Esterheld, J. M. and D. J. Giuliano, 2008: Discriminating between tornadic and non-tornadic supercells: A new hodograph technique. *Electronic J. Severe Storms Meteor.*, **3** (2), 1–50.

Electronic Journal of SEVERE STORMS METEOROLOGY

Discriminating between Tornadic and Non-Tornadic Supercells: A New Hodograph Technique

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(Submitted 5 October 2007; in final form 8 July 2008)

ABSTRACT

Thompson and Edwards (2000) first noted a prominent hodograph kink separating primarily speed shear from primarily directional shear in the environments of some supercells producing significant tornadoes. Responding to this observation, we compared similar thermodynamic and shear environments between the Moore, Oklahoma tornado of 3 May 1999 and non-tornadic supercell thunderstorms occurring in north Texas on 23 April 2003. The results suggest that certain characteristics of the kink could discriminate between tornadic and non-tornadic supercells. This combination of features consisted of a strong (> 10 m s^{-1}) nearly straight-line hodograph below 500 m above ground level (AGL) and storm-relative inflow orthogonal to the base of this hodograph segment at 10 m, yielding almost purely streamwise storm-relative inflow.

We evaluated this hypothesis by analyzing 67 severe convective events, 65 of which were supercells, in Oklahoma from 1997-2004, and dividing the events into non-tornadic, weak-tornadic (F0-F1), and significant-tornadic (F2-F5) storm classes. The results show improved discrimination between storm classes for 10-500 m storm-relative helicity and bulk shear magnitude when compared to 10-1000 m calculations of the same. Also, histograms of the critical angle (defined by the storm-relative inflow vector at 10 m and 10-500 m shear vector) revealed that the tornadic storms, and in particular the significant tornadic storms, tended to be characterized by angles near 90°, whereas the non-tornadic storms were not. Although the results are based on a relatively small sample, they suggest that a careful consideration of the evolution of the low-level hodograph in both time and space in relation to the storm motion can potentially be a valuable aid in forecasting supercell tornadoes.

1. Introduction

Although significant (F2-F5) tornadoes are low-probability, localized events, they often have a devastating impact on human infrastructure and welfare. While much progress has been made toward identifying environments conducive to their formation, prediction of specific tornadoes remains very difficult.

Two features long associated with the occurrence of tornadoes include a veering wind profile with height and the presence of deep-layer speed shear (Fawbush and Miller 1954). In particular, further studies recognized that it is a veering wind *shear* profile with height that provides an environment favorable for developing the prerequisite to many tornadoes: the right-moving supercell thunderstorm (Browning and Donaldson 1963; Browning 1964; Marwitz 1972). Unfortunately, the mere presence of supercells is a poor proxy for the occurrence of tornadoes (Brooks et al. 1994). For example, recent work showed that from only one of every four (Trapp et al. 2005a) to as few as 3% (Jones et al. 2004) of radar-detected mesocyclones were actually associated with tornadoes.

Using numerical modeling techniques, researchers investigating the effect of vertical shear in the lowest few kilometers AGL on thunderstorm morphology found that clockwise turning of the hodograph in the low and middle

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levels favors the development of cyclonic, right-moving supercells (Klemp and Wilhelmson 1978). A study by Rotunno and Klemp (1982) then proposed that this hodograph curvature produces shear-induced pressure perturbations, causing rightward propagation by selectively enhancing (inhibiting) upward vertical motion on the right (left) side of the updraft with respect to the storm's motion. Subsequently, Davies-Jones (1984) suggested a mechanism whereby the streamwise vorticity present in the inflow layer (considered to be contained within the lowest 2-3 km AGL) of the near-storm environment could cause an updraft to rotate.

Based on these findings, Davies-Jones et al. (1990) investigated 0-3 km storm-relative helicity (SRH) as a possible predictor for tornado occurrence. Their study indicated that stronger tornadoes tend to be associated with higher values of SRH; however, considerable overlap between non-tornadic and tornadic storms was noted. Attempting to remedy the situation, several studies (Rasmussen and Blanchard 1998; Rasmussen 2003; Markowski et al. 2003; Thompson et al. 2003) examined 0-1 km SRH and found slightly improved discrimination over 0-3 km SRH, at least when comparing significant tornado events to non-tornado events. Taking this a step further, Thompson et al. (2007) examined "effective" SRH in lieu of fixed-layer SRH, and found slightly improved discrimination over fixed-layer 0-1 km SRH, with both "effective" and 0-1 km SRH showing improvement in distinguishing between SigTor (F2-F5), WeakTor (F0-F1), and NonTor (non-tornadic) storm classes when compared to 0-3 km SRH.

In order to better distinguish between storm types, Thompson et al. (2003) combined two shear parameters (0-1 km SRH, 0-6 km bulk shear) with three thermodynamic parameters (mean layer CAPE, mean layer CIN, and mean layer LCL, where a "mean layer" parcel is an average of the lowest 100 mb AGL) to produce a composite parameter called the Significant Tornado Parameter (STP), which was updated later to use "effective" layers (Thompson et al. 2004). Both versions of the STP were found to be successful in providing enhanced discrimination between storm classes, with the updated STP providing a nominal improvement in false alarm ratio.

In an overview of the 3 May 1999 Oklahoma tornado outbreak, Thompson and Edwards (2000) noted that hodographs associated with four significant tornado events (including 3 May) were characterized by a low-level hodograph kink between 1 and 1.5 km.[1] Interestingly, hodographs indicate this kink occurred at the interface between a lower layer dominated by speed shear and a higher one dominated by directional shear. Miller (2006) has since documented several additional hodographs exhibiting this kinked structure for other tornado events across the United States. Further, Miller (2006) also noted that the depth of the kink appears to be considerably shallower than 1 to 1.5 km for an anecdotal sampling of significant tornado cases, so that the shear below the kink is similar in depth to the strong near-surface shear investigated numerically by Wicker (1996). The current study examines the predominantly speed shear segment of the kink in detail, and in particular explores its relationship to storm motion for different storm classes. For more detailed information on the background and use of the hodograph in severe weather forecasting, see Doswell (1991).

2. Development of hypothesis

a. The 3 May 1999 case

Data from the 3 May 1999 tornado outbreak reveal several interesting features. For instance, the KOUN (Norman, OK) 0000 UTC 4 May 1999 hodograph (Figure 1) displays a prominent kink separating primarily speed shear from primarily directional shear in the low-levels with the strongest wind speed in the low levels observed near 250 m (Thompson and Edwards 2000; Miller 2006). Further, the shear below the kink is so substantial (~15 m s^{-1}) and shallow (10-250 m) that the data may be interpreted to suggest that this segment of the hodograph could be a straight line, though it is inconclusive since only two data points are present. If this is roughly a straight line, however, the positioning

[1] Inspection of the soundings corresponding to these hodographs reveals that the kinks were actually uniformly located from 0.5 to 1 km, likely because the hodographs in Thompson and Edwards (2000) do not appear to include the fixed wind levels recorded every 1000 ft. above mean sea level (MSL) in the lower levels of a sounding.

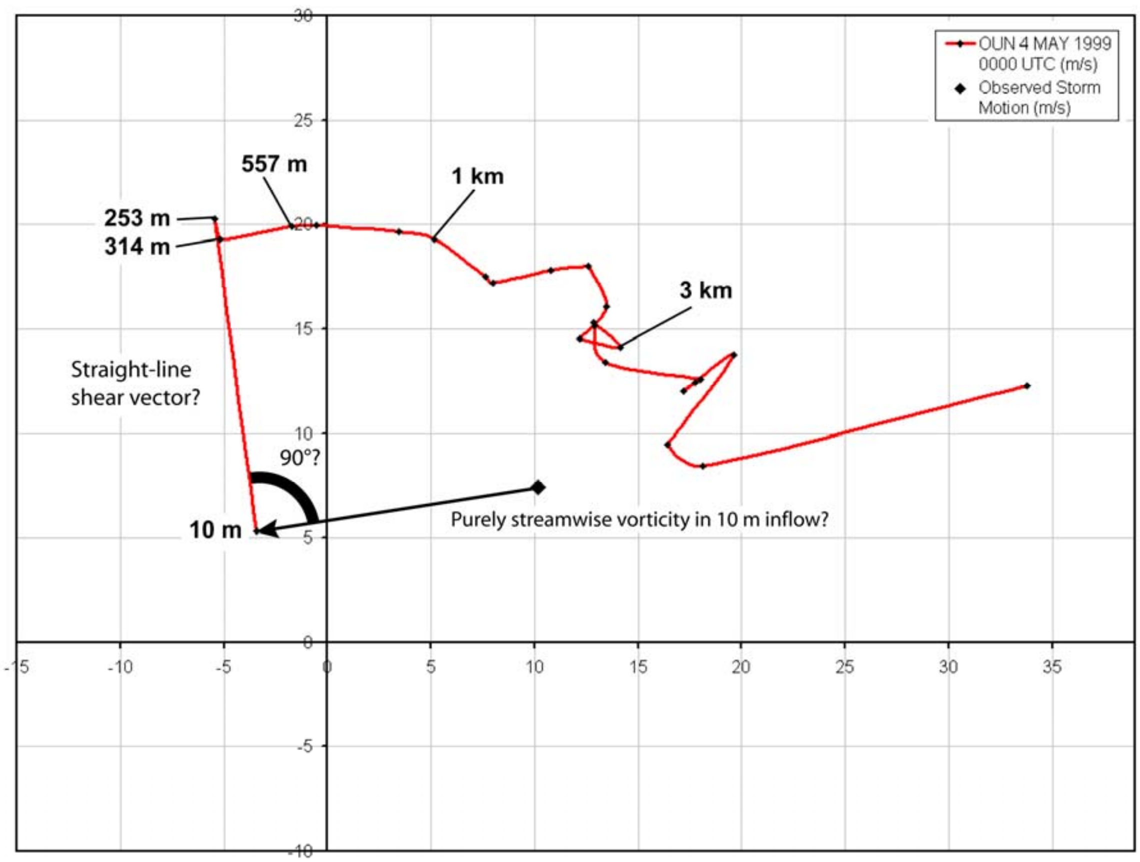


Figure 1: KOUN 0000 UTC 4 May 1999 hodograph. Axis units are in m s^{-1}.

of the storm motion vector[2] would imply that nearly all vorticity present at 10 m is streamwise, as the base of the low-level hodograph would then be approximately orthogonal to the storm-relative inflow vector at 10 m (hereafter, 10-m inflow vector). A similar hodograph and storm motion configuration is also found on the National Profiler Network (NPN) wind profiler observation from Purcell, OK at 0000 UTC 4 May 1999 (Thompson and Edwards 2000, Fig. 11b), but like the KOUN sounding the profiler is limited by the presence of only two data points and cannot be directly compared to the sounding in any event due to differences in how the data are sampled in both space and time. Further, it should also be noted that both errors in wind observations (Strauch et al. 1987; Weber and Wuertz 1990; Stensrud et al. 1990) and the calculation of storm motion, as well as uncertainties introduced by radiosonde (hereafter RAOB) data reporting requirements (Schwartz and Doswell 1991), could significantly impact the perceived orthogonality of this relationship. Therefore it is necessary to examine additional cases to determine whether sufficient evidence can be found to support this line of inquiry.

An inspection of Oklahoma Mesonet (Brock et al. 1995) wind observations at 10 m indicated that most stations in central Oklahoma were characterized by winds approximately 2-4 m s^{-1} weaker than the wind at 10 m observed on the KOUN sounding. Therefore the wind at 10 m was modified using a vector average of three 5 min observations at 15 min intervals from the Norman, Spencer and Washington Oklahoma Mesonet sites centered on 2330 UTC 3 May 1999, the approximate touchdown time of the Moore, OK, F5 tornado.

[2] The storm motion was calculated using Twin Lakes, OK (KTLX) Level II 0.5 deg base reflectivity by tracking the storm's reflectivity maximum for the one-hour period centered on 2330 UTC, the approximate time of touchdown of the long-track tornado that later produced F5 damage in Moore, OK; see Speheger et al. 2002 for more details on this storm.

b. Comparing 3 May 1999 to 23 April 2003

Around 0000 UTC 24 April 2003, two isolated supercells occurred less than 200 km west-southwest of the KFWD (Fort Worth, TX) RAOB site. These storms were non-tornadic, but occurred in an environment reminiscent of the 3 May 1999 event (Figure 2). For instance, the vertical wind profiles were similar, with the primary differences being that on 23 April environmental winds generally were stronger throughout the sounding and low-level flow more backed. Also, the observed storm motions on 23 April were faster and more rightward than those on 3 May, relative to the 0-6 km bulk shear (Figure 3). Consequently, an argument can be made that the 23 April 2003 environment could have been more favorable for tornadoes than that of 3 May 1999. Further, both cases were characterized by very strong low-level bulk shear magnitude of ~15 m s^{-1}, with this shear located from about 10-250 m on 3 May 1999 and from about 10-440 m on 23 April 2003. Yet no tornadoes were recorded with either of the storms near KFWD on 23 April 2003, while a catastrophic tornado outbreak occurred on 3 May 1999 that included the deadliest tornado since 1979 (Brooks and Doswell 2002).

It is important to note that radar evidence suggests that the 23 April storms became increasingly high-precipitation (HP) and elevated during their life cycle, particularly after 0100 UTC 24 April. However, around and prior to 0000 UTC they appeared surface-based, a condition supported by applying the Thompson and Edwards (2007) effective-layer technique to the KFWD sounding. Also of interest is that this sounding did not exhibit an apparent low-level straight-line hodograph (Figure 3) similar to 3 May, and instead appeared either to be curved, characterized by a 10-m inflow vector not nearly orthogonal to the low-level hodograph, or both. This suggests the possibility that differences in the low-level hodograph relative to the 10-m inflow vector could have been important in preventing the 23 April storms from becoming tornadic, keeping in mind that these storms may have been slightly elevated.

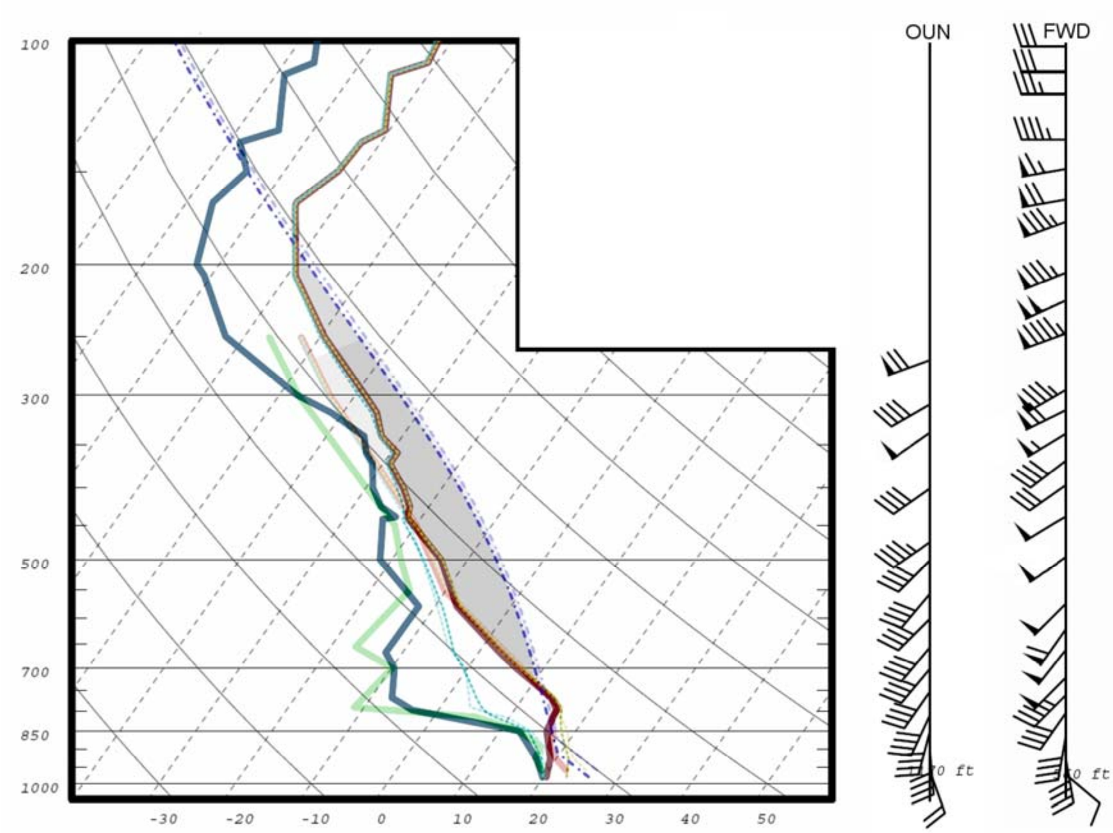


Figure 2: Skew-T representation of the KFWD (Fort Worth, TX) 0000 UTC 24 April 2003 RAOB (darker shading) overlaid on KOUN 0000 UTC 4 May 1999 RAOB (lighter shading). A half barb represents 2.5 m s^{-1}, a full barb 5 m s^{-1}, and a flag 25 m s^{-1}.

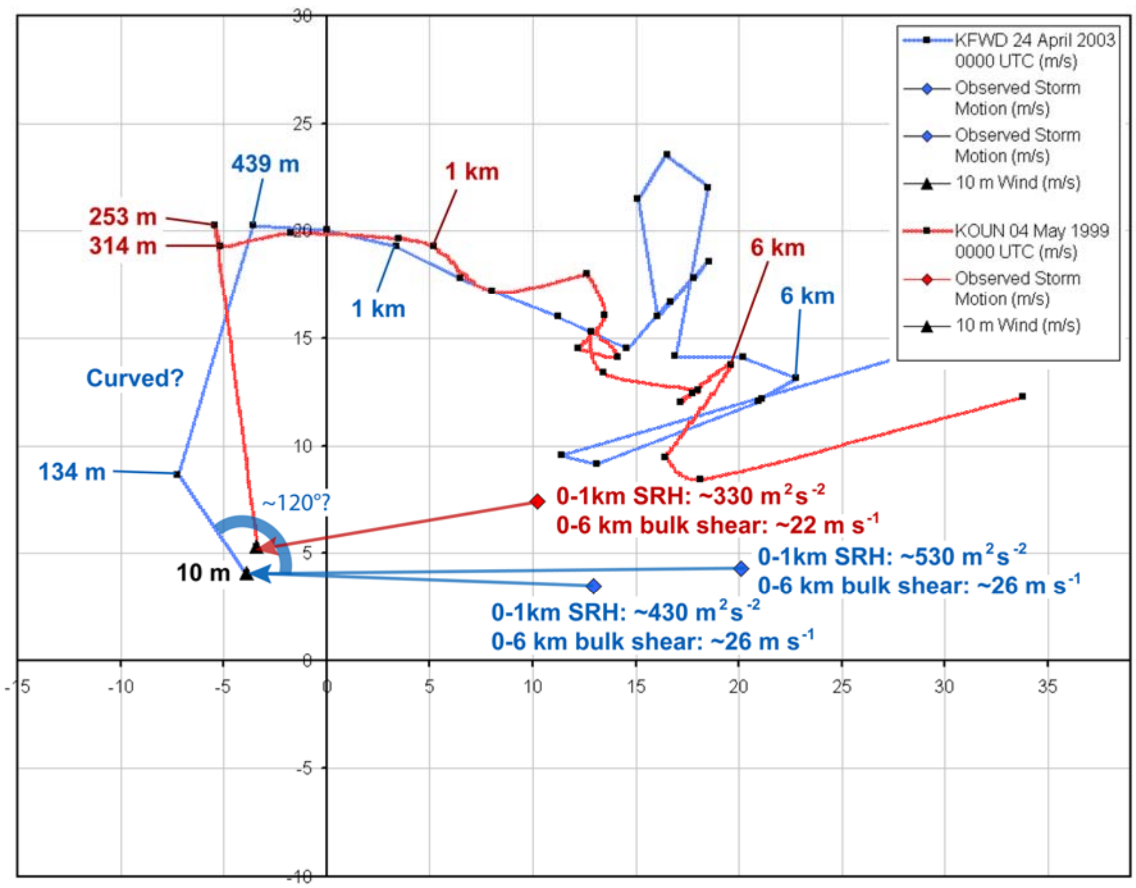


Figure 3: KFWD 0000 UTC 24 April 2003 hodograph overlaid on the KOUN 0000 UTC 4 May 1999 hodograph from Fig. 1. The wind at 10 m used for KFWD is an average of the KGKY (Arlington, TX), KGPM (Grand Prairie, TX) and KRBD (Dallas/Redbird, TX) 0000 UTC 24 April observations. Blue indicates data from 23/24 April 2003, red indicates data from 3 May 1999

c. Historical cases

The 3 May 1999 pattern was associated with numerous tornadoes. Also apparent was a straight-line low-level hodograph, oriented nearly normal to the 10-m inflow vector, that produced almost purely streamwise storm-relative vorticity available for ingestion into the storm updraft at 10 m. However, one case is insufficient to strongly suggest these features are of importance to tornado occurrence; therefore, many other historical cases were examined in a search for further evidence of this feature. In doing so, the authors qualitatively examined soundings near all F3 and greater tornado events 1950-2003 and noted many hodographs that appeared to exhibit this feature, several of which will be presented here.

In 1985, at least two large (~300-400 m wide), significant tornadoes occurred near Throckmorton, TX, around 0000 UTC 22 April. Interestingly, the KSEP (Stephenville, TX) sounding from this time (Figure 4) displays a nearly straight-line hodograph, with ~15 m s^{-1} of shear between 10-210 m that was roughly orthogonal to the 10-m inflow vector for a Bunkers right-moving storm motion (Bunkers et al. 2000; storm motion calculated by the NSHARP (v4.0) program available from Unidata). A true, observed storm motion was impossible to calculate as radar data are unavailable for that time frame; however, the storm reports associated with this apparent supercell tracked roughly northeasterly, which is consistent with the direction of the Bunkers estimate.[3]

[3] It should be noted here that all other events in this study use observed storm motions calculated from WSR-88D data, as in the 3 May 1999 case presented in Figure 1.

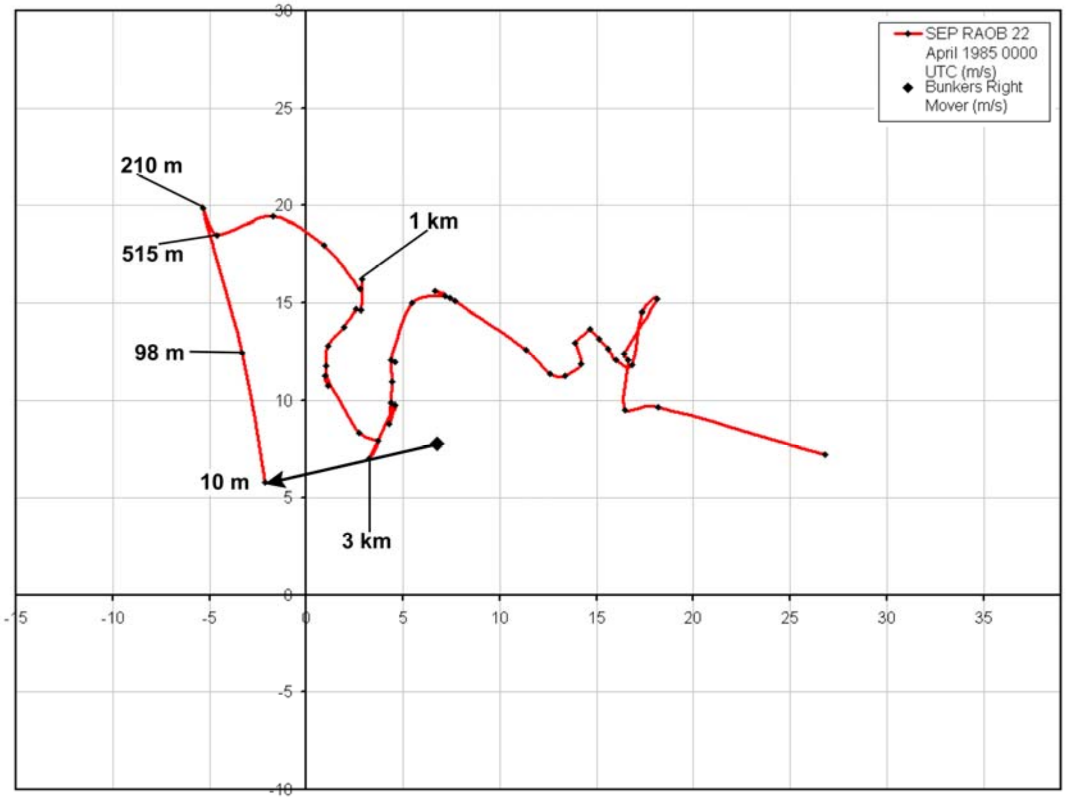


Figure 4: KSEP 0000 UTC 22 April 1985 hodograph. Axis units as in Fig. 1.

