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INTERNATIONAL CONFERENCE ON CLOUD PHYSICS

Australia, September 1961

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INTRODUCTION

An International Conference on Cloud Physics was held in Australia from September 11-20, 1961, under the joint sponsorship of the Australian Academy of Science and the Commonwealth Scientific and Industrial Research Organization.

The Conference was divided into two Sessions. The first Session was held in Canberra, and was a scientific one, devoted to the presentation and review of papers concerned with the physical processes which enter into the formation of cloud and precipitation in its various forms: cloud dynamics; aerosols, condensation nuclei, and cloud microstructure; growth of cloud particles, cloud electrification and coalescence; natural freezing nuclei and snow crystals; natural freezing nuclei; ice crystal nucleation; natural rainfall; hail; artificial stimulation of rain.

The second Session was held in Sydney and consisted of a series of informal Seminars, which provided opportunities for critical discussion on the methods, instruments and techniques used in cloud physics research. It also included flight and laboratory demonstrations of the techniques used in cloud physics and cloud seeding investigations in Australia.

ADDRESS OF WELCOME

by

The Rt. Hon. The Lord Casey of Berwick, P.C., C.H., D.S.O., M.C.

This is a most representative gathering of distinguished scientists from a number of important countries of the world, and all of us here are most glad to welcome you to what will clearly be a highly interesting and important series of discussions.

Australia is a very weather-conscious country, largely by reason of the economic importance to us of our pastoral and agricultural activities, coupled with the fact that we occupy the driest of the continents, surrounded by oceans from which we get very little meteorological data. Two thirds of Australia is in what may be called the arid and semi-arid zones, which means that we must make the most we can of our relatively well watered areas.

Meteorology seems to me as a layman to have suffered, until quite recent times, from having had very little direct means of getting experimental data to go on, except in the lower earthbound reaches of the atmosphere. It has only been in the last fifteen years that this has been remedied - but certainly in recent years it has been remedied with a vengeance . In this last decade or so, new tools have become available to you one after the other. Aircraft capable of flying at great speed at up to 60,000 feet, balloons up to 100,000 feet, rockets to far greater heights, and now satellites that circle the earth in an hour or so at heights up to several hundred miles. I don't suppose any science has ever had such a sudden wealth of data-gathering tools made available to it in such a short space of time. From having had to get along for a century with a knife and a spoon and a fork, you now have what I imagine are almost all the tools that you need to examine the atmosphere experimentally. It may be expected that this almost embarrassing wealth of experimental equipment will enable you to make giant strides of knowledge in the years immediately ahead.

Mark Twain in his day is credited with twitting you by saying that everyone talked a great deal about the weather but nobody did anything about it. This will not be a true charge much longer. Anyhow, I believe that Mark Twain never said this at all, but that it was said by a newspaper editor who was a friend of Mark Twain's.

Here in Australia, the driest of the continents, the possibility of artificially induced rainfall has intrigued us for some time. A lot of people in all parts of the world have cynically regarded it in something like the same way as the medieval alchemists' efforts to turn base metals into gold. By reason of unfortunate beginnings it tended to become discredited. In Australia we have not accepted this situation with equanimity, as the potential importance of being able to increase the rainfall even to a small extent in certain areas is of great consequence to us as a nation. I personally regard it as fortunate for this country that we have amongst us scientists, not only impressed with this national need, but with the skill to design experiments and the fortitude to undertake the quite arduous labours that are involved in conducting experiments in this field.

I understand that during the course of this Conference you will be told of these experiments, you will be given the facts and figures of what has been achieved, and I imagine that you will examine the evidence and give us the advantage of your opinion as to whether, in the circumstances, rain has been caused to fall in greater measure than it would naturally have done, and broadly to what extent. Your views on this will be of great value and interest to us.

The subject of artificially induced rainmaking is a very large and complex one, and I think we all realise that a few experiments, on however large a scale and however closely controlled scientifically, may not be accepted by the scientific world as final proof in all circumstances. It is for this reason that we are most interested to see similar experiments being conducted elsewhere. I welcome the news that Israel, which is also a country in great need of increased rainfall, is at present conducting a full-scale experiment in this field. We have noted with great interest also the considerable effort of the scientists of the United States of America, and are very hopeful that these will add to the knowledge that we ourselves will gradually accumulate. I notice also that Japan and some other countries are making significant experiments in connection with artificial rainmaking. During the course of this Conference these and other experiments of a related kind will be recounted. This, I think, will be the first occasion on which a wide variety of data related to this important subject will have been brought together.

We are all delighted that Professor Bergeron has been able to come to this Conference. In my discussions with scientific men in Australia Professor Bergeron's name is repeatedly mentioned, by reason of his being the author of the theory of freezing nuclei as the cause of rain from clouds. This theory is basic to much of the work at present going on in Australia, and I am sure that it must be of particular satisfaction to Professor Bergeron to realise that his ideas, put forward so many years ago, have not only been amply confirmed in many experiments but now form the basis of many of the advances in cloud physics.

May I also give a special welcome to Dr. Shaefer, who, working with Dr. Langmuir, was, I believe, the first to use dry ice to stimulate rainfall from clouds.

I would like now to refer briefly to the idea put forward by Dr. Bowen some years ago that there are regular days in the year on which the rainfall over large areas of the globe is appreciably above the average. During this Conference I understand that evidence will be placed before you, not only from Australia but from other parts of the world, on this aspect of this theory. I believe that there is now beginning to be some acceptance of this idea, although perhaps not universally as yet. It is certainly very heartening to note the interest taken in this aspect of meteorology, and particularly in the wide array of observations of freezing nuclei at ground level which are going on today throughout the world. I understand that in the United States, in South Africa, New Zealand, India, Japan, France, and in many other places, the techniques for the measurement of freezing nuclei at ground level which had its origins in Australia are now being used in an attempt to assess the facts of this problem.

Dr. Bowen has attempted to explain these days of high rainfall by his meteoric (or perhaps I should say "meteoritic") dust theory - the incidence of high rainfall occurring thirty days after those days on which the earth passed through streams of meteoric dust.

The meteoric dust theory of this phenomenon is obviously difficult to establish, and I imagine that it is by no means as yet as widely accepted. The task, even with the facilities for observation which are now available to meteorologists, of observing freezing nuclei at all levels in the atmosphere is indeed great, particularly as this needs to be done on a regular day-to-day basis. It is obvious that every opportunity must be taken. One of these is the opportunity of equipping the very high flying American U2 aircraft with nuclei trapping filters so that data may be gained as to whether such nuclei do, in fact, exist at high levels in the atmosphere.

I will look forward with very great interest to hearing the results of the debate on these subjects which you are obviously going to have. It will be for you to accept or to express philosophic doubts about these matters, and your reasons for so doing. Alternatively there is open to you the possibility of other and more acceptable theories.

In these matters in which Australia is greatly interested I would hope that you would regard yourselves as the world jury and let us have the benefit of your conclusions and your views.

Now, before I finish, let me add a layman's high appreciation of the latest meteorological tool which the genius of the United States has given the world - the Tiros world-circulating satellite - which photographs large areas of the world and its cloud cover. We now have possibilities to see what really is going on on a global basis and divorced from our limited earth-bound view of meteorological happenings. The possibilities

inherent in the Tiros photographs from the Australian meteorological viewpoint are most exciting.

I would remind you of the geographical situation of this continent. Our land mass is equal to that of the United States. To the west and to the south - where our weather originates - are very large ocean areas. From these areas we can get only the most limited of meteorological observations - a few ships, a few small islands at great distance from one another, are our only reporting points. Over the last ten years I have personally had considerable interest in the buildup of meteorological observing on the Antarctic Continent. This is beginning to give us a somewhat clearer picture of the meteorology of the Southern Ocean but even so the Antarctic Continent is several thousand miles remote from the shores of Australia itself.

This will remind you immediately of the great significance that the Tiros cloud cover observations will have to Australian meteorologists. If as a result we can detect the storm centres of the Roaring Forties and follow the progress of these storms through the Indian Ocean and the great Southern Ocean, this will do something of great consequence to our ability to forecast our Australian weather.

One does not have to be a meteorologist to realise the great importance of the things that I have been trying to discuss. I realise that they, and a great deal more, are matters of everyday concern to you. Even a layman can catch fire with anticipation of what you are going to give the world in the near future.

I hope you will not believe that, by concentrating on a few subjects of particular interest to Australia, I think that these are the only things that you are likely to discuss at this Conference. I realise very well that you are here to discuss the present state of the science of cloud physics in the broad, and that the matters that I have mentioned represent only part of your agenda.

SOME IMPLICATIONS OF PROGRESS IN THE ATMOSPHERIC SCIENCES

Opening Address

by

Thomas F. Malone, President
American Meteorological Society

It is a distinct privilege to fulfil my pleasant duty of bringing to the participants and guests of this International Conference the greetings of the American Meteorological Society and our best wishes for a spirited and fruitful exchange of scientific ideas and research results. To our hosts in the Australian Academy of Science and the Commonwealth Scientific and Industrial Research Organization, let me say how happy and honoured we are to be your guests. The warmth of your hospitality has already become manifest in many delightful ways and all of us are looking forward with keen anticipation to the next two weeks in this wonderful country.

The fact that so many distinguished atmospheric scientists from so many parts of the world have chosen this interlude to set aside their work and gather in Canberra is a measure of the high esteem with which Australian scientific research is regarded all over the world. We acknowledge its excellence, we commend you for your generosity in sharing your research results with the world scientific community, and we express the hope and conviction that pursuit of excellence and dedication to human welfare will continue to be the hallmarks by which your scientific and professional work in meteorology will be recognised throughout the world.

It would, I suppose, be appropriate for me in these opening remarks to set the stage for the detailed scientific papers which will follow by attempting to summarize some of the advances that have been made in cloud physics during the past few years and pose some of the many questions that remain yet to be answered before we solve satisfactorily the age-old question, what makes it rain? But in another sense, this would be like carrying the proverbial coals to Newcastle. Recent and comprehensive reviews of progress in this exciting and rapidly developing aspect of the atmospheric sciences are available, known to all of you, and any sage thoughts I might express today would likely be outmoded by the time this conference is adjourned.

Moreover, although recitation of the litany of unsolved problems in meteorology is a useful and salutary exercise, we must not allow it to become such a fetish that the real progress we are making becomes obscured. The rapidly expanding arc of scientific progress and the almost explosive advances in technological dexterity, characteristic of the present time, have interacted with developments in our own field and seem to presage acquisition and utilisation of knowledge about our atmospheric environment that may well enable us, during the next decade or so, to achieve capabilities that we can only dimly foresee at the present time.

It is with some of the implications of this accelerating progress, not only in your speciality, but in all of meteorology that I would like to invite your attention this morning. That this is a topic of proper concern for an international gathering of scientists can, I submit, be cogently argued in the affirmative. The case has already been made in several quarters and, in particular, the issues were succinctly set forth a little over a year ago by the Committee on Science in the Promotion of Human Welfare of the American Association for the Advancement of Science in these words:

"For nearly two decades scientists have viewed with growing concern the troublesome events that have been evoked by the interaction between scientific progress and public affairs. With each increment of power, the problem of directing its use toward beneficial ends becomes more complex, the consequences of failure more disastrous, and the time for decisions more brief.

The problem is not new, either in the history of human affairs or of science. What is without past parallel is its urgency".

We in the atmospheric sciences have rather special responsibilities in this regard for two reasons: the first is the immediate, vital, and profound part played by weather and climate in the lives and affairs of all men, and the second is the special international character of an atmosphere that is constantly in restless motion, crossing and recrossing national boundaries, defying isolation and analysis, in more than a limited sense, of one part separated from the entire system.

Within this context, let us consider first the progress in the atmospheric sciences; secondly, implications of this progress and finally, some measures toward which we might prudently direct our attention while there is still ample time to act in a deliberate and thoughtful manner.

To appraise properly what is going on about us today let us recall that nearly two thousand years elapsed between the first stirrings of a scientific attitude toward meteorology, initiated by the perspicacious Greeks, and the beginning of the quantitative era marked by the invention of the barometer, thermometer and hygrometer. These developments inaugurated an exciting period during which the footprints of some of the great scientists of the seventeenth and eighteenth century were left in meteorology as they became intrigued with the explanation of physical phenomena in the atmosphere from discrete single point measurements. The depiction of successive states of the atmosphere by arraying observations on a synoptic basis a little over a hundred years ago, followed shortly by the perfection of telegraphic and radio communications links, resulted in a temporary diversion of effort from the question, Why is the weather? to What will be the weather? Nearly a century of possibly premature preoccupation with weather prediction was not as great a blow to scientific progress as is sometimes believed, because the economic value of simple extrapolation of weather systems was sufficient to warrant establishing observational networks essential for the scientific study of hemispheric circulation patterns. During the past decade or so we have witnessed a renaissance of interest in the scientific problems of the atmosphere, so pronounced that early in 1958 such a distinguished and impartial spokesman for science as the President of the U. S. National Academy of Sciences was led to remark that "... an exciting opportunity for major progress in meteorological science exists at the present time... We are potentially on the threshold of a new era in understanding our environment."

What are some of the signs that mark the present period as one in which significant and substantial progress is possible? Why is it that it is appropriate that we cease referring to meteorology as a "neglected area" of science, turn our eyes toward the future and start thinking constructively and optimistically about what we must do to insure further progress and about some of the implications of this progress? Let us answer these questions by analyzing the problem we are attempting to solve and noting some developments in the several facets of this problem.

We know the atmosphere to be a thermally active hydrodynamic system, compressible, non-homogeneous and viscous, non-linearly responsive to a complex of internal and cosmic forces, interacting with the earth and ocean at its lower boundary and with the solar atmosphere at its outermost reaches. This gaseous envelope surrounding the earth is driven by sources and sinks of energy that have their cause in the variable reaction of the atmosphere to radiant energy, the latent heat involved in phase charges of water substance, photo-chemical processes at upper levels and the transfer of sensible heat to and from the underlying earth and oceans.

If we accept the postulate, as I believe we must, that the acquisition of scientific knowledge, the utilization of that knowledge, and the consequences for mankind of that use are all inextricably linked, the meteorological problem can be analyzed as the fourfold one of (1) describing this complex hydrodynamic system, (2) understanding the physical processes which are responsible for its behaviour, (3) predicting future states of the system, and, ultimately, (4) controlling such of those physical processes, as may be practicable, to influence future states in a beneficial manner.

Now, within this four-membered framework of description, understanding, prediction and control, let us assess progress, recognizing that, while logic suggests a certain sequential order, the interaction of one member with another is undeniable and progress of varying degree may take place simultaneously in all four members.

With appropriate regard to the dimensions of the system we are trying to describe, our progress in description has been remarkable. In the short interval of three decades we have developed the capability for measurements of pressure, temperature and humidity in the troposphere in sufficient detail to lay the groundwork for an explanation of the major features of the air motion in this layer. Automatic weather reporting stations are beginning to demonstrate a capability for bringing under surveillance atmospheric conditions at the surface over relatively inaccessible parts of the globe. Radar and airborne probes are rapidly making possible systematic analysis of atmospheric phenomena on the mesoscale, and intricate sensing systems mounted on airborne platforms are enabling us at long last to measure the physical parameters of cloud particulates and dynamics or even the detailed structure of an entire hurricane.

Within the past decade and a half, systematic exploration of the mesosphere, ionosphere and exosphere by means of rockets and satellites has been vigorously advanced with the result that we are beginning to form a broad picture of the structure of the upper atmosphere and of the photochemical and electrical phenomena as well as of the energetics which influence the behaviour of the atmosphere at these levels, and the manner in which these regions may link together solar and terrestrial phenomena. Surely, two decades from now, the successful development of meteorological satellites will be rated as a giant step forward in describing the atmosphere, regardless of how historical perspective evaluates its contribution to the daily forecast problem of the early sixties. In addition, more sophisticated methods of measurement employing indirect probes and isotopic tracer techniques have demonstrated effectiveness in contributing to the task of description. I do not mean to imply that we have mastered the problem of description. I do mean to state, categorically, that in view of the rate at which technological technique is progressing, this mastery is almost within our grasp - not in the sense that we can account in detail for each element in the broad spectrum of atmospheric phenomena, but in the sense that we will be able to describe the characteristics of the distributions of these elements in a physically meaningful fashion.

One could easily devote an entire series of lectures to progress in understanding and still not do justice to the topic. I would simply cite two instances that seem to me to be of especial significance and, moreover, representative of the sense of forward motion that characterizes the entire field. The first is the superb work that has been done in closing the gap between the theory of large-scale atmospheric motion as we conceive it in the equations of hydrodynamics, state and continuity and in the First Law of Thermodynamics, and the reality of nature as we perceive it in our synoptic representation of the general circulation. I suspect we do not yet fully appreciate the increment to our analytical power which has been afforded by the successful blending of physical and mathematical insight, the high-speed digital computer, and the rotating dishpan. Think, however, of what it means to be able to start with some physically tenable initial conditions and be able to generate circulation patterns which reproduce, with considerable fidelity, the modes of motion occurring in the atmosphere. Already the extension of this kind of mathematical modelling to the component systems of circulation is beginning to bear fruit.

The second instance of progress which, in historical perspective, is literally explosive, may be found in your own field of cloud physics. Significantly enough. it was the hint of some possibility of control, in 1946, that triggered an intellectual and observational effort through which the central problems have been identified, clarified and attacked with a vigor probably equalled only by research work on problems of the upper atmosphere. The basic problem of describing the physics of the condensation process, involving the phase transitions from vapor to liquid, assisted by the presence of condensation nuclei, now appears to be amenable to solution. The processes of cloud droplet growth in adiabatically cooling updrafts have been synthesized and treated numerically. The principal processes by which several millions of cloud particles units to form a single precipitation particle within stringent time limitations are becoming increasingly better known and have been treated quantitatively. The dynamics of clouds and the role of the electrical field are beginning to yield to analysis. Admittedly, much remains yet to be done, but my point again is that the gap between theory and nature is closing at an accelerating pace.

It is much more than merely a gracious gesture to acknowledge the outstanding contributions to knowledge of cloud physics by the Radiophysics Laboratory of C.S.I.R.O. Its scientists have shed new and important light on nucleation theory, on coalescence processes, and on the role of condensation nuclei in governing precipitation processes. They have developed a host of instrumental techniques now used all around the world and have significantly advanced the technology of silver iodide seeding of sub- cooled clouds. Probably no other single research group in the world has approached this Laboratory in total productivity in the area of cloud physics.

With respect to prediction, I would venture to say that, despite the popular pasttime of directing raillery towards the daily weather forecast, no other field of comparable difficulty in science has progressed as far as we have toward developing a retional, systematic approach to prediction, firmly based on description and understanding. The remarkable success of numerical prediction techniques in replacing empiricism and synoptic experience in daily forecasting for the middle troposphere in extratropical areas is really a milestone of progress towards the goal of true scientific prediction. The possibilities of supplementing physical prediction techniques with some of the newer statistical methods requiring high speed computers, where limitations of data or understanding preclude a purely physical appraach, offer attractive opportunities to enhance the usefulness of forecasts in decision-making processes in which future weather conditions constitute one element of the decision matrix.

And what may one say of progress toward control? Several rather significant things, I believe. First, we know that clouds can be modified. Certain kinds of supercooled fog can be dissipated over areas large enough to be of practical importance. There is general agreement that under certain specialized conditions, rainfall can be suggested artificially and the evidence that this increase may be of practical importance in certain areas must be reckoned with. Evaporation processes can be altered to some extent over water surfaces, and recent results suggest the possibility of artificially interfering with transpiration processes over vegetation-covered land. The significance of such interruptions in one phase of the hydrologic cycle is really not yet known, but the fact that it can be done is more than merely intriguing. It has been demonstrated that, by artificially modifying the space charge in the lower part of the atmosphere, the electrical properties of cumulus clouds can be altered. The possibility of triggering the release of energy stored in the upper atmosphere in the form of atomic oxygen and free radicals by the use of suitable catalysts has been recognized, although the consequences are still a matter of conjecture. We have come to appreciate the serious results of altering our atmosphere by polluting it with waste from industry, homes and transportation vehicles. We ponder the as-yet-undetermined effects on meteorological processes of using the atmosphere as the site for testing nuclear weapons. We are beginning to conduct some highly preliminary experiments in tinkering with the energetics of incipient hurricanes and tornadoes. Speculations have been offered on the possible consequences of filling in or deepening the Straits of Gibraltar, damming the Bering Strait and pumping water from the Arctic Ocean into the Pacific to stimulate warming currents from the Atlantic into the Arctic region, opening up passes in the Sierra Nevadas to permit intrusion of moist air into the Nevada desert, or creating an ice crystal fog over the Arctic to interfere with the radiation balance. Fantastic? Yes, but how many of us present here today had the wisdom and the vision three decades ago to anticipate the developments in nuclear physics or the exploration of space that have taken place within our lifetime. Who among us can say that the probability of breath-taking advances in meteorology during the next three decades is appreciably smaller now than similar probabilities in these two fields were thirty years ago? The scientific problem is probably no more - nor less - difficult, the intrinsic scientific challenge is certainly as great, and the direct benefits to mankind are probably even much greater, provided that we as scientists are sufficiently articulate and forceful in arguing for humanitarian objectives as our ultimate goal.

If some may take the position that this brief assessment of progress views affairs through glasses of an unjustifiably rosy hue, rest assured that I am painfully conscious of inadequacies in our observations, gaps in our knowledge, frustrations in research directed at closing those gaps, deficiencies in our forecasts, and hazards in speaking

of weather control and climate modification before we have carried out the meteorological equivalent of the theoretical work of Einstein or the experimental work of Fermi in nuclear physics.

But the implications seem to be clear. Progress over the past few decades has brought meteorology to a "take-off" point from which it is likely that significant, and just possibly spectacular, advances may take place during the next decade or two. There is a good reason to believe that concerted action to extend our description and to augment our understanding will lead to a substantial improvement in our ability to predict - with consequences of immense practical significance to all the peoples of the world. There is a small chance that we may be able to learn how to control local weather effectively or even modify climate. Balanced against this small chance are consequences of literally tremendous significance - for good or, perhaps, for evil. Fortunately, we don't know if this small probability of achieving these great consequences can be realized. A formidable scientific problem must first be solved - but no more formidable than problems that have been solved by our generation of scientists in other fields. We do know now that we have at hand theoretical tools such as physically realistic mathematical models, and technological tools such as electronic computers or hydrodynamic models, with which we can analyse the physical consequences of artificial perturbations and thereby design meaningful experiments which might be conducted in nature with the increasing amounts of energy that are likely to be at our command in the years to come. We do know that, if we proceed in a systematic manner, regardless of the ultimate result with respect to weather control, there are certain to be rich and rewarding by-products in such important areas as water resources, air pollution, agricultural productivity and others that only time will identify. More importantly, we do know that our best chance of success in achieving practical results is to address ourselves primarily to the task which is the principal motivation of the real scientist - to understand the beauty, the reasonableness, the order of nature.

For, as Warren Weaver has pointed out, "the whole history of science shows most impressively that the scientists who are motivated by curiosity, by a driving desire to know, are usually the ones who make the deepest, the most imaginative, and the most revolutionary discoveries - and those which, often as a result of the work of applied scientists, eventually turn out to be the most practical". ("A Great Age for Science" in Goals for Americans.)

Finally, we do know that, just as the atmosphere is an international entity, so must the effort to deal with it be international. We possess a rich tradition of international co-operation in meteorology. The effective work of the World Meteorological Organization in establishing observational networks and in exchanging data, the superb collaboration on the meteorological aspects of the International Geophysical Year, the imaginative planning for the atmospheric program of the Indian Ocean Expedition, this conference on cloud physics and similar international conferences held under the auspices of the International Association of Meteorology and Applied Physics of the International Union of Geodesy and Geophysics are all cases in point. I believe, however, that the time has arrived to strengthen and unify these efforts by seeking to establish within the framework of the International Council of Scientific Unions a committee which would be the meteorological equivalent of COSPAR and SCOR and to be the focal point for an international collaborative effort in atmospheric research for the scientific and academic community, complementing the function that the World Meteorological Organization provides for the national weather services and working closely with the World Meteorological Organization and IAMAP. Among its functions might be:

1. To identify specific research tasks which need to be accomplished to advance the scientific aspects of meteorology and to endorse appropriate proposals volunteered in support of this work.
2. To arrange through the several national governments adequate and stable financial support for this scientific research.
3. To encourage the establishment of centers at which high intellectual competence and major facilities would be brought together, perhaps with support from several nations, and research would proceed in close liaison with educational activities.
4. To encourage participation by existing university, private and governmental research groups and, where appropriate, to suggest that particular activities

be undertaken or expanded by these organizations.

5. To co-ordinate the many facets of the program, to arrange for the exchange of scientists among nations and to insure that any qualified scientist from any nation wishing to participate in atmospheric research has that opportunity.
6. To evaluate the progress at appropriate intervals and to recommend discontinuance or reorientation of portions of the work as best scientific judgment may dictate.

This is by no means to say that in this day of many commissions, committees and working groups, another I.C.S.U. committee is going to possess any superior attributes of wisdom or imagination. Nor can science be planned or directed too closely. Serendipity will always be important. The principal contribution of such a group might be simply to help create a favorable international climate and provide the mechanism for international collaboration in the increasingly complex research undertakings that we are encountering.

Nor is it sufficient merely to expect that simply strengthening our world effort in research will be sufficient. The fabric of scientific progress is woven from strands of education as well as research. It is not too much to expect that some of our world's great universities would recognize an opportunity to help provide scientific manpower of the superior quality and in the quantity that will be needed and enrich their own university programs dedicated to the extension of knowledge. Time and again, the environmental sciences in general, and the atmospheric sciences in particular, have been identified as examples par excellence of interdisciplinary fields with great potential for unifying and stimulating the intellectual life of a university.

Finally, we must recognize the wisdom and prudence of fostering international co-operation while the problem of weather control is a purely scientific one of uncertain outcome. Should this outcome be affirmative, a Pandora's box of scientific and political problems would be opened. Much could be gained, little would be lost by forging the links among scientists and nations that would better prepare us for the stresses and strains that we dare not exclude as a possibility. One cannot view with equanimity several unilateral crash programs to achieve weather control nor extended conferences at Geneva in attempts to resolve differences.

To a world general scientific community that sometimes expresses concern over the uses to which its knowledge has been put, to the nations of the world seeking humanitarian objectives in which they could find release for their desire to seek unity of purpose, to the people of the world who would benefit from a deeper understanding and more effective utilization of our environment, could one present a more magnificent challenge or one more likely to stir the imagination than the scientific problem of the atmosphere.

Finally, let us recall the thoughtful and thought-provoking words of Werner Heinsenberg, in his book Physics and Philosophy. He said:

"It is especially one feature of science which makes it more than anything else suited for establishing the first strong connection between different cultural traditions This is the fact that the ultimate decisions about the value of a special scientific work, about what is correct or wrong in the work, do not depend on any human authority. It may sometimes take many years before one knows the solution of a problem, before one can distinguish between truth and error; but finally the questions will be decided, and the decisions are made not by any group of scientists but by nature itself".

Strengthening a few small bridgeheads among "different cultural traditions" and thereby assisting man to learn how to control his own destiny, might, in the last analysis, be more important than learning how to control man's environment.

First Session : Canberra

I. CLOUD DYNAMICS

CLOUD BANDS IN THE EARTH'S ATMOSPHERE AS REVEALED BY THE TIROS SATELLITES AND THE MECHANISM OF THEIR FORMATION

by

P. Kuettner*

The Tiros weather satellites have confirmed the wide spread existence of cloud bands in the Earth's atmosphere suspected from earlier ground aircraft observations; but, they have also revealed that, in addition to the well-known convective scale, "cloud streets", impressive bands occur on the meso and synoptic scales.

In satellite pictures the counterparts to convective cloud streets oriented in the flow direction are the wave clouds in stable layers, oriented normal to the flow direction. As seen from orbital heights without sufficient resolution, the two types of bands look quite similar. The Tiros' narrow angle TV cameras are capable of making this distinction in the cloud texture.

At this time it must be left open to what extent the different scales of banded structure owe their existence to a common principle. Using the convective scale as a starting point, both the available data and the theory indicate that the conservation of vorticity (vertical gradient of vertical wind shear) by the convective parcels plays a major role in enforcing bands in the direction of the flow. Only if the solenoidal field is weak or negligible, one should expect this principle to have validity in the vertical plane, but this is precisely and exclusively the case in the convective state of marginal stability considered here.

The model underlying the present theory is kept as close as possible to the one used by Rayleigh in his clear, now classical, theory of cellular convection (1916). The only addition is a unidirectional horizontal flow the speed of which varies with height. In this way, the modifications introduced by the vertical velocity profile can be clearly demonstrated.

If this profile is uniformly curved in the convective layer (as indicated by the observational material), the convective amplification reaches a maximum for the same "Rayleigh number" as in the classical case, but now there is no ambiguity regarding the ratio of the cell sides as is the case in the Rayleigh theory. Instead, an infinite extension of the cells in the flow direction with a certain spacing normal to it is the preferred mode. This is minimized by stretching the cells in the flow direction.

This type of organized convection is closely related to the "Goertler rolls" forming in boundary layers along curved walls or the "Taylor vortices" between rotating cylinders.

If the flow direction changes with height, a qualitative argument shows that vertical wind shear will cause convective bands to form but that they will line up with the direction of the flow in the ground layer.

Some speculative thoughts are given to the bands on larger than convective scales. For these, the convective processes and wind profiles in the horizontal plane have to be taken into account.

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Discussion

GIBBS: A study of Tiros cloud pictures in the Australian Bureau of Meteorology suggests that there are two fruitful sources of large scale cloud bands. One is the zone of convergence of a synoptic scale with presumably steady upward motion of which the "warm front" is a special case. Another source, particularly in tropical areas, is the spreading out of the tops of convective cloud groups.

KUETTNER: With a change in terminology we can think of convection in a horizontal plane, with air masses of different properties being mixed into one another near a frontal zone.

The trailing of cirrus is obvious in some pictures, but this can clearly be distinguished from the very compact cloud bands near closed low pressure areas.

MARSHALL: Is there general agreement that the clouds in the bands (in the pictures we were shown today) are cumulus?

KUETTNER: Yes, in the small scale bands you can see clearly that they are cumulus. The large scale bands might be of a different nature, I don't know.

PRIESTLEY: A further point of support for Dr. Kuettner's ideas lies in the atmosphere near the ground. Here the curvature of the velocity profile is largest, and the small convection elements do seem to be organized in a line down wind.

Recent photographs from satellites have shown convection cells with horizontal dimensions about thirty times the vertical compared with the laboratory results of Rayleigh which gave 3:1. Have you any explanation of this?

KUETTNER: You are talking about this new type of circular cell which occurs? I have only seen one picture of this and was immediately reminded of the theory of toroidal cells, but again the dimensions don't fit and I am not sure that these cells are still small enough that the Coriolis terms can be neglected.

THE DYNAMICS OF THUNDERSTORMS AND SQUALL LINES

by

C. E. Anderson*

For the past two summers, an observational investigation into the dynamics of thunderstorms and squall lines has been conducted by the Geophysics Research Directorate's Cloud Physics Branch at Flagstaff, Arizona. Of the independent variables which are necessary to follow the dynamical development of a thunderstorm, we have depended primarily on measurements of the vertical motion field as revealed by the moving cloud boundaries and the micro pressure field using a tri-partite array of micro barovariographs. This paper discusses our observational findings regarding the typical pressure signatures made by moving thunderstorms and squall lines and how these are related to our previous conclusions about the dynamics as obtained from the analysis of stereophotographs.

Discussion

BYERS: What was the microbarograph? It looked to me like an ordinary barograph. How does the microbarovariograph work?

CUNNINGHAM: The microbarogaph record shown was made by the ordinary sensitive barograph. The microbarovariograph is a pressure measuring microphone connected between the ambient air and a tank with a slow leak. The sensitivity is of the order 10^{-3} millibars, and the chamber leak results in recording pressure variations with periods between 30 seconds and 30 minutes. The sensitivity is small for periods shorter or longer than this.

TELFORD: What is the magnitude of the particle velocities associated with the pressure wave?

