CORRESPONDENCE

Remarks on "Sea salt in a tropical storm"

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In a recent paper in the JOURNAL, Woodcock[1] reported some measurements of concentrations of airborne sea salt in a Florida hurricane. I would like to call attention to what was for me a misleading mode of presentation of the magnitudes of latent heat released (and of associated temperature increases) in table 2 of Woodcock's paper. These magnitudes, as computed and tabulated, represent total amounts of heat and temperature increase that would be observed if the associated amounts of liquid water had been built up on initially dry salt nuclei entirely by condensation. Was it Mr. Woodcock's intention that the reader assume such a process to be operative?

As a second comment on the paper, it may be pointed out that the amount of heat released by condensation on the observed 0.837×10^{-6} g of salt per gram of air, as it ascends from a level of 82 per cent relative humidity near the surface to the lifting condensation level (about 1300 ft above the surface for Woodcock's data), is percentually so small compared to the magnitude of the internal-energy decrease of the associated air as to raise serious doubt concerning the significance of salt content of air as an additional stability criterion. The magnitude of the very adiabatic cooling effect required to realize the tabulated latent-heat release for 98 per cent relative humidity is, for Woodcock's data, about eighty times larger than the dropwise condensational heating effect, (0.055–0.006)C. Only if Gautier's[1] and not Woodcock's salt-content measurements approach the true upper limit of attainable salt concentrations could one expect appreciable effect of salt content on stability, in view of the above relative magnitudes. Hence, I would like to ask whether Mr. Woodcock feels that the experimental techniques employed by Gautier, when he measured a salt concentration some thirty times greater than the maximum of Mr. Woodcock's random samples in a hurricane, were such as to justify acceptance of Gautier's values as a more reliable indication of the upper limit of attainable salt content over stormy seas than Mr. Woodcock's own observations in this one Florida hurricane. And if so, what meteorological or oceanographic conditions prevailing during Gautier's observations might have been responsible for this thirty-fold excess of spray formation in the storm off France over that off Florida? In particular, one wonders if Gautier's measurements might have been made at a lower level or nearer the surf zone than were Mr. Woodcock's.

As a third point, I would like to ask whether it may not be possible that a locally more intense effect, opposite to that considered by Mr. Woodcock, occurs within a very shallow layer above an agitated sea surface? If one makes some rough calculations, which need not be reproduced here, he finds that newly formed droplets of sea water will evaporate so rapidly in coming to vapor equilibrium with their new environment that virtually all of the evaporational cooling effect may be confined to a thin surface layer over the sea. The actual depth of this evaporationally cooled layer should depend in part on the size distribution of the nascent droplets, but probably still more on the relative-humidity profile in the surface layer. Without here attempting to examine very closely the magnitudes involved, I would merely like to point out that, in contrast to the roughly 1300-ft depth of the layer through which is distributed the condensational heating effect envisaged by Mr. Woodcock, the layer within which occurs the equal and opposite evaporational cooling effect must surely be measured in feet or, at most, tens of feet. That the lower rather than the upper limit of this suggested range of cooling depths is probably the more nearly correct is suggested by the fact that a drop of sea water whose initial diameter is only one micron would require only about a millisecond to evaporate down to a concentration implying vapor equilibrium with air of 82 per cent relative humidity, if suddenly placed in air of that humidity. However, the steady-state relative humidity above an agitated sea surface would be expected to exceed this 82 per cent value up to a height of perhaps a few feet above the surface, so the depth of the cooled layer might be expected to be small and of that order most of the time. (A relative humidity of 82 per cent, incidentally, has been referred to for no other reason that that it was involved in the paper here under discussion.) In view of these arguments, might it not be more profitable to look for possible micrometeorological effects of this concentrated cooling process than to look for stability effects of its much more diffuse inverse?