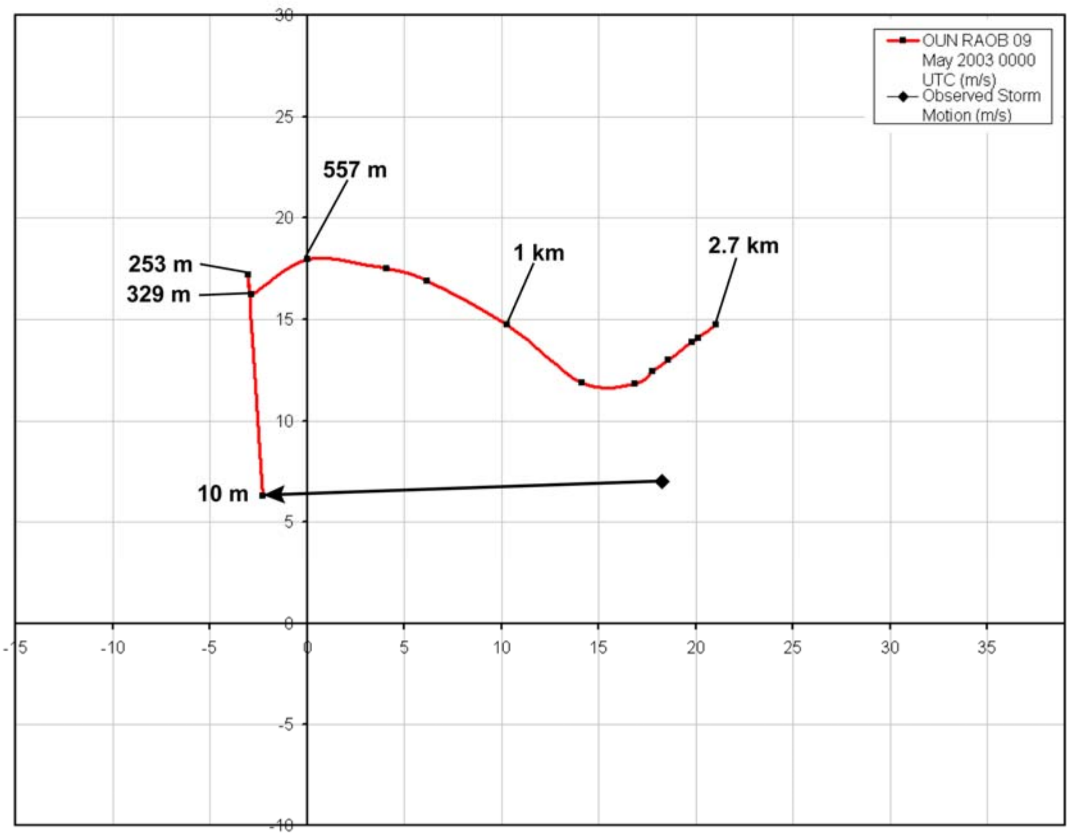


Figure 5: KOUN 0000 UTC 9 May 2003 hodograph. Axis units as in Fig. 1.

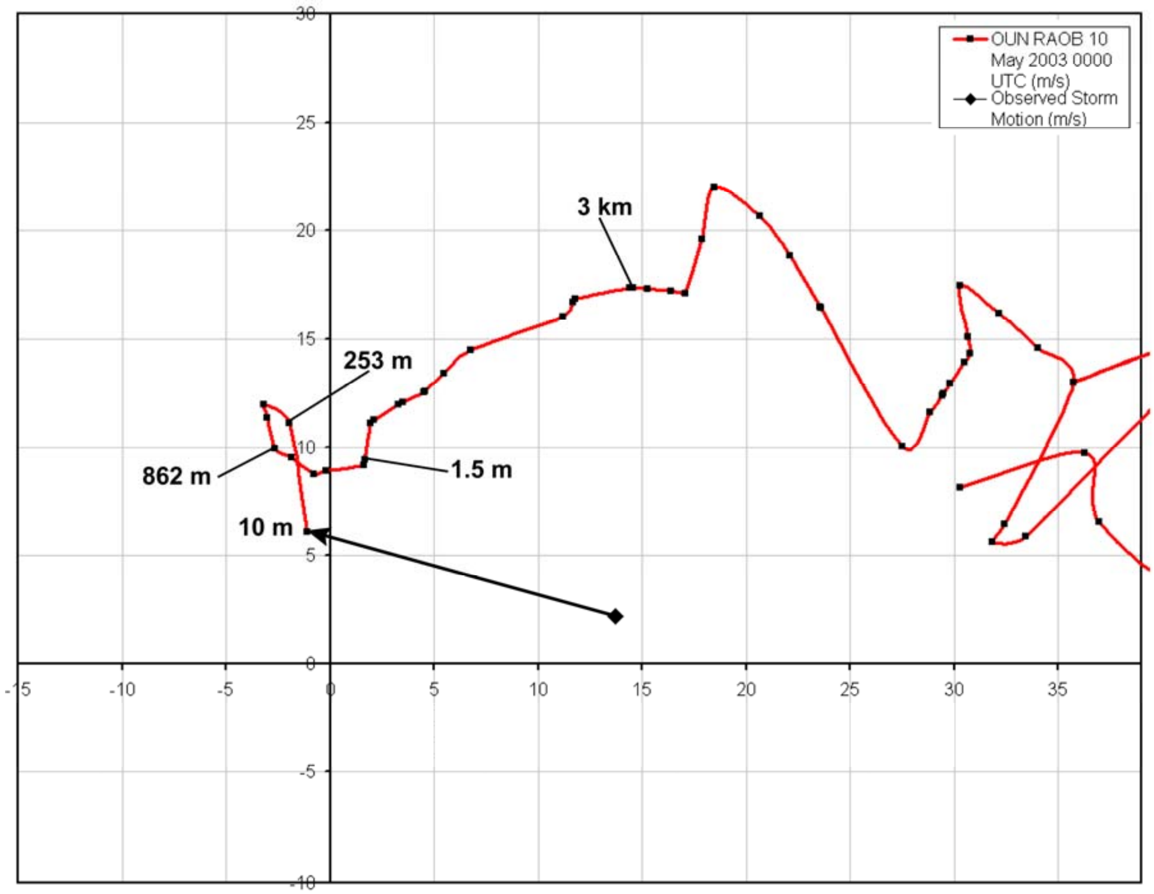


Figure 6: KOUN 0000 UTC 10 May 2003 hodograph. Axis units as in Fig. 1.

More recently, a violent (F4) tornado struck the southeastern region of the Oklahoma City metropolitan area on 8 May 2003. The KOUN 0000 UTC 9 May 2003 hodograph for this event reveals a nearly straight-line hodograph with ~10 m s^{-1} of shear between 10-250 m that is roughly orthogonal to the 10-m inflow vector (Figure 5). Though one could question the approximately straight-line assertion for this hodograph as only two data points are present below the kink, the KOUN sounding exhibits almost no directional shear between 10-250 m and is approximately in agreement with KTLX 0.5 deg base velocity data. Thus, there is little reason to suggest the hodograph is significantly curved in this layer.

The next day (9 May 2003), another significant tornado (SigTor) event took place as an F3 tornado occurred on the northeast side of Oklahoma City. The 0000 UTC KOUN 10 May 2003 hodograph (Figure 6) was unimpressive using most common metrics and exhibited little speed shear in the low levels, with any straight-line hodograph that may have been present not orthogonal to the 10-m inflow vector. However, the F3 tornado occurred between 0329 and 0406 UTC, nearly 5 hours after the RAOB sampled the boundary layer. Serendipitously, one of the authors was operating a 915 MHz wind profiler collocated with both the KOUN RAOB and Norman Oklahoma Mesonet sites during this time period. The 0405 UTC hodograph from this unit (Figure 7), coupled with wind observations at 10 m from the Oklahoma Mesonet, shows a nearly straight-line hodograph of ~15 m s^{-1} between 10-280 m that is approximately orthogonal to the 10-m inflow vector.

The SigTor cases presented above have two features in common. First, they appear to exhibit an approximate straight-line hodograph with at least 10 m s^{-1} of bulk shear in the lowest few hundred meters AGL, and second, the 10-m inflow vector is close to being orthogonal to this straight-line hodograph segment. These features are used to form the hypothesis examined in this paper.

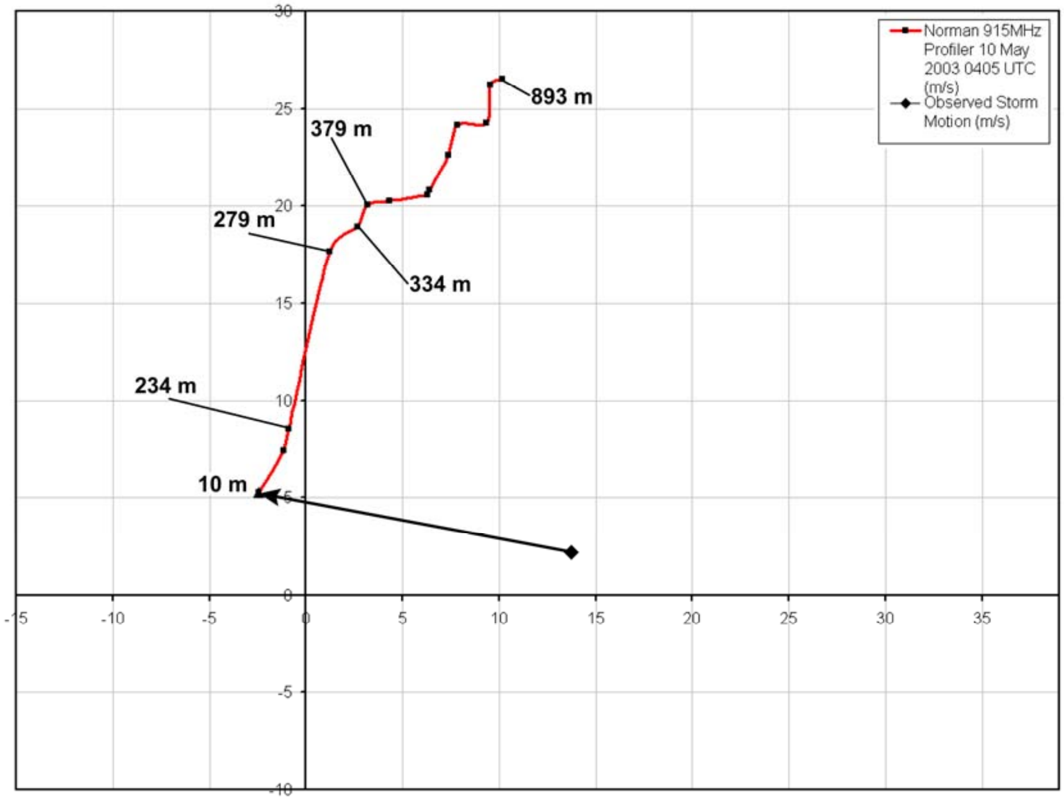


Figure 7: Hodograph from the Norman, OK, 915 MHz wind profiler, 0405 UTC 10 May 2003 hodograph. Axis units as in Fig. 1. The wind at 10 m was derived from an average of three 5 min observations at 15 min intervals from the Norman, Shawnee and Chandler Oklahoma Mesonet sites centered on 0330 UTC. The profiler was configured to capture 25 min consensus wind observations twice hourly at a vertical resolution of 55 m.

d. Hypothesis

Based on the observational data presented above, the authors hypothesize that a strong (~10 m s^{-1} bulk shear or greater) straight-line hodograph in approximately the lowest 500 m AGL oriented orthogonal to the storm-relative inflow vector at 10 m favors significant tornadoes given the occurrence of surface-based supercell thunderstorms. Such a combination of features has not been identified previously in the literature, but is strongly related to a number of other well-researched parameters. These parameters include low-level SRH, low-level bulk shear magnitude, and purely streamwise storm-relative vorticity.

3. Data and methodology

a. Event data sources

We searched three storm event databases while performing this analysis: (1) the Thompson and Edwards (hereafter, T & E; Thompson et al. 2003) database that included supercell events from 1999-2001 and 2003-2004; (2) a database compiled by Bunkers (Bunkers et al. 2000)[4] that included supercell events from 1997-2002; and (3) all tornado events from the National Climatic Data Center's (NCDC) online Storm Events database[5] from 1997-2004 not present in the aforementioned databases that satisfied the criteria outlined below.[6] The cases selected occurred primarily in the afternoon during spring; see Appendix A for a detailed table. Due to the sensitivity of the low-level

[4] Both databases have been updated since their respective publications.

[5] This database is accessible at http://www4.ncdc.noaa.gov/cgi-win/wwcgi.dll?wwEvent~Storms.

[6] We included (3) in order to increase the sample size of the WeakTor and SigTor bins.

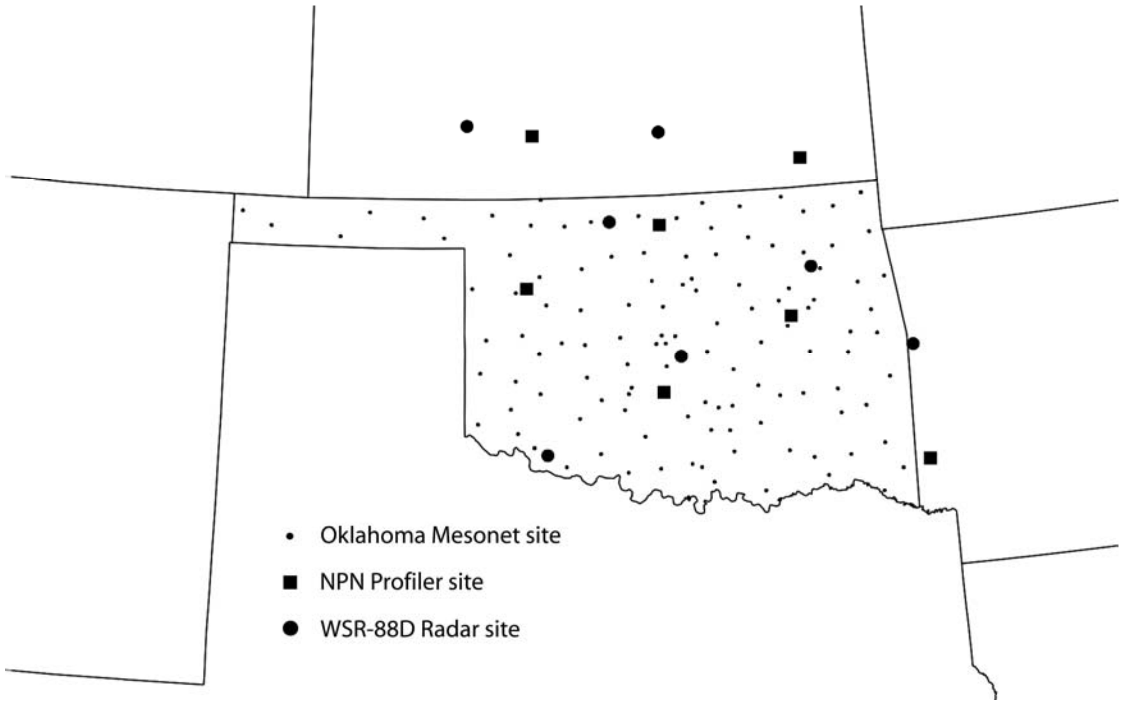


Figure 8: Geographical depiction of all current Oklahoma Mesonet sites, as well as specific NPN and WSR-88D installations used in this study. Each dot represents a Mesonet site, each square a profiler location, and each circle a radar.

hodograph to small variations of the wind at 10 m, the analysis was restricted to Oklahoma where high-resolution surface observations were available through the Oklahoma Mesonet.

b. Analysis data sources

We used three data sources to create the event hodographs: (1) Oklahoma Mesonet[7] 10-m wind data; (2) upper air data obtained from NPN wind profilers sited across the U.S. Plains[8]; and (3) WSR-88D data (used to calculate the observed storm motion for each event). Figure 8 displays the spatial density of each of these observational networks.

[7] The Oklahoma Mesonet is a dense network of surface observing stations with sites located throughout the state of Oklahoma.

[8] The NPN data were selected instead of RAOB due to the greater spatial and especially temporal resolution of the wind profilers; thus, it was not possible to use an "effective layer" approach similar to Thompson et al. (2007) due to the NPN's lack of both thermodynamic data and observations below 500 m.

c. Case selection and data extraction

All events from the T & E and Bunkers databases were considered for selection. Using WSR-88D Level II radar data[9], the average motion of each storm over a 60 min period was determined. This was accomplished by tracking the maximum value of 0.5 deg Base Reflectivity from the nearest available NEXRAD site from approximately *t* – 30 to *t* + 30 min, where *t* equals the time at which the event occurred as defined below.

For the NonTor cases, *t* was considered to be the event time identified by T & E since all NonTor cases were retrieved from this database. For Tor (SigTor and WeakTor) cases accessed from the T & E and Bunkers databases, *t* marks the touchdown of the highest-rated tornado associated with each storm. For tornado cases retrieved solely from the NCDC Storm Events database, only one event was included for each convective day (defined as 1200 to 1159 UTC) to avoid skewing the results in favor of outbreak

[9] Level II radar data are available at http://hurricane.ncdc.noaa.gov/pls/plhas/has.dsselect.

situations such as 3 May 1999. This was done by selecting the earliest occurring highest-rated tornado on each convective day. Two exceptions applied only to the SigTor class, due to special difficulties presented by the 9 June 1998 and 8 May 2003 cases. In the former, a storm produced multiple SigTors that shared the highest rating for the convective day, with more than two hours separating the initial touchdown of each SigTor and no intervening SigTors. In the latter, the highest-rated SigTor of the day was also not unique, with two storms separated by well over 100 km spawning tornadoes of the same rating. Inclusion of these cases resulted in the addition of two events to the SigTor dataset. All measures detailed above were motivated by a desire to eliminate bias toward outbreak days when selecting storms and to yield a dataset emphasizing quality over quantity.[10]

After calculating the storm motion, three nearby "representative" Oklahoma Mesonet sites in approximately the right-front quadrant of the storm's motion were determined, in order to calculate the wind at 10 m in the inflow region of the storm.[11] To assess whether an observation was representative, the only criteria applied were that the site location was required to be in the same ambient airmass[12] as the event and that the observations did not appear to be affected by nearby convection. After selecting the three sites to be used, wind data at 10 m were obtained for the times t – 15 min, t, and t + 15 min using the convention defined for t above. These nine observations then were averaged together to produce a single mean wind observation at 10 m for use in the analysis. This averaging was performed because the angle between the storm-relative inflow vector at 10 m and 10-500 m shear vector is highly sensitive to the wind at 10 m. For example, preliminary results from Haugland (2004) suggest the 10-m flow can be strongly influenced by microscale variations in terrain and land-use. These microscale variations were found to be both highly dependent on wind direction and peculiar to each individual observing station, so averaging the wind at 10 m across several sites should diminish these effects.

Next, the four[13] NPN stations closest to the event were determined and the case eliminated if none of the sites had valid, representative data using the same criteria as for the Oklahoma Mesonet observations (without using the right-front quadrant requirement, due to the lower spatial density of the NPN). If valid data at one or more sites were available, then the two nearest sites with valid, representative hourly averaged data encompassing the event time t were extracted, and the one with the lowest value of 10-500 m SRH was rejected. If only one site met these criteria, it was selected for use in the analysis without performing the SRH comparison. This was done because SRH is known to vary markedly over small distances due to its sensitivity to variations in the low-level wind field (Davies-Jones 1993; Markowski et al. 1998a). Choosing the higher of the two SRH values therefore provided a better opportunity to capture the maximum potential of a particular environment.

After completing this procedure, 67 storm events were selected (18 SigTor, 33 WeakTor, and 16 NonTor), of which 65 were associated with supercells. Both non-supercell cases were contained within the WeakTor bin and found to be tornadic events embedded within squall lines, one of which resembled the quasi-linear convective systems described by Weisman and Trapp (2003) and Trapp et al. (2005b).

d. Parameters derived from data extraction

Using the data recorded from each event several parameters were calculated, including SRH and bulk shear for the 10-1000, 10-750, and 10-500 m layers, as well as the 10-m inflow vector. In addition, the angle between the 10-500 m bulk shear vector and the 10-m inflow

[10] Since it could be argued that the inclusion of all four 9 June 1998 and 8 May 2003 cases potentially biased the dataset, the statistics presented in Section 4 were also calculated by excluding the second SigTor event occurring on each of these days. The results differed trivially, with these differences having no impact on the conclusions drawn in Section 5.

[11] In order to sample a representative airmass and remain inside the Oklahoma Mesonet domain, it was occasionally necessary to use sites slightly outside the right front quadrant. However, this had little impact on the analysis.

[12] To be considered "representative", the ambient airmass must approximate the inflow airmass.

[13] This number was arrived at through trial and error, and was determined to be the best compromise between expanding the number of cases and remaining as spatially close as possible to the storm event in question.

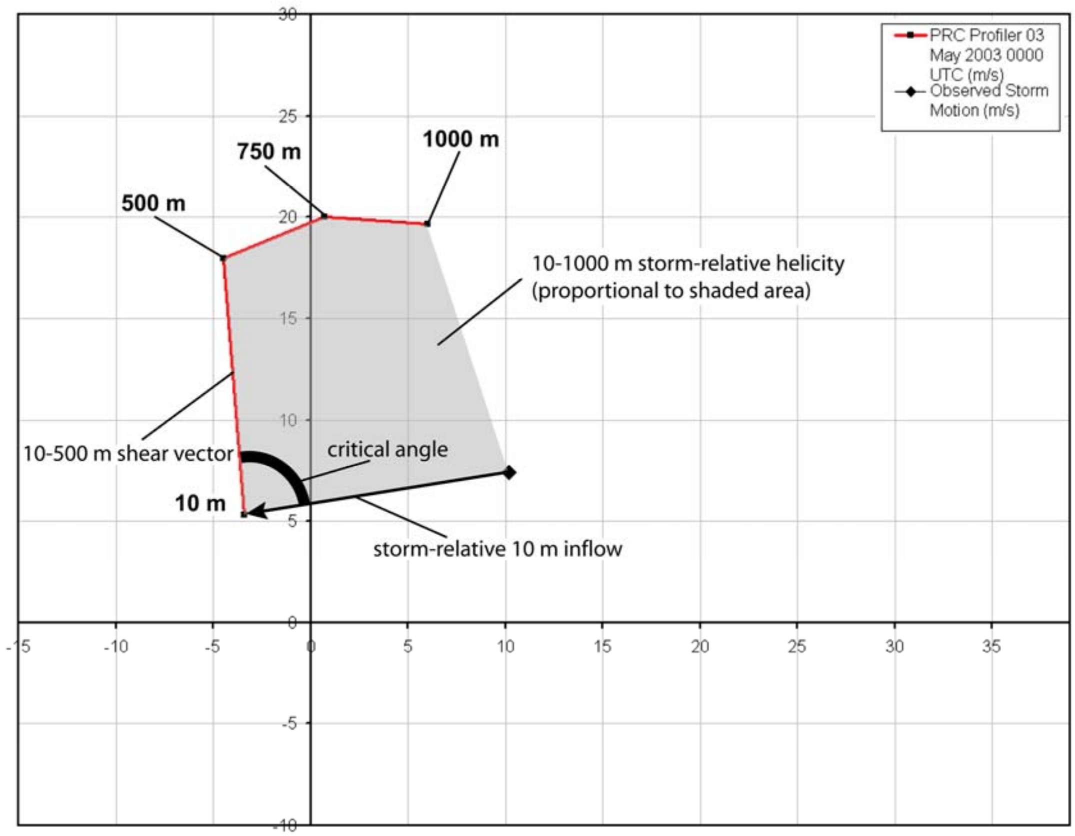


Figure 9: Example analysis hodograph with several key components of the analysis highlighted. Axis units as in Fig. 1.

vector was calculated and is henceforth referred to as the "critical angle". An example hodograph showing several key features of this analysis procedure is presented in Figure 9.

