Overall I think the paper is interesting (within the scope of this journal), but there are quite a number of major revisions that need to be done. Suggest accepting the paper with major revisions. My comments are below.

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General Comments

I think the vertical wind shear discussion is fundamentally flawed. Using a single sounding to diagnose the balanced part of the flow is not valid. Consider the zonal momentum equation:

$$\frac{Du}{Dt} = -\frac{1}{\rho}\frac{\partial p}{\partial x} + fv + F$$

Geostrophic balance...

$$\frac{1}{\rho}\frac{\partial p}{\partial x} = fv$$

is slowly evolving (small or zero acceleration following a parcel), inviscid dynamics. Taking a single sounding you are sampling an instantaneous portion of the atmosphere that has all the forces inherent within the sample (thus local accelerations and frictional dissipation are there). As a result, geostrophic wind balance doesn't apply and thus the thermal wind balance calcuations performed are invalid. To do this correctly, a spatial/temporal average of radiosonde data would need to be done to filter out the fast modes in the atmosphere and retain the slowly-evolving balanced part of the flow. However, even if you could do this, I don't think it adds any real substance to the paper. I found the discussion of the vertical wind shear very confusing. I suggest using the re-analysis data to give more of a synoptic description of the frontal system. You can compute warm and cold air advection from this data set as well as frontogenesis parameters (shearing deformation) that may help characterize the kinematic structure. In addition, the authors could use the radar retrieved winds to compute some of these parameters and compare them to the re-analysis dataset.

I think the authors are reading too much into the differences in the raindrop size distributions. Attributing these differences to warm/cold air advection may not be valid. Rainfall has much small-scale variability that is governed by many processes (i.e. cloud microphysics including growth, phase changes and fallout).

It appears that the gamma raindrop size distribution approximates the disdrometer observations very well. I think this is an interesting and useful conclusion to make and maybe should be stated clearer.

In my opinion, the real strength of this paper is the dual-Doppler analysis (I really liked what was done). Dual-Doppler is not an easy task and I think more focus on this part of the analysis would make the paper stronger.

Specific Comments

Fig. 1...Suggest showing the coverage for each radar so we can assess how much overlap exists for dual or tri Doppler analysis.

Was a power law relationship used for particle fall speed estimation? Gamma distributions are found to perform better than power law relationships (Ulbrich and Chilson 1994; Heymsfield et al. 1999).

What were the boundary conditions chosen for the vertical velocity integrations? The answer you get is sensitive to these boundaries because the assumption propagates through the column.

Technical Corrections

pg. 1527 line 2. Should be “anelastic mass continuity equation”.

pg. 1528 line 7. This sentence doesn't make sense. First off, I'm fairly sure you are not calculating reflectivity from the radar. The radar “measures” reflectivity based on the returned power from hydrometeors. How is the data averaged? Do you average horizontally in space to get a vertical profile and then plot that over time? What elevation scan are you showing? Clearer explanation is needed here.

Figures 5 and 6 are hard to read. I suggest adding color shading to the plots in Fig. 5 (since the article is on-line there shouldn't be any extra charge). I suggest making all the panels in Fig. 6 MUCH larger and with better labeling. As it stands now, I can't tell much of anything from Fig. 6.

There are numerous grammatical and writing errors throughout the paper and I don't have time to fix them all here. I realize that English is not the native language of the authors and I commend them for the job they have done. If you have an English speaking colleague, I suggest asking him/her to proof read the revised manuscript.

References:

Heymsfield, G.M., J.B. Halverson, and I.J. Caylor, 1999: A wintertime Gulf Coast squall line observed by EDOP airborne Doppler radar. *Mon. Wea. Rev.,* **127,** 2928-2949.

Ulbrich, C. W., and P. B. Chilson, 1994: Effects of variations in precipitation size distribution and fallspeed law parameters on relations between mean Doppler fallspeed and reflectivity factor. *J. Atmos. Oceanic Technol.,* **11,** 1656–1663.