It would be interesting to know if any such cool layer just above the sea surface has ever been observed instrumentally. It seems somewhat doubtful that it would have been detected by chance, because the very requirements for appreciable spray formation, strong winds and rough seas would render such observations difficult.

[1] A. H. Woodcock, "Sea salt in a tropical storm," *J. Meteor.*, 7, 397–401, 1950.

A minor error, appearing in the second equation on page 401 of Mr. Woodcock's paper may be pointed out. The specific heat should be in the denominator, not in the numerator.

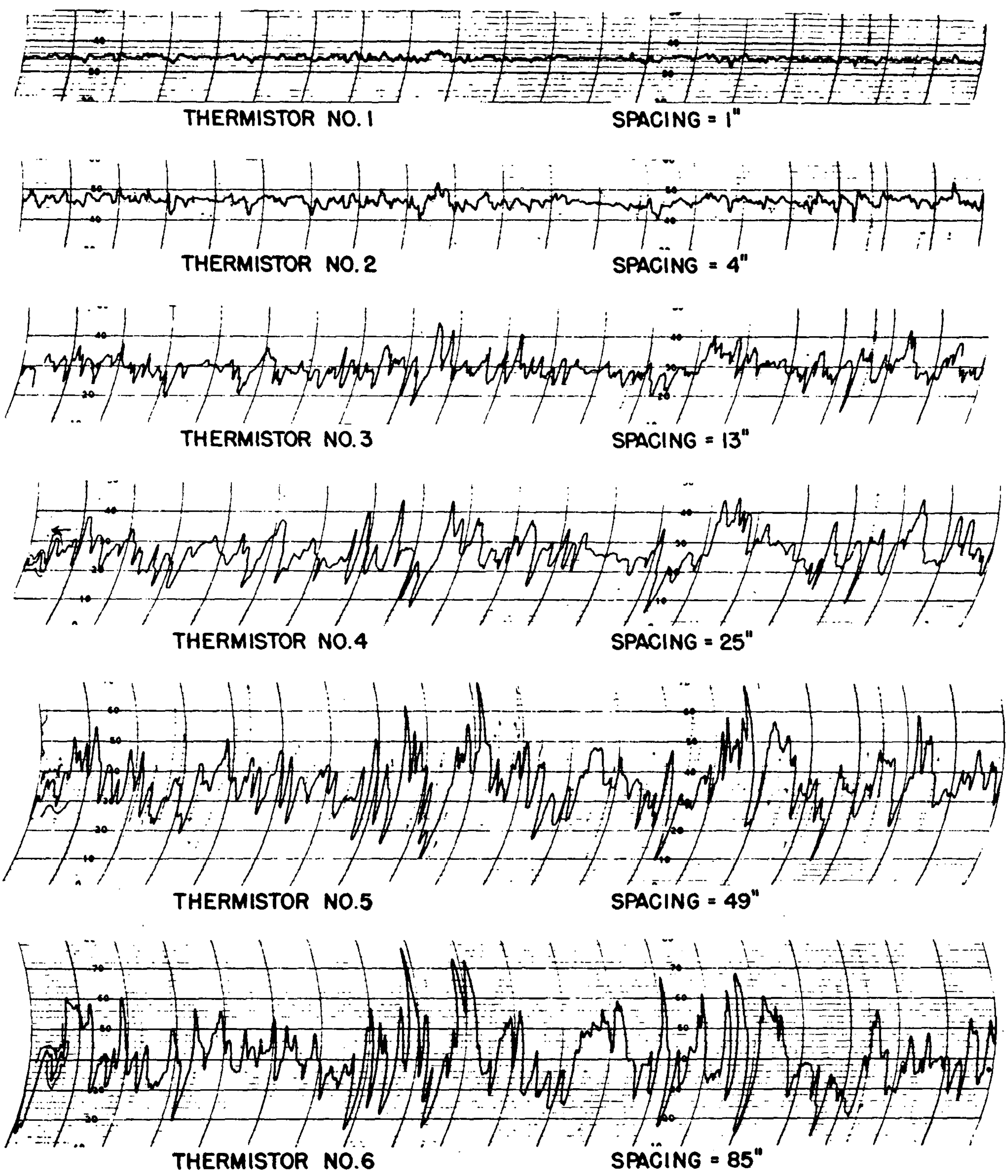

FIG. 1. Simultaneous temperature-difference recordings made at height of 3 ft above ground along line oriented perpendicular to mean wind. Recordings illustrate temperature-difference variations over 150-sec period. Temperature scale approximately 0.6C per ten recorder divisions.

It is proposed that a modification of the above system, to include both observation and analysis, be used to supplement the correlation analysis of atmospheric fluctuations. This modification involves the direct recording of the difference between several sensing elements and a reference element. With temperature once again as the example, with a series of sensitive thermocouples or thermistors located along a line for various orientations relative to the mean wind, it can be easily seen that the root-mean-square

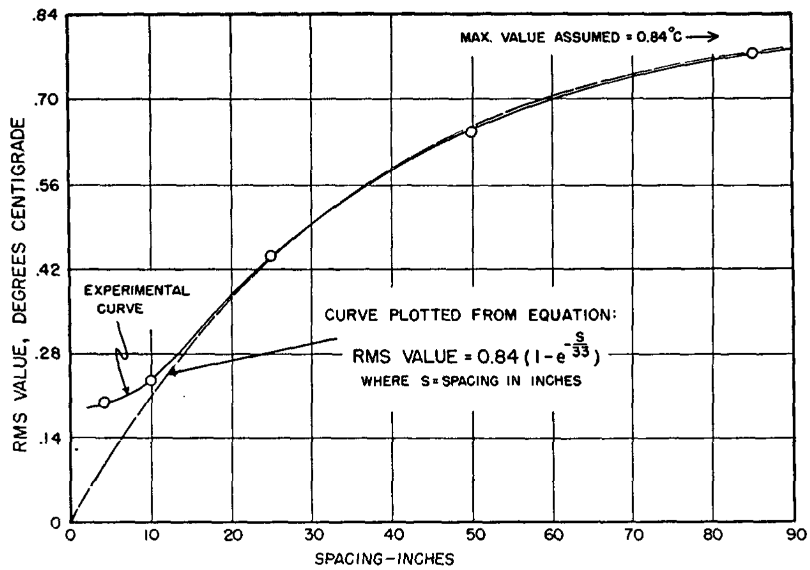

FIG. 2. RMS values of temperature-difference fluctuations as function of element separation for data shown in fig. 1.

