Comments on "Single-Doppler Radar Observations of a Mini-Supercell Tornadic Thunderstorm"

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1. Introduction

In their recent note, Kennedy et al. (1993, hereafter referred to as KWS) have presented observations related to a tornadic storm. Overall, this presentation is interesting and worthwhile, but there are several issues associated with the note about which I want to express concern.

First, I take issue with their description of the synoptic setting. And it appears that their Fig. 1a, purporting to show surface features at 1900 central daylight time (CDT), is mislabeled, actually showing data at 1900 UTC, which would be 5 h earlier than claimed. Moreover, I am quite concerned about their interpretation of the surface data and will present an alternative diagnosis that includes a reanalysis of their surface data as well as other information not shown in KWS.

Second, what I believe to be errors in identifying the 1900 UTC surface chart lead to an inconsistency between their Fig. 1a and their Fig. 4, the latter displaying a hodograph with a considerably different surface wind at Peoria than depicted on their surface chart. This inconsistency can be reconciled by using the correct surface data (i.e., at 0000 UTC) to compare with the hodograph winds. However, this calls into question the representativeness of the Peoria sounding at 0000 UTC and, therefore, leads to questions about their characterization of the prestorm environment as "nonthreatening."

Finally, based on the evidence shown by KWS, the storm that produced the tornadoes is clearly a classical supercell and, although it appears that the authors do not dispute this conclusion, their description of this storm as "atypical" of tornadic storms is rather hyperbolic. I will attempt to offer some justification for considering this storm to be *typical* of tornadic storms and try to provide a somewhat different interpretation of the event as a whole.

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2. Surface analysis

When considering the analysis shown in Fig. 1a of KWS, I was somewhat disturbed to see a warm front analyzed directly through the relatively cool air in central Illinois. By including the surface data with the analysis, the authors provided the reader with a chance to evaluate their analysis, a practice I applaud (see also Doswell 1990). It has been a long-standing concern of mine that some analysts in both the operational and the research communities seem to ignore the data (notably, temperature and dewpoint data) when delineating "features" of a surface diagnosis. I am afraid this note is yet another example.

I have included a temperature analysis of the 1900 UTC surface data shown in Fig. 1a of KWS (Fig. 1a herein, as well). A slightly different (albeit basically similar) picture of the fields can be obtained by analyzing *potential* temperature (Fig. 1b). The latter quantity should be resistant to terrain effects because it is an adiabatically conserved variable, but terrain features in central Illinois are rather bland. In either analysis, the warm front featured in KWS is not a very convincing interpretation of the surface data. Of course, a heavy-handed smoothing to highlight the "synoptic-scale setting" might be acceptable *if convection were not the primary issue in the situation.* It is not surprising that KWS point out that "no significant surface moisture convergence was diagnosed in the vicinity of the warm front" since I have my doubts about the validity of the interpretation leading to the warm front's location in Fig. 1a of KWS. The surface convergence is dominated by that along the leading edge of the cool pool.

Since KWS have not provided temperature data on their Fig. 1b, it is difficult to use vertical consistency to check on their warm front interpretation. As shown in Fig. 2 here, the 0000 UTC (1900 CDT) analysis at 850 mb reveals warm thermal advection south and southeast of the cyclone along the Illinois–Iowa border, as well as cold advection west and southwest of the cyclone. It is possible that the 850-mb warm thermal advection induced KWS to locate their surface warm front through the cool pool in central Illinois. If so, this is unfortunate for two reasons. First, given the

