute the precipitation process itself. Each of these can, of course, be considered a source of experimental noise or variation; and it was recognized that the tools of statistical test design and inference procedures have been developed explicitly to be of service for reaching decisions in experimental measurement situations plagued by variability and "noisy" data. When statistical tools were brought into action, however, the established analytical discipline associated with their proper use soon revealed a catalog of crippling restrictions under which modification tests were being attempted. As a result, it became clear that by trying to make effective inferences in the presence of so much variability, large experiments of many years duration (e.g., *Neyman and Scott,* 1961) would be required.

The main points to be deduced from the above summary are:

(1) That the modern era of interest in weather modification was stimulated by a theory of the critical role of ice nuclei in precipitation.

(2) That effective ice-nucleation seeding agents, notably silver iodide but also others such as dry ice, were discovered and made available.

(3) That, consequently, there emerged a new class of test problems of great practical and theoretical difficulty.

(4) That these test problems have not yet been solved, even with the aid of excellent statistical support, despite many costly and prolonged experiments, even for the case of silver iodide seeding.

It is emphasized that the failure indicated in (4) should not be considered the fault of inadequacies in statistical procedures *per se* merely because they indicate, under prevailing trial conditions (that is, small numbers of expected trials per season, few observing devices, uncertainties

about where the seeding actually went in each trial and where it ought to have gone, uncertainty of storm conditions and locations chosen for test, and the natural variability of the storms tested), that testing will be a painfully prolonged and costly process. These sources of variability, and many others, are reflected in the data to which the calculations of statistical-test-evaluation procedures must be applied; and it is one of the main virtues of modern statistics that their effect upon the deductions that may be made thereafter is explicitly pointed out. On the other hand, the atmospheric scientist planning an experiment may have little choice regarding the variability of storms nature sends through an area chosen for experiment, and may find himself fenced in by insufficiency of observing equipment, conflicts between small numbers of candidate trials, and the need for eliminating bias by randomization.

It is therefore clear that the problem of testing the effectiveness of any proposed weather-modification technique, unless it be spectacularly powerful and universal in its effects (which is most unlikely because of the physical scale of atmospheric events compared to those man can afford for the purpose) is blocked by quite fundamental difficulties. The fact that the answer to the question of silver iodide effectiveness has remained inconclusive may accordingly be considered a great handicap, but not surprising. Extensive discussions have been given in the literature by Neyman and his associates (*Neyman and Scott*, 1961; and refs. cited therein), in the Final Report of the *Advisory Committee on Weather Control* (1957), and in the Skyline Conference report (*National Academy of Sciences*, 1959) concerning problems encountered in such tests.

Although the test-problem aspect of weather modification is by no means the only aspect in doubt or worth considering, it is perhaps the one most immediately pressing from the point of view of both the outsider to the field concerned with the nation's water resources, and of the atmospheric scientist desiring to make progress along the lines of effective modifications (who needs to be able to pass through the cycle of "theory followed by experimental proof-test" with reasonable assurance and dispatch). Since the modification-test problem seems unlikely to vanish in the near future by itself, it follows that high priority should be given to the design and execution of effective tests in the particular case of silver iodide seeding. Also, it affords an ample, and probably typical, variety of conceptual, theoretical, experimental, and practical problems upon which to base a discussion of the role of statistics in weather modification. Accordingly, the subsequent discussion will be focused upon it.

The necessity for statistical design in atmospheric experimentation

Modern statistical procedures consist typically of methods of calculating whether or not certain effects in experimental data could probably have

been produced by chance. If the calculations indicate that the effects could, with reasonable probability, have been so produced, then the evidence that the effects were actually due to a specific hypothetical cause is considered weak; otherwise it is considered strong. The probability of the results occurring by chance is called the "significance level." The point on the significance-level scale at which the calculated results are considered to shift from weak to strong is purely arbitrary; for some purposes it may be chosen at odds of 1 in 10, and for others 1 in 100 or 1 in 1,000. Often 1 in 20, or 5 percent, is chosen as a convention.

Test procedures and calculating procedures of different sorts may be tailored for different hypotheses and experimental circumstances. Modern statistical test procedures consist essentially of sets of rules for configuring experiments, for carrying them out, and for performing calculations upon the resulting data so as to minimize the masking effect of undesirable "nuisance" variables that intrude upon the experiment, so that the desired observations or measurements can still be identified. The effectiveness of a given statistical test procedure in suppressing the effects of undesired variations so as to reveal the correct implication of the test with regard to a given hypothesis is called the "power" of the test. Test results can lead into errors of two obvious types, "type I" being the error of rejecting an experimental hypothesis when it is true, and "type II" the error of accepting it when it is false. An important property of a test relates to whether it is unbiased in the sense that the mathematical model involved is as likely (when in error) to indicate validity as invalidity, and vice versa, as it estimates the "betting odds" from the data, under various experimental conditions. Where experimental conditions vary greatly, it is not easy to assure this property of the mathematical scheme underlying the test design.

If the people obliged to base decisions on statistical tests of experimental results require that the significance level be chosen stringently (i.e., that the probability of a chance success be very low), and circumstances are such that the nuisance variables entering the experiment are large compared to the effect being tested, almost any conceivable test may prove to be of inadequate power to get the job done within reasonable time and cost. (Under other circumstances, very crude tests—even visual estimation—may prove to be of adequate resolving power to justify going ahead to the next phase of a problem.) Often it is necessary to use the systematic procedures of designing experiments according to statistical test criteria and to tax the test capabilities quite fully; and this is necessary most often when the experimental situation itself is inherently difficult. Under such circumstances, what may appear to be a rigmarole in the procedure—or worse, an insult to the experimentalist—is easily misunderstood. For example, there can occur a need to randomize a series of test trials, which means to select the occasions for test trials

by the equivalent of tossing a coin. The reason for this seemingly whimsical procedure is simply to enable one to use the laws of probability in assessing experimental evidence. It has nothing to do with questions of honesty or integrity on the part of the experimenter. It is assumed that the experimenter believes that a test will prove something, and that otherwise he would not have proposed it. The exact purpose of trial randomization is to permit the experimenter maximum freedom in choosing favorable experimental circumstances without violating the objectivity of a test. Ideally, all the variables that could affect the outcome of an experiment should be included explicitly in the statement of the test hypothesis, and in executing the test each trial of the experimental sequence should provide a specification of the value of every variable. Sometimes this is not possible, for various practical reasons, and certain variables that may implicitly be recognized as potentially important cannot be specified or chosen by the experimenter in advance, but the test must go ahead. Randomization provides a scheme for going ahead with the test under such circumstances, with the assurance that the test sequence is unbiased with respect to all variables implicitly recognized but not fixed by the hypothesis.

However indirect, rigid, and ritualistic such tools for designing and executing a valid statistical test may seem to experimenters, they constitute the most refined decision machinery known for resolving questions of cause-and-effect relationship. These devices and the underlying logic of their use have been studied and refined by statisticians over several decades, specifically for use in circumstances when intermediate or internal stages of a process are unknown, or cannot adequately be observed in sufficient detail, and where unwanted variability of any sort can intrude upon the experiment so as to mask the hypothetical relationship. Of course, it is not always necessary to invoke such "heavy artillery" to make everyday decisions, but the underlying logic cannot be dismissed and should be better understood by all scientists, since it is universal and fundamental. The logic of inferential deduction from experiments still appears to be frequently misunderstood within the community of atmospheric scientists and elsewhere. Especially where tests are difficult and variability is large and results frustratingly indecisive—the circumstances in which statistics can be most important in advancing the science—it is not unusual to hear statements based on appalling misconceptions of the logic underlying any test, in which statistics is named the culprit.

Finally, it should be emphasized that neither the statistical test scheme nor the calculations applied to the resulting data do anything beyond providing an objective estimate of the "betting odds" relative to the stated hypothesis. *Interpretation of these odds is left in the hands of humans faced with a need to make a decision based upon them.* The

effective and successful use of statistical tools is by no means a routine matter, nor is there any guarantee that the resolving power that can be brought to bear through competent use of statistics always provides a suitable base for decision. In fact, one of the virtues of these tools is that, when the outcome of an experiment is insufficiently resolved by acceptable criteria for significance, the test procedures automatically so declare. This is still largely the situation in the case of weather-modification tests concerning the best known, most widely used and long-recognized modification agent of promise, namely, silver iodide. Experimental tests to date have failed to show adequate significance to resolve this case conclusively. It is natural to ask whether there can be any way out of this dilemma.

Methods of design to strengthen evaluations

There appear to be several ways out of the dilemma of modification-test indecisiveness; none of these involves any attempt to sidestep the essential requirement of objectivity and validity imposed by statistical-test design, nor to accomplish unheard-of technical miracles in either statistics or onsite measuring technique. The main idea is to probe more thoroughly the package of constraints and basic assumptions under which it has been assumed that experiments must be designed and carried out. This can be done only with the best possible cooperative interaction between atmospheric scientists and statisticians in continued collaboration over ample time periods. The directions of probing in quest of new test schemes and effective practical methods for carrying them out so as to increase test power include:

(1) Enlarge the scale of the test in space and/or time, getting more data of the same sort.

(2) Choose new variables of observation, and a greater variety of them, so as to get new kinds of data to reduce the uncertainty of actual test trials.

(3) Partition the over-all process spanned by the test into a chain of subprocesses, providing new means to gather new types of data for each.

Of these approaches, method (1), *enlargement of test scale,* is so commonplace, easy, and effective in biology, agriculture, and other fields successfully using statistics that its use as the main avenue for improving test power is often assumed automatically for all applications. Certainly it is one of the most straightforward ways out of the dilemma. While weather-modification tests can be very expensive to set up and execute, and enlargements can be very costly, there is room for doubt that the size versus cost tradeoffs for modification tests have been communicated realistically by informed test experts to sponsoring management. This

doubt becomes very disquieting in view of the past piecemeal experi-
mentation, the cumulative expenditure on which appears more than ample
to have settled the silver iodide question. It hardly seems possible
that the economics of marginal or indecisive experimentation has been
squarely faced; statistical tools are capable of reasonably good *a priori*
estimates of the probable decisiveness of a test, and, furthermore, sequen-
tial test procedures are available that can allow a test to proceed until
a suitable level of decisiveness has been reached. In many cases, the cost
of an experiment is contained largely in the effort of getting it started;
after that the cost of extension is apt to be modest. The advantages of
designing for termination when a decisive significance level is reached,
or when it appears probable that a decisive level will not be reached soon
enough, should not be overlooked.

Furthermore, if several smaller experiments need to be run for some
reason, it is usually not possible afterward to find a valid method of
combining their data to increase decisiveness with respect to some over-all
hypothesis, unless this has been included in the design from the beginning.
One stated criterion for valid aggregation of several individual test
results into a single over-all result is that all tests must be physically
similar in all important respects (*Thom*, 1957a); another is that they be
independent. In the present state of meteorological knowledge, these
conditions are very difficult to establish satisfactorily. This point is often
not clearly understood by those who feel that it should be possible to
look into test results gleaned from here and there and combine them
into conclusions of high reliability. For this reason, in our evaluation
of 14 short-term seeding projects we only infer a *qualitative* indication
of positive results that strongly implies the need for valid *quantitative*
testing.

Aside from more realistic evaluation of the economics implied by
approach (1) and questions of better coordination and general planning,
there remain several interesting and potentially fruitful technical pos-
sibilities for enlarging the body of experimental data. One of these is
to study ways of increasing the space dimensions of an experiment rather
than prolonging it in time. This appears very worthwhile because, if
positive results are achieved, the savings in time may outweigh the
experimental cost. A number of very interesting and revealing subordinate
questions arise if the space-time tradeoff proposal is to be investigated
thoroughly, including questions of characterizing the area- and time-
distribution of storms, and the best configuration of rainfall-observing
devices over the experimental areas when practical conditions limit the
total number available. Also, questions of size, configuration, and arrange-
ment of target and control areas, etc. must be analyzed from a different
standpoint. It would seem that there are important parameters of estima-

tion, such as mean interaction-free distance and optimum correlative distance between control and target areas that have not been adequately analyzed, as well as parameters of choosing the most effective duration and spread of the experiment and of the modification variables and end-result observations.

Expansion to get more data may be an answer but not the only or best answer that can be offered, and alternative experiment designs more suited to a very different scale of time and cost should carefully be examined. One such design technique that has been used to increase the power of a randomized experiment is known as the "crossover" randomization technique (*Moran*, 1959), in which the randomization is applied with respect to deciding which of two areas is to be seeded and which used as a control at each occasion suitable for trial. It may indeed be possible to develop other types of experimental design schemes in which the unbiasedness of randomization is preserved, yet the power of the test is made relatively high. The task of developing such experiments has not been given the attention it requires in connection with weather modification, because, in the usual case that the statistician faces, "more data" provides a sure and easy answer, and because it is very likely that the only effective alternatives involve taking particular opportunistic advantage of special types of measurements and controls that may be possible in weather modification but not in other fields. To do this will require an unusual degree of intimacy between the thinking of meteorologists and statisticians; it is clear that this should be promoted.

Finally, there may be methods of obtaining more data in the spirit of method (1) by using what amounts to more widespread, naturally available, data-gathering systems. An example of this would be the use of large natural water-runoff systems as means of data gathering, which may prove equivalent to many hundreds or thousands of raingauges, read almost continuously. This scheme, and the use of radar configurations coupled to volume-rate-integrative computers, require and merit very thorough and adroit analysis.

Turning now to method (2), *obtaining new kinds of data* to reduce uncertainty, it is important to remark that, in principle, every source of variation entering an experiment that can be identified and observed during each trial occasion can be eliminated as a source of experimental uncertainty, thus increasing the decisive power of the experiment. This basic idea can lead to many different possible techniques for improving the effectiveness of an experiment, and is already used to some degree in observing "control area" rainfall to compare with that on a seeded "target area." More specifically, it is likely that there are many kinds of observable parameters to describe a storm or storm system, other than just its precipitation effect on a control area. For example, some storms

belong to widespread "systems" driven by large-scale features of the circulation, while others represent local upsets in thermally unstable air masses. The important point is that the state of the atmosphere during each trial of a test series is now considered as a random variable that is wholly unknown. It has been argued that to supply the information necessary to improve estimates of the state of the atmosphere at the time of each trial is equivalent to requiring perfect, or at least greatly improved, prediction. This would be an enormous help, of course, but is surely more than is needed to improve modification-test decisiveness. For the latter purpose, no prediction of future events is required, but only the accurate specification of what the situation is during each trial; this is a far less delicate problem, and one far easier to implement than prediction.

In short, method (2) involves re-examination of the parameters and the way they are combined to describe the exact experimental circumstances, and to seek, thereby, new parameters by which the different weather situations encountered in seeding trials may be classified. At present, the classification of cloud and storm types is very gross and pictorial, even though it clearly ranks among the most important criteria upon which success depends. A concerted effort is needed to determine what combinations of parameters adequately characterize the pertinent states of the atmosphere during an experiment. If ice nucleation is the key mechanism, what is the state of the test area with respect to natural nucleants? How much do these vary in the atmosphere through natural causes? What is the water content, water transport, and stability of the atmosphere at the time of the trial? What are the wind and turbulence conditions? How much convection actually occurs and what is its transport rate into freezing altitudes? Such measurements would eliminate many uncertainties about the system, but would not eliminate its variability; they could immediately be brought into the experimental design by means of what statisticians call "stratification" techniques, and would in many cases produce improvement in test decisiveness.

Finally, method (3) of increasing the power of weather-modification tests is to attempt to *partition the over-all process* spanned by the test into subordinate parts, and to obtain at each interface the observations and data permitting cause-and-effect testing of the in-between processes. In breaking up the single seeding-to-rain experiment into smaller and smaller parts, this approach corresponds to what is sometimes loosely called a "physical experiment." Precisely because atmospheric phenomena span 17 orders of magnitude, it is clear that the breakup of the over-all process must remain far from complete for a long time to come. A few basic subprocesses—perhaps half a dozen—represent the limit to which this can be carried in the foreseeable future. Yet, any such partitioning

is potentially capable of incorporation into experimental design in such a way as to increase decisiveness.

In summary, the modification-test problem is a profoundly difficult one, both in theory and in practice. It seems unlikely to yield to a purely meteorological approach, and also unlikely to be solved solely by diligent use of statistics. A penetrating and skillful interaction between the two seems the most promising. It is easy to be deceived into thinking that if only enough were known about ice nucleation, or drop accretion, or convection, or fine-particle distribution, or weather prediction, or whatever, this whole problem would become quite deterministic and it would become possible to describe exactly how the atmosphere would behave in any given set of circumstances. If any atmospheric scientist really believes that this is the one and only, or the best, approach to weather modification, attention should be drawn to the situation in hydrodynamic turbulence, where it is clear that the Navier-Stokes equations, which were developed something like a century ago, do indeed describe accurately the generative laws, but the consequent processes are so complicated that even today they are virtually ineffective for describing such a commonplace physical situation as turbulent flow in a pipe. The statistician is more modest, so to speak, and operates on the assumption that, for the forseeable future, it will not be possible to understand everything needed to describe the atmosphere. It is safe to predict that the rate of advance of atmospheric science will be gauged by the rapidity and effectiveness of the cycle between construction of candidate physical theories and their valid experimental proof—testing; and that in both phases of this cycle statistical tools will play a vital role.

Regarding the description—mathematical or otherwise—of the precipitation process, there is an important distinction to be made between understanding the physical processes well enough, say, to write down equations for them, and determining what will actually happen when they are subjected to particular boundary conditions, such as orography of the earth or the distribution of natural nucleating agents in the atmosphere. At some point, variables for which no theory or description exists are bound to enter the picture, and recourse must be made to frankly statistical statements. Furthermore, the principal hope of weather modification rests upon the notion of amplification of small deliberate disturbances, by nonlinear effects in the atmosphere, etc. Clearly, large-scale meteorological effects like the general circulation may be considered "forcing terms" with respect to any local region in which modification is being tested. But the actual triggering of the instability must be quite local. It is an important objective, from the standpoint of weather modification, to be able to separate and describe

independently these two classes of processes by analytical and statistical means.

Reporting procedure in operational projects

We have been discussing primarily important problems of *research* experimental design. *Operational* field projects, on the other hand, are established under an intrinsically different set of "design criteria," rendering them exceedingly difficult to evaluate in a statistically satisfactory way. A number of the relevant statistical questions are discussed in the earlier section on operational cloud seeding and in Appendixes 3 and 4. Here, only the underlying problem of adequate data reporting will be discussed.

In the course of its work, this Panel has examined several dozen evaluation reports on cloud-seeding operations conducted by a number of leading commercial cloud-seeding companies. From its experience in working with those operational reports, the Panel feels that it must make certain suggestions as to ways in which the quality and completeness of such reports might be improved. It has been suggested by some operators that it is not economically feasible for them to devote more of their efforts to preparation of more complete reports, and they say that their clients are not, in fact, interested in getting more complete reports. Consequently, there may be merit in finding some means by which outside support can be secured for adequate report preparation at least in selected commercial operations in which an opportunity to obtain scientifically useful information is recognized.

The effectiveness of such outside support probably depends upon assigning to some agency in the federal government the responsibility of regularly conducting independent evaluations of commercial cloud-modification operations. Since there is a rather wide spectrum of quality in existing operations, this assignment of responsibility should probably not be interpreted as imposing an obligation to evaluate all operations designated by the operators as cloud-seeding programs.

We recognize the serious difficulties in implementing the above suggestion, but feel that detailed discussion of them is beyond the scope of this report. We now pass on to the related question of maintenance of records on all seeding operations, including those of marginal scientific merit. Complete records of this type are indispensable. There must be some routine arrangement for publishing the locations and durations of all operations, with enough additional details as to the nature of nucleant and mode of seeding, numbers and locations of generators, locations of target and control areas, and other relevant data, that subsequent scientific studies can always be assured of adequate information for assessing

the likelihood of contamination from other projects, or for determining whether past seeding projects introduced nonhomogeneity into climatological and hydrologic records in such a way as to interfere with, say, historic regression analyses on some later project. The latter problem was noted repeatedly in the sample of operators' reports studied by the Panel. This difficulty will grow steadily greater if more areas come under temporary or sustained seeding. Climatological heterogeneity so induced must not be lost from sight in the scattered records of numerous seeding organizations. It must become part of an open scientific record, which may assume increasing importance in future studies of weather modification as well as in many other climatological studies. The nature and implications of this problem, we note, have received no attention from meteorologists and climatologists to date, due to lack of agreement that cloud-modification effects are real.

All the foregoing considerations, which are basically scientific, can be seen to be interwoven with a host of nonscientific considerations outside the purview of this Panel. A tangled net of legal, political, economic, and perhaps ecological problems can be dimly perceived. Is it necessary to view the atmospheric water vapor transported over regional and political boundaries as a natural resource in the public domain? How can it be properly apportioned? How can one decide on a fair adjudication of "upstream vapor rights," particularly in view of the present incomplete scientific information on the climatology of natural water-vapor transport? Our study cannot do more than adumbrate these problems that obtrude as soon as one begins to accept the evidence that cloud modification may produce real effects on precipitation.

When, as is usually the case, commercial operations employ some statistical methodology in arriving at an estimate of the additional precipitation stimulated by seeding, it is extremely important that *complete reporting of all evaluation methods and all relevant data be included.* (We now pass over the important question of just how this is to be insured and how it is to be supported in the face of the costs to the operator.) Many reports fail to state clearly the precise statistical techniques employed, and many fail to give data indispensable to statistical analysis of claimed results. Almost invariably, a historical regression of target-area versus control-area precipitation (or runoff or snow depth) is used as the standard to which seeded-period measurements are compared. Frequently the regression data are not given. In some instances, the seeded-period precipitation observed at control stations is omitted; occasionally not even the target station data are fully listed, yet these are obviously indispensable to assessment of results. Certain key quantities obtained at intermediate stages of evaluation must be clearly listed. For the often-used regression methods, for example, not only the raw

regression variates themselves, but also the historic means and standard deviations for target and control groups should be listed. The regression equation standard error of estimate and the correlation coefficient should be displayed. Maps that clearly and accurately show location of every target and control station and ground generator (if used) should be included in each report, and auxiliary lists identifying each station by its official name according to some coding scheme (as in the case of United States Weather Bureau raingauge stations, or United States Geological Survey streamflow gauges) should be included in the report. The inadequacies in reporting of such indispensable data as the foregoing are typical, rather than rare, if we are to judge from the sample of operational reports that we examined.