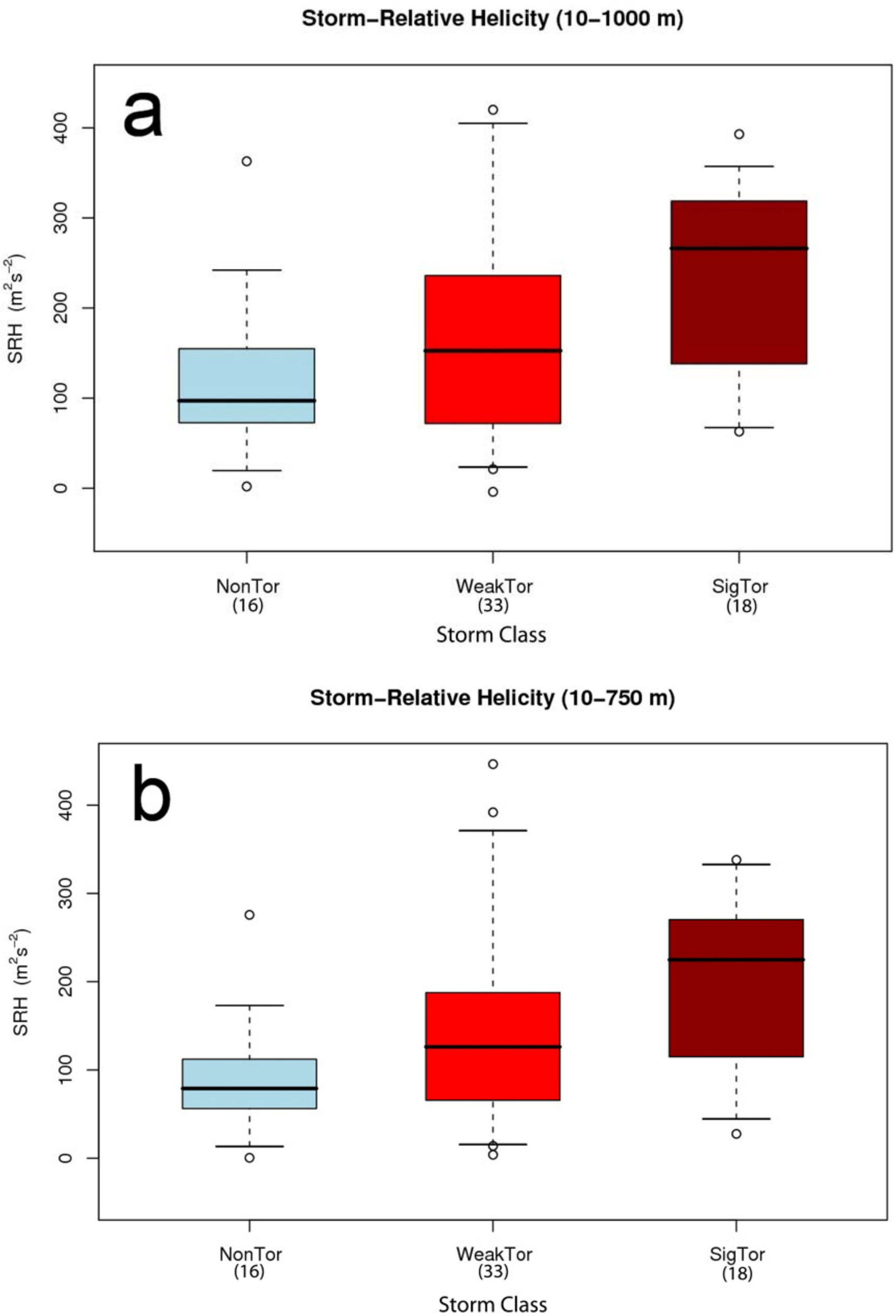
After completing this procedure and finalizing the dataset, the parameter distributions for each storm class were assessed for similarities and differences. This was done visually by employing boxplots and histograms and quantitatively by using significance testing.

4. Results

a. Storm-relative helicity

Examining the distribution of SRH is important because this parameter is related to the strong low-level straight-line hodograph oriented normal to the 10-m inflow vector identified in the hypothesis. Since 10-1000 m SRH is a particularly important component of the fixed-layer STP and is commonly used in the forecasting of severe storms, it will be presented first.

The 10-1000 m SRH boxplots in Figure 10a exhibit a tendency toward higher values for stronger storms (in terms of tornado occurrence and damage) and are very similar to the 0-1 km SRH boxplots presented in Fig. 8 of Thompson et al. (2007). Consequently, considerable interquartile overlap exists between storm classes; therefore, for any given value of 10-1000 m SRH from this dataset, it would be difficult to determine a priori which class the value would occupy. However, there is still useful information available for this layer, particularly when distinguishing between the NonTor and SigTor classes, as tornado occurrence appears increasingly likely for 10-1000 m SRH values greater than roughly 160 $m^2 s^{-2}$. Considering a shallower layer, the 10-750 m SRH shows a similar pattern, although there appears to be a slight increase in the separation of storm classes (Figure 10b). However, it is the 10-500 m SRH (Figure 10c) that is especially noteworthy. Again, a tendency toward increasing SRH for stronger storm


Storm−Relative Helicity (10−1000 m)
a
SRH (m2s−2)
0
100
200
300
400
NonTor
(16)
WeakTor
(33)
SigTor
(18)
Storm Class
Storm−Relative Helicity (10−750 m)
b
SRH (m2s−2)
0
100
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Storm Class

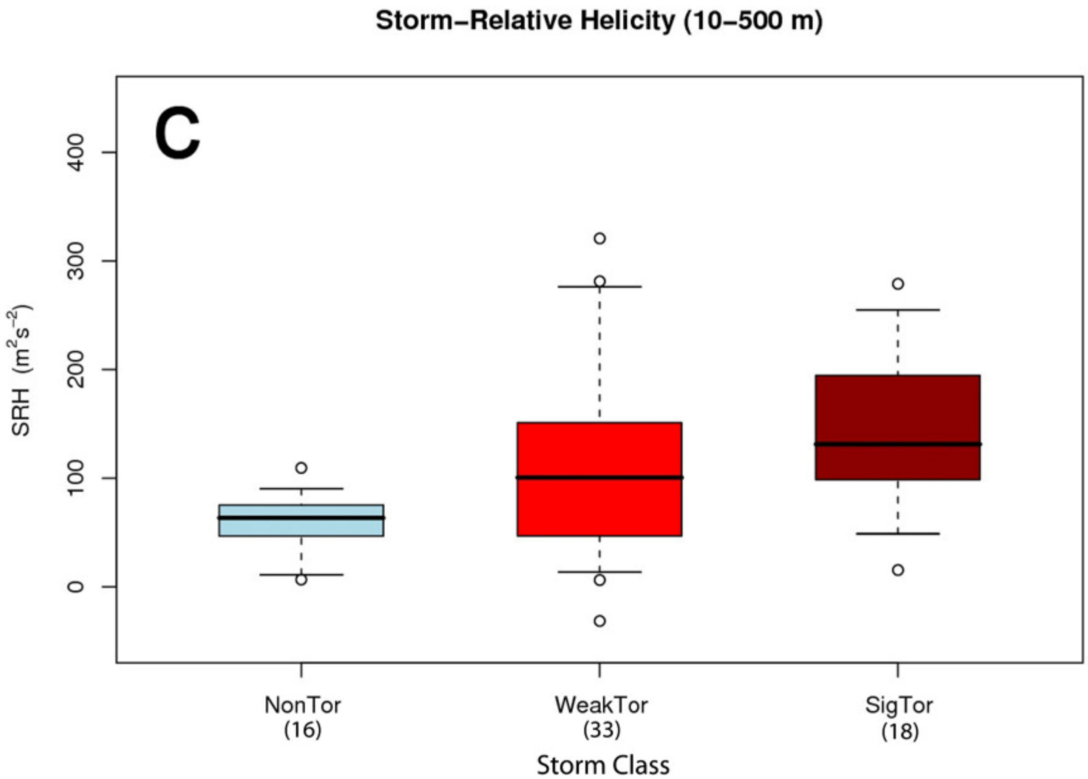


Figure 10: Boxplots of a) 10-1000 m, b) 10-750 m, and c) 10-500 m SRH vs. storm class. The box represents the middle 50% of the distribution, with the whiskers denoting the 10th and 90th percentiles. Outliers are plotted as open circles.

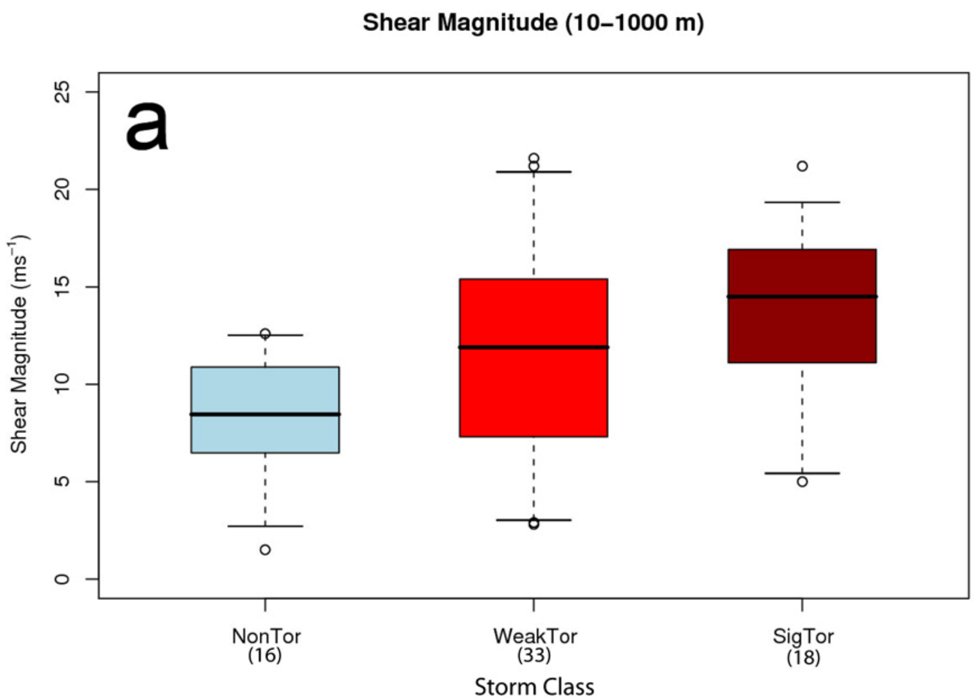
Table 1: Highest confidence interval to which differences in median SRH between storm classes are statistically significant, when compared using binomial significance testing.

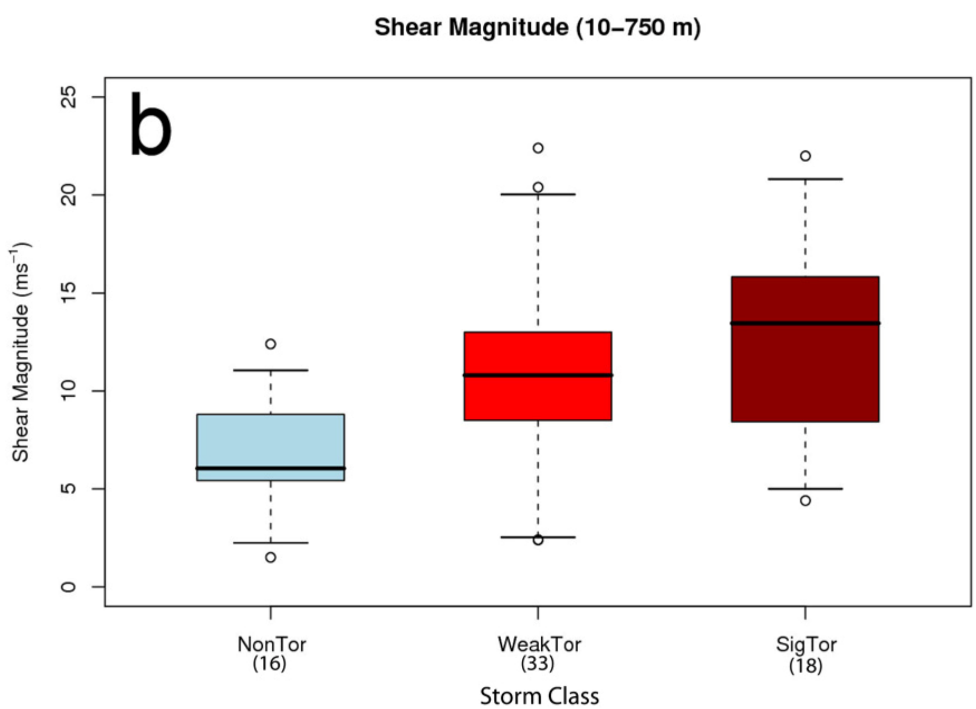
Increasing differences between storm classes →

10-1000 m SRH			10-750 m SRH			10-500 m SRH		
Sig/Weak	Sig/Non	Weak/Non	Sig/Weak	Sig/Non	Weak/Non	Sig/Weak	Sig/Non	Weak/Non
70%	80%	50%	70%	80%	70%	80%	95%	95%

classes is apparent, but the distribution of NonTor SRH is compressed into a very narrow region of low values uncharacteristic of both the WeakTor and SigTor classes, with no interquartile overlap present between the NonTor and SigTor classes. This relationship appears to be substantially stronger for 10-500 m SRH than for either 10-1000 or 10-750 m SRH.

To measure this change objectively, a binomial significance test was performed on the median of each population. Significance values from a t-table were used, since the sample size n of all three populations was such that $n \leq 33$. The test is briefly summarized in Appendix B and yields a confidence interval for the median based on the rank of the data elements. Here, the test was used to determine to what extent each population was different from each of the others, with the results shown in Table 1. This table demonstrates that, for the composite dataset used in this paper, there is a tendency toward greater differences between all storm classes as progressively shallower layers of SRH are considered. It should be noted that despite the small sample sizes (in particular evidenced by the medians for the SigTor and NonTor class, which are skewed from the center of the interquartile range), the differences between the Tor and NonTor populations are statistically robust to the 95% confidence interval. It is likely that increasing the sample size would improve these statistical relationships, as long as the data quality standards outlined in Section 3 are not compromised.





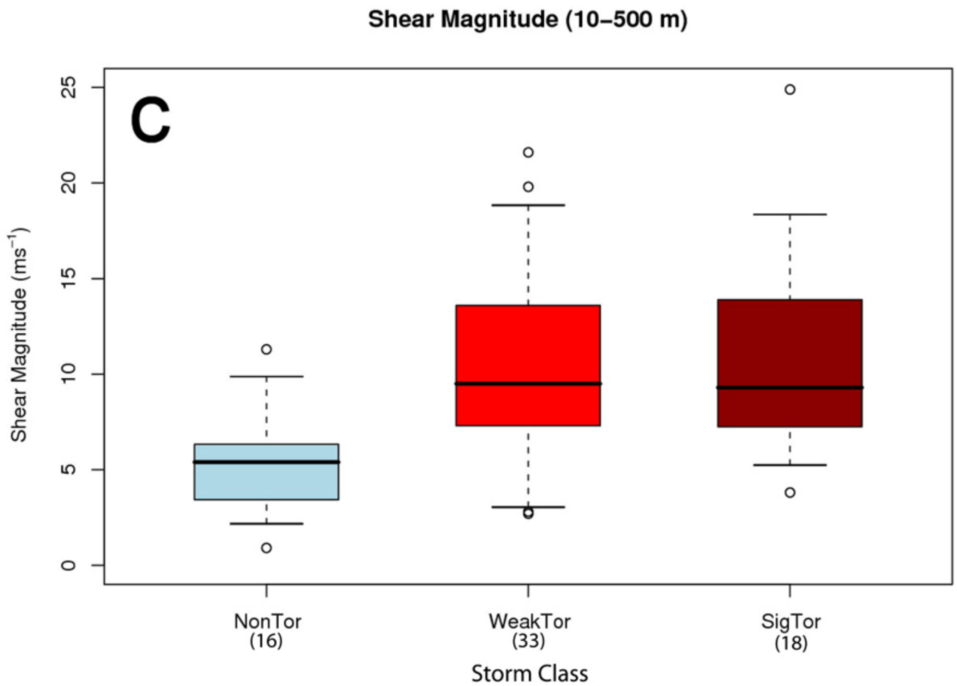


Figure 11: Boxplot of inflow magnitude vs. storm class. Plotting conventions are as in Fig. 10.

Table 2: As in Table 1, but for bulk shear magnitude.

Increasing differences between Weak/Non →

10-1000 m Shear			10-750 m Shear			10-500 m Shear		
Sig/Weak	Sig/Non	Weak/Non	Sig/Weak	Sig/Non	Weak/Non	Sig/Weak	Sig/Non	Weak/Non
80%	80%	60%	50%	80%	90%	0%	80%	95%

Decreasing differences between Sig/Weak →

b. Bulk shear magnitude

There are only three variables controlling 10-500 m SRH in this study—the magnitude of the 10-500 m shear vector, the magnitude of the 10-m inflow vector, and the angle separating the two. Thus, it is important to study each variable independently. First, the shear magnitude will be examined, as the 10-1000 m shear magnitude is commonly used in tornado forecasting (Craven et al. 2002; Thompson et al. 2003) and 10-500 m shear magnitude is a component of the hypothesis. Similar to the 10-1000 m SRH, the 10-1000 m shear magnitude (Figure 11a) indicates a general tendency toward larger shear for stronger storm classes. This is noteworthy in that there is some ability for this parameter to discriminate between the NonTor and SigTor classes, as values above ~11 m s^{-1} suggest an increasing likelihood for tornado occurrence. However, there is considerable interquartile overlap between the classes, causing difficulty in distinguishing from this value alone to which bin or set of bins a particular shear value belongs. These observations are consistent with Thompson et al. (2003).

Considering a shallower layer, the 10-750 m bulk shear shows a very different and important change in its relationship to storm class (Figure 11b). Instead of exhibiting a tendency toward higher values of shear magnitude for stronger storms, the WeakTor class appears to be much more distinguishable from the NonTor class, and much less distinguishable from the SigTor class. Thus, both tornado classes become more distinguishable from the NonTor class when examining bulk shear in the 10-750 m layer. The 10-500 m shear magnitude (Figure 11c) shows this pattern even more strongly, with the

WeakTor and SigTor classes indistinguishable from one another. Both were characterized by median values nearly twice as large as NonTor class values. Similar to the SRH, the median values for the NonTor and SigTor classes, in particular, are consistently skewed from the center of the interquartile range, likely due to small sample sizes.

The binomial significance test also was applied to the bulk shear magnitude. This test revealed that the statistical significance of the median value differences between the WeakTor and NonTor classes increased substantially for shallower layers of shear, while the differences between the SigTor and WeakTor classes vanished (Table 2). The difference between the SigTor and NonTor classes remained unchanged. At first glance it may appear unusual that the SigTor/NonTor classes are less distinguishable than the WeakTor/NonTor classes for the 10-500 m layer bulk shear, since the SigTor and WeakTor classes exhibit nearly identical distributions. However, this makes sense because the WeakTor class contains almost twice as many data points as the SigTor class, thereby yielding a narrower confidence interval about the median, and enhancing the statistical difference when comparing it to the NonTor class.

c. Storm-relative inflow

The second component of the 10-500 m SRH as determined in this analysis is the 10-m inflow magnitude, which is not part of the hypothesis but is of interest due to its influence on the SRH. Visually, the boxplots of inflow magnitude tend toward higher values for stronger storm classes (Figure 12), although the relationship appears to be weaker than for SRH and bulk shear. A binomial significance test identical to those performed earlier confirms this tendency (Table 3), but the results are not significant at the 95% confidence level. Note that this result is not as robust as those for 10-500 m SRH and bulk shear magnitude. This could partly be due to the median for the Tor classes being skewed toward the lower part of the interquartile range, whereas the NonTor class is skewed toward the upper part. In any event, Figure 12 does suggest that SigTors are less likely to occur for values of storm-relative inflow below ~11 m s^{-1}, and that their likelihood increases for values above ~13 m s^{-1}.

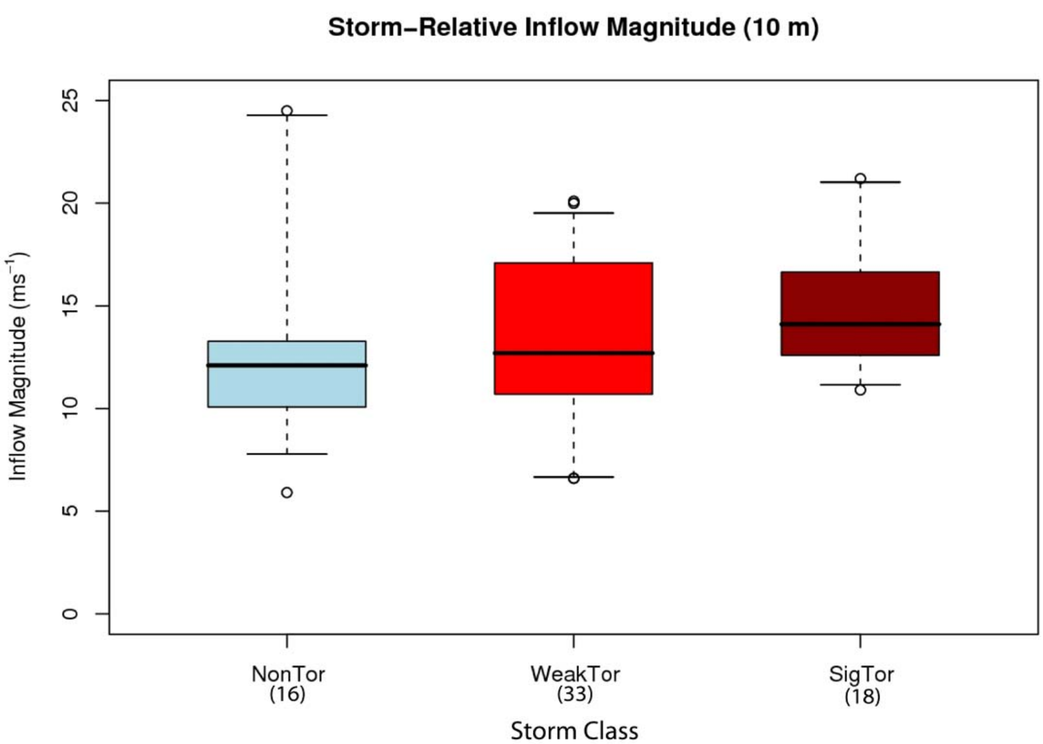


Figure 12: Boxplot of inflow magnitude vs. storm class. Plotting conventions are as in Fig. 10.

The third, and final, component of the 10-500 m SRH is the critical angle separating the inflow vector from the 10-500 m shear vector, and directly relates to the second component of the hypothesis. All else being equal and assuming a surface-based storm, as the critical angle approaches 90° the 10-500 m SRH maximizes with the 10-m inflow vector containing almost purely streamwise vorticity

Histograms of the critical angles were plotted to determine their distribution between storm classes. For the NonTor class, Figure 13a shows

Table 3: Highest confidence interval to which differences in median inflow magnitude between storm bins are statistically significant when compared with one another using binomial significance testing.

