WEATHER and CLIMATE MODIFICATION Problems and Prospects

VOLUME I

Summary and Recommendations

NATIONAL ACADEMY OF SCIENCES—NATIONAL RESEARCH COUNCIL

WEATHER and CLIMATE MODIFICATION

PROBLEMS and PROSPECTS

VOLUME I

Summary and Recommendations

Final Report of the
PANEL ON WEATHER AND CLIMATE MODIFICATION
to the
COMMITTEE ON ATMOSPHERIC SCIENCES
NATIONAL ACADEMY OF SCIENCES
NATIONAL RESEARCH COUNCIL

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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

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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.

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

Contents

Introduction

The subject of weather and climate modification is concerned with any artificially produced changes in the composition, behavior, or dynamics of the atmosphere. Such changes may or may not be predictable, their production may be deliberate or inadvertent, they may be transient or permanent, and they may be manifested on any scale from the microclimate of plants to the macrodynamics of the worldwide atmospheric circulation. The scope of intellectual effort needed in this field is correspondingly large. Problems in atmospheric modification present a major challenge to meteorology as well as to many other scientific, engineering, and professional disciplines. The major problem areas, furthermore, are similar in kind and scope to many found throughout the environmental sciences.

One might ask why so detailed a survey of the status and outlook of atmospheric modification as we have made should be undertaken at this time. During approximately the past decade, subtle but significant shifts have occurred in long-term prospects for weather and climate modification; in many fundamental respects, an earlier era of speculation had gradually been superseded by the present period, in which rational and systematic exploration of modification potentialities has become possible. Several changes stand out as factors causing this shift:

(1) Formulation of increasingly complete and elaborate theories of atmospheric processes has advanced to a state in which moderately realistic mathematical models can be constructed for a variety of atmospheric systems ranging in scale from micrometeorological to global. Admittedly crude and rudimentary in many instances, such models constitute a necessary first step in reducing the degree of empiricism that has characterized most past speculations concerning atmospheric modification.

(2) Prior to about 1950, such mathematical models were for the most part unproductive because of the sheer mathematical complexity of the systems of equations constituting the models. The advent of high-speed

automatic computers has, within the past decade or so, radically altered this picture. Computing speeds and storage capacity have risen by many orders of magnitude, and a growing body of investigators in the atmospheric sciences has seized this powerful new tool to use it in analyzing crucial aspects of the physics of our atmosphere. The important practical goal of improved numerical weather prediction became a stimulus that has recently led many workers to conduct increasingly elaborate computer studies in the broad area of numerical experimentation. Today, numerical simulation, albeit impressively complex and varied in scope, is almost certainly only a primitive first step toward future levels of understanding of the subtle and highly interdependent workings of our atmosphere; but it is a beginning with foreseeably profound implications for weather modification. This development alone is significant enough to justify a new and deeper examination of modification prospects.

(3) Man's ability to measure and to observe the atmosphere with its myriad parameters has been growing steadily. Two decades of improvement in use of aircraft as measurement platforms, two decades of elaboration of radar-meteorological techniques, and soon a full decade of experience with the incomparable synoptic observing capabilities of the meteorological satellite, combine with many other advances in instrumentation and observation systems to permit almost entirely new dimensions in man's ability to keep track of the rapid changes that are so characteristic of weather. Clearly, still further improvements may be expected in the future, but one senses that already we have available the measurement skills requisite to monitoring adequately many of these atmospheric systems we seek to modify.

The three considerations discussed above are of sufficiently basic importance to prospects for present and future weather modification that, even without the particular stimulus of current advances in cloud modification *per se,* it would be most timely to undertake a survey of the field of atmospheric modification. Undoubtedly our presently available (or at least presently recognized) techniques for modification will in coming years seem primitive, but we feel that a look ahead and an attempt to lay down certain guidelines for the near future are now in order.

Objectives of weather and climate modification focus on *control:* to produce deliberate beneficial changes in the environment and to bring under control, or avert, changes that are damaging to society. These objectives are as ambitious as any ever confronted by physical science. Even a small measure of control over the weather could be of very great value, the more so as modern industrial society grows increasingly complex and interdependent, and hence more vulnerable to natural disasters of all kinds.

In a sense, weather modification today is a reality. Man can and does interfere with the atmosphere in a number of different ways. His ability to produce deliberate beneficial changes is still very limited and uncertain, but it is no longer either economically or politically trivial. The consequences of inadvertent modification, in which we know man is participating increasingly, are also too little known. These crucial uncertainties will not be removed until artificial disturbances can be distinguished from the natural variations of the atmosphere. In short, the field of weather modification cannot progress independently of basic understanding leading to more refined predictability of the state of the atmosphere. The immediate scientific and engineering steps toward this composite goal are clear. They amount to a new research and development program for weather and climate modification of a kind and scale very different from that which exists today.

Progress and Problems In Weather Modification

The following paragraphs constitute a brief summary of our conclusions concerning the present state of research, development, and operational application in weather and climate modification.

Volume II of this report contains a quite detailed, critical survey of the field, and represents the scientific assessment on which the Panel has based its recommendations presented in the last section of this volume. We urge the interested reader to refer to Volume II, since many important details of our study are necessarily omitted from the type of summary we present here. The organization of Volume II reflects the Panel's identification of four major problem areas—modification of clouds and storm systems, modification of large-scale weather and climate, modification of local and regional climates, and the inadvertent modification of the atmosphere. In addition, we have identified a number of special problem areas, most of which deserve emphasis because they apply to many objectives and have been seriously neglected despite their importance.