value of the temperature differences between the various sensing elements and a common reference element will increase with element separation. Such a set of original data is illustrated in fig. 1. It has, in addition, been determined qualitatively that, for thermistors oriented perpendicular to the wind, a plot of the RMS value of these fluctuations will follow an exponential law as illustrated in fig. 2.

Several factors should now be kept in mind. Such a system automatically discriminates against fluctuations which have an effective wavelength significantly greater than the spacing between elements. There is, thus, no change in mean value of the recording as a result of occasional long-period temperature variations. Analyses can accordingly be made from a shorter sample of data, with significantly less danger of their being a function of sample length. It is possible also to make a quick visual inspection of the data, with regard to obtaining a measure of the distance over which the RMS differences have approached essentially constant values. It should be noted here that while the "time constant" of the exponential illustrated in fig. 2 affords a good indication of an atmospheric scale of turbulence, it is not mathematically equivalent to that originally defined by Taylor,[2] using the auto-correlation coefficient, although it is of the same order of magnitude.

The technical problems involved in the observation and analysis are relatively few. A Wheatstone-bridge arrangement, in which thermistors are used in two legs of the bridge, forms the basis of the measuring equipment. A relatively simple computer for determining the RMS value of the curves, as shown in fig. 1, may be used. One such computer, making use of the integrating characteristics of watt-hour meters, has been recently developed here.[3]

[2] G. I. Taylor, "Statistical Theory of Turbulence," *Proc. roy. Soc. London*, A, **151**, 421; **156**, 307; **157**, 540, 1935.