10 m Inflow		
Sig/Weak	Sig/Non	Weak/Non
50%	80%	<50%

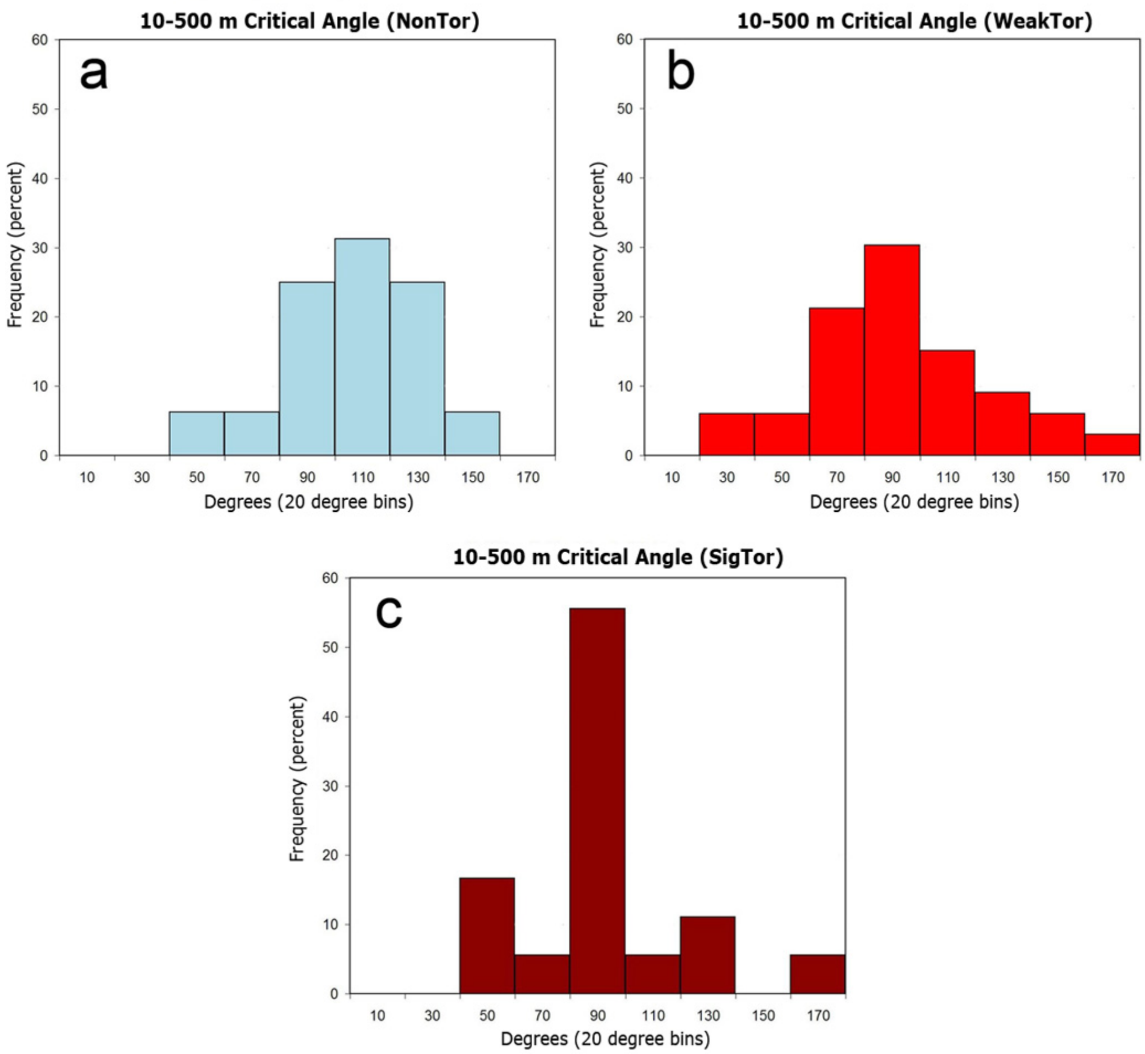


Figure 13. Relative frequency of 10-500 m critical angle for the a) NonTor, b) WeakTor, and c) SigTor classes. Bins are in 20° increments.

that the majority of the critical angles are broadly distributed around 110° (±30°), which is reminiscent of the 23 April case in Section 2a. The majority of the WeakTor class (Figure 13b) also exhibit broadly distributed critical angles, but are instead centered around 90° (±30°) instead of 110°, with a possible bias toward < 90° angles. Unlike either of these two, however, the SigTor class (Figure 13c) shows most critical angles tightly clustered around 90° (±10°) with the remaining events widely scattered about that peak and showing no discernible pattern.

5. Conclusions and discussion

a. The results are approximately consistent with the hypothesis.

As shown by the data, the SigTor events in this study tend to be characterized by strong (usually ~7.5 m s^{-1} or greater) 10-500 m bulk shear and critical angles near 90°. Although this value of bulk shear is somewhat smaller than anticipated, it is well-separated from the NonTor class. Because of this, it is clear that the NonTors simultaneously met both the bulk shear and critical angle conditions only rarely (if at all), while SigTor events frequently satisfied them, which is generally consistent with the hypothesis.

b. 10-500 m shear shows promise as a predictor of tornadoes.

The tornadic storms in this study were associated with high (usually ~7.5 m s^{-1} or greater) 10-500 m shear with no discernible difference between the SigTor and WeakTor classes. However, considerable differences are evident for both Tor classes when contrasted with the NonTor class, an important finding since this indicates that 10-500 m shear has considerable potential for improving tornado forecast skill (at least in a binary sense). It should be noted that any potential forecast enhancement would be predicated on the accurate prediction or observation of the 10-500 m wind profile in an operational environment, as well as the occurrence of a surface-based supercell.

c. Tornadic and non-tornadic storms tend to be characterized by different critical angles.

It is noteworthy that the two supercells that occurred on 23 April 2003 (Section 2a) in north-central Texas were moving in a direction that produced a critical angle of approximately 120°, whereas the 3 May 1999 case exhibited a critical angle of roughly 90°. As a whole, the NonTor class used in this study contained critical angles broadly distributed around 110°. Conversely, the WeakTor and SigTor contained critical angles closer to 90°.

d. Storm-relative inflow is related to tornado occurrence.

The strength of the 10-m inflow vector did not differ as robustly between storm classes in comparison to the 10-500 m SRH and bulk shear. However, the results do suggest that as 10-m inflow magnitude decreases below ~11 m s^{-1} significant tornadoes become increasingly unlikely, whereas for values above ~13 m s^{-1} they become increasingly likely. Since this study consisted almost entirely of surface-based supercells, both of these criteria should be interpreted in that context.

e. 10-500 m SRH is a better discriminator between storm classes than 10-1000 m SRH.

As we examined increasingly shallow layers of SRH during this study, we discovered SRH varying more among all storm classes. In particular, large variability between classes in the 10-500 m layer was found. Particularly noteworthy is the finding that this parameter is not interchangeable with the 10-500 m bulk shear, which exhibited a much different pattern of variability for shallower layers. This result, in combination with the results for 10-500 m bulk shear, in large part addresses Question 3 from Miller (2006), which proposed investigating common shear parameters from 10-500 m and comparing the results with those from 10-1000 m.

f. Better low-level observational networks and operational numerical weather prediction are sorely needed.

The results of this study suggest that knowledge of variations in low-level wind field is paramount in predicting tornado occurrence and intensity. These variations are not well sampled by existing observational networks (Stensrud et al. 1990; Markowski et al. 2003) and probably are not well forecast by the current operational models based on comparisons between real and modeled hodographs. For example, in Figure 14[14] note how the observed

[14] Fig. 14 is valid at the time the violent Greensburg, KS, tornado was causing EF-5 damage.

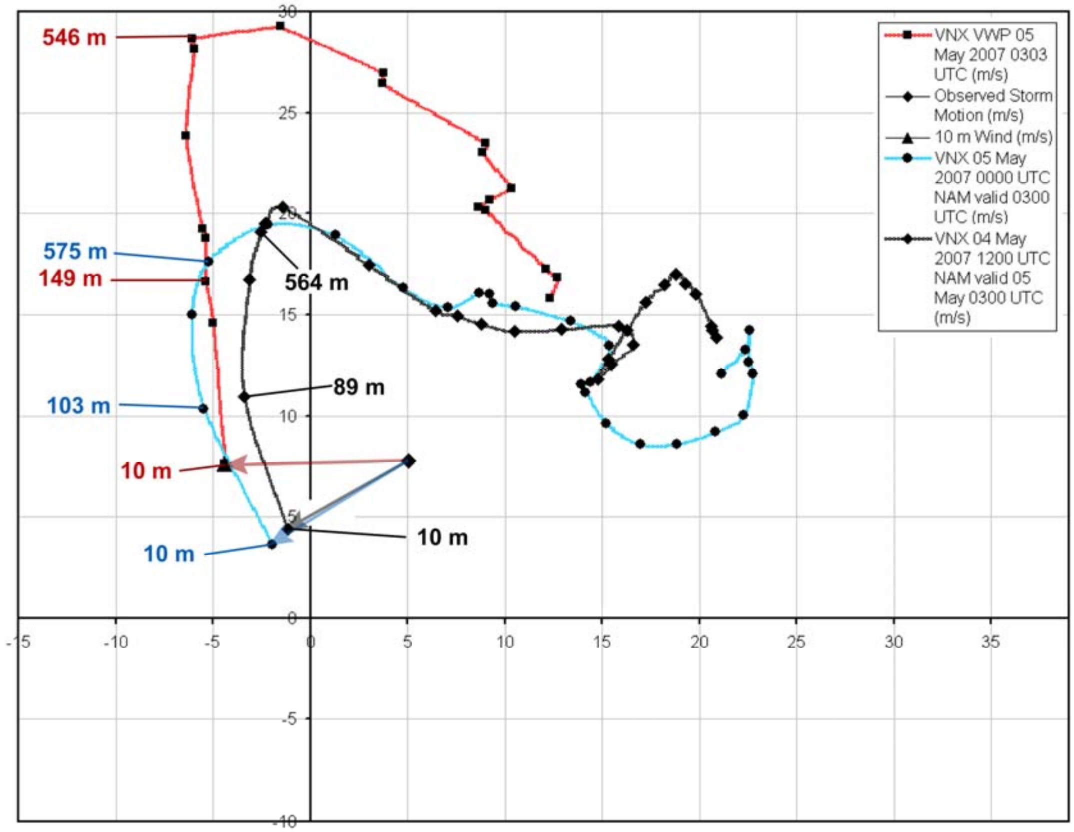


Figure 14: The 1200 UTC 4 May 2007 15-hour forecast and 0000 UTC 5 May 03-hour forecast hodographs (both valid 0300 UTC 5 May) for KVNX (Vance AFB, OK) from the NAM are overlaid on the 0303 UTC KVNX observed hodograph, which uses the KPTT (Pratt, KS) 0250 UTC observation for the wind at 10 m. Axis units as in Fig. 1.

wind profile is characterized by an approximately straight-line hodograph between 10-500 m and extreme shear (greater than 20 m s^{-1}), whereas the model forecast hodographs exhibit more curvature and weaker shear in that layer. It seems unavoidable that without improvements to these systems, both the prognosis and diagnosis of tornado events will remain problematic. Some forecasting advancements may be possible with current observational networks, but will be significantly constrained by the networks' inability to consistently resolve key low-level features.

6. Further research questions

The results discussed above raise a number of interesting questions about features favorable for the development and maintenance of strong low-level mesocyclones and tornadoes. Several are presented below.

a. Do right-moving supercells and backed surface winds really favor tornadogenesis?

The increased rightward propagation of a supercell and the backing of low-level flow are commonly thought to enhance tornado potential. In support of this, it is simple to envision a scenario in which a storm is moving in a direction which yields a critical angle less than 90°; in this instance, enhanced rightward propagation could bring the storm closer to a 90° critical angle. However, it is equally possible to envision a scenario where the enhanced rightward propagation of a storm would increase the critical angle to values greater, or even much greater, than 90°. Similarly, backing surface winds could have either a positive or negative impact on the tornadic potential of a storm, in terms of the critical angle. The hypothesis of this paper suggests that the pre-existing configuration of the storm motion relative to the low-level hodograph determines whether enhanced rightward propagation and/or backing surface winds increase or reduce the tornadic potential of a storm; however, further research is needed to verify this proposition.

b. Since the 10-500 m shear magnitude is directly related to the strength of boundary layer mixing, and the strength of boundary layer mixing is directly related to the amount of insolation, then what are the impacts of strong afternoon heating, anvil shadows, and sunset?

Similar to enhanced rightward storm propagation and backing surface winds, afternoon heating is not necessarily a negative factor for tornado potential and can accomplish several things which aid the development of tornadic storms. For example, it often facilitates initiation by weakening the cap, increasing instability and generating low-level convergence (such as along a dryline). However, this heating also tends to diminish boundary layer shear by encouraging strong mixing that forces the profile of momentum to become more constant with height. Given this physical conundrum, what would be the consequence of removing the forcing mechanism for boundary layer mixing (i.e., insolation) from a destabilized, well-mixed convective boundary layer? Two important impacts to be expected include decreased mixing and enhanced boundary layer shear as the boundary layer decouples and stabilizes.

One scenario that could cause such a decoupling of the boundary layer is the spreading out of anvils from thunderstorms, which has been shown in recent years to be a source of baroclinic vorticity. Upon investigating several anvil shadow cases, Markowski et al. (1998b) found a region of baroclinic vorticity on the order of 10^{-2} s^{-1} occurred approximately along the edge of the anvil shadows and suggested this zone may provide an important source of vorticity for low-level mesocyclones. However, in light of observational evidence that near-surface vorticity attributable solely to environmental shear can approach 10^{-1} s^{-1} (e.g., Figs. 1, 4 and 14), perhaps it is instead the decoupling of the boundary layer and ensuing enhancement of low-level shear an anvil shadow may cause that is more significant. If this is the case, it should be noted that boundary layer decoupling and enhancement of low-level shear also occur sometime between the passage of peak afternoon heating and sunset; therefore, this effect also would be an integral part of the diurnal cycle. What temporal and spatial scales over which this decoupling might occur are ill-defined, but should be related to the degree to which net radiation at the surface becomes negative—that is, outgoing radiation exceeds incoming radiation at the earth's surface (Maddox 1985; Maddox 1993).

Regardless, in any case where surface net radiation is negative, it is important that the boundary layer not cool excessively. Otherwise, it would become impossible to maintain an unstable, weakly-capped environment in which storms would remain surface-based. This may largely explain the influence of LCL heights on significant tornado occurrence (Rasmussen and Blanchard 1998; Craven et al. 2002; Rasmussen 2003; Thompson et al. 2003), as an environment characterized by low LCL heights would also be an environment where surface temperatures are unable to cool as much through radiative processes due to the relative abundance of moisture.

c. How common are low-level straight-line hodographs, over what depth do they occur, and does this depth impact tornado potential and/or strength?

Until Thompson and Edwards (2000), kinked low-level hodographs had not been identified in the literature. The relationship between a nearly straight-line low-level hodograph and storm motion identified in this paper is an extension of this concept, casting further doubt on the importance of near-surface hodograph curvature for tornadogenesis, while still confirming the general principle of surface to 3 km hodograph curvature. Consequently, a more exhaustive cataloguing of low-level straight-line hodographs could provide insight into what sort of synoptic and mesoscale patterns, if any, tend to produce them and what their frequency might be. Also, such a study could identify additional synoptic indicators of the likelihood for or against outbreaks of SigTors.

Further, over what depth nearly straight-line, low-level hodographs occur is unknown. The 22 April 1985 case was characterized by a ~15 m s^{-1} nearly straight-line hodograph over approximately the lowest 200 m AGL, which implies that there was very strong horizontal vorticity of roughly 10^{-1} s^{-1} in the vicinity of the storm. All else being equal, if this nearly straight-line hodograph were distributed over the lowest 1000 m instead of the lowest 200 m, it seems reasonable that tornado potential and/or strength would be negatively impacted. For the purpose of this paper, we assumed that 500 m was a good proxy in order to simplify the analysis, both because 500 m is the lowest gate available on NPN wind profilers and because observational evidence suggests the kink occurs near that level for at least some significant tornadoes (Miller 2006). However, further research is needed to assess the validity of this assumption.

ACKNOWLEDGMENTS

The authors would like to thank Rich Thompson, Roger Edwards, Matthew Bunkers, David Schultz, and Kevin Manross for providing data used in investigating storm environments and producing this analysis. We also would like to thank David Schultz and Chuck Doswell for their guidance and encouragement, Alan Shapiro for thoughtful discussion on the possible role of low-level straight-line hodographs, Michael Richman for his assistance with the binomial significance test, and Lou Wicker for discussing some of his past research results. The comments and suggestions offered by John Monteverdi, Dan Miller and Steven Weiss while reviewing our manuscript were also most helpful and greatly appreciated, as were the grammatical and technical edits suggested by Ryan McCammon and Roger Edwards and the layout editing performed by Elke Edwards.

In addition, there were a number of people who took the time to constructively criticize our thoughts, ideas and manuscript. To name everyone we interacted with is not possible, but we would especially like to recognize David Demko, Reed Timmer, Nicole Giuliano, Daniel Dawson, Gabriel Garfield, and Michael Armstrong. We appreciate your input.

APPENDIX A

A complete list of all cases used in this study is presented in Table A.1

Table A.1: All cases used in this study, as sorted by storm class and date. Relevant data such as the Oklahoma Mesonet sites used to calculate the surface wind are also included. An 'X' beneath the "Data Source" tab indicates in which database(s) each case was present other than NCDC's Storm Events, since by its nature it contained all the tornado cases.

SigTor Cases

						Data Source		Storm Motion	
Date	Time (UTC)	Profiler Station	Mesonet Stations			T & E	Bunkers	Direction (deg)	Speed ($m\ s^{-1}$)
19970525	2200	2200 UTC LMNO2	BYAR	PAUL	SULP			278	8.9
19970527	0129	0200 UTC DQUA4	MCAL	WILB	STIG			295	7.4
19980609	0035	0100 UTC HKLO2	CALV	EUFA	STUA			270	16.1
19980609	0255	0300 UTC HKLO2	TALI	WILB	WIST			272	12.8
19980614	0111	0200 UTC HKLO2	CHAN	NORM	SHAW			264	15.4
19990309	0007	0100 UTC HKLO2	EUFA	STIG	WEBB			265	16.6
19990503	2326	0000 UTC PRCO2	NORM	SPEN	WASH	X	X	234	12.6
19990601	2235	2300 UTC DQUA4	EUFA	STIG	WEBB	X		355	6.1
19991203	0043	0100 UTC PRCO2	CHAN	SPEN	STIL	X	X	207	17.2
20000327	0045	0100 UTC DQUA4	BROK	MTHE	TALI			314	13.6
20010520	2120	2200 UTC DQUA4	EUFA	MCAL	STUA	X	X	252	12.3
20011009	2140	2200 UTC PRCO2	BESS	WATO	WEAT		X	239	14.5
20030419	2305	0000 UTC HKLO2	CLRM	NOWA	SKIA	X		245	19.9
20030508	0754	0800 UTC PRCO2	ANTL	DURA	LANE	X		262	10.3
20030508	2210	2300 UTC PRCO2	CHAN	NRMN	SHAW	X		241	16.2
20030508	2346	0000 UTC HKLO2	COPA	PAWN	WYNO			245	21.9
20040530	0017	0100 UTC LMNO2	ELRE	GUTH	SPEN	X		271	9.8
20040530	0418	0500 UTC LMNO2	BRIS	HECT	OKEM	X		258	10.6

NonTor Cases

Date	Time (UTC)	Profiler Station	Mesonet Stations			Data Source: T & E	Data Source: Bunkers	Storm Motion: Direction (deg)	Storm Motion: Speed (m s^{-1})
19990427	0143	0200 UTC PRCO2	APAC	CHIC	MINC	X		336	11.3
19990701	0300	0300 UTC HKLO2	MARE	MRSH	STIL	X		330	10.3
19990912	0737	0800 UTC VCIO2	ARNE	CAMA	WOOD	X		294	18.8
19990920	0000	0000 UTC HKLO2	CHIC	FTCB	NINN	X	X	311	8.5
19991008	2300	2300 UTC PRCO2	ACME	MEDI	WALT	X		256	5.8
20000316	0000	0000 UTC LMNO2	BESS	RETR	WEAT	X		283	6.4
20000525	0100	0100 UTC PRCO2	APAC	MEDI	NINN	X		285	5.3
20000613	2300	2300 UTC VCIO2	FREE	MAYR	WOOD	X	X	258	10.2
20010507	0000	0000 UTC HKLO2	CALV	CENT	SULP	X		300	9.6
20010528	0130	0130 UTC PRCO2	ELRE	GUTH	KING	X		245	8.0
20030317	2000	2000 UTC PRCO2	APAC	FTCB	NINN	X		203	8.0
20030317	2200	2200 UTC VCIO2	BURN	RING	WAUR	X		239	6.4
20030403	2300	2300 UTC VCIO2	APAC	CHIC	MINC	X		243	11.2
20030419	2000	2000 UTC LMNO2	BREC	LAHO	MEDF	X		234	10.0
20030510	1900	1900 UTC HKLO2	STIG	WEBB	WILB	X		253	26.2
20030520	0100	0100 UTC HKLO2	BEEX	BURN	MADI	X		288	11.4

WeakTor Cases

Date	Time (UTC)	Profiler Station	Mesonet Stations			Data Source: T & E	Data Source: Bunkers	Storm Motion: Direction (deg)	Storm Motion: Speed (m s^{-1})
19970508	0000	0000 UTC NDSK1	BREC	LAHO	REDR			289	7.6
19970530	0155	0200 UTC VCIO2	BUTL	CHEY	PUTN			333	13.9
19970616	0010	0100 UTC VCIO2	BEAV	BUFF	SLAP			259	17.7
19970616	2310	0000 UTC DQUA4	MCAL	STIG	WILB			282	13.1
19970718	0005	0100 UTC NDSK1	MARE	REDR	STIL			14	8.1
19970819	0042	0100 UTC VCIO2	ELRE	HINT	KING			268	6.8
19970908	2145	2200 UTC HKLO2	OILT	PAWN	PERK			4	6.4
19971009	0015	0100 UTC PRCO2	CHAN	SHAW	SPEN			249	9.3
19980330	2318	0000 UTC DQUA4	EUFA	MCAL	STIG			222	20.3
19980618	2340	0000 UTC PRCO2	EUFA	TAHL	WEBB		X	286	11.2
19980921	2140	2200 UTC PRCO2	GUTH	KING	MARE			275	10.2
19981130	0210	0300 UTC HKLO2	BRIS	HECT	SKIA			241	21.4
19990403	0128	0200 UTC PRCO2	BESS	HOBA	MANG			228	9.9
19990601	0124	0200 UTC PRCO2	GRA2	MEDI	TIPT	X		260	10.6
19990605	0237	0300 UTC HVLK1	BEAV	BUFF	SLAP			226	19.8
19990629	2144	2200 UTC VCIO2	BEAV	HOOK	GOOD	X	X	324	5.7
19991123	0147	0200 UTC PRCO2	PORT	PRYO	TAHL	X		228	16.7
20000225	0229	0300 UTC VCIO2	ARNE	BUFF	WOOD	X		218	17.0
20000315	2252	2300 UTC LMNO2	BREC	LAHO	MEDF	X		288	6.5
20000525	0124	0200 UTC DQUA4	COOK	TAHL	WEST			274	11.5
20000527	0012	0100 UTC PRCO2	ELRE	FTCB	MINC	X		269	6.0
20000614	0456	0500 UTC VCIO2	CAMA	FAIR	SEIL			305	13.4
20001021	2241	2300 UTC HKLO2	KETC	RING	WAUR	X		185	10.4
20001023	0002	0100 UTC PRCO2	NORM	SHAW	SPEN			183	7.4
20010504	2105	2200 UTC VCIO2	ACME	APAC	WALT			209	18.0
20010506	0111	0200 UTC VCIO2	FTCB	HINT	WATO	X		238	11.7
20010506	2230	2300 UTC HKLO2	BURN	DURA	TISH	X		281	10.7
20010606	0034	0100 UTC LMNO2	ARNE	CAMA	SEIL	X		286	0.7
20010920	2320	0000 UTC VCIO2	BUFF	FREE	WOOD			66	4.7
20030317	2057	2100 UTC VCIO2	APAC	HINT	MEDI	X		234	10.4
20030415	2300	2300 UTC VCIO2	BUTL	CAMA	CHEY			232	17.6
20200417	0118	0200 UTC LMNO2	CLOU	DURA	HUGO			237	7.1
20200918	2316	0000 UTC PRCO2	BREC	LAHO	MEDF			253	8.6

APPENDIX B

The binomial significance test used to determine confidence intervals for the median is based on Woodruff (1952). The method consists of taking rank $r = \frac{n+1}{2} + t\frac{\sqrt{n}}{2}$, where n represents sample size and t is the t-statistic, based on degrees of freedom ($n - 1$) and the strength of the two-tailed confidence interval in question. After determining r, the interval [x_r , $x_{(n - r + 1)}$], taken from ordered set X, forms the confidence interval specific to a particular sample of data. If two intervals being compared do not overlap, then the two samples are considered to be statistically different at the confidence level characterized by t.

REFERENCES

Brock, F. V., K. C. Crawford, R. L. Elliott, G. W. Cuperus, S. J. Stadler, H. L. Johnson, and M. D. Eilts: The Oklahoma Mesonet: A technical overview. *J. Atmos. Oceanic Tech.,* **12**, 5-19.