Modifying Clouds and Storm Systems

1. *There is increasing but still somewhat ambiguous statistical evidence that precipitation from some types of cloud and storm systems can be modestly increased or redistributed by seeding techniques. The implications are manifold and of immediate national concern.* The evidence is complex and, in part, contradictory. The Panel's present view of the prospects of rain stimulation by cloud seeding differs from that presented in the preliminary report. This change in opinion is based not only upon our detailed analysis of operational projects but also upon our review of a substantial number of recently completed experimental programs. The theoretical basis for seeding effects is still very crude because we still do not have an adequate understanding of the physical details of many

basically important cloud processes. Therefore there are few guiding principles for improving the effects of seeding and for extrapolating seeding expectations to different localities. With respect to the operational projects, we emphasize that lack of randomization precludes drawing truly decisive conclusions. Nevertheless, we feel that useful inferences of seeding efficacy can be drawn from the kind of careful scrutiny of such projects that we have attempted. We shall now review cloud-seeding experience with three different types of systems: orographic storms, cumulus clouds, and extratropical cyclones. This threefold classification is somewhat artificial, but useful for our present purposes.

(a) *Orographic storms.* Evaluations (by this panel) of 41 project-seasons of winter orographic cloud seeding by commercial operators in the western United States support the earlier conclusion (by the Advisory Committee on Weather Control in 1957) that precipitation increases of the order of 10 percent apparently can result from ground-based silver iodide seeding of winter orographic storms. Similar but more variable and equivocal results have come from field research projects in many parts of the world. Two randomized winter orographic projects in the United States now are in progress; their results will be of interest.

(b) *Cumulus clouds.* Experimental and operational evidence relating to the stimulation of cumulus precipitation remains highly confusing. The Panel's evaluations of 14 operational silver iodide seeding projects in the eastern United States, including *but not limited to* cumulus clouds, indicate variable rainfall increases averaging about 10 to 20 percent in the nominal target areas. A recent follow-on study made at the Panel's request suggests comparable increases up to 150 miles downwind of the targets. Project Whitetop, a randomized cumulus seeding experiment in Missouri, tentatively indicates about 5-10 percent increases in the nearby target area, and also decreases of about the same size (rainshadow effect) downwind from the target area. Final Whitetop conclusions are awaited with interest. On the basis of a number of randomized experiments in Australia, the investigators have concluded that the greatest positive seeding effects tend to occur in systems of cumuliform clouds. Other past experiments have yielded only visual or statistically uncertain results. Several current projects are concerned with the important question of whether the release of latent heat, triggered by silver iodide seeding, may have discernible effects on the growth and the precipitation budgets of cumulus clouds.

(c) *Extratropical cyclones (nonorographic).* There is no clear evidence of success to date in stimulating precipitation from this type of storm in the United States; but little effort has been made except by commercial operators whose records make no distinction between cyclonic and other storm types. Reports from Russia, Israel, and Japan indicate increases of varying amounts due to both silver iodide and dry-ice seeding, while other results (Australia, Canada) show no apparent seeding effects. Reported Russian results show some evidence also of a downwind decrease (rain-shadow) beyond the nominal target area.

The major problem in assessing seedability is the great natural variability of the principal precipitating cloud systems. The variability is manifested in drop-size spectra, water content, ice content, temperature structure, internal circulation, electrification, and related factors. None of these factors can now be ascertained under field conditions accurately enough to permit other than crude distinctions between seedable and unseedable clouds or selection of the most promising geographical areas or synoptic weather situations. Furthermore, this variability leaves open the possibility that *decreases* in precipitation may sometimes result from seeding, a possibility that might well also be turned to useful purposes. It is likely that some clouds are not responsive to silver iodide seeding, e.g., those with an abundance of natural ice crystals and those that are warmer than 0°C throughout. The latter type has been seeded with water spray and with salt particles, giving generally positive results with the water spray and rather uncertain results with the salt. The large natural variability of cumuli makes rigorous statistical evaluation both mandatory and very difficult.

2. *There is a wide range of opinion on whether or not hail can be effectively suppressed or its damage mitigated.* The most prevalent premise is that producing many more hailstone embryos by silver iodide seeding will yield smaller hailstones, which would be less damaging and more likely to melt before reaching the ground. The U.S. experiments using ground generators or aircraft generators have been inconclusive. Major long-term experiments in Switzerland and France have been similarly inconclusive. Experiments in Argentina, however, show positive results for one type of storm and negative results for others. The Russians are far more optimistic. They claim significant success from introducing the silver iodide directly into the supercooled high-liquid-water-content portion of the cloud by means of antiaircraft shells and rockets. These programs are said to have saved over a million rubles of crop damage. Because hailfall is even more variable than rainfall, a definite proof of success is all the more difficult to obtain. On physical reasoning, for example, we cannot exclude the possibility that seeding may sometimes even increase hail damage. A fundamental obstacle to more effective hail suppression techniques is our incomplete understanding of important physical processes of hailstorm phenomena, particularly the dynamics of the large storm

clouds. In this regard, the U.S. hail-research program has been piecemeal and clearly of subcritical size; hence, we are pleased to note the current efforts aimed at establishing an integrated national hail-research program.

3. *Experiments in lightning suppression are beginning to show some promising results,* according to reports from the only active project expressly undertaken for this purpose (Project Skyfire, U.S. Forest Service). The physical concept is to produce in a thundercloud, by silver iodide seeding, an abnormal abundance of ice crystals that would act as added corona points and thus relieve the electrical potential gradient by corona discharge before a lightning stroke could develop. A similar approach proposed by the U.S. Army would distribute metallic chaff dipoles into the storms. Published Skyfire results indicate decreases in the number of cloud-to-ground lightning strokes. Further work should provide for assessment of stroke intensities as well as frequencies.

4. *With respect to both hurricane and tornado modification, no practical success can be expected before the development of adequate theories of the genesis and behavior of these storms.* Calculations indicate that brute-force techniques hold little promise for overpowering tornadoes, and even less for hurricanes. There are *distant* possibilities that modern techniques directed toward cloud-dynamics might some day be effective against tornadoes. Most of the crucial problems in hurricane theory remain unsolved. Recent hurricane-modification experiments (Project Stormfury) resulted in what may be artificial effects on the eyewall cloud, but the observed changes fall within the natural variability of hurricanes. The most pressing need is for continuous birth-to-death observation of hurricanes from above, within, around, and beneath the storms.

