Comments on "Mesoscale Convective Patterns of the Southern High Plains"

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In his recent article, Blanchard (1990 -- hereafter referred to as B90) has undertaken to develop a radar-based echo classification scheme for mesoscale convective systems. This is a laudable effort and I believe that a taxonomy of convective systems can become a valuable contribution to our understanding of such storm systems and their recognition by field forecasters. In fact, I concur with Gould (1989) that development of a taxonomy is not just an exercise for the feeble-minded; classification schema form windows on reality that influence how people think about the phenomena for a long time. Therefore, it is important that a classification scheme be as clear a window as possible. Gould (1989) has provided examples (from life science) where a cloudy window on reality has proven an obstacle to new understanding.

To undertake the development of a taxonomy almost invariably means that the author's view will be disputed. B90 is no exception and I am bringing this dispute into the open because of the importance of a proper taxonomy for mesoscale convective systems. I am not proposing an alternative taxonomy because I have not done the hard work required to produce one, nor am I an "expert" in taxonomy. Nevertheless, I must register my concerns about the scheme presented in B90.

A proper taxonomy must depend on readily identifiable characteristics and I do not believe B90 has presented a clear distinction between "linear" and "occluding" systems. Whereas the archetypes presented in B90's Fig. 2 seem to be quite distinct, the real examples shown in his Figs. 3 and 4 are much more ambiguous. As I examine his Figs. 3c and 4c, for example, I see small scale perturbations in Fig. 3c that look quite similar to the relatively large scale perturbation of Fig. 4c. Moreover, Rutledge et al. (1988) show that the particular "linear" system shown in B90's Fig. 3c went on to develop into an apparently "occluded" system (see Fig. 3h in Rutledge et al. 1988). Am I to conclude that the distinction between "linear" and "occluding" systems is really one of scale? Is the taxonomy only applicable to "snapshots" of systems, with their life cycles of no importance to their membership in one class or another?

Invoking the Glossary (Huschke, 1959) definition of occlusion has, in my view, not contributed significantly to the resolution of this conflict. There appear to be smaller scale versions of the occlusion process (as defined in B90) going on near the center of his Fig. 3c; again, there seems to be nothing more than a scale difference between these real situations. Of course, given B90's data, such a small scale "occlusion" is virtually impossible to detect. One can even dispute how conclusively B90's surface data and analyses support the occlusion hypothesis, even on the larger scale he prefers to emphasize. If the terms used in a taxonomy are not related unambiguously to the actual distinctions being drawn, then users of the taxonomy may well run into real problems in trying to use the scheme in practice.

The third category, "chaotic" patterns, is very close to being an oxymoron. I happen to know that an earlier version of this paper bore the "random" label for this category, which truly is an internal contradiction. Can a pattern be random? It seems to me that randomness implies the absence of pattern, which is what B90 seems to be implying, in spite of having changed "random" to "chaotic". The absence of a pattern may well be in

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the eye of the beholder; if there is nothing more characteristic of the elements in this category than that they do not reveal any pattern to the author, this seems rather a meager concept upon which to base a category. What if someone else sees a pattern where the author does not?

Changing "random" to "chaotic" also creates a terminology problem, since recently the term "chaotic" has come to have certain implications about the underlying dynamics that I don't believe B90 has considered, much less validated. It can be argued that this is "just a semantics issue" but scientists must agree on their terms if those terms are to have any real value in science. Thus, the "chaotic" terminology remains inappropriate for a useful taxonomy. I think that "unclassifiable"_ would be more descriptive than "chaotic" for this category (see also, Houze et al. 1990). In this context, "unclassifiable" should be taken to mean that the elements cannot be put into other, named classes. For the scheme of B90, this would mean that the members of this class would not fit in either the "linear" or the "occluding" categories. This leaves open the possibility that someone might find a way to classify them in the future, either by changing the two other classifications, or by adding one or more classes into which some previously unclassifiable examples might fit.

Interestingly, B90 has felt obligated to include scale distinctions within his "linear" category. I already have indicated that this argument could be extended to the "occlusion" scale as well, but he has not chosen to do so. This suggests to me that he has not succeeded yet in developing a proper measure of distinction between categories. The apparent differences in environment between "linear" and "occluded" systems shown in B90 are not evidence that the categorization is proper. Had one developed a different scheme, the environmental distinctions might have been sharper.

Gould (1989) has suggested it is not a trivial matter to make category distinctions in a way that allows the taxonomy to be useful, so it is not surprising that this effort has fallen short. The sample size is quite limited in comparison to, say, Houze et al. (1990) and B90 correctly indicates that the scheme should not be "extrapolated" beyond his dataset. In my experience, however, such caveats frequently are ignored or missed and the scheme probably will be used authoritatively in spite of the author's best intentions.

While I have taken B90 to task about the proposed taxonomy of mesoscale convective systems, I am in favor of the effort. I think that once a well thought-out taxonomy is developed for convective systems, it will end up being of considerable value, so making a proposal is a positive contribution, regardless of its flaws. Of course, there may be room for several competing taxonomies, based on different characteristics. The observing system upon which a scheme is based is a major factor in seeing structure in the data; it may be a long time before we have a taxonomy for mesoscale convective systems that is truly physical rather than keyed to one or another set of observatins. This suggests that it would be useful to have a variety of schema to suit a variety of purposes, observations, and situations. Most natural phenomena tend to resist being divided into neat categories with hard boundaries, so the classification of convective mesoscale systems may never be a closed book.

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