administration. After stress, when epinephrine is released from the adrenal medulla, the compound may be taken up by these two neuroendocrine organs and may thus affect function, for example, melatonin or adrenocorticotrophic hormone release (*9*). The ratio of H^3-epinephrine in the pineal to that in the whole brain at either 10 or 60 minutes after intravenous administration was 150:1; after intraventricular injection, the ratio was 2:1 (*10*). This finding raises the question of the ability of the pineal to exchange materials with the cerebral spinal fluid.

Although brain tissue can form epinephrine enzymatically (*2*), the endogenous concentrations of epinephrine in the brain of the rat are low. Bioassay of the hypothalamus suggests that epinephrine constitutes about 4 percent of the NE in the rat, whereas in the cat and the dog the corresponding values are 7 and 14 percent, respectively (*11*). However, turnover rates of the compound may have more relevance to the biologic role of epinephrine in the brain than absolute concentrations of the compound—which may merely be a measure of storage. Because of the limited knowledge about brain epinephrine (difficulties of assay have prevented investigators from studying regional and subcellular distribution), it is not possible to state whether exogenously administered material is handled similarly to endogenous material. Our data indicate that, when given intraventricularly, the half-life of epinephrine in the brain is at least as rapid as that of NE under similar conditions and that epinephrine is handled in a manner metabolically similar to that of intraventricularly administered NE (see *12*).

A great many studies have been conducted on the effects of mood-altering drugs or physiological and psychological states, or both, on brain NE and dopamine. However, there have been no such studies with epinephrine. Since the compound is made and metabolized by the brains of mammals, it is possible that some of the effects thought to be mediated through other catecholamines may be mediated through epinephrine (*13*).

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Magnetic Observations in Studies of Sea-Floor Spreading

The continuing stream of articles concerning sea-floor spreading suggests that many oceanographic geologists and geophysicists may be generally inexperienced in the use of terrestrial magnetic profiles. Larson and Spiess' report (*1*) illustrates my point. The authors seem to have difficulty explaining what they call "deep magnetics," and they review several hypotheses. In so doing, they introduce two terms which are in exact opposition to years of practical usage. Traditionally, "surface magnetics" refers to variations produced by mineral or rock concentrations near the solid surface of the earth; "deep magnetics" refers to anomalies and variations produced by deeper-buried, broader-dimensioned lithological units within the crust. "Sea surface profile" and "bottom profile" are terms consistent with the vast amount of airborne magnetic data obtained throughout the world over the past 20 years.

In airborne surveying practice, particularly as applied in mineral or petroleum exploration, the profile labeled here "deep magnetics" is a completely expected and normal result. The authors recognize the "proximity effect" (that is, flight altitude), but apparently they do not regard this as unusual or unique.

For example, surveys conducted at 150 m over basalts typically show the pattern observed. Raising the flight altitude to 450 m would show what they term "surface magnetics." Areas of glacial drift show the same features.

However, when such a phenomenon is observed over land, it is not explained in terms of "spreading" or "reversals"; rather, we regard it as a characteristic magnetic signature of recognized lithologic units, regardless of its cause, which is probably related more to conditions at the time of emplacement than to a sequential change of the earth's field.

Before others attempt to interpret this general type of observation, they should acquaint themselves with available aeromagnetic data, both on- and offshore, that will put their conclusions in a perspective dictated by nature's real variability in the "fine" as well as the "large" structure. Although the authors here deal mostly with fine structure, others who attempt to correlate "oceanographic ridges" with other phenomena should inspect the variability in the larger magnetic features found in the earth's field.

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Our familiarity with continental magnetic structures, in both large and fine scale, is, in fact, what led us to develop the methods we are using and to be well aware of the substantial differences between the oceanic and continental situation. In particular, the carefully mapped, highly lineated large-scale magnetic patterns in the northeastern Pacific (*1*), the recognition of strikingly similar sea-surface magnetic profiles in other far-distant locations (*2*), the lack of ancient rocks (older than 150 million years) in the ocean basins, and the petrographic uniformity exhibited by the East Pacific Rise (*3*) are all circumstances which those familiar with both sea and land data realize occur only in the oceanic environment.

