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Physical View of Cloud Seeding

A review of experimental data indicates that we are considerably further ahead than is generally realized.

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From the very beginning of cloud seeding in 1946, the subject has been controversial. There are many reasons for the controversies, not the least of which have been the strong personalities, beliefs, and convictions of the people involved. Throughout it all there has been an underlying uncertainty about weather phenomena in general. The question "Would it have rained, anyway?" has been unanswered and, until recently, unanswerable.

In this article I wish to present some views based on 25 years of intermittent association with this field. For the last decade I have been intensely involved in studies on the foundations of inductive logic and scientific inference. These two fields are basic to attempts to establish scientific hypotheses about weather modification. In the title of this paper I have emphasized the word *physical* because I wish to distinguish it from the *statistical* view. The distinction is extremely important to the planning and the prosecution of research. Statistical methods now routinely employed do not, in general, take into account known mechanisms or the physics of a process. I have written elsewhere on this weakness (*1*) and have used the following example: Suppose I claimed clairvoyant capabilities and, in support of this claim, I correctly predicted the makeup of the front page of the *New York Times* one week in advance, down to the smallest detail, including a few typographical errors. Would not your common sense tell you my claim was valid? For problems that use as evidence the results of a single observation, there are no classical statistical procedures. Most of us rely on common sense when evaluating this type of problem. It is possible (*1*) to develop procedures that can handle this class of problem, but the new procedures have not yet been generally accepted as valid by the majority of statisticians. (See the appendix for a brief mathematical analysis of the problems of clairvoyance and weather modification.) It is my belief, however, that the methods will be accepted in the not too distant future. The reason is simple: today we are dealing with many problems that require these new techniques, and necessity is still the mother of invention.

As I have argued (*1*), the main weakness of most existing statistical approaches is that they do not permit us to use all that we really know. It is not that the available methods are "wrong"—it is that they are inadequate. Cloud seeding is a complex process. During the process there are many intermediate stages of development, each with its own measurable characteristics. Thus, when a cloud is seeded, we observe such variables as the cloud base height, the updraft velocity distribution, the radar echoes from the core, the rate of growth of the top of the cloud, the pattern of the convective activity, the temperature distribution and the time at which water starts to fall from the cloud, where the water falls, and how much evaporates. When we compare these results with the prediction of a digital computer, a single observation in which we obtain very good agreement in all details obviously weighs more heavily in our minds than does a single statistical measure which merely considers the ratio of rainfall measured in gauges for seeded and nonseeded clouds. Of course, we must observe a few nonseeded clouds, in the same fine detail, to see if their behavior agrees with our computer prediction. But our common sense tells us that experiments that reveal this fine detail are more significant than experiments that do not.

The issue is not a trivial exercise in logic—a mere comparison of preferences as regards the proof of scientific claims. The perspective adopted determines how an experiment is planned and therefore how much money is spent in coming to a particular state of knowledge. Usually the biggest single item of expense involves the operation of aircraft. In addition to the capital costs of aircraft and sophisticated instrumentation, such factors as the cost of flight crews, fuel, and aircraft overhaul make each hour of flight time very expensive. Today it is not uncommon for experimenters to fly about half the missions without seeding, just to produce some "randomized results." My quarrel is not with the validity of the statistical approach—it is whether the classical statistical approach to design of experiments ought to be followed without regard to the expense. A more useful tool is decision analysis, which adds statistical considerations to cost parameters. Some experimenters, incidentally, recognize this fact intuitively and bias the randomization by making the odds two to one in favor of seeding on a given flight. Even if decision analysis is used, how-

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ever, it should not be applied without taking into account the physics of the process. Decisions are based on descriptions, and it is important that, in the process of developing a statistical description of our state of knowledge, we not ignore some data just because our mathematical tools cannot encompass them.

The key to what we call scientific understanding is the elucidation of mechanisms—that is, chains of events—each of which we can understand and link with others to describe an outcome. If all these individual events permit different outcomes, the final outcome may indeed be so variable as to make it very difficult to prove our knowledge by study of only the final outcome. If purely statistical methods are employed, some intermediate measures (that is, parameters) may be included in a statistical analysis via "stratification," but stratification requires that there be more, not fewer, data points to reach a given "confidence level." In systems as complex as meteorological ones, I argue that we will move ahead faster if we spend money to get a greater variety of carefully planned observations instead of a greater number.

I wish to consider first the general scientific view of cloud seeding and then particularize this view to specific applications. Then I shall return to the use and misuse of statistics in this field.

General Look at Cloud Seeding

As Joanne Simpson (*2*) has observed, analyses of cloud seeding may be considered to be either static or dynamic. A static analysis is one in which the natural flow field is not markedly affected by seeding. Static analysis seems to be appropriate to quiescent cloud chamber experiments, stratus clouds, and orographic lifting such as occurs either at a mountain range or in the frictional boundary layer where the wind over water first meets the land. In orographic lifting the buoyancy forces generated by seeding are usually too weak to cause a relative motion between air masses. The resultant motions of air are nearly the same with and without seeding. The practical effect is that, in describing the fluid flow field, the energy and momentum equations are decoupled and may be solved separately.

In a dynamic analysis, on the other hand, intervention markedly affects the flow field (for example, by artificial buoyancy), and the gross features of the cloud may be greatly modified by the presence or absence of phase changes in the cloud water. Complete diagnostic analyses are more difficult to perform because they inherently involve solving the equations of motion of a three-phase system (air, water, ice) in a nonisothermal field with the energy and momentum equations inextricably linked together. Although complete solutions starting from the primitive equation of energy, momentum, thermodynamic state, and rate processes would be desirable, they have not been obtained for many, even simpler systems in meteorology. It is not a weakness peculiar to cloud seeding that we must be satisfied, as of now, with approximate solutions. The approximate solutions for mixing, diffusion, and cloud droplet growth can be individually checked and put together to form a system of equations. Even the approximations are quite complex, and a digital computer is essential if any analysis at all is to be carried out. Computer analysis is therefore an essential adjunct of useful experimentation, for experiment without analysis cannot provide the basis for prediction.

Certain features are common to both static and dynamic analyses. In both, it is postulated that humid air is cooled by expansion and that initially small droplets of water are formed, too tiny to fall relative to the air. In the cloud chamber (*3*) the expansion rate may be controlled by evacuation pumps. In orographic lifting the rate of expansion of cloud mass is often controlled by the larger circulation patterns, which impose the motion but are relatively unaffected by cloud seeding.

In cumulus clouds, on the other hand, the rate of expansion is determined by the buoyancy forces, which in turn are determined by the condensation rate and by the vertical variation of the basic horizontal velocity field (which is why I said the equations of energy and momentum were inextricably linked).