5. *The dissipation of supercooled ("cold") fogs and low stratus clouds over limited areas is operationally practicable,* as demonstrated by the operational clearing of airport fogs in Greenland by the U.S. Air Force, in the U.S.S.R., and in the western United States by several airline companies. Three seeding techniques—dry ice, silver iodide, and liquid propane, all of which initiate ice-crystallization and subsequent fallout of cloud particles—have proved successful. Improved engineering methods are being actively sought by people concerned with aviation safety and efficiency.

6. *In recent years, no significant progress has been reported in efforts to dissipate "warm" (nonsupercooled) fogs and stratus clouds.* In 1938, small volumes of warm fog were cleared by a calcium chloride spray technique;

in World War II and subsequently, aircraft landings were aided in England and the United States by the costly "FIDO" method. The former method employed the extreme deliquescence of calcium chloride to "dry out" the foggy air; the latter method forced the fog to evaporate or lift by forced heating from below. Recent unsuccessful experiments have included dry-salt seeding and attempts to produce a strong electrical gradient across the fog layer. Very limited success has been attained by water seeding to "wash out" the cloud, carbon-black seeding to heat and evaporate the cloud, the use of jet engines as a modification of the "FIDO" approach, and mixing a ground fog with dry air above using helicopter "prop-wash."

Modifying the Weather and Climate of Large Areas

It can be stated categorically that there is, at present, no known way deliberately to induce predictable changes in the very large-scale features of climate or atmospheric general circulation. While man may attain the technological capability to induce perturbations sufficient to trigger massive atmospheric reactions, we cannot now predict with certainty all the important consequences of such acts. As long as our understanding is thus limited, *to embark on any vast experiment in the atmosphere would amount to gross irresponsibility*. This does not necessarily mean, however, that major modifications (continent scale or larger) may not some day be both feasible and safe. To hasten that day and to guard against premature projects, three converging and related avenues of research are suggested.

1. *A comprehensive theory of natural climatic change must be pursued, though this will be a most difficult task.* Some patterns and details of past climatic fluctuations are now slowly emerging. A major question is whether the present climatic regime is relatively stable or can change (or be deliberately changed) suddenly. Paleoclimatic evidence when properly understood may provide suggestions of large-scale instabilities in the atmosphere.

2. *Theoretical simulation of global atmospheric behavior is a means of testing climatic theory, and appears the most promising way to test proposals for modifying large-scale features of weather and atmospheric circulation.* The main effort must be in developing valid theoretical models of atmospheric circulation for mathematical integration on high-speed computers. Ideally, such models would simulate atmospheric dy-

namics and global climate on a theoretical basis so sound that the consequences of artificial perturbations could be assessed. Current numerical models are not adequate except for crude assessments. While the underlying need is for the strengthening of basic theory, we must also pay heed to two immediate practical obstacles—inadequate computers and inadequate global data—both of which are discussed further in the later section containing our recommendations.

3. *Much more research must be directed toward energy-exchange processes in the earth–air boundary layer.* One of the principal theoretical weaknesses of present-day atmospheric circulation models is the improper representation of important interactions across the air–land and air–ocean interfaces. In the context of atmospheric modification, this weakness appears to be especially critical, for most climate-modification schemes are based upon a mechanical alteration of some characteristic of the earth's surface. The contributions of boundary-layer research are potentially of significant value in *many* aspects of weather and climate modification. Field experiments should thoroughly explore the effects on energy exchange that can be brought about by altering the albedo, roughness, moisture content, and thermal and radiative properties of the surface material. Concurrently, numerical studies of boundary-layer exchanges should be pursued, and particular attention should be given to the simulation of the effects of turbulence. Results of research on small-scale processes should be extended, as soon as practical, to the development of adequate theories of convection, local wind systems, and other meso-scale manifestations of boundary-layer influence on the large-scale atmospheric circulation.

Modifying Local and Regional Climates

1. *The future potential of local (agricultural) and regional (desert) climate modification rests squarely on progress in understanding boundary-layer energy exchange.* Since the same research need was discussed in the preceding paragraph, we shall go into no further detail here except to emphasize that results of micro- and meso-scale researches have *direct* application in studies of local and regional climate modification. Various methods of altering transient aspects of the agricultural microclimate have been practiced for centuries. Modern objectives are essentially the same—primarily to reduce plant damage due to frost, excess heat, and wind. The Panel did not thoroughly investigate this smallest scale of climate control.

2. *It appears theoretically feasible to ameliorate desert conditions in certain regions by altering the thermal properties of limited ground areas.* No full-scale field tests of proposed methods have yet been made; when they are, they will require substantial funding and logistic support. The concept that has received most theoretical attention is the "thermal mountain effect." It is proposed to blacken the surface (decrease the albedo) over a limited area of coastal desert and thereby stimulate convection and, in turn, to increase cloudiness and precipitation downwind. Plans have been formulated for a field test of this technique, but are not definite at the time of this writing. There are many different ways by which the atmosphere produces deserts; no single modification approach could be universally applicable (and many deserts are probably not subject to significant modification except through a major change in the global circulation). Recent observational and theoretical studies of a Peruvian coastal desert have, for example, indicated that the opposite of the "thermal mountain" approach (i.e., increasing surface albedo) might be effective there.

Inadvertent Atmospheric Modification

Today, unintentional alteration of the atmosphere by man is a minor problem compared with what it will be one or two generations hence. No prospect appears for a slackening in the exponential growth of such civilized activities as burning carboniferous fuels, building cities, flying aircraft, and launching rockets. We are just now beginning to realize that the atmosphere is not a dump of unlimited capacity, but we do not yet know what the atmosphere's capacity is or how it might be measured. The overriding immediate need is for greatly improved and expanded methods of detecting man-made alterations in the composition and energy budget of the atmosphere.