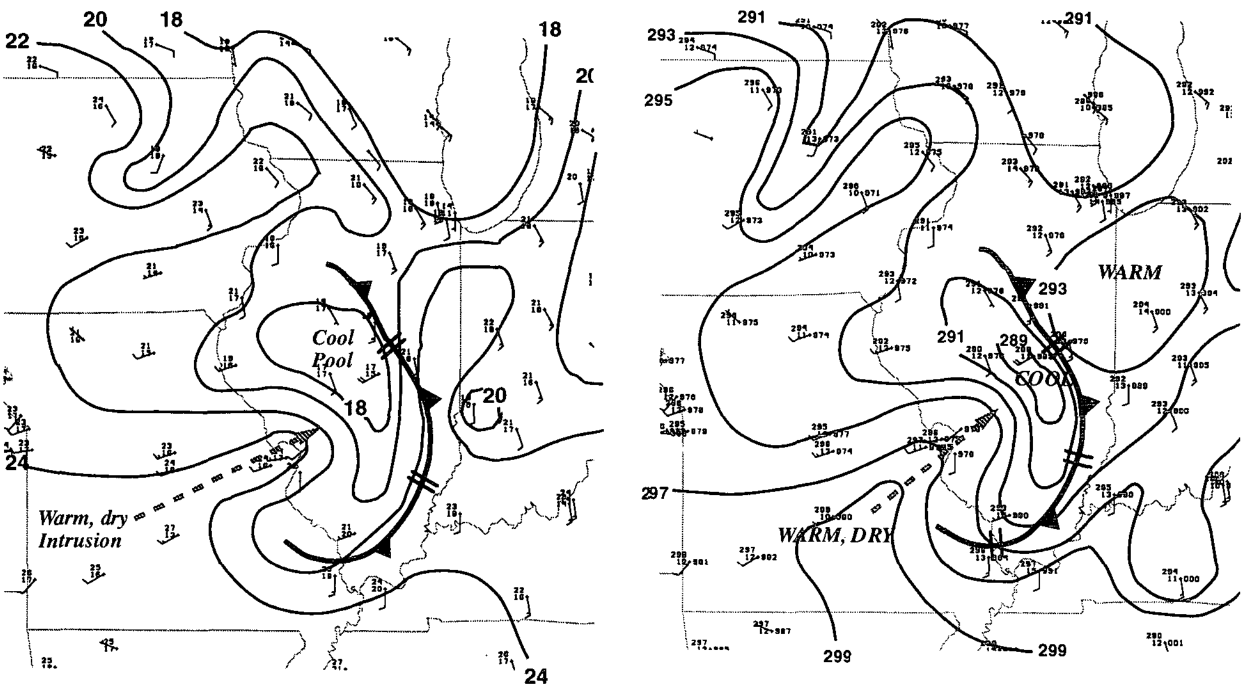

FIG. 1. Surface subjective analysis at 1900 UTC (1400 CDT) 19 May 1989: (a) selected isotherms (solid lines at 2°C intervals), and (b) potential temperature (solid lines at 2-K intervals). The warm, dry intrusion is indicated by the dashed, shaded arrow, and the outflow boundary is indicated by the thick line, following the convention proposed by Young and Fritsch (1989). In (a), the station model shows temperatures (°C, upper left), dewpoints (°C, lower left), and conventional wind barbs; in (b) the temperatures are replaced by potential temperature (K, upper left) and the dewpoints by mixing ratio (g kg^{-1}).

convention that a front is on the warm side of a thermal boundary, any warm front should be located equatorward of the region of warm thermal advection. Second, I believe that when analyzing the surface data, one should pay attention to the *surface data* and not be forced by some vertical continuity argument to locate a surface front when that is inconsistent with the surface observations. The warm advection at 850 mb is being masked at the surface by what appears to be a cloud- and precipitation-induced cool pool. I believe the clouds and precipitation are influenced, in turn, by the presence of the synoptic-scale warm advection (as in Maddox and Doswell 1982), but the surface data reveal a more complex picture than that presented in KWS. This mesoscale complexity is quite pertinent to proper assessment of the threat for subsequent convection.

If the KWS analysis of the surface data is questionable, what is a plausible alternate interpretation of the surface data? To deal with this issue, the origins and evolution of the cool pool shown in their figure must be determined. Using the hourly surface data, at 1500 UTC, the cool pool was north of St. Louis, Missouri (not shown), and by 0000 UTC (1900 CDT, Fig. 3), the cool air had moved into western Indiana, with a strong thermal contrast across southern Illinois, well south of the tornadic storm. This cool air apparently was created and maintained by precipitation and cloud cover (cf. Fig. 2 in KWS) associated with the main cloud band ahead of the cyclone shown in Fig. 2 here. The Bement tornadic storm, then, was on the *trailing* side of the cool pool, where a tongue of warm, moist air was approaching from the south-southwest. Figure 2 in KWS and Fig. 3 here show that the storm in question was developing at the leading edge of this warm intrusion. In this case, with the storm developing behind the main band of clouds and precipitation, the synoptic pattern is reminiscent of that described by Carr and Millard (1985) as "dry slot convection." Although this situation is rather more complex than those shown in the literature for "synoptically evident" (Doswell et al. 1993) tornado outbreaks, the case nevertheless is consistent with the notion that the basic ingredients for supercells are being concatenated by the synoptic and mesoscale evolution. The "warm frontal" interpretation of KWS ignores what appears to me to be a very significant mesoscale component in this case, namely, the cool pool.