CUNNINGHAM: The particle motion discussed in the paper is associated with

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the pulsating movement of the cumulus tops. Horizontal particle motions are associated with the outflow of cold air, which may also generate these wavelike motions. The association of the measured cloud top velocities with the pressure pulses is by inference only.

DETAILS OF CUMULUS CLOUDS EDGE STRUCTURE RELATED TO THE CLOUD LIFE CYCLE

by

M. Cunningham*

In a mountainous area like Flagstaff, Arizona one may watch cumulus clouds "boil" all morning. Many obtain about the same maximum height, then suddenly one looking at first little different than the rest gains a height appreciably above the others and often continues to grow into a dominating cumulonimbus.

Along convergence lines, frontal, prefrontal or "dry lines" similar activity can be observed. In the unstable portions of the trade wind regions only one cloud in thousands suddenly attains cumulonimbus size all the others are minor clouds.

There have been numerous theoretical and semi-theoretical studies of the properties of clouds. Some suggest reasons for the above behaviour. One must consider such items as: the size of the thermal source; the frequency and size of the thermals rising into the cloud; the past history of the air into which the cloud is growing, its wind, thermal and moisture structure; the protection afforded the rising columns by old cloud; and of course the heat of fusion if the freezing process has started.

If one is interested in modifying clouds and checking results statistically or by physical measurement it is exceedingly awkward that nature supplies a laboratory with a cloud population where one cloud in a thousand "goes off" by itself.

The theoretical approach is in need of a stimulus from more detailed cloud studies where the observations are related to the age of the cloud probed and to the history of the cloud in relation to its neighbors and to the whole cloud population.

If cloud modification experiments are to be conducted along experimental lines cloud growth must be predictable.

Measurements have become more diverse and detailed in the last few years. Instruments are described for measuring temperature and humidity in great detail. Water content and drop size in a bit less.

Observations of the fine structure of temperature, humidity and water content in cloud turrets at various stages in their life are shown for semi-tropical area in a growing cloud group over the San Francisco Peaks, Flagstaff Arizona. The manner and rate at which the cloud decays by entrainment is illustrated by the rapid changes of temperature and humidity structure at the cloud edge. The life cycles of the cumulus clouds probed were monitored by airborne cameras and also by airborne radar in the second location. The analysis of three cloud passes of the mountain clouds shows the following: Sharp boundaries were found at the upper edge at a growing cloud bubble at the early stage of cloud building; warm dry air surrounding the cloud in a narrow ring as the cloud group broadens and consolidates in a stable layer; and warm turbulent air in the downward overshooting of the descending turrets in the small hail stage. The analysis of the data taken during a pass through clouds in the small hail stage also shows the short life cycle of cloud turrets ($\sim$ 10 min) and how rapidly small hail forms in these turrets ($\sim$5 min). The disintegration of the cloud boundaries by entrainment in the descending cloud is also illustrated.

The mountain observations were carried out within the framework of Project High Cue. The eventual combination of cloud dimension changes from ground camera data, and precipitation growth from ground radar data, along with the aircraft observations taken in the summer of 1961 will provide the needed coordinated data to more completely describe the rapid growth and decay at the cumulus and associated precipitation.

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Discussion

McDONALD: 1) Couldn't one explain the warmer and drier regions found just outside several clouds as having been produced by adiabatic heating in compensatory downcurrents in the environment?

2) One slide showed what appeared to be a span of several thousands of feet of "cloud" in which the relative humidity reached 100% at only one narrow region. How could cloud drops persist in the presence of a R.H. less than 100%?

CUNNINGHAM: The results suggest that at least part of the compensating down-draught can occur very close to the cloud.

In answer to the second question I believe that the part of the cloud where R.H. is less than 100% is due to ice crystals which have just grown large enough to have an appreciable lifetime.

NEIBURGER: The relative humidity is with respect to liquid water?

CUNNINGHAM: That is correct.

MURGATROYD: When you have plotted R.H. from refractometer readings you have had to assume values of temperature and pressure. Did you obtain a baseline for these measurements outside the cloud, or did you assume that the maximum R.H. in the cloud was 100% and then scale down in some way for the lower values?

CUNNINGHAM: We did assume that the peak value in the cloud was at 100%.

MURGATROYD: Could icing or other intake of solid or liquid water have affected the performance of the refractometer?

CUNNINGHAM: We have attempted to pull air in at 90° and as far as we can see liquid water is not affecting any of the readings.

PRIESTLEY: Surely these measurements are not representative of cloud in general, since they all seem to be cooler than the air around them.

CUNNINGHAM: The first cloud I showed had temperatures higher than the surroundings. You usually do not get this near cloud tops unless you penetrate a freshly rising bubble or turret. It is important to note that most cloud probing is biased towards sampling "old" cloud because of the rapid development and decay of cloud turrets. The initial rapid growth stage is usually sampled by accident. These measurements however are probably representative of clouds in general as the mass of decaying cloud in existence at one moment in time is presumably greater than the mass of building cloud.

HOSLER: Relevant to the Chairman's question, I believe we should expect these penetrations to reveal negative temperature departures from the environment due to the fact that these were penetrations into stable air and probably represented over-shooting where the upward motion had not yet been reversed.

CUNNINGHAM: Yes, however, there was no inversion of temperature; it was only slightly stable at the level shown for the last pass. The ambient lapse rate for the first passes was rather steep.

CONVECTIVE COLUMNS, CUMULUS AND TORNADOS STUDIED WITH THE METEOTRON

by

H. Dessens and J. Dessens *

The Meteotron consists of 100 burners distributed in an area of 125 x 125 m, but it is possible to fire only some of the burners. The consumption of each burner is about 12 litre/minute, so that the calorific power is of the order of 7.10^3 for one burner and 7.10^5 KW for all the 100 burners.

The ascending column is visible owing to the carbon black produced by the

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incomplete combustion of the fuel. It seems, after measurements, that the column escapes partly from the horizontal wind field.

When the calorific power of the Meteotron is sufficient in respect to the conditions near the ground (relative humidity, wind velocity, thermic lapse rate), a cumulus grows in the upper part of the ascending column.

The latent heat of condensation released in an artificial cumulus is of the same order as the heat of combustion at the ground. A coupling takes place between these two sources of energy, Meteotron and Cumulus, the column being the link. The result of this coupling is the periodical growth of cumulus with formation of a cumulus street from the Meteotron. The process is called "coupled convection".

The most striking phenomenon observed during the first artificial convection experiments was the formation of tornados with a "funnel" along the axis of the vortex. This "funnel" or, better, this "tube" is not in very straight line and fluctuates in diameter and brightness. A simple explanation of the appearance of the vortex axis (sometimes white, sometimes black) is that water droplets were suddenly condensed along the axis. The "tube" of some artificial tornados was exactly similar to alto-cumulus lenticularis. I think that the droplets were continuously extracted from the "tube" by the centrifugal force and then evaporated: the curvilinear axis is occupied by new droplets in a similar manner to the upstream edge of altocumulus lenticularis.

In conclusion, few experiments have been made with the Meteotron in 1961, but these experiments prove the interest of such equipment for studies on the physics of the atmosphere.

Discussion

BYERS: What were the lighting conditions in the photographs of the black funnel as compared with the white one? Unless the black one was back-lighted, the blackness could not be accounted for by large drops, as suggested by the speaker.

DESSENS: There were no differences in the lighting in the two cases. In the case of the white funnel, pictures were taken from both sides and they both appeared white.

The black tornado was with a strong wind and became detached from the meteotron, while the white one was in a feeble wind and remained attached. The white colour was almost certainly due to concensed cloud; for the black I'm not certain.

SULLIVAN: In the photograph, two of the oil burners where not alight and an oil mist was being discharged. As it was from this area that the tornado was being generated, could the white appearance have been caused by oil droplets?

DESSENS: It is possible, but I don't see why droplets of oil could be lifted to 200 metres.

SCHAEFER: Dr. Dessens is to be congratulated for producing artificial tornados under conditions which can be controlled. The Forest Service of the U.S.A. is greatly interested in a better understanding of such effects, since fire whirls or "devils" or tornados cause severe problems in fire control.

Cases are known where low-level fire whirls of at least a mile in diameter and vertical columns of 2,000 feet of visible fire have occurred. One of my students is presently producing fire whirls in the laboratory under exact control. I am sure he will be greatly interested in these very fascinating effects.

NAQVI: About two years back we had a big fire in Karachi. The humidity had been falling since the morning and the fire started at the time in the afternoon when the humidity had reached the lowest value. The fire continued for more than an hour and developed a big plume like the one shown for the Meteotron. Then a sea breeze set in and was recorded at two observatories 5 to 9 miles from the coast, and the humidity increased considerably. It was evident that the effects of the convection cell were noticeable at considerable distances. Have these been measured in the lower layers surrounding the Meteotron?

FAIR WEATHER CUMULI : THEIR ENVIRONMENT AND ACTIVITY

by

J. Warner and J.W. Telford*

The fair weather cumulus has been the cloud form most subject to detailed observation because of its moderate physical size and lifetime and because its growth and decay are uncomplicated by the emergence of ice or even of rain. The main problem it poses is the mechanism of its growth and mixing with its environment.

In order to throw light on the various models of cumulus development that have been put forward an aircraft has been carefully instrumented for measurement both in clear air and in cloud and a series of observations commenced. The following quantities are measured and recorded simultaneously: dry and wet bulb temperatures, total water content, liquid water content and vertical air velocity. Observations are also made to enable the cloud droplet spectrum to be deduced. In addition various parameters relating to aircraft performance are recorded, such as pitot and static pressures, vertical acceleration, pitch angle and angle of attack. A response speed of the order of 0.1 second has been achieved throughout thus allowing a resolution in distance of somewhat less than ten metres. Temperature measurements are made with the aid of fine wire resistance thermometers. Total water is measured by psychrometric techniques after first heating and evaporating any liquid water present in a stream of air flowing through the instrument. Liquid water is deduced from changes in electrical resistance of a paper tape moistened by the cloud droplets. Vertical air velocity is measured by adding to the vertical velocity of the aircraft, obtained by integrating electronically its true vertical accelerations, the vertical velocity of the air relative to the aircraft obtained from a measurement of the latter's forward speed and the angle of approach of this air relative to a true horizontal datum.

Computations from the records are made to obtain the true values of temperature, velocity etc. In addition buoyancy and two properties conservative in adiabatic processes are computed. A knowledge of two conservative properties in a non-precipitating cloud and in its environment from the surface to above cloud tops enables deduction to be made regarding the mixing processes involved in the growth of that cloud. Our first observations in fair-weather cumuli have not enabled us to reach definite conclusions regarding the nature of the mixing process, but some of the details of the observations are of considerable interest.

The liquid water content is correlated very strongly, even in fine detail, with the virtual temperature or buoyancy. Areas of marked positive buoyance are relatively small and in the vicinity of cloud tops the negative buoyancy excursions may amount to 3 or 4°C. Near the cloud tops the updraughts are in general associated with positive buoyancy but there is not the same detailed correlation as occurs with liquid water, and some evidence exists of inertial effects. Small growing cumuli may have updraughts in excess of 5 m sec^{-1} of a few hundred metres across and down draughts of this magnitude have also been encountered, particularly at the cloud edge.

Discussion

MURGATROYD: I should like to congratulate the authors on their progress in aircraft instrumentation which is more advanced than any other I am familiar with. I would like, however, to query two relatively minor points.

a) In the system of deducing the vertical air motion we are led to an equation of the form:

$$W \doteqdot U\alpha + l(d\theta/dt) - \int n\,dt = U\theta$$

where W is updraught, U air speed, α vane inclination, θ pitch, n aircraft acceleration at the C.G. and l distance of the vane from the C.G. It is not clear in the system described that rate of pitch $d\theta/dt$ is being measured. Is this so and how important is

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the omission? I wondered if a vertical motion of 5 m sec^{-1} in clear air shown on one of the slides is an over-estimate on this account.

TELFORD: As you say, the rate of pitch is strictly necessary in the present system. I intend to obtain this by smoothing the pitch mathematically and then differentiating. This is possible, as we know the true pitch is in fact a relatively slowly varying parameter because of the moment of inertia of the aircraft.

Actually, the pitching rates are such that it is not usually a very large contribution to the velocity compared to the overall accuracy achievable from the system. I am confident the 5 m sec^{-1} in clear air is real to the accuracy of the system.

MURGATROYD: b) Since the liquid water content is measured directly and also a wet bulb reading is taken, I am not clear what is the function of the heated intake instrument. In order to get good results from this type of instrument, it is essential to have both isokinetic sampling and complete evaporation. If it is used to estimate liquid water content, it also requires a very accurate temperature measurement and the cloud has to be assumed saturated.

WARNER: The instrument is used for measuring total water content which is one of the two conservation properties we wish to use for our analysis of mixing processes. This quantity could certainly be derived from the separate measurements of liquid water content and vapour mixing ratio. There has, however, been some criticism of our values of liquid water and I prefer to use the one instrument. I agree that it would be most difficult to determine liquid water from a measurement of total water and temperature, but we have not contemplated doing this.

KENNEDY: Aircraft flown on what the pilot believed to be straight and level paths can in fact have vertical velocities of ± 30 ft/second. This is a property of the stability of the aircraft itself and is not due to vertical velocity components of the surrounding atmosphere.

These aircraft motions have been measured by ground-based cameras.

TELFORD: I would like to stress that aircraft stability does not affect our measurement. We have measured true vertical air velocity. However, I quite agree that large velocities can quite easily occur in an aircraft whose pilot has been requested to fly straight and level to the best of his ability.

RADOK: Is it possible that oscillations of the nose boom could produce the appearance of vertical air currents?

TELFORD: No periodicity is visible in the trace of angle of attack.

The rigidity of the probe is such that under the g loadings experienced, the deflection should be limited to 0.1". Certainly no oscillation is visible, though an accurate measurement of the reference datum of the probe and the pitch reference is extremely difficult.

CUNNINGHAM: Did I catch correctly the life-time of the cloud studied is 15 min? Was this life-time of the whole cloud or was the cloud made of a series of turrets?

WARNER: The life of the cloud was some tens of minutes and it was studied for about 25 minutes, during which time it grew and then commenced to evaporate. Our measurements relate to the whole cloud which was only 4,000 to 5,000 ft deep.

THERMALS IN UNSTABLE SURROUNDINGS

by

J.S. Turner*

In recent years various "bubble" theories have been replacing the steady state jet models of the entrainment of dry air into cumulus clouds. These seem more satisfactory since they take into account the development in time of the buoyant elements and can allow for mixing over the top as well as at the sides of the cloud. Early speculations about the form of these elements, and about their possible erosion or growth, have been resolved by laboratory experiments which have shown that a typical thermal in still, neutral surroundings spreads out along a cone as it rises and mixes with its environment. This result is in agreement with a simple dimensional theory.

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Most of such experiments, and therefore the only quantitative data on thermals, have so far been concerned with an element of constant total buoyancy rising in an adiabatic atmosphere. In a growing cloud, however, because of the release of latent heat, thermals are often effectively in an unstable density gradient, and in this paper it is shown how laboratory experiments may be used to study this important case directly. Again dimensional arguments predict that the spread should be linear under a wide variety of unstable conditions, but nothing can be said without experiment about the rate of spread.

An increase of total buoyancy in an element with time has been achieved in a laboratory tank of liquid using the chemical release of small gas bubbles, which change the mean density of the fluid moving with them. It has been found possible in this way to produce thermals having a constant upward acceleration, which corresponds to a constant density difference between the element and its surroundings, and elements with constant velocity have also been studied. The spread in both these cases is linear, in agreement with theory, and a little smaller than that for a thermal of constant total buoyancy.

Once it has been established that elements in unstable surroundings also spread linearly at nearly the same angle as those in a neutral environment, the calculation of the proportional entrainment rate per unit height becomes especially simple. This quantity is inversely proportional to the size of the element, (as previously suggested using other models) but the constant of proportionality can now be specified within narrow limits using our laboratory results. The entrainment rates found in this way 3-4 times greater than those predicted using plume or jet models, and therefore in better agreement with the available cloud measurements. The calculation of temperature and liquid water variations in a rising element may be carried out using purely continuity arguments, when its initial size and the properties of the environment are specified.

This extension of the thermal model to several unstable cases is a logical development of previous ideas, but I believe that a more accurate description of a convective cloud must combine features from both the plume and the bubble models. Mixing at the sides and especially at the top of a cloud, and also the continuing supply of buoyant air from below cloudbase should all be considered together.

Discussion

HOSLER: Recent experiments we have made indicate very large values for entrainment of air at the tops of cumulus clouds. Measurements of concentration of Sr^{90} in rain water during convective showers show large fluctuations in concentration during successive stages of the shower. Characteristically, the first rain from a shower has relatively little Sr^{90} in it. Counts show perhaps one or two dpm/liter of water. As the storm develops and towers penetrate to higher elevations, the count of activity increases, presumably due to entrainment of air at higher elevations and inclusion of Sr^{90} bearing nuclei in the condensation process. On the occasions when the cumulus towers have reached the tropopause, sudden large increases in Sr^{90} are observed. These increases have been as large as a factor of 70 in cases where the shower occurred under the jet stream where presumably mixing between the troposphere and stratosphere occurs between the leaves of the tropopause. We are inclined to interpret these observations as an indication of entrainment at the cloud top.

TURNER: I agree. Certainly this is an effect which is not taken into account in a jet model.

McDONALD: In considering further elaborations of such experiments it would be very desirable to try to devise a model in which there is not only an analogue of latent heat, but also the possibility of reversing the condensation process and permitting penetrative downdrafts to develop to whatever extent they may occur in such convective processes.

TURNER: The laboratory model I have used certainly implies that in a cloud everything happens at once, that is, an intimate mixing occurs between cloudy and drier air and the final concentration of liquid water at each level is produced immediately. I believe that this small-scale mixing is more realistic than the alternative suggested by models such as that of Dr. Squires for example. This requires a parcel of drier air to maintain its identity as it mixes with the cloud and sinks due to the cooling produced by evaporation. There does not seem to be a way for such a process

to start, since initially the trapped air will be much less turbulent than the cloud, and will be torn apart before mixing into it can begin.

BOWEN: In any discussion of cloud models I am always impressed by the difficulty which is experienced trying to explain entrainment in terms of the diffusion or passage of the environment air through the sides of an existing cloud or down through the top of a cloud.

The difficulty is probably due to the fact that the model is quite an unrealistic one and not at all what is actually happening in a real cloud. A preferred model is one in which a convective cloud in building up through the environment must not be thought of as displacing all the air in its path but rather that, as it grows, it incorporates some of the air which previously occupied that space, rather like a sponge.

It seems to me that if anyone has already worked out the mathematics of a sponge they might have a complete and realistic description of how air is incorporated into a growing cloud.

PRIESTLEY: I am not convinced that you can ever get a satisfactory model for unstable stratification so long as you neglect the effect of the latter on the environment, as such; to this extent a better way of doing model studies is to use a little bit of the atmosphere itself. In this context it should be pointed out that convective elements near the ground do not obey some of the relationships which you have deduced by your dimensional argument.

TURNER: There are two important differences between the atmosphere at the ground and at cloud levels. First, there is the effect of strong shear-induced turbulence in the environment of an element, which I excluded from my discussion. Secondly, the instability in a cloud can be confined to the moving region, with the environment at rest and statically stable until latent heat is released. Near the ground everything will overturn as soon as it is disturbed.

II. AEROSOLS, CONDENSATION NUCLEI, AND CLOUD MICROSTRUCTURE

IDENTIFICATION OF AEROSOLS

by

James P. Lodge, Jr*

Despite the continuing interest in the meteorological role of aerosols, our knowledge of their chemistry is still meager. This paucity of information derives in considerable measure from disciplinary division; relatively few chemists have given consideration to the problem. In addition, the collection and analysis of submicron aerosols present formidable difficulties.

Although no single technique is adaptable to the entire range of particle sizes and compositions, methods exist for most cases in the size range above one micron, and a few substances can be identified at much smaller sizes. However, there is a serious lag between the development of new methods and their application to the problem of cloud physics. It therefore seems worthwhile to review the current state of knowledge, to suggest new applications, and to indicate areas susceptible to further development.

It can be shown that a logical progression exists from techniques which merely indicate the presence of aerosols to those yielding detailed quantitative data concerning concentrations, size spectra and composition. Many sampling techniques may be adapted to the direct determination of particle size. Collected samples may be analyzed in bulk, or as individual particles. It is even possible to cause reaction of a single submicron particle with an appropriate reagent, and to view the reaction site under the electron microscope. No methods now exist for organic particles, but such developments should be expected in a few years.

The methodology is almost within reach for complete analysis of the Aitken nuclei. The solution of this problem will depend primarily on continuing efforts to draw chemists into the study of the atmosphere.

Discussion

VITTORI: It should be noted that bulk methods, even with some size separation, can lead to very erroneous conclusions of composition as a function of size in the case of heterogeneous particles. Microscopic, and especially microchemical methods permit the measurement of both physical size and mass of specific components. During a long series of measurement of chloride particles at Mt. Cimone we noticed that it is very rare to find fine chloride particles but, in general, they are composed of the insoluble core surrounded by a thin shell of chloride.

LODGE: That is right. Microchemical methods give a spot, the size of which is related to the mass of reactant. Combination of these methods with aerodynamic ones gives a measure of both geometrical and chemical particle size.

GEORGII: I would like to ask whether the particles shown on the slides were natural aerosols or artificially produced nuclei. As Dr. Vittori already mentioned we have to deal mostly with "mixed" nuclei in the atmosphere and it seems, therefore, very difficult to draw conclusions about the size of the original particle from the size of the color-spot.

LODGE: In the interests of clarity, these slides were prepared using laboratory aerosols, but the method works in the field.

SCHAEFER: The study of organic substances which form aerosols, using the Langmuir trough and related surface chemistry techniques holds considerable promise for identifying and studying the physical and chemical properties of small particles.

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A visible "indicator oil" spread on a cleaned liquid surface is displaced whenever surface active molecules from aerosol droplets which touch the surface contact the substrate on which they can become attached. Studies of these monolayers are possible using microtechniques. There is a good chance that proper attention to this procedure will yield information on the nature of the surface active materials.

It should be remembered that even very small amounts of surface active material dissolved or suspended in cloud droplets will diffuse to the surface of the particle until it is completely covered with a monolayer. When this happens it is possible that the particle may give reactions (plating etc.) which cause effects comparable to what might be expected of a pure substance of the same diameter. Such surface deposition on aerosol particles may slow down the evaporation of water droplets (as with guanidine stearate or hexadecanol) or may lead to the formation of solid shells (microbubbles) which remain after the liquid is gone. This is a research area of great promise which thus far has received little attention.

SULLIVAN: As indicated in a paper by Shore & Katz, X-ray diffraction offers some promise for the identification of aerosols. We have found in Sydney that it can be applied to small fractions separated microscopically. Certain organics such as phthalic anhydride from point sources, have been identified and more recently we have applied X-ray diffraction for the analysis of polynuclear aromatics, both directly and as trinitrofluorenone derivatives, in air pollution extracts.

LODGE: Yes; we might also mention the relatively new point X-ray emission methods, which may be workable down to micron sizes.

GHOSE: Has any one found a method by which moist air flowing through a pipe would automatically register and stimulate a mechanism, by which the presence of sodium particles or any other nuclei would be automatically photographed or recorded by some tracer-detection mechanism?

LODGE: In the case of NaCl, flame photometry, using a PE cell is very useful. But it cannot be made quantitative for individual particles, although Woodcock has shown that it can be used to measure the bulk concentration of Na. For substances other than sodium, detection should be possible by using a hotter source, e. g. a spark, though this has not been tried.

INVESTIGATIONS ON THE TRACE-GAS AND AEROSOL WASH-OUT BY PRECIPITATION

by

Hans-Walter Georgii*

The increase of anthropogeneous and industrial aerosols and trace-gases and also the investigation of the budget of natural atmospheric trace-substances demands an interpretation of the different processes leading to a removal of trace-substances from the atmosphere.

In most cases these substances are removed by precipitation. In the first part of the lecture the different mechanisms contributing to an accumulation of trace-substances in precipitation are briefly discussed. This knowledge is important for the interpretation of our analyses of the chemical composition of individual rainfalls which were carried out at different stations. Simultaneously with these analyses the concentration of atmospheric trace gases and aerosol particles was measured. Investigations carried out simultaneously at a mountain station and a base station in the valley a small horizontal distance from each other permit conclusions on the wash-out effect through falling raindrops within the ground-layer of the atmosphere below the cloudbase. The results show the effect of wash-out in relation to the amount of rain-fall. The concentration of chemical components in rainwater shows a close relation to the rate of precipitation which can be expressed by a power law. These results are compared with those gained by other authors and with those of our own measurements at other sampling stations. They show also the effect of the type of precipitation.

From the measurements we carried out at different locations but also from the

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evaluation of the data gained by other authors we can generally distinguish three different types of curves representing the relation between chemical concentration of rainwater and the amount of precipitation. The respective type is obviously depending from the production, supply and concentration of trace substances - aerosols and gases - in the air of that area.

No relation at all between chemical concentration and amount of rainfall was found at mountain-stations where the sampling site is in most cases within the clouds when it rains. This situation was met on the Zugspitze (German Alps) at 3000 m altitude and in Hawaii above 2000 m altitude.

In order to investigate more carefully the removal of trace-substances from the atmosphere and their increase in rainwater continuous records of the concentration of different components in rainwater as well as of the atmospheric aerosol-concentration in different size-ranges were gained during the course of individual rain-falls.

Continuous records of the electrolytical conductivity of rain-water gained during the course of individual rainfalls show clearly the influence of the quantity and of the intensity of rain on the accumulation of trace-substances in rain-water. These measurements hint also at the importance of gaseous components for precipitation chemistry.

There exist two processes mainly responsible for the increase of substance in rain-drops, namely (1) the collection of aerosol-particles and the absorption of trace-gases within the cloud during cloud-formation and (2) the increase of substance through wash-out by falling rain-drops below the cloud-base. These two processes cannot be distinguished from the analyses of rain-samples at the ground.

Present investigations aim to distinguish the importance of each of these two processes for the removal of trace-substance from the atmosphere.

Discussion

KUETTNER: You speak of "trace substances"; why is this phrase applied to the aerosol?

GEORGII: The phrase is meant to include both aerosols and gaseous components such as SO_2, Cl_2, NH_4, and many others, which cannot be left out of account, since their mass concentrations in general considerably exceed those occurring in the aerosol.

BYERS: One of your slides showed that, when the rain continued into the evening, the concentrations of trace substances remained relatively constant. Does this mean that equilibrium was reached between production at the surface and removal by rain?

GEORGII: Yes, the intensity of the rain did not fluctuate, and so an equilibrium was reached. This process may perhaps be more easily studied during the winter, when steady rains are commoner.

McDONALD: In your determinations of the concentrations of trace gases in rain-water, do you find that the dissolved gases have reached diffusional equilibrium with the ambient concentration?

GEORGII: No, I don't think so.

LODGE: The droplets are more or less in equilibrium with, and hence saturated with, oxygen and nitrogen, but hardly saturated by the trace substances which are present only in parts per million.

GHOSE: In what form does the sodium chloride reach cloud levels?

GEORGII: The NaCl or other salts reach the cloud levels in particle form. It is interesting that cloud water captured close to the base shows concentrations about ten times those found at the ground; I think that this happens because aerosols and gases are transported from the ground upwards to cloud base and accumulate there; Byer's work has shown that layers of high concentration of aerosols in the free atmosphere may occur. The condensational growth of droplets in the updraught results in raindrops which are more dilute.

BYERS: There are of course variations in the Na/Cl ratio.

GEORGII: Large changes are found, depending on distance from the coast. This is due to the addition to the atmosphere of other substances containing Na or Cl.

HOSLER: In investigations of Sr^{90} concentration in rain falling from clouds produced by large-scale uplift of stable air, we find that for three winter storms examined, 100% of the variations in concentration of Sr^{90} could be explained by the change in ceiling and hence the amount of evaporation the rain underwent in falling

from the cloud to the ground. When the ceiling rose, the Sr^{90} concentration rose. When the ceiling fell, so did the Sr^{90} concentration. For ceiling variations of 2000 feet to 400 feet the concentrations usually changed by a factor of about 5.

GEORGII: Probably in the ground layers there would be little Sr^{90}, and this would make it easy to study the evaporation process in this way.

THE MICROSTRUCTURE OF WARM CLOUDS AND THE DEVELOPMENT OF COALESCENCE RAINFALL IN ENGLAND

by

R. J. Murgatroyd*

For some years the Meteorological Research Flight has been investigating the importance of the coalescence mechanism in producing rainfall in Southern England. A series of flight measurements has been made of the particle sizes at different stages in warm cumuliform and stratiform clouds. Cloud droplet populations have been studied using the magnesium oxide technique and precipitation particles ($> 100\mu$ diameter) by their impacts on thin metal foil. Water contents have been measured by means of a hot wire instrument and by summation of the droplet counts. Chloride particles with masses of 10^{-8} to 10^{-13} gms and Aitken nuclei have also been sampled below the clouds using the Liesegang technique and a Pollak type counter respectively. The principal results from these flights will be summarized and the main conclusions are:

(i) Light rain due to the coalescence mechanism can occur from cumulus clouds of 1-2 km thickness and is common from layer clouds 0.5 to 1 km thick. It is in much greater evidence in stratiform than in cumuliform clouds and sometimes appears to provide a substantial contribution to frontal rainfall.

(ii) Coalescence is the predominant process down to cloud temperature of about -12°C. At lower temperatures the number of precipitation particles in cumuliform clouds appears to increase considerably presumably due to the Bergeron process.

(iii) Adiabatic water contents are often reached or exceeded in these warm clouds although they are restricted to local areas in the case of the cumuliform clouds.

(iv) The concentrations of the larger chloride particles can usually account for the larger droplets in the clouds. Although the British Isles is surrounded by sea the chloride particle concentration, however, can vary within wide limits between $50\ell^{-1}$ in continental air to $1500\ell^{-1}$ in unstable N.W. Atlantic air.

In general the details of the development of coalescence rainfall are now fairly well known and are in agreement with theoretical considerations. No satisfactory measurements of vertical currents however have as yet been made to use in conjunction with the nucleus and drop size spectra so that it is still not possible to produce a full description of this mechanism.

Discussion

ADDERLEY: Is the criterion for division of occasions into coalescence and Bergeron types purely that of temperature of the top of the cloud?

MURGATROYD: Yes. Most of the cases I have shown were divided into classes warmer or colder than 0°C, and for the case of freezing levels below 7000 ft this was also done for the -15°C level.

SOULAGE: In a previous paper you have said that the technique of a thin foil permits you to distinguish ice crystals and water droplets. Is this distinction easy and from what size is it possible?

MURGATROYD: The technique is not easy and takes a lot of practice. As well as using the foil it is possible in flight to see ice crystals impinging on a small black rod on the side of the fuselage. Another method is to let air flow through a tube illuminated by a beam of light at right angles to the line of sight, and ice crystals can

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then be seen.

BETHWAITE: Did you measure the durability of individual cumulus cells?

MURGATROYD: No. Most of the recent Meteorological Research Flight work has been confined to stratus. The Thunderstorm Project data and also our earlier observations suggested a lifetime of about 15-20 minutes.

BETHWAITE: The cross section of the front on your slide is in striking agreement with our observations of fronts in S. W. Australia.

BIGG: In your nucleus concentration slide you showed a higher concentration of freezing nuclei active at -20°C at 10,000 ft than at the ground. This is in sharp contrast to your measurements of salt and condensation nucleus concentrations. Have you an explanation for this?

MURGATROYD: No, I don't have an explanation. All concentrations of salt, spores and the like usually decrease upwards quite rapidly, the only exceptions occur when the source region is a long way off. Freezing nuclei do not fit into the picture. We flew all through January in 1956 and 1958 but could not detect any decrease with height.

EVOLUTION OF DROPLET SPECTRUM IN A CUMULIFORM CLOUD: A THEORETICAL STUDY

by

R. C. Srivastava and A. K. Roy*

Since Howell's attempt in 1949 in developing a complete quantitative theory on the evolution of droplet spectrum in a cloud, various investigators have examined the problem critically by carrying out rigorous computations of cloud droplet growth, on reasonably valid assumptions of nuclei concentration and their size distribution and of the rate of cooling of rising air. Despite various refinements introduced in the more recent computations, droplet spectra deduced theoretically, by considering condensational growth in uniformaly cooled air, have failed to explain fully the features shown by the observed spectra.