Brooks, H. E, and C. A. Doswell III, 2002: Deaths in the 3 May 1999 Oklahoma City tornado from a historical perspective. *Wea. Forecasting,* **17**, 354-361.

______, ______, and J. Cooper, 1994: On the environments of tornadic and nontornadic mesocyclones. *Wea. Forecasting,* **9**, 606-618.

Browning, K. A., 1964: Airflow and precipitation trajectories within severe local storms which travel to the right of the winds. *J. Atmos. Sci.,* **21**, 634-639.

______, and R. J. Donaldson, 1963: Airflow and structure of a tornadic storm. *J. Atmos. Sci.,* **20**, 533-545.

Bunkers, M. J., B. A. Klimowski, J. W. Zeitler, R. L. Thompson, and M. L. Weisman, 2000: Predicting supercell motion using a new hodograph technique. *Wea. Forecasting,* **15**, 61-79.

Craven, J. P., H. E. Brooks, and J. A. Hart, 2002: Baseline climatology of sounding derived parameters associated with deep, moist convection. Preprints, *21st Conf. Severe Local Storms*, San Antonio, TX, Amer. Meteor. Soc., 643-646.

Davies-Jones, R., 1984: Streamwise vorticity: The origin of updraft rotation in supercell storms. *J. Atmos. Sci.,* **41**, 2991-3006.

______, 1993: Helicity trends in tornado outbreaks. Preprints, *17th Conf. Severe Local Storms*, St. Louis, MO, Amer. Meteor. Soc., 56–60.

______, D. Burgess, and M. Foster, 1990: Test of helicity as a tornado forecast parameter. Preprints, *16th Conf. on Severe Local Storms,* Kananskis Park, Alta., Canada, Amer. Meteor. Soc., 588-592.

Doswell, C. A. III, 1991: A review for forecasters on the application of hodographs to forecasting severe thunderstorms. *Nat. Wea. Digest*, **16** (1), 2-16.

Fawbush, E. J., and R. C. Miller, 1954: The types of airmasses in which North American tornadoes form. *Bull. Amer. Met. Soc.,* **35**, 154-165.

Haugland, M. J., 2004: Isolating microscale phenomena from mesoscale observations. Preprints, *18th Conf. on Hydrology*, Seattle, WA, Amer. Meteor. Soc., CDROM, JP4.9.

Jones, T. A., K. M. McGrath, and J. T. Snow, 2004: Association between NSSL Mesocyclone Detection Algorithm-detected vortices and tornadoes. *Wea. Forecasting,* **19**, 872-890.

Klemp, J. B. and R. B. Wilhelmson, 1978: Simulations of right- and left-moving storms produced through storm splitting. *J. Atmos. Sci.,* **35**, 1097-1110.

Maddox, R. A., 1985: The relation of diurnal, low-level wind variations to summertime severe thunderstorms. Preprints, *14th Conf. on Severe Local Storms*, Boston, MA, Amer. Meteor. Soc., 202-207.

______, 1993: Diurnal low-level wind oscillations and storm-relative helicity. *The Tornado, Its Structure, Dynamics, Prediction, and Hazards. Geophys. Monogr., No. 79*, C. Church, D. Burgess, C. Doswell and R. Davies-Jones, Eds. Amer. Geophys. Union, Washington, DC, pp. 591-598.

Markowski, P. M., J. M. Straka, E. N. Rasmussen, and D. O. Blanchard, 1998a: Variability of storm-relative helicity during VORTEX. *Mon. Wea. Rev.,* **126**, 2959-2971.

______, E. N. Rasmussen, J. M. Straka, and D. C. Dowell, 1998b: Observations of low-level baroclinity generated by anvil shadows. *Mon. Wea. Rev.,* **126**, 2942-2958.

______, C. Hannon, J. Frame, E. Lancaster, A. Pietrycha, R. Edwards, and R. L. Thompson, 2003: Characteristics of vertical wind profiles near supercells obtained from the Rapid Update Cycle. *Wea. Forecasting*, **18**, 1262-1272.

Marwitz, J. D., 1972: The structure and motion of severe hailstorms. Part I: Supercell Storms. *J. App. Meteor.,* **11**, 166-179.

Miller, D. J., 2006: Observations of low level thermodynamic and wind shear profiles on significant tornado days. Preprints, *23rd Conf. on Severe Local Storms,* St. Louis, MO, Amer. Meteor. Soc., 1206-1223.

Rasmussen, E. N., 2003: Refined supercell and tornado forecast parameters. *Wea. Forecasting*, **18**, 530-535.

______, and D. O. Blanchard, 1998: A baseline climatology of sounding-derived supercell and tornado forecast parameters. *Wea. Forecasting*, **13**, 1148-1164.

Rotunno, R. and J. B. Klemp, 1982: The influence of the shear-induced pressure gradient on thunderstorm motion. *Mon. Wea. Rev.,* **110**, 136-151.

Schwartz, B. E. and C. A. Doswell III, 1991: North American rawinsonde observations: Problems, concerns, and a call to action. *Bull. Amer. Meteor. Soc.,* **72**, 1885–1896.

Speheger, D. A., C. A. Doswell III, G. J. Stumpf, 2002: The Tornadoes of 3 May 1999: Event verification in central Oklahoma and related issues. *Wea. Forecasting*, **17**, 362-381.

Stensrud, D. J., M. H. Jain, K. W. Howard and R. A. Maddox, 1990: Operational systems for observing the lower atmosphere: Importance of data sampling and archival procedures. *J. Atmos. Oceanic Tech*., **7**, 930-937.

Strauch, R. G., B. L. Weber, A. S. Frisch, C. G. Little, D. A. Merritt, K. P. Morgan, and D. C. Welsh, 1987: The precision and relative accuracy of profiler wind measurements. *J. Atmos. Oceanic Tech*., **4**, 563-571.

Thompson, R. L. and R. Edwards, 2000: An overview of environmental conditions and forecast implications of the 3 May 1999 tornado outbreak. *Wea. Forecasting,* **15**, 682-699.

______, ______, J. A. Hart, K. L. Elmore, and P. Markowski, 2003: Close proximity soundings within supercell environments obtained from the Rapid Update Cycle. *Wea. Forecasting,* **18**, 1243-1261.

______, ______, and C. M. Mead, 2004: An update to the supercell composite and significant tornado parameters. Preprints, *22nd Conf. on Severe Local Storms,* Hyannis, MA, Amer. Meteor. Soc., CD-ROM, P8.1

______, C. M. Mead, and R. Edwards, 2007: Effective storm-relative helicity and bulk shear in supercell thunderstorm environments. *Wea. Forecasting,* **22**, 102-115.

Trapp, R. J., G. J. Stumpf, and K. L. Manross, 2005a: A reassessment of the percentage of tornadic mesocyclones. *Wea. Forecasting*, **20**, 680-687.

______, S. A. Tessendorf, E. S. Godfrey, and H. E. Brooks, 2005b: Tornadoes from squall lines and bow echoes. Part I: Climatological distribution. *Wea. Forecasting*, **20**, 23-34.

Weber, B. L. and D. B. Wuertz, 1990: Comparison of rawinsonde and wind profiler radar measurements. *J. Atmos. Oceanic Tech*., **7**, 157-173.

Weisman, M. L., and R. J. Trapp, 2003: Low-level mesovortices within squall lines and bow echoes. Part I: Overview and dependence on environmental shear. *Mon. Wea. Rev.,* **131**, 2779–2803.

Wicker, L. J., 1996: The role of near surface wind shear on low-level mesocyclone generation and tornadoes. Preprints, *18th Conf. on Severe Local Storms*, San Francisco, CA, Amer. Meteor. Soc.,115-119.

Woodruff, R. S., 1952: Confidence intervals for medians and other position measures. *J. Amer. Stat. Assoc.*, **47**, 635-646.

______, C. M. Mead, and R. Edwards, 2007: Effective storm-relative helicity and bulk shear in supercell thunderstorm environments. *Wea. Forecasting,* **22**, 102-115.

REVIEWER COMMENTS

[Authors' responses in *blue italics.*]

REVIEWER A (John P. Monteverdi):

Initial Review:

Recommendation: Accept with Minor Revisions

A. Reviewer recommendations

The paper may be acceptable with minor revisions. (Please note…I think the revisions are mostly organizational/stylistic, depending upon how the authors are able to respond to my comments below. However, also depending upon how the authors are able to respond to my concerns, there could be more substantive research work required, in which case, my recommendation would be accept with major revisions).

Thank you for reviewing our paper and providing valuable input. You will find that many of your suggestions have been incorporated into the paper, which we now consider to be much more polished.

The authors are attempting to show that two important features of the low level hodograph that they noticed for the May 3, 1999 supercell tornado outbreak in Oklahoma really can be used to distinguish non-tornadic supercells from tornadic ones. In addition, and perhaps more importantly, these feature can also be used to distinguish between significant and non-significant supercell tornado environments. (Actually, since the significant tornado events are relatively infrequent, perhaps the true value of this work is in distinguishing between tornadic and non-tornadic supercells, at least as far as the hodograph is concerned)

Clearly, if this hypothesis is proven, it would represent and important step forward in tornado forecasting. So from the perspective of its scientific content and the significance of its results, this manuscript could represent an important contribution, and that alone would make it very publishable.

My concerns center on the presentation, which, in some places, makes it appear that the authors started with a conclusion and worked backwards to find supporting information. It was also not clear to me how they selected their "null" cases (non-tornadic supercells). By the way, both of these concerns can be fixed with "word-smithing" and just a bit more explanatory verbiage with respect to the data sources.

B. Major Concerns

My concerns summarized occurred to me as soon as I read the title and the Abstract. Once I got into the manuscript, I was expecting to read something different than that suggested by both. Wouldn't a better title be "Low-level Hodograph Characteristics as Discriminating Factors in Tornadic and Non-tornadic Thunderstorm Environments"? You are not really revisiting the hodograph, or revisiting the importance of the hodograph, but exploring something already observed about the hodograph by other authors and showing how that relates to what has already been found to be an important distinguishing characteristic between non-tornadic and tornadic supercells…the low-level shear, or 0-1 km shear. I'll have more to say on the title below.

In reading the manuscript, it's clear that the authors noticed something about the hodographs in Oklahoma on for the May 3, 1999 outbreak, and then began investigating other hodographs for other cases. Isn't that true? In short, isn't the way in which the authors cover things in Section 2.a. (Development of Hypothesis) the way they pursued and structured this study? If so, it's not true that they looked at a bunch of cases first, and then noticed the interesting geometry of the hodographs for the tornado vs. the non-tornado cases.

Yet, the Abstract makes it appear that the authors looked at many cases first, and then noticed that the lower level portion of the hodographs looked different for non-tornado events and even distinguished between non-significant and significant tornado events. The authors need to make the sequence of how they approached their work much more clear.

I think the quality of the figures is excellent. Yet I can't be sure about them until I understand how the authors approached this study. For example, in the Data Methodology section (Section 3) the authors mention data sources. You'll note that they mention a database set up by Rich Thompson and Roger Edwards, yet the reference they have there (Thompson et al. 2003) does not appear in the bibliography. Is that a data base of all tornado events in Oklahoma, or is it all thunderstorms, or all supercell thunderstorms? The authors need to give us some more background on that.

In addition, the authors mentioned Bunkers et al. (2000) as a reference for the Bunkers database. I do know that is a database of supercell events. But I think that needs to be spelled out in this section.

We have clarified what these data sources are within the text.

But what is confusing me is how the authors defined their "null" set….that is, non-tornadic cases. Were those all thunderstorm events in Oklahoma? Were those non-tornadic supercell cases? It's not at all clear to me how they got those.

All non-tornadic events occurred within Oklahoma and were drawn from the Thompson and Edwards database. We've added an appendix documenting this and other important details about our dataset.

In short, section 3a needs to be revised to be more explicit. In addition, I would suggest breaking out section 3a into two parts…one for the databases that were used to define "events" and the other for the data bases used for the production of hodographs. Section 3a is just too terse, not informative enough.

We have implemented this suggestion, and added a figure showing the site locations for each data reporting network used in the analysis.

This is why I can't comment on whether the inclusion/selection of all the figures/cases is appropriate. I'd like to have more information (and the reader would too, of course) about the bases for their selection.

Since these concerns are significant, I am going to release myself from the task of going through the manuscript line by line to comment on English style/syntax. If the authors are going to go through and reorganize things, I'd rather wait to spend the effort on their next draft.

All of that said, I must say that this is a very interesting and convincing presentation. Yes, the arguments the authors make about the correspondence of the geometry of the low-level hodograph to the ability of storms to "ingest" air in a sort of storm-relative Beltrami flow is intriguing. The comparison of the sounding/hodographs for the May 3, 1999 and the April 23, 2003 cases is really excellent.

[Suggested stylistic Abstract revision omitted…]

The authors state that "…Two features long associated with the occurrence of tornadoes included a veering wind profile with height…" They mean to say

"…Two features long associated with the occurrence of tornadoes included a veering wind shear profile in the lowest 3 km…"

Remember, that the wind veers with height even in straight hodograph geometries, but it's the veer of the wind shear vector that is associated with the pressure perturbation forces connected to the linear term (pressure perturbation proportional to the wind shear dotted with the buoyancy vertical velocity).

This is true, but in this context we are referencing Fawbush and Miller (1954), who were unaware of these specifics.

Substantive Comments:

In reading the manuscript, it's clear that the authors noticed something about the hodographs in Oklahoma on for the May 3, 1999 outbreak, and then began investigating other hodographs for other cases. Isn't that true? In short, isn't the way in which the authors cover things in Section 2.a. (Development of Hypothesis) the way they pursued and structured this study? If so, it's not true that they looked at a bunch of cases first, and then noticed the interesting geometry of the hodographs for the tornado vs. the non-tornado cases.

Yet, the Abstract makes it appear that the authors looked at many cases first, and then noticed that the lower level portion of the hodographs looked different for non-tornado events and even distinguished between non-significant and significant tornado events. The authors need to make the sequence of how they approached their work much more clearly.

Actually, there is some truth in both of these statements. We looked at many cases while developing these ideas, but used May 3rd as a kind of "benchmark". Our reasoning was that whatever we were looking for had to be present in that case based on what happened that day, so we compared it to all other cases we deemed "interesting" as we sought to detect common patterns/features. Evidently some of this ambiguity was translated into the text of the article. We have attempted to clarify this in the text without getting too sidetracked into explaining our thought process during the initial investigative phase, which was hardly marked by rigorous analysis.

All of that said, I must say that this is a very interesting and convincing presentation. Yes, the arguments the authors make about the correspondence of the geometry of the low-level hodograph to the ability of storms to "ingest" air in a sort of storm-relative Beltrami flow is intriguing. The comparison of the sounding/hodographs for the May 3, 1999 and the April 23, 2003 cases is really excellent.

We had considered mentioning the fact the flow is roughly Beltrami. Perhaps this has the effect of allowing vorticity to build/intensify as it is ingested into the storm without the usual dissipation that occurs via crosswise advection and associated turbulence…? We elected to omit it as we do not have a strong enough background in dynamics to argue this point.

Historical cases:
Well, this is an example of the major problem I outlined in Section B. How did you define or select the criteria for the historical cases? It's sort of obvious, I realize, but you need to rigorously discuss this.

There were no criteria other than that they at minimum appeared to show the combination of features we identified in reasonably diverse situations, and that the data appeared sound. The "best" historical cases conclusively show these features, instead of merely suggesting them. We do not see why this should be rigorously discussed in the text, as these soundings are not part of our objective analysis.

[Minor comments omitted...]

Second review:

A. Reviewer recommendations

The authors have made a forthright attempt to address all my major concerns. I recommend: Acceptance Pending Minor Revisions.

B. Remaining Major Concerns

The authors really have not addressed the core of my problem with their definition of "historical cases." The issue for me is simply this....one can pick and choose cases that appear to support a hypothesis. The reader needs to be assured that there weren't thousands of cases, to exaggerate, that didn't appear to support the hypothesis.

The way the authors responded to this concern, as quoted below,

"...There were no criteria other than that they at minimum appeared to show the combination of features we identified in reasonably diverse situations, and that the data appeared sound. The "best" historical cases conclusively show these features, instead of merely suggesting them. We do not see why this should be rigorously discussed in the text, as these soundings are not part of our objective analysis...."

…troubles me. The point is, to me, that the authors have gone out of their way to use the term "historical cases", which suggests that they have done a survey of past cases. What they are saying is that they've selected a case from the past that supports their contentions or their hypotheses. They need to reword and carefully explain this, no matter if the soundings were not used as part of their objective analyses.

We have attempted to address this by briefly summarizing our investigation of historical cases at the beginning of that section, and hope this explanation is sufficient.

[Minor comments omitted...]

The authors responded in an unsatisfactory way to my previous comment that it is the veer of the wind shear vector that is pertinent to understanding a hodograph favorable for a right moving tornadic supercell. They state:

"...This is true, but in this context we are referencing Fawbush and Miller (1954), who were unaware of these specifics....

The issue in context is not what early researchers on the relation of the hodograph to propensity for a storm to be tornadic or a supercell, but what is known now that will help us understand how the authors' interpretation of hodograph geometry is consistent with the present state of the knowledge. The authors need to change this sentence (assuming that it remained in the second version of the manuscript).

We initially misinterpreted this comment, and believe we have now corrected this issue.

REVIEWER B (Dan J. Miller):

Initial Review:

Reviewer recommendation: Manuscript is acceptable with major revisions

We appreciate the time and effort you spent reviewing our paper and have attempted to address each issue raised as thoroughly as possible. You will find that many of your suggestions have been incorporated into the paper, which we now consider to be much more polished, although we do disagree with some of your points.

[Minor comments omitted...]

What kind of vertical resolution to winds are you getting from the NPN? Did you use 6 second archived UA data – or the operationally archived data? Could also look at base velocity display from KTLX to ascertain how quickly winds increased above the surface – thus providing further evidence that the strong increase in speed in the lowest few hundred meters was not an artifact of data sampling.

For the NPN data, the first gate is located at 500 m, with two gates spaced 250 apart thereafter—we used no data above 1 km. The archived UA data were not 6 sec data, but standard archived data, and we have checked the base velocity display from KTLX and found it to be consistent with the wind speed increase found in the sounding.

With all due respect to Matt Bunkers, it is exceptionally dangerous to use the Bunkers storm motion as a proxy for observed storm motion. Operational experience dictates that observed storm motions are often quite variable, even within the same "environment" (ex: 3 May 1999). The authors implicitly admit this…

We now use the Bunkers storm motion for only one historical case, instead of two as before. Also, to reflect these concerns, we have moved the footnote discussing the Bunkers storm motion into the body of the text.

This is a fascinating topic, and despite the additional work that is required on this manuscript, I strongly encourage the authors to continue work on this topic and publish this work.

Substantive Comments:

Title of the paper: This really isn't a "new" topic. There have been at least 2 other works (Miller 2006 and Wicker 1996) that have discussed near-surface shear. Also, the authors' definition of "storm classes" is nebulous throughout the paper. Suggest re-wording the title to more accurately represent what the content of the paper is – a detailed examination of near-surface shear in relation to the potential for tornadogenesis, or tornadogenesis failure.

We respectfully disagree and believe this is a new topic, as no one has examined how environmental low-level straight-line hodographs relate to storm motion. Nevertheless, we have altered the title to more explicitly state what the study is about, as we agree the title was vague. Also, "storm classes" are now clearly defined early in both the abstract and main body of text, which should mitigate concerns over this previously ambiguous terminology.

Abstract: 2nd paragraph again mentions "storm classes" without much in the way of an explicit definition of what the storm classes are. It is implied that the 3 storm classes are all supercells, one class that is non-tornadic, and second which is weakly tornadic and a third that is strongly tornadic. Would suggest re-wording in the abstract and somewhere near the beginning of the paper to explicitly state what those definitions are. Also, the "kink" in the hodograph trace is seldom right at 500 m AGL, in many instances (especially in the Miller, 2006 dataset) the height AGL of the kink was significantly below 500 m AGL (mean height in Miller, 2006 was around 400 m AGL) – although 500 m is a nice clean number – suggest that the authors re-word the abstract to reflect the variability in height AGL of the near-surface straight line hodograph. Finally, since by convention 10 m wind is surface wind, simply say surface-XX m AGL, 10 m and surface are basically synonymous in operations.

The ambiguous "storm classes" issue should be resolved.

It is true that the "kink" is seldom exactly at 500 m AGL, though we are unconvinced it is usually below that level (we will address this in detail in response to a later comment). However, it should be noted that the hypothesis section of the abstract is not referring to specific observed hodographs, but rather to presumed "favorable" features, and that the results section of the abstract is constrained by the fact that the analysis used only the 10 m and 500 m observations.

We grant that by convention the 10 m wind is referred to as the surface wind. However, we prefer to specify "10 m".

5) Introduction: It is strongly recommended that the Miller 2006 paper be given a much more thorough reading and reference in this manuscript. Many of the concepts herein have already been examined in Miller 2006, and this work deserves much more than a footnote. Same paragraph, next to last sentence – the near-surface straight line hodograph trace does not always result from pure (or nearly pure) speed shear. Miller 2006 has several cases of near-surface straight line hodographs where the entire sickle shape of the hodograph trace was either rotated, in a different quadrant of the hodograph, or both, which still resulted in a straight line hodograph, but winds were not unidirectional through the same layer. Finally, same paragraph, suggest re-wording of last sentence to talk about the orientation of the 10m-kink height hodograph trace to the storm motion vector.

We have included several citations of Miller (2006) in the main body of the text and changed "documented" to "identified".

With regard to a combination of speed/directional shear producing a straight line hodograph, we do not deny this is possible and have seen instances where this appears to definitively occur. However, we are unsure which soundings might be referred to here, as Miller (2006) does not specify which RAOB locations were used for the analysis. The only example showing both speed and directional shear below the kink in Miller (2006) is Fig. 5, but no claim can be made that a straight line is present below the kink because it appears that only two data points make up the line. Further, this lack of additional data points below the kink appears to be true of all hodograph figures in Miller (2006), and nowhere in the text of the paper was the low-level hodograph postulated to be a straight line.

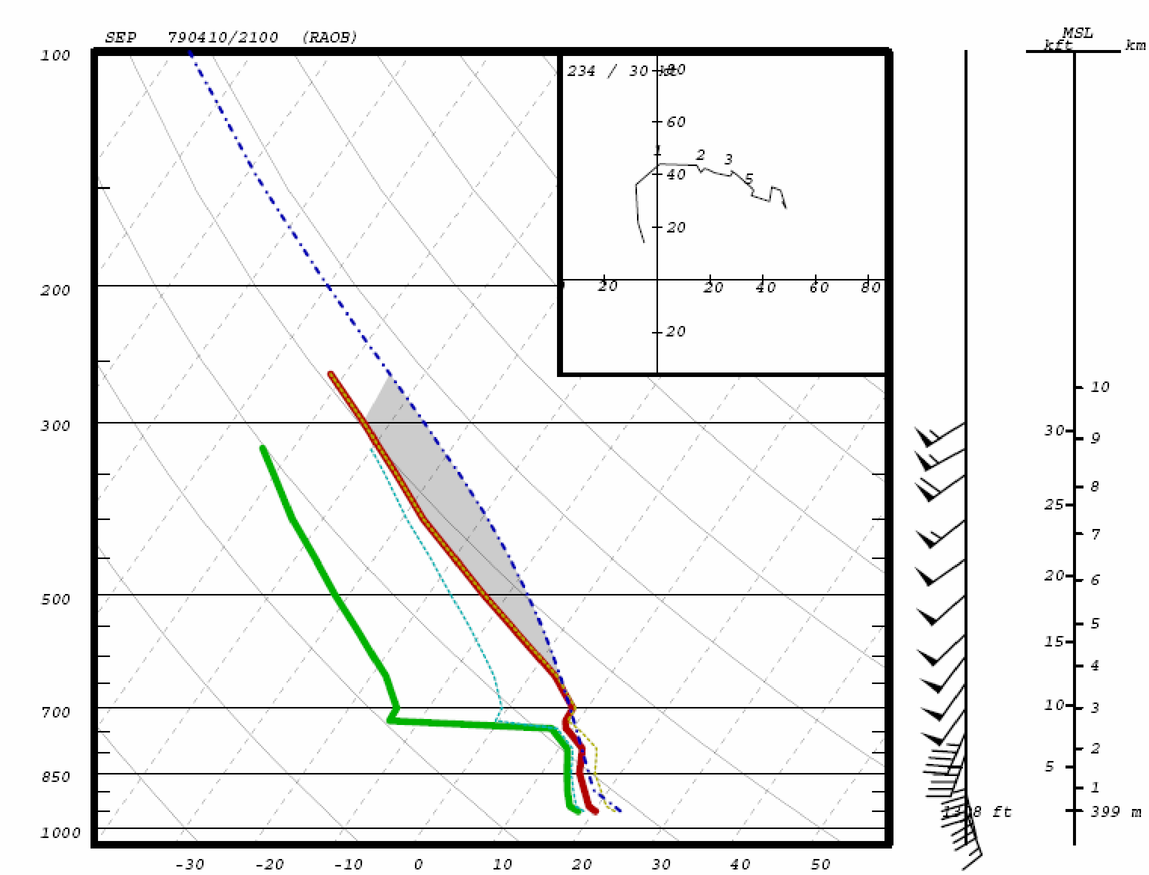
Also, by way of clarification concerning the orientation of the sfc-kink hodograph trace, we are less interested in the height of the kink and more interested in the amount of shear present below approximately 500 m. We discuss this in more detail in response to a later comment.