3. Representativeness of the Peoria sounding

There is a marked tendency to interpret the numbers generated from data too literally when using common forecasting parameters. KWS are not the first to make

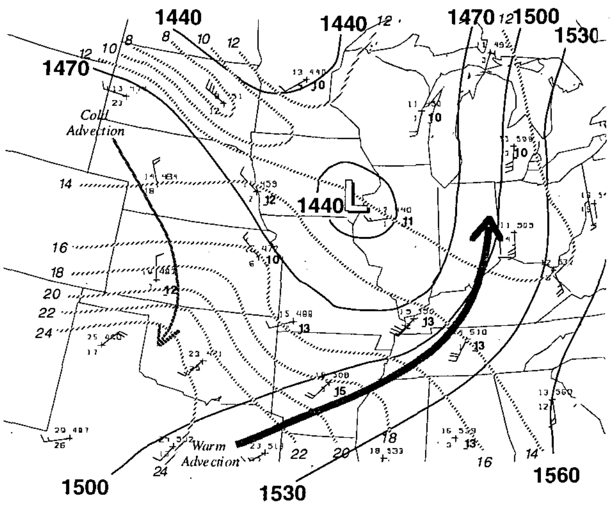

FIG. 2. The 850-mb subjective analysis at 0000 UTC 20 May 1989, showing isotherms (thin hatched lines, 2°C interval) and isohypses (thin solid lines, 30-gpm interval). The station model shows temperatures (°C, upper left), dewpoint depressions (°C, lower left), geopotential height (m, leading digit suppressed), and dewpoints (°C, lower right, underscored and boldface) for selected stations. The curved, hatched arrow shows a band of cold thermal advection; the thick solid arrow shows a band of warm thermal advection.

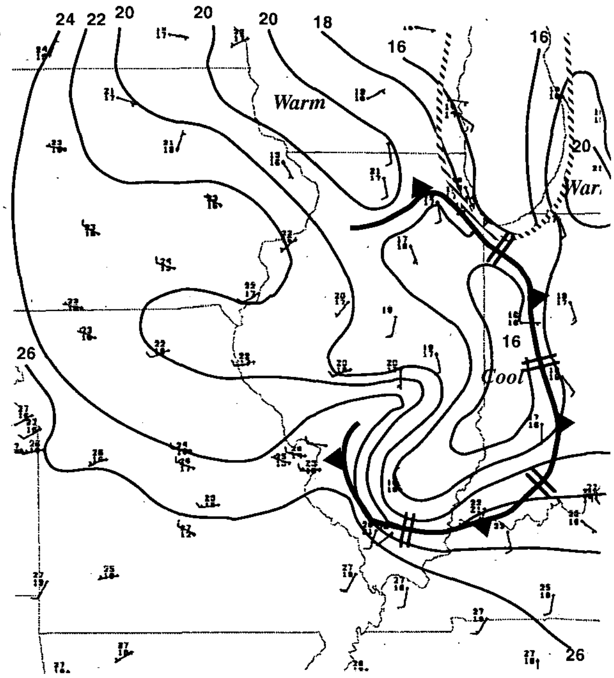

FIG. 3. As in Fig. 1a except at 0000 UTC 20 May 1989. The lake-breeze boundary around Lake Michigan is indicated by the hatched line.

this mistake; in my experience, operational forecasters do this far too often. In general, it is unrealistic to expect a sounding to sample the near-storm environment properly. As already noted in KWS, questions about the representativeness of the Peoria sounding at 0000 UTC can be raised. I have pointed out the apparent inconsistency in KWS between the surface data shown in their Fig. 1a and the hodograph shown in their Fig. 4. Given the correct surface data for 1900 CDT (0000 UTC, Fig. 3 here), the inconsistency can be resolved, but the question of representativeness remains. KWS acknowledge that "some differences probably existed between PIA sounding and the true near-storm environment," but, given the evolution shown here in Figs. 1–3, it appears quite probable that the Peoria, Illinois (PIA), sounding was taken in an environment that was not representative. In fact, as KWS have observed, "the southerly flow component was likely stronger" than shown in their Fig. 4. Unfortunately, they have chosen not to show *how* they modified the wind profile "plausibly" to "reflect the storm environment." I have analyzed the soundings and attempted to create a hodograph (Fig. 4) that is arguably more representative of the situation in the near-storm environment than shown in KWS. This interpolated sounding makes use of the constant pressure maps as well as the sounding and surface data; the surface-to-3-km (AGL) storm-relative environmental helicity (or SREH, Davies-Jones et al. 1990) resulting is 144 $m^2\ s^{-2}$.