In projects extending over a number of years, all additions and changes in populations of target and control stations should be unambiguously reported, with brief explanations of the reasons for changes. If the client requests interruptions of seeding for certain time periods, these should be very clearly identified and the reasons for the interruptions given. If the operator's choice of target and control stations does not include all long-record stations available within the contractual target area and within the chosen control area, reasons for exclusion of stations should be discussed. Since an element of arbitrariness is inherent in choice of control areas, it is somewhat difficult to give recommendations as to how an operator should clarify his choice of control, but at least brief discussion of this must be included.

All these and a number of additional minimal requirements do not stem from capricious inclination to cover all possible marginal needs for data in independent evaluations. Rather, in the Panel's experience in attempting to extract scientifically useful information from what is believed to be a representative sample of the better operational reports, it has disclosed some very serious deficiencies in present reporting practices. The foregoing list of needed items of information, long as it may seem, represents only a moderately complete list of data and information found to be indispensable in carrying out evaluations. Considerable correspondence with seeding operators and with public data-gathering agencies often was necessary in order to fill in the gaps left in operators' reports. The fact that many basic pieces of information are characteristically absent seems to indicate that many clients are not themselves critically aware of the difficulties in assessing the effects of cloud-seeding in the presence of the wide natural variability of precipitation; otherwise they would be demanding much better and more complete reporting. If an optimal program of operational cloud modification is to evolve, with proper assessment of effects and proper monitoring by suitable governmental agencies, at least the above level of detail of reporting must be assured.

INSTRUMENTS AND TECHNIQUES IN CLOUD-MODIFICATION RESEARCH AND DEVELOPMENT

Measurement of cloud properties

Regardless of which of the many cloud parameters one considers, there is urgent need for better measurement techniques to improve our present quantitative knowledge of each parameter. Humidity, temperature, vertical air velocity, turbulence spectral characteristics, nuclei characteristics, cloud drop-size distributions, liquid-water contents, and a wide range of cloud-electrical parameters are inadequately measured with existing equipment. Not only better instruments but also better vehicles and platforms for carrying instruments into clouds are sorely needed. The latter, particularly, call for greater research and development support, since vehicle design entails extensive engineering effort. Furthermore, more extensive use should be made of the better items of equipment that are now available.

There is particularly urgent need for more precise measurements of such apparently simple parameters as liquid-water content and drop-size distribution. These measurements should be taken in a variety of cloud types under a variety of climatological regimes, and in sufficient density in space and time within each cloud to disclose all significant variations of these parameters throughout the entire life cycles of individual clouds.

In indirect probing, as distinguished from actual in-cloud observation, pulsed Doppler radar appears to be moving toward success in providing a whole new category of detailed observational data on cloud kinematics that would not have been thought possible only a few years ago. Laser techniques now under study warrant vigorous development, as do new optical techniques for measuring drop sizes without the distorting factor of impaction on collection surfaces. Better methods of studying both condensation nuclei and ice nuclei are receiving attention, as are a number of other techniques for examining both the microphysics and the dynamics of clouds.

It has not been too difficult to devise equipment to give meter readings and to fly such equipment through a large number of clouds; but to develop new equipment and to demonstrate the fidelity with which it measures parameters has been exceedingly difficult. Few investigators have been willing to test, and check, and then skeptically test again. Thus, it is extremely important that, in the future evaluation of cloud-physics research, little merit be attached to sheer quantity of observational data. Indeed, the highest priority must be given to exceedingly careful planning of observational programs employing vehicles and instrumentation based on a well-conceived program of development and

testing. It is, unfortunately, far easier to make such a prescription than to find the persons to implement it. Here, as in every other part of the scientific enterprise, the bottleneck lies in the task of finding the people with the necessary scientific and engineering talent, high standards, energy, and willingness to undertake a comprehensive, and possibly lengthy, first-class experiment in preference to a shorter and less expensive experiment that is likely to be inconclusive.

Cloud-seeding technology

In considering the development of new materials capable of inducing modifications in clouds, it is important to distinguish between research and operational efforts. A certain amount of misleading publicity has been given to new "promising" nucleating agents for stimulating increased rainfall. With few exceptions, the crucial difficulty that has been overlooked is a means for dispersing the new nucleant into the atmosphere in a fine enough state of subdivision and in sufficient volume to make stimulation of rain economically attractive. Sheer tonnages of nucleant required may remove all practical significance from a new and "improved" nucleant. Further, if that agent cannot be dispensed in the form of particles of the order of 0.1 μ or less in size, many physical and chemical requirements cannot be met. Since particles that small are difficult to form by means other than an evaporation-condensation cycle, in which the material is brought into a vapor phase and then rapidly quenched to form a finely divided fume of submicron particles (as in existing silver iodide generators), little practical significance can be attached to exotic organic nucleants or to various types of carefully coated or treated particulates. Furthermore, it is important to stress the point that, since the difference in saturation vapor pressures of ice and supercooled water becomes very small as the temperature increases, ice nucleants that function at temperatures significantly warmer than the threshold of silver iodide probably offer only very limited practical benefit, although this conclusion may be altered if certain recent observations on marked temperature dependence of crystal growth rates are confirmed. For the present, no ice nucleant appears to offer significant advantages over silver iodide, in any important respect, unless it should develop that some of the exotic nucleants prove particularly useful in clearing fog and stratus at temperatures only slightly below 0°C.

From the research viewpoint, however, there is good reason to pursue the search for other nucleants in order to secure better understanding of the basic mechanisms of ice nucleation. Hence, the elucidation of the nucleating action of such odd nucleants as steroids, vanillin, and even urea should be encouraged. The identification of a nucleant particularly

effective in the temperature range of 0°C to −8°C would be of considerable importance. At the same time, it seems well to stress that little is likely to be gleaned from wholesale testing of all shelf-chemicals.

The two nucleating agents that have been used the most in past years are solid carbon dioxide (dry ice) and silver iodide. Our present knowledge of the action of each of these two agents is far from complete. Although dry-ice seeding to dissipate supercooled fog and low stratus is now operationally feasible, there has never been a definitive experimental and theoretical study of the microphysics of the dry-ice seeding process. Apparently dry ice induces crystallization by a two-step process of homogeneous nucleation: (1) water droplets form in the cold and highly supersaturated environment of a dry-ice pellet; (2) the water droplets then crystallize as long as they are colder than about −40°C. Once they have survived the initial transition phases they will grow in the environment of the supercooled cloud at the expense of the water droplets. An early optimistic estimate (*Langmuir*, 1948a) suggested that a 1-g pellet might produce 10^{17} ice crystals as it fell through a supercooled cloud. The dry-ice seeding rate selected for use in Project Scud was based on this early estimate. Laboratory studies by aufm Kampe and Weickmann later suggested that a value of about 10^{10}/g was more realistic. Still lower effective rates (about 10^8 crystals/g) were indicated by theoretical analyses that *Braham and Sievers* (1957) conducted in an effort to elucidate the negative results obtained in the University of Chicago dry-ice seeding of midwestern cumulus clouds. In a recent study, *Eadie and Mee* (1963) have given a plausible basis for understanding why the ice-crystal production rate must fall off rapidly as the degree of cloud supercooling diminishes. They present a simple model for relating crystal production to terminal fall velocity of dry-ice pellets and find a production rate of about 10^{10}/g at −7°C, about seven orders of magnitude lower than Langmuir's early estimate. These studies help to clarify the mechanism of dry-ice seeding, but they by no means answer all questions of practical interest in operational dry-ice seeding. More elaborate laboratory studies are in order, and more thorough theoretical analyses of the heat-transfer and aerodynamics of free-falling dry-ice pellets are needed in order optimally to plan fog- and stratus-dissipation operations.

In the case of silver iodide, much more work has been done in an effort to reveal its mode of nucleation. A good summary has been given by *Fletcher* (1962) for work done up to about 1961. That we do not know all we need to know about silver iodide is best illustrated by the fact that the number of workers who feel that silver iodide functions best as a freezing nucleus is still about equal to the number who feel that it functions best as a deposition (sublimation) nucleus. Inasmuch as optimal seeding techniques will probably be quite different, depending on which

of these modes of nucleation is the most effective, one can see how uncertain we are of the efficacy of operational silver iodide seeding programs to induce precipitation.

The further question of the extent and nature of photolysis of silver iodide nuclei when exposed to sunlight is by no means well answered. That photolysis does occur is clear from many studies (e.g., *Reynolds et al.*, 1951; *Smith and Heffernan*, 1956; *Bryant and Mason*, 1960; *Burley*, 1964), but all too little is known as to how photolysis destroys nucleating efficiency and at what rate destruction proceeds for specified illumination conditions for nuclei from specified generators. It remains a possibility that the nature of photolytic decay of the mixed-salt nuclei likely to be produced by typical field generators is very different from the laboratory decay of single crystals of silver iodide (e.g., *Boucher*, 1957). Further work both in the laboratory and in the field is needed to illuminate this very significant practical question.

The photolysis question is related to a recently raised question as to whether AgI nuclei can somehow persist in a target region and produce delayed seeding effects weeks and months after release. This so-called "carry-over effect," first suspected by L. O. Grant (as a result of observations made in the course of the Colorado State University winter snowpack seeding trials near Climax, Colorado) seems quite improbable on physical grounds, although weak evidence for the same effect has been reported for some of the Australian trials. If real, it will have disastrous implications for detection of seeding effects in typical randomized trials. It is an urgent problem demanding clarification. Perhaps instrumental effects are involved; perhaps some previously unrecognized phenomena are responsible.

The list of unanswered questions relating to the use of silver iodide as a nucleant in modifying supercooled clouds could be extended considerably. For example, almost nothing is known in detail about the chemical and physical processes taking place in silver iodide generators. Essentially empirical methods were used (*Fuquay and Wells*, 1957) in improving the output of the particular acetone-propane generator (Sky-fire generator) that has been regarded for a number of years as the most effective generator of its type (*Fuquay*, 1960). It is conceivable that full knowledge about rates and details of all pertinent processes taking place within such generators might lead to still further improvement in their output; in any event, the present level of understanding of generator processes is quite inadequate. The question of how best to calibrate generator output requires more careful attention, and the need for intercomparison tests under uniform conditions is very great. Something tantamount to a quality-control program of regular testing of the output of generators used in operational seeding may be desirable. The

rate at which a seeding agent is dispersed in order to produce a certain weather modification should be a parameter of prime concern; yet it is hard to obtain a clear picture from the literature. One finds that many commercial operators, and not a few noncommercial groups carrying out seeding on a research basis, have only very limited information on the actual output of the generators they use. Some seeders employ generators that burn string impregnated with a silver iodide solution; some use electric-arc generators; most use acetone-propane generators. Such intercomparison tests as have been carried out in the past (e.g., *Fuquay and Wells*, 1957) suggest large differences in efficiency, as measured in terms of nuclei produced (effective at a specified temperature such as $-10°C$) per gram of silver iodide consumed.

Since there now appears to be evidence that significant increases of precipitation may, under certain meteorological conditions, be stimulated by silver iodide seeding, the goal of optimizing operational seeding now demands much more vigorous effort directed toward the detailed physical and chemical processes taking place within generators. Above all, we must learn whether design modifications can in any way alter the chemical composition and perhaps the crystallographic form of the nuclei produced in generators of various types. It is quite conceivable that some of the marked differences in apparent seeding effects using different types of generators have resulted from unknown differences in nuclei emitted. Also, the controversy over whether silver iodide nuclei function better if introduced into the bases of clouds to be carried aloft by cloud updrafts or, alternatively, if injected directly into the upper supercooled levels (as by artillery, rockets, or aircraft), may be found to hinge upon physical or chemical differences in nuclei produced by different modes of generation. Careful laboratory and engineering work on generators seems very much in order.

Much more study of optimal methods of introducing AgI (or other nucleants) into clouds is required. As fundamental a question as that of the relative advantages of aircraft seeding and ground-generator seeding remains cloaked in uncertainty. In the few studies that have been carried out on rates of turbulent diffusion of ground-generated AgI nuclei, disparate results have been obtained. Although orographic conditions probably favor rapid ascent of nucleants into cloud bases, there is some evidence suggesting great complexity of actual plume trajectories. Over flatland target areas, commercial seeders have depended on selective ascent into the strongest convective clouds as the process assuring injection into the cores of the clouds; but quantitative data supporting the effectiveness of this process are very limited. The use of low-cost rockets as a means of introducing nucleants into clouds may warrant much more serious attention. Development of cheap, minimal-accuracy guidance and

fusing systems (say, capable of targeting to within a kilometer at ranges up to about 100 km) might completely alter the logistics and hence the economics of hail suppression, since seeding could then be carried on from widely separated launch centers that might be manned at costs compatible with hail-suppression benefits. The Russian experience with such techniques surely warrants careful study.

When one turns from modification agents intended to induce phase-change in natural supercooled clouds to agents intended to accomplish other modifications, one finds few concepts holding any great promise of practical significance, though some warrant attention at least on a re-search basis. Calcium chloride spray was shown by *Houghton and Radford* (1938) to effectively dessicate appreciable volumes of nonsuper-cooled fog, but since that time there have been no serious attempts to build upon their experience. Carbon-black seeding has been suggested both as a means of inducing convective formation of new clouds and as a means of inducing premature evaporation of warm clouds. Little promise has been demonstrated in either of these directions. Altering the drop-size distribution of warm clouds by increasing the number of condensation nuclei has been suggested by *Weickmann* (1963). Some preliminary field experiments have been conducted using chlorsulfonic acid as an agent intended to achieve this end. In these as in other scattered instances, it must be emphasized that the immediate need is for well-controlled field and laboratory experiments, and no claims are made for present operational usefulness.

On the need for large dynamic cloud chambers

As indicated many times in this report, the energetics of the atmosphere are of such an enormous scale, compared to the agents that man can afford to release to stimulate precipitation, that all such attempts must be contingent upon natural mechanisms capable of amplifying the effects of the man-made stimuli. Many potentially usable amplifying effects can be listed, such as the stimulated release of latent heat, the triggering of convective instability, and the initiation of droplet coalescence by elec-trical, turbulent, or chemical surface-energy effects. All these effects—and numerous others not mentioned here—represent highly nonlinear phenomena, capable of amplifying small disturbances and causing large energy exchanges to result from selective, low-energy-level disturbances that might intentionally be introduced to cause desired weather-modifica-tion effects.

Due to the fact that such effects must be used in an amplifying mode to induce precipitation in a practical manner (and, indeed, it is likely that they have an amplifying effect even in natural precipitation), the

equations describing each such effect are nonlinear. Consequently, the study of each of these amplifying effects by itself involves a complicated and delicate analysis of the instabilities of such equations. Moreover, the actual precipitation phenomena involve several of these effects concurrently, so that systems of such equations must be examined for interactive and cumulative instabilities capable of producing major energy conversions.

Under such conditions, the mathematical analysis of the system of equations leads to two distinct kinds of difficulty—first, finding suitable classes of solutions; and second, having found them, deciding which solutions are relevant to any physical situation of interest. If these problems are solvable at all, many different solutions are often possible, and these solutions are not related to each other in a simple way. In fact, the determination of which solutions are relevant to a given physical situation is apt to be critically influenced by the nature of the initial and boundary conditions. Usually the only effective approach to such problems is to set up and calculate such systems of equations in close coordination with a step-by-step experimental program of verification, whenever this is possible.

In connection with weather-modification programs, and also with research in cloud physics and meteorology *per se,* there might be great value in a new type of large-scale test facility, which could be called a "dynamic cloud chamber." The main purpose of such a facility would be to set up steady-state processes providing for the observation of irreversible changes on a dimensional scale on which many generations of particle accretion and coalescence could be viewed. Transport and exchange processes could be observed and various coefficients evaluated so as to provide step-by-step verification of theoretical relationships. It should be possible to control boundary conditions accurately and to measure dynamical variables near critical points. Nuclei of various sorts, and other classes of disturbance, could be introduced to permit gauging their effects on accretion, capture phenomena, radiant energy exchange, and related factors. In short, the aim would be to provide a means of exploring complicated exchange processes with regard to various critical amplifying instabilities.

There would be many technical problems to consider in the design of such an apparatus, of course; the suggestion being made at this time is simply to give the possibility early and careful study. It has been reported that very large meteorological cloud chambers have been built and are being tested in other countries (notably the Soviet Union), not specifically of a dynamic steady-state type suggested here, but nevertheless of far greater capability than any that now exist in the United States.

It is emphasized that a large-scale dynamic cloud chamber by itself would probably be relatively ineffective unless operated in intimate collaboration with an effective theoretical team working in cloud physics, and unless well supported by high-speed computational capability.

RESEARCH IN ATMOSPHERIC WATER BUDGETS

It now appears necessary to accelerate research on quantitative details of the atmospheric water budget in order to provide a variety of data on vapor transport needed in assessing the full consequences of artificial stimulation of precipitation. Present knowledge of vapor budgets of individual storm types and of the general circulation will have to be refined far beyond present levels of understanding in order to provide answers to questions centering around possible downwind effects and other indirect effects of cloud-modification operations. But, in addition, other more distant prospects for weather and climate modification demand that we prepare to estimate in advance what any given proposed modification technique will do to regional and global precipitation and water-vapor transport. The large natural variability of the latter quantities suggests the probability of equally large sensitivity to almost any type of large-scale modification program, regardless of whether that modification acts directly on precipitation or only indirectly on other atmospheric parameters. Even if we agree that such programs are still decades away, it seems essential to begin now to lay foundations of basic understanding that will ultimately be needed to back up decisions.

Other purely scientific considerations also make it highly desirable that our present rudimentary knowledge of global atmospheric water budgets, fluxes, and fields of flux divergence be rapidly improved. A single example may suffice to support this assertion: Water vapor is itself one of the principal heat-transfer media of the atmospheric heat engine; thus full knowledge of the distribution and movements of water vapor is basically important to our understanding of the energetics of the general atmospheric circulation and hence to prediction of weather.

One of the first studies to reveal the kind of insights that can be derived from regional studies of atmospheric vapor flux was that of *Benton et al.*, (1950). Using data on upper-level winds and humidities at points well distributed around the Mississippi basin, they were able to show that approximately 90 percent of the precipitation falling on the Mississippi basin is advected into that basin from remote, chiefly oceanic, sources. The remaining 10 percent is recycled water, that is, water that has previously fallen as precipitation within the basin and has subsequently evaporated only to be precipitated once again before it can escape the basin. (Earlier thinking had often led to the incorrect con-

clusion that recycled water is of dominant importance; there are widespread lay misconceptions about this effect even today.) Although his analysis must be checked further with the more complete data now available, it is interesting to note that in a similar study of transport into and out of the airspace over the European U.S.S.R., *Budyko* (1958) found almost exactly the same percentage contributions of imported and of recycled water. Also, rough confirmation of the dominance of imports from remote sources was obtained by *Begemann and Libby* (1957) in an indirect analysis based upon use of natural and nuclear-bomb tritium as a tracer. Their results suggested that at least two thirds of the vapor precipitated in the upper Mississippi Valley is of oceanic origin.

To cite only a single example of a modification proposal whose prospects for success are helpfully illuminated by availability of even such rough findings as the above, one may consider the recurrent proposals, previously discussed, to induce additional regional precipitation by creating artificial lakes in continental interiors. The folly of such schemes is strongly underscored by the findings of these regional transport studies, for these studies show clearly that the raw material out of which local precipitation is formed is brought in from quite remote sources. This inference has recently been supported in an interesting way by *Barnes* (1965), who notes that hemispheric patterns of convergence and divergence of vapor flux exhibit a rough average separation between divergence centers (sources of net evaporation) and convergence centers (sinks of net precipitation) of the order of 1,000 km, a conclusion that is also supported by considerations of mean atmospheric residence time for water vapor (about 10 days) and mean wind speeds. With such long mean travel distances for vapor molecules, it becomes clear that lake-building can scarcely be guaranteed to produce local precipitation increases. And, we might add, the fact that multiple recycling of water does not occur to any great extent precludes the hope of getting many units of precipitation for each unit of original evaporation.

During the past 10 years there have been a number of attempts to construct dynamical models of the atmosphere for the purpose of simulating short-term evolutions of hydrologic processes on the scale of extratropical cyclones. Although progress has been slow, the present level of understanding of the physics and dynamics has yielded an ability to predict large-scale precipitation for three and four days by wholly theoretical means (*Smagorinsky et al.*, 1965). The quantitative precision, although practically useful for many purposes (that is, within a factor of 2 or 3), is as yet inadequate as a modification control. However, it will provide the possibility for testing various assumptions on precipitation efficiency and will also provide a basis for dynamically accounting for the water-substance budget of individual large-scale storms.

Studies of vapor transport on a larger scale, carried out by *Benton and Estoque* (1954) for the flux over the entire continent of North America for the year 1949, have indicated, at least to first approximation, many intriguing features of the three-dimensional distribution and seasonal variations of vapor transport. Their results immediately suggest the desirability of securing more such data to permit reliable estimates of regional effects on extensive cloud-modification projects. To anticipate the downwind effect upon, say, the Colorado River basin or the plains states due to massive seeding of the winter snowpack along the Sierras and Cascades requires quantitative knowledge of natural patterns of flux convergence across these orographic barriers. The 1949 data analyzed by Benton and Estoque indicate that perhaps a quarter to a third of the vapor crossing the coast in winter is removed by natural processes by the time the westerly air currents reach the longitude of the Colorado Basin; but it would be rather unsafe to generalize from a single year's data. This study does show the need for more extensive sampling of the natural variability of such transport data to support valid inferences concerning interregional effects of modification programs.

Looking farther into the future of weather and climate modification, one sees a need for global vapor-transport data of improved quality. A study of vapor flux over the entire Northern Hemisphere for the year 1950 was carried out by *Peixoto* (1958), and a number of further studies of the data by Starr and his associates at the Massachusetts Institute of Technology have shed further light on many features of the hemispheric vapor budget and its intimate relation to energy budgets. In one of these studies, *Lufkin* (1959) examined hydrologic and climatological implications of the hemispheric vapor-divergence patterns and showed that oceanic salinity anomalies match rather well the location of major centers of vapor divergence, as they should if each is the result of intense local evaporation. Analyses of the total meridional flux of water vapor (*Starr et al.*, 1958) and of the eddy flux components (*Starr and Peixoto*, 1964) have been completed for the single year 1950. More recently, the particularly dense distribution of upper-air observing stations operative during the 1958 International Geophysical Year have come under analysis (*Barnes*, 1965; *Crisi*, 1965) and show in still richer detail the intricacies of Northern Hemisphere vapor-flux patterns that must be better understood before any large-scale modification proposals can be seriously considered. Barnes has employed some of these results to appraise the chances of success of two proposed regional climate-modification plans (flooding of the Qattara Depression in Egypt and damming of the Mediterranean).