When the cloud temperature goes below 0°C, most of the droplets do not turn to ice but remain supercooled. The size of the droplets at any stage depends upon the number of condensation nuclei present in the original air mass, the rate of cooling, the original humidity, and the number of various kinds of freezing nuclei present—that is, substances that catalyze the transition from water to ice. We still do not have adequate information on the number of ice and condensation nuclei necessary to initiate and sustain the precipitation process. Techniques now in use have exhibited inconsistencies up to 9 orders of magnitude when employed by various investigators. Development of more accurate methods for measuring these particles together with increased studies of the basic mechanisms of the nucleation process are urgently needed; if successful, they will result in more efficient cloud seeding techniques.

Langmuir (*4*) has described the furious competition between small and large drops which results in the growth of large ones at the expense of the small. This competition is greatly affected by the rate of expansion (which may be so large as to cause all drops to grow), the temperature (low temperatures reduce vapor pressures and hence growth rates), and the presence of freezing nuclei. Since the vapor pressure of ice is much lower than that of supercooled water at the same temperature, all liquid water drops tend to vaporize into the freezing nuclei, thereby liberating additional heat of freezing and artificial buoyancy. Also, the lowered saturation vapor pressure causes more condensation from the cloud air mass, which further enhances buoyancy. The "triggering effect" can be spectacular; it can create in seeded clouds strengthened updrafts leading to a vertical growth 4 to 5 kilometers higher than the tops of unseeded clouds. This growth leads to further condensation and frequently to increased "natural" precipitation.

Of course the effect of the introduction of "artificial" freezing nuclei into a cloud depends in part upon how many were already there from "natural" causes. I put quotation marks around the words "artificial" and "natural" because at many locations man's activities now so pollute the atmosphere that we cannot always distinguish "natural" from "artificial."

The behavior of the particles in a cloud is strongly dependent upon the interaction among nuclei, velocity field, and temperature. For a given amount of condensation, the more nuclei there are, the smaller the droplets will be. Small droplets will fall relative to the air more slowly than large ones and may, therefore, be carried up within the cloud. The temperature determines not only the amount of water vapor in saturated air; it also has an important effect upon diffusion and growth processes. In a few systems, especially systems in which the air motion is not affected by the thermal processes associated with condensation, it is possible to find simple solutions to the cloud growth equations without recourse to a digital computer. Langmuir's "time of rise" treatment of a cloud growth on Mount Washington is an example (*5*). In this work Langmuir successfully predicted the sizes of drops that would be measured at the summit, though he worked only with information about the cloud base height, the velocity of the wind (which determines the time for the droplets to pass from cloud base to summit), and the temperature. But usually the equations are so complex that they can be handled only on a digital computer.

When the energy released during condensation is large, the cloud density changes. The velocity field is then determined by the difference between the density that occurs inside the cloud and the density outside the cloud. If this density difference between the inside and the outside of the cloud is large, the result can be an extremely strong updraft in the cloud. This difference depends strongly on the ambient humidity and temperature lapse rate. In tropical clouds, which develop in a moist environment, it is not uncommon to see a cloud grow to 15,000 meters. Since the buoyancy effects are integrated over the vertical extent of a cloud, under some conditions seeding from the top can kill the cloud. This effect occurs when a dry stable layer surrounds the cloud's midsection with a less stable layer above. Then seeding can cause a too rapid growth of the cloud top compared with the condensation rate beneath it. Cold air comes in from the side and causes the top of the cloud to break away from the base. The total buoyancy force available within the cloud is thus diminished, and the base collapses.

There are more considerations. When cloud drops grow large enough to fall, they descend through smaller drops, collecting and coalescing as they go. The dynamics of this collection process have been extensively studied, both experimentally (*4*) and analytically.

Cloud seeding therefore involves many considerations of fluid mechanics, heat transfer, diffusion, thermodynamics, two-phase flow, particle dynamics, and surface chemistry. Instrumentation includes radars, reconnaissance aircraft, precipitation gauges, radiosondes, dropsondes, kites, cloud photography, and the human eye. Predicting detailed behavior in such a complex system resembles the problem of predicting the front page makeup of the *New York Times*.

The interplay of these effects can produce bewildering results for those who take the pragmatic view that all that counts is the results and who pay no attention to the elucidation of mechanisms. Thus, at very low temperatures there are many kinds of particles that can act as ice-nucleating agents. At higher temperatures, there are fewer active nuclei. Seeding at low temperatures, therefore, may produce an overseeded condition and reduce the rainfall. Seeding at higher temperatures can increase the rainfall. Even the level in the cloud at which the seeding occurred may make a difference. Neyman analyzed the Whitetop experiment statistically by lumping all seedings together and counting only rainfall. He thereby showed there had been a net decrease in rainfall due to cloud seeding (*6*). But Flueck (*7*) analyzed the same data, this time stratified as to maximum radar echo top heights, and showed that low cloud tops (less than 6100 meters above mean sea level) resulted in slight but not statistically significant decreases in precipitation, that intermediate cloud tops were associated with substantial increases, and that sufficiently high tops (more than 12,200 meters above mean sea level) favored significant decreases. The Neyman and Flueck analyses do not conflict. It is true, as Neyman says, that indiscriminate seeding without complete knowledge of the physics involved can lead to an unintended result. But Flueck's calculations also show that knowledge of the basic mechanisms involved permits the selection of optimum techniques for desired control. Neyman's methods of analysis do not admit the use of whatever insights were available from other experiments about the physics of the process. On the postulate that all future seeding activities will be conducted in equal ignorance, Neyman's warnings certainly follow from his analysis. But, as I have written elsewhere, we do know more than what is learned by reading rain gauges (*8*). It was on this basis that I asserted that we are closer to the capability to do operational cloud seeding than Neyman's article indicated. Let us turn to a review of the experiences upon which that assertion is based.

Seeding Applied to Orographic Lifting

Orographic lifting is one of the simpler cases to analyze because, even with seeding, the resultant buoyancy forces are usually too weak to have much effect on the overall motion. In a fixed fluid flow field, the instrumentation can be used to measure many details of the system with or without seeding.

The broad-scale synoptic situation induces a flow up the mountainside, which in turn produces a cloud that appears to hang motionless over the top of the mountain. Actually the cloud is extremely active, with moisture entering the cloud base and droplets growing as the air ascends. Vaporization takes place on the lee side as the air is compressed.

At the South Dakota School of Mines and Technology, under the sponsorship of the Bureau of Reclamation and the Department of Defense, Orville (*9*) has used detailed descriptions summarized by Kessler (*10*) (see Fig. 1) to study the effect of the various precipitation processes in a numerical simulation of cloud development over a mountain barrier. The mathematical model is two-dimensional and incorporates a vertical wind shear and an initially stable, incompressible atmosphere. The rain shower model is programmed so that each cloud affects its own development during its life cycle. As the multiple clouds form and grow into the mature stage, cloud shadow effects combine with the downdraft influences that result from evaporative processes. The behavior of the clouds is most realistic, and it will be of great interest to compare Orville's results with field measurements.