Departing from the usual conception of laminar updraft and assuming that the updraft structure in a cumuliform cloud is composed of a steady upward motion (at speed $\bar{U}$) with eddies superposed on it, and taking supersaturation (σ) within cloud to be constant, and neglecting, as a first approximation, the effect of curvature and of solute on the equilibrium vapour pressure over drops, the following expression is deduced for the number of droplets of radius r, at a height Z above cloud base,

$$N(r) = N(r_m) \exp \left[- (UZ/4E) \left[(r/r_m) - (r_m/r) \right]^2 \right]$$

where r_m (mode radius) is given by $\sqrt{26Z/\bar{U}C}$ and E (eddy diffusion coefficient = $\frac{1}{2}\overline{UL}$.

In the above, C is a constant and U represents the rate of circulation in the peripheral region of an eddy of size L. The bar over the product UL denotes average taken over eddies having different values of U and L. The equation represents a unimodal skew distribution, with mode radius r_m, a distribution which would result from the turbulent structure of updraft alone, even under condition of perfectly homogeneous condensation nuclei. It is seen that, for a given value of mean updraft $\bar{U}$, the shape of the drop size distribution curve at a certain height is determined uniquely by the two parameters, (i) eddy diffusion coefficient, E, and (ii) mode radius, r_m, as determined by the constant value of supersaturation, σ. Certain important differences between spectrum characteristics deduced from the above relation and those of spectrum computed on the usual assumption of laminar updraft and all adiabatic condition within cloud are discussed.

Theoretically derived spectra, when compared with spectra based on actual measurements in different cumuliform clouds, show a reasonably good agreement, the fit observed being particularly close in the case of fair weather cumuli. Supersaturation

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and eddy coefficient values deduced for cumuliform clouds of various types, on consideration of drop size distribution according to the suggested relation, appear to be of the right order.

Modifications in the theoretically computed drop size spectrum, when probable distributions of nuclei sizes are taken into account, are considered qualitatively.

Discussion

NEIBURGER: It is not clear to me which parameters were assumed or evaluated independently, and which were chosen specifically on the basis of the observed data. If enough of the parameters are determined from the observations, the "theoretical" curves must necessarily fit the observed distribution closely.

ROY: The main objective of the study was to show that, by postulating an eddy model of updraft structure within a cumulus cloud, it was possible to explain in a large measure the observed distribution of droplet sizes in such a cloud, without taking into account heterogeneity of nuclei sizes in cloud air. The equation used has two variables r_m and $\bar{U}Z/E$. For computing droplet concentrations from the equation, the value of r_m has been taken from the observed spectrum. As regards $\bar{U}Z/E$, this is obtained from the slope of the curve obtained by plotting $[(r/r_m) - (r_m/r)]^2$ against corresponding measured concentrations. The straight line character of the plots gives a good support to the scheme of study of droplet spectrum on the basis of the proposed eddy model of updraft within cumulus cloud. However, it was thought that graphs giving size distribution, as computed theoretically, and their comparisons with observed spectra would effectively establish the same point, and provide a more readily comprehensible picture of the applicability of the suggested equation.

SQUIRES: Mr. Roy, I have the impression that the computations concerning trade-wind cumuli agreed least well with the observations at the highest levels. Now it is well established that the effects of mixing between the cloud and its environment increase upwards (for instance, the ratio of w/w_a decreases steadily with height). How does this kind of consideration affect the expected agreement between computation and observation?

ROY: The highest level considered is about half way up the cloud; we considered that, at this level, the effects of mixing would probably not be important.

HITSCHFELD: One obvious way of testing the usefulness of this very interesting theory would be to examine the numerical values required for the adjustable parameters when comparison is made with the observed spectra. Thus, would the authors supply us with the values of E which they deduced in those cases where measured and computed spectra agree well? From these values, one could then deduce values of L, the size of the eddy. Better still, the authors might look for reasonable pairs of U and L, consistent with these deduced values of E.

ROY: In cumulus congestus we assume mean updraught of 3 meters per second. The values deduced for $\overline{UL}$ and for σ are given in the table below.

OBSERVER	CLOUD TYPE	$\overline{UL}$	σ
Diem	Fair Weather Cu	1.6×10^6 cm^2/sec	0.06%
Weickmann and aufm Kampe	" "	4×10^6 cm^2/sec	0.03%
Squires	Trade wind Cu	5.3×10^6 cm^2/sec	0.17%
Deim	Cumulus Congestus	$\approx 10^7$ cm^2/sec (taking Z = 2 km)	0.14%
Weickmann and aufm Kampe	" "	$\approx 10^8$ cm^2/sec (taking Z = 1.7 km)	0.024%

Assuming a certain probable mean value for eddy size (L), for example, of the order of 200 m in a fair weather cumulus, U would come to about 1 m/s. It will be seen that the values of $\overline{UL}$, a measure of turbulence, increase steadily as the cloud size increases. Also, the supersaturation values calculated are of the right order, except in the case of Cumulus Congestus observed by Weickmann and aufm Kampe.

III. GROWTH OF CLOUD PARTICLES, CLOUD ELECTRIFICATION, AND COALESCENCE

ON THE MICROPHYSICS OF CLOUD-DROPLET COALESCENCE

by

J. Doyne Sartor*

Two freely falling water drops in the process of coalescing are studied in some detail. Measurements made from successive frames of slow-motion photographs of the drops are used as a guide to the sequence of the intermediate steps in the coalescence process. Surface-energy considerations are then applied to the successive stages of the coalescence, assuming that the drops are not deformed under their own weight. The effect of varying dropsize ratio is studied quantitatively. The quantitative results are applied qualitatively to some typical drop-interaction situations in which the relative motion and "obesity" of the drops are considered.

The role of the electric field in promoting coalescence is considered as a function of droplet ratio, "obesity", and separation distance of near surface as well as the strength of the field.

The coalescence of cloud droplets is discussed in light of certain features of their relative collision trajectories. The features considered are those obtained from theoretical trajectories that have been verified by laboratory experiment. Finally, the combined consequences of collision, coalescence and partial coalescence on subsequent growth of the electric field are presented quantitatively.

In general it is concluded that drops coalesce more readily when they are of similar size and that partial coalescence of colliding droplets contributes to the growth of the electric field.

The results of comparing computed droplet trajectories using Hocking's solution for the viscous drag with Schotland's experimentally obtained trajectories support both Hocking's theory and the use of laboratory models except that the measurements of the model parameters must be made more accurately in order to obtain collision efficiencies.

Discussion

TELFORD: I am worried about your virtual mass. Is not your virtual mass the value associated only with an isolated sphere under potential flow?

SARTOR: Yes; but it applies in any situation when the density of the fluid is not negligible. It is correct to say the virtual mass used is derived from potential flow around an isolated sphere.

McDONALD: I formed the impression that you are looking for an energy barrier, a surface-energy maximum, at some intermediate stage of coalescence, but assuming a liquid-bridge exists at all times.

But from dynamical rather than surface-energy considerations I believe it can be shown that once the liquid-bridge is established there can be no stopping of the coalescence. This is because the internal pressure in the bridge or nexus region is intermediate in value between the small and large drop's internal pressures. Rapid squeezing-out of the water from the smaller drop into the larger drop follows inevitably. No sort of energy barrier arises to stop the coalescence once the bridge is established. Coalescence is then already assured.

SARTOR: I was not under this impression from the Brown, Palmer & Wormell paper; they conjecture that the drops could reach some configuration while coalescing for which the surface energy exceeds that for separate spheres in which case they would rupture again.

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I am really showing experimentally that it appears from the photograph that once a liquid bridge forms and no large external forces exist, the drops will complete the coalescence process uninterrupted.

THE INFLUENCE OF ELECTRIC FIELDS ON SMALL DROP COLLISIONS

by

J. W. Telford*

A vertical windtunnel was constructed for experimental study of small water drops. The air velocity can be adjusted so water drops are supported and slowly carried upwards. A coalescence between two drops yields a larger drop which can fall downwards.

The working section of the tunnel is a one foot diameter transparent perspex tuve, open to the room via an orifice through which drops can be flung. The drops, which must be nearly uniform if they are to be well separated in size from coalesced drops, are generated by a spinning disk generator which provides the inertia necessary to throw them into the centre of the tunnel.

Charged drops can be examined by deflecting them in a horizontal field which can be repetitively switched in the sequence 0, 0, +, -, 0, 0, +, -. This gives a signature indicating charge sign and magnitude.

The observations fall into three sections:

Section A: <u>Simpson's Drop Rupture Theory.</u>

Simpson proposed in 1909 that large water drops broken by an airdraught produced electric charge separation. Many experimental tests of this idea are unacceptable, since, as shown by Gill and Alfrez (Proc. Phys. Soc. London, B65 pp 546-51 July 1952), drops bouncing from a charge collecting probe acquire charge during the actual bouncing process.

The wind tunnel enables the drops formed by the rupture of the water surface at the edge of the spinning disk (where there is vigorous ventilation) to be examined for charge without contact with any surfaces. No charging effects were usually observed. Drops formed by water of very low conductivity were occasionally charged. The effect certainly does not appear to be a prolific source of charge generation, particularly when the water conductivities are comparable with rainwater.

Section B: <u>Inductive Charging.</u>

Two conducting bodies which are electrically connected and then separated, in an electric field, acquire equal but opposite charges. This mechanism was first proposed for closely approaching water drops by Elster and Geitel and has been recently reconsidered by Sartor.

In the wind tunnel this mechanism was examined but yielded no effect. Theoretically, no spark discharge connection is possible at these sizes, since there is not sufficient voltage between the surfaces to ionize air. This applies despite the very large field strength.

These observations are not however, relevant to the splashing of water from hailstones where inductive charging is probably effective.

Section C: <u>Drop Bounce.</u>

It has often been suggested that freely falling water drops bounce. Evidence for this is based on the behaviour of large drops on flat water surfaces or on small drops which have apparently intersecting trajectories not yielding coalescence. Having accepted that not all intersecting trajectories are fruitful the conclusions are extended by making deductions, which appear to me, more appropriate to bouncing ball bearings.

Wind tunnel photographs show some trajectories very similar to those for collisions resulting in coalescence, but which result in the drops separating again as independent entities. If these non-coalescing trajectories were in fact identical with coalescing trajectories up to drop contact then we do in fact have droplet <u>bounce</u>, since failure to

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coalescence is then unrelated to the aerodynamic flow problem. If similar conditions to those prevailing with skating drops on a flat surface prevailed in this instance, then surface active agents should markedly reduce the number of fruitful interactions resulting in coalescence.

Two recommended surface-active agents produced no detectable reduction in the coalescence rate. It is thus concluded that unfruitful collisions are purely an aerodynamic phenomenon. This agrees with Hocking's calculations and my observations at about 35 diameter, where non-coalescing collisions are frequent but follow the expected aerodynamic pattern.

In conclusion I would like to suggest that if delegates feel the need to use the word bounce in connection with water drops, they qualify it so that drop reaction without actual surface contact is not excluded.

Discussion

HITSCHFELD: When you tested the contaminents, did you actually have the two drops in contact? If not, how can you make any meaningful statement about the effect of the contaminants?

TELFORD: The argument depends on the observation that most drops coalesce on close approach, the non-coalescing events being in the minority. If the non-coalescing event was a surface bounce one would expect the surface active agent to reduce the number of coalescences by making the bounce more effective. This it did not do.

Hence I deduce that the non-coalescing events are close approaches followed by separations, controlled only by the aerodynamic airflow pattern.

SHAEFER: It has been our experience that the use of surface active materials greatly increases the degree of "bounce effect" of small water droplets. In the course of these experiments I have noticed, by watching the interference fringes underneath the floating droplets, (using the floating droplet as a simple lens) that the critical air thickness necessary to prevent coalescence is in the range 500-800 ÅU (50-80μ). As the air thickness drops below this value, coalescence with the substrate occurs instantly.

TELFORD: My observations on skating drops agree with yours but I have no experimental evidence to support any explanation of the phenomenon of variable life of the skating drops.

HITSCHFELD: It used to be thought that in a saturated atmosphere drops in contact coalesce easily, but that in unsaturated surroundings they do not. Is this still accepted, and if so, what sort of surroundings did you perform your experiment in?

TELFORD: The experiments were all performed in saturated air as the wind tunnel is a closed circuit and the drops rapidly sature it.

MARSHALL: You mentioned that the bounce of drops from hailstones is exceptional. What happens in this case?

TELFORD: If splashing occurs from hailstones due to their great fall speed, then inductive charging could be a very efficient process.

SARTOR: (Communicated) Mr. Telford's experiment is very interesting. Our theoretical computations indicate that, as Mr. Telford observes, one would not expect charge separation from contacts between freely falling droplets in a horizontal field. However, charge transfer would be expected under similar circumstances if the field were placed on vertically. Mr. Telford is urged to vary the orientation of his applied electric field.

TELFORD: There appears to be a conflict in the theoretical calculations. My calculations show that an electrical discharge cannot occur through a separating air film between the drops, as at these sizes there is insufficient potential difference to ionize air when the actual field strength is sufficient. I should like to stress this point.

The experiments tend to confirm this, as in a non -coalescing event the drops are initially placed with the larger above, while after the event the smaller is above the larger. Hence at some stage the drop centre will be in the same horizontal plane.

It is also clear that, in view of the fact that most events lead to coalescence, the point of closest approach, during a non-coalescing event, will occur when the drop centres are lined up at a large deviation from the vertical. Hence, in my experiments, a large fraction of the total field lies in line with the drop centres in at least some of the events. I would like to suggest that, while the vertical field is of greatest meteorological significance, it is not of any particular significance in designing an experimental test of the process.

RECENT STUDIES OF CLOUD ELECTRIFICATION

by

Bernard Vonnegut*

During the past several years experiments have been carried out directed towards obtaining an understanding of the effects that are produced by the electrification in clouds and of the mechanisms that are responsible for the accumulation of electric charge.

Our observations thus far are consistent with the idea that electrification may play an important role in accelerating the growth of precipitation by coalescence. In our observations of developing thunderstorms over the summit of Mt. Withington, New Mexico we found that the electric field inside of the cloud reverses and begins to increase prior to the detection of the first radar echo. Rough estimates of the rate of drop growth of the precipitation indicate collection efficiencies greater than unity, suggesting that perhaps electrical forces may be promoting collisions between droplets.

Observations of small warm cumuli made on Grand Bahama Island, B. W. I., show that rain forms with surprising speed (in only a few minutes) and that the rain formation is preceded by appreciable perturbations in the electric field above the cloud. The drop growth rate estimated from radar observations is appreciably greater than that predicted from classical coalescence mechanisms, again suggesting that the process may be accelerated by electrical forces.

Radar observations of a nocturnal thunderstorm that formed directly overhead on Grand Bahama Island show that prior to lightning strokes overhead there was little heavy precipitation falling. Less than a minute after the stroke a heavy rain echo suddenly appeared, and in two or three minutes this heavy rain reached the ground. Observations of several sequences of this sort suggest that the lightning discharge causes an accelerated rain formation process.

Measurements pertaining to the electrification process in thunderstorms reveal the following features:

1. The development of electrification is associated with the strong convective movement of the cloud.

2. Flights with a balloon-borne Faraday cage that measures all space charge except that carried by falling precipitation show that there are extensive regions of space charge of both polarities within thunderstorms and beneath them. The density of space charge is sufficiently high that if one sign of charge is systematically carried by updrafts while the other is carried by downdrafts, the resultant currents are adequate to sustain the observed electrification.

3. The currents carried by falling rain and hail to the ground are far less than the measured point discharge currents and appear inadequate to sustain the observed electrification.

4. The ice phase does not appear to be necessary for the formation of strong electrification, for clouds that at no time during their life ever reached the freezing level have been observed to give lightning.

Experiments in central Illinois show that the electrification of small cumulus humilis clouds can be significantly modified by space charge released from an electrified horizontal wire supported on masts 10 meters above the ground. The observations are in accord with the convective charge generation mechanism suggested by Grenet. If it is assumed that thunderstorms differ from small cumuli primarily in size and intensity and that their circulation is similar, it appears that the currents resulting from the convective transport of space charge are important items in their electrical budget.

Discussion

TELFORD: Have you considered the effect of turbulent diffusion in confusing your charge separation mechanism?

VONNEGUT: Yes, I have worried about this somewhat. However, this is a problem in any theory of electrification; if the charges get too thoroughly mixed up, no electrical effects can result. Thus, in very large cumulo-nimbi, with vertical

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drafts of 50 m sec^{-1} up and down, precipitation particles, however large, will be at their mercy and any charge separation process could be seriously affected by mixing.

BRAHAM: This is important work - certainly the kind we need more of in atmospheric physics. However, I question the implication that the circulations you find in the small cumulus cloud are comparable to those of the young building thunderstorm. From great experience in flying through cumulus clouds, I agree that circulation of small clouds seems better described by the bubble theories than that of a jet updraft. But to my knowledge, the only systematic measurements of thunderstorm circulation were those of the Thunderstorm Project. In those measurements we were not aware of systematic downdrafts around the edges of the clouds. On the other hand we may have missed them. This is the critical measurement that needs to be made; we need to look for systematic downdraft around the edges of cumulus clouds.

VONNEGUT: We were recently observing a cumulus which grew into a thunderstorm. Trying to fly over a turret, the aircraft which was circling it could not climb above 27,000 feet, although the control setting should have resulted in a rate of climb of 1000 ft. per minute; in fact, the aircraft was forced down at about this rate. We have frequently observed strong downdrafts on the periphery of these clouds. Also, lapse time photos from high-level balloons looking down on cumulo-nimbi from above show a radial outward flow of air on the cloud top. We disagree about the answer to the question: "Is there convergence or divergence over the tops of these thunderstorm clouds?"

MURGATROYD: Latham and Mason have recently published papers explaining how the generation of electric charge in thunderstorms is associated with the formation of soft hail. Their theory, which is based on the development of electric potentials in ice crystals under the influence of temperature gradients followed by charge separation due to the fact that H^+ ions have a greater mobility than the OH^- ions, requires freezing of supercooled droplets on collision with hailstones, i. e. the ice stage must be well advanced in the cloud. In this case, the onset of precipitation will precede the charge separation by some 20 minutes. These ideas, and indeed all our notions that the ice phase is basic to the charge separation process, are not apparently in agreement with your paper and I would be glad if you would comment.

VONNEGUT: One thing which makes me doubt the ice mechanism of cloud electrification is that in the Bahamas we have found warm clouds causing significant field perturbations, and a well authenticated case of such a cloud giving rise to four strokes of lightning. Mason casts doubt on this observation, but we are not alone in our conviction that warm clouds can make lightning. Further, I would expect that, if precipitation is the cause of electrification, some lightning strokes would terminate in the rain sheet below the cloud: however, lightning usually seems to ignore the precipitation.

BOWEN: I find no difficulty in reconciling the very stimulating evidence which Vonnegut has produced about the generation of electrical charge in warm clouds with the generally accepted view that electrical effects result from the co-existence of ice crystals and water drops in a supercooled cloud. From our observations I have no doubt that the ice mechanism is the one most frequently at work in the atmosphere, but this does not exclude the process described by Vonnegut which obviously happens, but perhaps on a minority of occasions. There are, of course, several well authenticated cases in desert regions of lightning strokes coming from a clear blue sky, when both ice crystals and water droplets are conspicuously absent.

IV. NATURAL FREEZING NUCLEI AND SNOW CRYSTALS

STUDIES OF SMALL-SCALE DISTRIBUTIONS OF RAINFALL

by

T. Bergeron*

Methods of climatic control on a large scale have probably more prospects of success when applied to precipitation than with any other meteorological element. To be able to forsee and verify any possible such success, it is fundamental, though, to understand the natural processes that lead to precipitation and to the final distribution on the Earth's surface of the precipitated water.

It is nowadays well recognized among geophysicists that the ultimate maxima of <u>snow</u> accumulation on the ground - owing to snow drift - may differ essentially from the position of the clouds within which the snow crystals were formed. On the other hand, seemingly it has been tacitly assumed that the pattern of rainfall distribution on the ground would not differ materially from the corresponding pattern of precipitation release within the clouds. This still seems, on the whole, to hold good for the more or less intense kinds of precipitation produced in convective cells or systems. With precipitation from frontal or oreigenic 1) systems, or even the drizzle from a stratus system, there seems to exist a very noteworthy and economically important "atmospheric low-level redistribution of precipitable water".

Rainfall distributions have been studied at the Department of Synoptic Meteorology of Uppsala University, generally in the period July - October, since 1953 with a network of raingauges of unparallelled density and extent. In the 1955 and 1956 campaigns 1000 gauges were operated in different fields. Since 1958 only one field, 20 x 30 km, around Uppsala was used, but having since 1959 more than 300 gauges down to 1 km apart. Results hitherto may be summed up thus:

1. The direct oreigenic effects on precipitation extend from the ordinary-scale patterns, detectable with stations 10 - 100 km apart, at least down the meso-scale patterns, only detectable with stations 1-10 km apart. A mechanism and a formula for this oreigenic effect, acting as "an atmospheric low-level redistribution of precipitable water" has been suggested.

2. Two principal types of meso-scale rainfall patterns are generally superimposed on the wellknown ordinary-scale patterns, the wave and helical motion patterns, corresponding to stably and unstably stratified air.

3. At a study of the effects 1. being originally the main concern of the project, the effects 2. evidently form a complication. Especially the convective-cell "swaths" tend to obscure the other patterns, and different methods had to be tried for filtering off these swaths.

4. On the other hand, the convective cells could be studied synoptically as to paths: and life history better than hitherto thanks to the density and extent of the network. The convective cells often seem to have preferred paths, still to be confirmed by further studies; in the summer 1958 the small town of Uppsala apparently was an efficient convection releaser.

5. Hitherto available rainfall maps for any country will be less representative than assumed (a) since official network stations mostly only will represent low or flat and/or tree-bare areas, and the effect of even small elevations and of forests may be considerable in certain weather types, (b) since, with a prevailing wind direction for convective swaths, two places only 2 km (1.3 mi) apart may differ by 1 mm rain, or more, even in a 30 years average for the monthly precipitation.

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Professor Bergeron was unfortunately prevented by illness from presenting his paper in Canberra.

1) "Oreigenic" from the Greek, meaning literally "born on the mountains", is a better term than "orographic", meaning "describing mountains".

6. Convective swaths, displacements of the precipitation maxima and minima with different wind directions, and with different release efficiencies, and not least measuring errors, cause a great spreading of the readings. Thus, when treating for instance monthly rainfalls statistically, a rather weak (in a few cases even negative) correlation with meso-scale orography etc. appears.

7. Therefore, at first the numerous synoptic maps of all main rains have been analyzed most carefully, enabling an individual criticism of all the observations, and directly displaying the different oreigenic effects and the convective swaths. Then follows the filtering off of the latter, the drawing of new maps and a statistical treatment of maps grouped according to wind direction, etc.

8. In many cases, though, even the meso-scales oreigenic effect is so pronounced that it shows up directly also statistically. So, the rainfall patterns of the night-periods 14 - 15 and 15 - 16 Oct. 1953, with a stationary frontal rain and NE wind for more than 48 hrs., show a positive correlation of 0.65 ± 0.03.

9. The orders of magnitude of all different measuring errors have been treated in a special study, i.e. those due to deficiencies of the gauge and its site, its readings, evaporation, up-wind and splash-effects etc. Gauges with the rim at ground level surrounded by a dense splash-free horizontal netting were used as reference gauges in this study.

10. The project running 1 July - 31 Oct. this year is equipped with 20 such "buried gauges", 20 pluviographs and a radar set (as aids at following the convective swaths); and a network of 80 unmanned gauges inspected by professional observers fills the uninhabited forests and fields of the test area with measuring points on an average 1 km apart.

MECHANISMS OF RAIN-PRODUCING SYSTEMS IN SOUTH AUSTRALIA

by

K. T. Spillane and K. Yamaguchi*

Occurrences of rain at Adelaide, South Australia, during 1957-58-59 have been examined by use of synoptic surface and upper air charts, over daily radio sonde reports from Adelaide Airport and surface observations of clouds. Cloud thicknesses have been estimated where possible from radio-sonde data, and surface observations of bases.

Tables showing frequency of occurrence and amounts of rain at the point station Adelaide are listed in divisions of cloud top temperature-cloud thickness, with subdivisions of cloud type (Cu & St.) and air mass trajectory (maritime, continental).

No useful estimates of vertical motion permit any measure of efficiency of rain processes as reflected by the gauging, at a point, but these data permit speculation on effects of cloud seeding.

Tables of frequency and amount of rain in divisions of cloud base height and thickness showed that it was not until cloud thickness extended above 15000 ft. that rain in any appreciable amount fell from cloud with base above 4000 ft. The importance of evaporation in the sub-cloud layer is demonstrated by its control of intensity of rain along with coalescence as indicated by thickness of warm position of deep stratified clouds. For these clouds and recorded intensity a simple model yielded drop sizes which by evaporation (after Kinzer and Gunn) in sub-cloud layer gave at the ground drop sizes in excellent agreement with max-drop size v. intensity curves of Blanchard and Spencer.

A clear relation between on-shore surface wind speed and cloud thickness for precipitation from cumuliform clouds with tops warmer than -10°C, appeared to provide evidence on the importance of giant nuclei (Woodcock). An attempt to detect an influence on the special separation of shower clouds attributable to the presence of giant salt nuclei as measured by surface wind speed was inconclusive. A similar study for off-shore winds indicated clouds in air masses that had spent a short time (less than a day) over the continent rained more readily than similar clouds of maritime origin when the wind speed was less than 7 knots, an effect not readily explainable.

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Discussion

BETHWAITE: In order to avoid confusion between the clouds at Adelaide, just reported, and the quite different clouds observed during the South Australian Cloud Seeding Experiment (sometimes called the Adelaide experiment) which will be discussed later, it should be noted that the experimental areas were remote from Adelaide, and in a different climatic regime.

SPILLANE: The area would have benefited from deep stratus cloud system which are generally State wide, but not common. I agree that the cumuliform clouds over the area, 50 to 100 miles north, may bear no relationship to those observed at Adelaide. Orographic effects can cause very different rainfall from Adelaide as little as ten miles away.

ROY: From radar census of precipitation cells within 100 miles around Delhi we had estimated that the contribution to rainfall over the area during the monsoon season by warm clouds - the most favourable season for rain occurrence from such clouds over the area - is only about 2 per cent. However, from this I would not be inclined to conclude that seeding of warm clouds in that area would not be of value. For it was just possible that some 90 per cent of warm clouds developing over the area failed to precipitate naturally, and seeding of such clouds might help to increase the season's rainfall by about 18 per cent. Might not similar considerations apply to Adelaide area also? For the statistical study made by the authors of the paper on incidence of rainfall from clouds of different depths, it might be better to take into consideration rainfall recorded at neighbouring rain gauges also.

SPILLANE: That may well be so. I should also mention that our definition of warm rain excluded cases where cirrus was present within the layer 5 km above the tops of clouds considered, to exclude effects of seeding with ice crystals.

DEVELOPMENT AND MOVEMENT OF CONVECTIVE SHOWERS OVER THE CENTRAL APPALACHIANS

by

Charles L. Hosler*

Two years of filmed radar coverage of the Central Appalachian area and measurements of vertical motion made by automatic radar tracking of balloons have been evaluated to ascertain the degree to which the topography may influence the development, movement, and dissipation of convective showers. Half of the area covered is a plateau sloping upward from west to east, reaching an elevation of 2200 ft. at its eastern edge. The plateau is dissected by narrow river valleys. The other half is lower land, averaging between 500 and 1000 ft. elevation, interrupted by long, narrow ridges extending to the northeast and southwest and extending to elevations of about 2200 ft.

Over 1000 single convective cells were examined. We looked for effects upon shower behaviour of parameters such as stability, large scale vertical velocity, 500-mb vorticity, wind shear, wind speed, precipitable water, dew point depression (surface and 850-mb), and speed size and direction of movement of the echoes. In addition, all aspects of shower behaviour were examined to detect differences in the behaviour of showers, depending upon the area over which they developed and moved. The mean duration of the echoes studied was 28 minutes; 80 percent of the echoes lasted less than 40 minutes.

Shower durations in the ridge and valley section were greater when the echo moved parallel to the ridges. On the other hand, showers over the plateau lasted longer when the flow was perpendicular to the edge of the plateau, which runs northeast-southwest. The best predictor of shower duration is temperature-dewpoint spread at the surface. A linear correlation coefficient of 0.81 was obtained for this parameter. 500-mb vorticity showed a correlation coefficient of 0.42 with shower duration. Duration of showers increased with echo size.

The areas of formation and dissipation of showers show a pronounced effect of

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the topography. To the east of the edge of the plateau, a westerly or southwesterly wind with a component perpendicular to the plateau produces a lee wave, which results in measured vertical displacements of as much as 2000 ft. In the trough of this wave, showers do not develop. Even lines of showers moving across the wave are weakened or dissipated to regenerate again down wind. The wave amplitude appears to be a maximum in the morning and in the evening when the valley air is more stable than in the middle of the day. These effects will be illustrated with slides. Balloon measurements also indicate waves of greater wave length in advance of cold fronts where strong winds aloft have a large component normal to the front. This indicates a "lee wave" caused by flow down the frontal surface.

Discussion

HITSCHFELD: One of the slides showed that the duration of showers was longer with "perpendicular" circulation over the plateau than over ridge and valley. But the ridge-and-valley showers had the same duration with parallel and perpendicular circulations. Hence it is difficult to arrive at the speaker's conclusion that the perpendicular circulation over the ridges and valleys shorten the life of the showers.

HOSLER: These are different cases since the different trajectory means that a parameter not shown on my slide is important. There is a systematic difference in dew point depression between perpendicular and parallel flows. This explains the choice of the abnormal case and verifies the reality of the shortening of the life cycle in the perpendicular case over the ridges and valleys.

SINGULARITIES IN DAILY RAINFALL

by

G. O'Mahony*

During the last ten years a considerable amount of meteorological literature has been devoted to investigations into apparent singularities in daily rainfall amounts and the problem of deciding whether there are preferred dates for the occurrence of heavy falls. The statistical distribution of daily rainfall is extremely skew, the skewness being positive since the modal value is usually zero or very near to it. Because of this most of the relevant studies have been based on distribution-free methods.

In this investigation daily data from four stations, Sydney, Adelaide, Melbourne and Brisbane, have been used, particular emphasis being placed on the Sydney figures. These were related to data from other N. S. W. stations and also to records from Rockhampton, Queensland. Daily records for the first three stations are available for 100 years.

Sample correlation coefficients between the mean rainfall and number of wet days on any one date were not significant, a fact which had been recognised by other investigators.

While the peaks in means of daily rainfalls are caused by relatively rare occurrences of heavy falls, two facts arising from the investigation are worthy of further study. The first of these is that major peaks in Sydney daily falls, especially during January, are shown to persist in four consecutive series each of 25 years. Also, these same peaks persist in percentile graphs, from the extreme value down to and including the 90th percentile. The effects are not present consistently in graphs based on data from the other stations.

Fitting a Poisson distribution to the number of occurrences of falls greater than one inch tended to confirm the presence of singularities in the Sydney data.

The distribution of daily falls on any one date is shown to conform fairly closely to a truncated Incomplete Gamma distribution. Then, the distribution was fitted to data of three days for Rockhampton, the days being selected because they showed peaks in Sydney, and also because the Rockhampton heavy falls did not usually occur in the same year as the heavy falls in Sydney. The distribution scale factor for these dates at Rockhampton was significantly different from the corresponding parameter for the complete month, and so these results also tended to confirm the fact that the peaks

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were not due to random causes.

The investigation was extended to the months of December and February and here the series were not significantly different from random.

Further study is required in more districts of Australia and over all months of the year to establish the geographic and temporal bounds of the effect if it is finally proved to be real.