No corresponding Southern Hemisphere analyses are today possible, due to quite inadequate coverage of upper-air observing stations. Whether

the two hemispheres are, on the average, coupled by vapor exchanges is still not clear, although existing studies suggest that only rather small net exchange occurs for the year as a whole. In limited longitudinal intervals, and for limited portions of the year, it is quite certain that interhemispheric vapor exchange occurs, as in the case of strong transequatorial flux of vapor from the Southern Hemisphere during the peak of the Indian monsoon. Detailed studies will have to await improvements in the network of Southern Hemisphere observing stations.

It should be stressed that it is highly desirable that any studies of water budgets should be combined, if at all possible, with concurrent studies of energy budgets, since from a fundamental point of view the water budget is best regarded as only part of the total energy budget. When one assembles all the upper-air data required to examine water budgets, he has available many of the raw data requisite to a concurrent analysis of energy budgets; thus the energy budgets should also be analyzed wherever possible. Indeed, until one relates energy and vapor budgets, the vapor budgets are not fully elucidated, so it follows that our understanding of storm and regional water-vapor budgets cannot really be regarded as complete until we study them in the broader energy context.

In the foregoing comments we have dealt with the need for more complete climatology of vapor transport on scales varying from watershed up to hemispheric and global dimensions. It is also important to attain far better understanding of the water budgets of individual storms, and even of individual clouds. Information on this scale is perhaps more urgently needed for immediate assessment of precipitation-stimulation programs than are the continental and hemispheric transport data. A model for stratiform (e.g., warm-front) precipitation was computed by *Wexler and Atlas* (1958). Their model indicated that the natural precipitation process is extremely efficient, but that variations of updraft velocity in the storm life may lead to water storage, and that increases of the particle numbers by seeding might increase the rainout by 5 to 10 percent at the expense of the areas farther downwind. *Elliott and Hovind* (1964b) have attempted a rough estimate of the fractional rainout of condensate in west-coast winter orographic storms, and conclude that only about one fourth of the water condensed in storms crossing the coastal range is actually precipitated. A pioneering study in the vapor budget of the thunderstorm, conducted by *Braham* (1952) on the basis of the Thunderstorm Project data, indicated that only about 10 percent of all the vapor condensed to the liquid or solid phase within a typical midlatitude thunderstorm appears at the ground as precipitation. Of the other 90 percent, a large portion is evaporated in the downdraft, and a small portion is evaporated from the top and sides of the cloud or left over as remnant cloud.

As one goes down the scale to smaller shower clouds, one finds nearly complete lack of quantitative estimates of water budgets. General considerations would suggest that the fractional rainout of condensate must decrease steadily as one goes to shower clouds of decreasing size, and is certainly zero for the fair-weather cumulus clouds. Urgently needed are systematic surveys, using observational facilities of fairly elaborate design, to secure reliable estimates of water budgets of all cloud types and storm types for which there is any hope for useful modification. Improvement in cloud modification obviously demands this type of data; today it is virtually nonexistent.

SPECIAL PROBLEMS IN ATMOSPHERIC SIMULATION

Measurement of global weather parameters

Our knowledge of the observed properties of the atmosphere is almost exclusively derived from the operational network of the world's weather services. Although scientific users of the data from these sources may sometimes be dissatisfied with their quality, on the whole they consider themselves fortunate for this relatively rich source. That the existing network is inadequate to meet the new and growing demands of atmospheric general-circulation research has already been elaborated in a report by the Panel on International Meteorological Cooperation, *National Academy of Sciences* (1966).

The principal conclusions that emerged from that study were summarized as follows:

1. A major international research and development program directed toward an experiment to measure the large-scale motions of the entire lower atmosphere for a limited period of time is fully justified by scientific potential and technological capabilities.

2. A system utilizing satellite tracking and interrogation of large numbers of constant-volume instrumented balloons and buoys is feasible for this purpose. It is strongly recommended that such a system be developed, and that efforts be made to reduce balloon weight and cost as far as possible. Efforts should also be made to ensure that the balloons will constitute no hazard to aircraft. This system should be supplemented by the use of satellite radiometric observations and conventional radiosonde observations in order to ensure satisfactory data coverage throughout the atmosphere. The global extension of the present meteorological network by conventional means is not economically feasible.

3. The principal objective to be achieved by the global observation experiment is that it define the entire atmosphere as a single physical entity. The integrity of this concept should not be lost through any consideration of expediency or through diversion of the observational system to other purposes.

4. Preliminary numerical-model calculations indicate that if the initial state of the entire atmosphere were known with sufficient accuracy, the large-scale motions would,

in principle, be predictable as a determinate physical system for a period of approximately two weeks, but that beyond this period, only averages or statistical distributions would remain predictable. Additional calculations with more accurate models should be made to sharpen this estimate.

5. Steps should be taken to extend our knowledge of dry and moist turbulent exchange processes in order to relate the turbulent fluxes of heat, momentum, and moisture to the large-scale atmospheric variables. This will require development of observational facilities and new instrumentation for use in a series of observational studies over various types of terrain and sea conditions in both high and low latitudes. Special observational studies of the interaction of macro- and meso-scale convective phenomena with the macro-scale motions should be carried out in the tropics as a means of acquiring better physical understanding of tropical disturbances and their role in the general circulation.

6. Electronic computers with speeds 100 to 1,000 times the speed of an IBM 7094 will be needed to make full use of the global data for general-circulation and long-range-prediction calculations. Parallel-processor computers capable of these speeds are now in the advanced planning stages or beyond, and could become available by the time a global observation system is established. It is strongly recommended that such computers be procured.

This report of the Academy Panel on International Meteorological Cooperation provides an eloquent scientific justification for a global observational experiment and has also made an attempt to provide a first-cut specification for an observational system. Whereas these specifications are adequate for preliminary design purposes, it is clear that more definitive studies will be required.

Perhaps the most pressing immediate problem to be settled is whether the relative advantages of volumetric measure over point measures are completely overwhelming for defining the atmosphere appropriate to dynamical modeling. The impact of instrumental accuracy hinges crucially on the observation system's spectral properties with respect to those of the prediction model. For example, it may be that the accuracy demands on a volumetric instrumental system are as much as an order of magnitude less stringent than what we think desirable for present-type instrumentation. Such a determination may completely eliminate from consideration whole classes of instrumentation or types of platforms or vehicles.

Hence, in order to specify scientific requirements upon a world weather system, a number of systematic studies must be conducted to determine the following:

(a) the relative influence of volumetric versus point measures in defining spectral properties, with particular attention to the effects of aliasing*;

*Aliasing is the introduction of error in the Fourier analysis of a discrete sampling from continuous data, by which frequencies too high to be analyzed with the given sampling interval contribute to the amplitude of lower frequencies.

(b) how best to exploit the quasi-redundancy of atmospheric-state variables resulting from dynamical coupling. This will differ in the boundary layer versus the free atmosphere and in equatorial versus higher latitudes.

(c) how to extract the maximum information content from an observational system of which a sophisticated dynamical prediction model is an essential element; i.e., it is a four-dimensional curve-fitting problem, representing the ultimate sophistication of current "objective analysis" techniques;

(d) whether atmospheric nonlinearity demands a computational mesh denser than the data mesh;

(e) the relative consequences of instrumental errors;

(f) the climatological distribution of time-space spectral properties. The above determinations will permit an optimal specification as to what should be measured and where, how often, and how accurately, for a given limitation on predictability that the observational system imposes.

However, the atmospheric circulation cannot be considered independently of the oceans. That this is true for seasonal time scales is brought to focus by the fact that in the summer hemisphere the meridional solar-heating gradient is virtually nonexistent. The atmosphere is driven by the oceanic meridional temperature gradient established by the solar radiation of many previous winters. But even for shorter periods, of the order of a week, there is growing evidence of significant interaction of the surface layers of the ocean with the atmosphere. Conversely, the deep oceans must play a significant role in accounting for atmospheric variations from year to year.

Hence, to study the entire fluid envelope as a single system requires a congruous observational knowledge of the oceans. Here oceanic science is not as fortunate as atmospheric science. Systematic, operational, observational surveillance of the oceans is virtually nonexistent. Oceanographers, when they need more data, must organize a special cruise of limited geographical scope and time duration. Fortunately, the time scales significant for heat transports in the ocean are much longer than those in the atmosphere. Thus the high observing frequency in the atmosphere (at least every 12 hours) is probably unnecessary for the oceans. The space resolution, however, must be of the same order as for the atmosphere, that is, of the order of hundreds of kilometers. An exception may have to be made in the major ocean currents. For example, higher resolution may be required in the vicinity of the Gulf Stream. The primary dependent variables are, of course, the two horizontal-motion components, the temperature, and the salinity.

Measurements of boundary-layer processes

In meteorology, as in general fluid dynamics, the term "boundary layer" is used to denote that fluid layer adjacent to an interface (usually, a solid or liquid surface tangential to a gas stream, or a solid boundary tangential to the flow of liquids), in which the presence of the boundary significantly affects the distribution of motion vectors (velocity profiles) and other fluid properties. For global considerations of the general circulation, the entire troposphere could be considered as the boundary layer. However, it is most customary to restrict the use of the term to the lowest kilometer or so of the troposphere, where the effects of surface friction and of exchange processes in connection with the local energy budget of the earth–air interface are most directly and immediately displayed.

Due to the vertical extent of this layer, the special instrumentation to measure meteorological processes can be listed in two categories. First, largely nonstandard ground-based equipment, typical for micro-meteorological and microclimatological studies, is needed for the measurement of boundary fluxes of mass, momentum, and energy, turbulent fluctuation, and mean gradients of pertinent atmospheric quantities. Second, largely nonstandard equipment of another type is needed to measure transfer processes and variables in the bulk of the boundary layer, which requires either the use of very tall towers or remote-sensing or airborne equipment. Regardless of which of the numerous boundary-layer characteristics one considers, there is continuing need for improved sensing, more efficient data-transcription, and automatic data-handling, and last but not least, interpretation of the data in direct relation to the existing larger-scale weather and circulation conditions. It has not been too difficult to devise new sensors and to expose them on a tower. But demonstrating the physical significance, separating signal from noise, and aligning the results with the state of the lower atmosphere have not always been possible according to ideal standards.

The vertical extent of the atmospheric boundary layer and the complexity of its structure probably can be best studied with some degree of completeness in large and coordinated field programs. For example, the "Great Plains Turbulence Field Program," organized by the Geophysics Research Directorate of the U.S. Air Force in 1953 (see *Lettau and Davidson,* 1957), was a coordinated field effort involving a dozen different research groups.

For future work, it would be especially desirable to have the light airplane perfected as a tool for boundary-layer surveys, with remote measurement of surface characteristics. Ideally, it would be desirable to

record or derive all properties of the underlying surface by flying above it. Only then can the horizontal variability of surface heat-budget conditions be really assessed.

On the need for larger computers

A number of problems now being worked on within the context of geophysical hydrodynamics may provide us with the basis for an estimate of the computation demands we can anticipate within the next decade. The characteristic time and space scales range from those relevant to turbulent exchange processes to those responsible for maintaining the large-scale ocean circulations. Although studies of these problems are being conducted with presently available computer capacities considerable compromise is being made in order to solve them in a nontrivial fashion. Table 8 lists these problem areas, with space resolutions, dimensionality, and pertinent dependent variables that represent somewhat less of a compromise than now necessary. It is of interest that, despite a span of simulated-experiment time ranging from 10 sec to 30 years (a ratio of 10^{-8}), the number of dependent variables generated during the experiment is roughly invariant, i.e., 10^{10}. Thus, the problems are of similar computational magnitude. It is also typical of and common to such hydrodynamical calculations that approximately 200 computer operations are required to generate one dependent variable at each new time step, so that somewhat in excess of 10^{12} computer operations are necessary to complete a single experiment.

It is worthwhile at this point to remark on the significance of the basic assumptions made in evolving the above figure. It is quite clear that the computational bulk depends most sensitively on the space resolution. Taking into account the effect of space resolution on time step, then for quasi-horizontal motions (atmospheric and ocean general circulation) a twofold increase in the linear resolution requires an eightfold (2^3) increase in total computation. For fully three-dimensional problems (turbulence and convection), the same increase in linear resolution requires $2^4 = 16$ times more computation. However, an increase in the number of dependent variables by one (e.g., adding ozone in a general circulation model) will increase the number of calculations by only 20 percent. Likewise, an increase of vertical resolution in the quasi-horizontal models has a relatively small impact on computing time. Increases in physical complexity and fidelity in formulating the slow-acting processes (e.g., radiation, ocean coupling on the atmospheric general circulation) are trivial in the total number of computations because they are done rather infrequently. Hence, increases in resolution to reduce truncation error may easily increase the number of computer operations per experiment by one to three orders of magnitude.

TABLE 8

	Type of Simulation Experiment			
	Turbulence	Convection	Atmospheric General Circulation	Ocean General Circulation
Filtering approximation	Boussinesq	Boussinesq	hydrostatic	geostrophic
Dimensionality	3	3	3	3
Dependent variables	$4 (u, v, w, T)$	$5 (u, v, w, T, r)$	$4 (u, v, T, r)$	$4 (u, v, T, S)$
Number vertical levels	(Estim. 30)	100	10	10
Number horizontal mesh points	(Estim. 100^3)	100^2	100^2	100^3
Time step[a]	(Estim. 0.001 sec)	5 sec	5 min	5 hr
Simulated experiment time	(Estim. 10 sec)	3 hr	100 days	30 years
Dependent variables/time step	10^8	5×10^5	4×10^5	4×10^5
Time steps/experiment	10^4	2×10^3	3×10^4	6×10^4
Dependent variables/experiment	10^{12}	10^9	10^{10}	2×10^{10}

[a] Dictated by the space resolution and the linear computation stability requirements.

We are now ready to define what is a reasonable time to spend in doing a numerical-simulation experiment. The purpose of doing experiments is to study the nonlinear response of the numerical model to changes in parameters. By this process one develops the sought-after insight. Since a series of such experiments is necessary in order to span a physically realizable range of each of the parameters, we can subjectively arrive at a threshold of tolerance. Obviously 1,000 hr of machine time (one half year of first-shift time) for a single experiment is intolerable. The threshold is more likely to be 100 hr (2.5 weeks of first-shift time), but a convenient time is closer to 10 hr. Since we have noted that an order of magnitude can easily be lost through a factor of 2 increase in linear resolution, we shall use the "convenient" time as the basis of determining the needed computing power. Hence, we need to be able to perform 10^{12} computer operations in 10 hr, or approximately one operation every 30 nsec. This is the computing power needed to solve, "conveniently," problems now under study, and exceeds by a factor of 50 the power of the fastest of the present computing systems. (Stretch is a 3-μsec machine and CDC-6600 is a 600-nsec machine.)

It is to be noted that the above estimates were based on currently prevailing techniques of explicit differencing of nonlinear hydrodynamical equations. In the context of differencing techniques, considerable effort is presently being given to devising and studying the properties of other methods (e.g., implicit higher-order approximations) that may be more desirable in terms of accuracy, isotropy, stability, quadratic integral preservation, and efficiency. Furthermore, spectral or semi-Lagrangian techniques may gain favor if some of the present technological difficulties are overcome. Such changes will not only influence the computer operations necessary for a given geophysical simulation, but may also fundamentally alter the degree of parallelism of calculation and thus the utility of particular logical organizations of computers.

THE SPECIAL PROBLEM OF TROPICAL METEOROLOGY

Convective processes in the tropics

The tropics, about 75 percent of which is ocean, covers about half the area of the earth. It is the boiler of the giant heat engine that runs the atmosphere. Even though most of us are affected by the weather in the higher latitudes, we cannot be disinterested in the meteorology of the tropics.

In the tropics the sun provides more energy to the earth than is lost to space by long-wave radiation, while the reverse is true at higher

latitudes. Recent measurements from TIROS satellites show that the albedo of the tropics is about 5 percent lower than was estimated previously. This apparently small error has a very large effect because the actual heat source of the tropics is determined by the difference of two large numbers—the solar input and the infrared radiation to space. The solar energy actually absorbed requires that 40 percent more heat be exported out of the belt 20°N to 20°S by ocean and atmospheric currents than was thought to occur in earlier estimates. Most of the fraction of the solar energy not reflected back out to space (about 72 percent in the belt 20° of latitude each side of the equator) passes through the atmosphere to be absorbed in the upper layers of the ocean. The ocean can absorb great quantities of heat because it has three times the area of land; it has a larger heat capacity than soil; and it has a deeper layer involved through vertical mixing than does the land. Ocean currents account for 10 to 25 percent of the heat transport out of the tropics, but these currents are mainly wind-driven, so that even this heat removal process is coupled to the atmosphere, which removes the remaining much larger fraction.

Heat is added to the lower layers of the atmosphere by contact, by radiation, and by evaporation from the ocean surface, but it is removed from the tropics in the upper layers of the tropical troposphere far removed from the surface. Moreover, the middle layers of large regions of the tropical atmosphere are decoupled by the trade-wind inversion from the surface layers where the heat exchange with the ocean takes place. Heat is carried into the upper layers of the troposphere by towers of cumulus convection and also by radiation. Thus, while we can describe in general terms the chain of processes by which the average heat absorbed by the oceans is added to the atmosphere and carried out of the tropics, we cannot describe the details. Links in the chain involve nonlinear processes that make the interaction between the various processes rather complex. The details in the chain of processes are not understood well enough to be quantitatively useful. This lack of understanding is a serious barrier to the development of mathematical models of the general circulation that will treat the tropical atmosphere and its reaction to the ocean heat source more realistically. The basic processes and scales of circulation in the tropics are really no different from those at higher latitudes. The differences are mainly in degree. Horizontal temperature gradients are much smaller in the tropics; kinetic-energy generation on smaller scales is more important; and local heat sources can be relatively more important. The simplifying assumptions one ordinarily applies to the motions in the higher latitudes cannot be applied to the same degree in the tropics; hence, the interactions are more difficult to unravel. Consequently, it will be necessary to relate the mean

values of heat, moisture, and momentum transfer to the ordinarily ob-
served synoptic macrovariables such as temperature, pressure, and mois-
ture gradients, and cloudiness, rainfall, and radiation.

Cumulus convection is the process by which the latent and sensible
heat gained by the surface air layer from the ocean is mixed into the
lower half of the troposphere. However, deep convection is required to
carry the heat into the layers where the maximum transport to higher
latitudes occurs. But the large-scale tropical motions may suppress or
intensify the cumulus activity. These large-scale motions modulate and
organize the release of buoyant energy in the small space-scale and short
time-scale motions of cumulus convection. Thus, whether or not the
convection is deep depends among other things upon the large-scale
motion. The large-scale motions depend upon how the heat is removed
from the ocean and released in convection activity. So it is necessary to
determine the distribution of convective systems, characterize the popula-
tion of individual cells in each system, and determine the vertical transfer
of heat, momentum, and moisture of individual and groups of convective
cells. We can scarcely hope ever to measure the heat transfer of all the
convective elements of the vast ocean areas. All we can hope to do is
obtain information on the statistics of the effects of individual cloud
systems on the general flow, and vice versa.

A satellite in suitable orbit can give us the necessary information on
the geographical distribution of cloud systems. If it has high enough
visual and infrared resolution we can even characterize the population
of these distributions, and further, if these measurements have precise
geometry and are available frequently, we can determine the displace-
ments of the systems and possibly gain information on their organiza-
tion. All this is not enough; we must still "calibrate" these cloud systems
in order to obtain their vertical heat flow. "Calibration," as we shall
shortly see, will be no easy task.

A key to understanding the tropics is understanding the dynamics of
cumulus convection. In the paragraphs that follow, the reader should get
the impression that much remains to be done in the laboratory, in the
field, and in the development of a suitable theoretical basis for under-
standing the dynamics of convective processes. This is not to say that
little work has been done in the past; indeed, a considerable advance has
already been made by a mere handful of researchers here and abroad
who have been bold enough to observe, simulate, and theorize on this
complex process. Yet they are the first to say that "today the develop-
ment of a quantitative theory for the birth and development of cumulus
clouds is very necessary." In this document we have available only a
few paragraphs in which to discuss the complexity of the problem and
its central importance. In doing so we run the risk of making the problem

sound so complex as almost to defy solution. Thanks to those who have already done the pioneering, a solution seems definitely within reach, and the sizable investment required to make it a reality is fully justified.

The energy of ascending currents is derived from atmospheric instability. The instability can develop fair-weather cumulus clouds, a few hundred meters deep with vertical velocities near 1 m/sec, that last only tens of minutes. Or, the instability can develop a cumulonimbus cloud, extending to the stratosphere and with vertical velocities in excess of 20 m/sec, that lasts a few hours; in this process, a few inches of rain is typically released. Or, the instability can develop into a roaring hurricane, with chimneys of cumulonimbus clouds making up the eye wall and tower to the stratosphere (others can be arranged in spiral bands), the whole affair lasting days. On the opposite end of the violent convective scale is the tornado, matching or exceeding the hurricane in concentrated violence. The wide variations in both time scale and space scale make convective systems not only difficult to understand, but also to observe.

To add to the difficulty, the atmospheric instability can be triggered by a number of different mechanisms. Over land, "thermals" from the heated surface can trigger the instability, as can the vertical currents caused by the atmosphere moving over an undulating land surface. Differences in temperature and surface roughness between land and water surfaces can also trigger the instability. Large-scale motions can trigger or suppress the atmospheric instability, and convective motion of one cloud element can trigger release of atmospheric instability in a new neighboring cloud or a new community of neighboring clouds.

Finally, the convective process can change the surface to suppress or enhance further release of instability. Cool rain or cloud shadow can suppress the triggering thermals. In a hurricane the violent surface wind tears off the tops of gigantic waves, so it is difficult to say where the ocean stops and atmosphere begins. The vastly increased heat transfer across the agitated ocean surface is the primary energy source that maintains the storm.

All these interactions mitigate against a quick solution of the dynamics-of-convection problem. Yet a much better understanding of it is fundamental to further progress. It goes without saying that a better understanding of the dynamics of convection would permit better answers to the questions of where, when, how much, and when-not-to concerning studies of artificial stimulation of precipitation. Answers to these questions can make the difference between success and failure in dealing with the problem. This alone is reason enough to pursue the cumulus-dynamics questions. But this understanding, theoretical or observational, will also allow us to "calibrate" the tropical cloud systems mentioned earlier that are of great importance in the tropical heat budget.