Under National Science Foundation sponsorship, Lewis Grant and his colleagues at Colorado State University have produced what may be regarded as a classic experiment in atmospheric sciences. Figure 2 shows the system that Grant chose for study (*3*). By using scale models, they have investigated the flow field over the mountain and have compared it with field measurements obtained by radiosondes, constant-level balloons, and tethered kites and parafoils. In a dynamic cloud chamber they have observed the rates of droplet growth and nucleation under conditions approximating conditions on the mountain. With tracer techniques they have tracked the release of particles from ground-based generators to learn how to put nuclei where they

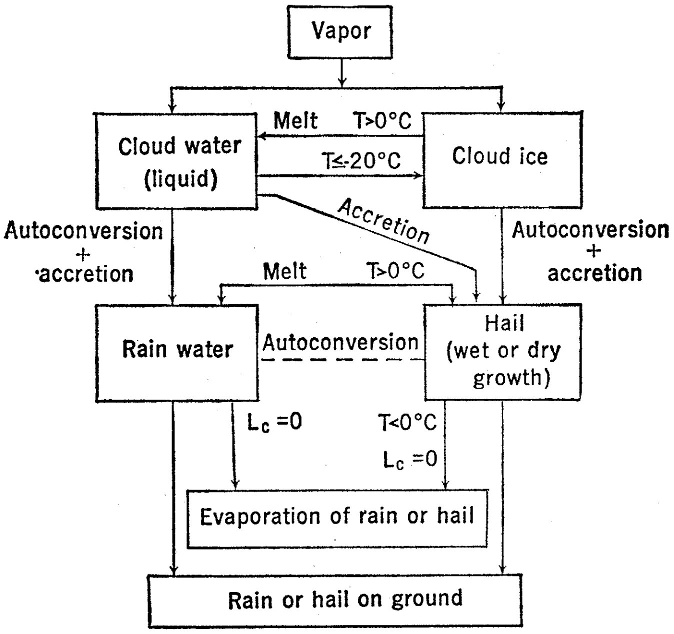

Fig. 1. Schematic diagram of cloud physics processes in cumuli.

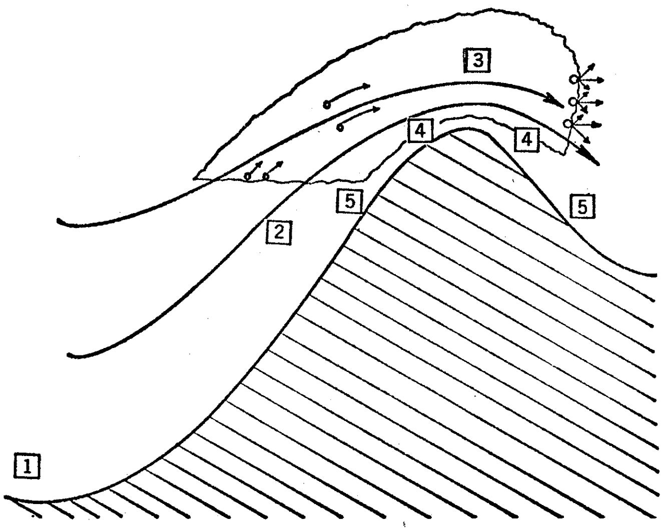

Fig. 2. Systems approach to orographic cloud and orographic cloud modification research. Number 1, source of nucleation and seeding agents; number 2, zone of transport of seeding material; number 3, zone in which cloud and precipitation physics dominate; number 4, zone for measurement of natural and artificial precipitation; number 5, zones for hydrologic measurements.

want them over the mountain; they have checked field data against model data. By means of nuclei counters suspended from kites, they have measured the number of "natural" nuclei as well as the number of nuclei produced by their generators. In addition to rainfall and snowfall gauges, they have utilized stream-gauging stations on the upwind and downwind sides of the mountain. Finally, they have developed a computer simulation of the process and have shown excellent agreement with experiment in the fine details of the mathematical model (*11*). By using the computer they can tell how to deposit snow on either the upwind or downwind slopes.

The practical consequences are most important. By seeding on days with relatively warm cloud tops, when there are few natural nuclei and there is plenty of water, they found they could increase the snowfall by factors of 2 or 3 on these days. If they wished to overseed on cold days, they could reduce snowfall, perhaps even to zero in some cases!

The agreement between the computed and the observed results, particularly in the fine details (*11*), seems to me to be persuasive enough to allow us to go forward to apply such techniques to a number of river basins. The applications should not be made blindly. It is tempting merely to set up seeding stations and snowpack measurements and to see if any good comes of it. To do so would be to repeat the mistakes of the past 20 years. Each situation in which it has been decided to affect the snowfall should be studied carefully, and field measurements should be made ahead of time to determine such factors as the prevalent wind conditions, rates of orographic lifting, and the numbers of nuclei present naturally. Computer models, perhaps even wind tunnel models, should be used. In other words, methods such as those developed by Grant, and by other groups as well (*12*), may be employed to survey a given locality and to determine whether or not the conditions are likely to be favorable. An economic evaluation and an assessment of the various interests that might be affected need to be made. On the basis of such surveys, we can decide whether or not to undertake the project, to invest the money in seeding and monitoring equipment. Because of the thoroughness with which Grant and his colleagues have approached their work, we can now consider the task to be one of engineering, not science. The Bureau of Reclamation shares this judgment and is now acting on it in its Upper Colorado River Pilot Project. We shall no doubt find that there are improvements that ought to be made in the methods developed by Grant—certainly many of the details can be refined. There is still a great need to develop better instrumentation, particularly remote sensors that can measure nuclei, snowpack, snowfall, runoff, and the many other variables that affect the hydrologic system. These improvements will have an important bearing upon the ratio of benefit to cost in operating systems, for, if we resort to blindly seeding every cloud that comes over the mountains, we shall probably do more harm than good.

It is important to stress that in each attempt at operational snowpack augmentation the instrumentation plan should be very thorough. Many years of experience show how easy it is to fool ourselves. Unless there is good agreement between the observations and the analysis, claims of success are rightfully viewed with skepticism. More importantly, opportunities to gain efficiency will be lost.

Lakeshore Snow Removal

Another example of lifting over a geographic feature is represented by the flow of cold air over the Great Lakes toward, say, Buffalo.

The air mass passing over Lake Erie from land to lake changes its vertical flux of momentum drag because of roughness differences between land and water. Also, the flux of heat changes drastically as cold winter air passes over the lake (which may be 20° warmer than the air). When the air reaches the other coastline, the changes in drag and in buoyancy that are superposed on the mountain effects lead to a drastic increase in depth of the cold air mass. This action, in combination with the flux of water vapor from the lake, is responsible for the heavy snowfalls on the southern shores of the Great Lakes (Fig. 3).