Discussion

RADOK: Two features of the Sydney data seem to call for explanation; the more frequent occurrence of heavy rain on two calendar dates in January and the persistence of these peaks when the rainfall series is subdivided. However, given the first there is little mystery about the second feature, nor about its persistence down to the 20th percentile: a random distribution of the large number of heavy falls on the dates in question makes both likely. Thus it is only the primary clustering of heavy falls which needs to be examined.

Now if there are N heavy falls on record for January the average number calendar per date is $N/31 = m$. In absence of genuine clustering the numbers of dates with i (=0, 1, 2,..) heavy falls might be expected to be given by the terms of a Poisson series, $31 \exp(-m) m^i / i$. The following table shows these numbers for rainfalls between 1" and 3", as well as for falls above 3", together with the corresponding numbers observed in 100 years of Sydney rainfalls:

N° of dates with i falls between 1" and 3" (total: 85)								
i	0	1	2	3	4	5	6	>6
observed	1	8	6	6	6	2	2	0
expected	2.0	5.6	7.7	7.0	4.8	2.6	1.2	0.1

N° of dates with i falls above 3" (total: 10)				
i	0	1	2	>2
observed	23	6	2	0
expected	21.9	7.1	1.1	0.9

This suggests that taken by themselves the peaks in Sydney rainfall are a chance phenomenon.

(Communicated): The joint Sydney-Rockampton data might perhaps be explained by a similar analysis of all occasions when a "rain influence" produced heavy falls at Sydney, Rockhampton, or both stations.

O'MAHONY: It is obvious that the peaks in the parent series must be contained in the subseries, but what is not obvious in the case of the Sydney data, is that <u>all</u> the subseries show the effect.

I also had applied a test using a Poisson distribution. I used a variate of number of falls of one inch or more. A significant value was obtained for the occurrence of such falls on the major peak day. The test is, of course, not very powerful.

Concerning the Rockhampton data, the a priori probability that two 25-year series will have the extreme peak on the 23rd of the month is $(1/31 \times 1/31)$, assuming of course the subseries are independent of each other, as I believe they are, and assuming also, that the Rockhampton data are independent of Sydney data. This last assumption is not completely true, as we know, but I have shown that in only 5 years out of 22 years of heavy falls, have they occurred in the same year, and in only one of these cases did synoptic situation indicate a very close relationship. That was in 1955.

V. NATURAL FREEZING NUCLEI

STUDIES OF ATMOSPHERIC PHENOMENA IN A NATURAL OUTDOOR LABORATORY

by

Vincent J. Schaefer*

One of the most important requirements for making progress in the experimental phases of the atmospheric sciences depends on suitable atmospheric conditions for conducting experiments. Over the past ten years I have located a number of places where nearly ideal conditions occur for making field studies using the atmosphere as a laboratory.

An area of this type which we explored in 1948 and since 1956 have been using extensively for cumulus studies is the San Francisco Peaks in northern Arizona. In this region it is not uncommon to have isolated daytime thunderstorms during a 30 to 40 day period.

For some types of outdoor experiments it is desirable to have localized phenomena on a still smaller scale. Such conditions have now been located in the Old Faithful Geyser area of Yellowstone Park in north-western Wyoming, U.S.A. Here is located the largest geyser and hot springs area of the world. The hundreds of thermal units produce extensive areas of supercooled clouds and ground fog during the winter when temperatures may drop to -40°C. For the past two winters I have visited this area and conducted numerous exploratory studies of some of the natural phenomena of the area. During ten days in February of this year, with support from the National Science Foundation, eight colleagues and I carried out a series of experiments and studies which illustrated the unique advantages of such an area for field studies.

The research was carried out in an area about two miles in diameter centered about Old Faithful Geyser. This geyser erupts every hour to form a convective cloud of water droplets 300 - 500 feet high. During the late afternoon, night and early morning the supercooled cloud formed by the eruption in the very cold air drifts westward as a supercooled ground fog or a low stratus cloud, depending on atmospheric conditions. This cloud is augmented by condensed vapour from hundreds of hot springs, the open waters of the Firehole River and a number of other smaller geysers to produce an ideal environment for studying supercooled droplets, the relatively rare naturally occurring ice crystals and the effects which may be induced by seeding the air with dry ice, silver iodide and other seeding materials.

It was found that the number of condensation nuclei in the area during the winter period is so low (often less than 200 Aitken nuclei per cubic centimeter) that supersaturation with respect to water is of common occurrence. This makes it possible to conduct a number of atmospheric studies which hitherto have been restricted to air in specialized chambers in the laboratory.

Studies were made of electrification of geyser plumes, potential gradients, crystal growth rates, glaciation of clouds, supersaturation relationships, cold air entrainment, the coalescence of droplets, micrometeorological regimes, columnar frost, rime ice and hoar frost, air circulation, heat budgets of hot springs, condensation and ice nuclei concentrations, the sampling of water and ice particles by formvar replication and a host of other studies of natural phenomena based on the interest of the individual scientist.

Further research in the area is planned for successive winters and it is hoped that the area will become a meeting place for atmospheric scientists interested in experimental reactions in the atmosphere.

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Discussion

SWINBANK: Can Dr. Schaefer make some provision at Yellowstone Park next winter to disturb the colloidal stability of the supercooled clouds by the application of ultrasonic vibrations?

SCHAEFER: There are many places where such experiments could be done. Usually, it is best to work at night using artificial illumination to observe the effects produced. Logistic considerations dictate the use of a small portable ultrasonic generator and your suggestions in this respect would be welcome. Some day we might get you to come and do it.

ON THE GROWTH OF NATURAL SNOW CRYSTALS

by

Choji Magono*

Natural snow crystals and meteorological conditions were observed by the Japanese Cloud Physics Group making use of microscopic photographs, replicas of snow crystals and ordinary routine surface meteorological observation methods at three observation points at different altitudes at Mt. Teine (1023 m), Hokkaido, Japan in 1959 and 1960. Rawin sondes and aircraft were used to observe the meteorological and cloud conditions at altitudes higher than 2000 m.

The results obtained are summarized as follows:

1. If the boundary of cloud regions was extended to the zone of humidity 85% with respect to ice measured by the rawin sondes, Nakaya's Ta-s diagram represented the meteorological conditions in which natural snow crystals of various types developed.
2. Regarding humidity, it was found that the value gained from the rawin sonde required considerable correction due to the difference in moisture between mountain and plain areas, even if the rawin sonde station at the plain area was close to the snow crystal observation points at the mountain area.
3. Nakaya's classification of natural snow crystals was insufficient to permit the use of the type of snow crystal as an aerological sonde. So a more detailed classification was supplemented to his original one.
4. Even if low clouds existed and no upper layer clouds were observed, snow fall continued for a long period. Numerous slight pieces of ice were observed with an impactor which was used in the measurement of cloud droplets. There were thought to be no plentiful sources supplying sublimation nuclei to the windward of the observation area, because Hokkaido and Siberia were covered by deposit snow. This situation suggests a possibility that broken pieces of falling snow crystals or pieces of ice blown up from deposit snow acted as nuclei of snow crystals like chain reaction seeding in rainfall.
5. When snow crystals of one type were predominant, it was usual that a marked thick air layer existed whose temperature was suitable to the development of that type of snow crystal. This phenomenon was particularly the case when an inversion layer existed.

Considering the results briefly described above, a simple method was designed to presume the aerological conditions to the windward, and the results obtained were satisfactory at the present time.

Discussion

NEIBURGER: Could the discrepancy between the rate of growth of crystals observed and that computed theoretically be explained by the growth at the computed rate in the layer of supersaturation, and then evaporation at lower levels where the air in the cloud was unsaturated?

MAGONO: I do not think so. Microscopic examinations of the crystals revealed sharp extremities which did not indicate any evaporation. In addition the snow crystals seem to grow even in the lower cloud levels, although at a small rate.

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SOME PARAMETERS GOVERNING DIFFUSIONAL GROWTH OF ICE CRYSTALS

by

James E. McDonald*

I. The most important phase of Bergeron-Fineisen growth of ice crystals in the presence of subcooled water droplets can be specified by certain growth parameters that are functions solely of temperature and pressure and independent of all other conditions in the cloud. Hence it is desirable to obtain numerical values of those parameters. Results of computation of a variety of these parameters will be summarized and from them information drawn concerning altitude-dependence of growth rates for several climatologically dissimilar regions.

II. The shape of a diffusionally growing ice crystal incluences the boundary conditions controlling rate of growth. By the electrostatic analogy it is customary to approximate the given crystal shape with that of some one of the small number of shapes for which there exist closed solutions to Laplace's equation. If the electrostatic capacitances were known for those crystal shapes, this approximation could be avoided. Results of laboratory measurements, made in a large Faraday cage, on the electrostatic capacitances of metal models of a wide variety of crystal shapes will be summarized, including dendritic crystals of progressively more open structure, hexagonal plates and columns of progressively varying slenderness ratio, and other less common shapes. Thin hexagonal plates are found to have capacitances equal (to within scatter of only a few per cent) to thin circular plates of the same area. As the thickness of a hexagonal plate increases, its capacitance does not increase significantly until the area of the six prism-faces is appreciable, that increase being initially proportional to the ratio of prism-face area to the sum of the areas of the two basal faces. As one generates a dendritic plate from an initially hexagonal plate, only very slight reduction in capacitance occurs until the degree of insection is rather large. On the other hand, addition of even thin radial arms quickly augments the capacitance. Relation of these findings to crystal growth problems will be discussed.

Discussion

SCHAEFER: Do your studies lend any explanation to the fact that thin circular plates of ice (frazil ice) are more stable than hexagonal columns growing in water slightly colder (-0.01 - 0.1°C) than 0°C? Discs will grow at the expense of columns when immersed or on the surface of supercooled bulk water.

McDONALD: This problem is not controlled by diffusion as in the cases described by the theory presented; the conditions of the liquid environment of the crystals control the growth in an entirely different way.

MARSHALL: In the electrical side of the analogue, it is voltage (not charge density) that is assumed constant over the surface. For the ice crystal to which vapour is diffusing, the assumption is made that the vapour density at the surface is the same everywhere. This is hardly valid. Even if the surface temperature were the same everywhere, it is still conceivable that the vapour density would vary with surface curvature and/or vary from one crystal plane to another. As a separate point, since Dr. McDonald took pains to reconcile us to the analogy, it may be noted that an analogue that is more complete is found in the electrolytic tank. I suspect that the effect of the mounting and electrical connection is also more readily reduced.

McDONALD: The uncertainty in the capacitance analogue is limited to corners and edges where Kelvin effects might be important. The electrolytic analogy might, however, be better.

SUTHERLAND: There is available an accurate method of measurement of small capacities called the three terminal method. This would also avoid the problem of the leads. Details are available from the U.S. National Bureau of Standards of the Electrotechnology Division of C.S.I.R.O. I also believe that there are conformal transformation methods which would enable many of the capacities to be calculated.

McDONALD: To do it using a three-dimensional relaxation method on a computer would be very much more expensive than my method.

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RELATION BETWEEN THE ICE FORMING POWER AND THE TRANSPARENCY OF AIR

by

G. Soulage*

Except in close proximity to some sources of ice nuclei, the part played by these nuclei in the extinction of light is always very small because they are not numerous enough.

However a relation of proportionality may occur between the ice forming power and the opacity of air when the ice nucleus concentration is in a constant ratio to the concentration of the particles responsible for the extinction of light. The theory of Mie tells us that, for a standard and homogeneous aerosol, these are "large" particles ($0,1\mu < r < 1\mu$). In practice, one may hope to find a constant relation in two cases: (1) very far from sources of ice nuclei when, after much mixing, the atmosphere becomes homogeneous and each particle gets a probability of being effective as an ice nucleus in direct ratio to its surface; (2) downwind from a polluting source important enough to be the main disturber of visibility and dispersing a number of ice nuclei in direct proportion to the ineffective "large" particles produced with them. In all the other cases, that is to say for the most part, a constant relation between the instantaneous values of the ice forming power and the visibility may not exist.

Numerous measurements of the ice forming power carried out by the author in variously polluted places permit verification of the second case of relation experimentally and show that the first is infrequent. They support the view that a constant relation does not occur in the other cases; however, on an average, low ice forming powers are frequently connected with great visibilities and inversely. Finally, the measurements give some information about the part played by dust concentration and relative humidity of the air in the relation between ice forming power and visibility.

Discussion

BIGG: We have observed in measurements in Australia that ice nucleus concentrations at cold temperatures, say -25°C, are inversely related to visibility, but at warm temperatures, say -15°C, the relation disappears. Have you noticed a similar temperature dependence of the relation between visibility and ice forming power?

SOULAGE: In Europe the ice nucleus count at -15° in creases with decreasing visibility but conditions may well be different from those in Australia due to industrial smoke. The relationship should improve at lower temperatures as more kinds of particles become effective.

GEORGII: With respect to the diurnal trend of freezing nuclei found at Clermont-Ferrand I can fully confirm the results from our measurements at Frankfurt/M. We also found a minimum of concentration in the early afternoon and a maximum during the night. This fluctuation parallels completely the diurnal trend of large particles of the natural atmospheric aerosols. That means we have in the early afternoon a reduced supply of aerosol particles resulting in a reduced number of ice nuclei and a better visibility.

The relation between visibility and ice-forming power is in my opinion also a result of the relation between numbers of large aerosol particles and visibility. Low visibility means high supply of large nuclei.

SOULAGE: I agree, but small aerosols affect this relation. For instance silver iodide has a high nucleating ability but does not affect visibility; it is the same thing for smoke from a steelwork consisting of 1/10 to 1/100 micron particles if they are not accompanied by large particles.

MURGATROYD: The ice forming power of nuclei depends primarily on their composition whereas the visibility must be largely independent of this but rather depends on their sizes and concentrations. In natural conditions it would therefore be difficult to obtain good correlations unless the observation were always made downwind of a specific nuclei source.

SOULAGE: In the case of a homogeneous aerosol the composition is important

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only for the value of the ratio between visibility and ice forming power.

HITSCHFELD: I wonder whether the author could explain in detail the equation he used for "ice forming power" of the air? In particular, could he justify the assumption that each nucleus contributes proportionately to the square of its radius?

SOULAGE: Yes, this assumption is valid for a point far from the source where ice nuclei are formed. At such a point, we may think that after much mixing the aerosol is composed of agregates of various substances each agregate having on the average the same composition and so the same properties for forming ice. As a consequence, each particle has a probability of bearing an effective site in proportion to its surface area. It follows that the ice forming power is proportional to their surface and so to the square of the radius of particles.

LODGE: It seems that you have demonstrated, to the extent that you have any correlation between visibility and freezing nuclei count, that the nature of the population of particles in the range 0.1 -1 is about constant.

SOULAGE: That is correct, applied to one place and far from ice nuclei sources.

ISONO: I noticed as did Dr. Bigg that ice nucleus concentration increase with decreasing visibility at temperatures lower than -25°C. What do you think about the increase of concentration of ice nuclei with increasing relative humidity which you showed in your slide?

SOULAGE: This may be due to the dilution of giant hydroscopic nuclei and the oxidation of metal smokes. In the laboratory, Serpolay has shown that metal particles form effective ice nuclei when humidified.

ON ICE NUCLEI IN THE ATMOSPHERE

by

Kenji Isono*

The result of ice-nucleus counts made at Tokyo shows that the main source regions of ice nuclei in the air over Japan are the arid and semi-arid regions in the continent of Asia, especially in the basin of the Yellow River in the North China. In maritime air-masses the concentration of ice nuclei is usually very low. It was found that ice nucleus concentration increased markedly after eruptions of volcanoes, for instance, Volcano Asama.

Ice-nucleating properties of various rock-forming minerals, volcanic ashes, clay minerals and meteorites were studied in the laboratory. The particles of kaolinite are very active as ice nuclei. Quartz, amphibole, olivine, calcite and orthoclase are found to be less effective. Magnetite, hypersthene and ash from volcanoes, Kusatsushirane, Asama, Sakurajima etc, which eject acidic lava are more active than kaolinite. Bombs from these volcanoes are less active. Loess particles sampled in North China and those found in snow sampled in Japan were found to be the same in their ice-nucleating abilities.

The direct identification of the composition of ice nuclei in the air may be made, to some extend, with the electron microscope and X-ray microprobe analyser, but it is impossible to apply such a method in the identification of nuclei observed every day. We can presume the material of the main component of nuclei in an airmass, tracking backwards the trajectory of the airmass and comparing the temperature dependence of nucleus concentration observed with the temperature spectra of known materials determined in laboratory.

The ice-nucleating abilities of salt particles produced by drying out sea water were studied. Calcium and potassium sulphate are effective, while sodium chloride thus produced is not effective at -15°C. By spraying sea water into a cold camber kept at temperatures above -20°C no ice crystal appeared. This result agrees with the observational result that ice nucleous concentration is very low in maritime airmasses.

As magnetite and hypersthene are very effective as ice nuclei, there may be a possibility that meteoritic dust, which contains magnetite or some minerals similar to it or chorudrite which contains hypersthene, acts as an ice nucleus.

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Discussion

GIBBS: In view of the fact that large oil and forest fires have been observed to generate cumulo-nimbus clouds from which substantial amounts of rain have fallen we should admit the possibility that the apparent connection between rainfall and volcanic activity may result from convection originated by the heat contributed by the volcano rather than by the increased number of freezing nuclei generated by volcanic activity.

ISONO: Neither heat nor water vapour are released from volcanoes in quantities enough to explain any but very local effects; but the correlation is observed over much larger areas and must, therefore, be due to nuclei.

VITTORI: Did you find any relationship between volcano activity and the fall of hail?

ISONO: Dr. Komobayasi of our laboratory found that the frequency of thunderstorms is closely correlated with the instability coefficient. On days of eruptions however, the number of thunderstorms is less for a given instability coefficient than it is on normal days. Apparently eruptions tend to suppress thunderstorms.

SULLIVAN: Have correlations between ash particles from solid fuel combustion and ice-forming nuclei been attempted? Under normal circumstances these would probably be the most numerous aerosols to be found in the lower atmosphere in the significant size range.

ISONO: Particles from industrial sources form no ice crystals at -15°C whereas particles of volcanic ash are active at -13°C and very active at -15°C.

SOULAGE: Concerning the effectiveness of sea sprays, I have found no enhancement of the ice forming power on introducing them into a cloud chamber. But this does not permit one to set aside all contributions of the sea in the ice forming power of air. Carrying out measurements on the Atlantic coast, I have found high ice forming powers when the wind was blowing from the sea. These seemed due to an activation of continental nuclei (metallurgic nuclei) in contact with sea sprays or only in wet air.

ISONO: Sea sprays does not form ice nuclei active at temperatures warmer than -20°C. It acts only when dried out into salt particles.

KLINE: Were your tests with sea-salt and its components showing correspondence with the Washington, D. C., data conducted with the expansion or mixing chamber technique?

ISONO: The mixing type.

SANGER: What is the purity of the solutions, of which the ice nucleability was measured, and how have these materials been dispersed?

ISONO: The purest grade commercially available was used. They were dispersed by heating a platinum wire on which they had been deposited.

STRATOSPHERIC AEROSOLS AND ICE NUCLEI

by

E.K. Bigg*

Numerous sources have been proposed for natural atmospheric ice nuclei, but the conspicuous world-wide tendency for high concentrations to recur on certain calendar dates, identical with those of rainfall anomalies, supports Bowen 's hypothesis that meteor dust is an important world wide source. Comparable concentrations of ice nuclei at ground level, upper troposphere and lower stratosphere (in sharp distinction to the rapidly diminishing concentration with height of terrestrially produced Aitken nuclei) found by Telford, is consistent with this view.

Collection of particles on "Millipore" cellulose filters, and their subsequent activation has been used to sample stratospheric ice nuclei with the aid of meteorological balloons. Results showed concentrations of ice nuclei in the height range 13 to 20 km to be considerably in excess of normal ground values. A similar technique was used when filters were flown for us by the U-2 aircraft of the United States Air Force "Project Crowflight" in November 1960, and showed appreciable concentrations of very

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active ice nuclei at 20 km on the same days.

Of the large particles thought to have been collected in stratospheric flight the following were conspicuous: (1) Thin opaque plates of metallic lustre, ranging from nearly black to gold or silver. (2) Opaque black shiny particles. (3) Dull opaque particles, granular in appearance and black, grey or yellowish. (4) Translucent grains, white or pale yellow to red. (5) Spheres some of which were brightly coloured and drop shaped particles, or particles with a vitreous appearance.

Chemical tests showed that many of the metallic particles contain nickel and iron and that many of the granular types are coated with iron oxide.

The ice nucleating ability of individual particles has been variable, with none particularly active, but as yet the sample tested cannot be considered representative. The relatively large size and porous structure of these particles may allow them to influence cloud development irrespective of their ice nucleating ability, which may perhaps be acquired by their sweeping up aerosols of ground origin which would otherwise remain at cloud level.

Discussion

VITTORI: Concerning the absolute value of the concentration of the large particles you find, Dr. Lodge said that there may be a critical limit of capturing large particles, because they can rebound from the filter during the collection. So the actual concentration can be higher than the one you find.

BIGG: This would be possible in the U-2 tests, but in the balloon tests sampling rates were low (5 litres per minutes) and rebounding would be unlikely.

SOULAGE: I think that the way followed by Dr. E. K. Bigg is the best for resolving the problem of meteoritic nuclei. His technique is objective and good for this work and one can hope that by carrying out measurements at higher altitudes with rockets, it will be possible to learn what are the numbers of nuclei produced by the fall of meteorites and on what dates they appear.
But two points must not be forgotten:

(1) It is possible that the combustion of meteorites produces very small nuclei (diameter less than 0.1 microns) by recondensation of vapour. These cannot be counted by the technique of Dr. Bigg because many will not be collected by the filter or will not act after deposition on it. One may contemplate counting these nuclei by taking samples of air at high altitude and introducing them in a cloud chamber on the ground. These nuclei being very small, their sedimentation will be slight if the walls of the boxes used for the sampling are kept warm.

(2) In the comparison between the ice-forming power at high altitudes and on the ground, we must not forget the expansion of air. One nuclei per litre on the ground becomes one-tenth nuclei per litre at 16 km altitude if any mixing does not occur.

BIGG: Certainly much remains to be done. It is to be hoped that the simple start which we have made will lead to more advanced techniques in the future.

HOSLER: I would not be dismayed by the lack of ice-nucleating ability of particles brought from the stratosphere to the ground. If the particles were maintained at subfreezing temperatures until tested for nucleating ability, they may well be quite active. The preactivation in the stratosphere may be preserved in the natural case until they reach cloud levels.

BIGG: It is a promising approach which will be tried.

GEORGII: Have you observed any correlation between radio-activity and freezing nuclei?

BIGG: I have no evidence on this.

MOSSOP: Further to Dr. Hosler's remarks on the effect of low temperatures on the nucleation ability of particles, we have made a few preliminary experiments which show that "pre-activation" of some of these high-latitude particles is possible.

The particles are cooled to -40°C, humidified so that ice forms on all particles, then warmed so that the ice is seen to evaporate. If the particles are then brought up to -10°C and humidified, far more produce ice crystals than would do so without this "pre-activation". This retention of an ice embryo upon certain particles is very dependent on the under-saturation to which they are subjected and further tests at controlled humidity are necessary.

MARSHALL: Dr. Bigg noted a lack of evidence of layers descending through the stratosphere. Surely in view of the high winds that are possible, a layer would have to be very extensive before its gradual fall could be measured.

BIGG: But very extensive layers not changing height at one point in the course

of a week is just the sort of evidence we have.

GHOSE: It would be interesting to check up on the presence of the man-made nuclei which are being produced in the stratosphere whenever the carriers of the satellites burn up on re-entering the Earth's troposphere. They could produce snowfalls.

BIGG: I have no evidence on this matter.

McDONALD: In abandoning the concept of a highly uniform fall-speed of meteoritic particles, are we to understand that your group is turning to some new alternative to the 28 $\pm$ 2-day fall-time in matching rainfall singularities with dates of meteor showers?

BIGG: The fact that I cannot explain an observation does not mean that the observation is wrong but merely illustrates my ignorance.

MURGATROYD: The evidence of stratification in the lower stratosphere is in agreement with that obtained from measurements of the vertical distribution of ozone using the chemical radio-sonde recently developed by Dr. Brewer at Oxford. A recent study of wind variation in the lower stratosphere by Sawyer also shows considerable stratification.

It might be worth while to consider using a high-altitude searchlight in Australia for investigating aerosol content in the stratosphere. This would avoid also some of the difficulties inherent in the twilight method. Recent work in the U.K. shows that light scattered down from the searchlight beam at levels up to 80 km or so is much greater than could be due to air molecules alone and a large proportion must be due to dust particles.

BIGG: This experiment would be very desirable in Australia, but our resources have not yet allowed us to undertake it.

SULLIVAN: The high ice-nuclei peaks shown by Dr. Bigg reveal a close similarity to the usual diurnal smoke density cycles observed in various part of the world, including the east coast of New South Wales. This is normally explained by the "Hewson" fumigation mechanism.

BIGG: Yes, this is the type of mechanism which produces the peaks, but the particles involved may be different.

VI. ICE CRYSTAL NUCLEATION

A YEAR'S STUDY ON FREEZING NUCLEI VARIATIONS AT GROUND LEVEL AT DELHI

by

Bh. V. Ramana Murty and A. K. Roy*

The results of measurements of freezing nuclei concentration in surface air layers at Delhi, in northwest India, during the period of one year (June, 1960 to May, 1961) are presented. Using equipment similar to the one adopted by Bigg for his ealier measurements (liquid air, instead of dry ice, has been used by us as the cooling agent), the number of ice crystals formed at temperatures, ranging from -10°C to -30°C, have been determined. From the temperature - ice crystal count curves, based on observations on individual days, temperature values corresponding to 10 freezing nuclei per litre and also their concentrations at two fixed temperatures, -15°C and -20°C, have been noted, and their mean values for each month worked out. Comparisons of the monthly mean values bring out a clear tendency for the nuclei activity to vary with the season, the average for monsoon months being much higher than that during winter. One striking departure from this general trend, shown by unexpectedly high nuclei concentration during October, 1960, which is a transition month between the monsoon and winter seasons, would need to be explained suitably if corroborated by further measurements.

Measurements on individual days have shown marked fluctuations in nuclei concentration from one day to another, the activation temperature relative to 10 nuclei per litre varying on occasions by as much as 10°C or more in course of a day. Instances of sharp rise in activation temperature, marking dates of peak nuclei activity, have been studied with reference to weather or air mass changes and also to possible influence of meteor showers occuring during the preceding 4 to 5 weeks, as postulated by Bowen. 22 out of 29 dates of peak nuclei concentration during the period under study are found to agree well with peak rainfall dates for Rajasthan, based on means of five stations.

Discussion

McDONALD: Does the low-level airflow arrive at New Delhi from the southwest (across the dust-source regions of the Thar Desert and the Sind) frequently enough to explain the observed monsoon increase of nuclei counts as due to dust?

ROY: The situation you describe is rather unusual during the monsoon season. The usual air trajectories in the monsoon season are either across Rajasthan or more frequently from the East up the Gangetic Valley to Delhi.

No difference was found between the freezing nucleus concentrations in the two situations.

GEORGII: I should like to support Mr. Roy's statements with regard to freezing nucleus-counts on days with showers and continuous rain.

On days with showers we always found a freezing-nucleus concentration above average while on days with continuous rain the freezing-nucleus counts are generally low. High counts on days with showers are probably connected with the down draught of so-called "trained" nuclei, while low counts on days with continuous rain are probably caused by particle wash-out.

NAQVI: The monsoon air reaching Delhi from the east passes over the Gangetic Valley which is quite well-developed industrially. Would not this air be rich in nuclei produced in industrial activity?

ROY: As I mentioned earlier, we could find no significant difference between monsoon air approaching Delhi across Rajasthran and that coming from the East up the Gangetic Valley. The influence of industrial activity, if any, was not detectable.

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ICE NUCLEI CONCENTRATIONS AT MOUNT WASHINGTON DURING THIRTEEN YEARS

by

J. Schaefer and H. C. S. Lanford*

Since January 1948 when the senior author initiated routine 3-hourly observations of ice nuclei concentrations at the Mt. Washington Observatory (6344 ft. M. S. L.) in the State of New Hampshire, U. S. A., more than 36,500 observations have been made. The data now available constitute the most extensive record in time of routine observations of ice nuclei concentrations in existence.

All of the measurements were made with the mixing chamber technique as developed by Schaefer. The 100-liter cold chamber was lined with black velvet to eliminate contamination by fragmentation nuclei. After introducing an air sample from outside the Observatory, a period of two minutes elapsed before a cloud was formed in the chamber. Most observations were made at a temperature of -18 to -20°C. During recent years a considerable number of runs were made at warmer temperatures (-10 to -15°C).

An extensive analysis of the data is now under way. All of the local weather parameters have been put on punched cards by the U. S. Weather Bureau and the special nuclei information is now being added. A series of correlations will be run to determine whether a preliminary indication of a small positive relationship with the so-called "Bowen Peaks" can be supported by a more rigorous analysis.

The presence of ice nuclei storms of high concentrations which characterized the data prior to 1954 has been found to be nearly absent during the later period of 1954-1961. These high count periods sometimes lasted for several days. Although there was a tendency for these high concentrations to occur more frequently during the winter months, some outstanding "storms" occurred during the summer months. Case studies generally showed that these unusually high counts were in air which had a trajectory from the arid south-western United States. In a number of instances the air was found to be associated with severe dust storms in the desert regions.

In one outstanding example an ice nuclei storm accompanied a vast cloud of smoke from a forest fire more than a thousand miles distant which took 24 hours to pass the station. The ice nuclei concentrations increased by five orders of magnitude as the smoke engulfed the mountain, and then dropped to the previous low levels as the smoke zone passed eastward.

Much of the preliminary analysis of the Mt. Washington ice nuclei data shows a fairly high relationship to probable terrestrial sources of such particles. Under most conditions the air passing the summit of the mountain is of continental origin. This could mask possible correlations with the "Bowen Peaks", since those correlations which have been established thus far in the southern hemisphere and other locations show total concentrations, even on high-level days, considerably lower than the values found at Mt. Washington much of the time.

Further studies of the data will be made using a 1620 Computer. Various relationships with atmospheric phenomena will be run in the near future. This should establish whether north-eastern America is a likely area to study the "Bowen Peaks" and should show other relationships with synoptic and mesoscale weather phenomena.

Discussion

JAFFE: Knowing that high radioactivity has been observed on days of a double tropopause, wouldn't it be worthwhile to try a correlation of high peaks in freezing nuclei counts with high radioactivity or even with the occurrence of the double tropopause?

HOSLER: We have noted that high freezing counts coincide with the ouflow from the thunderstorm or shower and that Sr^{90} concentrations in rain water also are increased during showers and thunderstorms above that normally found in steady rain.

BIGG: Was the air intake to the Mt. Washington cold chamber heated?

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SCHAEFER: No, but the procedure was to wait for 2-3 minutes so that any ice crystals from outside could fall out before humidifying.

BIGG: Is it possible that the ice had only partially evaporated from the nuclei?

SCHAEFER: Yes, this is possible but I do not consider that it was important. We obtained equally high counts in the summer when there was no possibility of such errors.

MURGATROYD: It is interesting that ash from forest fires was found to act as freezing nuclei on Mount Washington while ash from newly-burnt wood and fir cones did not. I wonder whether this might be a case of "trained" freezing nuclei as the former must have spent some time at low temperatures before sampling.

SCHAEFER: This cannot be ruled out, though I have not been able to reproduce such "training" in the laboratory.

MURGATROYD: On the whole, which school of thought do your data support, the Bowen theory or the idea that ground particles are carried upwards and activated at low temperatures?

SCHAEFER: We have found that some high counts are very definitely due to dust storms in the west which have been followed all the way to Mount Washington. But I cannot be categorical about a terrestrial origin of the freezing nuclei that cause the peak of, for example, January 13th.

BIGG: As you have said we are evidently measuring different phenomena, since 40/litre is the highest concentration of freezing nuclei we have found at -20°C in Australia.

SHAEFER: At present we ourselves do not find the high counts that we used to. Unfortunately as far as we know, no one else has carried out a continuous series of measurements over this 13 year period.

SMITH: Have measurements been made of other physical parameters which could be correlated with the freezing nucleus measurements?