1. *It is generally agreed that the total amount of carbon dioxide (CO_2) in the atmosphere has increased by 10 to 15 percent in this century, and that the increase is due to the burning of fossil fuels.* The most recent calculations of the enhanced "greenhouse effect" of increased atmospheric CO_2 indicate that surface temperatures may have been caused to rise only about 0.2°C since 1900, but the stratosphere may have cooled by ten times that amount. Considering the continuing consumption rate of carboniferous fuels, the implications of this for future tropospheric stability cannot be ignored. The obvious needs are for continued monitoring of atmospheric and oceanic CO_2 content and for the simulation of CO_2

effects (including effects on atmospheric circulation due to altered thermal structure) using the most sophisticated atmospheric models and numerical computers available.

2. *The problem of air pollution may already be growing beyond its urban domain—local in size and mainly biological in effect—to become a widespread nuisance having possible significant effects on weather and climate over much larger areas.* Little attention has been given to the effects of pollution on cloudiness and precipitation or on the radiation balance. The growth trend of pollution sources will no longer permit ignoring them. The city climate itself, altered as it is by the physical character of the city, is becoming a growing economic factor as larger populations concentrate in urban areas. A key problem is that as cities grow their natural ventilation decreases while their emission of heat and pollutants increases. *Urbanization is, in fact, a kind of continuing experiment in climate modification; and this source of important environmental knowledge is essentially untapped.*

3. *Compared to the effects of large-scale city building, the meteorological effects of altering the rural landscape (by forestation, deforestation, and irrigation, for example) appear to be quite small and localized.* Some results of altering the micrometeorology of the boundary layer can have profound influence on local hydrology (runoff, erosion) and on local ecology. These topics, though important, are outside the purview of this study.

4. The aerospace age has added another dimension to the problem of inadvertent modification. The advent of supersonic transport aircraft, flying routinely in the stratosphere, has raised a question concerning possible consequences of the additional water vapor to be injected by these aircraft into the stratosphere. Our *tentative* conclusion, based on an assumed traffic volume of four flights per day for 400 supersonic transport airplanes, *is that neither additional cloudiness (contrails) nor water-vapor absorption of long-wave radiation will be sufficient to disturb appreciably either stratospheric properties or the large-scale circulations that are influenced by its thermodynamic state.* Rocket-exhaust contamination of the higher atmosphere is, however, a much more complex problem, primarily because the chemistry of the mesosphere and thermosphere (including the radiational roles played by the minor constituents), and the processes in those regions that disperse or remove impurities are so little understood. Since rocket activity will increase, and since we are already capable of doubling, in a single year, the quantity of some exotic con-

stituents (e.g., atomic sodium), vigorous investigation of aeronomy and high-atmospheric dynamics is a clear prerequisite to settling the issue of rocket-caused contamination.

Additional Research and Development Requirements

We have stated a number of conclusions regarding present and future research directly related to weather modification. We now turn to five problem areas, advances in which are desirable both for achieving useful weather modification and for increasing our understanding of the atmosphere.

1. *The question of acceptable statistical design and evaluation of atmospheric experiments must be resolved; to do so, much more direct and continuing communication between statisticians and experimenters is required.* The great variability of natural atmospheric processes will always impose a high "noise level" on data collected for experimental evaluation. To arrive at significant conclusions, two approaches, not totally exclusive, are possible: either collect a very large quantity of pertinent data or select for evaluation those phenomena whose natural variability is minimal. The former may be accomplished by conducting an experiment over a long time or by enlarging the space scale of the experiment. The effects of natural variability can be reduced by subdividing (in statistical terminology, "stratifying") the experimental data according to physically defined classes of experimental situations, e.g., "storm types," and evaluating each class separately. Another method is to divide a total hypothesized process into a series of subprocesses, and evaluate each subprocess separately. Both of these evaluation methods demand experimental measurements much more sophisticated than those now being employed. Other approaches are possible and should be thoroughly examined. The search for more powerful evaluation techniques, however, should not delay the establishment of certain important field projects in which evaluation can proceed according to accepted standards that are adequate, if not optimal.

2. *The design, development, and testing of instruments, vehicles, and other equipment for field measurement and experimentation have been inadequate.* Accurate, reliable, quantitative information on many meteorological parameters is crucial. The needed data can be obtained only by more extensive use of greatly improved or new instruments. Similar, perhaps standardized, measurement programs and experiments must be initiated in different locations under a variety of meteorological con-

ditions. Since silver iodide and dry-ice cloud seeding seem to produce interesting and potentially valuable effects, the uncertainty of our quantitative knowledge of the whole process, from generator output through cloud microphysics to precipitation, is no longer tolerable.

3. *The study of atmospheric water budgets on all scales must be accelerated and expanded.* Detailed knowledge of water-vapor transport, which is quite limited today, is essential to further experimental efforts in cloud modification and must underlie the assessment of any larger-scale modification schemes. Atmospheric vapor flux and flux divergence should be studied as a part of the total atmospheric energy budget. While global evaluations will for some time be hampered by lack of data in the tropics and the Southern Hemisphere, Northern Hemisphere data are now adequate for detailed surveys of vapor transport over continents and large watersheds. Detailed water-budget studies of individual clouds and storms will depend strongly on improved measurement programs of the type discussed above.