The lifting of an air mass over the shore is much more sensitive to the flow field in the general circulation than it is to flow over a mountain. Once the flow field for the lakefront region is worked out, which can best be done with a digital computer, the seeding needed to produce a desired effect may be found by using a computer model that takes into consideration the topography of the lake, the temperature difference between lake and land, and the same factors used by Grant. By choice of seeding techniques, according to a computer model developed for the Buffalo area by Lavoie (*13*), it should be possible to drop the snow or rain near the lake (small number of nuclei) or, if the city wishes to avoid snow, some distance inland (many nuclei). Experiments over Lake Erie that are now under way tend to substantiate these arguments. Weickmann hopes to produce the detailed data required to verify the analysis. If verification is obtained, these results can be extended to the conclusion that any city that wished to purchase snowstorm protection (from similar lake or sea winds) could plan to do so if an intelligent and careful instrumentation and simulation plan were adopted. What is needed is a fluid flow description that correctly takes into account the local situation and that has been verified beforehand. Clearly, this technique can be developed, and much work needs to be done. Continued effort, in my view, could lead to operational methods in the near future. I base this prediction not on any statistical analysis (none has been performed) but on my knowledge of the physical processes and how our knowledge of them is advancing. In this regard the experiences of others, such as Grant and Simpson (whose work will be discussed next), have as much bearing on this conclusion as does the work of Weickmann. If we were to rely on statistical analysis alone, we would not be justified in asserting anything about the chances of success in diverting lakefront storms. But we do know that the same laws of physics apply, and we do know a great deal about fluid mechanisms; therefore we can plan ahead with the confidence that we know a great deal about what we are doing.

Stratus Clouds: A Poor Prospect for Rain

The earliest experimental work was carried out in stratus clouds, and the results may be analyzed statically. Although such experiments may be of interest to those who wish to modify the radiation balance, they are not very interesting to those who wish to make rain. The atmospheric conditions are usually just too stable. The buoyant forces usually produce a fine-scale turbulence, which distributes the ice nuclei and thus nibbles away the cloud. But no large-scale buoyancy is produced. When a stratus deck is low and near an airport, it interferes with aviation. If the temperature is below 0°C, it can be readily dissipated (*14*).

Cumulus Clouds

Cumulus clouds require a dynamic analysis, for the buoyancy induced by seeding can provide the mechanism to turn an innocent-looking, small summer cloud into a towering thunderhead that pumps warm surface air to 12,000 meters and drops many centimeters of water on the earth below. The behavior of cumulus clouds has been extensively examined by Joanne Simpson and others (*15*), and her results are a model of simplicity and brilliance. Simpson's computer model enables her to predict, in advance of seeding, certain important characteristics of seeded and unseeded clouds: namely, the maximum top heights; whether the cloud top will detach and drift away from the base; the strength of the radar echoes; the amount of precipitation formed in and falling from the rising tower, the rise rate of the tower, and its temperature above that of the environment.

Simpson has proved that her computer model works by comparing the predicted and actual behavior of over 30 clouds in fine detail. It is true that the computer model is based on the use of rather simple subroutines. It represents the vertical flow in the cloud in terms of the one-dimensional empirical description of jets. Other simple descriptions are used. But the descriptions are approximately correct and are verifiable. Any description of the cloud flow caused by buoyancy will, given today's state of affairs in fluid mechanics, contain a certain amount of empiricism. The question is not, "Does the analysis start from fundamentals?" Rather the proper question is, "Does the empirical description correspond well enough with reality?" In this case, it does.

Figure 4 shows the kind of agreement between analysis and experiment that Simpson has been able to obtain. From her computer analysis she predicts how high a cloud should grow after seeding. Then she seeds it and compares results. The agreement between calculation and observation leaves nothing to be desired.

The work of Simpson and the work of Grant corroborate each other. Both show that, when the temperature is too low (or the air is too dry), seeding can decrease rather than increase rainfall. By choosing a cloud judiciously, a cloud that would not have dropped rain can be seeded to produce rain. In some cases the rainfall can be doubled or tripled predictably.

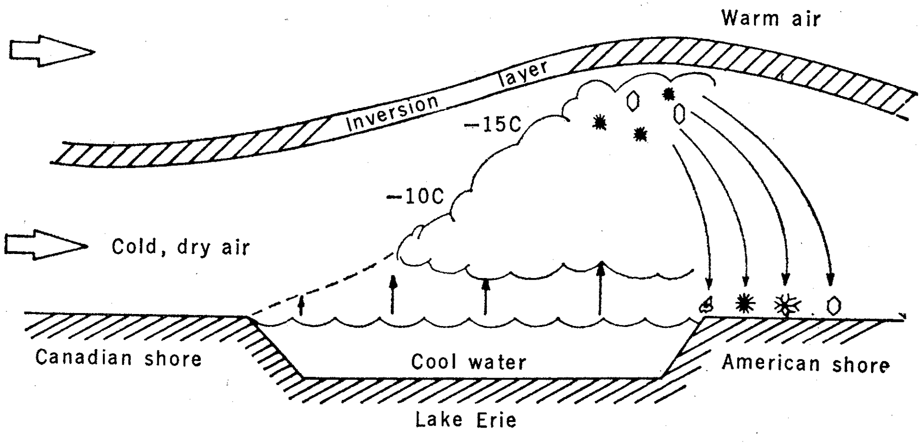

Fig. 3. Great Lakes snowstorms occur when currents of cold arctic air sweep across the warmer waters of the lakes, where they pick up heat and moisture. This moisture is then deposited in the form of snow over the downwind lakeshores.

The Simpson approach involves the addition of a massive amount of silver iodide (kilograms compared with grams) to produce a very large number of nuclei to "prime the pump" and produce precipitation as a result of artificially stimulated growth. As the cloud grows in height and if the upper air is not too dry (and the other conditions are predicted to be favorable by the computer), the convective activity is not only initiated but, once initiated, will continue long after the seeding is over.

The issue of whether or not to use these techniques operationally depends upon many factors not yet known. For example, how often do the "seedable" clouds occur? Are they available in a season in which the cost of added water is worth the expense?

This point should be stressed: Joanne Simpson's work is valuable for the insight it gives, not for the rain. She correctly left to the last the measurement of rainfall, and the question "How much extra rain was produced?" was subordinated to the question "How well do I know what I am doing?" The commitment is to understanding, not to rainmaking, and the analysis is of validity, not liquidity.

There are still many things to learn, of course, but, if we accept Simpson's analysis, many opportunities for application will suggest themselves to the creative operator.