SCHAEFER: We have good data on visibility and 6-8 years of radioactivity measurements. Mr. Lanford will be examining parameters of this kind for possible correlation with freezing nucleus counts.

SULLIVAN: Measurements of dust in the lower atmosphere in Sydney showed some exceptionally high concentrations in the late 1940's period, particularly 1947, as far as I can recollect. On certain days dust counts of siliceous material were of the order of several hundreds to two thousand particles per c.c. compared to a normal range in the city of 5 to 50 particles per cubic centimetre.

SCHAEFER: We have found high counts associated with dust falls in Texas, which coloured the surface of the snow yellow. These dust particles were found to be active as freezing nuclei at about -12°C.

In New Mexico and Arizona I have observed very thin clouds to snow actively following dust storms.

SHUMWAY: Because of the occurrence of high nuclei counts attributable to continental sources, wouldn't it be unrealistic to expect the Mount Washington data to support the Bowen hypothesis (without suitable adjustment)?

SCHAEFER: I think we should not allow the high counts which we obtained in the earlier years to mask possible tests of the Bowen theory. The data of the later years should therefore be tested independently.

SOME ASPECTS OF THE MEASURABILITY AND INTERPRETATION OF NATURAL ICE NUCLEUS FLUCTUATIONS

by

D. B. Kline*

This paper summarizes the major results from a program of natural ice nucleus measurements sponsored jointly by the U. S. Weather Bureau and National Science Foundation with the assistance of a number of cooperating groups. In addition to coordinated daily measurements during selected periods, considerable attention has been devoted to diagnostic studies of sampling variations, compatibility of instruments and techniques, and the identification of possible synoptic meteorological linkages with

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anomalous counts. Among the results emerging from this investigation are the following:

(1) Internal data consistency checks using the USWB version of the Bigg-Warner expansion design showed evidence of a highly "measurable" phenomenon (within a factor of 2 at -20°C under field usage) provided careful consideration was given to replication procedures and other quality control considerations;

(2) An examination of the compatibility of expansion and mixing chamber methods on the other hand revealed that the latter gave higher values by factors of 5 to 10 at -20°C, and that the differences displayed evidence of nonuniformity between low and high concentration levels, although both methods reflected parallel changes during appreciable increases or decreases with a correlation coefficient of about 0.8;

(3) With special reference to Washington, D.C. and Mauna Loa Observatory data, there are strong indication that atmospheric circulation parameters, low level aerosol sources, and airmass influences may be significant, if not dominant, factors contributing to major changes in concentration levels as measured with current techniques, and

(4) The previously marine contribution to positive concentration anomalies in the Washington, D.C. area has been confirmed by measurements within a few hundred feet of the Atlantic shoreline, but Hawaiian data showed lowest values during flow regimes directly from the Pacific ocean, thus suggesting that sea salt per se should be ruled out as a possible ice nucleating agent. Median concentration levels at different U.S. sites have varied over approximately two orders of magnitude.

With respect to the singularity hypothesis, although some interesting coincidences in ice nuclei anomalies at ground level at widely separated places in the U.S. have been observed, it is suggested that these could be the consequence of synoptic meteorological factors rather than reliable indications of sources external to the atmosphere. Confirmation of the latter will require a more direct attack. There is also room for concern that refrigerated cloud chamber devices of reasonable dimensions may tend to activate particles unrepresentative of those utilized by natural clouds. Further research will be required to resolve this question.

Discussion

BOWEN: I have two observations to make on the excellent paper we have just heard:

a) It is very gratifying to see so much emphasis being placed on obtaining a proper physical understanding of exactly what is being measured in a given type of cloud chamber and on the necessity for careful calibration.

b) I think it must be obvious from this and earlier discussions that some peculiar effects on the freezing nucleus count are bound to occur if measurements are made in dust-bowl regions, near volcanoes or steel works, under the influence of forest fires etc., and that any comment on the possibility of some freezing nuclei being of extra-terrestrial origin can only be made on the basis of measurements made in a region which is reasonably free from contamination or in the atmosphere at heights of 40,000 feet or more.

MURGATROYD: It is important to keep in mind that both for studies of natural rainfall production and also checking Dr. Bowen's hypothesis, that we are really interested in freezing nuclei which cause natural cloud drops to freeze, i.e. the freezing of droplets of the order of tens of microns radius at temperatures around -10°C. Mr. Kline has shown that mixing chambers give spectra about 5°C warmer than expansion chambers, probably because they contain larger droplets. Even so, they do not properly simulate conditions in natural clouds and work with freezing nucleus chambers will not be satisfactory until this difficulty can be overcome instrumentally or a reliable method of predicting from the cloud chamber readings what would happen in natural conditions.

MARSHALL: Following on Dr. Murgatroyd's comment, I wonder how closely the -10°C count can be forecast from the -20°C count. That is to say, if an expert in this field is given the count from a box working at -20°C, how closely could be estimate the count that would be yielded by a box, hypothetical if practical difficulties are involved, working at -10°C?

KLINE: Slopes of temperature spectrum concentration curves vary widely from day to day, so much estimates are unreliable.

LODGE: Coarse molecular sieve granules in a shallow bed will remove gaseous impurities (including water) from a gas stream without disturbing the aerosols. It would be interesting to see the effect of such treatment on freezing nuclei counts. Certainly this should decrease the frost problem.

SCHAEFER: I would like to raise the question as to whether care is being taken in sampling for ice nuclei that the sampling is done on a 24-hour day rather than the normal working day (0800-1700)? There is a danger that significant effects may be lost due to the working habit of the operator. This points up the great value of using automatic counters which can monitor continuously.

KLINE: We found slightly higher counts in the early morning measurements in the Washington area, but this trend was not consistent from site to site in the U. S. A.

GIBBS: It is obvious that the nature and number of naturally occurring or artificially induced nuclei are important in determining the nature and number of condensation products. However, there is still uncertainty regarding the extent to which the kinematics and the rainfall yield of the cloud are altered by differences in the nature and number of nuclei present. My feeling is that cloud producing moderate or heavy rain will have its kinematics or rainfall yield altered little by the nature and number of the nuclei.

On the question of the correspondence of rainfall occurrences with high nucleus counts referred to by Mr. Kline, I wonder whether the evaporation of raindrops could contribute significantly to the freezing nucleus count. Presumably all evaporating raindrops originally containing freezing nuclei or having been frozen, will yield freezing nuclei. This may have a bearing on Mr. Kline's observation that freezing nucleus counts are larger in the northern hemisphere. A possible explanation is that the northern hemisphere counts are made in higher latitudes where warm rain is proportionately less frequent than in the lower latitudes of the southern hemisphere.

BOWEN: The Bergeron theory provides an obvious connection between ice nucleus concentration and rainfall.

McDONALD: The suggestion that the evaporation of raindrops near the ground during a rainstorm initiated by the Bergeron effect should yield anomalously high ice nucleus counts seems to overlook an essential feature of the Bergeron process: to make it go to completion, it is important not to have too many ice nuclei per million cloud drops, so each raindrop arriving at the ground would, even granting complete evaporation, release only a few ice nuclei, at most.

KLINE: The coincidences between rainfall and ice nuclei anomalies shown in our U. S. data, while interesting, cannot be construed as proving that nuclei are a significant cause of the large-scale rainfall. There are subtle problems of a "hen and egg" nature involved. We believe that the most likely explanation is that, in our area, moisture for widespread precipitation and the nuclei tend to have a common origin. This is not to imply that the nuclei are irrelevant to the precipitation process.

With respect to increases in ice nuclei counts with the onset of precipitation, we have occasionally observed this phenomenon, but not in any consistent or regular pattern.

Another speculative consideration might be related to the work of Twomey. The drying out of soil particles or possibly the "splattering" of the raindrops on the dry ground in the case of the onset of precipitation may occasionally account for the release of ice nuclei as well as the hydroscopic substances studied by Twomey. We are, in any case, confronted with the possibility in our U.S. data that precipitation-producing storms may, in effect, "generate" their own nuclei due to linkage of nuclei to air mass, circulation parameters and possibly other mechanisms, e. g. activation, etc.

ROY: So far as Delhi observations went, the association found between nucleus concentration and rainfall has been that the concentration is high after very light rain or temporary showers, and is low when observation follows prolonged rain. In case of precipitations of the former type, some amongst the very small raindrops or droplets produced as a result of shattering would tend to evaporate, leaving free some of the freezing nuclei floating in the surface air layers. This, at any rate, was the kind of explanation that I was inclined to give tentatively to account for observed sharp rises in nucleus concentration following showers, a large majority of which were found to be associated with cold convective clouds.

SINGULARITIES IN ICE NUCLEUS COUNTS

by

E. K. Bigg and G. T. Miles*

There is now a considerable body of published evidence of the existence of worldwide singularities in the mean daily concentration of natural atmospheric ice nuclei, which shows remarkable agreement with the dates of rainfall anomalies demonstrated by Bowen.

For two years in Australia, continuous automatic ice nucleus counters were operated at fixed temperatures during December and January, and fluctuations of the mean daily concentration of ice nuclei followed this pattern of singularities very closely. The mean and peak levels were similar on the two years, and the continuous records showed some interesting details of the ice nucleus "storms". Many of the fluctuations in concentration were brief and because they occurred at times after sunrise which varied with season, have been assumed to be due to the overturning of layers rich in nuclei situated above the measuring site, and caused by the onset of convection. The growth and decay of such nucleus showers is characteristic, and is typical of the majority of showers even though some of these occur during the night. It is concluded that a similar mechanism is responsible, although the onset of transfer of the nuclei from higher levels must be due to other factors. The size distribution and magnetic properties of the nuclei were studied during other months and neither was significantly different during nucleus storms, although on one occasion at least, a burst of large ice nuclei preceded a major storm.

A new method has been developed for routine measuring of mean ice nuclei concentrations over any specified period, which has the virtue of great simplicity. Air is drawn through a cellulose filter having a sub-micron pore size by a small pump at, say, 10 l/min. In the course of a day $14m^3$ of air can then be sampled, and activation of the nuclei carried out at relatively warm (and meteorologically interesting) temperatures. The procedure adopted was to cool the filter to the desired temperature, humidifying it by producing a cloud in the air around it (but keeping it covered to avoid ice crystals falling on to it) then rendering the activated ice nuclei visible by pouring on them a supercooled sugar solution. An unexposed control filter was developed simultaneously to check the technique. The results showed a mean concentration somewhat lower then obtained with other nucleus counters, possibly because the smaller nuclei become lodged in the pores of the filter and are not activated, and again convincingly demonstrated the Bowen singularities.

Discussion

SCHAEFER: I suggest using a Gelman millipore strip automatic sampler with high flow operation. These could be exposed for 5, 30 or 60 min. periods.

BRAHAM: To what size do you permit the ice crystals on millipore to grow prior to covering them with sugar solutions?

BIGG: It is difficult to get these to grow large as the millipore appears to act as a sink for moisture. We normally humidify and then pour on the supercooled sugar before the crystals have grown to visible size.

IRIBARNE: With reference to the shape of the changes in concentration during the "nuclei storms", I find easy to understand the sudden rises, but not so easy to accept that the turbulent mixing should lead to a decay of the exponential type. This decay rather suggests a mechanism of inactivation which could be described as a first order chemical reaction.

McDONALD: The turbulent stirring of such a surface layer of high nucleus count, that Dr. Bigg adduces to account for his "nucleus storms", will tend to approximate to just the kind of first-order process that Prof. Iribarne suggests.

SULLIVAN: We find that atmospheric pollutants exhibit similar behaviour to that described by Dr. Bigg. We have also recorded sudden increases in concentration shortly after sunrise and the mechanism is confirmed by the fact that this happens 2-3 hours earlier in summer. Hewson demonstrated simultaneous SO_2 increases at various points downwind of a particular source of the gas, caused by similar turbulent mixing.

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SOULAGE: Do the variation curves of ice nucleus concentrations obtained with cloud chambers and with millipore filter technique display the same features? I ask you this question because comparing measurements of ice forming power carried out with a cloud chamber and carried out in collecting nuclei on glass slides in a cascade impactor, I have not found variation curves having the same form. This had led me to avoid the latter technique for measuring the general ice forming power of air.

BIGG: We have not yet made systematic comparisons over a period, but the checks that we have made indicate that the millipore counts are down by a factor of 100 as compared with the cold box. We may be either (i) not achieving saturation or (ii) losing the smaller particles.

LODGE: It should be noted that particles considerably smaller than the pore size are still retained on the surface of membrane filters. I have seen evidence to suggest that MF of 0.65μ pore size retained sodium chloride of 0.02μ size at the surface.

BIGG: My suggestion is that many of the smaller particles are retained in the tubes of the millipore. This is based upon a test upon a silver iodide aerosol in which, although no active nuclei passed through the millipore, the count of active nuclei on developing the filter was down by a factor of 100 on that expected.

GEORGII: I would like to know whether you have an idea of the size range of meteoric dust particles reaching the ground?

BIGG: I do not know. The black particles that we have collected are commonest in sizes less than 1 micron diameter, concentration decreasing with increasing size.

GEORGII: I would also like to mention that the increase of condensation-nucleus concentration shortly after sunrise is a regular phenomenon which you observe when continuously recording the number of condensation-nuclei.

DWYER: Has any correlation been established between wind direction and the arrival of the nuclei "front", having in mind the desirability of excluding the arrival of pollutants from industrial sources as being the cause of sudden rises in the crystal counts?

BIGG: Analysis of a year's data hour by hour showed that counts during a South wind were up by a factor of 2 as compared to counts when the wind was in other directions. The higher average was due to 1 or 2 sudden large influxes. There was no evidence of an industrial source.

McDONALD: The great potential importance of the Bowen hypothesis in settling the broad question of seedability of clouds, plus C.S.I.R.O.'s evident ability to do the difficult and the involved, leads me to suggest two further experiments that might illuminate important aspects of the hypothesis.

a) Operation of one or more high- volume samplers of the type used by the U.S. Public Health Service in its recent air pollution studies might permit chemical identification of any constituents characteristic of the "nuclei storms", e.g. magnetite which has today been intimated as a rather good possibility.

b) Use of some sizing technique to get at the size distributions of ice nuclei characteristic of "nuclei storms" could aid in settling the crucial fall-time question. For example, use of a Goetz Aerosol-Spectrometer, with suitable post-collection development of the size-spectrum deposited along its channels, might serve such a purpose.

LODGE: If iron analyses are of interest, the Gelman polystyrene filters should be used for collection, rather than the Public Health Service standard glass filters, which are contaminated with iron.

SWINBANK: I find it rather odd that, if we concede that there may be singularities in daily rainfall, and if it is proposed to seek an explanation for these in terms of freezing nuclei e.g. meteoritic dust, those occurring in the month of January in Sydney should be chosen for demonstration. This is the time of year of highest freezing level, about 16,000 feet, and presumably also the time of year with the highest proportion of "warm-type" rain, about two thirds of the atmosphere involved in the production of rain being above 0°C. I should like to ask whether anybody present would provide an estimate of the proportion of warm rain in Sydney at this time of year.

O'MAHONY: I have analysed the weather situations responsible for the heavy falls of rain during January in the Sydney area, and most of these are associated with northern troughs and therefore warm type rain.

BETHWAITE: The experience of our seeding crews does not support the view that most rain in January comes from warm clouds.

VII. NATURAL RAINFALL

ICE FORMING ACTIVITY AND SURFACE PROPERTIES OF NUCLEATING MATERIALS

by

R. Sanger, G. T. Barnes and U. Katz*

Recent experiments in our laboratory give some insight into the mechanism of ice crystal nucleation by silver iodide and other powders. In cloud chamber experiments with fogs formed from salt solutions of various concentrations the ice forming activity was found to depend on the saturation of the air relative to ice, whereas the saturation relative to water was not important. It appears that a given activity value is obtained at the same ice supersaturation and that temperature is only a significant factor through its effect on the ice supersaturation.

These results indicate that the ice embryos are in equilibrium with the vapour phase and can only become stable when the free energy of deposition of vapour into the crystal lattice is sufficiently large. The necessary free energy will of course vary from site to site. From a kinetic point of view this means that the rate of deposition of water molecules must exceed a critical value for the embryo to grow to a stable size. In this way a mechanism analogous to the fluctuation theory of homogeneous nucleation can be envisaged.

It can also be shown that the nucleating activity does not depend on the amount of adsorbed water present on the nuclei, but, as the humidity relative to water was always higher than 80%, this may only mean that under the experimental conditions the formation of embryos was not hindered by a lack of adsorbed water.

Nevertheless the results of proton spin resonance measurements on the adsorbed water layers on nuclei indicate that the behaviour of the adsorbed water has an important bearing on the ice nucleating activity. The water adsorbed on various powders was observed to undergo a phase transition from a liquid-like to a solid-like state when the temperature was lowered. Furthermore the onset of this transition corresponded reasonably well to the temperature at which appreciable ice forming activity had been observed in the cloud chamber.

It appears therefore that incipient ice embryos form in the adsorbed water layers at temperatures determined by the substrate and the nature of the site, but that their development to stable embryos which will grow to ice crystals is determined by the supersaturation of the air with respect to ice.

Discussion

SUTHERLAND: An electrolyte like HF can be incorporated in ice. Presumably this will affect the vapour pressure P_i of the ice and consequently the free energy change for the transition vapour to ice. Has this been attempted as a further check on your relation?

SANGER: No, the experiments reported represents the first group of investigations of that kind. However, the suggestion of Dr. Sutherland is very interesting, also in relation to Dr. Granicher's work (Zurich), who studied the dielectric properties of ice contaminated by HF.

SUTHERLAND: I am considerably interested in the orientation changes induced in adsorbed films as shown by line widths of NMR. Similar cases have been investigated for porous silicas and carbon where there is a change in dielectric loss at about -30°C. Perhaps a relation to nucleation can be established here also.

SANGER: Recently we began with measurements on silicon and found no change to -30°C. Barnes will soon publish a paper on it. The active surface of AgI is 04m^2/gm

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In the active surface of AgI is 0.4 m^2/gm, and of CuO 12m^2/gm. In the case of silicon we are on the way to finding whether it freezes in the pores below the surface or on the surface. It is most important to have a large active surface and we now have a device for measuring this by the BET method.

BARKER: Was the AgI used in the proton resonance experiments exposed to water vapour or liquid water?

SANGER: It was sealed into a tube having an atmosphere saturated at 0°C and remained there for three days before the experiment.

McDONALD: If one examines the usual type of nucleation rate equation one finds dominant influence of the factor $e^{-\Delta G/RT}$ where ΔG is the molar Gibbs free energy of the phase transition. Since

$$\Delta G = RT.\ln(p_w/p_i)$$

it follows, if we ignore some less important T-dependent terms, that the rates and hence the threshold of nucleation should occur at very nearly a constant vapour pressure ration p_w/p_i. The slight departure from constance of p_w/p_i can probably be accounted for in terms of the equally slight T-dependence of the pre-factor of $e^{-G/RT}$ in the rate equation. Thus your salt-solution droplet experiments are apparently interpretable as a nice confirmation of this feature of the nucleation-rate equation.

IRIBARNE: Have you any explanation for the discrepancy of results with those of other authors, such as Hallett and Mason?

SANGER: There is no strict discrepancy from Mason's observations, if you realise the curves representing these measurements consist of two linear parts with a pronounced bend between.

THE ADSORPTION OF WATER VAPOUR ON SILVER IODIDE

by

A. R. Blake and R. C. Seymour*

The adsorption of water vapour on silver iodide has been investigated in the temperature range 35 - 60° C using a quartz microbalance. Heats of adsorption have been determined for a wide range of surface coverage and in the multilayer region it has been shown that the heat of adsorption approaches a value close to the heat of sublimation of water. The amount of vapour adsorbed was large compared with that normally expected for a physically adsorbed system and it may be concluded that this is connected with the persistence of an abnormally high heat of adsorption in the multilayer region. Calculation of the thermodynamic properties supports the view that water molecules are highly oriented on the surface, perhaps in an ice structure even, above the normal melting point.

The importance of these findings in nucleation of supercooled water vapour by silver iodide is discussed.

Discussion

SUTHERLAND: In the many theories about freezing and sublimation nuclei I think that attention should be drawn to the fact that even below water or ice saturation there are many monolayers of water already on the surface of AgI. The distinction between freezing and sublimation becomes somewhat academic.

SANGER: That is why we say "ice-forming nuclei" so that we escape the difficulty. How many molecular layers of water were there on your AgI?

BARKER: About 9 at relative pressure 0.6. Under the same conditions Birstein's results indicated 10 times as much adsorption.

SANGER: We have strong indications that the AgI used in our NMR experiments has a film less than 10 molecular layers in thickness.

BARKER: The present results are apparently inconsistent with Sänger's because the energy is nearer to the latent heat of sublimation of ice than to the heat of vapourization of water even at temperatures well above 0°C.

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McDONALD: It is encouraging to see some new work on water adsorption, since meteorologists have had to rely for half a dozen years on Birstein's work, which has some disturbing implications. From Birstein's published isotherms I compiled a differential heat of adsorption of about 16 kilocalories per mole even out at 10 monolayers from the interface, which seemed quite absurdly high compared with the heat of sublimation.

Have the authors established any adsorption isotherms at the sub-zero temperatures that are of chief concern to meteorologists?

BARKER: No, it has not yet been contemplated.

McDONALD: If there are 6 layers at +35°C perhaps there are only 2 at -10°C.

SUTHERLAND: I know of no substance that gives such a variation in adsorption of water as does AgI. Between different samples, the amount varies from less than a monolayer at say 0.7 - 0.8 saturation to some 50 or more. Nonetheless, adsorption of nitrogen or of alcohol does not exhibit these variations. There is some need for isotherms at lower temperatures.

THE EFFECT OF SURFACE CHARGE ON THE NUCLEATING PROPERTIES OF SILVER IODIDE

by

L. F. Evans*

The striking similarity between the crystal lattices of silver iodide and ice, led to the conclusion that the efficiency of a substance as an ice nucleator depended on the closeness of fit between the ice lattice and the lattice of the nucleating substance. This view persisted for some years. But it has become increasingly apparent that nucleation is not governed by lattice spacing only and that a good nucleator must possess some other characteristic also. By taking into account the entropy changes involved in growing ice on a polar substrate, Fletcher has predicted that the second necessary characteristic of a good nucleating surface is that the surface should not polarize the ice which grows on it. As a first approximation, this requires that a uni-univalent substance such as silver iodide should exhibit a surface comprised of equal numbers of positive and negative ions.

In order to test the effect of polarization it is necessary to vary the polarity of the nucleating substrate without altering the other properties of the nucleus known to influence nucleation. This requirement is met by determining the freezing point of droplets of aqueous solutions in which silver iodide particles are immersed, the polarity of the particles being controlled by adjusting the concentration of potential-determining ions in the droplets.

As Fletcher's theory would predict, nucleation is most effective in the vicinity of the isoelectric point of silver iodide where, statistically at least, oppositely charged ions are present in equal numbers on the silver iodide surface.

It may be of considerable practical importance that the negative charge which silver iodide acquires even in pure water is sufficient to hinder nucleation. This suggests a new approach to the search for the best cloud-seeding substance - namely, that the substance should be at the isoelectric point under the conditions prevailing within a supercooled cloud droplet.

Discussion

GEORGII: This is a nice confirmation of Hosler's work and our own on natural aerosols, regarding particle size dependence.

BRAHAM: Could you please explain again why you would expect increased nucleating ability when the nuclei are prepared in an excess of silver ions?

EVANS: The only point is that a slight concentration of silver ions is needed to offset the natural tendency for the surface to have a negative charge.

BRAHAM: Therefore one would not want too many silver ions?

EVANS: No. The activity decreases considerably if the concentration of excess

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silver ions is increased to 10^{-3} Molar.

VONNEGUT: Is the excess iodide ion as produced in commonly used AgI burners a good thing?

EVANS: No, our conclusion was more the other way. An excess of I would be a bad thing as the system is so sensitive to iodide. That is why we think a silver-rich mixture, such as one having $AgNO_3$ in the AgI solution, would be better.

SMITH: We have made some tests which may be relevant as to the effect of NaI in cloud-seeding solutions of AgI. A hundred-fold increase in the normal concentration of sodium iodide ions was tried, but the ice nucleation properties were unaffected.

EVANS: You could have been doing so much damage with the NaI already present that a little more wouldn't matter.

VITTORI: Can your explanation help in understanding the photolytic deactivation of AgI?

EVANS: No. I know of no effect on photolysis of adsorbed Ag ions.

RECENT DEVELOPMENTS IN THE THEORY OF ICE CRYSTAL NUCLEATION

by

N. H. Fletcher*

Whilst many aspects of nuclear behaviour in the ice-water-vapour system can be given a fairly satisfactory treatment on the basis of classical nucleation theory, it now seems that several interesting phenomena depend for their interpretation on a consideration of the ways in which ice differs from a simple solid. This work is still largely spleculative, but already points to some interesting new possibilities.

Because a water molecule is far from being a symmetrical dipole, the molecules in a water-vapour interface are strongly oriented, and it can be shown that the preferred direction of orientation is that in which the negative molecular vertices are directed outwards. The energy gained by such orientation can be evaluated in a quite straight forward way. Because of the high degree of bonding in the water structure, such orientation is not confined to surface layers, but decays approximately exponentially below the surface with a characteristic distance of about 10 molecular layers.

This mechanism provides an immediate explanation for the observed fact that condensation of water vapour occurs much more readily on negative than on positive ions.

When a crystalline ice surface is considered, such orientation is no longer energetically advantageous, since the essentially complete bonding in ice makes the relaxation depth much too great. If, however, one considers a structure in which the ice surface is covered by a thin quasi-liquid layer in which the surface orientation can relax, then it can be shown that at temperatures above about -30°C such a structure has a lower free energy than does a crystalline ice surface. The discussion also yields, to a first approximation, the thickness of the liquid layer, as a function of temperature. In the temperature range 0-30°C the film thickness is of the order of some tens of Ångstroms, decreasing as the temperature falls and vanishing below about -30°C.

Such a structure is not of course new, having been proposed by Faraday as long ago as 1850, but this theory establishes its thermodynamic equilibrium and makes some numerical predictions.

If ice has such a liquid surface layer, then this has several interesting consequences. From the nucleation point of view it means that at temperatures not too far below 0°C no rigourous distinction can be drawn between sublimation and freezing nuclei, though the curves previously derived for these cases still retain much of their validity if appropriately considered.

The existence of such a highly mobile quasi-liquid surface film will also have an important bearing on the shape of ice embryos and on the growth of ice crystals, topics which have yet to be considered in detail.

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Discussion

GEORGII: Does your theory suggest that if the temperature is raised from a low value to one higher than 0°C the liquid film would retain an ice-like structure for a while and perhaps be able to act again as a freezing nucleus only little below zero centigrade when recooled?

FLETCHER: I rather doubt whether the liquid film would have any icelike structure, but ice embryos might be retained in steps and cracks.

MARSHALL: I suppose the liquid film concept would be less relevant in picturing freezing nucleation?

FLETCHER: The theory of freezing is complicated and I have concentrated on discussing the sublimation case first.

MARSHALL: How does one get at the equilibrium vapour pressure of ice when it is pictured as covered with a liquid film? And will the effect of curvature be just as for liquid?

FLETCHER: A molecule can jump from the ice into the liquid and a compensating molecule from the liquid to the vapour. Since in equilibrium the film is restricted to a fixed thickness, the situation is thermodynamically the same as the case of ice without a liquid film. The effect of curvature will be mainly on the outside surface, since most of the surface energy is associated with this. Because of the small thickness of the layer, this makes no practical difference.

MAGONO: Consider a snow crystal of a hexagonal column type having planes perpendicular and parallel to the C axis. There are differences in the rates of growth of these two planes, especially near -15°C. Is your theory consistent with the fact that the difference depends on temperature?

FLETCHER: The theory as yet is too undeveloped to help with discussion of this problem. I suspect that the liquid film would simply speed up surface diffusion, the crystal habit being determined in some way by the underlying ice structure.

IRIBARNE: Does the decay in the orientation of water molecules along the adsorbed layer mean that defects of orientation are being introduced from each layer to the following one?

FLETCHER: The defects responsible for decay of orientation are essentially an unbalance between broken bond-ends of either type in the water structure. Some of the negative bond-ends may be regarded as having migrated to the liquid surface.

SUTHERLAND: I am not clear whether Dr. Fletcher believes that nucleation of water or water vapour to ice is unusual and that his orientation (entropy) effects are responsible for this. It would be instructive to see if nucleation of other liquids (particularly those not involving hydrogen bonding) was different. This would then give a clue to the importance of his model in ice nucleation.

FLETCHER: Many other liquids with polar molecules show marked surface entropy deficits and presumably have oriented surface zones. It is possible that in some cases the solids may have anomalous surfaces. Examination of nucleation effects would certainly be interesting.

HOSLER: 10 years of experimentation with ice have led me to promote an idea such as Dr. Fletcher has expounded, that the surface of water has an oriented surface structure and that ice has a surface film of liquid even to temperatures of -25°C. With reference to Dr. Magono's question, I can say that experiments with ice crystals show that the basal planes of crystals are stickier than the sides, indicating that the liquid film may be thicker on the basal plane.

FLETCHER: Difference in film thickness upon different crystal faces is a second order effect which the present theory is insufficiently detailed to cover. Perhaps it might be possible to treat this later by extending the theory.

ICE NUCLEATION BY SOME ORGANIC SUBSTANCES

by

R. B. Head*

A number of organic substances have been found to nucleate ice. The activity of these depends to a large extent on the physical form in which they are prepared. Nucleation takes place at specific sites, and especially at cracks. Particularly active sites are produced by crystallising from the melt.

While the crystal structures of few of the steroids have been determined, it appears that, owing to the parallel stacking of the molecules and their molecular dimensions, a correlation with the ice structure may be possible.

After interchanging substituents at opposite ends of the molecule it appeared that the essential position for the hydrogen-bonding group was position 3 in ring A. Where this group lies at right-angles to the molecular plane, nucleation is prevented, evidently by steric effects.

A further class of nucleators has been found in some fluorene derivatives. One of these, 9-hydroxymethyl-fluoren-9-ol can be prepared from the melt in such a way that the onset temperature is as low as -2.5°C.

Nucleation has been shown to occur with compounds prossessing -OH, =O, or NH_2 groups.

Discussion

ISONO: Dr. Komobayasi of our laboratory has also made experiments on ice nucleating properties of organic substances and found that some are very effective. Have your effective ice-nucleating substances modified the growth habit of ice crystals, for we have found that with some organic substances (which have chemical bonds similar to those you have tested) their molecules are easily adsorbed on ice crystal faces.

HEAD: They appear to be more spiky than crystals grown on silver iodide.

GHOSE: I will send you a list of 80 organic substances used to make rain by putting them on sacrificial fires in India.

HEAD: I would very much like to try some of them.

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VIII. HAIL

ON THE PHYSICS AND METEOROLOGY OF HAIL

by

H. R. Byers*

Studies of hail have been carried out by two associates - Dr. Tetsuya Fujita on the meteorological side and Mr. Phanindramohan Das on the physical side.

Computations have been made on the growth of hailstones in a model cloud derived from the "mean hail sounding" in the Denver area. The computations are based on the Schumann-Ludlam formulation, slightly modified on observational and conceptual grounds. One modification was to include the effect of <u>all</u> the water intercepted by the falling hailstone on the heat balance at its surface. The suggestion that the water that does not freeze for lack of sufficient heat transfer remains on the hailstone surface has been incorporated in some computations. It is found, from the particular cloud model adopted, that the effect on the rate of growth is not remarkable.

In part of the computations the cloud model has been deformed in the vertical by wind shear. The result indicates that one would expect a higher probability of hail in thunderstorms formed under conditions of strong vertical shear. However, under the assumed conditions the growth of a hailstone will be greater in a cloud with no shear than in one with a strong shear, other conditions such as the strength of the updraft, the vertical distribution of liquid water and the temperature being the same.

In studies of the structure and circulation of hailstorms, photogrammetric and mesometeorological analysis are used. From the hail cloud of 11 September 1958 near Cheyenne a model was evolved which consisted of a large, nearly steady updraft with a very restricted down draft. Strong winds above 10 km were an essential part of the circulation. As a further consequence of the wind distribution, the air in close proximity to the cloud was cooler than in the undisturbed environment. Whereas in the ordinary essentially hail-free convective storm alternate zones of pulsating updrafts and downdrafts are found, in the hail cloud the updraft was found to predominate and to persist.