4. *The tropics have been grossly neglected in meteorological research despite the central importance of the tropical oceans and tropical convection in driving the global general circulation.* The tropics, comprising about half of the total earth's surface and being about 75 percent oceanic, have long been considered inaccessible to detailed observation. Part of the remedy for this situation will be the use of synchronous-orbit satellites to observe certain radiation parameters and the geographical distribution of tropical cloud systems. To interpret the satellite observations, however, it will be necessary to have detailed quantitative understanding of energy-exchange mechanisms typical of the air–ocean interface and of the different cloud systems in the tropics. The only way such understanding can be acquired is through a *comprehensive field-measurement program* concentrated from the beginning on tropical convective processes. The long-term scientific objectives of this program may be prescribed by the needs of atmospheric numerical simulation, but the basic questions as to geographic and dynamic differences in the nature of cumulus clouds and precipitation processes come first.

5. *There are many compelling reasons for thorough investigation of hurricane energetics and establishment of an adequate organizational structure capable of managing the required large and complex field program.* Such a program must include (1) continuous monitoring of hurricane generation areas and observation of the large-scale features of storms and their environments throughout their life cycles; (2) frequent penetration of the storms and surrounding atmosphere with instruments

to measure the time and space distributions of air motion, temperature, water vapor, heat flux, cloud structure, and related factors; and (3) frequent measurements of the thermodynamic properties of the ocean–air interface beneath and surrounding the hurricanes. Until hurricanes are studied on such a comprehensive scale, we have little hope of devising any effective means of modifying them or even of improving significantly our present ability to predict their intensity and movement.

Federal Support of Weather-Modification Research

The support of meteorological research in the United States is shared by a large number of agencies. Table 1 lists the contributions of the various agencies to atmospheric research, excluding aeronomy. The large and rapidly fluctuating component of support by the National Aeronautics and Space Administration should be noted. The total government expenditures in atmospheric sciences and meteorological services in fiscal 1965

TABLE 1. *Federal Support of Research in Meteorology* (in Millions of Dollars)[a]

Agency	Fiscal Year		
	1963	1964	1965
Agriculture	1.2	0.5	0.5
Commerce	10.4	8.5	8.2
Defense			
Army	8.5	9.7	11.2
Navy	4.5	4.9	5.2
Air Force	8.0	7.6	7.6
Health, Education and Welfare	1.5	1.6	2.0
Interior	1.6	1.0	2.9[b]
Atomic Energy Commission	4.0	5.1	5.7
National Science Foundation	9.5	11.7	9.7
Subtotal	49.2	50.6	53.0
National Aeronautics and Space Administration	54.1	63.2	31.2
Total	103.3	113.8	84.2

[a] Data extracted from: "Government Weather Programs (Military and Civilian Operations and Research)," First Report by the Committee on Government Operations, 89th Congress, 1st Session, *House Report No. 177*, March 17, 1965 (Table 8. National Atmospheric Sciences Program Summary by Agency Programs, p. 27).

[b] Includes $1.0 million appropriated subsequent to issue of *House Report No. 177.*

TABLE 2. *Agency Support of Weather Modification in Fiscal 1965* [a]

(in Millions of Dollars)

Agency	Fiscal Year		
	1963	1964	1965
Agriculture	0.13	0.12	0.14
Commerce	0.19	0.18	0.11
Defense			
Army	0.43	0.73	0.25
Navy	0.35	0.50	1.00
Air Force	0.18	0.18	0.20
Health, Education and Welfare	—	—	—
Interior	0.10	0.18	1.26
Atomic Energy Commission	—	—	—
National Aeronautics and Space Administration	0.05	0.07	—
National Science Foundation	1.32	1.57	2.01
Total	2.75	3.53	4.97

[a] Data extracted from the National Science Foundation annual reports on *Weather Modification* (Nos. 5, 6, and 7) to the President and the Congress.

was $431.5 million. If the National Aeronautics and Space Administration operations are excluded, meteorological *research* depends upon 12 percent of this amount. The proportion of these research funds supporting academic research is difficult to determine, but we would estimate that universities received about $20 million in fiscal 1965, or about 40 percent of the meteorology research budget.

The expenditures in direct support of weather modification form a small fraction of the total funds available for meteorological research. The support in fiscal 1965 totaled $4.97 million, with seven agencies sharing responsibility (see Table 2). We estimate that about $1.6 million of this is used in support of academic research.

Among the agencies, the National Science Foundation supports a wide variety of activities in weather modification. The Navy's program shown in Table 2 represents the Stormfury Project and Project ACE. The former is a hurricane-modification attempt, and the latter is a more general program in weather modification. The principal concern of the Air Force is the development of capability to disperse all types of low stratiform clouds and fog. Work in the U.S. Army deals with the dissipation of supercooled cloud layers and fog, with development of condensation-nuclei seeding techniques and study of lightning prevention. The Department of the Interior, in its Bureau of Reclamation Atmospheric Water Resources Program, is endeavoring to ascertain the feasibility of operational cloud-seeding to increase precipitation in several western U.S. watersheds.

No hard figures are available on the level of research in weather modification in other countries, but we believe we can make a rough estimate of expenditures in the Soviet Union. The Soviet Union is a technologically advanced country with a meteorological setting somewhat similar to that of the United States. In 1964, the Hydrometeorological Services in the Soviet Union employed some 70,000 persons, as compared with 51,000 in 1960.* This manpower level corresponds to an annual budget of $700 million to $800 million if the ratio of expenditures to manpower in meteorology is comparable to that in other areas of research and development (as discussed by Korol). The total level of funding would then be nearly twice that in our country. If we assign about 10 percent of the total expenditures to research, then the Soviet Union is devoting some $70 million to $80 million to the atmospheric and related sciences, and very probably this figure does not include satellite operations. Two members of our panel, Verner E. Suomi and Joseph Smagorinsky, have visited important centers of research in the Soviet Union. The Panel has also discussed the Soviet effort with David Atlas and Louis J. Battan, both of whom have recently made extensive tours of Soviet facilities. It is estimated that about 25 percent of Soviet atmospheric research is devoted to weather modification—perhaps about $20 million. Although this can at best be only a rough estimate, the total Soviet effort in weather modification is greater than the U.S. effort by at least a factor of two. We do not intend to imply by this comparison that we should attempt to duplicate or imitate the Soviet effort. We note, however, that the Soviets have assigned a higher priority for weather modification than we have.