Hail Reduction

Our progress in hail reduction has been curious. We have only recently seen vigorous attempts to validate a mathematical model of the process.

Progress in this field, as in other cloud seeding, also requires the development of both computer models and observations. The developments must go hand in hand. The analysis tells us what to observe, and the observations tell us what to analyze. The initiation of the process might well be considered a "chicken and egg" proposition, incapable of being started were it not for the fact that we do not now start from complete ignorance or innocence. We already know a great deal about cloud physics, for many laboratory studies have been made. We also have many observations. It is time to put together a simple, consistent computer simulation of a hail system and preferably to run it in real time, both to guide and to compare field observations. The instrumentation plan ought to be designed to check the fine details of the computer model. If the two approaches are not developed together, I see no real hope for the development of understanding.

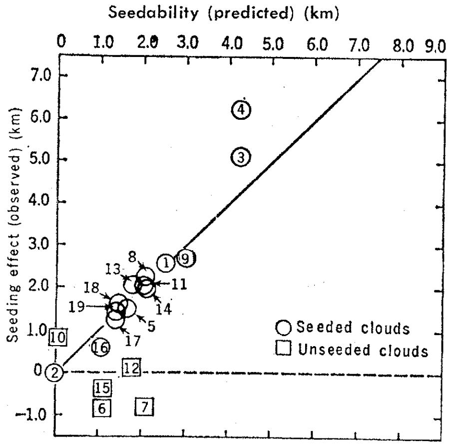

Fig. 4. Seedability versus seeding effect (Florida, 1968) (*2*).

Hail is formed in clouds that are able to provide for hail embryos a long trajectory through regions containing appreciable quantities of supercooled water. There are a number of simple cloud models in which this requirement is met. Even without the benefit of computer analysis, I strongly suspect that there ought to be many ways to turn off a hailstorm. Changes in supercooled water concentrations, updraft speeds, and embryo concentrations could provide the means for reductions in hail.

Recent results from field seeding trials yield tantalizing data that suggest that we may be uncovering some of the techniques for effective hail suppression. Perhaps best known are the reports from the Soviet Union (*16*), which indicate impressive reductions in hail damage from direct injection (via cannons, rockets) of lead iodide into the heart of the cloud, located by radar. Progress in the development of hail control procedures in the Soviet Union is such that a senior Soviet scientist is quoted (*17*) as stating that ". . . the problem of hail control is successfully solved. . . ." A review of the effects of seeding on hail (*18*) indicates that methods other than direct injection of seeding agents, as used in the Soviet Union, may be better.

The exact mechanisms for hailstone growth and the processes by which cloud seeding procedures might be employed to turn off a hailstorm are still largely subjects of speculation. Some numerical analysis has become available recently. The early work initiated by Ludlam (*19*) and Douglas (*20*) has been continued more recently by Musil (*21*), whose calculations indicate that the primary zone of hail growth is at relatively cold temperatures near −20°C. If substantiated by further numerical analysis and field observation, the simple concept of glaciating the upper portions of cumulonimbus clouds may yet be the most straightforward and promising approach to hail suppression (*22*).

Sufficient systematic observational evidence now exists to permit general qualitative agreement on the essentials of hailstorm structure. Because there is as yet no agreement on quantitative representations, however, there is also disagreement on how best to proceed. The suggestions by Henderson (*23*) and others for seeding the trailing edge of a hailstorm cloud appear reasonable, since this zone is frequently the source of air for the hail "factory." But then, other proposals, such as those of the Russians, appear equally reasonable.

Future research will likely reveal additional approaches to restrict the growth of hailstones. A proposal, now being developed for National Science Foundation support, for a major experiment in hail suppression should provide further insight and must include a determined effort to develop computer simulation and the observational system simultaneously. If this effort is successful, there is reason to hope that effective economic, predictable systems for hail suppression can be developed for routine application in this decade.

Hurricane Modification

Project Stormfury, the hurricane modification program of the Environmental Science Services Administration and the Department of Defense, is an excellent illustration of the need and opportunity for the more effective experimentation that I have been proposing here. During the past decade we have tested our ability to modify hurricanes deliberately on three occasions: Hurricane Esther in 1961, Hurricane Beulah in 1963, and Hurricane Debbie in 1969.

The challenge posed by the new results from Project Stormfury demands the highest abilities from the scientific community. The damage from Hurricane Camille in 1969 was measured in billions of dollars, thousands of disrupted lives, and many deaths. The mathematical modeling of hurricane processes is of necessity based on many simplified assumptions. According to the working hypothesis, as described by Gentry (*24*) and the Simpsons (*25*), massive seeding can produce a highly localized warming effect in the hurricane system and can alter the circulation. According to the analysis, if a density decrease is produced just outside the region of maximum tangential velocity (that is, just outside the "eyewall"), the point of maximum velocity will move radially outward, and the peak velocity will diminish. The mathematical model is at present based on very simple treatments of the connection between seeding and density change, and thus it is difficult to compare observation and experiment directly. The computer analysis indicates that if the reduced density is produced inside the eyewall, the peak velocity may increase!

There is yet another consideration. Up to one-sixth of the moisture received from June to October by the eastern United States comes from hurricanes. Considerable effort must therefore be expended to be certain that even weakly applied hurricane suppression will not produce water shortages. Clearly this is not a field for random experimentation.

The exciting news that has come out of these experiments is that on each occasion the resulting changes in hurricane structure, although within the approximate range of natural variations of such storms, have been in the direction predicted by the basic Stormfury hypothesis. But the fact remains that the necessity to provide adequate public safeguards from any unforeseen effects of experimentation and the formidable logistics problems involved in mounting large experiments of the Stormfury class have limited the number of trials to only three attempts in 9 years. The infrequency of "seedable" hurricanes, when taken in conjunction with the very high costs of conducting hurricane modification missions (on which I have already commented), limit very seriously our ability to run a large number of blind experiments for the sake of providing a statistical test of any given seeding hypothesis. What then is the alternative? I feel strongly that the reasonable answer is to place primary reliance on theoretical approaches to the hurricane modification problem. It follows that we must develop an improved analytical plan, so that we can make better theoretical use of the information we collect. And concurrently, this effort will require an improved instrumentation plan, so that we can acquire more specific kinds of data. By a process of feedback, the data gathered will be used to test and improve our ability to simulate by computer methods a physical sequence of events such as (i) growth of the eyewall in a predicted sector of the hurricane, (ii) changes in the tangential and radial component of the wind, (iii) collapse of the eyewall, (iv) changes in the pressure, tangential field, and the radial pressure gradient, (v) rebuilding of the eyewall in a given sector at a larger radius, and so on, all as a function of time. The approach I have outlined here will prove to be a much more productive means for advancing the state of our knowledge of hurricane energetics than reliance upon statistical treatments of the numbers we could hope to collect by attempting to repeat the "same" experiment a large number of times.