A hailstorm in the plateau area near Flagstaff, Arizona, which produced a considerable accumulation of small hailstones appeared to be different in several respects from the Cheyenne storm. It occurred without appreciable wind shear, and reasonably detailed studies have failed to reveal any characteristic that would distinguish it from an ordinary convective shower. Some of the 1961 hailstorms are being studied to obtain further details.

Discussion

HITSCHFELD: What is the biggest size of hailstone which your theory can reasonably be expected to yield?

BYERS: It would have a radius of the order of 1 cm. To get a larger stone would require a different cloud with a lower cloud base. We have yet to understand why the hailstones occuring in the Denver area are larger than our computed values. It may be that the cloud base is lower than in our model.

MARSHALL: Possibly it is not necessary for theory of this sort to account for the largest stones. Is the structure of the very large stones such that they can all be attributed to the aggregation of several or many stones, each of which has grown to a fair size by itself?

BYERS: From looking at cross sections of the stones it does not look as if there is agglomeration. However, melting could cause uncertainty in interpretation.

GIBBS: Prof. Byer's vertical motion model apparently retains the field of vertical motion in a fixed geographical position with the horizontal motion "blowing through" the cells and transporting the hail through the cell. Is this correct?

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BYERS: Perhaps I did not make it clear that the horizontal velocity was the shear velocity, taken as zero at the cloud base and 20 mps at 5 km above the base.

ROY: Leaving out some of the very much exaggerated reports about unbelievably large hailstones having fallen in certain parts of India, there is ample reliable evidence to show that hailstones of sizes 3 to 4" diameter (or even more) often fall in northern India, particularly during pre-monsoon months, March to May. I have with me a photograph showing very extensive damage caused by hailstones to a Viscount aircraft while flying at a height of 19,000 ft. on 27.5.59, about 70 miles WSW of Delhi. The largest hole made in the body of the aircraft, of diameter about 5", suggested that some of the hailstones were of that size; one of the dents caused on the aircraft body covered an area of about 15" x 10".

BYERS: Marshall's suggestion that agglomeration applies may account for this; the ordinary growth equations certainly cannot.

GROWTH OF HAILSTONES IN A RAIN-FILLED ENVIRONMENT

by

R.H. Douglas, W. Hitschfeld and J.S. Marshall*

Recent wind-tunnel observations by List and by Macklin suggest that hail stones can grow without disintegrating even when containing large fractions of liquid water. This finding has removed a serious difficulty in hail theory: the limitation to growth supposedly imposed by the slow disposal of the latent heat when supercooled water is frozen onto the stone. Apparently, the ice content develops as a complex but open structure capable of containing large amounts of water in its interstices. The growing hail can thus hold onto all the cloud water with which it coalesces.

The terminal speeds of raindrops are comparable with updraught speeds. Because of this, it is possible to have concentrations of rain water considerably greater than the concentration of cloud water that would exist in the absence of precipitation of cloud to rain by coalescence. The concentrated rain represents a large store of water substance which greatly speeds up the growth rate, to produce big stones in the 10 to 20 minute periods in which this has been observed to happen.

Limits to this rapid growth are imposed by progressive freezing of rain and cloud. The probability of this freezing increases with height, so that where the temperature is just less than 0°C, only a few drops are frozen (which provide the required hail embryos). At temperatures -15°C and colder, nearly all the rain is frozen, making for a much reduced rate of hail growth; at temperatures -30°C and less, only the unfrozen cloud water remains to contribute.

Trajectories of growing stones, constructed with these factors in mind, indicate that for sustained and efficient hail development, growth starts low in the cloud and is confined to a relatively narrow region bounded by the 0° and -25°C isotherms. Under the conditions prevalent in Alberta, where our observations are made, this layer extends from about 3 to 7 km above surface. The conditions for efficient hail growth are fairly critical: an updraught speed of 15 m sec^{-1} is more than enough, and the average total water content (cloud plus rain), while it needs to be greater than 10, should not exceed 30 gm m^{-3}. These seem to be quite realistic values.

Discussion

BRAHAM: I am a bit concerned regarding progressive freezing of drops at the temperatures shown in your graphs. On the basis of laboratory experiments we have come to believe that freezing of cloud droplets is not likely at temperatures relevant to this problem viz. -10°C to -25°C. Of course our experiments suffer from the very fact that they were laboratory experiments and we realize this, so we have gone into the clouds to "take a look". Here we find other surprises. In summertime, in non-hail-producing cumuli over Central U.S. we have been surprised to find unusual numbers of ice particles at rather warm temperatures. Therefore our observations would be better fitted if your theory involved collection of ice crystals rather than frozen water drops.

HITSCHFELD: As long as these ice particles have enough mass, this would not affect our story. In our field studies in Alberta we have been collecting hail and using the melt water for experiments in the freezing of droplets. The results of these have been used in the calculations reported in this paper. Of course, results obtained from the freezing of droplets on cold plates are certainly open to criticism, but what I should make very clear is that the details of the theory do not matter too much: we have used Bigg's theory, but any other phenomenological theory would be as good. We do require that under given conditions, large drops freeze faster.

TELFORD: With 20gms/m^3 of rain, coalescence, leading to large drops which shatter, must be very prevalent. Does this permit the 20 gms/m^3 to be sustained?

It is notoriously difficult to suspend large water drops without sideways motion. Will sidewise motion not seriously deplete the liquid rain water concentration? In connection with these questions, how long does it take to reaccumulate this liquid water concentration if it were suddenly removed? What is the time constant of rain storage recovery?

HITSCHFELD: It is quite possible that the drops shatter but I doubt if this would deplete rain storage. Drops will tend to be swept sideways but will be replenished. I do not now know the time taken to reaccumulate the water, but I expect that this can be worked out from a consideration of the fall rates of cloud and rain drops.

MACKLIN: An important assumption underlying these computations, is that it is possible to store rain. In Browning and Ludlam's model of the cumulonimbus, the updraught increases with height from about 15m sec^{-1} at 0°C to 40m sec^{-1} at the -40°C level. Is it possible to store raindrops under these conditions?

HITSCHFELD: I am afraid my use of the term "storage" has caused confusion. It is not necessary that the raindrops are held fixed in place. All that we are saying is that an updraft slows down the fall rate of rain (or even reverses it), and that as a consequence of this simple fact there will be more water substance in unit volume than there would be in rain in the absence of an updraught.

Regarding the quoted figure of 40m sec^{-1} updraught. I am a bit doubtful as to how realistic it is.

SQUIRES: Have you taken into account the effect of those large concentrations of rainwater on the buoyancy of the updraught?

MARSHALL: Yes, the negative buoyancy due to the liquid water has been considered. It will of course to a large extent be counter-balanced by the positive buoyancy associated with the latent heat released by the progressive freezing of the raindrops.

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GROWTH OF ARTIFICIAL HAILSTONES

by

S.C. Mossop, A.E. Carte and R.E. Kidder*

Hailstones which are symmetrical about a growth centre demonstrate by their structure that they have grown by accretion while tumbling randomly in sub-cooled cloud. Many deviations from such perfect symmetry are found, e.g. conical and disc shapes, and these indicate that the stone spent at least part of its growth period in a preferred orientation relative to the airstream. Conditions under which such shapes are formed have been investigated in the laboratory.

Starting with ice spheres of diameters 0.2, 1 and 2cm, fixed at the mouth of a vertical wind tunnel, their growth was investigated in a sub-cooled cloud at temperatures down to -20°C, and liquid water contents between 1.5 and 14 gm/m^3. The artificial hailstones were fixed to the arm of a drag balance; by adjusting the upward airspeed

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this could be kept in the null position so that the hailstone weight was balanced by the aerodynamic drag and the stone was effectively falling at terminal velocity relative to the airstream. The rate of spraying of droplets into the airstream was adjusted to keep the liquid water content constant throughout an experiment. The hailstone was kept under observation to detect whether there was free water on the surface.

The resultant shapes are greatly influenced by whether the ice growth is "dry" or "wet". "Dry" growth produces accretion in the shape of a cone, apex upwards, apex angle about 40°. Under "wet" growth conditions, water is driven back towards the equator of the stone and the shape tends towards that of a flattened disc. As shown by Macklin and by List, the water is held in a mesh of inter-penetrating ice crystals.

Natural disc-shaped stones were closely imitated by growth on a 2mm embryo at high liquid water content (approximately 7 gm/ m^3 and temperature warmer than -10°C. Such stones grew with the shortest axis vertical and in this orientation had drag coefficients as high as 1.2.

Ice layers of uneven thickness in hailstones can be formed when the stone preserves a constant orientation relative to the airstream for a short time. This may happen under "dry" or "wet" growth conditions, but if the liquid water content exceeds a certain limit, the resultant flattening of the growing layer will produce a double maximum in the thickness instead of the usually observed single maximum.

Under dry growth conditions, rotating the hailstone about an axis at right angles to the airstream can produce a flattened stone with indentations at the ends of the axis. This supports Maier's explanation of the mode of growth of apple-shaped stones. However, examination of thin sections of natural stones of this shape indicates that the indentations were produced by fracture of the stones, presumably by the freezing of internal water.

Discussion

BRAHAM: I second the speaker's inference that flat hail stones may develop from the cracking of larger stones. About 5 weeks ago while tent camping in south central Missouri, U.S.A., I experienced a hail fall of stones up to 2" diameter. Among these were several which were flat. These flat stones were similar in every way to thin sections through large stones. Concentric rings of micro-and macro crystalline growth, perpendicular to the flat sides of the stones, were clearly visible in these flat stones. One infers that these resulted from in-flight splitting or rupturing of large quasi-spherical stones. This inference is strengthened by the observation of a stone which split into two fragments after coming to rest on the ground. I suggest the possibility that this in-flight rupture of hail stones resulting, perhaps, from thermal stress, may be responsible for both the flat stones and the "apple" shaped stones. The latter, perhaps, are flat stones which have undergone further growth after splitting.

HITSCHFELD: Have you any information as to the fraction of liquid water you can have in your stones?

MOSSOP: No. We were interested mainly in explaining the growth of the flattened stones. It is hoped to repeat the experiments later with this point in view.

HITSCHFELD: I hope you will find it possible to rock your hail stones to simulate a little more closely the obviously much less steady motion of the stones through the air.

MOSSOP: I am sure you are right. This is a refinement for the future.

TELFORD: The shape of the hailstone in dry growth appears to differ according to the rate of growth. This implies that surface migration of the water occurs. Is this likely to be so or have J misinterpreted the slides?

MOSSOP: No. In dry growth the stones show similar growth shapes. There is however a wide transition between dry and wet growth when there could be spreading of water over the surface.

ON THE GROWTH OF HAIL

by

W. C. Macklin*

Using experimentally determined values for the density of ice formed by the accretion of super cooled water droplets, calculations have been made for conditions appropriate to the formation of soft hail and of hailstones. In a cloud whose base is at -10°C, 900mb the density of soft hail pellets a few millimetres in diameter may be expected to be less than 0.2 to 0.3 gcm^{-3}, being generally in the range 0.05 to 0.15 gcm^{-3}. The density of soft hail pellets grown in a cloud whose base is at 20°C, 900 mb is likely to be in the range 0.4 to 0.7g cm^{-3}; such pellets can be initiated either by ice crystals or by small frozen raindrops. The mean density of hailstones of 1 to 2 cm diameter is inevitably high regardless of the density of the core due to the growth of high density outer layers.

The opacity of ice formed by accretion is dependent on the temperature of the surface of the deposit which is higher than that of the environmental air due to the release of heat of fusion. At low values of the surface temperature the droplets freeze individually from the outside inwards thereby entrapping the dissolved air and giving rise to opaque ice. Clear ice layers are formed when the surface temperature is raised to 0°C and the surface is covered by a thin liquid film through which the dissolved air escapes. Computations based on the assumption that the transitions from opaque to clear ice in hailstones occur when the surface temperature just reaches 0°C, show that the structure of spherical stones is consistent with their having risen steadily through the supercooled region of a parent cloud having a liquid water concentration of about 4g m^{-3}. This is possible in Browning and Ludlam's model of the cumulonimbus since the updraught increases with height.

Discussion

MURGATROYD: I think it would help the discussion if a short description were given of Ludlam's recent model. This model will of course allow some recycling of the stones within the storm and this, if it occurs, will considerably affect the growth calculations.

MACKLIN: One of the important features of Browning and Ludlam's model, which can be seen in a diagram, taken from an article by Ludlam in the May (1961) issue of Weather, is that the updraught is inclined in the lower levels. This permits re-cycling of the hailstones which are thrown forward from the top of the updraught, and this leads to further growth. I have not attempted to deduce the actual trajectories of the stones as this depends on a detailed knowledge of the air motion in the storm. What I have shown is that spherical stones can be grown in the model by rising steadily throughout the supercooled region at about the "just wet" condition. The reason for this is that the updraught increases with height.

THE GROWTH AND DISTRIBUTION OF LARGE HAIL

by

P. Squires**

The geographical and seasonal distributions of the occurrence of large hail present a number of peculiarities which are not readily explained. One of these is its preference for the interiors of continents. This is no doubt related to the thermodynamic instability of the air masses in these regions especially during spring and early summer, but this does not appear to be the whole explanation.

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It is known that the microstructure of cumuli in these regions, especially in hot dry weather, is radically different from that in maritime air masses. This has been explained by the large concentrations of cloud nuclei in continental air, apparently originating from the dry continental surface; it is likely therefore that the same difference in microstructure would be found between maritime and continental cumulonimbi. The typical continental cumulus consists of a large number of small drops, and displays marked colloidal stability, coalescence of droplets being inhibited by low gravitational collection efficiencies.

It is possible that in maritime cumulo-nimbi some of the precipitation results from an all-water coalescence process. This may be considered as affecting the production of large hail in two ways: 1) the efficient formation of precipitation particles may possibly reduce the liquid water content of the cloud before the ice phase appears at all (in the absence of an established kinematical model, this is very dubious) and 2) in a cloud where much of the liquid water is present in the form of large drops, the growth hail-stones, falling with high velocity, may be severely restricted by the splashing which occurs on impact. Some experiments (preliminary) have been made to study the impact of droplets on a wet ice surface. The onset of splashing seems to be little affected by supercooling in these circumstances, and measurements in the range d = 6.0 to 0.9mm indicate that it occurs when $v^2d > 10^4$cgs approximately.

This inequality seems also to agree with the experience of Latham and Mason (Proc. Roy. Soc. 260 (1303), 537), working with droplets of diameter about 100 microns, although it is not known whether, in this case, the ice surface was wet.

The mean droplet spectrum found in cumulo-nimbi by Weickmann and Kampe' indicates that, at a hailstone velocity of 20m sec^{-1}, about half the liquid water content of the clouds was in the form of droplets large enough for some splashing to occur, on the basis of the inequality given above. Thus, it seems likely that, in discussing the growth of hail, it may be necessary to take into consideration not only the concentration of liquid water present, but also the manner in which it is subdivided.

Discussion

MARSHALL: The possibility that a change in number of cloud droplets would change the production of hail is interesting. Given a large number of condensation nuclei, might it be practicable to add something to the atmosphere such that cloud droplet numbers would still be relatively small?

SQUIRES: It would be necessary to "poison" the existing nuclei to prevent them becoming active and this would be exceedingly difficult.

DESSENS: I have to say that in France the SW coastal regions often receive large hail. The situation is the same for some Mediterranean coastal regions.

SQUIRES: It would be interesting to know in what synoptic situations these storms occur.

DESSENS: Ahead of an advancing cold front, aligned roughly parallel to the Atlantic coast, with south west winds aloft, and easterly winds at the surface.

SQUIRES: A good deal of sizeable hail occurs on the east coast of Australia, typically however also ahead of an advancing cold front, with westerly winds. The air in which the hail clouds form is continental. Perhaps the same may be true on the Atlantic coast of southern France, since the low level winds are easterly.

McDONALD: High-speed motion pictures of splash processes suggest that a full-fledged splash involved penetration by several drop diameters. Films of thickness less than this may yield distinctly different splash dynamics, with the result that hailstone film-thickness (and laboratory film-thickness) may exert considerable influence on splash processes. Before drawing final conclusions it would thus seem wise to do tests in which the water film thickness span the likely values that might arise on actual wet hailstones.

SQUIRES: I agree. We did not notice the effect you mention; splashing does not occur until there is some water on the ice surface and while it did not appear necessary for the film to be very thick we have no measurements of film thickness.

ROY: I should like to comment further on the point brought out by Dr. Squires about considerable heights being reached on occasions by some of the Cb. clouds over North India during monsoon season and yet these being not associated with well marked hailstorms. A radar survey conducted at Delhi for 3 years has shown that, while radar echo of Cb. cloud up to heights of order of 55 to 60 thousand feet at times was apparently not associated with hailstorms of any significance, more limited build up to heights of the order of 40 to 45 thousand feet during premonsoon season was accompanied frequently by severe hailstorms. I wonder if the explanation of this lies in the possibility that the build up is much more rapid or the updraught is much stronger in Cb. clouds during premonsoon months, when instability is more pronounced, and that growth of Cb during monsoon season is comparatively sluggish.

SALT DISTRIBUTION INSIDE HAILSTONES

by

O. Vittori *

Assuming that ice crystals resulting from condensation of vapour contain less salt than water droplets formed on condensation nuclei, a chemical micro-analysis of the salt of the hailstone sections has been carried out.

Each hailstone was sliced into a number of sections, 1 to 2mm in thickness, using a hot wire, and the melted water absorbed by chromatographic paper. Representative slices, in particular the ones containing the core, were photographed after being put on a gelatine plate chemically treated for chloride analysis. These were placed in a box which was cooled to -25°C and which could be evacuated.

The ice sublimed at the pressure of saturated vapour at this temperature; on complete evaporation the salt particles contained in the hailstones were left on the surface of the gelatine. (The behaviour of the slice was observed by a lapse rate movie camera).

The gelatine was heated by blowing in filtered warm air causing the chemical reaction to occur. Photographs were then taken of the distribution of the reaction rings and these were then superimposed on the photographs previously taken of the hailstone sections.

They show that the symmetry of the opaque and clear layers inside the hailstones is respected by the distribution of the salt. This should be related to the manner of freezing of the accreted water. The opaque layers contain more salt than the clear ones and ther is a concentration of the chemical reactions at the boundary between the clear and the opaque layers.

Although at the present moment there is no very satisfactory theory on the growth of hailstones, some suggestions are presented here according to the present knowledge in this field.

Discussion

SUTHERLAND: What is the distribution of salt when water is frozen in the laboratory? In particular is the salt concentrated at crystal grain boundaries?

VITTORI: If you freeze a cylinder of salt solution the freezing pushes the salt in front of it leaving a clear cylinder of water. There is no evidence of salt at the grain boundaries formed on freezing.

MACKLIN: In interpreting the salt concentrations found in the cores of hailstones, consideration must be given to Ludlam's suggestion that the cores may be drops produced by the melting of small stones in the base of the cloud. This is also pertinent to the question of whether large condensation nuclei are necessary to produce the drops which freeze and grow into hailstones.

VITTORI: Yes, this is true.

DESSENS: The sodium chloride nuclei were scattered and always numerous in Vittori's slide. Is not this fact in favour of the hypothesis of a solid growth of the hailstones, without great volumes of liquid water?

VITTORI: I believe that the salt tends to concentrate as freezing progresses.

KENNEDY: One slide showed a hailstone with an opaque outer layer and a salt concentration in this layer. Could you explain the preferential separation mechanism in this case.

VITTORI: It could be due to dry growth even in this case.

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BRAHAM: Might it not be due to contamination in handling?

VITTORI: No. We were very careful in our laboratory techniques.

SARTOR: Could not the concentration of salt on the outside opaque layer be due to the evaporation of an outside clear layer as the hail falls out of the cloud ?

VITTORI: This could be true; the results of this analysis will be explained better as the knowledge of the structure of clouds progresses.

LODGE: The localization of chloride in the spongy, opaque layers of a hailstone may be at least partly caused by the extrusion of salt from the clear layers immediately beneath.

VITTORI: Yes, I agree.

MURGATROYD: It has been found by Wilson, analysing snow from mountains far removed from the sea that the relative concentration of sodium and chloride compared to that of the other elements e. g. potassium, magnesium etc. is reduced compared to that in sea water in the bulk. This has also been shown by Oddie to occur in samples of cloud water obtained by aircraft in the U. K. This must be taken into account in any attempt to draw quantitative conclusions from chemical analysis of hailstones.

LODGE: It is probably, on geochemical grounds, more proper to speak of a surplus of other salts, such as sulphates, rather than a deficiency of chloride and sodium in cloud water.

VITTORI: The distribution must be considered relatively as between the different kinds of ice in the hailstone.

THE EXPERIMENT ON HAIL PREVENTION IN MENDOZA (ARGENTINA)

by

H. N. Grandoso and J. V. Iribarne*

The aim of the experiment is to investigate whether it is possible to diminish the damage caused by hail in cultivated areas (mostly vineyards) near Mendoza, by means of the method of cloud seeding.

Investigations are carried out after an agreement between the University of Buenos Aires (Department of Meteorology in the Faculty of Exact and Natural Sciences), the National Meteorological Service and the Institute of Agricultural Insurance of Mendoza, an official institution in which producers are represented. The experiment is conducted by the University.

Experimental area and seeding technique

The experimental area has about 1,000 square miles of flat country, around the city of Mendoza, just at the east of the Andes. The mountains are very high in this latitude, with the highest point - the Aconcagua - surpassing 7,000 meters.

All the seeding has been made up to now with silver iodide generators from the ground. The generators are based on charcoal impregnated with an acetone solution of silver iodide, placed in wire baskets which are suspended inside a chimney about 1 meter high. Their efficiency has been determined to be in the order of 10^{12} nuclei/sec, or 10^{14} nuclei/ gram of AgI, active at -12°C. They are operated by farmers.

20 generators were located on the east slope of the Andes, and 100 were distributed in the experimental area, in rows perpendicular to the prevailing direction of the wind (E), placed 10-15 km away from each other, while in each row generators are 4-5 km apart from each other.

It is expected that in the third and subsequent years of the experiment the ground seeding shall be complemented by seeding from a small aircraft, at the base of the clouds.

Operation system

The storm forecast is reported at noon to an operation center. The center decides at random whether to seed or not and reports the decision to the local broadcasting

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stations. At 1 o'clock pm, the broadcasting stations report the orders to the farmers, who start seeding with generators for 10 hours (2 loads of 5 hours for each generator). This time schedule is based on the hourly distribution of thunderstorms in the area, which shows a maximum at 20 hs. pm, local time, and practically no hailstorms between 3 and 11 a. m.

During the first year that the experiment ran randomization was made on all storms. During the second year the randomization was made separately for local air mass and for frontal storms, due to the different behaviour of both types of storm, as far as damage is concerned.

Evaluation and duration of the experiment

The statistical evaluation shall be based on the reports of more than 10,000 farmers to the Institute of Agricultural Insurance, which performs the inspections and evaluates the hail damage. These data are recorded in punched cards.

The tentative statistic variable is the logarithm of the "total damage", defined as the sum of the areas affected during the storm, multiplied by the percentage of the damage suffered in each area.

The experiment is planned to last a minimum of 5 years. Two years have run: 1959/60 and 1960/61. The analysis of the results of these two years is now under way. The hail season extends from October to March.

Instrumentation and special research

A complete synoptic study of the convective activity in the area was undertaken from the beginning of the project.

The observational foundation of such study is provided by the surface and upper air networks, mainly of Argentina and Chile, as far as the synoptic scale is concerned. The local observations include two daily radiosoundings, two weather radar 3cm wavelength, one of which is mounted on a trailer, 16mm film time-lapse cameras, whole sky time-lapse cameras with 16mm film, lightning counters, freezing nuclei counters, etc.

There are two aspects of this study which are important in the operational side of the project. One is the elaboration of a semi-objective technique for the forecasting of hailstorms in the area, and the other is the finding of different types of synoptic situations associated with different behaviour of hailstorms, especially with respect to damage on crops.

A short statistics of natural freezing nuclei at ground level was made, by using a Warner expansion chamber. Measurements of natural hygroscopic nuclei at the ground and in aircraft were also made; a cascade impactor was used for taking the samples. The concentration of hygroscopic nuclei is much greater above 3000m of altitude, where the westerly winds coming from the Pacific Ocean prevail, than in the lower levels.

On five occasions of seeded test days, freezing nuclei were counted in an aircraft at an altitude of about 6,000 ft over the ground; the results were compared with non seeded days and the measurements at the ground. The concentration was found to be about one order of magnitude higher on seeded days. This confirms that the turbulent mixing of the lower layers was efficient in transporting nuclei from the ground to the base of the clouds. The measurements were made with a portable cold chamber.

For the future seasons, it is planned to continue with observations and measurements on these lines, to make seeding experiments on isolated cumulus with sodium chloride, to determine the level of initial radar echos in seeded and non seeded storms, and to determine the trajectories of hail shafts.

Discussion

BYERS: I would like to comment on your finding of an increase with height of the number of large or giant particles. As we reported at the first Woods Hole Conference ("Artificial Stimulation of Rain", Pregamon Press) an increase of this character is always found over the continents. It is thought that the particles are removed from the lowest levels by collection on trees, other vegetation, etc.

IRIBARNE: We have a special situation here, close to the Andes. Above a certain level there is always a westerly flow coming from the Pacific Ocean. In the lower levels we have another air mass coming from the eastern sector.

GEORGII: With respect to your measurements of freezing nuclei I should like to ask whether you find a pronounced change of ice nucleus concentration during a dust-storm which you sometimes encounter in Mendoza or during a sudden change of airmasses as you find it connected with a "pampero" situation?

Regarding your measurements of large and giant nuclei I should like to comment that I made some ascents in the aircraft during my stay at Mendoza measuring large nuclei at different altitudes, and I also found an increase of the number of large nuclei in higher altitudes. This increase was not pronounced when the aircraft ascents were made in the area of Cordoba.

IRIBARNE: We have not got a long series of daily measurements, and dust storms are not very frequent so no conclusions can be drawn.

MARSHALL: Has thought been given to the probable effect of seeding a hail-storm with silver iodide: does scientific consideration suggest that the hail will be made more severe, or less?

IRIBARNE: There is too little data on which to form any conclusion yet and no significance can be attached to the statistical analysis so far.

MARSHALL: Is not the damage that you are using as a measure of hail severity actually a function of both hail severity and crop susceptibility? The latter has seasonal variations that could have a serious effect on the results.

IRIBARNE: In principle randomization can take care of this difficulty.

SMITH: It is possible that your seeding may affect other things than hail such as precipitation or lightning. What other parameters are you measuring?

IRIBARNE: We have not so far provided for precipitation measurements but propose to do so. We have records of lightning counts, which have not yet been analysed.

RADOK: Repeating Professor Marshall's question which appeared not to have been understood: What hail-impeding effects were expected to result from seeding?

IRIBARNE: It is hoped that by releasing precipitation before it would fall by natural processes the hail would be smaller.

RAINBIRD: As damage to vineyards is the parameter used in evaluating the efficacy of seeding, what control exists over the repetition of hail over an area already damaged by an earlier occurrence of hail, say within a week or two?

Presumably there is a limit to the amount of damage which can occur; if a vineyard is severely damaged on one occasion, little further damage could result from another severe storm following shortly afterwards.

IRIBARNE: The insurance assessors evaluate the percentage loss of the total harvest. If a second storm follows they estimate the percentage damage on what is left.

VITTORI: Even when hail is small high winds can do much damage.

IRIBARNE: The damage does depend on several factors as well as hail intensity for instance the stage to which the crop has developed, the species of vine, the wind etc. Again, randomization should take care of these aspects.

NEW EXPERIMENTS FOR DETERMINING THE NUMBER OF ICE NUCLEI NECESSARY TO PREVENT HAIL

by

G. Soulage*

Efficient prevention of hail requires the use of ice nuclei effective near 0°C and that a number of such nuclei greater than a certain minimum be brought into clouds. If this minimum is not reached, nuclei may have no effect or a negative effect.

One has tried to determine the minimum number of ice nuclei to be dispersed by looking for the value of the natural ice forming power beyond which it never hails whatever may be the type of cumulonimbus. Our last measurements confirm that such an ice forming power does not occur in France. With this method, we have been able to obtain only an approximate value of the investigated number by taking the highest ice forming power observed during a hail storm and multiplying it by a factor of reduction of the hailstones sufficient for making them harmless. The value so found for the number of ice nuclei effective at -6°C and to be dispersed on a level with this isotherm is about 1 per litre.

In order to obtain a better defined number, seeding of cumulo-nimbus clouds with increasing and well known numbers of ice nuclei are just being undertaken. These seedings will be achieved with a variable number of generators of silver iodide nuclei carried by balloons into clouds and following them during their evolution. Balloons, in comparison with planes, offer the advantage of being able to go along the chimneys of clouds and staying in their supercooled part during a long time. Each generator will disperse 5.10^{15} nuclei active at -6°C in 15 mins above the -5°C isotherm. Their trajectory will be determined with a radar. The effects of seeding on the glaciation of clouds and the form of precipitation will be checked by photogrammetry and with the aid of many observers. For the first experiments, balloons will be flown from the summit of a hill overlooking a great plain and a table land. For further experiments one will try to fly them from a plane in order to avoid missing clouds. The use of parachutes able to ascend in chimneys of cumulonimbus may be also considered.

One hopes to draw from the above experiments a reliable conclusion about the possibilities of preventing hail with ice nuclei and using balloons or parachutes for bringing these nuclei into clouds.

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Discussion

McDONALD: You did not state how you plan to evaluate the efficacy of your seeding experiments. Do you plan to use some randomization scheme?

SOULAGE: No. The results will be checked by time lapse photography. Also a dense network of ground observers will report on the form of precipitation. An instrument being developed may be used to measure the concentration and appearance of ice crystals in the clouds.

MARSHALL: You propose to increase the number of nuclei by a factor 10. Is it your hope that this will increase the number of hailstones by a factor of 10, and will decrease the average volume of ice per hailstone to one-tenth?

SOULAGE: Yes, the hailstone diameter will be reduced by a factor of two. The factor of ten may perhaps be an underestimate of what is necessary but it is an increase on the maximum concentration observed in any hailstorm.

THEORY OF HAIL MODELS OF CONVECTIVE CELLS

by

R. Genève*

One of the earliest theories of hail takes into account the capture of condensed elements by a precipitating corpuscule (coagulation and aggregation). This embryo represent any form of solid aqueous precipitation, or even, exceptionally, an extraneous solid body. A difficulty arises in computing the growth time of hailstones and the maximum diameters, obtained with classical growth equations and observational data on cloud structure. To my mind, the existing measures of liquid water content, ascending vertical velocity, and lifetime of certain convective cells seem indeed inconsistent with some observations of big hailstones. However, usual cells are, perhaps not representative of privileged cells, which have been little explored in the region of total or partial supercooling. In order to study these structures, we tried to use theoretical models.

On a particular example of hailfall, in which radar data were combined with those of a radio-sounding, we compared with observational data the results of the computation of a hailstone's growth, in single models of convective cells. We successively studied the adiabatic model and another with entrainment. Arbitrary choices of values, the range of which being insufficiently known, do not allow to draw a positive conclusion. Nevertheless, it appears that a proper model must take in account the cell dimensions, its heterogeneity and the evolution of outer air peculiarities.

We think that, with the actual state of knowledge concerning hail formation, great importance must be confered to the study of the peculiarities of these especially intense convective cells, which compose hail clouds.

* Meteorological Office, France.

IX. ARTIFICIAL STIMULATION OF RAIN

PROJECT WHITETOP

by

R. Braham *

Still enchanted by the potential benefits of cloud seeding and, as yet, undaunted by comparatively little success in previous experiments, the Cloud Physics Group, University of Chicago, has undertaken another series of experiments aimed at further elucidating the physics of natural precipitation and the possibilities for changing it in useful ways. A decade of experience had shown that those experiments were most productive which placed strong emphasis upon furthering basic understanding of clouds while at the same time providing for a carefully controlled randomized seeding program. Project Whitetop, for the study of summertime cumulus clouds, is of this dual nature.