Each convective cloud, large or small, in its complete life history slightly rearranges the atmosphere in which it is immersed. The new arrangement, expressed in terms of the sensible, latent, and total heat and geopotential energy, represents the information needed. One could, in principle, measure the cloud itself—its vertical velocities, its vapor, liquid, and solid water content, its temperature—as functions of space and time. Because the life history of a cloud is so short, usually less than an hour, such measurements pose a severe sampling problem with presently available techniques. An alternative proposal is a before-and-after measurement on the environment itself. However, in the tropics, horizontal temperature and moisture gradients are very small, and detection of small changes in the wind, temperature, moisture, and radiative fields is difficult. While the parameter changes are indeed small, the sampling problem is not so severe. If the measurements can be made with sufficient precision, a major portion of the problem is solved. For example, recent developments in inertial guidance systems indicate that reference platforms that can be carried by aircraft may indeed have the required stability and accuracy necessary to detect integrated winds of only a few centimeters per second in distances of tens of kilometers.

Technology can make an enormous contribution to the needed cumulus convection and tropical observational study program. On the other hand, the mathematical atmospheric-model makers are in an excellent position to define what needs to be measured, and how well, how often and on what space scale it must be done. Further, they will be able to test new systems of observations before any costly program to procure and deploy these new systems is carried out on a large scale. Unfortunately, the model makers are not fully informed on what observational technology can do for them, and the technologists have only a superficial understanding of the needs of the model makers. The situation is not likely to improve unless a definite program to improve communication between these two groups is devised. Only then will the full impact of new technology on the observing program be realized.

The comprehensive investigation of hurricanes

Our understanding of the hurricane mechanism is now good enough to warrant a substantial increase in the research effort to tie down the hurricane parameters in quantitative terms, with some assurance of a payoff. New tools to aid in this research include the synchronous-orbit meteorological satellite, subsonic jet aircraft that can reach high altitudes, surface vessels, submarines, and associated data-telemetry devices. These observational tools can provide the data that must eventually be fed into the mathematical models that are expected ultimately to reproduce hurricane dynamics quantitatively.

Since synchronous-orbit communication satellites have already been proved by the Syncom program and the Early Bird satellite, a synchronous-orbit satellite equipped to "televise" the earth's disk below is now technically feasible and would provide the opportunity to monitor hurricanes continuously from birth to death. The view from a near-earth satellite is so fleeting that it is not possible to obtain any real measure of weather motions. For example, in the TIROS series of satellites, the life history of a model storm had to be derived from a number of different storms at different times and places and in different stages of development. On the other hand, a synchronous satellite could continuously monitor the weather during the daylight hours over a large portion of the earth's disk centered on the subsatellite point. A new photo of the same portion of the earth's disk would be available every few minutes, compared to every 12 hr with the TIROS or NIMBUS satellites.

This frequent observational capability is no trivial matter; it is fundamental. Frequent observations of the same place make it possible to measure the motions of the atmosphere and storms directly rather than inferring them from details in one picture. A synchronous weather satellite, even at the price of lower resolution, can yield direct evidence of cloud and storm evolution, because the many pictures form a "movie" of the clouds' motions. The synchronous satellite will provide us not only with key information about the internal motions of the hurricane, but also information concerning the motions of the atmosphere in which the storm is immersed—how they affect the hurricane, and vice versa. An understanding of this interaction is crucial to better understanding of hurricane motion needed for improved forecasts. Thus, the synchronous meteorological satellite is a key observational tool—one that can provide us with vital information that cannot be obtained by other means at comparable cost.

While the synchronous meteorological satellite can give us a much better look at the hurricane from above, a much-improved view from below is also required. The major portion of the energy of the hurricane is derived from the warm ocean under the storm. The air temperature near the surface in the interior of a hurricane is approximately the same as the temperature outside the hurricane. The low pressure at the hurricane's core is, in extreme cases, equivalent to that found near 5,000 ft altitude. Thus, the isothermal expansion of the air from the outside of the hurricane to the inside can only mean that large quantities of heat have been removed from the underlying ocean and added to the hurricane air mass as sensible and latent heat. There is some evidence that a hurricane leaves a cold "wake" as it passes over the ocean, but it has not been possible to do the enormous "calorimeter experiment" because of lack of data on ocean temperature and current in the

hurricane's vicinity. It may be possible to obtain this valuable ocean temperature and current data from a submarine equipped to probe the ocean near the surface while it remains at a safe depth at which the violent wave action is damped out. Sounding devices released from a submarine may measure the vertical temperature distribution in the disturbed ocean layers, and may even be made to rise through the atmospheric boundary layer.

The new views of the hurricane's top and bottom do not dispense with the need to probe the hurricane's interior and environment, as is being done in present hurricane-research programs. Since the main object of the research program is to obtain quantitative information about the hurricane over the early portions of its life history, longer-range reconnaissance aircraft, properly instrumented, are also required. These are needed for high-level measurements as well as measurements at intermediate and low levels. Such aircraft must be properly stressed for hurricane penetrations, and must be equipped with high-quality meteorological instrumentation and recording systems. The capability must also be developed for dropping low-altitude sounding devices in hurricanes to study the details of the atmospheric boundary layer and the sea surface temperature.

Many of the components of the synchronous satellite–airplane–submarine system needed to determine the hurricane's energy budget are already being implemented in various government agencies, but so far there is no program organized to make use of the opportunity that these new tools present. Such a program must include a detailed statement of the requirements, a determination of how well these new tools can meet these requirements, what new developments (such as submarine sensing systems, or precision-navigation schemes) are needed, a plan for operating the program, a plan of scientific and operational management, a budget plan, and a host of others. Many of the new tools needed and much of the manpower and skill required are available in different government agencies; yet there is no single responsible agency to mount such a large observation program. Under present lines of authority, it is likely that the measurement tools will be available long before the management and program tools can be organized.

If such an observational program can be mounted and we gain a better understanding of hurricane energetics, then we will be able realistically to assess the many schemes—some of which look quite promising when measured in terms of our present understanding—that have been suggested to ameliorate the effects of such mighty storms. We may be able to control this giant some day, but only if we understand her first.

References

Adderley, E. E. (1961), Analysis of cloud seeding trials in Australia, paper presented at International Conference on Cloud Physics, *I.U.G.G. Monographs 16*, 77–78.

Advisory Committee on Weather Control (1957), *Final Report, I*, U.S. Government Printing Office, Washington, D.C.

Appleman, H. (1959), Investigation into the formation of hail, *Nubila, 2*, 28.

aufm Kampe, H. J., H. K. Weickmann, and H. H. Kedesdy (1952), Remarks on "Electron-microscope study of snow-crystal nuclei," *J. Meteorol., 9*, 374.

aufm Kampe, H. J., J. J. Kelly, and H. K. Weickmann (1957), Seeding experiments in subcooled stratus clouds, in *Cloud and Weather Modification, Meteorol. Monographs, No. 11*, American Meteorological Society, 86–111.

Badgley, F. I. (1961), Heat balance at the surface of the Arctic Ocean, *Proc. 29th W. Snow Conf.*, Spokane, Wash., April 1961.

Barnes, A. A. (1965), Atmospheric water vapor divergence: Measurements and applications, in *Humidity and Moisture, 2, Applications*, Reinhold, New York, pp. 513–522.

Battan, L. J., and C. H. Reitan (1957), Droplet size measurements in convective clouds, in *Artificial Stimulation of Rain*, Pergamon Press, London, p. 184.

Battan, L. J. (1959), Duration of tornadoes, *Bull. Am. Meteorol. Soc., 40*, 340.

Battan, L. J., and A. R. Kassander, Jr. (1960), Artificial nucleation of orographic cumulus clouds, in *Physics of Precipitation, Geophys. Monograph No. 5*, American Geophysical Union, pp. 409–411.

Battan, L. J., and A. R. Kassander, Jr. (1962), Evaluation of effects of airborne silver iodide seeding of convective clouds, *Scientific Report No. 18*, Institute of Atmospheric Physics, University of Arizona.

Battan, L. J. (1963), Relationship between cloud base and initial radar echo, *J. Atmospheric Sci. 2*, 333.

Battan, L. J. (1965), A view of cloud physics and weather modification in the Soviet Union, *Bull. Am. Meteorol. Soc., 46*, 309.

Beckwith, W. B. (1965), Supercooled fog dispersal for airport operations, *Bull. Am. Meteorol. Soc., 46*, 323.

Begemann, F., and W. F. Libby (1957), Continental water balance, ground

water inventory and storage times, surface ocean mixing rates and world-wide water circulation patterns from cosmic-ray and bomb tritium, *Geochim. Cosmochim. Acta., 12,* 277.

Benton, G. S., R. T. Blackburn, and V. O. Snead (1950), The role of the atmosphere in the hydrologic cycle, *Trans. Am. Geophys. Union, 31,* 61.

Benton, G. S., and M. A. Estoque (1954), Water-vapor transfer over the North American continent, *J. Meteorol., 11,* 462.

Bergeron, T. (1933), On the physics of cloud and precipitation, in *Verbal Proceedings, I.G.G.U., Fifth General Assembly, Lisbon, 2,* 156–175.

Bergeron, T. (1949), The problem of artificial control of rainfall on the globe, *Tellus, 1,* 32.

Bergeron, T. (1960), in *Physics of Precipitation, Geophys. Monograph No. 5,* American Geophysical Union, pp. 399–401.

Black, J. F. (1963), Weather control: Use of asphalt coatings to tap solar energy, *Science, 139,* 226.

Blanchard, D. C. (1957), Discussion of rain-drop distributions made during Project Shower, Hawaii, 1954, in *Artificial Stimulation of Rain,* Pergamon Press, London, pp. 213–223.

Bolin, B., and C. D. Keeling (1963), Large-scale atmospheric mixing as deduced from the seasonal and meridional variations of carbon dioxide, *J. Geophys. Res., 68,* 3899.

Bonner, W. D. (1965), Statistical and kinematical properties of the low-level jet stream, *Research Paper No. 38,* Satellite and Mesometeorological Research Project, University of Chicago.

Boucher, R. J. (1957), Operation Overseed, in *Final Report of the Advisory Committee on Weather Control, II,* pp. 127–136.

Bourquard, A. D. (1963), Ice nucleus concentrations at the ground, *J. Atmospheric Sci., 20,* 386.

Bowen, E. G. (1950), The formation of rain by coalescence, *Australian J. Sci. Res., Ser. A, 3,* 193.

Bowen, E. G. (1952), A new method of stimulating convective clouds to produce rainfall and hail, *Quart. J. Roy. Meteorol. Soc., 78,* 37.

Bowen, E. G. (1965), Lessons learned from long-term cloud-seeding experiments, *Proc. Intern. Conf. Cloud Phys.,* May 24–June 1, 1965, Tokyo and Sapporo, IAMAP-IUGG, pp. 429–433.

Braham, R. R., Jr. (1952), The water and energy budgets of the thunderstorm and their relation to thunderstorm development, *J. Meteorol., 9,* 227.

Braham, R. R., Jr., L. J. Battan, and H. R. Byers (1957), Artificial nucleation of cumulus clouds, in *Cloud and Weather Modification, Meteorol. Monographs No. 11,* American Meteorological Society.

Braham, R. R., Jr., and J. R. Sievers (1957), Overseeding of cumulus clouds, in *Artificial Stimulation of Rain,* Pergamon Press, London, pp. 250–266.

Braham, R. R., Jr. (1963), Some measurements of snow pellet bulk-densities, *J. Appl. Meteorol., 2,* 498.

Braham, R. R., Jr. (1964), What is the role of ice in summer rain-showers?, *J. Atmospheric Sci., 21,* 640.

Braham, R. R., Jr. (1965), Project Whitetop—a five-year randomized cloud seeding study, paper presented at 244th National Meeting, American Meteorological Society, *Conference on Cloud Physics and Severe Storms,* Reno, Nevada, October 18–22, 1965.

Brewer, A. W. (1949), Evidence for a world circulation provided by the measurements of helium and water vapor, *Quart. J. Roy. Meteorol. Soc., 75,* 351.

Brooks, C. E. P. (1951), Geological and historical aspects of climatic change, in *Compendium of Meteorology,* American Meteorological Society, Boston, pp. 1004–1018.

Brown, H. (1957), The carbon cycle in nature, in *Fortschr. Chem. Org. Naturstoffe, 14,* 317.

Brown, E. N., and R. R. Braham, Jr. (1963), Precipitation particle measurements in cumulus congestus, *J. Atmospheric Sci., 20,* 23.

Brownlee, K. A. (1960), Statistical evaluation of cloud seeding operations, *J. Am. Stat. Assoc. 55,* 446.

Bryant, G. W., and B. J. Mason (1960), Photolytic de-activation of silver iodide as an ice-forming nucleus, *Quart. J. Roy. Meteorol. Soc., 86,* 354.

Bryson,R. A., and D. A. Baerreis (1965), Possibilities of major climatic modification and their implications; Northwest-India, a case study, *Proc. Conference on Social and Economic Implications of Weather Modification,* Boulder, Colorado, July 1965.

Budyko, M. I. (1958), The heat balance of the earth's surface, transl. by Office of Technical Services, U.S. Dept. of Commerce, pp. 224–231.

Budyko, M. I. (1962), Climatic change and climatic control, (Translated title; original paper in *Akademiia Nauk, USSR, Vestnik, No. 7,* 1962) AMS Translation under Contract AF 19 (628)-3880, Boston, Mass., 1964.

Burley, G. (1964), Ice nucleation by photolyzed silver iodide, *Phil. Mag., 10,* 527.

Byers, H. R. (1965), Identification of ice nuclei in the atmosphere, *Proc. Intern. Conf. Cloud Phys.,* May 24–June 1, 1965, Tokyo and Sapporo, IAMAP-IUGG, pp. 126–130.

Callendar, G. S. (1940), Variations of the amount of carbon dioxide in different air currents, *Quart. J. Roy. Meteorol. Soc. 66,* 395.

Callendar, G. S. (1958), On the amount of carbon dioxide in the atmosphere, *Tellus, 10,* 243.

Chapman, S. (1958), Corona-point-discharge in wind and applications to thunderclouds, in *Recent Advances in Atmospheric Electricity,* Pergamon Press, London.

Colegrove, F. D., W. B. Hanson, and F. S. Johnson (1965), Eddy diffusion and oxygen transport in the lower thermosphere, submitted to *J. Geophys. Res.*

Cospar, (1964), Report of the Consultative Group on Potentially Harmful Effects of Space Experiments, Florence.

Court, A. (1960), Reliability of hourly precipitation data, *J. Geophys. Res., 65,* 4017.

Craig, H. (1957), The natural distribution of radiocarbon and the exchange time of carbon dioxide between atmosphere and sea, *Tellus 9,* 1.

Crisi, A. R. (1965), Hemispheric water vapor balance during 1958, in *Humidity and Moisture, 2, Applications,* Reinhold, New York, pp. 502–512.

Davies, D. A. (1954), Experiments on artificial stimulation of rain in East Africa, *Nature, 174,* 256.

Davies, L. G., D. R. Booker, and L. C. Hosler (1965), Observations of natural and artificial alteration of cumulus buoyancy, *Proc. Intern. Conf. Cloud Phys.,* May 24–June 1, 1965, Tokyo and Sapporo, IAMAP-IUGG pp. 20–24.

Dessens, H. (1960), Severe hailstorms are associated with very strong winds between 6000 and 12,000 meters, in *Physics of Precipitation, Geophys. Monograph No. 5,* American Geophysical Union, pp. 333–338.

Dessens, H. (1963), Conperendu de la campagne, *Associations d'Etudes No. 12,* pp. 3–28.

De Vries, H. (1959), Measurements and use of natural radiocarbon, *Res. Geophys.,* 169.

Dixon, J. P., and J. P. Lodge, (1965), Air conservation report reflects national concern, *Science, 148,* 1060.

Donaldson, R. J., Jr., A. C. Chmela, and C. R. Shackford (1960), Some behavior patterns of New England hailstorms, in *Physics of Precipitation, Geophys. Monograph No. 5,* American Geophysical Union, pp. 354–368.

Douglas, R. H., and W. Hitschfeld, (1959), Patterns of hailstorms in Alberta, *Quart. J. Roy. Meteorol. Soc., 85,* 105.

Downie, C. S., and R. B. Smith (1958), Thermal techniques for dissipating fog from aircraft runways, *Air Force Surveys in Geophysics No. 106,* AFCRC-TN-58-477, Geophysics Research Directorate, Air Force Cambridge Research Center, Bedford, Mass.

Dubois, E. (1965), Fog dispersal on runway approaches, *Weather, 20,* 313.

Eadie, W. J., and T. R. Mee (1963), The effect of dry-ice pellet velocity on the generation of ice crystals, *J. Appl. Meteorol., 2,* 260.

East, T. W. R., and J. S. Marshall (1954), Turbulence in clouds as a factor in precipitation, *Quart. J. Roy. Meteorol. Soc., 80,* 26.

East, T. W. R. (1957), Precipitation of convective water clouds, in *Artificial Stimulation of Rain,* Pergamon Press, London, pp. 192–201.

Elliott, R. D. (1962), Note on cloud seeding evaluation with hourly precipitation data, *J. Appl. Meteorol., 1,* 578.

Elliott, R. D., and R. W. Shaffer (1962), The development of quantitative relationships between orographic precipitation and air mass parameters for use in forecasting and cloud seeding evaluation, *J. Appl. Meteorol., 1,* 218.

Elliott, R. D., and E. L. Hovind (1964a), On convection bands within Pacific coast storms and their relation to storm structure, *J. Appl. Meteorol., 3,* 143.

Elliott, R. D., and E. L. Hovind (1964b), The water balance of orographic clouds, *J. Appl. Meteorol., 3,* 235.

Erickson, E. (1963), Possible fluctuations in atmospheric carbon dioxide due to changes in the properties of the sea, *J. Geophys. Res., 68,* 3871.

Estoque, M. A. (1963), A numerical model of the atmospheric boundary layer, *J. Geophys. Res., 68,* 1103.

Estoque, M. A., and W. S. Yee (1963), Air–earth interface characteristics and the atmospheric boundary layer, *Scientific Report No. 3,* Meteorology Division, Hawaii Institute of Geophysics, University of Hawaii.

Ewing, M., and W. L. Donn (1958), A theory of the Ice Ages, *Science, 127,* 1159.

Fawbush, E. J., and R. C. Miller (1953), A method for forecasting hailstone size at the earth's surface, *Bull. Am. Meteorol. Soc., 34*, 235.

Fenn, R., and H. Oser (1962), Theoretical considerations of the effectiveness of carbon seeding, in *USASRDL Tech. Rept. No. 2258*, Fort Monmouth, N.J., p. 29.

Findeisen, W. (1938), Die Kolloidmeteorologischen Vorgaenge bie der Niederschlagsbildung, *Meteorol. Z., 55*, 121.

Fletcher, N. H. (1962), *The Physics of Rainclouds*, Cambridge University Press, Cambridge.

Fletcher, J. O. (1965), The heat budget of the Arctic Basin and its relation to climate, *Rept. No. R-444-PR*, The RAND Corporation, Santa Monica, Calif.

Flint, R. F. (1953), Evidence from glacial geology as to climate variation, in *Climate Change*, Harvard University Press, Cambridge, Mass., pp. 165–178.

Fournier d'Albe, E. M. (1957), Cloud seeding trials using common salt, in *Artificial Stimulation of Rain*, Pergamon Press, London, pp. 207–212.

Fujita, T., and H. R. Byers (1962), Model of a hail cloud as revealed by photogrammetric analysis, *Nubila, V*, 85.

Fultz, D., R. R. Long, G. V. Owens, W. Bohan, R. Kaylor, and J. Weil (1959), Studies of thermal convection in a rotating cylinder with some implications for large-scale atmospheric motions, *Meteorol. Monographs No. 21*, American Meteorological Society.

Fuquay, D. M., and H. J. Wells (1957), The Project Skyfire cloud seeding generator, in *Final Report of the Advisory Committee on Weather Control, II*, pp. 273–282.

Fuquay, D. M. (1960), Airborne silver iodide generator, *J. Am. Soc. Civil Engr., Irrigation and Drainage Division, 86, 1R*, 87.

Fuquay, D. M., and R. G. Baughman (1962), Project Skyfire lightning research, *Final Rept.*, National Science Foundation Grant No. NSF–G10309.

Gabriel, K. R. (1965), L'expérience de pluie provoquée en Israel, Quelques résultats partiels, *J. Rech. Atmos., 2*, 1.

Geiger, R. (1965), *The Climate near the Ground*, Harvard University Press, Cambridge, Mass. (4th ed.).

Gilman, D. L., J. R. Hibbs, and P. L. Laskin (1965), *Weather and climate modification, a Report to the Chief United States Weather Bureau*, U. S. Department of Commerce.

Godson, W. L., C. L. Crozier, and J. D. Holland (1965), *Silver iodide cloud seeding by aircraft in western Quebec, Canada, 1959–63*, Dept. of Transport, Meteorological Branch, Toronto, Canada.

Goyer, G. G. (1965a), Effects of lightning on hydrometeors, *Nature, 206*, 1203.

Goyer, G. G. (1965b), Mechanical effects of a simulated lightning discharge on the water droplets of "Old Faithful" geyser, *Nature, 206*, 1302.

Grandoso, H. N., and J. V. Iribarne (1961), The experiment on hail prevention in Mendoza, Argentina, *Proc. Intern. Conf. Cloud Phys.*, Canberra, Sept. 1961, pp. 64–66.

Greenfield, S. M., R. E. Huschke, Y. Mintz, R. R. Rapp, and J. D. Sartor (1962), A rationale for weather control research, *Trans. Am. Geophys. Union, 4*, 469.

Hadley, G. (1735), Concerning the cause of the general trade winds, *Phil. Trans. Roy. Soc., 39*, 58.

Hagen, G. E. (1961), Study, development, and testing of a fog-clearing device, *Final Rept., Contract DA36-039 10-84962*, Island Research Inc., Southfield, Mass.

Hall, F. (1957), The Weather Bureau ACN Project, in *Cloud and Weather Modification, Meteorol. Monographs No. 11*, American Meteorological Society, pp. 24–46.

HASP (1960), Special report on high altitude sampling program, *DASA Rept. 532B*, NAS 14–24386, pp. 1–252.

Hide, R. (1958), An experimental study of thermal convection in a rotating liquid, *Phil. Trans. Roy. Soc. London, A, 250*, 441.

Hocking, L. M. (1959), The collision efficiency of small drops, *Quart. J. Roy. Meteorol. Soc., 85*, 44.

Hoecker, W. H. (1963), Three southerly low-level jet streams delineated by the Weather Bureau Special Pibal Network of 1961, *Monthly Weather Rev., 91*, 573.

Hoffer, T. E., and R. R. Braham, Jr. (1962), A laboratory study of atmospheric ice particles, *J. Atmos. Sci., 19*, 232.