Section 2, part b: It could also easily be argued based on considerable operational experience that the weak mid level SR winds and the faster and more rightward movement of the storms on 23 April, are tell-tale signs of HP supercells, of which the updraft propagation is being driven by cold outflow at the surface. There is also a considerable difference in the near-surface parcel thermodynamics – the 23 April case is much cooler in a very shallow near-surface layer. In order for the storm to realize the shear – a majority of the inflow parcels should be originating from the layer where the shear is. It is this reviewers opinion that a strong case can be made that the 23 April storms were slightly elevated and HP in nature, and therefore much less likely to produce significant tornadoes – regardless of the near surface shear or hodograph trace.

Although not 100% conclusive, there is strong evidence from the KFWS WSR88-D data that the storms were not HP in nature, as they displayed a classic reflectivity structure and lacked the "fine line" reflectivity structures commonly associated with the flanking line of an HP supercell, despite being in close proximity to the radar.

With respect to weak mid-level flow, we have removed this wording as the RAOB hodograph did not evidence this weakness and another reviewer pointed out that the weakness on the KFWS VWP relative to the sounding could almost entirely be accounted for by instrument error (NOTE: we have modified this comparison to use the KFWD RAOB at this reviewer's suggestion). Furthermore, even if we had not removed this wording, we would like to point out that the 22 April 1985 hodograph would be expected to strongly favor non/weak-tornadic HP supercells on the same grounds and yet large, significant tornadoes occurred in very close proximity to the sounding in both space and time.

To address the thermodynamic concerns, we would like to note that by the time of the Mulhall, OK, tornado of 03 May 1999, surface temperatures had cooled to 21-22 C for Oklahoma Mesonet stations in the vicinity of Mulhall. Even when corrected for differences in elevation, these values are not all that different from the 20-21 C observed near Fort Worth on 23 April 2003. Further, we would like to compare both of these skew-T diagrams to KSEP 2100 UTC 10 April 1979 (the infamous Red River outbreak, figure at end of paragraph). This sounding could be argued to be the one most conducive to elevated supercells of the three, as is has a deep layer of poor lapse rates below 800 mb, which is also the level at which the most unstable parcel resides (not shown) according to NSHARP.



THERMODYNAMIC PARAMETERS

MEAN MIXING LAYER PARCEL

LPL:	952mb	21C/ 17C		70F/ 62F	
CAPE:	1447 J/kg	LI:	-6 C @	500mb	
BFZL:	16 J/kg	LImin:	-0 C @	650mb	
CINH:	-65 J/kg	CAP:	2 C @	789mb	

LEVEL	PRES	HGT (AGL)	TEMP
LCL	894mb	1794ft	
LFC	650mb	10512ft	4 C
EL	M	M	M
MPL	699mb	8559ft	

Precip Water:	1.11 in	Mean RH:	41 %
Mean Q:	12.7 g/kg	Mean LRH:	87 %
Top of Moist Layer:		744 mb / 6871 ft	

700-500mb Lapse Rate:	21 C / 8.1 C/km
850-500mb Lapse Rate:	30 C / 6.9 C/km

Total Totals:	54	K-Index:	19
SWEAT Index:	515	Max Temp:	79 F
ThetaE Diff:	24 C	*Conv Temp:	72 F
FRZ Level:	12272 ft	WBZ Level:	7467 ft

Output produced by Unidata postscript driver:
NSHARP (SkewT-Hodograph Analysis and Research Program)

KINEMATIC PARAMETERS

Sfc - 6 km Mean Wind:	221 / 49 kt	(25 m/s)
LFC - EL Mean Wind:		M / M
850 - 300 Mean Wind:	216 / 47 kt	(24 m/s)
Sfc - 2km Shear:	25 kt	(12 m/s)
Sfc - 6km Shear:	47 kt	(24 m/s)
*BRN Shear:		91 m2/s2

STORM STRUCTURE PARAMETERS

Sfc - 3km SREH:	468 m2/s2		
Effective SREH:	468 m2/s2	from 0 m.	
0-2 km SRW:	29 kt	EHI:	4.2
4-6 km SRW:	20 kt	BRN:	16
6-10 km SRW:	M		

[Former version] Fig. 3: I assume these are the same hodographs that are in the upper right hand corner of Fig. 2. So, why do the hodographs in Fig. 3 look different from those in Fig. 2 – especially the 23 April one? There are also 2 storm motions for 23 April plotted on the hodograph – why not plot all the sigtor storm motions from 3 May 1999 on the hodograph as well?

We have removed the hodographs from the upper right hand corner. Plotting all the storm motions from 03 May 1999, although interesting, is not a valid comparison as many of them occurred considerably later that evening.

[Former] Fig. 4: In the instance of a tropical cyclone, I think that rotating the hodograph trace is far too simplistic a methodology to estimate a wind profile.

We have removed this case as per reviewer consensus.

[Former] Fig. 8: what about KTLX VWP and Purcell profiler data?

Although the KTLX VWP showed a curved low-level hodograph rapidly transitioning to a straight-line hodograph around 0330 to 0400 UTC (possibly supporting our hypothesis), it appeared that the profile had problems with convective contamination since the storm's inflow was very strong over a large area just northwest of KTLX. Therefore, we elected not to use this source due to data quality concerns. The Purcell profiler data were QC'd out (this seems to be a fairly common occurrence when the nocturnal LLJ strengthens) and are not available as raw text from the NPN website. [This now refers to Fig. 7]

Part d: Yes, this feature has been documented already in Miller 2006, the difference between your manuscript and that is merely semantics. Miller 2006 also makes note of how this affects many popular near-storm environment parameters.

We respectfully and strongly disagree that the only difference between Miller (2006) and this manuscript is "merely semantics". Miller (2006) did not identify the portion of the hodograph below the "kink" as potentially being a straight line, nor suggest that such a straight line hodograph may be normal to the storm relative inflow vector. These key elements of our hypothesis can hardly be described as being simply "semantics".

Nevertheless, to reflect this concern we have changed "feature" to "combination of features" and substituted "identified" for "documented".

There are major problems with using the 10-500 m shear vector, if the authors are trying to evaluate only the straight line near-surface sector of the hodograph, the layer is going to be variable – the kink in the hodograph is not always at 500 m AGL. Suggest a methodology as in Miller 2006, looking at surface-kink shear vector, and then evaluate the orientation related to storm motion/inflow etc. Was the inflow vector calculated from observed 88D motion or from Bunkers?

We are well aware that the height of the kink is variable. Unfortunately, the lowest gate for the NPN profilers is 500 m, and we found the other two alternative data sources (RAOB and VWP observations) to be undesirable. In the case of RAOB, this was due to the very poor spatial and temporal resolution of the data—the spatial and especially temporal resolution of NPN profilers is much superior. To illustrate why this is a problem, several years ago we performed a preliminary study using proximity RAOBs that yielded poor results, even when restricting it to F5 cases. Subsequently, we were able to link this failure to poor spatial and temporal resolution upon comparing the RAOBs to wind profiler data for recent events in the Plains. In the case of VWP, we elected not to use this data source because the convection we are seeking commonly contaminates the data and we did not feel that we could properly select cases without either introducing an unacceptable amount of subjectivity into the process or severely complicating the analysis. By contrast, since NPN wind profilers use a much higher elevation angle than an 88D is capable of, a storm would have to be in extremely close proximity to the profiler site in order to affect the near-surface wind observations. Also, higher resolution operational VWP data have only become available in the last few years since the introduction of the embedded alphanumeric block within the NEXRAD Level III NVW file (for more details see the ICD for Open RPG for Class 1 User, dated 29 December 2002, document number 2620001E), so the vertical resolution would actually have been degraded by using VWP, although the first gate would have been at least 200 m lower. Our conclusion was (and is) that sacrificing some low-level vertical resolution when compared with the sounding and temporal resolution when compared with *the VWP is an acceptable compromise in exchange for the increased data reliability and decreased analytical complexity offered by the NPN, particularly for such an initially speculative study.*

If the kink is not at 500 m AGL, the SRH is going to have little to do with the sfc-500 m shear vector as SRH is determined by the area under the hodograph from the observed storm motion.

Even if the kink is not at precisely 500 m, the 10-500 m SRH will still be strongly correlated to the sfc-kink SRH. Regardless, in this study there are only three data points used to calculate the 10-500 m SRH. We do not see a problem with this...

Further research questions: part b is addressed in Miller 2006, as is much of part c. In fact, Miller, 2006 calls for a comprehensive study of the operational UA database to look for "sickle" shaped hodographs, to see how common they are and how often they are associated with tornado events.

We presume that the statement "part b is addressed in Miller (2006)" refers to Question 4. With all due respect, we strongly disagree with this assessment. Question 4 from Miller (2006) examined how a highly sheared, non-well mixed boundary layer could coexist with strong surface heating. Part 6b in this manuscript is instead concerned with the response of the boundary layer to the diminishing of surface heating, and how a well-mixed boundary layer with little near-surface shear may rapidly transform into a highly sheared near-surface environment as mixing ceases. These are two very different questions.

We also disagree with the assessment that much of part c is covered by Miller (2006). We are not concerned with "sickle" shaped or "kinked" hodographs per se. Our only concern is whether or not an approximate straight-line hodograph in approximately the lowest 500 m is oriented normal to the 10-m storm-relative inflow. If this configuration is produced on a hodograph that is not "sickle" shaped or "kinked", our hypothesis suggests the effect should be the same. Thus a similar search of the operational UA database based on the premise of this manuscript would instead search for strong straight-line hodographs below ~500 m without necessarily focusing on the "kink".

Miller, 2006, addressed the issue of the mean height agl, of the hodograph kink. I believe the authors' selection of sfc-500m severely oversimplifies the problem.

Based on our experience examining seemingly innumerable vertical wind profiles from numerous data sources (including operational RAOB), we think the height of the kink on these data sources is probably higher than that reported in Miller (2006). Thus we do not consider the use of 10-500 m to be an oversimplification and are of the opinion that our results are evidence of this. Further, close examination of the cases used in Miller (2006) reveals several problems with the analysis which lead us to question the results, particularly in relation to the kink:

1. Fig. 4 from Miller (2006) is said to be from KPIT at 0000 UTC 01 June 1985. We examined the sounding from this date, time and location using two separate data sources (NSSL GEMPAK archive and University of Wyoming online archive) and found that neither the thermodynamic nor kinematic profiles match the ones shown in Fig. 4. Both of the data sources we used do exhibit a weak kink, however the kink height on both resides between approximately 560 and 860 m instead of the 400 m indicated in Miller (2006) Fig. 4. We recognize that it is entirely possible that the sounding from Miller (2006) is in fact the correct sounding for this date and time, but problems such as this exemplify why we restricted our use of RAOB to the initial development phase of our study and did not use it for the objective analysis, as the RAOB archive is unfortunately a minefield of such issues.

2. At least one, and possibly more, of the cases in the Miller (2006) dataset may have occurred prior to the MiniART upgrade to the NWS RAOB network in the mid-1980s. Soundings prior to this upgrade must be used with extreme caution when examining low-level wind profiles as the only mandatory levels at the time below 500 mb were 1000, 850, 700 mb, and one additional level somewhere < 20 mb AGL (see FMH-4, 1976, Chapter B2 – Levels to be reported, and FMH-3, 1981, Chapter B4 – Release and recorder record, pp. 11-21). Perhaps even more importantly, significant levels were only required to be reported for substantial changes in thermodynamics and not wind—in fact, the very inclusion of significant levels even *for thermodynamic changes was discouraged due to the constraints of the communications infrastructure. We are uncertain what data archive Miller (2006) used for the analysis, but it is interesting to note that the University of Wyoming's archived Nashville, TN, sounding from 0000 UTC 04 April 1974 appears to conform to the reporting specifications instituted after the MiniART upgrade by including fixed wind levels at 1, 2, 3 kft,… above mean sea level (FMH-3, 1997, Chapter 5 – Processing sounding data). This conformance should be regarded as highly suspect, as it is distinctly possible this sounding was later interpolated to the new reporting requirements. To illustrate this, the first few levels reported in the University of Wyoming sounding for Nashville are presented below, with suspect levels marked by an asterisk:*

	PRES	HGHT	TEMP	DWPT	RELH	MIXR	DRCT	SKNT	THTA	THTE	THTV
	hPa	m	C	C	%	g/kg	deg	knot	K	K	K
1.	977.0	210	22.0	18.9	83	14.27	180	12	297.1	338.6	299.7
2. *	966.2	304	22.8	19.1	79	14.60	190	19	298.9	341.6	301.5
3.	959.0	368	23.4	19.2	77	14.83	192	23	300.1	343.7	302.8
4. *	932.1	609	22.6	16.3	68	12.66	200	38	301.8	339.3	304.1
5. *	899.1	914	21.6	12.6	57	10.30	205	41	303.9	334.8	305.7
6.	899.0	915	21.6	12.6	57	10.29	205	41	303.9	334.8	305.7
7. *	867.0	1219	19.1	12.1	64	10.32	220	42	304.5	335.6	306.4
8.	850.0	1385	17.8	11.8	68	10.33	220	44	304.8	336.0	306.7
9. *	807.0	1828	15.1	11.2	78	10.47	220	51	306.4	338.2	308.4

Highly suspect data points are present at 304, 609, 914, 1219 and 1828 m MSL as they all fall at the fixed wind reporting levels 1, 2, 3, 4, and 6 kft; these levels were instituted with the MiniART upgrade and were not required to be reported before then. In particular, the 899.1 mb data point lends credence to the idea these points are suspect, as a sounding recorded by hand would not contain two data points 0.1 mb apart—in fact, it is not even certain this sounding was taken by an automated tracking system, as WMO Instruments and Observing Methods Report No. 50 – Historical Changes in Radiosonde Instruments and Practices (1993—see page 86) lists 1974 as the year the original ART system was implemented by the NWS. The rest of the data points are probably valid, as the data point at 959 mb is < 20 mb AGL, the point at 899 [mb] exhibits a large change in dewpoint temperature and likely satisfies the criteria for selecting a significant level, and the point at 850 mb is a mandatory level. Considering only these three data points and after adjusting their altitudes to AGL, a kink height between approximately 710 and 1180 m can be obtained—much higher than the 400 m that can be calculated otherwise that coincides with the median reported by Miller (2006).

Again, in our judgment problems with the RAOB archive such as this only serve to reinforce our decision to use an alternative upper air data source for our analysis, although the very poor temporal resolution of RAOBs was the biggest factor in our decision to use NPN data instead.

3. It is impossible to reproduce the results, as the specific sounding sites used to calculate the statistics are not presented, nor the method used to determine the height of the kink, though it appears to be assigned to the point below the one that is distinctly above the kink. Although a good working approximation, this cannot be definitively stated to be the height of the kink in the absence of additional data points.

Second review:

Reviewer recommendation: Manuscript is acceptable with major revisions

Thank you for your latest comments. … We partially agree with your conclusions in the 23 April case, especially later on in the life cycle of the storms, and appreciate the effort you put into it.

Also, we want to emphasize that we do wish to give credit where credit is due, and are not trying to undervalue either T & E (2000) or Miller (2006). As such, we have carefully reworded a couple passages to zero in on the primary focus of this paper, which is the combination of features that creates the critical angle. We still disagree that T & E (2000) and Miller (2006) identified the segment of the hodograph below the kink as a straight line, but have nevertheless removed all references to our identifying this feature as they are not necessary to our scientific presentation.

[Minor comments omitted...]

Leading up to the writing of Miller, 2006, I had numerous in-depth discussions with many researchers and operational meteorologists about the appearance of the low level straight line hodograph and whether or not it was real, or simply an artifact of data sampling, which is why I raised question about base velocity and 6 second data in the initial review (you also raise this same “two data point” issue in your paper several times). Looking at both the 6 second data and radar velocity data, I believe, can greatly increase confidence as to whether or not the feature is real (I believe that it is in almost every instance I have seen), or merely an artifact of limited sampling through a shallow layer. I concur with your findings of the KTLX velocity data being consistent with the sounding data, but you make no mention of this agreement with the radar velocity data in your revised manuscript – I think a few sentences to explicitly state this agreement would go a long way to increasing the reader confidence that the feature is real, and allow you to shorten up section a) of Part 2). I think it is also worthy to note that we are never going to know *exactly* what the low level shear profile looked like (presuming that we can make a distinction as to where the “near-storm” environment stops, and the “storm” environment begins anyway). From an operational forecast point of view, you have to pick something you think is representative and go with it, and I think you’ve done that to my satisfaction.

In addition, I also still do not agree with the authors’ assessment that the “kink” or interface between primarily speed vs. directional shear usually occurs above 500m AGL. (there are several more comments regarding this topic below). Therefore, I believe that it is questionable, at best, to use any data from an observation system that has no data points between the surface and 500m AGL. I simply do not think that the NPN data has sufficient near-surface (I choose to define near-surface as below 500m AGL) vertical resolution to be very useful for purposes of diagnosing the near –surface shear adequately. If NPN had another gate at 250m AGL, it would be much more useful.

We define the height of the kink as specifically being the level at which directional shear begins, which we have often found to be at or above 500 m. However, in nearly all of these instances, the layer of strong speed shear is entirely contained within the 10-500 m layer (for example, Fig. 4). In other words, there appears to commonly be a layer where the ground-relative wind is nearly constant between the region where speed shear ends and directional shear begins, and in our experience 500 m usually falls either within or very close to this layer. Even if this is an artifact of the various observing networks, since we are binning our results for the critical angle it should have little impact on our analysis. Therefore, although we agree it would be beneficial for the NPN data to have a 250 m level, we do not regard it as necessary.

The “storm class” ambiguity has been resolved to my satisfaction.

I think you may have misinterpreted what I meant by stating that this isn’t a new topic. It is a fact that this particular hodograph feature has been noted, or discussed in detail in 3 prior papers: Wicker 1996, Thompson and Edwards, 2000 (I forgot to include this one in my initial review comments), and Miller, 2006. Although perhaps not explicitly stated in any of those works (I know Miller 2006 made no *specific* reference to a “near-surface straight line hodograph”), but all 3 of those papers clearly showed illustrations of the specific low level hodograph trace in question, and it is self-evident to the reader when reading any of those papers that the low level hodograph is nearly a straight line. (As both I and you have mentioned, there is a question whether that feature is real or an artifact of vertically deficient sampling, but I also have examined enough base velocity displays from 88Ds over the past 7 years that I feel rather confident in stating that I think the feature *is* real, and I think that without using 6 second UA data you are never going to get the vertical resolution that you need.)

We did not initially cite Wicker (1996) as it does not note or discuss in detail straight-line hodograph structure or imply it in any way. Wicker (1996) investigated the possible impact of near-surface shear on tornadogenesis when environmental barotropic vorticity is aligned with versus normal to baroclinic vorticity generated along the forward flank of a modeled supercell. However, because he dealt with strong near-surface shear, we have added a reference to this article in our literature review.

With respect to the existence of the low-level straight-line hodograph in the literature, we think that suggesting this is self-evident from hodographs presented in Miller (2006) or any other article is highly dubious. A quick 30-minute check of a file folder of relevant literature yielded 11 papers other than T & E (2000) and Miller (2006) containing figures with sickle-shaped, kinked, and/or low-level nearly straight line hodograph structure, 8 of which used them specifically in reference to tornadic storms. Many of these hodographs also show storm-relative inflow that is normal to the low-level hodograph, and not one of these papers suggests the low-level hodograph is straight (in fact, some purposely curve it) nor points out that the storm-relative inflow is normal to the low-level hodograph (there is no reason to do so if one considers that part of the hodograph to be curved). Clearly, these features are not self-evident, and below are these eight references that use hodographs exhibiting some or all of these features in relation to tornadic storms:

Davies-Jones, 1983: The Onset of Rotation in Thunderstorms (Figs. 5, 7)
Davies-Jones, 1984: Streamwise Vorticity: The Origin… (Figs. 10, 13)
McCaul, 1987: Observations of the Hurricane "Danny"… (Fig. 6)
Davies-Jones and Burgess, 1990: Test of Helicity as a Tornado Forecast Parameter (Figs. 1-4)
Davies et al., 1994: Some Noteworthy Aspects of the Hesston, Kansas,… (Fig. 3)
Brooks et al., 1994: The Role of Midtropospheric Winds… (Fig. 10)
Hanstrum et al., 2002: The Cool-Season Tornadoes… (Figs. 3, 10)
Monteverdi et al., 2003: Shear Parameter Thresholds for… (Fig. 3)

I didn't realize that the specific focus of the paper was the relation of low level straight line hodographs to storm motion – perhaps because I was rather confused about your use of the term "storm classes" in the initial manuscript. To me, your investigation is really not "relation of low level straight line hodographs to storm motion", it's the relation of low level straight line hodographs and observed storm motion to tornadic potential.

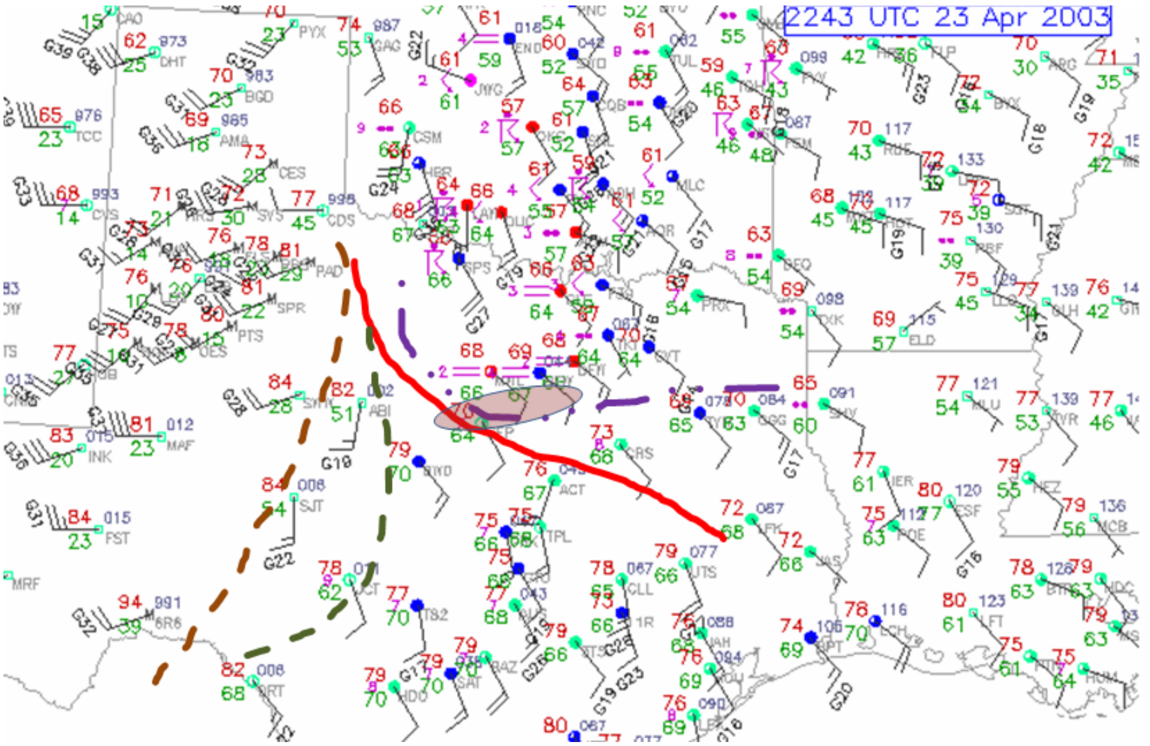
This is correct.