After careful pre-design studies, an area in South Central Missouri, U.S.A., was selected for this study.

The primary research tools in this project are a 3cm height-finder radar; a twin -engine airplane equipped for measuring cloud liquid water content, precipitation drop spectra, air temperature, air moisture, cloud electric fields and providing a means for inflight sampling for precipitation particles; and a surface network of recording rain gauges.

Operational days are selected objectively on the basis of early morning meteorological conditions. All clouds which form between noon and 10 pm within a 60 mile radius of the radar are routinely examined by the radar on all operational days. The scope of the radar is continually photographed. Silver iodide seeding is carried out from three light planes flying near cloud bases on the randomly selected sample of the operational days. Detailed studies of individual clouds are carried out from the twin-engine airplane both on seeding and non-seeding days.

Analysis of data from the 43 operational days of the summer 1960 is still underway and no statement can be made concerning seeding effects, however, several new and interesting aspects of the cumulus precipitation process have come from the detailed cloud probes. Probably most important of these is our study of the early phase of ice formation in seeded and unseeded cumulus clouds.

Discussion

WARBURTON: I was very impressed with Professor Braham's approach to the problem of measuring the physical parameters of the cloud before and after seeding.

One question which seems to me of importance is: "What happens to the seeding substance when it is released into the cloud?" Mr. Watt of the Australian Atomic Energy Commission and I have been developing a technique which uses γ-emitting radio-isotopes as tracer substances. We have used Xe^{133} and $Cu^{64}O$. The source, up to 10 curies in strength, is released into the isolated cloud and traced by using a scintillation counter. One of the advantages of this technique is that it is not necessary to come into direct contact with the particles to detect them. We have been able to follow the source for periods up to 50 minutes in clear air and 15 minutes in a cloud. It is possible to use directional devices to follow the source. I would like to ask Professor Braham what he thinks of this part of the tracing problem and whether he has made any experimental steps to solve the problem.

A second point that I would like to mention is that concerned with the location of cumulus cloud roots. The scintillation detector we have used is able to see the natural background of radio-activity. The more concentrated Radon-laden air from below can be seen as bubbles of activity at the time of appearance of instability prior to cloud

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development and after the cloud has formed. It may be possible to use a device of this kind to aid the location of the cloud roots.

BRAHAM: I would encourage you to continue with this work and I am sure your measurements are not in discord with our results on cloud roots. We are aware of the problem of locating AgI. The "pigtail" technique developed by Koenig for identifying AgI particles larger than 200 Å seemed to solve this problem, but field experimental difficulties and the possibility that smaller particles than 200 Å are involved have reduced its value. We have found AgI by this technique on some occasions, but more frequently there were no such nuclei present.

BOWEN: Could Professor Braham tell us the height of the terrain, and the mean height of cloud base and freezing level during the experimental period?

BRAHAM: The terrain is 700-1200 feet above mean sea level. The cloud base is 3,000 to 4,000 feet above the terrain. The freezing level during the summer is usually between 15,000 and 17,000 feet.

MORAN: With 43 days of observation it should now be possible to estimate the statistical power of the experiment. Has this been done?

BRAHAM: I cannot yet quote numbers, as we have not yet seen the rainfall data which is worked out by an independent body, the University of Missouri. It is our impression that findings based solely on rainfall gauges are not encouraging.

ADDERLEY: (1) What is the statistical variate derived from the precipitation gauge readings?

(2) Professor Braham mentioned that the ice crystals collected in a cloud in a region warmer than -10°C were not present in a previous passage of the aircraft through the cloud. To what extent does he think the passage of the aircraft could modify the cloud during its first passage?

BRAHAM: The variate for the statistical assessment is the average "in plume" rainfall versus the average "out of plume" rainfall for both seeded and non-seeded days.

I have no idea what the aircraft does to ice crystals. If it makes ice crystals, then perhaps we should be using airplanes for seeding instead of AgI.

WHITE: How do you define the target and control areas?

BRAHAM: We have two kinds of control. The primary test is a comparison of "in plume" rainfall for seeded and non-seeded days. We can make a second comparison between the rain in the plume area and the rain outside the plume area. We place less reliance on this, as we do not know just where the limits of the plume are.

BOWEN: I would like to ask Professor Braham how he defines the position of the AgI plume in each seeding period.

BRAHAM: The lower limit of the plume is considered to be cloud base where seeding is carried out. The mixing we think is due to the thousands of small clouds which form and dissipate. If a cloud extends to 14,000 feet, it will almost certainly reach a region where the AgI will be effective, so that we consider only the winds in the range 4,000 - 14,000 feet to define plume limits. We consider only the most divergent directions at any height in this interval. For the leading edge, only the maximum speed is considered and for the trailing edge, the minimum.

A DESIGN OF A CLOUD SEEDING EXPERIMENT IN ISRAEL

by

S. Jaffe*

A design of a cloud seeding experiment consists of four distinct stages:

1. The climatological survey and the evaluation of the seeding potential.
2. The choice of the seeding agent, seeding equipment and seeding procedures.
3. The statistical design that will be able to detect the true seeding effect.
4. Last but not least, a program of cloud physics studies.

A number of cloud seeding experiments have been carried out in Israel since 1949. They indicated high precipitation amounts in the seeded area but failed to show the true amount of precipitation added by seeding.

*Artificial Rain, Project, Tel Aviv, Israel.

A new experiment has been started in 1960. The preliminary climatological survey indicated that most of the rain-bearing clouds over Israel are supercooled and the major part is cumuliform.

Silver iodide burners have been constructed, according to the C.S.I.R.O. design. Two burners have been installed on a DC 3 aircraft and were operated during the 1960-61 rainy season.

The statistical design of this operation has been prepared by Dr. Gabriel from the Hebrew University in Jerusalem. Two areas, one in northern Israel and the second in central Israel have been seeded according to the random scheme. A random choice indicated which area should be seeded first. This area has been seeded for seven days as far as supercooled clouds existed. After these seven days the other area has been seeded, again for seven days and then a new random choice has been made.

The square root difference between the average precipitation amount of evenly spaced rain gauges in each area has been found to have a nearly normal distribution and will be used as the variable for the evaluation of the seeding effects. It has been preferred to ratios and logs where zero needs special treatment.

Instruments for ice forming nuclei counts and droplet size measurements have been developed and measurements will be carried out during the 1961-62 season along with the seeding, with a cloud census and radar tracking of the clouds.

Cloud physics studies should be carried out on a regional or international scale rather than in a small country like Israel and the hope is expressed that such a cooperation will be made possible in the near future.

Discussion

BRITTON: It is thought that maximum rainfall in the tropics at sea occurs at night. Hence it would seem radiation from tops of cumulus clouds could be an important factor, particularly in coastal regions and hence one could probably deduce an optimum time of day in any locality to carry out seeding.

JAFFE: We have maximum cumulus development in the early morning and minimum in the afternoon. But clouds of course appear quite frequently during the rest of the day as well.

GIBBS: There seems to be considerable advantage in stratifying cloud seeding experiments with regard to synoptic situation, as is apparently the aim of "Project Whitetop". This requires short periods for randomization of not more than 24 hours and preferably less.

JAFFE: We have been trying to find a way of classifying the synoptic situation, but we have not found a solution.

BRAHAM: I hasten to point out that use of objective selection criteria applied to air mass properties does not ensure completely homogeneous synoptic types. This certainly was our aim in deriving the objective criteria for selecting operational days, but the criteria scheme is not perfect. We find that diverse synoptic weather types may occasionally meet the criteria. The number of "unwanted" situations is markedly reduced and the power of tests enhanced, however, by use of selection criteria.

MORAN: Two comments. The advantage of using single days is that it is possible to classify them according to synoptic type. If completely randomized sequences are used, it is possible to use daily values validly if they are later grouped in sets of 10 or 20, say, and then analysed by regression analysis.
Secondly, in Mr. Jaffe's experiment using x - y as a test criterion, the power can be markedly increased if x + y is used as a covariate.

METEOROLOGICAL FACTORS IN THE DESIGN OF CLOUD SEEDING EXPERIMENTS IN MOUNTAINOUS AREAS

by

A. F. Rainbird*

The potential of persistent orographic cloud systems for increasing precipitation has long been recognized and numerous experiments have been conducted in areas where orographic cloud is of frequent occurrence. However mountainous areas introduce additional complexities, particularly in the measurement of precipitation, which make it more difficult to establish the magnitude of any increase in areal precipitation due to seeding operations.

The problems associated with operations in mountainous areas are illustrated by particular reference to the Snowy Mountains region of southeastern Australia. Conditions experienced in this region would be common to mountainous areas in many other parts of the world.

Various methods of determining areal precipitation are discussed and compared. The feasibility of meteorological stratification of data, or storm typing, is considered and possible advantages of determining areal precipitation by a different method for each storm "type" are discussed.

As an extensive hydro-electric power generation scheme is located in the Snowy Mountains area, possible relocation of precipitation due to seeding is of interest. The influence of wind on the "catch" of precipitation gauges, particularly with snowfall, is well known. If some gauges are more exposed to wind influence than others and a significant proportion of the precipitation is associated with moderate or strong winds, then apparent "shifts" in precipitation depth maxima may be detected in natural precipitation.

Such apparent "shifts" in natural precipitation patterns due to wind influence would make relocation due to seeding effects more difficult to detect. The possibilities of reducing the influence of this factor by meteorological stratification of data are discussed.

Discussion

WHITE: Mr. Rainbird showed figures, attributed to Wilson, of the collection efficiency of rain and snow gauges as a function of wind velocity. This is such an important point that I would wish to know how these figures were obtained. From what base-line could such measurements be made?

RAINBIRD: Wilson's work studied small catchment areas, using run-off as a control for rainfall, and for snow, snow courses. In the Snowy Mountains area over a whole season the run-off shows that conventional rain-gauges underestimate the total snow. I simply wanted to use Wilson's curve as a qualitative basis for discussion.

POSSIBILITIES OF ARTIFICIALLY CHANGING THE PRECIPITATION CHARACTERISTICS IN ARID AND SEMI ARID PARTS OF WEST PAKISTAN

by

S.N. Naqvi**

Six experiments on inducing rainfall by seeding of clouds and the atmosphere carried out in Pakistan from 1953-1957 have indicated that the precipitation can be increased by a certain percentage of normal amount of precipitation expected in any area. Although the statistical techniques employed for the assessment of the results are not very sensitive, the examination of the amounts of rainfall recorded around the

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seeding stations with reference to the synoptic situations and trajectories of the seeded air parcels leave little doubt about the significance of the results.

The experiments so far conducted indicate that by causing rainfall artificially although it may not be possible to change the economic pattern of the country, it would certainly be possible to save crops and vegetation from the effects of long droughts and increase the yield by causing even a slight precipitation at critical periods in farming operations.

The Government of Pakistan is therefore prepared to utilise the technique on a regular basis for increasing the precipitation to the maximum possible extent.

In this connection daily meteorological data of all the observatories in West Pakistan for 5 to 12 years have been examined and periods of droughts and rainfall as well as of cloudy and clear weather have been worked out with a view to select periods when artificial rain making should be tried.

A study of synoptic patterns affecting West Pakistan in a year of heavy rainfall (1956) and in another year of meagre rainfall (1952) has been made. Favourable synoptic situations when seeding operation could be expected to yield precipitation and to break the long period of drought have been studied.

It is, however, not known whether artificial stimulation of rainfall over the plains would deplete enough moisture to decrease the amount of snowfall and rainfall over the unseeded regions of the country and thus disturb the water balance of the Indus Basin. This is a problem on which suggestions are invited.

The operational requirements of the project and the practical difficulties which would have to be solved before making artificial rain making a routine procedure for exploitation of the moisture resources of the atmosphere and in semi-arid regions like West Pakistan have been discussed in the paper.

Discussion

WHITE: I congratulate Dr. Naqvi sincerely for his courage in this matter. In Australia during the years over which we have been investigating the possibility of increasing precipitation, periods of severe drought have occurred causing grave agricultural loss. We have often been under pressure to indulge in ad hoc rain making. I can thus well understand the difficult position that Dr. Naqvi has had to withstand. I commend his wisdom in wishing to undertake a properly designed experiment.

Pakistan can now take advantage of the accumulated knowledge and experience resulting from experiments in other parts of the world. If this is done, it may result in consequences of great practical interest. It would certainly be an important contribution to science.

I feel sure that many in this audience would welcome the opportunity of helping Pakistan in this matter.

SCHAEFER: Several years ago while in East Africa I met a petroleum engineer who told me that in West Pakistan in a region of pure desert, oil exploration disclosed a region of underground water close to the surface. It was decided to use this underground water to help in the establishment of a localized forest. A square-mile plantation was planted, the trees were irrigated until their roots reached the underground water, after which (according to my informant) they continued growing. After 10 years or so it was said the "green" zone began to extend in the down wind direction.

In my opinion this is an observation of tremendous import if true, since it suggests that the establishment of a tree zone may serve to "nucleate" the atmosphere with clouds. That is - the presence of a disturbance to uniform air flow over a featureless or smooth surface such as may be caused by a forest.

If the extension of such a green zone can be demonstrated experimentally (as may actually be so at present in some part of Pakistan) this would be of very great scientific interest.

NAQVI: Yes, we are investigating underground water supplies and carrying out integrated surveys to estimate climatic and soil potentials. This knowledge will be used to show how to use any precipitation increase.

ROY: It would perhaps be more useful to base the statistical survey on cloud types also and not just amount. Would it be worth while carrying out rainmaking experiments in areas of highly variable rainfall?

NAQVI: We have obtained data of this kind and put it on punched cards and we have used this in preparing our future plans.

CLOUD SEEDING EXPERIMENTS IN ARIZONA

by

L. J. Battan and A. R. Kassander, Jr.*

Observation of the convective clouds over the mountain ranges of southeastern Arizona show that a large fraction of them reach temperatures far below freezing but fail to produce precipitation. During the summer periods of the years 1957 through 1960, a randomized cloud seeding experiment was conducted to test if these clouds could be modified by airborne silver-iodide seeding.

The essential features of the experimental design were the following: (1) An objective technique was evolved for predicting days with suitable clouds; (2) one of a pair of adjacent days was seeded on a random basis; (3) measurements on all days were made with rain gages, radar and a pair of ground-located aerial cameras mounted at the ends of a 3-mile base leg; (4) the sign-rank test was used to make statistical evaluation of the effects of seeding.

The results after four years are the following:

1. The rainfall statistics do not allow a conclusion that seeding had any effect. Statistically, the rainfall on both seeded and not-seeded day was the same.
2. The frequency of occasions of large thunderstorm echoes was statistically the same on seeded and not-seeded days.
3. The cloud-census studies suggest that in some cases, seeding may have led to the initiation of precipitation echoes but the results are not conclusive.

Some brief comments are made regarding the reasons for the failure of these experiments to increase rainfall. The experimental design has been revised and a new set of experiments is now underway.

Discussion

Dr. Battan's contributions have been added after the Conference.

BOWEN: Could Dr. McDonald clarify the operational procedure. Did the aircraft seed in clear air upwind of the cloud system throughout the experiment?

McDONALD: We always seeded 30 minutes upwind of the windward edge of the mountain range, out of cloud at -6°C.

BATTAN: However, since the mountain range was about 20 mi across, before the nuclei were carried away from the mountain by the wind, a period of several hours was usually necessary.

BETHWAITE: I have several questions to ask about your procedure. Was the ratio of the amount of stratus to cumulus identical year by year and was the durability of the cells measured ? Did the aircraft remain at all times in clear air? In your new experiments, does the aircraft seed under individual cumulus bases?

McDONALD: All the clouds were cumulus, but we have no data on their lifetime or distribution. As I recall there was a systematic difference in the trajectory of the air masses. In the first two years the movement was from the south-east. In the second two years it was abnormal, with movement from the north-west from the Pacific.

BATTAN: Impressions are often misleading. The relative frequency of the direction of the 10,000 ft. winds was no different in the two periods. In 1959 and 1960 there was a greater frequency of 20,000 ft. winds from the north-east quadrant but statistically the proportions of wind direction in the four quadrants were no different in the 1957-58 and 1959-60 periods.

McDONALD: Another difficulty arises when heavy rain occurs and prevents the aircraft from flying on "seeded" days. We are now flying the aircraft on "unseeded" days to try to eliminate the effect of this on the statistics.

Returning to the final point, we are now seeding at the level of cloud base, still 30 minutes upwind.

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SOULAGE: How long is your plane seeding clouds and what is the concentration of ice nuclei introduced into it?

McDONALD: We seed for four hours from mid-day. We estimate a concentration in clear air from 10 to 100 nuclei per litre effective at -10°C.

BATTAN: These values were calculated assuming that the nuclei which were dispersed along a flight 20 mi long with a cross wind of 10 mph were uniformly distributed in a volume 3km and 1km deep respectively. We agree that the assumption of uniform dispersion is not valid, but these numbers give a rough measure of the numbers of nuclei available.

WHITE: Two observations were made - the rainfall and the radar observations. The former gave a null result but the latter a positive result. Looking at this experiment as one in cloud physics, why was so little attention paid to the radar results?

McDONALD: We have no results which are statistically significant. There is some evidence, however, from the radar echo data that there was a systematic change in echo distribution on the seeded days.

GHOSE: Did the aircraft fly only round the clouds and never through them? There could be considerable wastage of silver iodide by seeding in this way.

McDONALD: Flying in a steady pattern outside the clouds gives a better dispersion over the whole of an area.

SCHAEFER: What studies were made to determine whether the silver iodide emitted by the plane was actually carried to the cloudy areas and not lost in the regions free of cloud?

McDONALD: We have no information on this point.

GIBBS: Could the change in relationship between radar echoes and rainfall in the two two-year periods be due to higher cloud bases and therefore more evaporation in the second two-year period? Are there any observations of cloud bases at the time of the experiment?

McDONALD: This is conceivable, but the cloud base is controlled by surface moisture and there is no systematic trend in these observations that I know of.

BATTAN: One might take the 850 mb temperature as a factor related to the height of the cloudbase. The average value on the experimental days in 1957 and 1958 was 12.7°C; in 1959 and 1960 it was 12.5°C. On this basis there appears to have been no systematic difference between the two periods.

SMITH: Can Dr. McDonald suggest any other parameters, changes of which may have been associated with the difference in the early and later results?

McDONALD: The only parameter that was different in the last two years was a synoptic one - the direction of the wind. We regard as anomalous the frequency of N. E. winds in the last two years. (See earlier comment by L. J. B. regarding winds).

LODGE: The apparent differences in trajectories in the second two years compared with the first two suggest that there might be differences in freezing nuclei. Do you have data on this point?

McDONALD: No, there were no continuous ice nucleus counts. In 1958 we used a mixing chamber in an aircraft and subsequently a Weather Bureau counter on a mountain. I should point out that many other things are being investigated by Battan, but the real lesson from these experiments is one in the variability encountered and possibly the importance of its anticipation by statistical treatment of historical data.

BATTAN: Ice nuclei measurements were made with the Weather Bureau instrument (see paper by D. Kline) during 1959 and 1960 on 21 seeded and 20 not-seeded days. The average counts of -10°C nuclei were 0.30 and 0.32/litre on seeded and not-seeded days respectively. The counts were 0.38 and 0.24/1 on rain and no-rain days respectively. The differences are insignificant.

BRAHAM: An important fact is emerging from cloud seeding studies over all the world. We are increasingly aware of the need to combine with our cloud seeding trials a thorough study of the meteorological phenomena being seeded. These studies are much more than the usual kind of synoptic study. We need to make detailed meso-scale studies on a space-time scale appropriate to the seeding experiments. For the most part, these meso-scale studies require a much finer measuring network than that used for conventional meteorological practice.

SCHAEFER: What guarantee do we have that the new approach being made in southern Arizona will in fact introduce silver iodide in the cloudy area which will be evaluated?

McDONALD: Remember that we are now seeding at cloud base and so it should get into the clouds.

SCHAEFER: We should track the material in the roots of the clouds, and be very sure that the nuclei are reaching the clouds.

WARBURTON: Aerial tracing with radio-isotopes appears to offer a means of tackling your problem of the passage of the seeding substance through the atmosphere. Since you seed in clear air upwind from the cloud, the problems of following the activity are much smaller and the trajectory of the activity could be followed very readily.

McDONALD: Tracking in this way may be a good idea but it does not tell us exactly what we want to know. We need to track the silver iodide over the whole area, not just to the first row of clouds.

HOSLER: From the very beginning of cloud seeding attempts, it has seemed that the approach has been one that assumes the effect of seeding would be the same on all cloud systems at all stages in their development. The cumulus seeded here have life cycles of 20 or 30 minutes. Certainly there is an optimum time in the life cycle for producing maximum effect on increasing the precipitation amount and an optimum time for decreasing it. If all clouds are seeded at all times, it seems that the net effect may be zero, even if the effect is significant in any individual case.

HITSCHFELD: It is agreed that seeding can be effective on single clouds. The statistical evaluation of large experiments seems a very expensive way to test the methods.

McDONALD: These experiments are done to see if we can increase precipitation in a particular place or not. The experiment is started using the best statistical design you can achieve; once you have made this decision, you must stick to it.

BATTAN: There is some evidence that precipitation may be initiated in single convective clouds, but it is <u>not</u> agreed that rainfall to the ground from single cumulus clouds can be increased. Cumulus clouds may develop echoes without producing rain at the ground.

MACKY: In designing experiments it is essential to arrange that accurate measurements can be made of the rain which fell. In rugged mountain areas there is great variation in rainfall over short distances and to get a true representation of the fall requires a very large number of gauges, often a number which is not possible economically. Hence experiments designed to test the efficiency of seeding procedures should be done in areas where a true measure of rainfall is possible.

MALONE: It is clear from these experiments that the lessons learnt are still more important than specific results, and that we must not generalize too quickly.

SOME RESULT OF CLOUD SEEDING EXPERIMENTS WITH USE OF SILVER IODIDE GROUND GENERATORS

by

Kenji Isono*

Two series of randomized cloud seeding experiments using kerosene silver iodide smoke ground generators were made in the Okutama and the Gumma regions in Japan. The periods of seeding or not-seeding were assigned on the basis of a set of random numbers. Samples of rainwater were collected at stations in the target areas and the concentration of silver iodide, chloride and fluorescent material were determined.

The result of a statistical analysis of the ratios of total amount of precipitation at each station in the seeded periods to that in the unseeded periods shows that the increase of precipitation in the target area was significant at the level of 0.005 in the case of Okutama experiment in winter (from January to March) of 1960. The averaged ratio of the precipitation amount in the target area was 148% and there was an area in the target area where this ratio exceeds 300%. With the intention of displacing the area of maximum increase in precipitation amount about twenty kilometers to the south, the site of seeding was displaced about twenty kilometers to the south in the experiment in 1961.

The results obtained are summarized as follows:

(1) A statistically significant increase of precipitation was found in the experiments

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made in both regions in the period from January to March.

(2) The increase in precipitation amount in seeded periods relative to not-seeded periods was larger when precipitations were brought by warmer moist clouds in winter.

(3) The concentration of seeded silver iodide in rain (or snow) water collected in the target areas was high in areas where precipitation was large. The areas of maximum silver iodide concentration in rain water appeared not in the area near to the seeding site where silver iodide concentration in the air was highest, but in the area at some distance from the seeding site (about 50km in the case for Gumma region). These facts rule out the idea that silver iodide detected in rainwater resulted from the capture of silver iodide particles by falling raindrops.

(4) The distance from the generators to the areas of maximum increase in precipitation amount increased with increasing height of the freezing level above the ground.

(5) The area of maximum increase in precipitation in the Okutama region in 1961 moved to the south relative to that in 1960 in accordance with the displacement of the seeding site.

(6) Observation of the shapes of fallen snow crystals reveals that needles occurred more frequently on seeded days than on unseeded days.

Discussion

ADDERLEY: Am I correct in thinking you said that when you displaced the AgI generators to the south, the rainfall maximum was also displaced to the south by the same amount?

ISONO: About the same amount.

YANG: Did you try any experiment to check how high the silver iodide smoke went up from your ground generator in Japan?

ISONO: Yes, I made measurements of AgI nuclei with an ice nucleus counter both in an aircraft and on top of a mountain in the target area. The AgI concentration was about 1 - 10 per litre at -15°C and was observed up to 5,000 metres altitude.

SMITH: The concentration of AgI in rainwater which you have measured seems high. What was the proportion of the AgI used which came down in the rain?

ISONO: We estimate that 3% came down.

GHOSE: I wish to know why the hydrogen balloon method of dispersing silver iodide nuclei by exploding bombs impregnated with AgI was given up. Was it because the dispersal of silver iodide by means of ground generators was found to be more effective in stimulating rainfall?

ISONO: The effect of seeding is to give a continuous supply of ice nuclei and so increase the rate of release of rain. Hence we need to seed continuously for as long as possible. We do this by mounting the AgI generators on the mountains.

BOWEN: I have three observations on Professor Isono's paper:

(a) First I would like to congratulate him on his technique for the detection of AgI in rainwater and secondly for relating the place where AgI is detected to the place in which an increase in precipitation is observed. We find this to be very convincing evidence that real effects are produced.

(b) It appears from Professor Isono's description that he has an exceptionally favourable site - rather like some of those we know of in California - for the use of AgI ground generators.

(c) In view of some discussions which are likely to come later, it is very interesting to observe that Professor Isono's results are obtained in winter and that he was unable to obtain similar effects in summer.

AUSTRALIAN CLOUD SEEDING EXPERIMENTS

by

E. J. Smith*

Cloud seeding experiments which have been performed in recent years in Australia have been designed to determine whether rainfall can be increased over large areas. The main characteristics of these experiments is that seeding is performed from aircraft operating at cloud level and the seeding sequence is fully randomized.

A total of five experiments have been embarked upon, and their general form is as follows:

Two areas are chosen each of 1000 to 2000 square miles, with well correlated rainfall. Time is divided into "periods" usually of about 12 days and clouds over one area are seeded during the whole of one period, the choice of area depending on a random process. Rainfall is measured in both areas by the Bureau of Meteorology and statistical methods are used to determine whether there is a difference in the rainfall relationship in the two areas according to which is seeded.

This technique of stimulating precipitation should be successful if suitable clouds occur which are deficient in freezing nuclei. The distribution and properties of these clouds are not known so a chain of five of these experiments has been set up in representative areas where clouds are thought likely to occur. These experiments are by no means complete, but results to date are described in the next paper.

ANALYSIS OF CLOUD SEEDING TRIALS IN AUSTRALIA

by

E. E. Adderley*

The forms of the five experiments of which two have terminated and three are current are as follows:

1. South Australia: An experiment which ran during the winter and spring months of 1957, 58 and 59. Two areas - both of which were seeded during random periods of about 12 days duration.
2. Snowy Mountains: Commencing in 1955 the experiment ran for five years to 1959 during the months of March to November inclusive of each year. Two areas, only one of which was seeded in random periods of about 12 days duration.
3. New England (N.S.W.): Commencing in February 1958 this experiment is still current. It runs throughout the year with the exception of the months of November and December in each year. Two areas both seeded during random pairs of periods of about 10 days duration.
4. Darling Downs (Queensland): Originally intended as a Summer-Autumn experiment it ran throughout the whole of the first year 1960 to assess the possibilities of another experiment in Winter-Spring. Two areas - both seeded during random pairs of periods.
5. Warragamba (N.S.W.): This experiment runs throughout the year commencing in December 1959. Two areas - both seeded using random pairs of days to allow simple meteorological stratification.

The basic data used for analysis of these cloud seeding experiments are the mean precipitations over the areas involved. These are derived from a dense network of precipitation gauges specially installed for the experiments.

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The preference of the author is for the use of non-parametric methods of statistical analysis. However both parametric and non-parametric methods of analysis are used, and it was found in these five experiments that the results from all the tests applied did not vary much among themselves. The significance of each experiment is therefore given for only one method of analysis - a Wilcoxon order test.

The results of the five experiments are as follows:

Experiment	Duration	Air Mass	Observed Effect	Significance (one sided)
South Australia	3 years	Maritime	- 5%	0.27
Snowy Mountains	5 years	Continental	+19%	0.06
New England	3½ years	Continental	+10%	0.06
Darling Downs	1½ years	Mixed	-13%	0.03
Warragamba	1½ years	Maritime and Continental	+ 2%	0.46

REVIEW OF AUSTRALIAN CLOUD SEEDING EXPERIMENTS

by

E. G. Bowen*

Early cloud seeding experiments in Australia left no doubt that in continental regions, seeding of supercooled cloud with materials like dry ice and silver iodide induced the formation of the ice phase, followed by the release of precipitation to the ground.

However, the broader question remained - to what extent could the techniques known to be successful in single cloud systems influence precipitation over large areas. Five experiments have been undertaken or are in progress in Australia in an attempt to answer this question.

Two of these experiments show positive results, with a high probability that the effects are real. Both of these are in regions in which the formation of good convective clouds is assisted by orographic features, and the clouds are frequently supercooled.

One experiment shows no nett change in precipitation. This is known to be due to a preponderance of warm, maritime clouds which rain by the coalescence process.

One experiment shows a decided decrease in precipitation, again with a high probability of a real effect. The reasons for this are not yet adequately known, but in all experiments there appears to be a tendency for increases in precipitation to be obtained when there is good supercooled cumulus cloud with moderate to strong convection, and a tendency for precipitation to be reduced with stratiform cloud in which turbulence is low.

The fifth experiment has not yet progressed far enough for conclusions to be drawn.

The conclusions to be drawn from these experiments are that:

(a) Success in cloud seeding experiments will come from the seeding of carefully selected cloud systems in favourable topographical situations.

(b) It is impossible to predict the results of cloud seeding in any given region and this may be established only by a carefully designed experimental procedure.

Discussion

McDONALD: Could you tell us a bit about how you made the choice of 12 days as your unit of randomization?

SMITH: In most of the experiments there is an 8 or 10 day minimum period

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length, after which the period changes on the first occasion when fine weather is forecast by an independent source. This avoids problems arising due to rain gauges which are inaccessible or liable to be read at unscheduled times during precipitation. The Warragamba experiment has one-day periods.

McDONALD: My question concerns the interpretation of the downward trend of seeding effects in the Snowy Mountains.

The steady increase in seeding hours over the five years could only have lowered the effect if the stratus seeding actually gives a negative effect.

But the earlier table reveals that stratus seeding gave an actual reduction in only one of four cases (Darling Downs), the other three being positive, though small, effects.

Thus your implied explanation of the downward trend in the Snowy area does not seem to be consistent with your experience in stratus seeding elsewhere.

SMITH: The double ratio figures for Warragamba and New England in stratus conditions, although positive, are biased and do not necessarily indicate an increase. The only definite evidence concerning stratus comes from the Darling Downs: If that effect occurred in the Snowy too, it could explain the observed effects. Other features could contribute, however; for example in 1959, Mr. Wiesner reported that the Snowy area had an unusually large proportion of easterly (i.e. maritime) air masses.

DESSENS: For ten years I have been seeding with silver iodide ground generators, sometimes with more than a hundred generators. My opinion now is that the most probable increase of rain during our trials has been obtained in Central Equatorial Africa, in the Congo Basin, during the dry seasons. This region is very isolated from maritime influences and is very convective. This result is in good accordance with Dr. Bowen's conclusions.

ROY: It has been stated that the probability of the negative effect of seeding (-13%) in the Darling Downs area being real was high. I wonder whether it was possible to say this on the basis of seedings carried out over a limited period of 1½ years only. In considering the probable effects of cold cloud seeding in different areas, I have been inclined to think that this would probably depend, to a large measure, on the frequency of occurrence of clouds with their top temperatures falling within the range -5°C to about -15°C. In an area where this frequency was high, we would expect more promising results. It is just possible that a small positive effect (or even negative result of trials over a short period) might be due to infrequent occurrence of clouds with the indicated cloud top temperatures.

BOWEN: In the Darling Downs the variance of the result has been small right from the beginning, rapidly giving it a high significance level.

VONNEGUT: There is good evidence, even in supercooled clouds, that much natural rain is generated by a coalescence mechanism. Is it not possible that by transforming the drops into ice crystals one might in some cases be interfering with a more effective natural rain-producing mechanism?