The fine-scale magnetics, only part of the new data presented (*4*), are not surprising. They have been seen in nearly all of our near-bottom magnetometer tows (*5*) and show a strong tendency toward lineation of the same orientation as the large-scale features where these coexist. Explaining these details in a manner consistent with all the above data was the problem we faced (*4*).

Finally, the use of continental paleomagnetic data, gathered and analyzed by geologists concerned with the origin of rock units and the conditions at the time of their emplacement, provided the first clear evidence of magnetic-field reversals, including the time scale of these events based on potassium-argon dating methods (*6*).

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Whitetop Experiment

In the article "Areal spread of the effect of cloud seeding at the Whitetop experiment," Neyman, Scott, and Smith (*Science*, 28 March 1969) clearly show that there was less rain on days with seeding than on days without seeding. Neyman *et al.* do not make a good case for their implied conclusion that the deficiency of rainfall, over an area of radius 180 miles, was caused by the cloud seeding. Throughout the article, it is suggested that the deficiencies were in fact a "decrease" and were caused by seeding. One reads on page 1446, "Figure 2 was constructed to bring out more clearly the continuity of the effect of seeding. . . ." Tables 1 and 2 are entitled "Estimated effects of cloud seeding. . . ."

The two-tailed significance levels in the two tables are not so small as to make it self-evident that the rainfall differences were caused by seeding.

Several hypotheses might be offered to explain effects of seeding downwind of the seeding area, but no plausible hypothesis has been offered to explain effects upwind and to the side to distances of 180 miles. Before concluding the analyses ". . . indicate strongly not only that cloud seeding can affect rain, but also that its effect can spread over very large areas . . . ," the authors should have sought other explanations for the observed differences in rainfall. Specifically, it is essential to examine whether the rainfall differences can be ascribed to meteorological differences having nothing to do with the seeding. Summer rainfall sometimes occurs in the form of widely scattered showers and thunderstorms. On other occasions, organized zones of thunderstorms extending over hundreds of miles may sweep along and produce heavy and widespread rainfall. Is it possible that, in the sample of days without seeding, notwithstanding the randomization, there were more occasions of widespread, convective cloud systems which produced rainfall over almost the entire area shown in Neyman's Figure 1? Were the differences in percentage of rainfall a result of a few days without seeding with unusually heavy rainfall over most of the area? Until these questions can be answered, it is premature to suggest that rainfall differences over an area 180 miles in radius were caused by cloud seeding. In conclusion, we agree with Neyman *et al.* about the need for more research aimed at resolving the many uncertainties about the effects of cloud seeding.

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The principal message of our article is concerned with the question of whether the verifiable numerical results of the Whitetop experiment support the frequent claims that cloud-seeding technology is sufficiently developed to justify federal expenditures on large-scale operations intended to alleviate water shortages. Earlier studies of the Whitetop trial are all agreed that the average instantaneous precipitation within a small variable area labeled Missouri Plume, observed on days with seeding, was about one-half that on days without seeding and that this difference is highly significant. We supplemented these findings by studying the 24-hour precipitation in six concentric regions, up to a distance of 180 miles. We found that in all these regions the precipitation on seeded days was always less than that without seeding. "The estimate of the average seeding effect in the entire region is a 21-percent loss of rain. *In the absence of a real effect, chance alone could produce such an estimated loss, or a larger one, about once in 15 independent trials.*" (Italics added.)

This was our principal finding. Battan is certainly entitled to his opinion that "significance levels . . . are not so small as to make it self-evident that the rainfall differences were caused by seeding." In fact, we agree about the lack of self-evidence. But, if there is anything in the contention that a gain in the rainfall of 5 to 10 percent is worth talking about, then a 20 percent loss, experienced over a vast area of some 100,000 square miles, must be a disaster. In these conditions, the odds of 14 to 1 that this loss was caused by seeding do not appear negligible to us. We feel that it is imperative that the general public and the government be informed of the situation.

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