Hori, T. (1953), ed., *Studies on Fogs in Relation to Fog-Preventing Forest*, Institute of Low Temperature Science, Hokkaido University, Sapporo.

Houghton, H. G. (1938), Problems connected with the condensation and precipitation processes in the atmosphere, *Bull. Am. Meteorol. Soc., 19*, 152.

Houghton, H. G., and W. H. Radford (1938), On the local dissipation of natural fog, *Papers in Phys. Oceanog. and Meteorol., IV:3*, Massachusetts Institute of Technology and Woods Hole Oceanographic Institution, October 1938.

Houghton, H. G. (1950), A preliminary quantitative analysis of precipitation mechanisms, *J. Meteorol., 7*, 363.

Houghton, H. G. (1957), Present position and future possibilities of weather control, *Bull. Am. Meteorol. Soc. 38*, 567.

Hovind, E. L. (1965), Precipitation distribution around a windy mountain peak, *J. Geophys. Res., 70*, 3271.

Howell, W. E. (1949), The growth of cloud drops in uniformly cooled air, *J. Meteorol., 6*, 134.

Howell, W. E. (1960), Cloud seeding in the American tropics, in *Physics of Precipitation, Geophys. Monographs No. 5*, American Geophysical Union.

Iribarne, J. V. and Rosa G. DePena (1962), The influence of particle concentration on the evolution of hailstones, *Nubila, V*, 7.

Isono, K. (1955), On ice-crystal nuclei and other substances found in snow crystals, *J. Meteorol., 12*, 456.

Isono, K. (1961), Some results of cloud seeding experiments with use of silver iodide ground generators, paper presented at International Conference on Cloud Physics, *I.U.G.G. Monographs No. 16*, pp. 75–76.

Istomin, V. G. (1961), Magnesium and calcium ions in the upper atmosphere of the earth, *Dokl. Akad. Nauk SSSR, 136*, 1066.

Johnson, F. S., and E. M. Wilkins (1965), Thermal upper limit on eddy diffusion in the mesosphere and lower thermosphere, *J. Geophys. Res., 70*, 1281.

Jeeves, T. A., L. LeCam, J. Neyman, and E. L. Scott (1955), On the methodology of evaluating cloud seeding operations, in *Weather Modification Operations in California*, Bull. No. 16, California State Water Resources Board, June 1955.

Junge, C. E., O. Oldenberg, and J. T. Wasson (1962), On the origin of the sodium present in the upper atmosphere, *J. Geophys. Res., 67*, 1027.

Junge, C. E. (1963a), *Air Chemistry and Radio-activity*, Academic Press, New York.

Junge, C. E. (1963b), Studies of global exchange processes in the atmosphere by natural and artificial tracers, *J. Geophys. Res., 68*, 3849.

Kanwisher, J. (1960), pCO_2 in sea water and its effect on the movement of CO_2 in nature, *Tellus, 12*, 209.

Kaplan, L. D. (1960a), The influence of carbon dioxide variations on the atmospheric heat balance, *Tellus, 12*, 204.

Kaplan, L. D. (1960b), Reply to Gilbert N. Plass: Comment on Lewis D. Kaplan: Influence of carbon dioxide variation on the atmospheric heat balance, *Tellus, 13*, 301.

Kellogg, W. W. (1956), Diffusion of smoke in the stratosphere, *J. Meteorol., 13*, 241.

Kellogg, W. W. (1964), Pollution of the upper atmosphere by rockets, *Space Sci. Rev., 3*.

Koenig, L. R. (1963), The glaciating behavior of small cumulonimbus clouds, *J. Atmospheric Sci., 20*, 29.

Korb, G., and F. Moeller (1962), Theoretical investigation on energy gain by absorption of solar radiation in clouds, *Final Tech. Rept., Contract DA–91–591–EUC–1612*, Ludwig-Maximilian University, Meteorological Institute, Munich.

Kratzer, P. A. (1956), *The Climate of Cities* (translation of a book entitled: *Das Stadtklima*, originally published in 1956 by Vieweg, Braunschweig, Germany) AMS Translation under Contract AF 19 (604)–6113, Bedford, Mass., 1962

Kraus, E. B., and P. Squires (1947), Experiments on the stimulation of clouds to produce rain, *Nature, 159*, 489.

Kumai, M. (1951), Electron-microscope study of snow-crystal nuclei, *J. Meteorol., 8*, 151.

Kumai, M. (1957), Electron-microscope study of snow-crystal nuclei II, *Geofys. Pura Appl., 36*, 169.

Kumai, M. (1961), Snow crystals and the identification of the nuclei in the northern United States of America, *J. Meteorol., 18*, 139.

Kumai, M., and K. E. Francis (1962), Nuclei in snow and ice crystals on the Greenland Ice Cap under naturally and artificially stimulated conditions, *J. Atmos. Sci., 19*, 474.

Kumai, M. (1964), A study of ice fog and ice fog nuclei at Fairbanks, Alaska, *Research Report No. 150*, U.S. Army Cold Region Research and Engineering Laboratory, Hanover, N.H.

Landsberg, H. E. (1956), The climate of towns, in *Man's Role in Changing the Face of the Earth*, University of Chicago Press, Chicago.

Langmuir, I. (1948a), The growth of particles in smokes and clouds and the production of snow from supercooled clouds, *Proc. Am. Phil. Soc., 92*, 167.

Langmuir, I. (1948b), The production of rain by a chain reaction in cumulus clouds at temperatures above freezing, *J. Meteorol., 5*, 175.

Langmuir, I. (1951), Cloud seeding by means of dry ice, silver iodide, and sodium chloride, *Trans. N.Y. Acad. Sci., 14*, 40.

Langmuir, I. (1953), Analysis of the effects of periodic seeding of the atmosphere with silver iodide, *Final Rept., Project Cirrus, Part II, Rept. No. RL–785, Contract No. DA–36–039–SC–15345*, General Electric Research Laboratory, Schenectady, New York.

Leighton, P. A. (1961), *Photochemistry of Air Pollution*, Academic Press, New York.

Leith, C. E. (1965), Numerical simulation of the earth's atmosphere, in *Methods in Computational Physics, 4, Applications in Hydrodynamics*, Academic Press, New York.

Leonov, M. P., and T. D. Nerobeeva (1965), Intensification of precipitation over a weather experiment range in the winter of 1963–1964, *Kiev Ukr. Nauchn.-Issled. Gidrolog. Inst., Tr. No. 47*, pp. 88–99 (transl. by S. W. Kellogg).

Lettau, H., and B. Davidson (1957), eds., *Exploring the Atmosphere's First Mile*, Pergamon Press, New York and London, Vols. 1 and 2.

Lettau, H., and C. R. Stearns (1963), Report on two wind-profile modification experiments in airflow over the ice of Lake Mendota, *Sec. 4, Ann. Rept. 1963, Contract DA–36–039–AMC–00878*, University of Wisconsin, Madison, 1963.

Lettau, H. (1964), Preliminary note on the effect of terrain-slope on low-level jets and thermal winds in the planetary boundary layer, *Sec. 4, Ann. Rept. 1964, Contract DA–36–039–AMC–00878*, University of Wisconsin, Madison.

Lorenz, E. N. (1963), Deterministic nonperiodic flow, *J. Atmospheric Sci., 20*, 130.

Ludlam, F. H. (1951), The production of showers by the coalescence of cloud droplets, *Quart. J. Roy. Meteorol. Soc. 77*, 402.

Ludlam, F. H. (1955), Artificial snowfall from mountain clouds, *Tellus, 7*, 277.

Ludlam, F. H. (1958), The hail problem, *Nubila, 1*, 12.

Ludlam, F. H. (1959), Hailstorm studies, *Nubila, 2*.

Lufkin, D. H. (1959), Atmospheric water vapor divergence and the water balance at the earth's surface, Sci. Rept. No. 4, *General Circulation Project, Scientific Report No. 4*, Dept. of Meteorology, Massachusetts Institute of Technology.

McDonald, J. E. (1962), The evaporation-precipitation fallacy, *Weather, 17*, 168.

Malkus, J. S. and M. E. Stern (1953), The flow of a stable atmosphere over a heated island, Part 1, *J. Meteorol., 10*, 30.

Malkus, J. S. (1963), Tropical rain induced by small natural heat sources, *J. Appl. Meteorol., 2*, 547.

Manabe, S. (1965), Dependence of the climate on the change of the content of some atmospheric absorbers, paper delivered before the Panel on Weather and Climate Modification at Woods Hole, Mass., August 1965.

Manabe, S., and R. F. Strickler (1964), On the thermal equilibrium of the atmosphere with convective adjustment, *J. Atmospheric Sci., 21*, 361.

Manabe, S., J. Smagorinsky, and R. F. Strickler (1965), Physical climatology of a general circulation model with a hydrologic cycle, *Monthly Weather Rev., 93*.

Marsh, G. P. (1874), *The Earth as Modified by Human Action,* Scribner, Armstrong and Co., New York.

Mason, B. J. (1952), Production of rain and drizzle by coalescence in stratiform clouds, *Quart. J. Roy. Meteorol. Soc., 78,* 377.

Mason, B. J. (1956), The nucleation of supercooled water clouds, *Sci. Progr., 175,* 479.

Mason, B. J., and J. Maybank (1958), Ice nucleating properties of some natural mineral dusts, *Quart. J. Roy. Meteorol. Soc., 84,* 235.

Mason, B. J., and J. Maybank (1960), The fragmentation and electrification of freezing water drops, *Quart. J. Roy. Meteorol. Soc., 86,* 176.

Mintz, Y. (1965), Very long-term global integration of the primitive equations of atmospheric motion, *World Meteorological Organization Tech. Note No. 66,* WMO-IUGG Symposium on research and development: Aspects of long-range forecasting, Boulder, Colorado, 1962.

Möller, F. (1963), On the influence of changes in the CO_2 concentration in air on the radiation balance of the earth's surface and on the climate, *J. Geophys. Res., 68,* 3877.

Moore, C., B. Vonnegut, and R. Semonin (1962), Fair weather atmospheric electric potential gradient and space charge over central Illinois, *J. Geophys. Res., 67,* 1061.

Moran, P. A. P. (1959), The power of a cross-over test for the artificial stimulation of rainfall, *Australian J. Stat., 1,* 47.

Mordy, W. A. (1959), Computations of the growth by condensation of population of cloud droplets, *Tellus, 11,* 16.

Mossop, S. C. (1963), Atmospheric ice nuclei, *Z. Angew. Math. Phys., 14,* 456.

Muller, H. G. (1964), I. Bericht uber die Hagel-Abwehrvesuche im Landkreis Rosenheim, Deutsche Versuchsanstalt für Luft- und Raumfahrt e. V.

Myers, J. N., and C. L. Hosler (1965), Preliminary investigations of fog disposal methods applicable to Greater Pittsburgh Airport, *Special Research Rept. No. CR-47,* Dept. of Meteorology, College of Mineral Industries, The Pennsylvania State University, University Park, Pa.

National Academy of Sciences (1959), Report of the Skyline Conference on the design and conduct of experiments in weather modification, *NAS-NRC Publ. 722,* Washington, D. C.

National Academy of Sciences (1966), The feasibility of a global observation and analysis experiment, report of the Panel on International Meteorological Cooperation, *NAS-NRC Publ. 1290,* Washington, D.C.

Neiburger, M., and C. W. Chien (1960), Computations of the growth of cloud drops by condensation using an electronic digital computer, in *Physics of Precipitation, Geophys. Monographs No. 5,* American Geophysical Union, pp. 191–209.

Newton, C. W., and J. C. Fankhauser (1964), On the movement of convective storms, with emphasis on size discrimination in relation to water budget requirements, *J. Appl. Meteorol., 3,* 641.

Newton, C. W. (1963), Dynamics of severe convective storms, *Meteorol. Monographs, 5,* 27, 33.

Newton, C. W., and H. R. Newton (1959), Dynamical interactions between large convective clouds and environment with vertical shear, *J. Meteorol.*, *16*, 483.

Neyman, J., E. L. Scott, and M. Vasilevskis (1960), Statistical evaluation of the Santa Barbara randomized cloud seeding experiment, *Bull. Am. Meteorol. Soc.*, *41*, 531.

Neyman, J., and E. L. Scott (1961), Further comments on the Final Rept. of the Advisory Committee on Weather Control, *J. Am. Stat. Assoc.*, *56*, 580.

Ohman, H. L. and R. L. Pratt (1956), The daytime influence of irrigation upon desert humidities, *Tech. Rept. SP–35, U.S. Army Quartermaster Res. and Dev. Command*, Natick, Mass.

Oort, A. H. (1964), On estimates of the atmospheric energy cycle, *Monthly Weather Rev., 92.*

Peixoto, J. P. (1958), Hemispheric humidity conditions during the year 1950, *General Circulation Project Scientific Rept. No. 3*, Dept. of Meteorology, Massachusetts Institute of Technology.

Phillips, N. A. (1956), The general circulation of the atmosphere: A numerical experiment, *Quart. J. Roy. Meteorol. Soc., 82*, 123.

Plass, G. N. (1956), The influence of the 15-micron carbon dioxide band on the atmospheric infrared cooling rate, *Quart. J. Roy. Meteorol. Soc., 82*, 310.

Plumlee, H. (1964), Effect of electrostatic forces on drop collision and coalescence in air, *Rept. No. CPRL 8–64 Tech. Rept. ANC 63–G2*, Charged Particle Research Laboratory, Department of Electrical Engineering, University of Illinois.

Revelle, R., and H. E. Suess (1957), Carbon dioxide exchange between atmosphere and ocean, and the question of an increase of atmospheric CO_2 during the past decades, *Tellus, 9*, 18.

Reynolds, S. E., W. Hume, B. Vonnegut, and V. J. Schaefer (1951), Effect of sunlight on the action of silver iodide particles as sublimation nuclei, *Bull. Am. Meteorol. Soc., 32*, 47.

Saenger, R. (1960), Mechanism of hail formation, in *Physics of Precipitation, Geophys. Monograph No. 5*, American Geophysical Union, 305.

Sartor, J. D. (1954), A laboratory investigation of collision efficiencies, coalescence and electrical charging of simulated cloud droplets, *J. Meteorol., 11*, 91.

Sartor, J. D. (1960), Some electrostatic cloud-droplet coalescence deficiencies, *J. Geophys. Res., 65*, 1953.

Schaefer, V. J. (1946), The production of ice crystals in a cloud of supercooled water droplets, *Science, 104*, 457.

Schleusener, R. A. (1962), The 1959 hail suppression effort in Colorado and evidence of its effectiveness, *Nubila, V*, 31.

Serpolay, R. (1965), A ground-based device for dispersal of supercooled fogs, *Proc. Intern. Conf. on Cloud Phys., May 24–June 1, 1965*, Tokyo and Sapporo, IAMAP–IUGG, pp. 410–413.

Siliceo, E. P., A. Ahumada, and P. A. Mosino (1963), Twelve years of cloud seeding in the Necaxa watershed, Mexico, *J. Appl. Meteorol., 2*, 311.

Simpson, G. C. (1941), The formation of clouds and rain, *Quart. J. Roy. Meteorol. Soc., 67*, 152.

Simpson, R. H., M. R. Ahrens, and R. D. Decker (1963), A cloud seeding experiment in Hurricane Esther, 1961, *National Hurricane Research Project, Rept. No. 60,* U.S. Department of Commerce, Weather Bureau, Washington, D.C.

Simpson, R. H., and J. S. Malkus (1963), An experiment in hurricane modification: Preliminary results, *Science, 142,* 498.

Smagorinsky, J. (1963), General circulation experiments with the primitive equations, I. The basic experiment, *Monthly Weather Rev., 91,* 99.

Smagorinsky, J., S. Manabe, and J. L. Holloway (1965), Numerical results from a nine-layer general circulation model, *Monthly Weather Rev., 93.*

Smagorinsky, J., and staff members (1965), Prediction experiments with a general circulation model: precipitation, *IAMAP/WMO International Symposium on Dynamics of Largescale Processes in the Atmosphere,* Moscow.

Smith, E. J., and K. J. Heffernan (1956), The decay of the ice nucleating properties of silver iodide released from a mountain top, *Quart. J. Roy. Meteorol. Soc., 82,* 301.

Smith, E. J., E. E. Adderley, and F. D. Bethwaite (1963), A cloud-seeding experiment in South Australia, *J. Appl. Meteorol., 2,* 565.

Smith, E. J., E. E. Adderley, and D. T. Walsh (1963), A cloud-seeding experiment in the Snowy Mountains, Australia, *J. Appl. Meteorol., 2,* 324.

Smith, E. J., E. E. Adderley, and F. D. Bethwaite (1965), A cloud-seeding experiment in New England, Australia, *J. Appl. Meteorol., 4,* 433.

Smith, T. B. (1962), Physical studies of the Santa Barbara cloud seeding project, *J. Appl. Meteorol., 1,* 208.

Sourbier, R. H., and R. C. Gentry (1961), Rainstorm in southern Florida, January 21, 1957, *Monthly Weather Rev., 89,* 9.

Spar, J. (1957), Project Scud, in Cloud and Weather Modification, *Meteorol. Monographs No. 11,* American Meteorological Society, pp. 5–23.

Squires, P., and E. J. Smith (1950), The artificial stimulation of precipitation by means of dry ice, *Australian J. Sci. Res., 2,* 232.

Squires, P. (1952), The growth of cloud drops by condensation II, The formation of large cloud drops, *Australian J. Sci. Res., A, 5,* 473.

Squires, P. (1956), The microstructure of cumuli in maritime and continental air, *Tellus, 8,* 443.

Squires, P., and S. Twomey (1960), The relation between cloud droplet spectra and the spectrum of cloud nuclei, in *Physics of Precipitation, Geophys. Monograph No. 5,* American Geophysical Union, pp. 211–219.

Starr, V. P., J. P. Peixoto, and G. C. Livadas (1958), The meridional flux of water vapor in the Northern Hemisphere, *Geofis. Pura Appl., 39,* 174.

Starr, V. P., and J. P. Peixoto (1964), The hemispheric eddy flux of water vapor and its implications for the mechanics of the general circulation, *Arch. Meteorol. Geophys. Bioklimaeol., 14,* 111.

Stout, G. E., R. H. Blackmer, and K. E. Wilk (1960), Hail studies in Illinois relating to cloud physics, in *Physics of Precipitation, Geophys. Monograph No. 5,* American Geophysical Union, pp. 369–383.

Sullivan, H. N., and D. M. Hunten (1964), Lithium, sodium and potassium in the twilight airglow, *Can. J. Phys., 42,* 937.

Thom, H. C. S. (1957a), A statistical method of evaluating augmentation of precipitation by cloud seeding, in *Final Rept. of the Advisory Committee on Weather Control, II*, pp. 5–25.

Thom, H. C. S. (1957b), An evaluation of a series of orographic cloud seeding operations, in *Final Rept. of the Advisory Committee on Weather Control, II*, pp. 25–50.

Thomas, W. L., Jr. (1956), ed., *Man's Role in Changing the Face of the Earth*, University of Chicago Press, Chicago.

Twomey, S. (1959a), The influence of droplet concentration on rain formation and stability of clouds, *Bull. Obs. Puy de Dome, 1.*

Twomey, S. (1959b), The nuclei of natural cloud formation, *Geofis. Pura Appl., 43,* 243.

Twomey, S. (1963), Measurements of natural cloud nuclei, *J. Rech. Atmos., 1,* 101.

Veraart, A. W. (1931), *Meer Zooneschijn in het Nevelig Noorden: meer Regen in de Tropen*, N. V. Seyffardt's Boek and Muziekhandel, Amsterdam.

Vickers, W. (1964), Investigation of optimum design for supercooled cloud dispersal equipment and techniques, *Rept. No. AFCRL–64–427*, Technical Operations, Inc., Cambridge, Mass.

Vonnegut, B. (1947), The nucleation of ice formation by silver iodide, *J. Appl. Phys., 18,* 593.

Vonnegut, B. (1948), Production of ice crystals by the adiabatic expansion of gas, *J. Appl. Phys., 19,* 959.

Vonnegut, B. (1956), Warm fog and stratus dissipation, *Final Rept.* June 4, 1956, *Contract DA36–039–SC–64569*, Arthur D. Little Co., Cambridge, Mass., p. 85.

Vonnegut, B., and C. B. Moore (1958), Preliminary attempt to influence convective electrification in cumulus clouds by the intervention of space charge into the lower atmosphere, Arthur D. Little, Inc., Contract Nonr–1684 (Coco), unnumbered report.

Vonnegut, B. (1960), Electrical theory of tornadoes, *J. Geophys. Res., 65,* 203.

Vonnegut, B., and C. B. Moore (1960), A possible effect of lightning discharge on precipitation formation process, *Physics of Precipitation, Geophys. Monograph No. 5,* American Geophysical Union, pp. 287–290.

Vonnegut, B., C. B. Moore, G. E. Stout, D. W. Staggs, J. W. Bullock, and W. E. Bradley (1962), Artificial modification of atmospheric space charge, *J. Geophys. Res., 67,* 3.

Waggoner, P. E., P. M. Miller, and H. C. DeRoo (1960), Plastic mulching: principles and benefits, *Conn. Agr. Expt. Sta. Bull. 634,* Storrs, Conn.

Warner, J., and S. Twomey (1956), The use of silver iodide for seeding individual clouds, *Tellus, 8,* 453.

Webb, W. P. (1931), *The Great Plains*, Ginn and Co., New York.

Wegener, A. (1911), Kerne der Kristallbildung, in *Thermodynamik der Atmosphare, Leipzig*, pp. 94–98.

Weickmann, H. K. (1953), Observational data on the formation of precipitation in cumulonimbus clouds, in *Thunderstorm Electricity*, H. F. Byers, ed., The University of Chicago Press, Chicago, pp. 66–138.

Weickmann, H. K. (1963), A realistic approach to weather control, *Z. Angew. Math. Phys., 14,* 528.

Weickmann, H. K. (1964), The language of hail storms and hail stones, *Nubila*, *VI*, 7.

Wexler, H. (1958), Modifying the weather on a large scale, *Science, 128*, 1059.

Wexler, R., and D. Atlas (1958), Moisture supply and growth of stratiform precipitation, *J. Meteorol., 15*, 531.

Wexler, R. (1960), Efficiency of natural rain, in *Physics of Precipitation, Geophys. Monograph No. 5*, American Geophysical Union.

Wiin-Nielsen, A., J. A. Brown, and M. Drake (1964), Further studies of energy exchange between the zonal flow and the eddies, *Tellus, 16*, in press.

Woodcock, A. H. (1952), Atmospheric salt particles and raindrops, *J. Meteorol., 9*, 200.