Isn't the depth of the low level straight line hodograph (and therefore the depth of the near-surface layer that is strongly sheared primarily in a speed sense) important? I strongly believe that it is, and if so, then the height of the kink is important.

See part 6.c of our paper.

I must respectfully and strongly disagree with the authors' assessment and evaluation of the radar data for this event. While supercell storm classification to the LP/CL/HP regime is highly subjective between visual observer to visual observer and also between visual observer to radar, I am of the opinion that that radar data is 1) the better tool to use (vs. the human observer since many hydrometeor cascades from storms can be translucent or nearly transparent) and 2) there is strong evidence of HP or even hybrid HP/bow echo structure in the KFWS volumetric reflectivity/velocity data.

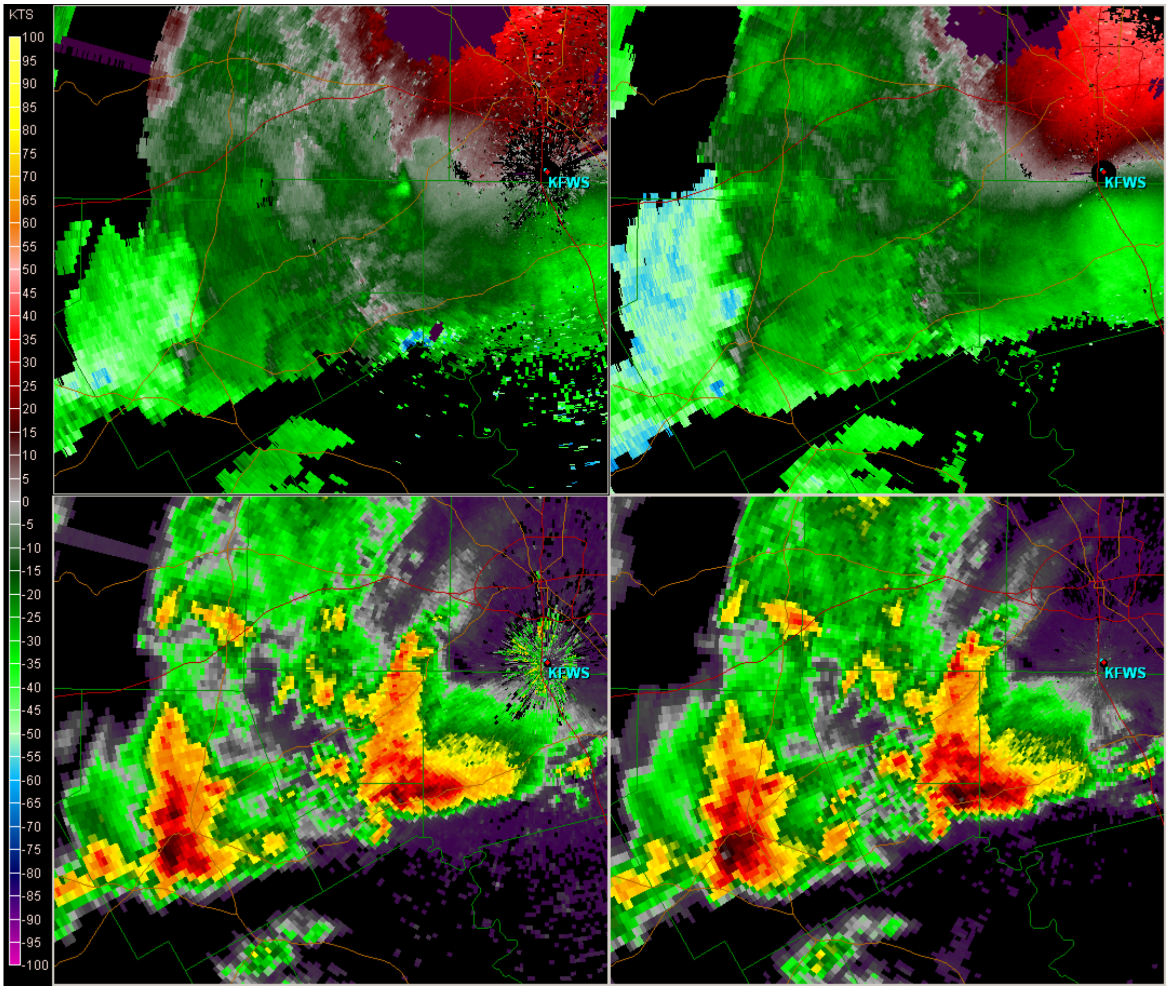
In addition, my investigation of the radar data led me to take a closer look at the larger scale environment, and after doing so, I find myself to be in even stronger disagreement with the authors regarding their overall assessment of this event regarding the overall synoptic and mesoscale environment. Although it is impossible to say for sure, I am convinced even stronger than I was on the first review that these storms were elevated above a shallow cool layer near the surface – the one evident on the FTW sounding in Fig. 2. I would also very strongly caution the authors about using the 00Z FTW sounding at all as a representative sounding for the storms in this case, as the surface data clearly indicated some type of subtle outflow boundary/warm front combination just south of the DFW metroplex at 23 UTC:



Dashed purple line = outflow boundary, Red line = synoptic scale warm front, brown dashed line = current day dynamic dryline, dark green dashed line = residual dryline from day before, maroon transparent area = area where storms in question were located.

It is exceptionally important to note that the FWD sounding location is located just to the west of site DFW, and that the balloon is actually released at 23z for a 00z observation. Thus comparing surface observations and low level winds (sampled much closer to the release time) to the lower levels of the sounding data at 23z is more valid than using 00z surface information. The very shallow near-surface cool layer noted in Fig. 2 on the FWD sounding makes complete sense when viewed in this context, as the low level cool air was also still being slightly reinforced by ongoing elevated convection to the north, northeast and east of FWD at this time – per radar imagery (not shown). The storms in question initiated in the axis of warmer air (and presumably greater instability and deeper mixing) to the west, and then moved across the boundary complex and became slightly elevated. (Important note: despite one of the more widely known VORTEX cases being an instance where storms crossed a boundary set up similar to the one in this case and then producing strong/violent tornadoes just on the cool side of the boundary, operational experience rather strongly suggests that it is more common for storms to cross a boundary set up like this and become slightly elevated on the cool side of the boundary – most often because the very lowest layers on the cool side of such boundaries are just cold enough to sufficiently limit what this reviewer terms truly surface based convection – i.e. a majority of the parcels ascending into the primary updrafts originated from within 400-600 m of the ground. The shallow near-surface cold layer observed on the FWD sounding strongly supports this idea.)

Based on this assessment of the synoptic and mesoscale environment and using GrLevel2 and archived level 2 radar data from NCDC, I have examined in detail the observed storm structure between 00-02z 4/24 (7-9 PM CDT 4/23) 2004, and I believe that there is ample evidence of HP structure for the two storms evident to the southwest of the city of Fort Worth, the eastern storm affecting the corridor from Glen Rose to Cleburne, and the western or “second” storm that affected the Stephenville area. This assessment is based on the presence of several signatures, that have strongly been associated with HP structure based on 10+ years of operational experience, and discussions with other expert warning forecasters who have considerable experience and expertise using radar data (I have consulted one such person while writing this review and they agreed with my conclusions):



Lower left: 0.5 Z, lower right: 1.5 Z, upper left: 0.5 V, upper right: 1.5 V all from KFWS 0102z 4/24/2003.

1) Consistent multiple reflectivity cores aloft, which is an indication of multiple updraft cores and a hybrid multi-cell/HP supercell structure

2) The absence of a well-defined or temporally consistent BWER with either storm.

3) At the 0.5/1.5º elevation slices, both storms exhibit an anticyclonic hook shaped appendage in reflectivity data, which is consistent with large amounts of hydrometeors on the upshear flank of the main updraft, and is also consistent with visual images of most HP supercells, where the RFD area is completely filled with hydrometeors, and the leading edge of which takes on a convex bowing structure when visually observed from the inflow side of the storm.

4) At the 0.5/1.5º elevation slices the western storm exhibits a large and fast moving outflow boundary in base velocity data. The storm motion also accelerates to ~255º 37 kt (much faster than the ~255º 25 kt motion for the eastern storm), and both of these signatures are solidly consistent with a strong low level outflow/RFD cold pool that is driving storm propagation and new updraft development much faster than would be expected based on rough estimates of storm motion calculated from a hodograph. The fact that this outflow boundary is able to be sampled at 4,000 to 5,000 ft AGL (assuming standard refraction), also indicates that the cold pool has substantial depth.

5) The eastern storm, while having a motion that is much more consistent with a discrete propagation motion for supercells in this environment, also exhibits an increasingly disorganized low level mesocyclone the farther east it moves, and also exhibits a very slight leftward change in motion. These are both very strong indications that the storm is becoming increasingly elevated.

Several of these radar features are evident in the image capture on the previous page from 0102z 4/24.

Based on my more detailed analysis of the 23 APR 2004 event here, I think it casts serious doubt on much of the “development of hypothesis” section of the manuscript.

We appreciate your taking the time to perform a detailed analysis on this event, and do not disagree that the storms were probably elevated around/after 0100 UTC. Please note that we specify “around 0000 UTC” in the text of the paper because this time is a good compromise between observing the storms as close to 2300 UTC in time and as close to KFWS in space as possible. As we stated in our earlier response, at 0000 UTC the storms did not exhibit signs of being elevated, such as multiple reflectivity cores and anticyclonic hook appendages. Thus we disagree with your assessment that this aspect of our article is flawed; however, despite our disagreement, we have added a mention that there is a possibility that the storms were elevated to address this concern.

I do not have access to the data here, but if I recall correctly, dewpoint temperatures also increased slightly N/NW of OKC on 3 May 1999 between 00-03z near the Mulhall and other intensely tornadic storms in the same area. The addition of even a slight amount of moisture at those temperatures will have a much greater effect on the instability than temperature alone. In addition, the comparison of surface temperatures between the two events is not really valid, because the surface data cannot be viewed in a vacuum with respect to the rest of the thermodynamic profile in the vertical.

Actually, dewpoint temperatures held steady and even slightly decreased in the region near Mulhall. For example, the Guthrie mesonet site T/Td observations fell from 73/67 at 00 UTC to 70/66 at 03 UTC. Further, we are not comparing these surface observations in a vacuum, but rather in respect to both the KFWD and KOUN soundings from 2003 and 1999 respectively. Using the 03 UTC Guthrie surface temperature and dewpoint on the KOUN sounding from 03 May yields a low-level thermodynamic profile essentially identical to that of KFWD from 23 April.

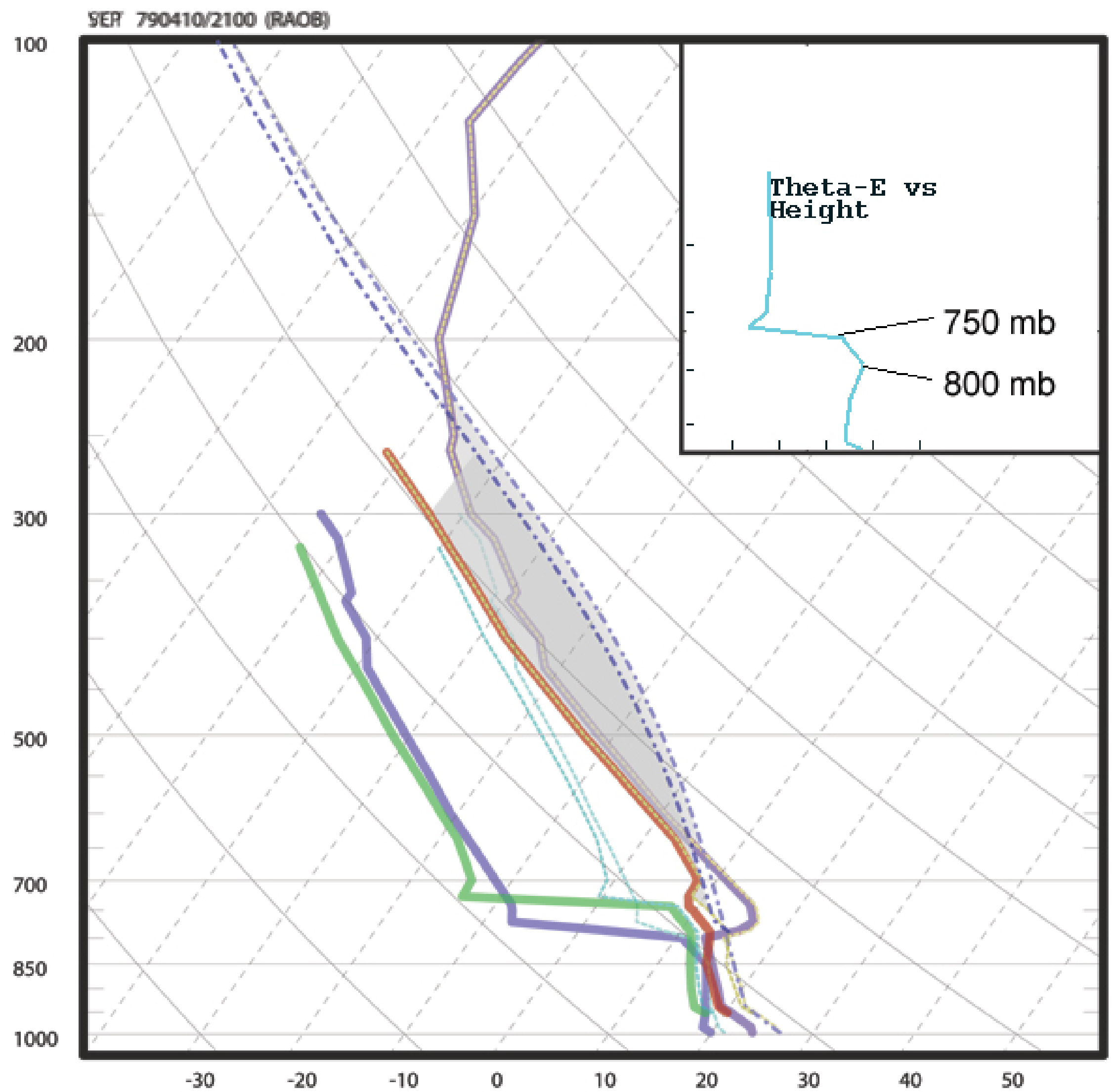
The point I was trying to make is that there is a near-surface neutral or slightly stable layer on the FWD sounding, which is not present on the OUN sounding from 03 May and I believe this feature matters – significantly (for reasons stated above).

The point we are trying to make is that it is likely a stable layer existed in at least some locations during the 23 April 2003 event, the 03 May 1999 tornado outbreak and the 10 April 1979 outbreak. In fact, performing a surface analysis on the 10 April event yields results that are remarkably similar to that of 23 April, including the placement of the storms relative to the low-level boundary. Even assuming the storms on 23 April are HP, why should we assume this is entirely a function of slight differences in the low-level thermodynamic field, and that low-level shear itself does not play a role in determining storm mode?

*Further, the definitions of “stability” and “neutrality” in this context are important. Below cloud base in the absence of condensation and attendant latent heat release there is *always* a stable/neutral layer unless potential temperature decreases with height; thus, cloud-free air ascending into a thunderstorm must initially be forced upward by some mechanism other than thermodynamic instability unless the lapse rates are superadiabatic. 03 May 1999 was certainly characterized by such a stable regime, and although 23 April 2003’s stability was slightly greater, it was not by much.*

Also respectfully and strongly disagree that the SEP sounding [above] is the most supportive of elevated supercells of the three. The lowest 150-200 mb of this sounding strongly suggests that a MAUL is present, which can only be produced by lifting a nearly saturated layer. I have seen numerous soundings similar to the one below that have been associated with supercell tornadoes. The authors’ statement “This sounding could be argued to be the one most conducive to elevated supercells of the three, as is has a deep layer of poor lapse rates below 800 mb, which is also the level at which the most unstable parcel resides (not shown) according to NSHARP. " also completely neglects the limitations of parcel theory in an environment where layer lifting processes (the most commonly accepted way that MAULs are developed and maintained) are involved. In such environments, realized CAPE can exceed computed parcel CAPE by as much as 2 to 4 times.

We respectfully disagree that the sounding shown below strongly supports the existence of a MAUL in the lowest 150-200 mb. Bryan and Fritsch (2000) define a MAUL as a layer characterized both by dewpoint depressions < 1C and decreasing equivalent potential temperature with height. This sounding exhibits neither of these conditions in that layer, and instead appears to be the result of advection with a possible MAUL between roughly 800 and 750 mb. The VCT sounding supports this, and overlaying the two shows that the only region that does appear to be a MAUL on the SEP sounding corresponds exactly to the region where capping was observed at VCT. This further supports our suggestion that the SEP sounding could be argued to be favorable for elevated storms since there appears to be a region of elevated lift based at 800 mb. SEP is red/green, VCT purple, both soundings are from 2100 UTC, and the theta-e profile is from SEP.



Perhaps our disagreement [with sfc-"kink" SRH] relates to our difference on the height of the kink. If you have two hodographs, both with straight line hodographs, but one with a kink at 400m and the other at 500m, the 10m-500m SRH will be substantially larger for the one with the kink at 400m the way that SRH is integrated. For a straight line hodograph with a kink above 500m (your level of interest), you are correct, the difference will be smaller, and perhaps negligible.

Even in your example with a kink at 400 m, the SRH will still be similar unless a large amount of veering occurs between 400 and 500 m. In our experience this is not usually the case.

[re: Miller 2006, Question 4] Okay, fair enough, but significant tornadoes have occurred in both types of environments, reference 4 May 2003 in SW Missouri for the former and 9 May 2003 in Oklahoma for the latter.

We agree, and were not trying to indicate that Question 4 from Miller (2006) is unimportant. We were simply clarifying that it is a different question.

Let me pose this question: if you have a strongly sheared straight line hodograph, but the hodograph does not have a sickle shape, what type of deep layer shear orientation to the storm motion results? You will never get a storm motion even close to perpendicular to the low level straight line segment, even when propagation effects are considered. My point is that is if you have a strong low level straight line hodograph, and it is associated with tornadic supercells (or even supercells in general in most cases) by default you need a "sickle", similarly shaped hodograph. I stand by what I said that this is mostly a different name for roughly the same thing.

We agree that mid-level hodograph curvature is almost always necessary to force the storm motion into the correct orientation relative to the low-level hodograph. However, this is exactly our point—both low-level and mid-level hodograph curvature have historically been considered crucial to tornadogenesis, not just the latter. Pointing out that some hodographs appear to be "sickle" shaped or "kinked" does not indicate that the lowest levels of the hodograph compose a straight line. In fact, as we worked on this research over the last 6 years, many severe-weather researchers reacted with extreme surprise and skepticism to the straight-line hodograph proposition, researchers who were well aware of the kinked hodographs identified by T & E (2000). It seems highly unusual that so many scientists abreast of the latest research would be surprised by such an assertion if the low-level straight-line hodograph feature had already been identified. Nevertheless, we do want to give credit where credit is due, so we have added a reference here to T & E (2000), as well as more precisely clarifying this paper's primary focus—the relationship between the straight line hodograph and the storm motion.

All of the soundings and hodographs in Miller 2006 were reconstructed using the AWIPS interactive sounding program, using the standard archived data, and all heights are AGL – not MSL, so perhaps that explains some of the differences.

Additional comments:

General Comments:

In this second round of review, I have chosen to keep my recommendation as acceptable with major revisions for three primary reasons:

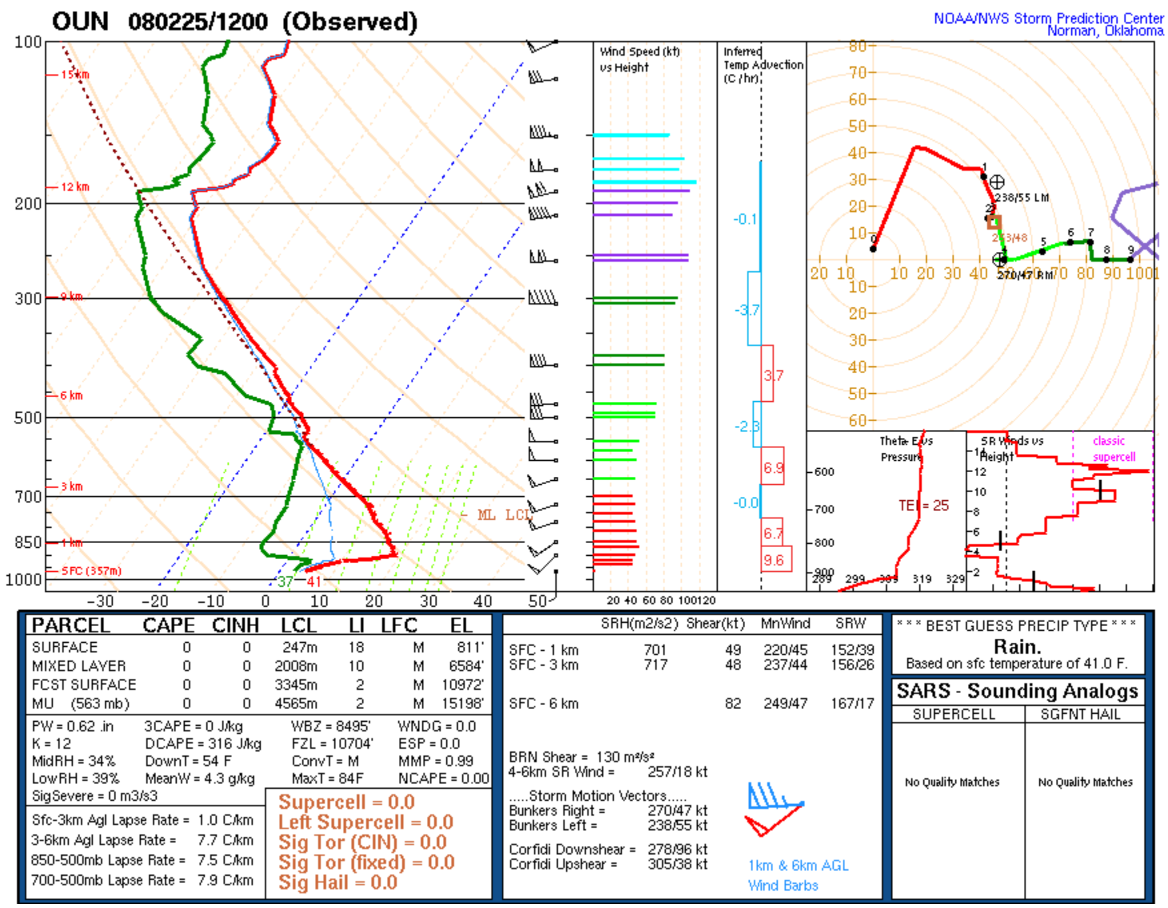
1) Although the authors have made some effort to rectify this issue in their revised manuscript, they still insist on claiming that this topic is "new". It is not. I have tried to supply additional comments and clarification in my reply to the author response in 1) in the previous section.

The low-level "kinked" or overall "sickle" shaped hodograph has already been documented or mentioned in at least three papers: Wicker, 1996, Thompson and Edwards, 2000 and Miller, 2006. The way the paper is currently written, I am left with the impression that the whole concept is new and has never been documented anywhere. If the authors' intent is to present an in-depth investigation of the near-surface straight line hodograph segment (and I believe it is), then that is a new topic, but a concise and thorough literature review needs to be done in the introduction section, followed by a clear statement of the authors' intent to examine, in depth, the low level straight line hodograph feature, and their findings related to nontor, tor and sigtor events in a way that is unique. T&E 2000 and Miller 2006 both show images of a sickle or kinked hodograph, and while they do not necessarily say the words "low level straight line hodograph" it is self evident to me simply looking at the images in those papers that a low level straight line hodograph exists.

Given that the terms "straight-line" and "curved" are widely used descriptors when categorizing hodographs and that low-level curved hodographs have historically been considered favorable for tornadoes, we continue to disagree that the identification of "sickle-shaped", "kinked" hodographs equates to the identification of straight-line low-level hodographs.

One other thing occurred to me regarding the contention that the "kink" in the hodograph (or top of the near-surface straight line hodograph) is 1-1.5 km AGL - and it's been sitting on the SPC sounding analysis site right in front of my face.

Attached (below) is a sounding image capture example from OUN a few mornings ago that illustrates the point quite well. If you simply look at the 1km chit mark on the hodograph trace, you can see that the kink is well below that level. Obviously, this isn't a tornadic case since there's no CAPE, but I see this signature all the time… (JAN and BNA exhibited excellent signatures 00Z 2/6/2008 - it just keeps showing up on sigtor days).