While this system cannot replace the correlation method of analysis insofar as the determination of frequency spectrum of fluctuations is concerned, the RMS-difference system does provide a relatively simple method for the analysis of atmospheric scale and intensity of turbulence. Some preliminary studies using this method have been reported by Best[4] and Birnbaum[5], although the observations for the various spacings were not simultaneous.

[3] C. M. Crain and M. F. Hainey, "A simple device for finding the RMS value of a curve," *Elec. Engin. Res. Lab. Rep.*, No. 51, Univ. Texas, 1951.

[4] A. C. Best, "Horizontal temperature differences over short distances," *Quart. J. r. meteor. Soc.*, **57**, 169–176, 1931.

[5] G. Birnbaum, "Fluctuations in the refractive index of the atmosphere at microwave frequencies," *Phys. Rev.*, **82**, 110–111, 1951.

An observation of the freezing of droplets in ion tracks

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In an experiment using a continuously sensitive cloud chamber, we have observed ion tracks to change from liquid droplets to scintillating crystals as they fell in the chamber. The observation was so clear as to leave little doubt as to whether droplets actually froze.

The chamber used[1] was one of the downward diffusion type, using distilled water as the source of warm vapor at the top of the chamber and propyl alcohol as

[1] C. E. Nielsen, T. S. Needels, and O. H. Weddle, "Diffusion cloud chambers," *Rev. sci. Instrum.* (in press), 1951.

the source of cold vapor at the bottom of the chamber, the atmosphere of the chamber being air. The temperature of the cold reservoir was achieved by placing the propyl alcohol in a pie tin on a cake of dry ice. The warm reservoir was a petri dish full of water, mounted a few inches above the alcohol pool. The chamber was completed by inverting a crystallizing dish over the watch glass. After the usual period of fogginess, the condensation nuclei were cleaned out, and ion tracks were formed 10 to 15 mm above the alcohol surface. These were observed to scintillate before reaching the alcohol pool, the flashes being detected earliest at the lowest portion of sloping tracks and progressing rapidly along the tracks as they fell into the cold bath.

The temperature of the region where scintillations were observed was estimated to be in the range −40 to −45C. Under the assumption of a linear vapor-pressure gradient for each vapor (water and alcohol) from saturation at the liquid source of the vapor to zero at the surface of the other liquid, it was determined that the composition of the vapor in the ion sensitive region to a first approximation was 99.95 per cent water to 0.05 per cent alcohol. One cannot exclude some influence of the alcohol in the observed phenomena, but this influence is difficult to judge. The principal known effects of the alcohol vapor are that its presence facilitates the observations (a) by lowering the ion limit appreciably and (b) by preventing the formation of white frost on the black velvet background material.

There are a number of reasons for regarding this as a significant observation:

1. No ion tracks were observed to form initially as ice-crystal tracks, *i.e.*, crystallization of water vapor on ions was not observed in a situation in which it should have occurred if it were probable with temperatures near −40C;
2. The ice crystals which were observed were formed by freezing of the liquid droplets which became visible as ion tracks a few mm above the level at which scintillations first appeared. Dobson[2] makes special mention of the fact that "no droplet was ever seen to freeze" in his laboratory, but he infers from the careful observations which were made that "a very small quantity of water is first deposited on the nuclei which then freezes, after which further ice is deposited to form the full-grown ice particle." This inference is substantiated by our observation;
3. The observation of ion tracks assures that no contaminant particles which might act as nuclei for condensation or freezing were present in the chamber.

[2] G. M. B. Dobson, "Ice in the atmosphere," *Quart. J. r. meteor. Soc.*, **75**, 117–130, 1949.