Weather modification in the United States is supported by a large number of relatively small efforts. Only two or three projects could be classed as major, that is, having expenditures of over $250,000 annually. We now anticipate a number of large efforts, perhaps involving the capabilities of several agencies. The present allocation procedures clearly do not provide a sound basis for comparison and selection among competing projects entailing major investments. How is coordination to be achieved among the various agencies? In principle, it is now undertaken by the Interdepartmental Committee for Atmospheric Sciences, but the authority and influence of this committee are limited. Under Public Law 85-510, the National Science Foundation has broad responsibility for the promotion of the study of weather modification, but the Foundation has not been able to develop a national program in this field because it is

* Korol, A., *Soviet Research and Development: its Organization, Personnel and Funds*. The MIT Press, Cambridge (1965).

only one of a number of agencies with legitimate interest and responsibility in weather modification.

In our examination of administration and management in weather modification, we identify four problem areas:

1. *Level of effort.* All technical achievements in meteorology contribute to the economic growth of our country, but the magnitude of the contribution is and will continue to be a matter for economic debate. The situation with respect to weather modification is part of the economic complex. Here the marginal returns could be very great. We cannot estimate the quantitative economic value of further research in this field, but we believe that the present level of effort is in no way commensurate with the demonstrated opportunities for further research likely to have early practical implications.

2. *Dissipation of research resources.* We are concerned that the major portion of research resources (money and manpower) in weather modification is being dissipated by supporting subcritical efforts. While opportunities have been and must continue to be provided for the individual researcher, it is a disturbing fact that few opportunities have been provided for major research groups to adopt weather modification as their primary research interest. Indeed, the scientific community has not pressed for major weather-modification efforts. Further progress in the field will depend upon the explicit recognition of weather modification as an exciting and productive field for research. New efforts should not merely expand the scale of current work; they should be of the quality of research in other areas of atmospheric science.

3. *Implementation of large field studies.* At present the complete implementation of large field programs, such as the joint Navy-Weather Bureau study of tropical cumulus clouds, depends on the initiative and persistence of small and dedicated scientific groups. They are severely hampered by lack of a central management organization with authority and skill to consummate interagency negotiations and operations. While occasional field studies can be mounted successfully on an *ad hoc* basis, the large-scale programs required in the future must, in general, be provided with adequate management support. In this way, the small cadre of scientists could concentrate on the scientific objectives and technical requirements of the undertaking, rather than on matters that are properly the concern of government.

4. *Interagency cooperation.* The Interdepartmental Committee for Atmospheric Sciences has, in principle, responsibility for coordinating efforts in weather modification. The National Science Foundation plays a special role through the responsibilities assigned to it by Public Law 85-510. Through the annual Interagency Conference on Weather Modi-

fication, the Foundation has provided for essential communication among scientists in the government. As things now stand, however, any major agency can bypass these bodies and introduce a large perturbation in the national effort by unilateral action, which in itself may involve only a very small fraction of that agency's total budget. Most importantly, the Interdepartmental Committee for Atmospheric Sciences has no power to initiate action within any agency. Thus, the dedicated efforts of a competent group within an agency can be effectively stifled by a nonsympathetic agency administration.

Conclusions and Recommendations

Administration and Funding of Research and Development in Weather Modification

We are convinced that weather modification presents some of the most pressing scientific problems facing our society; therefore, the temptation to beg for more money for an enlarged research effort is great. This temptation is reinforced by awareness of the speed with which advances in understanding could be applied to the vexatious environmental problems of our growing civilization. On the other hand, we have become increasingly aware that the present deficiencies in the magnitude and administration of the support of research and development in weather modification are not unique, but rather, are common to all environmental sciences.

Government involvement in the environmental sciences includes several research areas—meteorology (including weather modification), climatology, aeronomy, oceanography (including marine biology), solid-earth geophysics, geology—and several engineering areas—pollution, ocean technology, operational weather modification, weather forecasting and climatological services, hydrology, and mineral exploration and technology. These areas of research and development have the following features in common:

(a) The subjects tend to be interdisciplinary and, as a rule, lack strong professional or academic tradition.

(b) Support for each is diffused among many agencies. Each agency has a primary social purpose and function within the limits of which it conducts its own relevant environmental investigations. As a result, for most of these environmental fields there exists no single natural advocate in the federal structure, nor is there a clear mechanism for making budgetary decisions in the agencies. Indeed, the advocacy for programs in these fields often comes from sectors of society other than the Executive Department or the academic community. As a consequence, environmental problems often do not get the serious attention or correctly balanced emphasis that their importance to the nation requires.

(c) The "big science" part of these areas consists of large field programs that place stringent demands on logistic support. Often the support must be furnished by agencies other than the one responsible for a particular project. Large vital projects often have no single logical home in the present federal structure.

(d) The present Interdepartmental Committee for Atmospheric Sciences provides help on questions of policy coordination. In addition to the needs for *policy* coordination, there is an important need for *operational* coordination and detailed technical planning of large-scale experiments. The requirements of operational coordination demand a line organization rather than an advisory group. This line organization would need to control a larger portion of the over-all budget for weather modification than any single agency now does.

We conclude that the administrative division of the environmental sciences, according to the diverse social purposes of different federal agencies, has been rendered obsolete by the increased interdependence among the various areas of environmental research and engineering.

We conclude that the present support and administrative mechanisms do not provide adequate means for the setting of priorities among the many large field experiments and projects that will eventually be needed.

We also conclude that the present fragmentation of effort in weather-modification research and development is unusual in that many of the fragments are below critical size or quality needed for effective work.

