**Reply to**

# Comments by Charles A. Doswell III on "Mesoscale Convective Patterns of the Southern High Plains"

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I welcome this opportunity to respond to Doswell's (1990) remarks concerning the classification scheme presented in Blanchard (1990; hereafter referred to as B90). Doswell and I have had several occasions to discuss this topic and he also reviewed a related, unpublished manuscript. We have learned that we are in disagreement about several features in B90 and have agreed to disagree. It is useful, then, to elaborate on those points on which we disagree, and to what degree, so that members of the meteorological community can make their own decisions about the information presented in B90.

Doswell and I agree that a taxonomy can be a useful tool, but disagree on the usefulness of this particular taxonomy. Doswell implies that B90's classification scheme is flawed and may be misleading. There can be no perfect classification scheme because nature does not work that way; a continuum exists in almost any process or system, but one of our traits as humans is to try to "pigeon-hole" what we observe. It was and remains my intent to create a simple classification scheme to demonstrate that there are three basic categories of mesoscale organization of convection, only one of which has been extensively sampled, studied, and modeled (i.e., linear systems) and to draw attention to the other two patterns of mesoscale convection. A goal for future field programs should be to try to collect high spatial and temporal resolution data on many of these systems fitting into the "occluded" and "chaotic" categories of B90 so that the taxonomy of B90 (and others described in B90) can be refined, modified, or rejected if necessary.

Doswell correctly points out that in an unpublished manuscript the term "random" was used to describe a pattern of mesoscale convection, but B90 is not that manuscript and the point is irrelevant. Doswell also does not like the term "chaotic pattern," suggesting that B90 has not considered or validated the underlying dynamics implied by this terminology. Although we try to be as precise as possible when we define or discuss an object or thing, there can be variations in interpretation among individuals. Doswell has chosen to interpret my use of chaotic in terms of the currently popular field of chaos theory. This interpretation is regrettable because I simply meant to use the word as it is defined in any standard dictionary, i.e., "a state of disorder; a disorderly mass." Used this way, there is no conflict between the object and its name.

I am disappointed that Doswell does not acknowledge the distinction between linear systems and occluding systems. He notes that there appears to be an occlusion-like feature in the linear system (see Fig. 3c in B90) and concludes that the difference is one of scale. Clearly, the curved feature in Fig. 3c is simply a minor subelement and is not the dominant feature of the system, whereas the occlusion shown in Fig. 4c is the system. More important, he has ignored the fact that the occlusions occur on the intersections of warm fronts with cold fronts or outflow boundaries. Whereas soundings taken ahead of the linear systems show little variation between the northern and southern portions, there are significant differences evident in the soundings taken in the northern and southern portions of the occluding systems. Additionally, an east-west convective band typically develops before the north-south band and is an integral part of the occlusion; such an east-west convective band does not appear with the linear systems. Doswell notes that Rutledge et al. (1988) show that the system depicted in Fig. 3 evolved into "an apparently occluded system"; this feature has been noted in Table 2 of B90.

I strongly disagree with Doswell that the chaotic systems should be an "unclassifiable" group. This would imply a large variability within this group such that these systems do not constitute a clearly defined class of their own. As pointed out in B90, the systems in this group form in environments with similar characteristics; i.e., they occur over the cool air north of the warm front in which there is instability aloft and a cool, stable boundary layer. B90 speculated that the stability of the boundary layer prevents the downdrafts from penetrating to the surface where they can organize the convective elements. Since the environmental features and mesoscale organization of the convection are highly repeatable, they constitute a distinct class of their own.

Of the three types of mesoscale convective patterns discussed in B90, only the linear category is subdivided into larger and smaller scales, i.e., meso-$\alpha$- and meso $\beta$-scale linear systems. Doswell asks why this was not done for the other two types. The analysis of the linear systems showed a large spectrum of sizes and it was natural to divide them into two scales. The results shown in Fig. 7a,b of B90 indicated some subtle differences. The other two mesoscale patterns did not exhibit the same spectrum of sizes and subsequently were not subdivided. It is unfortunate that this was not stated more clearly in the text and I appreciate this opportunity to do so now.

Lastly, I find that I am in agreement with Doswell's final paragraph. There are already many classification schemes (as noted by B90) based on different goals and data, yet they tend to agree in many aspects. It is the position of this writer that each of these taxonomies has addressed one or more important issues in the understanding of mesoscale convection. None is definitive, nor is the taxonomy of B90 definitive. They are all contributions to the incremental increase in understanding of these highly complex systems and should not be considered final descriptions, but starting points.

**References**

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