We have never suggested the kink is 1 – 1.5 km AGL. Quoting the footnote on page 2 of our paper:

"Inspection of the soundings corresponding to these hodographs reveals that the "kinks" were actually uniformly located from 0.5 to 1 km, likely because the hodographs in Thompson and Edwards (2000) do not appear to include fixed wind levels every 1000 ft. above mean sea level (MSL) in the lower levels of the sounding."

Also, we certainly agree the hodograph structure shown below and throughout our paper can be found very frequently, as we have personally examined thousands of soundings exhibiting this feature.

[Minor comments omitted...]

REVIEWER C (Steven J. Weiss):

Initial Review:

Recommendation: Accept for electronic publication after completion of major revisions.

Thank you for so thoroughly reviewing our paper and providing such valuable input—we never expected to get a review longer than our original draft and consider your interest in our work a high compliment. You will find that most of your suggestions have been incorporated into the paper, which we now consider to be far more polished as a result of your criticisms.

General Comments:

This paper explores some new ideas and concepts related to tornado forecasting that focus on the structure of low-level hodograph and its relationship to the surface (10 m) storm-relative inflow. Given the many challenges inherent in the operational forecasting of tornadoes, particularly significant tornadoes, the study clearly is most interesting and the authors are to be commended for their proactive hypothesis formulation, collection of data, extensive analysis procedures, and careful attempts to place the results into an operational forecasting framework. Thus, this manuscript submission is clearly appropriate and suitable for the EJSSM. In addition, my interest in this work was very substantial and I believe it is most important to publish these initial results so they can be examined, applied, and judged by others in the forecasting community.

Please be aware that this review is very lengthy, in large part because of my strong interest in the subject matter coupled with the desire to help the authors strengthen and improve the manuscript. The work has many strong points, including originality, well-founded scientific context, use of meaningful subjective and objective assessment methods, clarity of graphics, and the posing of meaningful and thoughtful questions when considering real-world forecasting challenges. On balance, there is much positive about this submission and it has very good potential to make a contribution to the literature.

However, there are a number of concerns related to the written presentation of material in the paper, particularly the lack of sufficient explanation and/or justification of the data collection and analysis methodologies employed in many parts of the text. These include:

1. Observational data reporting standards and inherent observational system errors can impose non-trivial uncertainty in the quality and characteristics of the data collected for this study, such that some observed hodograph features may be a varying function of the different observational systems utilized in the study. This is especially true regarding the low-level winds reported by radiosondes, VAD winds, and wind profilers. However, the data error and reporting requirements of these systems are not addressed in the paper, and this should be remedied in the revised version.

We have addressed this by mentioning these problems and adding several references.

2. Analyses and results are too often presented in the first third of the paper without adequate explanation of the data analysis methods being employed, so the reader knows little about why certain choices and

decisions were made. At times, more detail is offered much later in the paper, but in other instances little if any explanation is provided at all. The reader needs to be kept much better informed about what data are being collected, the strengths and limitations of these data, why certain arbitrary choices and decisions were made, and whether or not these choices impact the results. More emphasis needs to be placed in these areas to better clarify, document, or justify the methods used in the study.

We have addressed these problems.

3. There is not enough distinction between findings that are well supported by data presented and analyzed in the paper and what is largely speculation because insufficient data are available to firmly draw certain conclusions. Thus, the scientific tone of the paper, while generally good, needs to be strengthened in some parts of the manuscript.

We have drawn sharper distinctions and removed the most speculative statements.

[Numerous minor comments omitted...]

After the Davies-Jones et al. (1990) study, some research attempted to distinguish between tornadic and non-tornadic supercells, including Brooks and Wilhelmson (1993), Brooks et al. (1994b) and Stensrud et al. (1997). In addition, Brooks et al. (1994b) used numerical cloud model simulations to deduce that low-level SRH was primarily associated with mid-level mesocyclone development, and other factors were necessary for tornado development, unlike the more straightforward rational of Davies-Jones et al. (1990 – e.g., SRH leads directly to tornadoes). How do these intermediate studies suggesting that low-level SRH is insufficient by itself to explain tornado occurrence impact the current study? The authors may want to briefly discuss this in the text.

Brooks and Wilhelmson (1993) only examined hodograph curvature as it related to updraft intensity and did not attempt to discriminate between tornadic and nontornadic environments, thus we consider their work (though very important) to be irrelevant to this paper. Originally, we did not cite Brooks et al. (1994b) because we consider the results to have been contradicted by recent observations of RFDs, but after reflecting have decided this is worth pointing out in the text and have added a sentence discussing this. We also have decided to cite this paper a second time when discussing the results, and link it to Stensrud et al. (1997).

Since Thompson et al. (2007) found the effective-layer methodology to be an improvement over fixed-layer computational approaches, some readers may ask why the fixed-layer approach is used in the present study. Is it necessary at some point in the text to briefly explain that the current work focuses on the near-ground layer wind variations within the lowest 1 km AGL, such that a variable depth effective-layer determined by thermodynamic characteristics may not be appropriate?

This is a valid point, and one we debated before submitting our first draft. We now address this when we list the profilers as a data source in the methodology.

The discussion of the Purcell profiler should also mention the profiler data reporting requirements since the lowest profiler gate is 500 m AGL. Thus, there will always be a straight-line hodograph in the lowest 500 m using wind data from a profiler site since there can only be two data points – the surface wind and the lowest gate at 500 m AGL. In addition, examination of Fig. 11b in Thompson and Edwards (2000) suggests that the angle between the shear vector and 10 m inflow vector is somewhat less than 90º, suggesting that some leeway in the implicit orthogonal requirement is necessary. This is done much later in the text … (e.g., 90º ± 10º). However, it is necessary to: 1) account for inherent observational errors in wind observations and storm motion estimates, 2) account for the wind data reporting vagaries from radiosondes and profilers when analyzing these data, and 3) acknowledge that there are limits to how precisely the vector relationships can be realistically determined. See Stensrud et al. (1990) for a detailed discussion of the strengths and limitations of various wind sensing platforms. Thus, the implied observation up to this point in the paper of seemingly exact 90º critical angles resulting in pure streamwise vorticity needs to be tempered.

We have highlighted the issue with two data points here as well, and toned down the implication of absolutely pure orthogonality, which we did not intend. Also, we have addressed the issues of observational error and differences between profiler and RAOB data…

Fig. 2 shows the FWD RAOB at 00z 23 April whereas Fig. 3 shows the hodograph constructed from the FWS NEXRAD VAD winds compared to the KOUN RAOB-derived hodograph. Why has there been a switch to the VAD winds rather than continuing analysis and interpretation with the FWD RAOB data? This data change is rather subtle and not mentioned nor explained explicitly in the text; it would not be surprising if some readers fail to notice that the hodograph in Fig. 3 does not come from the same source as the FWD sounding and inset hodograph shown in Fig. 2. Since the VAD winds and RAOB winds can have different accuracy, precision, and reporting standards, it is very unclear why this is being done. Furthermore, a single surface station wind is used for the FWS hodograph rather than the averaging of three stations as was done for the KOUN hodograph. This mixing of "apples and oranges" datasets can raise questions about the consistency of the environmental comparison depicted in this figure. The authors need to be much more consistent in their data analysis procedures, and need to provide justification for the methodologies being employed. It would have been much more straightforward to simply use the FWD RAOB based hodograph from Fig. 2 at this point and do the comparative analysis from RAOB and collocated surface data only.

We think that due to a lack of observations in the right front quadrant of the storms' motion, we had selected the observation nearest FWS as a placeholder while continuing the analysis and neglected to revisit it. We think the best solution to attempt an apples-to-apples comparison is to simply use the FWD RAOB data and have changed the text and Fig. 3 to reflect that. Unfortunately, there is no way to determine a surface wind observation in exactly the same manner as was done for the KOUN sounding due to the lack of a mesonet, but we have addressed this issue as much as possible by averaging three nearby ASOS observations from 0000 UTC.

Note that the use of a Bunkers storm motion may introduce other uncertainties into the analysis, as Bunkers et al. (2000) reported vector MAE values of 4.5 m s^{-1} in their sample of cases.

We are aware of this, and have moved the footnote discussing the Bunkers storm motion into the main text to emphasize this uncertainty. It is not a major concern, however, as this is the only case where we use the Bunkers storm motion, and we are only seeking evidence that the straight-line/90° idea may be important at this point.

Belatedly, there is some discussion of the limited data points in the low levels and the impact of this on the hodograph geometry (straight-line). This should be discussed at its first appearance in the 3 May 1999 KOUN hodograph. However, the lack of directional shear cannot automatically be used to assume a lack of any curvature, as RAOB reporting standards require more than 10 kt of speed change and/or 10° of directional change before additional data points are included between mandatory or fixed altitude levels.

*The limited data point issue has been now been dealt with earlier in the text. With respect to the directional shear issue, we would like to point out that we did not assume the May 8 case is in fact characterized by a *purely* straight line, only pointed out that the lack of directional shear supports this idea because there is little reason to believe substantial directional shear exists between 10-250 m—because otherwise there would be an additional data point in the sounding.*

Since the Thompson/Edwards database was also incorporated into the Bunkers database, there is overlap between the two. Thus, it is assumed that the total sum of the two data samples is actually less than the total number of actual cases. It would be very helpful if a table were created that lists the cases in the final Oklahoma-only data base, including their dates and source (Thompson, Bunkers, *Storm Data*). At the present time, the reader has no information about the climatology of the database being constructed (years covered, seasonal and diurnal distributions, etc.) and this information is necessary to characterize the sample. So a bit of re-write is also needed to capture the "flavor" of the database and to relate it to details contained in the "to be constructed" table.

We agree this is a good idea, and have added an appendix to deal with this issue.

For the profiler data, were 6-min or 1-hr profiler data used? Unless specified in the text, most readers will assume the 1-hr data are used. However, the hourly profiler winds are computed by averaging up to the last ten 6-min samples during the previous hour, and this average is then assigned to the current hour. Since the hourly profiler data actually represent the preceding 60 min, does the use of the profiler data introduce any temporal "mismatches" between the assignment of surface winds and winds aloft? Please discuss this possibility in the text.

We used the 1-hr profiler data in which the time t occurred as identified in the text with respect to the selection of surface wind data. We have clarified this in the text.

Section d: The relationship between 10 m storm-relative inflow (which likely accounts for only a small portion of total storm inflow) and supercell occurrence cannot be determined using a data sample that is almost entirely composed of supercells. To examine this, an equivalent large sample of non-supercell events must be added to the database. Thus, this part of the conclusion is not appropriate. However, the available data in Fig. 12 do suggest that: 1) significant tornadoes become increasingly unlikely when the 10 m inflow is less than ~10 m s^{-1}, and 2) tornado occurrence becomes more likely as the 10 m inflow increases above 13-15 m s^{-1}.

Although the three storm classes did exhibit overlap in the 10 m inflow distributions, there were still meaningful interpretations regarding threshold values that either indicated lower (higher) probabilities of significant tornadoes (tornadoes).

The finding that increasing the 10 m inflow above a threshold value of ~ 10 m/s has little impact on tornado potential does not appear to be correct.

We have addressed [these concerns].

Substantive Comments:

The cases noted by Thompson and Edwards (2000) had hodograph "kinks" in the 1.0-1.5 km AGL layer, rather than below 1 km AGL as documented in the current study. Is the altitude of the kink below 1 km AGL important or is simply the presence of a near 90 deg kink somewhere in the lowest 1-2 km most important? Please address this in the text.

Actually, a close inspection of the soundings from T & E (2000) indicates the kinks were in fact uniformly between 0.5 and 1 km. This may have something to do with the fact that it appears that their hodographs are missing all the fixed wind levels. We did not point out this discrepancy because we regard the kink itself to be unimportant and wish to focus on the shear present in the roughly straight-line low-level hodograph. Regardless, we have added a footnote noting this.

The wording "this segment of the hodograph may in fact be a straight line" (and followed in the next sentence by "If this is the case…") suggests that its presence may be questionable in some ways that are not discussed by the authors, and this is confirmed by the label of "straight-line shear vector?" within Fig. 1. Exactly what are the authors trying to say here about their confidence in the low level hodograph shape? It is entirely possible that the straight-line segment is simply an artifact of the NWS radiosonde data reporting requirements, because wind data points are not required to be listed between mandatory pressure and fixed wind levels unless the wind speed changes by more than 10 kt or the direction changes by more than 10 degrees from a linear fit with height. (See Federal Meteorological Handbook No. 3. Rawinsonde and Pibal Observations at http://www.ofcm.gov/fmh3/fmh3.htm) Thus, it is entirely possible that the apparent straight-line segment could actually have embedded curvilinear segments to it, but the NWS radiosonde reporting standards are not stringent enough to require additional reporting levels in the low level wind data below 253 m. Thus, it is necessary to discuss the reporting requirements of radiosonde winds and their possible impact on hodograph shape in the text.

Our intention here was to question the validity of the straight line on the basis that it consists of only two data points, so we have clarified this in the text. Your reservations regarding the potential for the reporting procedure to favor straight lines is well-founded, which is why we address this by displaying examples of straight-line hodographs from at least two other data sources—namely, VWP (Greensburg hodograph at end of paper) and 915-MHz profiler. Also, the 22 April 1985 hodograph indisputably displays a straight line, even after being corrected to remove what is likely an interpolated data point within the straight line, which we have now done. Thus we do not see the need to intimately discuss the reporting requirements and have instead mentioned it along with a reference to Schwartz and Doswell (1991).

There are several key differences between the KOUN and KFWD radiosondes shown in Fig. 2, especially the much warmer near surface temperature in the KOUN sounding and the stronger low level winds in the FWD sounding. In addition, the KOUN temperature/dew point profile above 500 mb is being affected by cirrus from upstream severe storms in Oklahoma resulting in substantially cooler mid/upper level temperatures compared to the FWD profile. Thus, the wording in the text that the environments were "strikingly similar" is not supported by the soundings and needs to be modified to more realistically describe the sounding comparison. At the minimum, the word "strikingly" should be replaced by "somewhat".

By the time of the Mulhall tornado on May 3rd, temperatures at Mesonet sites nearest the Mulhall storm were generally between 21 and 22 C, a little cooler than the 22-23 C observed at the time of the Moore tornado. In comparison, the Forth Worth event was characterized by temperatures between 20 and 21 C. Although a little warmer, "much" warmer seems a bit of a stretch (even when accounting for changes in elevation), unless the broader environment further from the storms is taken into consideration on each day. However, in response to the concerns mentioned above, we have no problem with wording this more conservatively.

[re: OK Mesonet footnote] While it is agreed that the Oklahoma Mesonet provides higher resolution surface data in time and space, the surface data tell us nothing about the low-level winds above the ground. There can still be variations in the low-level wind field that are not sampled nor resolved by available data, given the accuracy, precision, and data reporting standards of various sensing platforms including the profilers (which have a lowest gate at 500 m AGL and the VAD winds which are reported at 1000 ft (~300 m) intervals. (See comments 20-22.)

Rephrased to "sensitivity of the low-level hodograph to small variations of the wind at 10 m". Clearly, if additional data were available, we would have utilized it.

It is not clear why multiple mesonet sites were used to determine a mean surface wind in time and space, rather than simply using the surface station that was closest in time and space to the wind profile assigned to each event case. The latter approach is much more amenable to real-time operational application, and it has not been demonstrated that the three-mesonet station time and space averaging approach provides superior results to a simpler and more straightforward way of blending surface data to the wind profile. An explanation is needed of the rationale behind the surface wind methodology and why it was necessary to take this approach.

We have clarified why we took this approach in the text of the article and have added a reference to Haugland (2004).

I don't understand the rationale that the number of data points is the primary reason for a result. What is perhaps the most interesting is that the relationship between SRH, layer depth, and discrimination between storm classes is noticeably different from that for shear magnitude. The discrimination between classes using SRH increases as the layer depth decreases, whereas when using bulk shear magnitude the discrimination between the two tornado classes decreases as the layer depth decreases. So why are these results fundamentally different? There may be a number of possible factors, including data sample size effects, physical processes represented by SRH versus shear, and computational methods. Regarding the latter, SRH incorporates the entire hodograph length within a layer, but bulk shear only incorporates the top and bottom points within the layer. Would the relationships be more similar if cumulative or total shear were computed instead of bulk shear (e.g. Bunkers et al. 2002)? The results presented in the current study indicate that low-level bulk shear and SRH are not interchangeable when used to assess tornado and significant tornado threats, and these results differ somewhat from other studies. This has potentially important ramifications and should be discussed in more detail.

The number of data points is only used as a rationale when comparing the Sig/Non to the Weak/Non results for bulk shear alone, not when comparing the bulk shear results to the SRH results. This rationale makes sense because the confidence interval is narrower for the WeakTor cases due to the increased sample size, yielding a higher confidence that the WeakTor bin is different from the NonTor bin as opposed to the SigTor/NonTor comparison. We have now clarified this in the text. Regardless, we agree the non-interchangeability of low-level bulk shear and SRH is the real story here, but prefer to address that later in the paper.

The statement that the Non Tor events met the 10-500 m bulk shear and critical angle requirements "only rarely (if at all)" is somewhat ambiguous. While the bulk shear sample in Fig. 1c supports this conclusion, 25% of the Non Tor cases had critical angles near 90 deg ±10 (Fig. 13a). Further, the conclusion that the "results support the hypothesis" is too simplistic in that it fails to acknowledge limitations of sample size (discussed earlier) and data analysis interpretation. For example, the critical angle data in Fig. 13 reveal that 31of the 51 (61%) tornado events occurred with critical angles falling outside of the 90 deg (±10) bin, suggesting that a wider range of critical angles should be considered when evaluating tornado potential. And for significant tornadoes, 44% of the cases occurred with critical angles falling outside of the 90 deg (±10) bin. So care needs to be taken to ensure that conclusions drawn are fully supported by available data and they are not overreaching.

We should have been clearer here, as our intent was to say that the NonTor events rarely met both the bulk shear and critical angle requirements simultaneously. We have clarified this in the text.

Inasmuch as the critical angle suggestion is concerned, we would like to note that the hypothesis only addressed SigTors, and said nothing of WeakTors. Further, combining WeakTors with SigTors to create a "Tor" bin is inappropriate when considering percentages calculated by this composite class, particularly since we made no effort to ensure our samples were in climatologically "normal" ratios with respect to each other.

Finally, in response to the concern about overreaching in our conclusions, we …worded this conclusion more conservatively.

The shear and critical angle values can be sensitive to relatively small changes in storm motion and/or surface and wind profile data, which may be on the order of observational error. For example, Markowski et al. (2003) noted that although differences in storm-relative winds for their NonTor and SigTor cases were statistically significant for specific layers, they were not considered to be meteorologically significant, meaning that the differences were less than what the operational observing network could reasonably be expected to sample or resolve. Since Markowski et al. used the Thompson et al. data base of events, there are common events in that study and the current study, so similar concerns should apply.

Finally, the choice of SRH or shear magnitude must also be addressed, since an accurate prediction of storm motion is required for SRH (thus introducing additional potential errors) but shear is independent of storm motion.

These concerns do apply, however it should be noted that there are configurations of low-level straight-line hodographs and storm motion that are very resistant to observational error (e.g., when the 10 m inflow and straight-line hodograph trace out two sides of a large square). The 03 May 1999 case is a perfect example of this, as it is evident that large changes in the low-level hodograph and/or storm motion are required in order to have a substantial impact on the critical angle. We think that many outbreak days may be characterized by this sort of "resistant" critical angle, but have not addressed that in the current study.

Also, we agree that bulk shear magnitude could be the better choice in terms of a forecasting perspective; however, it should be noted that the operational models may perform poorly at predicting 500 m wind, as in Fig. 14.

Fig. 14. The VAD profile from VNX contains numerous data points below ~300 m. It is unclear where these data points come from, as the VAD algorithm only provides data at 1000 ft (300 m) intervals. Please clarify the source of the low level VAD data points.

This is true for the part of the NEXRAD Level III NVW product that contains approximately the last hour's worth of VWP data. However, several years ago an alphanumeric text block was embedded within this file that contains only the latest observation in a tabular format, reports heights to the nearest 100 ft, and whose data recording procedure can be configured by the radar operator. More detail may be found in the ICD for Open RPG for Class 1 User, 29 December 2002, document number 2620001E. The data can easily be accessed by running the UNIX "strings" command on the raw, uncompressed NVW file available from NCDC, in this particular instance "strings KOUN_SDUS34_NVWVNX_200705050303". This is what the resulting data looks like for the lowest few hundred meters of the Greensburg VNX hodograph—ALT is MSL, and note that VNX's antenna is 1243 ft MSL (it appears we neglected to account for the fact that VNX's antenna is itself 10 m AGL, so we have updated the figure to reflect that):

```
P                    VAD Algorithm Output  05/05/07  03:03
P    ALT      U       V       W     DIR   SPD   RMS    DIV      SRNG    ELEV
P   100ft    m/s     m/s     cm/s   deg   kts   kts   E-3/s      nm      deg
P    015    -4.9    14.6     NA     161   030   8.4     NA      4.73    0.5
P    017    -5.4    16.4     NA     162   034   6.7     NA      4.73    0.9
P    020    -5.3    18.9     NA     164   038   6.4     NA     12.01    0.5
P    021    -5.6    19.4     0.0    164   039   6.3  -0.0015   13.50    0.5
P    026    -6.3    23.7    -0.1    165   048   5.9   0.0072   13.50    0.9
P    030    -6.0    27.9     NA     168   056   5.6     NA     11.89    1.3
P    033    -5.9    28.5    -0.8    168   057   5.6   0.0375   13.50    1.3
P    040    -1.7    29.5    -1.0    177   057   5.4   0.0074   13.50    1.8
P    040    -1.5    29.3     NA     177   057   5.5     NA     13.66    1.8
P    048     3.6    27.2     0.4    188   053   5.4  -0.0568   13.50    2.4
```

Rasmussen et al. (2000) and Rasmussen and Blanchard (1998) discussed the apparent need for the background field larger scale SRH field to be enhanced on most tornado occasions by the presence of boundaries with backed low level winds on the immediate cool side of the boundary, suggesting that higher values of SRH are typically needed for tornadoes to develop. The current study appears to modify this concept by suggesting that storm motion needs to optimize the critical angle to fall within a narrow range centered around 90 deg, even if this specific storm motion does not result in the largest SRH. The ramifications of this concept is most interesting and could be important, but given the complexities and unknowns associated with tornadogenesis, and the range of critical angles associated with tornadoes in this data sample, it is apparent that more work is necessary to further explore this concept.

We agree with this assessment.

The statement that low-level straight-line hodographs have not been documented in the literature is open to question. Thompson and Edwards (2000) and Miller (2006) both documented cases where significant tornado environments were associated with low-level hodograph "kinks" below which contained straight-line structures. While they did not focus on the straight-line nature of the hodograph, these earlier papers contained examples of this structure. It might be more correct to modify the text to replace "documented" with "emphasized".

This is a valid point, as using a loose definition of "document" one could argue that these structures have been documented for decades. We've elected to change "documented" to "identified", which we think more accurately describes what we have done.

Was it possible "that 500 m was a good proxy to use" because that level coincides with the lowest gate in the NPN network? If not, it was quite coincidental.

Yes and no. We found observational evidence to suggest this would be a good "proxy" level to use, but did not mention it as we have not performed a rigorous analysis. Preliminary work by Miller (2006), however, does suggest that 400-500 m is the approximate level of at least some "kinks", so we have added a citation to his paper here.

Second review:

Recommendation: Accept for electronic publication after completion of minor revisions.

General Comments:

In revising their paper, the authors have addressed nearly all of the comments and suggestions offered in the initial review process, and their extensive efforts to improve the manuscript are greatly appreciated. Accordingly, I believe that the current version is nearly ready for publication. In this second review, a smaller number of minor comments and suggestions are listed, and I will defer to the editor and the authors to determine the validity of the suggestions. Overall, the authors have done an excellent job to improve the scientific framework, the orderly and logical flow of the presentation, and the interpretation of results. This paper will provide a meaningful contribution to the literature documenting new aspects of the mesoscale environment associated with tornadoes, and it should spur additional discussion and testing of the findings to operational tornado forecasting. The authors are strongly commended for their diligence (and patience!), and application of a useful scientific framework in designing and focusing this research.

[Minor comments omitted...]