Woodcock, A. H. (1957), Atmospheric salt in nuclei and in raindrops, in *Artificial Stimulation of Rain*, Pergamon Press, London, pp. 202–206.

Zimmerman, S. P., and K. S. W. Champion (1963), Transport processes in the upper atmosphere, *J. Geophys. Res., 68*, 3049.

APPENDIXES

Appendix 1

PREPARATION OF REPORT

This final report is based on a two-year series of meetings, the dates and guest participants of which are listed below.

First Meeting: February 2, 1964
General discussion and presentation by Professor Tor Bergeron

Second Meeting: March 14 and 15, 1964

March 14, 1964
NEAR-SURFACE MODIFICATION

H. LETTAU, Department of Meteorology, University of Wisconsin, Madison, Wisconsin
P. E. WAGGONER, Agricultural Experimental Station, New Haven, Connecticut
G. E. HARBECK, General Hydrology Branch, U.S. Geological Survey, Federal Center
   Building 25, Denver, Colorado
V. E. SUOMI, Department of Meteorology, University of Wisconsin
   Madison, Wisconsin
H. WEICKMANN, U.S. Army Signal Research and Development Laboratory
   Ft. Monmouth, New Jersey

March 15, 1964
SEVERE STORM MODIFICATION

J. F. BLACK, Esso Research and Engineering Company, P. O. Box 51
   Linden, New Jersey
C. J. TODD, Meteorology Research, Inc., 2420 N. Lake Street, Altadena, California
B. VONNEGUT, Arthur D. Little Company, Acorn Park, Cambridge, Massachusetts

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L. J. BATTAN, Institute of Atmospheric Physics, University of Arizona
  Tucson, Arizona
D. M. FUQUAY, Northern Forest Fire Laboratory, U.S. Forest Service
  Missoula, Montana
R. A. SCHLEUSENER, Department of Civil Engineering, Colorado State University
  Fort Collins, Colorado

Third Meeting: May 2 and 3, 1964

May 2, 1964
IMPLICATIONS OF GENERAL CIRCULATION MODEL EXPERIMENTS
ON THE SIMULATION OF CLIMATE MODIFICATION

E. N. LORENZ, Department of Meteorology, Massachusetts Institute of Technology
  Cambridge, Massachusetts
C. LEITH, Lawrence Radiation Laboratory, University of California, P. O. Box 808
  Livermore, California
Y. MINTZ, Department of Meteorology, University of California
  Los Angeles, California
J. SMAGORINSKY, Geophysical Fluid Dynamics Laboratory, Environmental Science
  Services Administration, Washington, D. C.
R. HIDE, Department of Geology and Geophysics, Massachusetts Institute of
  Technology, Cambridge, Massachusetts

May 3, 1964
PROBLEMS OF THE CONTAMINATION OF THE UPPER ATMOSPHERE

D. M. HUNTEN, Kitt Peak National Observatory, P. O. Box 4130, Tucson, Arizona
F. MARMO, Geophysical Corporation of America, Bedford, Massachusetts
N. W. ROSENBERG, Chief, Ionospheric Perturbations Branch, Upper Atmosphere
  Physics Laboratory, Air Force Cambridge Research Laboratories
  Laurence G. Hanscom Field, Bedford, Massachusetts
F. S. JOHNSON, Southwest Center for Advanced Studies, Dallas, Texas

Fourth Meeting: June 8 and 9, 1964

June 8, 1964
CLOUD MODIFICATION

H. WEICKMANN, U.S. Army Signal Research and Development Laboratory
  Ft. Monmouth, New Jersey
R. M. CUNNINGHAM, Air Force Cambridge Research Center, Bedford, Massachusetts
J. E. DINGER, Naval Research Laboratory, Washington, D. C.
E. KESSLER, Environmental Science Services Administration, Norman, Oklahoma

June 9, 1964

P. Squires, National Center for Atmospheric Research, Boulder, Colorado
R. List, University of Toronto, Toronto, Canada
R. R. Braham, Jr., Department of the Geophysical Sciences, University of Chicago
  Chicago, Illinois
J. E. McDonald, Institute of Atmospheric Physics, University of Arizona
  Tucson, Arizona

Fifth Meeting: August 8 and 9, 1964
Discussion of the preliminary report by Panel members

Sixth Meeting: November 5 and 6, 1964

November 5, 1964
INTERAGENCY CONFERENCE ON WEATHER MODIFICATION

November 6, 1964
FEDERAL AGENCY MISSION REQUIREMENT,
ADMINISTRATIVE AND POLICY RESPONSIBILITIES
(Joint Meeting with Commission on Weather Modification,
National Science Board)

W. R. Gommel, Lt. Col., Office Director Defense Research and Engineering
  Department of Commerce
H. Lemons, Geophysics Br., OCRD, Department of the Army
R. Martin, Naval Weather Service, Department of the Navy
B. A. Silverman, Air Force Cambridge Research Laboratory
  Department of the Air Force
R. M. White, Environmental Science Services Administration
  Department of Commerce
J. Spar, Environmental Science Services Administration, Department of Commerce
H. Eckles, Office of Science Advisor, Department of the Interior
A. B. Goodman, Bureau of Reclamation, Department of the Interior
W. Garstka, Bureau of Reclamation, Department of the Interior
R. M. Robertson, National Science Foundation
T. C. Byerly, Cooperative State Research Service, Department of Agriculture
K. Arnold, U.S. Forest Service, Department of Agriculture
W. A. Raney, Agriculture Research Service, Department of Agriculture
V. G. MacKenzie, Public Health Service
  Department of Health, Education and Welfare
W. E. Eggers, Federal Aviation Agency
M. Tepper, Office of Space Science and Application
  National Aeronautics and Space Administration

Seventh Meeting: January 30 and 31, 1965

General discussion of Panel studies; presentation by Dr. Jerzy Neyman, Department of Statistics, University of California, Berkeley, California.

Eighth Meeting: March 18 and 19, 1965

March 18, 1965
GENERAL DISCUSSION: TROPICAL AND HURRICANE RESEARCH

V. E. Suomi, Environmental Science Services Administration, Washington, D. C.

March 19, 1965
ATMOSPHERIC OXYGEN AND CARBON DIOXIDE;
BOUNDARY-LAYER METEOROLOGY

L. V. Berkner, Southwest Center for Advanced Studies, Dallas, Texas
C. D. Keeling, Scripps Institution of Oceanography, La Jolla, California
H. H. Lettau, Department of Meteorology, University of Wisconsin
  Madison, Wisconsin
L. C. Marshall, Southwest Center for Advanced Studies, Dallas, Texas

Ninth Meeting: April 9 and 10, 1965

April 9, 1965
GENERAL DISCUSSION OF PANEL STUDIES

April 10, 1965
PREDICTABILITY OF THE ATMOSPHERE

J. Smagorinsky, Environmental Science Services Administration, Washington, D. C.

Tenth Meeting: May 8 and 9, 1965

May 8, 1965
FEDERAL AGENCY PROGRAMS AND PLANS;
COMMERCIAL CLOUD-SEEDING PROGRAMS

E. G. Droessler, National Science Foundation, Washington, D. C.
R. D. Elliott, North American Weather Consultants, Goleta, California
B. A. Silverman, Air Force Cambridge Research Laboratories, Bedford, Massachusetts
J. Spar, Environmental Science Services Administration, Washington, D. C.
H. K. Weickmann, U.S. Army Research and Development Laboratory
  Ft. Monmouth, New Jersey

<div align="center">

May 9, 1965

FEDERAL AGENCY PROGRAMS AND PLANS;

DISCUSSION OF PANEL STUDIES

</div>

A. M. KAHAN, Bureau of Reclamation, Denver, Colorado

<div align="center">

Eleventh Meeting: June 18 and 19, 1965

June 18, 1965

GENERAL DISCUSSION OF PANEL STUDIES;

COMMERCIAL CLOUD-SEEDING PROGRAMS

</div>

W. E. HOWELL, W. E. Howell & Associates, Inc., Lexington, Massachusetts

<div align="center">

June 19, 1965

DISCUSSION OF FEDERAL REORGANIZATION PLAN NO. 2

(Environmental Science Services Administration)

</div>

R. M. WHITE, Environmental Science Services Administration, Washington, D. C.

<div align="center">

Twelfth Meeting: August 16 through 28, 1965

Technical and Working Summer Session, Woods Hole, Massachusetts

August 16 through 20, 1965

TECHNICAL SESSIONS

</div>

D. ATLAS, Air Force Cambridge Research Laboratory, Bedford, Massachusetts
J. F. BLACK, Esso Research and Engineering Company, Linden, New Jersey
G. W. BRIER, Office of Meteorological Research, Environmental Science Services
   Administration, Washington, D. C.
L. D. CALVIN, Department of Statistics, Oregon State University, Corvallis, Oregon
L. O. GRANT, Department of Atmospheric Science, Colorado State University
   Fort Collins, Colorado
D. K. LILLY, National Center for Atmospheric Research, Boulder, Colorado
S. MANABE, Environmental Science Services Administration, Washington, D. C.
V. J. SCHAEFER, Schenectady, New York
R. A. SCHLEUSENER, South Dakota School of Mines and Technology
   Rapid City, South Dakota
J. SIMPSON, Environmental Science Services Administration, Washington, D. C.
S. M. GREENFIELD, RAND Corporation, Santa Monica, California
W. E. HOWELL, W. E. Howell & Associates, Inc., Lexington, Massachusetts
L. HURWICZ, Department of Economics, University of Minnesota
   Minneapolis, Minnesota
W. S. VON ARX, Massachusetts Institute of Technology, Cambridge, Massachusetts
W. W. KELLOGG, National Center for Atmospheric Research, Boulder, Colorado

W. KRUSKAL, Department of Statistics, University of Chicago, Chicago, Illinois
N. E. LASEUR, Department of Meteorology, Florida State University
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J. SPAR, Environmental Science Services Administration, Washington, D. C.
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August 20, 1965 (AM)
**JOINT MEETING WITH ADVISORY PANEL ON WEATHER MODIFICATION,
NATIONAL SCIENCE FOUNDATION**

August 21 through 28, 1965
WORKING AND DRAFTING PANEL SESSIONS

August 23, 1965 (PM)
**JOINT MEETING WITH COMMISSION ON WEATHER MODIFICATION,
NATIONAL SCIENCE BOARD**

Appendix 2

PRELIMINARY STUDY OF METEOROLOGICAL EFFECTS ON SPECIFIC SEEDING TRIALS

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At the request of the National Academy of Sciences Panel on Weather and Climate Modification, the RAND Corporation undertook an examination of operational cloud-seeding evaluation reports provided by the Panel. These reports had been very thoroughly reanalyzed by the Panel, which had checked the basic data and redone all the analysis. Our examination was not a study in depth. There was not enough time or manpower to do more than make a cursory study of a part of the operations. We chose to look at a group of seeding projects that took place in the northeastern United States during the latter half of 1964. Our objective was to see if there were any *obvious* natural reasons for finding more precipitation in the target areas than in the control areas. If a natural explanation of the results had been found, it would have cast doubt on the validity of the cloud-seeding results. The fact that we came to no definite conclusions does not necessarily imply, on the other hand, that the cloud-seeding was the cause of the positive results that were reported.

A SEARCH FOR STORM-TYPE DIFFERENCES

The correlation functions that were used to estimate the amount of rainfall that would have fallen on the target in the absence of seeding did not, in our opinion, consider the different types of rainstorms. The operator used control areas that in general extended from the southeast of the

target area through west to the north of the target area. A perusal of the
day-to-day surface and upper-air charts for the selected period seemed
to indicate that the type of storms that moved through the area during
that period brought in low-level moisture from the southeast. For weak
disturbances with this type of flow, it appeared that the topography
favored an excess of precipitation over the target area for those targets
near the coast. This type of storm over the east coast is generally char-
acterized by a trough to the west of the seeded areas. Since the seeding
evaluations were based on total monthly precipitation, it was decided to
stratify the historical data, on which the regression lines were constructed,
on the basis of the direction of the mean monthly 500-mb wind over the
seeded area of sectors 3, 4, 5, and 6 (see map, Figure 2.1). The sample of
historical data was therefore divided into two groups, one in which the
mean 500-mb wind was west to north and the other in which it was south
of west.

It is admitted that the synoptic approach used here is not definitive,
but it is empirically sound. Since the monthly averages of the flow patterns
aloft correspond with the historical precipitation figures, they are com-
patible. But it is difficult to apply synoptic experience to an arbitrary
average. Any future study should follow our attempted analysis on a
storm-by-storm basis, considering frontal type, with surface and upper-air
flow patterns included, along with the moisture distribution within the
system.

If, in fact, this weather type favors the precipitation over the target,
we would expect that the regression line for the south-of-west cases would
have a smaller slope and a larger intercept than the north-of-west cases;
i.e., that the south-of-west cases would predict more rainfall over the
target area when the rainfall amounts over the control area were small.
We would not expect that the effect would be noticeable when large storms
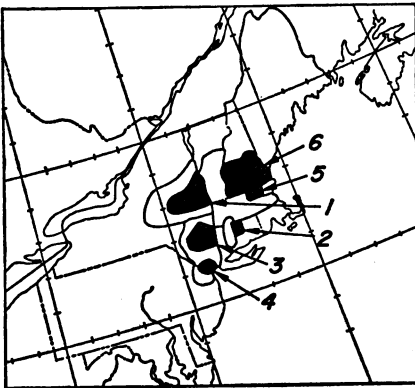covered the entire area, nor would we expect this same pattern of rainfall

FIGURE 2.1. Map of sectors analyzed.

TABLE 2.1

Section	Δa(N—S)[a]	Δb(N—S)[b]	$t_{\Delta b}$[c]	n_N[d]	n_S[e]	t_{05}(crit)[f]
3	—0.5405	0.4043	2.23	25	12	2.032
4	—0.2111	0.1144	1.41	29	25	2.010
5	+0.0817	—0.0634	0.39	24	19	2.020
6	—0.3324	0.2218	1.44	20	17	2.032

[a] Difference in intercepts: northerly group minus southerly group.
[b] Difference in slopes: northerly group minus southerly group.
[c] Student's *t* value for the difference in slope.
[d] Number of cases in the northerly group.
[e] Number of cases in the southerly group.
[f] Value of *t* that would be exceeded only 5 percent of the time by chance.

to apply to any target area located inland of some significant mountain barrier. We further decided to eliminate hurricane rainfall from the regressions since a single large rainfall amount can unduly influence the regression. We believe that a hurricane represents a clearly identifiable weather type that should be kept separate from any verification of cloud seeding. New regression lines were computed for the two different groups for each of the four sectors 3, 4, 5, and 6. The differences between the slopes and intercepts, Δa and Δb with the significance tests of the differences are shown in Table 2.1.

The negative differences of the intercepts (Δa, N−S) indicates that the regression lines for the southerly flow cases predicted more precipitation than the lines for the northerly flow cases at the small rainfall end of the curve. The positive differences in the slopes (Δb, N−S) show that for larger rainfall amounts the northerly cases are apt to have more precipitation than the southerly cases. Of the four tests that were made, three supported the hypothesis of bias due to weather type and one showed a contrary result. Of the three that supported the hypothesis only one showed a significant difference in the regression lines at the 5 percent level.

The results of our rather crude definition of weather types suggest, but certainly do not prove, that there may be differences in the rainfall patterns with different types of storms. If seeding were conducted during a month when a southerly type of storm was persistent but not very strong, then one would expect from our results that the target would show a positive anomaly from the regression line based on all the historical data.

If the seeded months for the data of sector 4 are compared to the precipitation data as independently analyzed by the Panel, it is found that three of the four months show small positive anomalies from the regression line based on all the data. If these same months are stratified by the 500-mb wind, the three positive are all southerly types and they

all fall almost exactly along the regression line for the southerly type. The other month is northerly and it is below the regression line for northerly types.

The 1 month of seeding reported for sector 3 was a northerly case and shows a great increase over the regression line for the northerly cases.

Although there is some slight evidence that a simple stratification of the data leads to slightly different relationships between target and control precipitation, the differences are not statistically significant. When the stratified regression lines are used in place of the total sample lines some of the effect of the seeding seems to be explained, but in some cases the anomalies are greater than those from the total sample. We believe that there is a hint in these studies that a good portion of the deviation of the precipitation from the regression line could be explained by careful, objective synoptic studies. Perhaps future experiments can be planned to use better objective estimates of the precipitation than the simple control-versus-target regression. They might also be so designed to use more natural periods of precipitation; that is, the verification might be arranged on a storm-by-storm basis so that additional meteorological variables could be included in the prediction system.

We have not, by this approach, been able to explain the large number of positive anomalies reported.

As another check on the indicated statistical increase of precipitation due to cloud stimulation, target and control points for August 1962, 1963, and 1964 were plotted on the regression diagram for sector 3 (Figure 2.2), August 1962 and 1964 were seeded months, and August 1963 was not seeded. As shown, August 1963 fell on the Panel's regression line while August 1962 and 1964 (the seeded months) were above, showing relatively more precipitation in the target area. It became evident also that August 1962 showed an appreciable increase in precipitation not only within the population of points on the regression diagram of the 14 target and 7 control stations for the summer-of-1962 project, but also when adjusted, to fit the 7 target and 8 control stations of sector 3 during August 1964. Both of these groups of stations were in approximately the same area, however.

To check further the validity of the gains for August 1962 and 1964, it was decided to plot the months before and after. For the seeded Augusts to show a noteworthy gain, July and September should not favor the target, that is, should fall clearly within the cloud of points on the regression diagram. As it turned out, not only was the scattering as expected, but also the plots for August 1964 and 1963 came within the envelope of this scattering. Figure 2.2 shows one July and one September favoring the

FIGURE 2.2. Comparison to regression of seeded and non-seeded months, sector 3, 1962, 1963, and 1964.
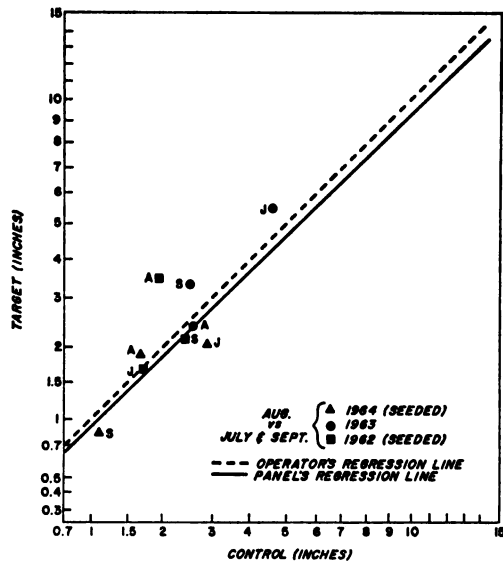
target (above the regression line), another July and September favoring the control, and the final July and September falling approximately on the regression slope.

Although hastily contrived, it seems fair to postulate therefore that August 1962 does represent a month in which cloud stimulation did increase the precipitation, but that August 1963 and August 1964 are to be considered only within the context of normal scattering upon which the regression slope was developed.

POSSIBLE EFFECTS OF OPTIONAL STOPPING
IN REGRESSION EXPERIMENTS

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1. INTRODUCTION

In a personal communication, Arnold Court * raised a number of questions about possible biases in cloud-seeding experiments. These questions warrant study, and this note will examine possible quantitative effects of one suggested source of bias, which will here be called "optional stopping." Another possible source suggested by Court, which might be called "starting-time bias," will be commented on briefly and from one aspect only at the end of this note, but I have not had time to make any quantitative assessment.

The basis of Court's point concerning optional stopping is that a cloud-seeding procedure, once started, may continue until substantial rain is received in the target area, at which time it may be shut off if the customer has received plenty of rainfall. It should be noted that stopping effects may relate not only to the time chosen to terminate an operation, but also to the selection of times at which continuing operations are reported.

The essential point, without considering the effects of regression, which will be treated below, can best be made by considering a sequence of independent normal random variables $N(\mu, \sigma)$ (i.e., mean value μ and standard deviation σ), $y_1, y_2, \ldots$. If m observations are taken, then $\bar{y} = (y_1 + \ldots + y_m)/m$ is an estimate of the mean μ. However, if the stopping

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time m is selected with reference to the data, then the expected value of $\bar{y}$ may be changed. As an example, consider the following simple rule. If $y_1 \geqq \mu$, stop with $m=1$ and put $\bar{y}=y_1$. If $y_1 < \mu$, take one more observation and stop with $m=2$, in which case $\bar{y} = (y_1 + y_2)/2$. If this rule is used, then $E(\bar{y}) = \mu + 0.2\sigma$. (Throughout this article, E will denote expected values, while barred expressions such as $\bar{y}$ will denote sample averages, $\bar{y} = (y_1 + \ldots + y_m)/m$.) We may also consider the following three-stage rule. If $y_1 \geqq \mu$ take $m=1$. If $y_1 < \mu$ but $(y_1 + y_2)/2 \geqq \mu$, take $m=2$. Otherwise take $m=3$. In this case $E(\bar{y}) = \mu + 0.28\sigma$.

One might wonder whether other two-stage or three-stage rules might produce a higher bias. I am indebted to Ralph Strauch for the information that the above two-stage rule produces the maximum possible for two stages, and the three-stage rule is within 0.01 of maximal.

Such stopping procedures may also affect the significance levels of tests, as noted below.

In the commercial operators' reports provided by the Panel on Weather and Climate Modification for this brief study, there are only three definite indications of the possible presence or effect of optional stopping. There is a possible indication in the frequency chart of the percentage increases reported by the operators, which, when plotted, gives the impression of being "censored," i.e., the frequency plot looks sheared off on the left. Optional stopping might have some tendency to produce such an appearance. (Compare the frequency plot in Figure 3.2, which looks sheared, but much less so.)

To assess the possible effects of optional stopping in the operations here considered, I have made some calculations, based on assumptions whose plausibility the reader may judge. If we assume that a three-stage rule is used, where a stage is taken to be a month, and that stopping is based on observations only on the *target* rainfall, then the *percentage increase* in precipitation could be biased upward by an amount that I think is not much more than 5 percent. The amount could reach perhaps 8 percent, using a stopping rule depending on the residuals $y - a - bx$, i.e., if the operator stopped at a time particularly chosen to make his results look good, rather than to suit the customer. However, it is difficult to see how any operator could do this and stay in business.

The effect on significance levels has not been calculated for three-stage procedures. For two-stage procedures the effect was calculated, because of its simplicity, only for the case in which stopping depends on looking at the residuals. Table 3.1 gives the effect on significance levels. The effect does not seem very large, and would be less for the more plausible case in which stopping depended on target rainfall.