This may be a rather conservative estimate of the hodograph. The Paducah, Kentucky (PAH), sounding's hodograph (Fig. 5) gives a considerably different picture of the helicity, with a resulting SREH value of 292 $m^2\ s^{-2}$. Of course, it is *not* obvious that helicity of this magnitude actually was available in the Illinois tornadic storm's environment. The influence of the mesoscale cool pool on the environment experienced by the storm in question is not known. Perhaps the Doppler data collected on the storm could be used to create a local environmental wind profile via the velocity–azi-

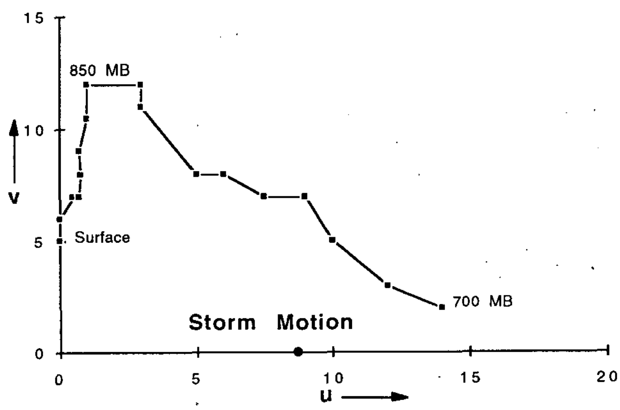

FIG. 4. Subjectively modified hodograph from PIA at 0000 UTC 20 May 1989, through the layer from the surface to 3 km AGL, with 850- and 700-mb levels noted. Storm motion is indicated by the solid circle.

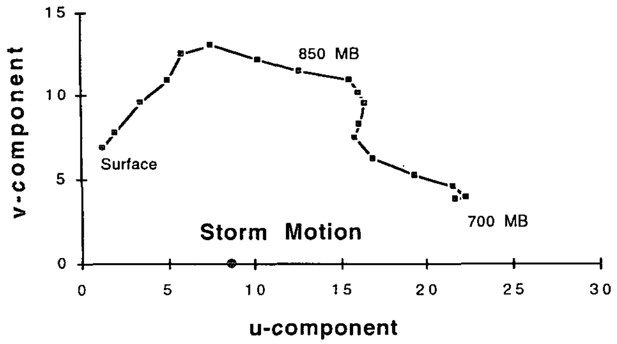

FIG. 5. As in Fig. 5 except for the observed hodograph at PAH.

muth display (VAD) technique (Browning and Wexler 1968), but when mesoscale structure in the wind field is present, the assumptions used in the VAD technique may be violated. The fact that high helicity was present in a nearby sounding is a clue to possibilities that should not be ignored.

By the same argument, the stated value in KWS for the CAPE (convective available potential energy) of approximately 350 J kg^{-1} (or, equivalently, 350 m^2 s^{-2}), may not be representative. It is quite possible that the CAPE value for the actual storm environment was larger than that given by KWS, but (as with SREH) there is no obvious way to determine the actual CAPE in the storm environment. Again looking at PAH for 0000 UTC (Fig. 6), it is possible to obtain about 1200 J kg^{-1} using a parcel very near the surface. Now Fig. 3 shows that the near-surface air at PAH clearly is *not* present at the surface near the storm in question. However, Figs. 2 and 3 suggest that the PIA sounding is unlikely to represent the thermodynamic properties of the air involved in convection behind the cool pool.

This discussion should *not* be construed to imply that CAPE and SREH are infallible parameters; I am not suggesting that if the data do not show these parameters to be high, then the soundings must be unrepresentative. No argument in favor of these (or any other) parameters is being made here. Johns and Doswell (1992) have published a CAPE–helicity diagram for strong (F2–F3) and violent (F4–F5) tornadoes, but the event in question here apparently was not that strong, so the current literature is not obviously helpful in characterizing situations like the Bement storm. Whereas Davies-Jones et al. (1990) have suggested that tornado intensity may be related to environmental helicity, that relationship recently has been questioned (Brooks et al. 1994), with helicity perhaps being a better predictor of supercells than tornadoes or tornadic intensity, per se. The point is that I believe that KWS have underestimated the supercell potential that can be deduced from the observations and there may be nothing particularly unusual going on to produce a tornadic supercell in this case. All that is needed is some relatively minor modifications to the PIA sounding and hodograph, moving them closer to their counterparts at PAH, and the possibility of a supercell (with or without a tornado) becomes nonneglible.