Lightning Modification

Another area of concern in which cloud seeding can play a role is ignition of forest fires by lightning. The arguments about rainmaking attempts also apply to lightning suppression. Just as it is insufficient merely to look at the data from rain gauges, so is it insufficient merely to count lightning strikes.

Recent progress in this field is encouraging because new insights are being generated. It has been observed that the main source of ignition is the hybrid lightning discharge. Recent experiments with seeding indicate that these discharges can be modified (*26*). Since we know that there is an intimate connection between the cloud dynamics and cloud electrification, this result is to be expected on theoretical grounds; that is, the dependency should surprise no one. Plans for Project Skyfire include development of better computer models, which will be compared in detail with field observations. From the evidence at hand, it seems fair to place lightning suppression in the category of weather modification activities—as nearly ready to use.

The Role of Statistics

In the preceding sections I have taken some pretty hard swipes at statistical methods, and therefore it is my obligation to say a few words about how I believe statistical methods ought to be used in the field of weather modification. Elsewhere I have set forth in detail my approach to the field of statistics (*1*). Statistical methods have an important role to play, but, in my view, not the role that has been used for about 20 years.

While I was in the Army Air Corps at Wright Field in 1945 and was responsible for solving the aircraft icing problem, I became very much involved with Irving Langmuir and Vincent Schaefer at the General Electric Research Laboratory in Schenectady. I arranged to support their work at Schenectady and Mount Washington, which was concerned with aircraft icing and the nature of supercooled clouds, growth of particles, and nucleation. This study was responsible for Schaefer's discovery in July 1946 of the effectiveness of dry ice in the seeding of supercooled clouds.

I was actively interested in the conclusion of Langmuir and Schaefer that they could do something to natural clouds on a massive scale. I was much impressed when Schaefer demonstrated the validity of their ideas with the first successful seeding of a natural cloud on 13 November 1946 and was also impressed with the results of more than 150 research flights under Project Cirrus from 1947 to 1952.

While at the University of California, Los Angeles, I used some of Schaefer's Mount Washington data in my heat transfer studies and thus remained in touch with the Project Cirrus Laboratory field and flight studies, which continued until 1953.

During the latter part of this period, Langmuir was diverted from his physical studies by his conclusion that periodic silver iodide seedings in New Mexico had a large effect on weather in the eastern United States. He devoted much effort to attempts to statistically verify his beliefs. In those days (nearly 25 years ago) many of us were unhappy to see so much of Langmuir's energies devoted to what now seems to have been a premature effort to explain the strange periodicities that occurred in the precipitation patterns at that time.

There is, of course, a continuing

battle between those who want to understand everything before they make a move and those who feel that they can make moves without always knowing the fine details of what they do. There is much to be said on both sides —if we had had to understand combustion in the fine detail with which we now understand nuclear fission, combustion chambers would never have been built. But nuclear reactors cannot be built the way we have built combustion chambers. And meteorology is too important to our lives to permit blind development of modification techniques. Meteorology is a more complex study than is combustion. The inability to do simple, reproducible experiments in the field changes the game.

Statistical methods are strongest when one can do a repeatable experiment—repeatable in the sense that essentially all the variables that can be controlled or observed are kept constant in repeated trials. However, when the known important variables are not susceptible to control and are not even measured and when the general effects of the variables are known, statistical methods are of more limited use. We have long known that the number of nuclei originally present in a cloud was important, that nuclei generators vary enormously in their outputs, and that their effectiveness depends greatly on temperature. Yet time after time we have, with inadequate control or measurement, run blind experiments in the hope that these and other effects would "average" out. But in a complex chain of events, such "averaging" may not be possible without an inordinate number of experiments.

For example, we now know [see (*6*)] that if we ignore cloud top temperature, there is only a modest dependence of rainfall on seeding. I have discussed the problem of finding statistical dependence when the dependence is weak (*1*, pp. 202–207). It is not easy. When the physics of the process is known and important variables are under control or observed, strong dependence can often be established. Basically we are seeking a signal masked by noise. Scientific knowledge is the filter that can hold back the noise. When we throw away the advantages of this filter, we must observe for much longer times to find the signal in the noise.

Langmuir emphasized the difference between what he called "convergent" phenomena and "divergent" phenomena. A convergent phenomenon was, by his definition, one in which statistical averaging took place to give a uniform result, even though the conditions at the start were highly nonuniform. Statistical mechanical systems are of this sort—perturbations vanish and the system goes to what we call an "equilibrium state." But many systems cannot usefully be analyzed by the methods of statistical mechanics. The chain of events is too complex and too dependent upon what happens at a particular "branch point." For example, if I stumble and fall while running through a traffic intersection, I may be run down by a truck. Clearly, this possibility would be an amplification, not an attenuation, of a perturbation. Recall George Herbert's famous proverb, which recounts what happens "all for want of a horseshoe nail." These events are divergent, and their possible paths are much too complex for us to understand by merely looking at simple measures of the outcomes. We might as well try to count military victories as a function of the supply of horseshoe nails. Uncontrolled amplification and unpredictable developments in different directions produce the divergence Langmuir cited. These systems can be studied, however, if the separate elements are studied one by one.

Here we come to the crux of the matter. Statistical methods are fair for evaluating the claims of those who pretend to do something without being able to describe precisely what it is they are doing. If I claim to be able to tell the color of dresses that women want simply by looking into their eyes but I do not reveal how I do it, it is proper to test this hypothesis by statistical means. But if I claim that I have a means for affecting the weather and if I claim to have a detailed understanding of how it occurs, the proper tool for testing this claim is not the statistical method. The proper tool is the observational method—that is, the use of detailed observation, which checks each element of the claim. As an extreme example of this approach, consider the testing of a nuclear power plant control system.

If my claim is not that I have a solution but rather that I have the beginnings of a solution, I assert that the blind application of statistics will be counterproductive. What we need is insight into the physics. Although statistical analysis can often provide clues as to which way to look for more insight, it can be expected to do so only when applied by someone who has the physical system clearly in mind.

Requirement for Operational Weather Modification

In my opinion we now have firmly in hand the knowledge to proceed to operational weather modification (*27*) in two areas: (i) the increase or decrease of snowpack in some mountains; and (ii) the increase of rainfall in some tropical regions.

In each area it is necessary to make a detailed analysis of the local conditions and then to decide whether or not the benefits will be greater than the costs. In other words, attention must be given to the measurement of numbers of nuclei naturally occurring, the probability of occurrence of suitable conditions, the development of computer models for local conditions, and so forth. We should approach each application with the expectation that we must have good measures of what we do as well as what we spend. We need quality control on artificial nuclei production and distribution, as well as quality control on the measurement of the local conditions of wind, temperature, humidity, snowfall, runoff, and pollution. Each aspect requires further development of equipment and technique—but the matter ought to be approached as a problem of development, not research. Research must be unfettered to be productive; development must be controlled or it will seldom be productive.

