Letters

Electrostatic Field and Freezing

In a recent report, Salt [*Science* **133**, 458 (1961)] presented experimental data which, in his opinion, show that supercooled water can freeze at a higher temperature in the presence of an electric field than it does in the absence of the field. Because this, if true, could have far-reaching effects in many areas, I feel his findings should be examined critically.

In the first place, Salt makes no mention of the ice-forming nuclei [Mason, *The Physics of Clouds* (Oxford Univ. Press, 1957)] that are present in the atmosphere. While most of these nuclei are usually relatively sparsely distributed and not active at temperatures above −10°C, high concentrations, of laboratory origin and active at near-zero temperatures, may have existed in Salt's laboratory at the time his experiments were performed. These nuclei, under the influence of the electric field, might have entered the supercooled drops and initiated freezing. Second, and more important, a pointed wire probe at a potential of 15,000 volts and held only about ½ inch from the electrically grounded supercooled water undoubtedly produces a corona current. This corona current is not easily detected and begins at a potential far lower than that required for the spark discharge. The possibility that ice-forming nuclei are created by the corona current, perhaps from material from the surface of the wire probe, should not be overlooked.

I suggest that the experiments be repeated in air from which the ice-forming nuclei have been filtered. Further, the electric field should be created by smooth, parallel, plane-to-plane electrodes. Only in this way will the possibility of a corona current be minimized. Until these suggestions are incorporated into the experiment, I do not feel that one can, with any certainty, conclude that an electric field can play a primary role in the initiation of freezing in supercooled water.

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Blanchard's criticism of my paper is, essentially, that I did not consider the possible action of airborne ice-forming nuclei. He proposes that these may be created, attracted, or concentrated by the electric field or corona and may thus supercontaminate the sample.

This is a reasonable possibility in the case of my exposed water droplets, but how could it be applicable to the insect larvae and rubber-encased water samples, which possess coatings resistant to nucleation from without? Nothing that is known about the nucleation of water would lead one to doubt that my insect larvae and encased water samples were nucleated internally.

Blanchard suggests that the experiments be repeated in clean air with parallel-plate electrodes to minimize the corona current. As stated in my report, I used parallel plates in some tests and found them quite as effective as the probe and plate electrodes.

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Advancement of Scientists

T. C. Kahn [*Science* **133**, 656 (1961)] does not, I believe, give sufficient credit to the AAAS for its newly effective policy of publicly relating science to human welfare and culture. I submit that, if the public is kept sufficiently aware of this relation, the advancement of scientists will be adequate. The "Ph.D. scientist," forced into a pecking order with "real doctors," may admire the American Medical Association from afar. Some of us, however, would not like to emulate the AMA, which threatens to replace the physician's concern for human health with "medical economics." I rejoice that the AAAS has not found it necessary to caution scientists not to carry professional insignia on their Cadillacs, as has been reported of a county medical society in California.

One large group of scientists—the teachers—is inadequately recognized and compensated, but I doubt that we would be wise to single out teachers of science for preferential treatment among teachers in general.

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Enzyme Nomenclature

F. Bernheim's delightful letter [*Science* **133**, 654 (3 Mar. 1961)] is somewhat inaccurate. He says, "In 1957, Wallach and Grisolia [*J. Biol. Chem.* **226**, 277 (1957)] further purified the enzyme, which they said we called hydantoin peptidase—a name we had not thought of." Actually Bernheim, in his article "Enzymes in detoxication" [in *The Enzymes*, J. B. Sumner and K. Myrbäck, Eds. (Academic Press, New York, 1952), vol. 2, pt. 2, p. 862], wrote a subsection entitled, "Hydantoin peptidase," referring to his previously named (1946–1949) hydantoinase.