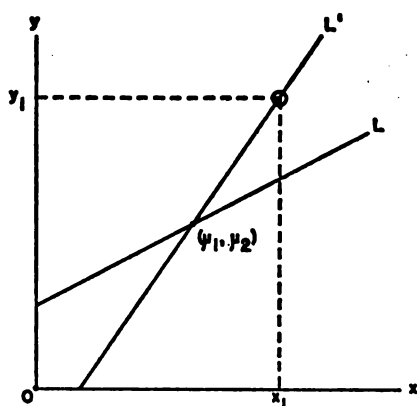Tentatively, I conclude that optional stopping effects should be con-

FIGURE 3.1.

sidered in assessing the total effect of various biases, but that by them-selves they do not account for the positive results in the operators' reports.

(I am much indebted to Ralph Strauch for assistance in preparing this note. In addition to determining stopping rules having maximal effects, he did many of the calculations. The three-stage rules that we examined in connection with effects on the percentage increase are not maximally effective among all three-stage rules, but are probably close to it.)

2. THE TWO REGRESSION LINES

Before discussing stopping rules, some remarks about regression are required.

We will consider random variables x and y, which are assumed to have a joint normal distribution with mean μ_1 and μ_2, standard deviations σ_1 and σ_2, and correlation ρ. Then

$$y = \mu_2 + \rho(\sigma_2/\sigma_1)(x - \mu_1) + \epsilon, \tag{1}$$

where the random variable ϵ (the residual) is independent of x and has mean $\mu = 0$ and standard deviation $\sigma = \sigma_2(1 - \rho^2)^{\frac{1}{2}}$. The regression of y on x is the line L,

$$\text{L: } y = a + bx, \quad a = \mu^2 - \rho(\sigma_2/\sigma_1)\mu_1, \quad b = \rho(\sigma_2/\sigma_1). \tag{2}$$

Similarly,

$$x = \mu_1 + \rho(\sigma_1/\sigma_2)(y - \mu_2) + \epsilon', \tag{3}$$

where the residual ϵ' is independent of y and has $N(0, \sigma')$, $\sigma' = \sigma_1(1 - \rho^2)^{\frac{1}{2}} = \sigma\sigma_1/\sigma_2$. The regression line of x on y is then

$$x = \mu_1 + \rho(\sigma_1/\sigma_2)(y - \mu_2),$$

which can also be written as

$$\text{L': } y = \mu_2 + (1/\rho)(\sigma_2/\sigma_1)(x - \mu_1). \tag{4}$$

Figure 3.1 shows the two lines L and L'. The general idea is the following. Suppose we have a procedure that tends to select values of y that are larger than its mean. If the value y_1 were selected, then the corresponding expected value of x, as indicated on the diagram, would be at x_1, and hence (x_1, y_1) tends to be above the line L. Similarly, selecting a value of y_1 below its mean would tend to produce a point below L.

We will suppose the regression is linear in the measurements of actual inches. The effect of the cube-root transform will be mentioned in Section 5, below.

3. TYPES OF STOPPING RULES

In what follows we will ignore the fact that the regression parameters must be estimated from past data, and will suppose they are known: $y = a + bx$, where a and b are given by Eq. (2).

It will be supposed that operations are carried out in successive intervals of time, after each of which a decision to stop may be made. I have no way of judging what is the proper interval to use. It is natural to use months, since the operational data presented to us are arranged that way, although the analysis is, of course, artificial at this point. If other data were available, one might consider using storms as the natural units.

For purely computational reasons I have not considered anything more complicated than three-stage procedures. Presumably the effects would be larger—I do not think much larger—if four or five stages were possible.

It is conceivable that seeding operations could be stopped at the mth stage, using a criterion depending on the residuals from regression $y_i - a - bx_i$ for $i \leqslant m$. For example, the simplest two-stage rule would be to stop the operation with $m = 1$ if $y_1 - a - bx_1 \geqslant 0$, and otherwise stop with $m = 2$. The expected value of the quantity

$$\sum_{i=1}^{m} (y_i - a - bx_i)/m$$

is then 0.2σ. The average upward bias of the fractional increase,

$$\frac{\sum_{1}^{m} (y_i - a - bx_i)}{\sum_{1}^{m} (a + bx_i)} = \frac{\overline{y} - a - b\overline{x}}{a + b\overline{x}},$$

may be taken roughly as $0.2\sigma/(a + b\mu_1)$, if σ_1 is not too large. For a three-stage rule we could reach $0.28\sigma/(a + b\mu_1)$.

(Note: We use $\bar{x}$, etc., to mean $\sum_1^m x_i/m$, etc.)

Although this sort of stopping rule could make an experimenter's results look better, it seems to me unlikely that it would be of practical use to a commercial operator. However, some attention will be paid to it, since it is both simpler to treat theoretically than the sort of rule discussed next, and also furnishes an upper bound for the effect of the second sort of rule.

The second sort of rule is one by which stopping is determined on the basis of observations on only the *target* rainfall y_i. This seems not unlikely, and would be done by decision of the customer, not the operator.

Because of their simplicity, we discuss first the effect of rules depending on the residuals from regression.

4. STOPPING RULES DEPENDING ON RESIDUALS FROM REGRESSION

a. *Effect on the significance level*

Here we will consider only two-stage rules. We will suppose that m is taken to be 1 (one stage) if $(y_1 - a - bx_1) > c\sigma$, where c is a parameter that will have various values, and σ is the standard deviation of residuals from the regression line. Otherwise $m = 2$.

In performing a significance test the usual procedure would be to calculate the quantity

$$z = \sum_{i=1}^m (y_i - a - bx_i)/\sigma m^{\frac{1}{2}}. \tag{5}$$

If m were a fixed number, z would have a standard normal distribution. Since m is determined by our stopping rule, z has a different distribution, which can be calculated numerically.

In Table 3.1 below some points are given on the probability distribution of z, for several values of c. The probabilities were not calculated exactly; instead a lower bound p and an upper bound P were calculated. In the table, the probability that z exceeds t is thus between p and P. For comparison, the column headed Q gives the probability that a standard normal deviate exceeds t. Thus, under the heading $c = 0.84$, with $t = 0.84$, we see that the normal significance level is 0.20 while the true significance level is between 0.26 and 0.29.

Far larger values of c, the differences between the upper and lower bounds p and P in the table are considerable. It is probable that the true value in these cases is closer to the lower-bound p.

TABLE 3.1

t	Q	p	P
	c = 0.00		
0.00	0.50	0.625	0.625
0.50	0.309	0.36	0.36
0.84	0.20	0.22	0.22
1.28	0.10	0.11	0.11
1.65	0.05	0.05	0.05
2.05	0.02	0.02	0.02
	c = 0.50		
0.50	0.309	0.40	0.42
0.84	0.20	0.24	0.26
1.28	0.10	0.11	0.12
1.65	—	—	—
2.05	0.02	0.02	0.02
	c = 0.84		
0.84	0.20	0.26	0.29
1.28	0.10	0.12	0.14
1.65	0.05	0.05	0.06
2.05	0.02	0.02	0.02
	c = 1.28		
1.28	0.10	0.12	0.17
1.65	0.05	0.05	0.08
2.05	0.02	0.02	0.03
	c = 1.65		
1.65	0.05	0.06	0.11
2.05	0.02	0.02	0.04

On the whole, the increases in significance level do not look important, even in this case where selection is done on the residuals. Use of three or more stages could, of course, increase the effect.

b. *Effect on the percentage increase*

Consider the following three-stage rule. If $y_1 - a - bx_1 \geqq 0$, take $m = 1$. If $y_1 - a - bx_1 < 0$ but $\sum_{i=1}^{2} (y_i - a - bx_i) \geqq 0$, take $m = 2$. Otherwise take $m = 3$. As noted in Section 1, no three-stage stopping rule gives an appreciably higher expectation for the average

$$\sum_{i=1}^{m} (y_i - a - bx_i)/m.$$

It seems plausible that one cannot do much better than this rule by way of improving the expected fractional increase.

The reported fractional increase is

$$I = \frac{\sum_{i=1}^{m} (y_i - a - bx_i)}{\sum_{i=1}^{m} (a + bx_i)} = \frac{\bar{y} - a - b\bar{x}}{a + b\bar{x}}. \tag{6}$$

Strictly speaking, $E(I)$ is infinite if the x's are normal. In practice, presumably the denominator will not vanish. If the standard deviation of x is not too large we can make a rough approximation by putting

$$E(I) \sim \frac{E(\bar{y} - a - b\bar{x})}{a + bE(\bar{x})} = \frac{E(\bar{y} - a - b\bar{x})}{a + b\mu_1} = \frac{E(\bar{y} - a - b\bar{x})}{\mu_2}, \tag{7}$$

noting that $E(\bar{x}) = \mu_1 = E(x)$.

For the above three-stage process the result of Section 1 then implies that

$$E(I) \sim \frac{(0.28)\sigma}{\mu_2}. \tag{7a}$$

A numerical evaluation for one case will be made below.

5. STOPPING RULES DEPENDING ON RAINFALL IN THE TARGET AREA

a. *Effect on the significance level*

Using the expressions in Eq. (2) for a and b, and replacing x by its value in Eq. (3), we can write

$$y - a - bx = (1 - \rho^2)(y - \mu_2) - \rho(\sigma_2/\sigma_1)\epsilon', \tag{8}$$

where ϵ' is independent of y. Then the quantity used in a significance test is

$$\frac{\sum_{i=1}^{m}(y_i - a - bx_i)}{\sigma m^{\frac{1}{2}}} = \frac{(1 - \rho^2)\Sigma(y_i - \mu_2)}{\sigma m^{\frac{1}{2}}} - \frac{\rho\sigma_2}{\sigma_1\sigma}\frac{\Sigma\epsilon_i'}{m^{\frac{1}{2}}}. \tag{9}$$

Noting that $\sigma = \sigma_2(1 - \rho^2)^{\frac{1}{2}}$ and $\sigma_1\sigma/\sigma_2 = \sigma'$, the right side of Eq. (9) is

$$(1 - \rho^2)^{\frac{1}{2}}\frac{\Sigma(y_i - \mu_2)}{\sigma_2 m^{\frac{1}{2}}} - \rho\frac{\Sigma\epsilon_i'}{\sigma' m^{\frac{1}{2}}} = (1 - \rho^2)^{\frac{1}{2}}Z - \rho w. \tag{10}$$

In Eq. (10), the quantity Z is the test statistic we would obtain if we were testing the significance of a standard normal random variable; for example, for a two-stage rule, Z is given by Eq. (5). The quantity w is standard normal and is independent of Z. In the cases of interest to us,

we always have $\rho \geq 0.7$. Thus, whatever is the effect on the significance level of "residual" stopping, it is considerably diluted for "target" stopping, since the test statistic is Eq. (10) for target stopping. Since the effect for two-stage rules was not large for residual stopping, it is probably quite small for target-stopping. I would not expect it to be large even for three-stage target stopping, but have tried no calculations.

b. *Effect on the percentage increase*

Using Eqs. (2) and (3) again, we have

$$\frac{\sum\limits_{i=1}^{m}(y_i - a - bx_i)}{\sum\limits_{1}^{m}(a + bx_i)} = \frac{(1-\rho^2)\,(\bar{y} - \mu_2) - \rho(\sigma_2/\sigma_1)\bar{\epsilon}'}{\rho^2(\bar{y} - \mu) + \mu_2 + \rho(\sigma_2/\sigma_1)\bar{\epsilon}'} = I, \qquad (11)$$

Using the same rough approximation as in Section 3b above, we obtain

$$E(I) \sim \frac{(1-\rho^2)\,E(\bar{y} - \mu_2)}{\rho^2 E(\bar{y} - \mu_2) + \mu_2}. \qquad (12)$$

Consider a three-stage target-stopping procedure where the rule is

$$y_1 - \mu_2 \geq 0 \to m = 1,$$
$$y_1 - \mu_2 < 0, \ \sum_{i=1}^{2}(y_i - \mu_2) \geq 0 \to m = 2,$$
$$m = 3 \text{ otherwise.}$$

For this procedure, $E(\bar{y} - \mu_2) = 0.28\sigma_2$, and Eq. (12) becomes

$$E(I) \sim \frac{(1-\rho^2)\,(0.28)\,(\sigma_2)}{\rho^2(0.28)\,(\sigma_2) + \mu_2} = \frac{(1-\rho^2)^{\frac{1}{2}}(0.28)\,(\sigma)}{\rho^2(0.28)\,\sigma_2 + \mu_2}. \qquad (13)$$

6. NUMERICAL RESULTS

Consider the data for one of the two operators' reports done without the cube-root transformation of precipitation data. Here, letting x and y be natural inches of precipitation,

$$y = 1.38 + 0.701x + \epsilon,$$
$$x = 1.0759 + 0.7167y + \epsilon',$$
$$\mu_1 = 4.15,$$
$$\mu_2 = 4.2892,$$
$$\sigma_1 = 1.72,$$
$$\sigma_2 = 1.7011,$$
$$\rho = 0.7088,$$
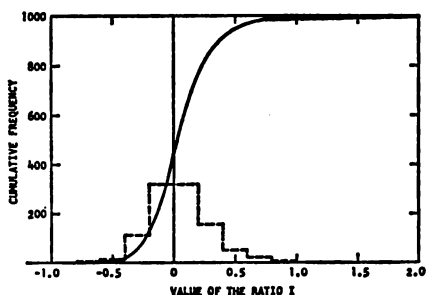$$\sigma = 1.20,$$
$$\sigma' = 1.2133.$$

FIGURE 3.2.

(For the sake of internal consistency, the values are slightly altered. Also, for present purposes the true regression parameters are assumed to be the estimated ones.)

Using residual stopping, Eq. (7a) yields (three stages)

$$E(I) \sim \frac{(0.28)\,(1.20)}{4.2892} = \frac{0.336}{4.2892} = 0.078 \sim 8\%. \tag{15}$$

Using target stopping Eq. (13) yields (three stages)

$$E(I) \sim \frac{(1-\rho^2)\,(0.28)\,(\sigma_2)}{\rho^2\,(0.28)\,(\sigma_2) + \mu_2} = 0.05234 \sim 5\%. \tag{16}$$

As a check, the distribution of I in Eq. (11) was estimated by Monte Carlo sampling of 1,000 values, using the above parameter values. Figure 3.2 shows the cumulative distribution (plotted at intervals of 0.2) and the frequency count for the class interval $[0.2r, 0.2(r+1)]$, $r = 0, \pm 1, \ldots$. The mean for individual values was not calculated, but the mean for the grouped observations is 0.051, which agrees with Eq. (16). The median is smaller and seems to be about 0.03. [Incidentally none of the 1,000 denominators for Eq. (11) turned out negative.]

(I am indebted to June Buell for programming the calculations.)

As another example, I considered a linear regression for natural inches that seemed more or less appropriate to the parameters of the reports that employed the cube-root transformation. In this case an appropriate P seemed about 0.9, and the biases correspondingly less than for the above example. I am not sure how appropriate my linearization is, but at least it does not suggest that the resulting values in Eqs. (15) and (16) should be any larger.

7. OPTIONAL STARTING

Cloud-seeding operations may start after there has been a dry period, and thus the historical data from which the regression coefficients are

estimated could be biased. This may be considered apart from the effect of possible weather cycles or weather types. To illustrate the point without the complications of regression, suppose we are comparing the rainfall during a seeded period with the mean of past periods. If the last observation of the historical period is based downward, the estimate of the historical mean will be accordingly low, and the seeded period will look comparatively better. With regression, the effect is more subtle. It appears to me, however, that the effect could be to make the historical regression line lower in the region of small x values (control-area values), and consequently to make it easier for data on seeding results to appear above the line in years when the control rainfall is below average. Assuming a reasonably long historical period, I should think the effect would be less than the possible effects of optional stopping discussed above, but I have not make any quantitative estimates. One would also have to consider whether the dry period that led to the seeding extended back at least a year (in which case it would influence the historical data) or less (in which case presumably it would not).

Appendix 4

STATISTICAL NOTES ON CLOUD-SEEDING PROJECTS

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1. The variance of $d = y - \hat{y}$ is

$$\sigma^2 \left\{ 1 + n^{-1} + \left[(x - \bar{x})^2 \Big/ \sum_{i=1}^{n} (x_i - \bar{x})^2 \right] \right\},$$

where σ^2 is the variance around the regression line. Letting

$$\sigma_\sigma^2 = \sum_{i=1}^{n} (x_i - \bar{x})^2 / n,$$

this variance is

$$\sigma^2 [n + 1 + (x - \bar{x})^2 / \sigma_\sigma^2] / n.$$

Note the n rather than $n-1$ in the denominator. Though theoretically n is correct, the error introduced in using $n-1$ in the expression for E may be practically of no importance.

2. The method of combining m months of data, via the "Stouffer statistic," is questionable. This procedure treats the t_i's as approximately standard, normal and independent. Two factors make the independence assumption difficult. First, the same random variable, S_y, is in the denominator of all the t_i's. Second, though y_i and y_j are independent, $\hat{y}_i$ and $\hat{y}_j$ are not, as they are both based on the estimates of α and β. Since

$$\mathrm{Cov}(d_i, d_j) = -(1-n) - [(x_i - \bar{x})(x_j - \bar{x}) / n\sigma_\sigma^2],$$

for large n this covariance is negligible, and also the variance of S_y is small for large n; thus we see that the reasonable use of the Stouffer statistic depends on large n.

TABLE 4.1

| | | | | Significance Level | |
Project	n	m	F	Recomputed	Panel
1	39	1	0.626	0.21	0.20
2	13	1	2.140	0.09	0.07
6	15	5	1.830	0.18	0.30
9	31	1	1.185	0.14	0.17
10	38	1	2.350	0.07	0.12
14	56	3	1.312	0.27	0.50
12	44	1	0.288	0.30	0.34
13	38	1	0.884	0.18	0.21

An exact procedure for combining the data for several months has been given by Lieberman.* A summary of the procedure follows. Let

$$\sigma_{ij} = (x_i - \bar{x})(x_j - \bar{x}),$$

where $\bar{x}$ is the mean of the historical independent variables and x_i is a new independent variable, and let

$$v_{ij} = \delta_{ij} + n^{-1} + (\sigma_{ij}/n\sigma_e^2),$$

where $\delta_{ij} = 1$ if $i = j$ and $\delta_{ij} = 0$ if $i \neq j$, for $i,j = 1, \ldots m$.
Let V be the matrix of v_{ij} and Δ be the vector

$$\Delta = (y_i - \hat{y}_i, \ldots, y_m - \hat{y}_m) = (d_i, \ldots, d_m),$$

where y_i is the observed and $\hat{y}_i$ is the predicted dependent variable. Thus we reject the hypothesis that all the data are from the same distribution as the historical data if

$$\frac{\Delta V^{-1} \Delta'}{m S_y^2} \geq F_a(m, n-2),$$

where the prime denotes transpose and $F_a(m, n-2)$ is the upper 100α percentage point of the F distribution with m and $n-2$ degrees of freedom.

Note that for $m=1$ this test is equivalent to the corrected version of the t test, as $F_a(1, n-2) = t_a^2(n-2)$.

3. Table 4.1 is a summary of a recalculation of the significance tests for projects numbered (in Table 1 of the basic report) 1, 2, 6, 9, 10, 14, 12, and 13, using the above F statistic based on the Panel's values of the regression parameters and, except for projects 12, 13, and 14, the Panel's data. (The reason I use the operators' data for Projects 12 and 13 is that I was curious in this case to see how those data would compare with the

* Lieberman, G. J. (1961), Prediction regions for several predictions from a single regression line, *Technometrics*, *3*, 21.

Panel's regressions.) Also listed are the significance levels given in Table 1 of the main report, for comparison, with the exception of project 14, which is the operator's computed significance level.

The differences between the significance levels for the t-test cases (except for projects 12 and 13) are undoubtedly due to the use of S_y as an approximation to the denominator of the t statistic in the Panel's calculation.

4. The analysis above depends critically on m, and consequently is useless if m is not specified and may be arbitrarily large. In that case, we are interested in a region that will cover some $100P$ percent of the distribution of y's, centered at $\alpha + \beta x$ for any x, and at 100α percent confidence. This is called a tolerance region, and it is of the form

$$\hat{y}^* \pm C_{P,a} S_y \{ n^{-1} + [(x^* - \bar{x})^2 / n\sigma_s^2] \}.$$

The probability content of this interval is at least P, with confidence α for all x^*.

The procedure for finding $C_{P,a}$ is the following:

Let

$$F(x^*, h) = \Phi\left\{ \frac{1}{n^{\frac{1}{2}}} + \frac{x^* - \bar{x}}{n^{\frac{1}{2}}\sigma_s} + h\left[\frac{1}{n} + \frac{(x^* - \bar{x})^2}{n\sigma^2} \right] \right\}$$
$$- \Phi\left\{ \frac{1}{n^{\frac{1}{2}}} + \frac{x^* - \bar{x}}{n^{\frac{1}{2}}\sigma_s} - h\left[\frac{1}{n} + \frac{(x^* - \bar{x})^2}{n\sigma_s^2} \right]^{\frac{1}{2}} \right\}.$$

Let $G(h) - \min_{x^*} F(x^*, h)$.

Find h_0 such that $G(h_0) = P$.

Then

$$C_{P,a} = (n-2) h_0 / x_a^2 (n-2).$$

5. Various modifications to cover the case in which one wishes to vary P with x are covered in Lieberman and Miller.[†]

6. The test given in paragraph 2 does not make use of an assumed linear structure for the y_i^*. Suppose

$$y_i = N(\alpha + \beta x_i, \sigma^2), \qquad i = 1, \ldots n,$$
$$y_i^* = N(\gamma + \delta x_i^*, \sigma^2), \qquad i = 1, \ldots m,$$

where the error variance is the same for all observations. We wish to test the hypothesis that $\alpha = \gamma$ and $\beta = \delta$. To do this, let $\hat{\alpha}$, $\hat{\beta}$ be the estimates of the common α and β from the combined sample, and $\hat{\alpha}$, $\hat{\beta}$, $\hat{\gamma}$, $\hat{\delta}$ be the standard estimates from each sample separately. Then

$$\frac{m+n-4}{2} \cdot \frac{n[y - \hat{\alpha} - \hat{\beta}\bar{x}]^2 + m[y^* - \hat{\alpha} - \hat{\beta}\bar{x}^*]^2 + (\hat{\beta} - \hat{\beta})^2 n\sigma_s^2 + (\hat{\beta} - \hat{\beta})^2 m\sigma_s^{*2}}{\Sigma[y_i - \bar{y} - \hat{\beta}(x_i - \bar{x})]^2 + \Sigma[y_i^* - \bar{y}^* - \hat{\beta}^*(x_i^* - \bar{x}^*)]^2}$$

† Lieberman, G. J., and R. G. Miller (1962), Simultaneous tolerance intervals in regression, *Stanford Tech. Rept. No. 59.*

is distributed as $F(2, n+m-4)$ and is the test statistic for this hypothesis when $m \geqq 2$. When $m = 1$, this likelihood ratio test statistic reduces to

$$\frac{n[\bar{y}-\hat{a}-\hat{\beta}\bar{x}]^2 + (y^*-\hat{a}-\hat{\beta}x^*)^2 + (\hat{\beta}-\hat{\beta})^2 n\sigma_s^2}{\Sigma[y_i-\bar{y}-\hat{\beta}(x_i-\bar{x})]^2/(n-2)},$$

which is distributed as $F(1, n-2)$. This is different from the usual $F(1, n-2)$ statistic

$$\frac{(y^*-\hat{a}-\hat{\beta}x^*)^2(n-2)}{\Sigma[y_i-\bar{y}-\hat{\beta}(x_i-\bar{x})]^2\{1+n^{-1}+[(x^*-\bar{x})^2/n\sigma_s^2]\}}.$$

This is because the former test is constructed merely by using $(y^*-\hat{a}-\hat{\beta}x^*)/S_y$ as a pivotal quantity, regardless of hypothesis.