There are, of course, many unexplained phenomena, particularly in relation to the behavior and origin of many of the nuclei that can be found in our air over cities and countryside. We do not know how much material is removed in some of our storms, and we must therefore arrange to measure many variables that we do not now measure. Most of all, since we have not developed systems for trying to track the downwind effects of the seeding and for proving that their social impacts are nil or favorable, we should include extended observations in the pilot operation.

Some of our computer models are too cumbersome, and the input information is too empirical to be satisfactory. The computer models do not, for example, usually take into account electrical phenomena, and we know that there are some systems in which electrical phenomena must play a large role.

But these questions need not delay us. We do not know everything, but we know enough to proceed. The instrumentation I have proposed provides the safeguards we require. The questions that remain to be answered will, when answered, lead to new efficiencies, new opportunities, new options, or new understanding. But they are not critical to the decision to move forward.

We ought to identify other circumstances in nature for which it is promising to apply our knowledge and ought to proceed to organize our understanding about them. The five areas that seem to be ready are (i) hailstorms, (ii) cumulus clouds in temperate zones, (iii) lake and coastal zone storm moderation, (iv) hurricane modification, and (v) lightning modification.

It might be justified to choose as many as five national projects, one for each item listed, and to apply to the systems the same methodology that was used by Grant and by Simpson. Indeed, many of the elements of their computer programs can be applied directly.

We ought to study more carefully the relation between the computer models and the actual predictions to see where our knowledge is weakest, and we ought to fund research intended to improve this knowledge. In short, it is time for mission-oriented research on these projects.

In addition to the mission-oriented research described above, we must encourage those who have a different view of the phenomena to develop their views to a point where the ideas can be tested against observation. Cloud seeding methods have shown they can be made to work under certain defined circumstances. There may be other ways to start a strong updraft in an unstable atmosphere.

Langmuir used to say, "You can't plan to make a discovery." Clearly I cannot enumerate the discoveries we shall make in this field. But it would be foolish indeed to presume we have stopped learning. I do believe, however, that we can proceed toward deliberate weather modification in the areas I have named and that it should be the policy of this government to do so, whenever the expected social benefits outweigh the costs.

We Can, but Should We?

Nuclear physics has its "dictatorial principle," which states, roughly, that, if a thing can happen, it will. I think there is a dictatorial principle that covers man and his technology: if he can do it, he will.

Thus, we will control the weather to the extent that the state of the art allows us. But, in using this new art, we must keep firmly in mind certain things. Weather modification means deliberately changing the environment, perhaps on a large scale. We see that clearly, and we see it in advance. The developers of the internal combustion engine did not have the data, even if they had had the foresight, to realize what their invention would do to the environment.

We are far more sophisticated today; we know that a change in temperature of a couple of degrees in a small body of water can alter the balance of life there significantly. We are far more aware and should therefore be far more careful.

Weather modification, even in its current beginnings, has a potential multiplication effect to escalate small actions into results of major proportions. This we must remember even as we take our first small steps. Fortunately, we have a growing knowledge of the parameters of weather and their interactions. We have growing computer hardware and software capability to process the vast amount of data that is necessary to keep track of what we are doing and to predict where our path of action may lead.

Still, all this equipment is technological window dressing at best, and is instrumentation for havoc at worst, without the conscious realization of the possible consequences and without a conscious moral commitment to make the environment more livable than before we started. With such an attitude we can use our technology to anticipate and even to plan the results of our experimentation.

The problems we face here are new in nature and entirely unprecedented in scale. Effects of weather modification are, at least, regional, often national, and, in many cases, international. We must be able, on a systems basis, to assess consequences and to assign responsibility. The moral burden will be heavy on the individuals—scientists and engineers—who originate operations.

Perhaps even more important than these considerations are the human and social considerations. A scientist can bombard a nucleus with neutrons without asking the permission of the nucleus. He cannot engineer the environment without consulting the people who will be affected.

Some of the conflicts are clear already. Farmers and picnickers may often be at odds on the desirable weather for Saturday; cities with snow removal problems and nearby ski resorts will also have conflicting desires. Political problems abound and will multiply as the frequency and scope of weather modification develops.

Any procedure for planning weather modification activities must include a provision for giving public and private interests a voice in the process. According to our historical political philosophy, we make important decisions by some adversary procedure whose main criterion for validity is that it gives all parties and interests an equal opportunity to be heard. We must not close the decision-making process in weather modification. The results are too important to too many citizens and too many groups.

We will be playing a new game, with new rules, which involves such questions as human rights and national goals. As we move now to the pilot stage, we must recognize that we have as much to learn about the political and social problems as we do about the scientific ones (*28*). We must find regions for experimentation in which not only the meteorological climate is appropriate but in which the social and economic climate is inviting.

In addition to a need for instruments to measure meteorological impacts, we need to develop techniques for measuring the social impact. Nature's feedback is through our instruments; we must find social instruments through which affected parties can talk to us. I am convinced that we can find specific applications in which everyone benefits. But I am also convinced that we cannot do so without their participation. We have reached the point where the scientific fraternity and the public (in particular those portions of the public most affected) must talk together and decide what to do with this new ability we have created. Meteorology is too important to be left only to the meteorologists!

Appendix

We shall give here only a brief statement of material that can be found elsewhere [see (*1, 29*)]. We define the following symbols:

H = a particular hypothesis.
h = the denial to H.
D = a statement about data.
X = general background information.

We interpret the symbol $p(|)$ as in the following examples:

$p(H|DX)$ = the probability assigned to the truth of H given that D and X are taken to be true.
$p(H|X)$ = the probability assigned to the truth of H given only the truth of X.
$p(D|HX)$ = the probability assigned to the truth of D given that H and X are true.

I have previously discussed why it is necessary to "encode" knowledge of uncertain events in this language and have demonstrated that there are two basic equations that relate various possible "p" values. These equations, plus Boolean algebra, suffice to generate a general system of inference.

