# Reply

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Comments by Williams (1991), on the paper by Colman (1990a), hereafter BC, focus on possible sampling biases and/or inadequacies in the data-collection process. Williams (1991), hereafter EW, expresses concern that the horizontal, vertical, and temporal sampling techniques in BC, which utilize standard radiosonde data on three surfaces, could fail to locate existing positive CAPE. This is a reasonable concern, and one that the author has considered at great length.

Williams proposes two tests that he feels would more rigorously assess Colman's conclusion that thunderstorms can occur in the absence of conditional instability. The first test addresses the effect of passively selecting the 850-mb level to represent the source air above the frontal inversion. There is no doubt that this procedure would lead to smaller values of CAPE than if maximum $\theta_e$'s were used, as discussed in BC and suggested in EW. However, there is little evidence that this difference would be large enough to shift the distribution of LI-85s (BC, Fig. 13b) sufficiently to show conditional instability.

Cases are presented in BC and Colman (1990b) where the vertical variation of $\theta_e$ is taken into account. For these cases, calculations of the maximum available potential energy (MAPE) pass the test proposed in EW. In other words, even when the highest $\theta_e$ is used, it is not possible to demonstrate the existence of conditional instability for all occurrences of thunder.

In collecting the data, a decision was made to interpolate horizontally to the storm's location since elevated thunderstorms occur in areas of large horizontal gradients. This decision removed the ability to assess the vertical variations of $\theta_e$ for all cases, since single soundings were not used. As a result, 850 mb was selected as the representative level for the source air. Nonetheless, to fully account for horizontal and vertical variations, a full, three-dimensional analysis would need to be performed, as was done in Colman (1990b) for AVE-SESAME I.

The Showalter index (Showalter 1953) reveals conditional instability for summer-season convection and it is based upon the 850-mb level without consideration of the maximum $\theta_e$. In addition, practice in assessing convective instability for surface-based convection has shown that a layer average is the better indicator since some finite layer is needed to provide the necessary mass flux to sustain an updraft.

Finally, as discussed in EW, the source air for elevated thunderstorms travels great distances over a frontal surface and mixing processes would act to diminish maximum values. This is particularly true in the low static stability, strongly sheared environment associated with elevated thunderstorms. The overall effect of this mixing would be a deeper, relatively homogeneous layer, which is at least a better target for the methodology used.

The second test proposed by EW addresses upstream and downstream soundings and potential horizontal biases in sampling. As suggested by EW, there is decreasing available potential energy with increased distance from the surface front. This gradient is clearly visible in Fig. 17 in BC. Thus, if nearby soundings had been used in BC there would have been a possibility of bias due to sampling. However, as stated here previously, and in BC, nearby soundings were not used in the study. Parameters were instead horizontally interpolated to the location of the thunderstorm to effectively eliminate potential horizontal biases due to sampling; this is an aspect of the study apparently missed by EW.

The extraordinarily large positive lifted indices (in excess of 10°C) that are evident in Fig. 13b of BC are questioned in EW. These possibly represent failures of the methodology employed—not unexpected for such a large sample. Nonetheless, this cannot be stated with certainty without conducting detailed individual case studies, and that has not been done.

Another aspect of the study apparently missed by EW concerns sampling in time. Unacceptable error would be introduced if interpolations were attempted in time as well as in space, and only those thunderstorms occurring at either 0000 or 1200 UTC were studied. This is discussed in BC. The timing of radiosonde releases is nearly ideal for assessing conditional instability for storms being reported on the 0000 or 1200 UTC observations. This results from a radiosonde

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release time that is nearly 1 h prior to the reported time, and a surface observation time that is typically 10 min prior to the reported time. In other words, the radiosonde release is typically 40–50 min prior to the reported occurrence of thunder. Therefore, Williams' concern about errors from time sampling has no apparent basis.

In EW, an argument is made that a lack of theory-supporting field data does not justify looking for an alternate explanation for an observed phenomenon. Williams uses lightning as an example, a phenomenon that has a well-developed theoretical basis, yet, to date, has little supporting field data. He states that the reason for this lack of data is that the electric field is simply undersampled in both space and time, and suggests that this may also be the case for elevated thunderstorms.

In the case of lightning, it is not surprising that the desired measurements are difficult to obtain since the electric field is highly transient and grows rapidly from the molecular scale. This is simply not the case for conditional instability. Our "rather coarsely spaced radiosonde network" has proved to be adequate to assess conditional instability time and time again. This is certainly true for the boundary layer, which one would expect to show even higher horizontal variability than the 850-mb level used in BC. Furthermore, it was demonstrated in Colman (1990b) that elevated thunderstorms are relatively long-lived, mesoalpha-scale phenomena, increasing the likelihood that the conventional radiosonde network would sample the proper environment.

The reliability of using radiosondes to measure conditional instability is demonstrated by the data collected by Stone (1985); both time (within 6 h) and space (within 230 km) requirements were less strict than those in BC, yet the existence of significant instability was demonstrated. Thus, there is a class of thunderstorms where considerable field data support conditional instability as the likely cause, and a class of thunderstorms (those identified by Colman) where the same sampling technique fails to identify the existence of that instability. It must be asked, "Is this a failure in the sampling or a newly identified mechanism?"

A final point raised in EW is that the latent heat of freezing (left out of the calculations in BC) can add nearly 1°C to a lifted parcel's temperature. While this is true, the parcel temperatures used in BC are calculated using an entropy-conserving reversible scheme similar to that described by Lorenz (1979). This scheme produces parcel temperatures that are warmer than those obtained from a pseudoadiabatic scheme by an amount similar in magnitude to the correction for the latent heat of freezing. Furthermore, the values used are for undiluted parcels and lateral mixing and entrainment should act to further diminish any small instability.

The discussion in EW on potential mechanisms for lightning generation is very interesting and makes two points very clearly. First, "mixed-phase microphysics and lightning can be in principle independent of conditional instability"—a necessity if the conclusions in BC are correct. Second, there remains a great deal of uncertainty as to what vertical velocities are required to produce lightning. Williams suggests that a few meters per second are required, yet cites references that show that vertical velocities of several tens of centimeters per second are capable of producing lightning in the trailing stratiform region of squall lines. Such velocities are not at all unreasonable, perhaps conservative, for CSI (Emanuel 1983) or intense frontal circulations (Emanuel 1985).

It is not possible to define a typical slope for an $M$ surface, and they can range from very shallow to vertical. The slope of 0.01 used in EW would yield very large positive LIs and likely does not represent the typical environment of elevated thunderstorms identified by BC. As the slope becomes more vertical, the transition to upright convection is not well understood. Emanuel (1983) demonstrated that the hydrostatic approximation is valid for conditions very close to neutral stability, and stated that "the transition from strongly sloped hydrostatic motions resulting from moist symmetric instability to vertical moist convection will in general be accompanied by only a very brief period of nonhydrostatic symmetric instability." The scale and appearance of possible circulations in weakly hydrostatically stable, near-neutral symmetric-stability environments are not known. Much work still needs to be done in this area, both theoretically and observationally. High-resolution kinematic and thermodynamic observations, collected from the near-environment of ongoing elevated thunderstorms, in concert with Doppler radar data, would be very beneficial in addressing these outstanding questions.

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