We believe that major responsibility for weather modification should be centered in a single agency; at the same time, however, a degree of delegated responsibility should be maintained that will allow other agencies to meet their mission requirements for work in this field. The Panel considered a number of possible administrative arrangements for the support of weather-modification research—for example, a national laboratory for weather modification; a lead agency, either existing or new, with prime responsibility for weather modification; or multiagency sharing of mission responsibility. However, the complexity of the problem is such that the Panel does not wish to make a firm statement as to the most desirable administrative means of achieving the goals set out in this report.

Recommendations

We recommend an immediate and thorough study of the administration and support of research and development in weather modification.

The group performing the study should consider the consolidation of primary responsibilities for weather modification in one agency (either existing or new) and should consider whether or not weather modification can sensibly be separated from the rest of the environmental sciences. Due attention should be given to the regulatory aspects of operational weather modification.

We recommend that immediate steps be taken by the agencies to raise the support from the 1965 level of $5 million to at least $30 million by 1970.

This recommendation is based on a rough estimate of the cost of needed research with emphasis on field research programs. Future progress in weather modification will depend in a critical way on progress in understanding the atmosphere, and in extending our capability for predicting atmospheric phenomena both in time and on a local scale. Increased funding in weather modification should therefore be accompanied by a commensurate increase in funding for the supporting atmospheric sciences and for the development and operation of supporting research facilities and systems, including new computers and observational networks for simulating atmospheric circulation.

The Panel does not anticipate that efforts in weather modification will be limited in manpower over the next five years. The problems of weather modification are of such broad scientific interest that atmospheric scientists will be joined by workers from related areas. Moreover, experiences in other fields, such as space and oceanography, suggest that the recognition of a field as one of national interest will tend to guarantee an effective scientific force.

Projects in Stimulation of Precipitation

We have studied a broad range of experimental and commercial precipitation-enhancement programs conducted in recent years both in the United States and abroad. The available evidence, though not conclusive, indicates that artificial nucleation techniques, under certain meteorological conditions, may be used to modify the space or time distribution of precipitation. Specifically, we find some evidence for precipitation increases of as much as 10 or even 20 percent over areas as large as 1,000 square miles over periods ranging from weeks to years. Whether cloud seeding produces significant effects far downwind of the nominal target area remains open to question; there are tentative indications of both increased and decreased rainfall up to 150 miles beyond the primary target area. Whether appreciable decreases can also be induced by artificial cloud-nucleation methods is not yet clear; this question demands careful study. The uncertainties of cloud seeding are such that we cannot condone indiscriminate operational seeding that may destroy opportunities for significant evaluation; rather, we believe that continuing research in both the statistical and physical aspects is required in order to maintain necessary objectivity.

Recommendations

We recommend the early establishment of several carefully designed, randomized, seeding experiments, planned in such a way as to permit assessment of the seedability of a variety of storm types.

In order to obtain more precise conclusions concerning the magnitude of cloud-seeding effects, the need for carefully designed orographic projects continues. Furthermore, our analysis points to a need for similar experiments in both the western and the eastern United States. A broad range of observational techniques should be incorporated into these experiments to provide essential information on the detailed cause-and-effect relationships governing the seeding potential of different storm types. These experiments should be planned to continue for as long as is necessary to provide decisive results.

Scientific information on seeding efficacy may be derived from commercial operations of a type that have not, in the past, contributed directly to the fund of scientific knowledge concerning precipitation enhancement.

We recommend, therefore, that means be found, at federal expense if necessary, to secure much better evaluative reports on operational programs than are currently available.

We recommend that attention be given immediately to careful monitoring and regulation of operational programs for weather modification.

New legislation will be required, and this legislation should reflect the economic, political, social, and scientific implications of the programs. To ensure maximum over-all benefit and public welfare, legislation should include means of assigning to a single federal agency, possibly created for this and related purposes, the responsibility for monitoring and regulating operations and for ensuring the publication of full reports. Such an agency should have powers and resources to conduct independent evaluations, and may need the authority to adjudicate among conflicting projects.

We stress the complexity of all these matters, many of which appear to present unprecedented problems. We therefore urge great care in setting up the necessary legislation and regulatory machinery. Existing agencies of the federal government with responsibilities in the environmental sciences should immediately undertake such studies as may be required for the information of legislators attacking these new problems.

Research Properties

We are reluctant to judge relative importance among a large number of important research problems. Yet, the establishment of the type of national program we envisage must proceed in an orderly manner and can do so only if guidelines are provided. The research requirements are, of course, of many different kinds. We have already recommended the early initiation of new experiments in silver iodide cloud seeding to stimulate precipitation. The recommendations that follow are *in addition* to these and the subsequent major recommendations appearing in the next two sections of this report.

Recommendations

We recommend that planning be started immediately on all the following major field investigations:

1. *A comprehensive exploration of hurricane energetics, leading to the development of a theoretical hurricane model and, subsequently, to hypotheses for hurricane modification.* This program should be planned so that it can commence full-scale operation with the launching of a synchronous-orbit meteorological satellite over the tropical Atlantic.
2. *Measurement of tropical convection and other aspects of energy-exchange processes in the tropics.* We urge that this program be planned to investigate a wide variety of tropical weather phenomena, including "cold lows" and stratospheric and high tropospheric circulations and clouds, and various types of cumulus clouds. This program should evolve over several years, but field work on a small scale could be started immediately, utilizing, for example, the experience and facilities of Project Stormfury. This program will be, in part, closely related to the hurricane investigation, but neither should be allowed to dilute the other.
3. *A comprehensive investigation of hailstorms.* This recommendation is essentially in support of the recommendation passed at the First National Symposium on Hail Suppression, Boulder, Colorado, in October

1965. We stress that this project should be pursued until hailstorms are sufficiently understood to justify specific modification experiments.