If HD is read "H and D are true," the first equation is

$$p(HD|X) = p(H|DX)p(D|X) \qquad (1)$$

which may also be written

$$p(HD|X) = p(D|HX)p(H|X) \qquad (2)$$

The second equation is

$$p(H|DX) + p(h|DX) = 1 \qquad (3)$$

which, on multiplication through by $p(D|X)$ and by use of Eq. 1, may be written

$$p(HD|X) + p(hD|X) = p(D|X) \qquad (4)$$

On equating the right-hand sides of Eqs. 1 and 2 and by use of Eq. 4 to eliminate $p(D|X)$, we find

$$p(H|DX) = \frac{p(D|HX)p(H|X)}{p(D|HX)p(H|X) + p(D|hX)p(h|X)} \qquad (5)$$

This equation, known as Bayes' equation, expresses the probability assigned to the truth of H when D is known to be true in terms of the probabilities that would be assigned to H, h, and D under different states of knowledge. It simplifies the writing to let:

$x = p(H|X)$ = the "prior" assignment of probability to the truth of H, given only X (and not knowing about D);

$y = p(D|HX)$ = the probability assigned to the truth of D given the truth of H and X (that is, if H and X are true, y is how probable it is that D will be true);

$z = p(D|hX)$ = the probability assigned to the truth of D given the truth of h and X (that is, if H is false, z is how probable it is that D will be true); so that Bayes' equation is

$$p(H|DX) = yx/[yx + z(1 - x)] \qquad (6)$$

Bayes' equation is merely a consistency constraint on how different probabilities may be assigned [see (*1*)]. x corresponds to an "encoding" of the knowledge before doing an experiment and learning D. The values of y and z refer to how the outcome of an experiment is regarded if first H and then the denial, h, are taken to be true.

If we wish to do experiments which lead to a state of knowledge in which $p(H|DX)$ goes very sharply to either 1 or 0 (that is, a very definite opinion on H), there are several ways to do so. (i) Find a way to make x either 1 or 0; that is, work only on an hypothesis already known to be true or false. This is truly "safe research." It is also unproductive! [Note that as $x \rightarrow 1$, $p(H|DX) \rightarrow 1$ and, as $x \rightarrow 0$, $p(H|DX) \rightarrow 0$.] (ii) If x is not exceptionally close to 1, find an experiment in which $z \ll xy$.

If $xy \neq 0$ (that is, HD not impossible), Eq. 6 may be divided by xy to give

$$p(H|DX) = 1/[1 + (1 - x)/x(z/y)]$$

If x is neither close to 0 or 1 (that is, the hypothesis is seriously in question), the value of $p(H|DX)$ depends mainly on z/y; that is, it depends on the likelihood of finding D true on H or h. We can make this clear by simple examples.

Suppose I were to claim as my hypothesis H that I could dissolve a stratus cloud by putting CO_2 pellets into it, and to support my claim I predictably produced, as data D, the General Electric monogram burned into a cloud over Schenectady at a specified place and time. Clearly, for such evidence z/y is extremely small, and, since we have said $x \neq 0$, $p(H|DX) \rightarrow 1$. In other words, my ability to dissolve stratus clouds can be proven by very few experiments or even by only one of this type. On the other hand, if I only burn a small hole in the cloud, even though z/y is a large number, $p(H|DX)$ is larger than x but is not raised up to unity. Here I will need a large number of randomized experiments to prove my hypothesis. This result coincides with the commonsense observation that we often see holes in clouds (z is not zero). It is the design of the experiment that determines the number of tests.

The words "predictably produce" are very important. There are patterns to be found in every random sequence, if we just look hard enough and creatively enough. Langmuir and Schaefer reported (D') a 7-day national cycle in rainfall when they were seeding in New Mexico on a 7-day period; this fact would have had a very pronounced effect on the subsequent state of science if they had *predicted* it in advance. The fact they did not predict it in advance does not make $p(H|D'X)$ smaller than $p(H|X)$, but it does mean that the assignment of a value to $p(H|D'X)$ depends more on X (other knowledge) than on D'.

If any one observation, say D'', has about the same probability given H true as it does given h true, it will take a large amount of data to produce a value of z/y which is very persuasive. Such cases correspond to observing a D'' which is of itself not very surprising. It takes a large collection of different D'' to be persuasive. In such cases, randomized experiments are indeed essential.

But, we may also make progress in physics and its many derivative branches of science by the performance of "critical experiments," which are experiments which enable us to verify in fine detail a complex theory. When the predictive process and the instrumentation have been properly designed, and if the prediction concerns a very complex process, there is no need for randomization. If these conditions cannot be met, randomized "blind" runs are essential. As Bayes' equation demonstrates, the cases in which randomized experiments are necessary and the cases in which randomized experiments are not necessary merge smoothly into one another. It is only necessary to make an analysis in each case to decide which conditions can be made to pertain.

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Apollo 13 Lunar Heat Flow Experiment

Direct measurement of the heat escaping from the lunar interior will be made during Apollo 13.

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On the Apollo 13 mission, the astronauts will set in place a heat flow experiment—part of the Apollo Lunar Surface Experiments Package (ALSEP)—to measure the steady-state heat flow through the lunar surface. This experiment will provide the first direct measurement of the rate at which the moon's interior is losing energy to outer space.

For planetary bodies as large as the earth and the moon, both energy retained from initial formation and energy generated by interior processes contribute to the net surface heat flow. The initial interior temperature distribution of these bodies is in part determined by the fraction of gravitational energy retained during accretion. Also, if the earth and moon were formed soon after the creation of the heavier elements, then the decay of short-lived isotopes could have contributed significantly to the initial temperature. The principal continuing process generating heat within planets is the decay of the long-lived radioisotopes of uranium, thorium, and potassium. Because of the smaller size of the moon, it is probable that it has lost a greater percentage of its initial heat than has the earth. Consequently, surface heat flow results from heat sources distributed throughout a greater percentage of the moon's volume than the earth's, and therefore could yield more information about the moon's internal constitution than terrestrial measures yield about the earth.

The limitations on equipment weight and astronaut extravehicular time demand that the heat flow measurement be made at shallow depths. The feasibility of making a valid measurement depends to a large extent on the rapid attenuation with depth of the extreme surface temperature variations. This rapid attenuation is due to the very low thermal diffusivity of the lunar regolith (*1*). Nevertheless, at practical measuring depths (3 meters) thermal gradients due to heat flow from the interior will be superimposed on a transient temperature field that includes significant contributions from other sources. Special temperature sensors and techniques for the measurement of thermal conductivity had to be developed to meet the stringent range, resolution, and stability required for an accurate measurement of the heat flux from the interior in the lunar surface layer.

The measurement of heat flow in the lunar soil consists of independent determination of the steady-state vertical temperature gradient

$$\frac{dT}{dz}$$

and the effective thermal conductivity K of the material across which the gradient is measured. The heat flux per unit area $\vec{Q}$ is related to these quantities by the conduction equation:

$$\vec{Q} = -K\frac{dT}{dz}$$

These measurements will be made with slender probes 1 meter long placed at the bottom of two 3-meter boreholes separated by about 10 meters (Fig. 1). An astronaut will make the boreholes by driving a fiberglass tube (2.5 centimeters in diameter) into the lunar surface with a drill (Fig. 2). These probes will be used to make two measurements of temperature gradient and four of thermal conductivity in each borehole. The purpose of making multiple measurements is to detect local subsurface

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