7. The classical way of combining significance levels from t independent tests of significance is to compare -2 times the sum of the natural logarithms of the significance levels with $\chi^2(2t)$, the upper 100α percentage point of the χ^2 distribution with $2t$ degrees of freedom. Doing this for the 8 Panel significance levels listed in paragraph 3 above, we find that this statistic equals 25.395, as compared with $\chi^2(16) = 26.296$, and so is not significant at the 5 percent level. Using my levels will make the test significant with $\chi^2 = 29.073$.

The test statistics on which these levels of significance are based have a special structure that enables us to use a better method of pooling than the aforementioned nonparametric method. Suppose that the variances around the regression line are the same for all projects. (This appears to be true for projects 6, 9, 10, 14, 12, and 13, as S_y ranges from 0.076 to 0.109.) Now let $F_i = (S_{1i}/S_{2i})(f_{2i}/f_{1i})$ denote the ith project F statistic, with the S's denoting sums of squares and f's denoting degrees of freedom. Then $(\Sigma S_{1i}/\Sigma S_{2i})(\Sigma f_{2i}/\Sigma f_{1i})$ is distributed as F with Σf_{1i} and Σf_{2i} degrees of freedom. The value of this statistic for projects 6, 9, 10, 14, 12, and 13 is 1.244, corresponding to a level of significance of 0.26. The classical χ^2 pooling of my significance levels yields $\chi^2 = 21.136$, corresponding to a level of significance of about 0.05 for these 6 projects.

[If we were dealing only with t statistics and not with F statistics and under the same circumstances as above—equal variances around the regression lines—then, writing $t_i = d_i/(S_i/f_i)^{\frac{1}{2}}$, a combined statistic would be $\Sigma d_i/(\Sigma f_i S_i/\Sigma f_i)^{\frac{1}{2}}$, which is distributed as t with Σf_i degrees of freedom.]

8. Looking at the individual significance levels indicates that the effects of the seeding, though not significant in any one case, were all positive. When one looks at the monthly data, however, this is not quite the case. We list in Table 4.2 the predicted and actual target rainfall (actually cube root of rainfall, except for projects 1 and 2) for each month of seeding, using the Panel data and regression parameters. In the projects used in

TABLE 4.2

Project	Predicted	Actual	Sign of difference
1	5.663	6.640	+
2	2.741	3.640	+
6	1.237	1.048	−
	1.447	1.560	+
	1.462	1.471	+
	1.736	1.768	+
	1.558	1.635	+
9	1.274	1.396	+
10[a]	1.078	1.241	+
14	1.047	1.140	+
	1.393	1.245	−
	1.620	1.674	+
12[a]	1.605	1.656	+
13[a]	1.520	1.601	+
A[a]	1.176	1.160	−[b]
B	1.128	1.080	−
	1.249	1.680	+
	1.404	1.108	−
	1.182	1.127	−
C	0.964	1.442	+
	1.652	1.512	−
	1.015	1.496	+

[a] Operator's control data used.
[b] This differs from the operator's report of a positive difference.

the Panel evaluation, there were only 2 out of 14 negative differences. However, in the other three projects (A, B, and C) there were 5 out of 8 negative differences.

A particularly interesting point in juxtaposing the signs of differences between predicted and actual with the significance levels is seen from project B. Here 3 of the 4 differences were negative, yet the significance level for the over-all project was less than 0.5, i.e., indicated that the net effect was in the positive direction. The moot question is: What is the appropriate hypothesis to be tested—that prolonged seeding produces a net positive effect, or that seeding in any month produces a positive effect in that month? If the former, the problem of optional stopping arises.

9. The procedures discussed in paragraph 4 above require that the tests being combined be independent. There is evidence, though, that some of the tests are not independent, in that the residuals between tests are correlated. In particular, this is true for the data of projects 9 and 10 for which Harris obtained a correlation of 0.54, significant at the 0.05

level.* To combine the correlated tests to obtain an over-all level of significance, we must go back farther than the F statistics, to the residuals themselves.

The covariance matrix of a vector of residuals for a single project has as a typical element,

$$\sigma^2\left[\delta_{ij}-\frac{1}{n}-\frac{(x_i-x)(x_j-x)}{n\sigma_s^2}\right],$$

where σ^2, n, and σ_s^2 (as well as the x_i) are peculiar to the project. We assume a simple form of the cross-covariance matrix for the pair of residuals from two projects, namely that it be zero unless the projects were done in the same month, in which case it is equal to the covariance of the historical rainfall in that month in the two project areas. Thus, for example, as projects 9 and 10 were done in August 1964, we can estimate the covariance of the residuals for these two projects from the common 1948–1963 (excluding 1952) historical target data. Thus, also, the October residual for project 14 is uncorrelated with all other project residuals, but the November and December residuals are correlated with those for projects 12 and 13. (As projects 12 and 13 began in mid-November and ended in mid-December, we use the average covariance as the covariance of the residuals for these projects with the residual for project 14.)

Using this rule, we find the following covariance matrix of the residuals of projects 9, 10, 14, 12, and 13.

	Aug 9	Aug 10	Oct 14	Nov 14	Dec 14	Nov/ Dec 12	Nov/ Dec 13
	0.01247	0.0045	0	0	0	0	0
	0.0045	0.01121	0	0	0	0	0
	0	0	0.00906	0.00029	0.00009	0	0
$S=$	0	0	0.00029	0.00865	0.00013	−0.00243	0.00049
	0	0	0.00009	0.00013	0.00862	−0.00243	0.00049
	0	0	0	−0.00243	−0.00243	0.28819	0.0052
	0	0	0	0.00049	0.00049	0.0052	0.88428

If Δ is the vector of residuals, then, ignoring that S is a random matrix, $\Delta S^{-1}\Delta'$ will have a χ^2 distribution with, in this case, 7 degrees of freedom. This χ-squared variable can be added to independent χ-squared variables (e.g., -2 times the sum of the natural logarithms of the significance levels

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* There is an interesting technical problem here, in that the residuals are not independent, so the distribution of the correlation coefficient is not that of Student's t. However, based on a Monte Carlo it was determined that the t distribution is an adequate approximation in this case. This is due to the large value of n, which makes the residuals virtually independent.

of projects 1, 2, and 6) to obtain a combined χ-squared statistic with degrees of freedom equal to the sum of the component degrees of freedom.

For this problem

$$\Delta = (-0.1215, -0.1623, -0.0925, 0.1481, -0.0536, -0.0510, -0.0800),$$

and $\Delta S^{-1}\Delta' = 6.592$ with associated level of significance 0.47. Combining this with -2 times the sum of the natural logarithms of the significance levels of projects 1, 2, and 6 yields a value of $\chi^2(13)$ of 17.959 with associated level of significance 0.16.

10. As part of a general consideration of principles for stratification of the historical data (e.g., by wind direction, discussed by Rapp and Schutz, Appendix 2), I wondered whether control rainfall was a significant stratifier. Project 14 had the greatest amount of historical data listed, and so I stratified the data into three groups—low $(0.29 \leqq x \leqq 2.50)$, middle $(3.13 \leqq x \leqq 5.23)$, and high $(5.74 \leqq x \leqq 10.54)$, and computed the regressions for these groups separately. Following are the values of b:

Low	1.376	$n=13$;
Middle	0.732	$n=26$;
High	1.148	$n=17$.

These b's are significantly different, but I know of no physical explanation of this phenomenon.

11. Following is a derivation of the t test for fractional month data. Let

$f =$ fraction of month in which data is observed,
$x' =$ cube root of control precipitation observed,
$y' =$ cube root of target precipitation observed,
$x^* = x'/f, y^* = y'/f,$
$y^* = \hat{a} + \hat{\beta}x^*,$
$y' = f\hat{a} + \hat{\beta}x' = f\hat{y}^*.$

Then

$$f^2\mathrm{Var}\,(y^* - \hat{y}^*) = \mathrm{Var}\,y' + \frac{\sigma^2}{n} + \frac{(x'-\bar{x})^2\sigma^2}{n\sigma_x^2}$$

and

$$t = \frac{f(y^* - \hat{y}^*)}{S_y\{A + n^{-1} + [(x'-\bar{x})^2/n_x^2]\}^{\frac{1}{2}}}$$
$$= \frac{y' - \hat{y}'}{S_y\{A + n^{-1} + [(x'-\bar{x})^2/n\sigma_x^2]\}^{\frac{1}{2}}},$$

where $\mathrm{Var}\,y' = A\sigma^2$ and S_y is the estimate of σ. If it is taken that $A=1$, it is believed that this overestimates the variance of y'.

12. I also investigated the effect of the cube-root transformation on the residuals. Using the data of project 14, I computed the residuals based

on a regression of the raw data and the transformed data, and computed measures of skewness and kurtosis for both these data. They are:

	Raw	Transformed
skewness	−0.46	−0.56
kurtosis	1.98	0.83

Thus the transformation makes the residuals more normally distributed.

The transformation also reduced the variance of the residuals from 0.56 to 0.008. However, I have not checked whether the transformation stabilizes the variance, i.e., makes the variance of the residuals independent of x. However, if it were to do so, this would imply also that a non-least-squares technique would be better. For if $y = Y^{1/3}$, with Var $y - \sigma^2$ independent of X, then, since

$$y \simeq (EY)^{1/3} + (1/3)(EY)^{-2/3}(Y - EY),$$
$$\sigma^2 = \text{Var } y = (1/9)(Ey)^{-4/3} \text{Var } Y,$$

or

$$\text{Var } Y = 9\sigma^2(EY)^{4/3}.$$

Thus if $EY - \beta^3 X$ (so that $Ey \simeq \beta X^{1/3} = \beta x$), then one ought to minimize

$$\Sigma (Y_i - \beta^3 X_i)^2 / X_i^{4/3}$$

for an optimal estimate of β.

STATISTICAL ANALYSIS OF CERTAIN
WEATHER-MODIFICATION OPERATIONS

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During September we examined the cloud-seeding evaluations made by
the National Academy of Sciences Panel on Weather and Climate Modi-
fication and selected a total of 16 projects for an evaluation of possible
seeding effects extending beyond the target areas specified in these reports.
As shown in Figure 5.1, this involved 11 project areas for which a total

FIGURE 5.1. Location of proj-
ects selected for evaluation.

TABLE 5.1. *Summary of Projects, Number of Months and Stations Used, for the Analysis of Precipitation in the Original and Extended Target Areas*

Project	Title	Number of Months		No. of Stations	Station Months	
		Seeded	"Random"		Seeded	"Random"
A	New Hampshire	2	2	36	72	72
B	Massachusetts	1	1	12	12	12
C	Southeastern New York	1	1	39	39	39
D	Connecticut	1	1	82	82	82
E	New Jersey	3	3	84	252	252
F	Eastern New York	1	1	121	121	121
G	Eastern Pennsylvania	2	2	104	208	208
H	Southern Pennsylvania	10	10	102	1,020	1,020
I	South Carolina	5	5	54	270	270
J	Mid-Potomac	24	3	62	1,488	186
K	North Carolina	12	12	88	1,056	1,056
	Total	62	41	784	4,620	3,318

of 62 seeded months were used for evaluation. In cases where seeding was conducted for less than a full month, the same procedure was used in adjusting the precipitation totals to an entire month that was described in the evaluation reports of the Panel. In addition to the 62 months of seeding evaluated, a set of "random" or fictitious months was chosen for each project as a statistical control. Table 5.1 lists the project areas along with the number of months and stations used in this analysis.

Regression equations, based on the historical records of months of no seeding, were derived for these 784 stations. Wherever possible, the control areas used were the same as those used in the analyses by the Panel. For each station in each project, a regression equation was developed as a basis for estimating the precipitation that would have been expected to fall on the basis of the past relationship between the target stations and the appropriate control area. The cube-root transformation was made on all the monthly precipitation totals. All results given here are in those terms. For each station and for each month of seeding, an anomaly d was determined by $d = Y - \hat{Y}$, where Y was the observed rainfall and $\hat{Y}$ was the estimated or predicted rainfall from the regression equation. Also determined was the statistic $t = d/E$, where E is the error-estimate of d as given by classical regression theory. Each "random" month was analyzed in the same manner.

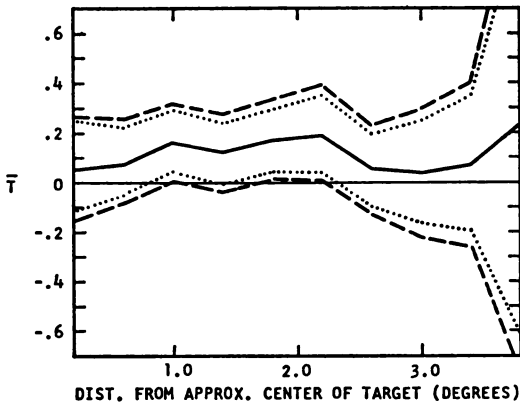The values of t and d were summarized according to station, month,

FIGURE 5.2. Average variation of t for 62 seeded months as a function of distance from center of target.

DIST. FROM APPROX. CENTER OF TARGET (DEGREES)

project, and distance and direction from the center of the operational target. Generally speaking, stations analyzed were located to the east of the control areas, but our evaluation included stations ranging from north-northwest through east all the way around to southeast of the original target. Some projects had stations as far as 200 miles away from the control area, while others had none beyond 50–60 miles, due to lack of data, proximity to coastlines, other projects, etc. Figure 5.2 shows the summary of results in terms of averages of all the t's for all projects according to distance from the center of the target area. Distance units are in terms of degrees, since, for computer programming purposes, the station locations were expressed in terms of latitude and longitude relative to the approximate target center. Stations were grouped in class intervals of 0.4 degrees for this analysis. The figure also shows the 90 and 95 percent confidence levels, based on the variance of the individual project monthly means. All 62 project months have stations in the first class (within 0.4° from the target center) so the mean $\overline{T}$ for the first group is based on 62 separate values. As the distance increases, fewer projects are represented and the standard error of $\overline{T}$ increases.

Figure 5.3 shows the summary for the rainfall anomalies, $\overline{D}$. Figures 5.4 and 5.5 show results for the "random" months. Figures 5.2 and 5.3 indicate that positive anomalies in rainfall are detectable up to 100 miles or more beyond the intended target. These results do not exclude the possibility that significant negative anomalies exist farther downwind or that such effects may exist in localized areas for some operations. However, such effects would have to be much more pronounced and consistent to be detected with the type of data and procedures used here. The suggestion of negative anomalies near the target area in the "random" years is puzzling, and may indicate some bias due to the persistence of drought conditions for several years in very localized areas that may lead

FIGURE 5.3. Average departure of rainfall from "expected" value (in terms of the cube-root transformation) for 62 seeded months as a function of distance from center of target.
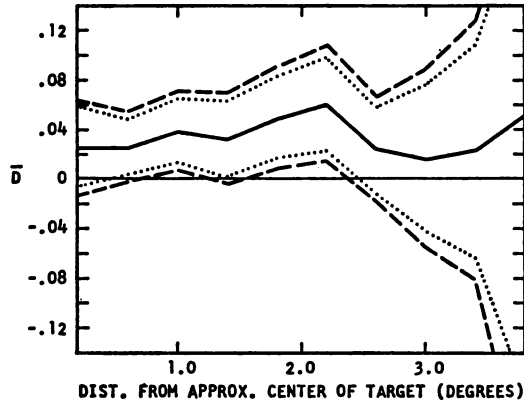

FIGURE 5.4. Average variation of t for 41 "random" months as a function of distance from center of target.
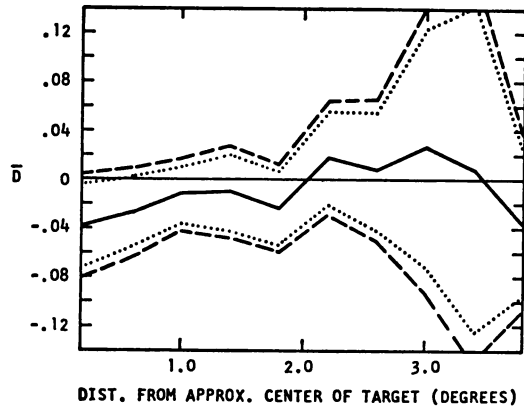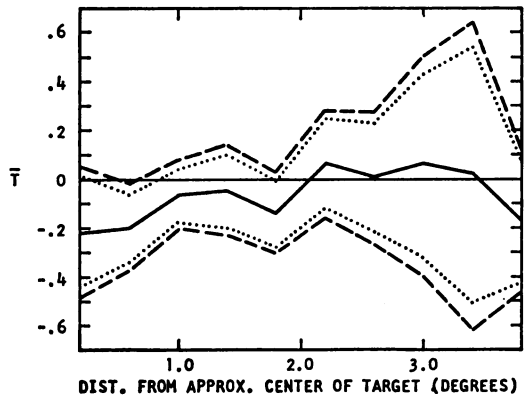

FIGURE 5.5. Average departure of rainfall from "expected" value (in terms of the cube-root transformation) for 41 "random" months as a function of distance from center of target.

to the inauguration of cloud-seeding programs. The "random" years chosen were from recent years, near to the years of seeding, and any such bias produced by such persistence would tend to operate against the finding of significant seeding effects.

We do not consider these results equivalent to those obtainable from carefully designed experiments. Nor would we suggest that these results are meaningless within the context of the intended objective in view of the large amount of data processed and the lack of evidence of any significant inconsistencies in the indicated trends.

The nature of the investigation and time available were such that hand retrieval of data was required. No doubt, some copying or tabulation errors occurred. Checks and safeguards were used, and we are reasonably confident that any such errors are not of major proportions. We are not prepared to comment in depth on whether our extended target regions were consistently "downwind." This would require an investigation of far greater magnitude and detail. It is worth noting, however, that this downwind uncertainty, as well as questions of control-area contamination, would tend to produce a negative bias in our apparent results.

ADDITIONAL NOTE ON "STATISTICAL ANALYSIS OF CERTAIN WEATHER-MODIFICATION OPERATIONS"

In Figures 5.3 and 5.5 of the above analysis, the average departure of precipitation from "expected" value (in terms of the cube-root transformation) are shown as a function of distance from the center of the

TABLE 5.2. *Mean Precipitation Anomaly ($\overline{D}$) for All Stations in Target and Extended Target Areas* [a]

	Seeded	Unseeded (random or control)	Comparison Test
Number of Project Months	62	41	(101 DF)
$\overline{D}$	0.0352	—0.0120	Difference = 0.0470
S	0.1082	0.0928	0.1024
$S_{\overline{D}}$	0.0138	0.0145	
$t = \overline{D}/S_{\overline{D}}$	2.56	—0.83	2.28
Probability (two tails)	$\rho = 0.01$	$\rho \cong 0.40$	$\rho \cong 0.03$
Probability (one tail, for increase)	$\rho = 0.005$	$\rho \cong 0.80$	$\rho \cong 0.015$

[a] Scale is cube root of precipitation.

TABLE 5.3. *Mean Precipitation Anomaly by Project Area, Cube-Root Scale, for All Stations in Target and Beyond*

Project	Title	Seeded	Unseeded (random or control)	Seeded Minus Unseeded
A	New Hampshire	0.11	0.09	0.02
B	Massachusetts	0.04	0.01	0.03
C	Southeastern New York	0.09	0.06	0.03
D	Connecticut	—0.01	0.07	—0.08
E	New Jersey	0.04	0.01	0.03
F	Eastern New York	0.14	0.01	0.13
G	Eastern Pennsylvania	0.18	—0.13	0.31
H	Southern Pennsylvania	—0.03	—0.04	0.01
I	South Carolina	0.06	—0.01	0.07
J	Mid-Potomac	0.03	—0.03	0.06
K	North Carolina	0.02	—0.01	0.03
Means		0.06	0.00	0.06

(10 DF) $t = 1.99$
two tail $p \cong 0.074$
one tail $p \cong 0.037$

operational target area. These results are for the 62 project months of seeding analyzed, as well as for a set of 41 "random" months, or months without seeding, used as statistical controls.

Subsequently, a summary has been made by combining all the target and extended target stations, irrespective of distance downwind from the control-area (or generator) sites.

Table 5.2 shows the average anomaly D for the 62 months with seeding and 41 months without seeding. It is noted that the average anomaly for the cases of seeding is positive (0.0352) and differs significantly from zero. The months without seeding have a slightly negative but not significant departure from zero. The variances are practically identical (0.1082^2 versus 0.0928^2). The standard t test for the differences between two means shows statistical significance.

Table 5.3 shows a comparison between the months with and without seeding by individual project. In only one project of the eleven does the average anomaly for the months without seeding exceed the anomaly for the months with seeding. The binomial-sign test gives a probability of 0.012 for this. The months without seeding, which are independent from the historical records used in determining the regression parameters, have a mean anomaly essentially equal to zero, the expected value. The months with seeding have a mean anomaly of 0.06 and a t test on the paired com-

parisons, with only 10 degrees of freedom available, show statistical significance on the one-tailed test.

The results of the analysis to date thus point out three facts with respect to the average precipitation anomalies "downwind" from the control area.

(1) The months with seeding are different from the historical months.

(2) The months with seeding are different from the months without seeding (chosen independently of the historical record).

(3) The independent months without seeding show slight but not generally statistically significant anomalies in the negative direction, suggesting that *if* any bias exists in the statistical analysis, it tends to operate in a direction against finding "positive" effects of seeding.