BOWEN: This is a very good point which we will follow up.

HOSLER: In the Darling Downs case where a large decrease is observed, I wonder how often multiple cloud layers exist and if often we are not seeing a case where destruction of the lower clouds is destroying the feeder clouds in the feeder-seeded system discussed by Bergeron. Dr. Bergeron has often pointed out the role of these lower clouds in reinforcing and increasing the intensity of precipitation falling from higher clouds.

SMITH: This is quite possible, but definite evidence is not available.

MURGATROYD: It would help considerably in thinking about the physical background of the results if we had a breakdown of the observations in terms of cloud sizes or, if this is not available, in terms of rates of rainfall. (Low rates are often due to the coalescence mechanism). Are the data available in this form?

ADDERLEY: With the large amount of data available, to stratify according to all available parameters would obviously be out of the question. Some have been carried out which do not include the one Dr. Murgatroyd mentioned. However, we would be very grateful for such suggestions when the Conference is in Sydney next week.

MAGONO: Air masses coming from different sources to Japan may contain different numbers of natural freezing nuclei. I feel, therefore that an estimation of ice nucleus concentration is desirable before seeding is carried out.

ADDERLEY: The evidence we have from cloud-seeding aircraft in the South Australian area is that a very large proportion of the precipitation there was of coalescence type and the freezing nucleus population could not be expected to be relevant in this case.

MIZON: To avoid any confusion I should like to ask that references to "warm" rain be confined to rain from clouds which nowhere have temperatures below 0°C; otherwise, use the term "coalescence".

SCHAEFER: With relation to the terminology of clouds, may I suggest that the terminology proposed by Langmuir be adopted, i.e.: Warm clouds - clouds, all parts of which are warmer than 0°C. Cool clouds - clouds which extend both above and below 0°C. Cold clouds - clouds, all parts of which are colder than 0°C.

MARSHALL: Is it possible to draw a single conclusion from all the experiments, and arrive at a figure for the confidence one can have that there is a real effect in at least one of the experiments?

MORAN: It is possible to combine tests into an overall test, but since in this case it will be necessary to use two-sided tests, I would expect the power to be so small that no overall significance would appear.

MALONE: I think we can summarize today's proceedings under five points:

(i) The measure of the problem has been taken and it is very large.

(ii) There is compelling evidence of complete objectivity in the experiments, but they are fraught with dangers.

(iii) There is persuasive evidence of real physical effects. This evidence has contradictory features and there are many practical points to be considered.

(iv) There are many different kinds of results and many exciting avenues to be explored in this field.

(v) The kinds of advances in thinking and in techniques that have been discussed give great encouragement and we trust we will be significantly further down the road than we are today when we meet again.

Second Session : Sydney

INTRODUCTION

The second Session of the Conference was held in Sydney from the 18th to the 20th of September. Four Seminars were held on each of these three mornings on subjects of particular interest thrown up during the scientific discussions.

These included :

I Instrumentation for Cloud Physics Research
II Detection and Identification of Freezing Nuclei
III Hail
IV Artificial Stimulation of Precipitation.

INSTRUMENTATION FOR CLOUD PHYSICS RESEARCH

Chairman: Mr. J. Warner

Three topics were discussed: the measurement of temperature with particular emphasis on its value inside cloud, the measurement of cloud water content and the measurement of vertical air velocity.

Dr. Cunningham opened the discussion on temperature measurement with a review of the methods that have been used in aircraft observations. Measurements in dry air are not difficult at low aircraft speeds though troubles arise as the speed increases. A correction needs to be subtracted from the indicated temperature which is equal to $\alpha\,(V/100)^2$ where V is the true airspeed in m.p.h. The value of α is 1 for a total head thermometer. It is necessary to calibrate aircraft thermometers in flight and Cunningham suggested that the absolute accuracy obtainable was of the order of $\pm$ 1/2°C. For normal methods of measurement in cloudy air it is necessary to prevent cloud droplets impinging on the element and producing evaporative cooling. Various types of reverse flow and vortex thermometers were described in which the element is kept dry. It was suggested that infrared techniques might provide a possible alternative to conventional methods of temperature measurement, though it was remarked that to be useful they would have to be able to give a measurement over path lengths of only a metre or two. This had not been done and appeared difficult to realize.

In the discussion which followed on temperature measurement in cloudy air Warner queried the accuracy of "removal" methods and asked if they had been compared with other techniques. Cunningham replied that no absolute calibration had been made and that the absolute accuracy was uncertain. However instruments of different design compared favourably with each other and in any event showed simultaneous small fluctuations of temperature of the order of 0.1°C during such comparisons. Braham reported that by the use of a filter paper strip coated with a water soluble dye and attached to the element it had been shown that even when flown through heavy rain the element remained dry. It was possible however that the droplets might evaporate and cool the element if the air was subsaturated. Squires suggested that extrapolations of the usual heat transfer relations obtained at moderate flow rates to the high droplet velocities occurring in vortex and reverse flow thermometers may not be valid. He suggested that these instruments should be mounted on a motor car and driven along an airstrip in a ground fog in which accurate measurements could be made by a stationary observor. Braham remarked that attempts to make calibrations in a closed circuit wind tunnel had proved very difficult due to temperature fluctuations. Hitschfeld described the results of some observations of cloud temperatures made by measuring the black body radiation in the 8-12 micron "atmospheric window". It should be possible to measure temperature to about 1/2°C but the measurement would be an average over a distance depending on the cloud droplet spectrum but of the order of 100 metres. It was suggested in discussion that while this distance was very large in comparison with the requirements for most cloud studies the technique could provide a useful check on the other instruments.

The discussion on measurement of water content in cloud was opened by Dr. Murgatroyd who suggested the following specification for an instrument:

- Range 0.05 - 10gm^{-3}
- Droplet diameter range 5μ - 5mm
- To respond equally to water in all phases
- Continuous recording
- To operate at pressure levels down to 200mb and temperature down to -80°C
- Response time 0.1 - 1sec.
- Accuracy ± 15%
- Ground calibration is most important as is siting of the instrument on the aircraft.

The different types of water content instruments that have been used are as follows: impaction types, those depending on bulk capture, absorbent paper techniques, icing rate meters, heated intake instruments in which all the water is evaporated prior to measurement, heated wire types, instruments depending on optical scattering or transmission, and miscellaneous types such as the lithium chloride strip. The results obtained indicated liquid water contents of 0.5 to 1gm^{-3} in cumuli with the adiabatic value rarely occurring except in small regions; large Cu and Cb had water contents of up to 4gm^{-3} and 7gm^{-3} had been recorded; layer type clouds gave values of 0.1 to 1.5gm^{-3} but usually less than 1gm^{-3}.

While speakers in discussion commented that the specification suggested by Murgatroyd could probably not be fulfilled by any one instrument Vonnegut remarked that there was a tendency to underestimate the range of any quantity in the atmosphere and suggested that an even wider range in water content and operating pressure and temperature should be available. Braham suggested that insufficient attention was often given to the siting of instruments on aircraft and that it was highly desirable to investigate the pressure field round the aircraft to determine flow patterns. Incorrect siting of water content meters could result in large errors.

Mr. Telford next described the requirements that must be fulfilled in order to measure the vertical velocity of the air from an aircraft. Two elements are required: the vertical component of the aircraft's velocity, and the vertical component of the air velocity relative to the aircraft. The aircraft velocity can best be measured by electronically integrating and then recording the output of a high quality accelerometer which is held erect by a gyroscope. The acceleration due to gravity must of course be substracted from the accelerometer's output prior to integration. If an accuracy of 0.1m sec^{-1} is desired over a period of 5 minutes it can be shown that errors in acceleration must be less than 3 x 10^{-5}g, which is difficult to achieve. If pressure altitude is available to an accuracy of say ± 2 metres we can derive an average velocity to within 0.1m sec^{-1} after an elapsed time of 20 seconds for comparison with the integrated acceleration. This procedure permits a less stringent requirement in acceleration, namely 5 x 10^{-4}g. It was suggested that the process of recording acceleration and integrating later from the graphical record is not likely to yield sufficient accuracy, since it is improbable that reading and other errors are random to the degree necessary for complete cancellation. For the necessary precision in acceleration it is further necessary to hold the accelerometer vertical within about 1° by a gyroscope. Otherwise changes in the gravitational component of the measured acceleration will occur which will be in the nature of systematic but unknown errors. In order to obtain the vertical component of the velocity of the air relative to the aircraft the inclination of the approaching air must be measured, which can be done by a horizontally stabilized vane on a long rigid boom in front of the aircraft nose. The vane angle must be measured relative to the true horizontal to about 0.1° which implies that aircraft pitch must be known to this accuracy. This can be done only by means of a free gyroscope or one to which an erection cancelling the Earth's rotation can be applied. The ordinary gravity erected gyro is likely to produce large errors as a result of aircraft accelerations.

In the discussion which followed Murgatroyd remarked on gust research in the U.K. and U.S. and suggested that it was necessary to measure both pitch and rate of change of pitch and considered that pitch could be obtained by integrating the latter. He also queried the necessity to stabilize the accelerometer. Telford replied that stabilization was necessary against roll and pitch to eliminate components of gravity, and expressed considerable doubt that integration of rate of change of pitch would yield the necessary precision of 0.1°. Kennedy commented that due to the mounting of different components of the velocity measuring system at different places flexing of the aircraft structure could be of importance. He also described a pressure measuring system which was capable of measuring angle of attack. This was a null system in which the two sensing air vents were moved by a servo to obtain equal pressures. In reply to a query about the effect of aircraft vibration Telford stated that "noise" due to this source was filtered by the electronic integrator. There was general discussion on the requirement of a system for measuring air velocity in regard to its accuracy and response time but no clear conclusion emerged. In reply to a query Telford stated that while 30m sec^{-1} might be an upper limit to what the aircraft or pilot might tolerate the system he described was limited to a maximum of about 8m sec^{-1} due to maximum travel in the vane and pitch measuring systems.

DETECTION AND IDENTIFICATION OF FREEZING NUCLEI

Chairman: Dr. K. L. Sutherland
Assistant Chairman: Dr. E. K. Bigg

Dr. Soulage described methods of measurement of ice forming power in the atmosphere. He suggested that the method of collecting particles before testing their ice nucleating properties had many inaccuracies and that the mixing chamber method was the most useful and accurate. He provided a list of rules that should be observed with this technique, as follows: 1) clouds should have 2-3 mins duration, 2) frost must be prevented by glycerine on the walls, 3) air should not be introduced before the sugar is supercooled, 4) if ambient temperatures are subzero the air should first be warmed, 5) a sugar solution at least 2mm deep should be used for detection of ice crystals, and it should be changed after every experiment.

The methods of identification of ice nuclei through their chemical composition or physical characteristics was briefly outlined by Prof. Isono, who then described in more detail their identification by the nucleus concentration method, showing how high concentrations could be related through air trajectories to specific sources. Another aspect of this measurement was the relation between the observed temperature spectrum of nucleus activity of the atmospheric aerosols and the temperature spectrum of nuclei obtained from the suspected source. He also suggested that the study of deep-sea sediments might give some indication of whether ice nucleus concentrations in past times might have been related to climatic changes.

Mr. Kline elaborated on the formidable list of phenomena which may influence the formation of ice crystals in clouds, and made the following additional points: 1) comparison of mixing and expansion techniques is usually possible by a simple adjustment in temperature. (The mixing chamber produces a given number of crystals at a temperature 5°C warmer than the expansion chamber), 2) replication of results was a serious problem, 3) there are enormous fluctuations in ice crystal concentration at any one point, and there are consistent differences at different sites, 4) the simultaneous analysis of sulphate, chloride and ice nucleus concentrations is a useful method for studying the origin of the ice nuclei, and has indicated that sea salt is not an important ice nucleus, 5) the high counts in some marine airstreams are perhaps due to silicates suspended in the water and released with the salt.

In the ensuing discussion a very wide variety of topics was considered and the following is necessarily a much abbreviated description of the main points.

Concerning the sampling of ice nuclei, the possibility that measurements may often be unrepresentative of a particular atmosphere was stressed by Georgii and Hosler. Evans suggested that this could be overcome by storing the air sample under test in a large polythene bag, although objections were raised that large nuclei might fall out and the fluctuations themselves might in any case be important. Bowen proposed that the most relevant measurement was not that at the ground but at the height in the atmosphere at which freezing nuclei might be expected to influence clouds. Spillane, however, remarked that since cumulus clouds have their roots near the ground, ground measurements need not be irrelevant.

In discussing the size distribution of ice nuclei Braham recalled Kumai's work which showed that the central nuclei of snow crystals in Michigan were commonly 1-3 μ in diameter, while in Greenland their modal size was 0.5-1μ. This provoked some argument about the problem of identification of the central nuclei, Soulage and Isono pointing out cases where the central nucleus was not necessarily the ice nucleus. Braham and Byers stressed that Kumai's methods were valid, and that the problems of identification were relatively simple in clean atmospheres. Bowen suggested the Antarctic as the most suitable place for examining the central nuclei.

Lodge called attention to the uses of the "volatilization" method for determining the organic content of aerosols and cited some results which showed surprisingly high concentrations of organic substances. Sutherland also emphasised that we need to know the surface rather than the gross composition, that nuclei may very commonly be heterogeneous and their organic content deserves more attention. Schaefer reinforced this thought by suggesting that protein-like monolayers on nuclei might be extremely important and could be responsible for the extrarodinarily active nuclei which promote the growth of frazil ice in rivers at temperatures only 0.01°C below freezing. Marshall suggested that these were so rare that they were unlikely to be important in the atmosphere but Shaefer said they occurred in enormous numbers in rivers. He also remarked that their disc-like shape distinguished them from the regular crystal shapes which grew on ice crystals seeded into the water. The foaming of seawater, thought to be due to the presence of proteins and the production of protein-like aerosols by forests was mentioned by Sutherland as possible evidence in favour of the idea.

The problem of pre-activation was considered, Mossop suggesting that measurements should be made without warming the air sample above 0°C. McDonald however cited an experiment which showed the absence of ice nuclei under a cirrus anvil which made him doubt whether the effect was important, while Bowen remarked that if the number of ice nuclei above and below the 0°C isotherm was vastly different it would have been noticed by now. Soulage quoted experiments which he had made using a microscope to examine ice crystals on a glass surface in a cold chamber, in which the activity of a nucleus, inactived by successive actions as ice forming nucleus could be restored by activating it at a much lower temperature.

Several speakers emphasized the importance of warmer reference temperatures than the commonly used -20°C, remarking that ice crystals in natural clouds often appeared to be numerous at -10°C, and in aircraft icing statistics in the U.S. the 50% probability of the ice phase was reached at -10°C. Experimentally the difficulty is one of sampling a sufficient volume. This topic led to experimental methods and Soulage analysed some of the difficulties of large volume sampling methods in which particles are collected on a filter or impaction slide. Such considerations are: covering of active nucleation sites by the substrate or superimposed particles, the proximity of nuclei (which may preclude ice crystal growth through lack of water saturation) and loss of the smaller particles. No agreement was reached on a standard operating temperature since, as Bigg remarked, present techniques commonly recorded zero counts at temperatures of most interest in natural clouds.

A discussion of temporal and regional variations in ice nucleus concentrations, revealed a variety of experiences, perhaps the most striking being the enormous concentrations of active ice nuclei which Soulage and Amdirat have found near certain industries in France, notably steelworks, alumina plants and power stations.

HAIL

Chairman: Prof. J.S. Marshall
Assistant Chairman: Dr. S.C. Mossop

Dr. Macklin opened the discussion by describing the characteristics of ice grown by accretion as observed by him in a wind tunnel. "Dry growth" occurs when there is no free water on the surface; the density ranges from 0.05 to 0.9gm/cm^3 and the ice is opaque or milky. "Wet growth" ice can be a spongy mixture of ice and water, so the density may range from 0.9 to 0.98, and the ice is translucent. Rapid growth will be favoured by accretion of ice crystals as well as supercooled water droplets. At the boundary or "wet entry" condition, clear ice of density 0.9gm/cm^3 is formed.

Dr. Mossop described some of the inferences that may be made from the examination of natural hailstones. Ice layers consisting of randomly oriented large crystals (greater than about 0.5mm diameter) are formed under wet growth conditions, where a mesh of ice crystals interpenetrates the unfrozen water. This ice can be completely clear, if there is sufficient agitation during freezing, or can contain air bubbles trapped by the freezing of an outer skin of ice. Near the wet entry condition, long narrow crystals, oriented radially with respect to the growth centre, are formed. The ice can contain bands of bubbles perpendicular to the growth direction, with the crystals passing straight through. Here the ice grows with a thin, possibly discontinuous, layer of water on the surface. In the case of ice layers containing small crystals and plentiful air bubbles, the situation is confused as such ice can result either from wet growth when ice crystals are also being accreted, or from dry growth.

It is apparent that more experimental work is needed on the crystal structure and bubble arrangement of ice produced under various accretion conditions before this confusion is resolved. It will then be possible to utilise the boundaries between ice zones in a hailstone to plot the trajectory in an assumed model cloud. Studies of hailstone shape are also important; for example, flattened shapes are an indication of high liquid water content.

Macklin explained that there is no sharp discontinuity in the transition from dry to wet growth ice if the change in the relevant parameters is gradual. The clearly defined boundaries often found in natural hailstones between ice of widely different crystal sizes and bubble contents can be due to sudden changes of temperature and/or liquid water content; sudden changes in liquid water content of the air seem to be the most likely explanation. This point was queried by Prof. Hitschfeld who felt that a slight drop in ambient temperature, for instance, could depress the temperature of a barely wet stone sufficiently for it to continue in dry growth.

The question of whether hailstones can grow in an all-ice environment was raised by Hosler. His experiments showed that ice crystals of less than 1mm extent could stick to one another. With particles of larger relative velocity this does not seem possible; List (as reported by Sänger) finds that ice crystals do not adhere to dry hailstones. It was noted though, that marked charging effects do occur, presumaly due to friction between the particles.

Discussion followed on some of the features incorporated in model hailstones by those who have made calculations on the growth of hailstones. Prof. Byers referred to some of the main findings of the Thunderstorm Project. The downdraught is an integral part of a mature thunderstorm cell. Updraughts are persistent, columnar, lasting for up to 20 minutes, and are therefore not just bubbles. Updraughts of 10-20 metres/sec were often measured, with a maximum of 30 metres/sec. Few of the storms studied contained hail. Prof. Byers felt that storms producing large hail could differ from ordinary thunderstorms and for one set of calculations he and Mr. Das had assumed that the hailstone grew in two successive adjacent updraughts with adiabatic values of liquid water content, without entering a downdraught. An alternative model also incorporated shear, but with a steady updraught leading into an overhanging anvil. This overcame the necessity for storage of liquid water.

Professor Hitschfeld described a model which was an improvement on previous approximations in that it took account of the freezing of an increasing proportion of water drops with increasing height. Growth of large stones was assisted by high liquid water content produced by storage. This was supported by radar evidence of Donaldson that the radar reflectivity of hailstorms showed a pronounced maximum between 15,000 and 20,000 ft. which was absent from ordinary rainstorms. An illustration of this, photographed at the McGill University radar, was shown. Constant flux arguments in a long duration storm were used to show that a high liquid water content in the form of raindrops was not inconsistent with observed rates of moisture input and the low observed rates of hail fall-out.

Macklin emphasised that an essential feature of Browning and Ludlam's deductions from intensive study of a severe storm, was that there was no storage of liquid water because of the increase of updraught velocity with height. The strong shear enabled growing particles to be re-cycled into the updraught near the base of the cloud.

Discussion followed on the question of whether wind shear was an essential feature of the violent hailstorm. Hosler mentioned that his studies of the duration of radar precipitation echoes showed that this was greatest for intermediate values of shear, echoes being of short duration when wind shear was very high or very low. Byers said that extreme shear inhibits thunderstorms unless you have strong feeding of the updraughts. It would seem to be possible for large hailstones to be formed without shear.

Professor Iribarne then considered three possible mechanisms by which silver iodide seeding might affect the production of hail:

1. If the release of precipitation were advanced and the mature stage reached earlier, updraught speeds would be smaller so that an essential feature for the production of large hail would be absent. This was queried by Marshall who pointed out that the release of latent heat by freezing would give added buoyancy at an earlier stage and thus greater updraughts.

2. Hailstone growth would be prevented if massive glaciation of the cloud could be achieved at a temperature of say -20°C instead of the colder temperatures at which it occurs naturally. This, however, would require an excessive number of nuclei 1 to 10 per cm^3 active at this temperature. List (in a communication) estimated that as many as 45 nuclei per cm^3 would be required for hail prevention by massive glaciation.

3. Competition for the available water would prevent growth to large sizes if the number of hailstones could be increased above $100/m^3$. A suggested dilution factor of 10 indicates that one AgI nucleus per litre would be necessary to give this number of additional stones. Splintering might reduce this requirement.

Discussion ensured as to the temperature at which silver iodide was likely to be effective. Schaefer, referring to measurements by McCready, thought the temperature range to be -9 to -10°C. Dessens reported that massive seeding in the Pyrenees had brought the visually observed glaciation level from -35° to between -12° and -22°C.

Hitschfeld considered that hailstones must start close to the 0° level in order to grow large and it was doubtful if seeding could affect the number at temperatures in the vicinity of -5°C. Telford suggested that for large hail the growth by coalescence of a few large drops below the 0° level, or the growth of a few large crystals just above the 0° level was necessary.

It was generally agreed that development of ways of hail prevention is contingent upon better understanding of processes in natural clouds.

Noting the apparent absence of hail at sea, Britton enquired as to the effect of the nature of the terrain on the incidence of hail. Squires replied that the tracks of thunderstorms were undoubtedly affected by topography and that this would also be true for hail. Marshall recalled Appleman's work on hail climatology as an important contribution indicating that topographic effects are considerable.

Squires made a plea for further study of the climatology of hail and for both a microphysical and a dynamical approach to such questions as why large hail is chiefly a continental phenomenon and why it occurs, for example, in the pre-monsoon period in India and not in the towering cumulo-nimbus of the monsoon.

ARTIFICIAL STIMULATION OF PRECIPITATION

Chairman: Prof. R. R. Braham
Assistant Chairman: Mr. E. E. Smith

The Chairman opened the Seminar by stating that discussion would cover three fields, namely: a) Measurements of areal precipitation in experiments on the artificial stimulation of precipitation. What should be measured and how? b) The area between meteorology and micro physics: the type of clouds in which cloud-seeding experiments have been conducted, the steps which should be taken to specify them, and the types of experiment appropriate to them. c) Miscellaneous.

A. Measurements

Mr. G. O'Mahony opened the subject with an address on the measurement of areal precipitation in area experiments. In large inhomogeneous areas these measurements were made difficult by factors such as the variation of the catch efficiency of gauges with wind and precipitation type, and the combined influence of synoptic situation and terrain. Mr. O'Mahony considered that the best overall figure was the average of the gauge readings, and concluded with the suggestion that small homogeneous areas should be used as targets for this type of experiment.

The use of small areas received some criticism, by Hitschfeld, (on the grounds that they might approach the size of meteorological phenomena) and by Smith as they would involve difficulties of aiming.

The use of radar was then discussed. Marshall stated that considerable developmental effort has been expended on this technique but it is not yet in use for hydrological measurement purposes anywhere. The general feeling was that it was ready to be tried for this purpose, at least in flat country, but a network of precipitation gauges would still be necessary as a check. Doubts were expressed by Bowen as to the value of radar in mountainous terrain and as to the correlation between radar echoes from precipitation in the air and the precipitation which actually reached the ground.

A list of workers actively engaged in efforts to use radar for quantitative precipitation measurement was compiled and is given below.

Mr. R. T. H. Collis	Stanford Research Institute, Menlo Park, California
Mr. R. Donaldson	U.S.A.F. Research Center, Cambridge, Bedford, Massachusetts
Prof. Heiser	University of Miami, Florida
Prof. J. S. Marshall	McGill University, Montreal, Canada
Mr. G. E. Stout	Illinois State Water Survey, Urbana, Illinois
Mr. Tarbell	U.S. Weather Bureau, Washington, D.C.
Dr. K. Arnold	Pacific S.W. Experimental Station, U.S. Forest Service Soda Springs, Berkeley, California.

Other methods of measuring precipitation were discussed. Runoff was suggested by Hitschfeld, but was generally considered unsuitable for use in cloud-seeding experiments owing to the long time-lag, although there was reason to suppose that a small increase in precipitation might lead to a large increase in runoff. Methods such as neutron scattering as a measure of the snowpack, and the optical reflectivity of airborne snow, were discussed but experience was lacking.

B. The types of Clouds in which Cloud-Seeding Experiments have been conducted

Dr. V. J. Schaefer opened by stating that clouds had widely differing characteristics. For instance, cumulus clouds rise to 40,000 feet over the Congo with no glaciation or precipitation, whereas in Puerto Rico clouds develop anvil tops while wholly warmer than freezing. Similarly, fogs varied markedly, some glaciating at -10°C while others remained supercooled to -30°C. It was therefore of prime importance that experimenters on clouds should specify in sufficient detail the types of cloud with which they were dealing.

Professor K. Isono described the clouds encountered in his Okutama experiment. Those in which precipitation increases have been associated with the release of silver-iodide have usually been deep nimbostratus forming in maritime air masses, with south-easterly winds in the warm sectors of advancing depressions, and often associated with warm fronts. The cloud bases are at about 3,000 to 6,000 feet and tops 15,000 feet or more, the terrain rising to 7,000 feet. On some occasions, northerly winds with a shorter trajectory over the sea brought shallower layers of cloud from which light snow fell, but in these conditions increases were not detected.

Mr. F. D. Bethwaite spoke of clouds over south-western Australia, which he had observed developing in the same air mass as it crossed the coastline and moved inland. Near the coast, stratus or strato-cumulus clouds were mostly formed which were shallow, wholly warmer than freezing, precipitated readily by coalescence and the individual cells were of small duration. As the air mass drifted further inland, clouds became progressively more scattered, of longer duration, deeper and less likely to precipitate. Finally they became so deep that their tops became cold enough to glaciate. There was often a considerable zone starting a few hundred miles from the sea in which precipitation by the coalescence process had ceased, precipitation by the ice crystal process had not started and there were cumulus clouds with supercooled tops apparently suitable for seeding with freezing nuclei.

Professor Braham described the clouds in the cloud-seeding experiment near Tucson, Arizona. These were orographic cumuli with individual convective turrets having a 15-minute life, building over an isolated range of mountains about 7,000 feet above the surrounding plain. Their tops were typically at about 30,000 feet. Radar echoes from precipitation usually started at about 15,000 to 20,000 feet and almost always descended, moving less than 1 mile horizontally.

An attempt was then made to decide the measurements which should be made in order to specify the types of cloud involved in a given experiment. While at the present it was not thought possible to state precisely which features were relevant, the observations ideally would cover three fields: i) Microphysical, including the cloud droplet spectrum and the concentration of the nuclei of the type with which the clouds were to be seeded. ii) Macrophysical, including the air mass type and trajectory. iii) Intermediate, including cloud type, size, durability, etc.

The question of whether the necessary information could be obtained from the ground, e. g. by balloons was raised by Naqvi and Elamly, but general agreement was expressed that it could only be obtained in a manner acceptable for cloud physics purposes by the use of aircraft.

The next question discussed was the distribution of effort which should be devoted to single cloud experiments, in which a cloud is seeded and its subsequent history is observed in as much detail as possible, and the area type of experiment in which the clouds are seeded and attention is primarily directed to changes in areal precipitation. Most delegates (e. g. Schaefer and Marshall) were of the opinion that both types of experiment should be performed, as each complemented the other. A general suggestion was made by Magnitsky for experiments on a still larger scale, for which purpose Britton suggested the Maldive Archipelago, where larger numbers of homogeneous clouds were reported in uncontaminated air.

C. Miscellaneous

i) Stimulation of the coalescence process

Seeding of warm clouds by various means was briefly reported, including water drops, salt, carbon black and electrostatic charges. Probably the most definitive experiments have involved direct seeding with water drops from aircraft. Braham stated that in the Caribbean this technique produced radar echoes, but the cost was prohibitive. Promising early experiments in Australia have not been confirmed subsequently.

Carbon black, in principle, might influence the growth of clouds. Preliminary experiments were reported (Cunningham and Adderley) which indicated that quantities of the order of tons were required to produce a significant effect and coagulation difficulties were considerable.

ii) The use of sonic or supersonic waves

In principle these could influence coalescence. However, typical cloud droplets are so big that theory indicates that a prohibitive amount of energy would be required to cause an appreciable effect. Schaefer reported experiments which tended to confirm this conclusion.

iii) What is the required concentration of freezing nuclei for the release or inhibition of precipitation?

Considerable discussion of the required number of nuclei revealed that, while several theoretical estimates have been made, supporting evidence is conspicuously lacking. The estimates were of the order of one per litre for stimulating precipitation

or several per c. c. for "overseeding".

It was pointed out that both might be over-estimates if fragmentation proved to be important: the general feeling was that it probably is important in some conditions.

Summary of Conclusions

A. At the present there is no proved substitute for the average of a network of precipitation gauges for the measurement of precipitation over an area, but other methods such as radar may well be of value, at least in flat terrain.

B. (i) Experimenters should specify the types of clouds on which they are working in as much detail as possible. (ii) Both experiments on seeding single clouds and experiments on the effects of seeding on areal precipitation should be continued.

IUGG MONOGRAPHS

Monograph N° 1 - **Seismicity of Europe,** by M. Bath ($ 0,30, 2/6, NF 1,50)

Monograph N° 2 - **The International Union of Geodesy and Geophysics : its scientific role, its international character, its organization** (out of print)

Monograph N° 3 - **Symposium on Atmospheric Ozone,** Oxford, 20-25 July 1959 ($ 0,40, 3/-, NF 2,00)

Monograph N° 4 - **Symposium on Radiation,** Oxford, 20-25 July 1959 ($ 0,40, 3/-, NF 2,00)

Monograph N° 5 - **Antarctic Symposium,** Buenos Aires, November 1959 ($ 1,00, 7/6, NF 5,00)

Monograph N° 6 - **Commission Séismologique Européenne,** Réunion d'Alicante, 27-31 Octobre 1959 ($ 0,40, 3/-, NF 2,00)

Monograph N° 7 - **The July 1959 Events and Associated Phenomena** ($ 2,50, 18/-, NF 12,50)

Monograph N° 8 - **Zonal Harmonic Analysis of the Northern Hemisphere Geostrophic Wind Field,** by J. Van Mieghem ($ 1,00, 7/6, NF 5,00)

Monograph N° 9 - **Seismicity of Europe,** Part II, by V. Karnik ($ 0,40, 3/-, NF 2,00)

Monograph N° 10 - **Symposium on the Radiant Energy in the Sea,** Edited by N. Jerlov ($ 2,50, 18/-, NF 12,50)

Monograph N° 11a - **Instruction Manual on World Magnetic Survey,** by E. H. Vestine, 27 pp., 6 ill. ($ 0,30, 2/6, NF 1,50)

Monograph N° 11b - **Manuel d'Opérations pour le Levé Magnétique Mondial,** par E.H. Vestine, 28 pp., 6 ill. ($ 0,30, 2/6, NF 1,50)

Monograph N° 12 - **Symposium on Geophysical Aspects of Cosmic Rays,** Helsinki, July 1960 ($ 3,00, 21/-, NF 15,00)

Monograph N° 13 - **Catalogue of Published Mean Sea Level Data (1807-1958)** IAPO Publication Scientifique N° 23 ($ 0,60, 5/-, NF 3,00)

Monograph N° 14 - **Report of the Meeting on the International Seismological Summary,** Paris, July 1961 ($ 0,40, 3/-, NF 2,00)

Monograph N° 15 - **Report of the UNESCO Seismological Survey Mission to South East Asia 1961** ($ 0,40, 3/-, NF 2,00)

Monograph N° 16 - **International Conference on Cloud Physics,** Australia, September 1961 ($ 1,00, 7/6, NF 5,00)

IN PREPARATION

Monograph N° 17 - **Symposium on Atmospheric Ozone,** Aroza, 14-19 August 1961 (about 100 pages).