4. Is the storm an atypical supercell?

With what I see from the storm environment and the storm's visual and radar appearance as shown in KWS, I am hard pressed to see why anyone would find this event to be atypical of supercells. All of the radar data and storm photographs show this to be a typical supercell, with the possible exception of the absence of a clear BWER (bounded weak-echo region); many but by no means all supercells have a BWER. Although KWS have not shown enough of the data to validate it, I suspect there was a *nonbounded* WER present. The storm top is relatively low, but low-topped supercells have been documented a number of times (e.g., Davies 1993; Marwitz 1972). The storm's low top represents a challenge to radar observations for an operational forecaster (now and in the foreseeable future) due to the radar horizon problem, but I believe that our scientific understanding of supercells indicates that a storm's *structure* is the key to recognizing its supercellular character, not its top height (or its peak reflectivity). Such superannuated severe storm criteria as echo intensity and storm-top height need to be discarded as outdated and unreliable, especially as we move toward a nationwide network of Doppler radars.

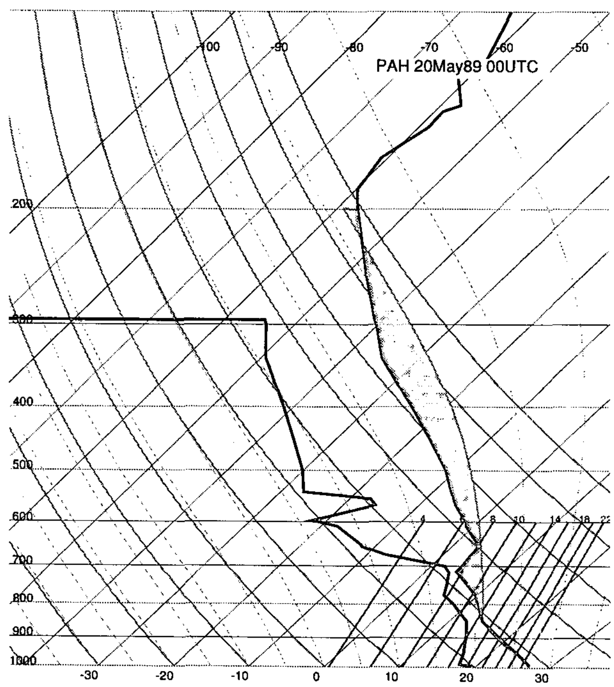

FIG. 6. Skew T–log$p$ diagram showing the observed sounding at 0000 UTC 20 May 1989. The shaded region shows the CAPE for a near-surface parcel.

5. Discussion

The main concern I have is in the KWS interpretation of the storm's environment as nonthreatening; that is, what they have proposed separates this supercell from others is its "unusual setting," which is purported to give little evidence of a supercell threat. I have indicated that the "broad-brush" surface analysis in KWS virtually precludes having an accurate perception of the storm's environment. Although I agree that this situation is hardly a "synoptically evident" case, a careful diagnosis of the data suggests that there is a least marginal supercell potential implied in the available data. Part of a forecaster's responsibility is to recognize the threatening weather that might develop in a given situation, even if such an event has only low probability of actually happening.

Therefore, characterizing the supercell tornadic storm as having arisen in a nonthreatening environment conveys the impression that this event probably could not have been anticipated correctly in operations. It is this impression that concerns me; if forecasters interpret KWS to be discouraging them to consider the possibilities (especially on the mesoscale) even in subtle situations, this becomes a self-fulfilling prophecy: no forecaster would try to anticipate an event like the Bement storm because it would be conceded that no forecaster *could* anticipate it. This is not an easy case to forecast, and I have no wish to "second guess" forecasters who might choose to downplay the threat in subtle situations. But I do hope that forecasters will at least consider the possibilities inherent in situations that are not synoptically evident supercell cases. To describe this case as nonthreatening can be interpreted as giving implicit support to ignoring what I see as threatening potential. Knowing what actually happened is, of course, a secure position from which to make this statement and I must reemphasize that this would have been by no means an easy forecast. As I see the case, it is clear that the supercell probability is low, given the available data, but that probability is not zero. Storms developing on the backside of the cool pool would need to be monitored carefully.

A conclusion in KWS that can hardly be faulted is that spotter reports and Doppler radar base-data display capabilities will be important even after implementation of severe weather detection algorithms using WSR-88D data. However, an important aspect of event *detection* is *anticipation* of the event. Forecasters might well be caught by surprise in such a case as this one, even with the WSR-88D, simply because they might not be looking. Such a low-topped supercell as this one would escape even WSR-88D detection unless it was close enough to the radar. While we cannot claim to understand everything about how severe thunderstorms and tornadoes arise, I believe that clues existed in the prestorm environment that should have indicated at least the possibility of supercells on this occasion. Being aware of that potential, even if the forecasts that went out to the public never mentioned it, would increase the chances that forecaster would recognize the event as it unfolded.

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