4. *A coordinated set of projects to measure the dynamics and water budgets of a variety of precipitating storm types.* No one of these projects need be very large. A certain amount of standardization of instruments and techniques will be required to ensure the quantitative comparability of the different results.

5. In addition to precipitation stimulation studies and to the field research projects recommended above, the Panel recognizes the important potential of other specialized studies involving field activity. Such studies include continued efforts in lightning research, the modification of cumulus dynamics, the initiation of convection, and the dissipation of warm fogs and of extended supercooled cloud layers.

It is clear that research throughout the atmospheric sciences will contribute to the goals of weather and climate modification. Of the research promising the most direct contributions, we *recommend* that highest priority be assigned to the following studies:

1. *Studies of atmospheric water budgets, initially on vapor transport over those portions of the United States where the potential of cloud seeding is important.* Studies of global vapor transport and of individual storm water budgets should be extended as soon as adequate data are available. These studies impose an urgent requirement for the acquisition of data on a global basis.

2. *Studies of boundary-layer energy-exchange processes.* Important questions must be answered concerning the effects of surface roughness and albedo upon the meso-scale circulations. It is apparent that such studies will require the support of extensive field measurements and experimental projects.

3. *Continued development of theoretical models of condensation and precipitation mechanisms, including the early incorporation of dynamical and electrical influences and the effects of changes in concentrations of condensation and freezing nuclei.* These models should evolve into more complete simulations of storm systems, whereby modification possibilities may be disclosed and tested.

4. *New and comprehensive studies of the meteorlogical effects of atmospheric pollution (including carbon dioxide) and urbanization.* This must involve more thorough analyses of urban climates *per se* in light of the fact that they constitute continuing experiments in meso-scale climate modification. Further, the effects on the atmospheric energy balance of pollutant dispersal over large areas and to high altitudes will have to be

evaluated by means of the most advanced numerical models available, coupled with measurements of the physical and chemical characteristics of the pollutants.

Major Research Facilities and Support Systems

From the very beginnings of numerical atmospheric simulation in 1949, computer technology has lagged behind the needs of the atmospheric sciences by at least five years. That the disparity has not been worse is due mainly to the stimulus to the computer industry provided by the applications of nuclear physics. It is clear, however, that the possibilities of atmospheric modification and long-range forecasting are creating urgent new needs for computer development that should be assigned very high priority. Not only must computer technology be responsive to these crucial needs, but also the number of computers must be commensurate with the scientific talent available to exploit them.

Closely associated with the need for more powerful computers is a need for a greatly improved worldwide weather-observing system. Such a system is an obvious prerequisite for more accurate and longer-range prediction, and is thus receiving international attention at the present time. Less obvious is the fact that adequate simulation for testing large-scale atmospheric modification also depends on global data of far greater density and quality than have so far been realized.

Acquiring and coordinating the components of measurement systems that are the heart of large field investigations has proven extremely difficult in the past, to the detriment of scientific objectives. For example, aircraft have proven indispensable for probing the atmosphere, both in direct cloud-modification attempts and for research investigations basic to weather and climate modification. The two civil facilities, at the Environmental Science Services Administration and the National Center for Atmospheric Research, have provided many of the needed aircraft. However, where additional aircraft have been required, and where specialized aircraft (e.g., high-altitude or hovering types) have been required, it has been necessary to call upon military sources. Military priorities have made this an impractical and often awkward alternative.

Recommendations

The best computer just now becoming available has only one fiftieth of the effective speed required for our needs at the present time. In 10 years our computation requirements may grow by two to three orders of magnitude.

We recommend, therefore, that all necessary steps be taken to encourage the computer industry to respond to these prospective requirements.

The requirements for such an advanced computing system have been developed by several groups, including the National Center for Atmospheric Research, the University of California at Los Angeles, and the Interdepartmental Committee for Atmospheric Sciences.

We recommend full U.S. support and leadership in promptly establishing an advanced global-observational system.

The scientific guidelines for appropriate action have been set down by the National Academy of Sciences Panel on International Meteorological Cooperation.* International planning for a "World Weather Watch" is proceeding under the auspices of the World Meteorological Organization.

In order to promote the maximum effectiveness of large field-research programs, such as those needed in the study of clouds, hurricanes, and tropical convection:

We recommend that the civil research aircraft facilities be enlarged to include diversified types of aircraft and supporting data-gathering systems to meet the requirements placed upon them.

INTERNATIONAL ASPECTS

It is clear that a long-range program of weather control and climate modification can have a direct bearing upon relations between nations. It can aid the economic and social advancement of the less-developed countries, many of which face problems associated with hostile climates and serious imbalances in soil and water resources. And, quite importantly, it can serve to develop common interests among all nations and thus be a stimulus for new patterns of international cooperation.

The challenge and opportunity presented to the world by the prospect of man's achieving the power to modify his atmospheric environment is one of the most exciting of the long-range aspects of the subject. We are dealing with the possible consequences of a new and perhaps enormous power to influence the conditions of human life. Its potentialities for beneficial application are vast. It will ultimately be essential, internationally as well as domestically, to develop political and social controls over the use of this power in order to limit its use to predictably constructive purposes.

The very fact that the development of a capability for influencing the

* *The Feasibility of a Global Observation and Analysis Experiment,* NAS-NRC Publ. No. 1290 (1966).

atmospheric environment is still in its infancy enlarges the opportunity to develop optimal attitudes and patterns of collaboration. Small beginnings in collaboration on problems of weather and climate have already been made; one may hope that these will prove useful in stimulating a pragmatic recognition of the material advantages to be derived from further collaboration in these matters.

In summary, the Panel strongly urges that a broad research program in weather control and climate modification be conducted with appropriate recognition of international implications. Accordingly:

We recommend that the federal agency assigned major administrative responsibilities in this field also be empowered to deal with the complex international issues arising from weather-modification projects.