Responses to Reviewer A

1. As mentioned above, the authors do a very thorough job of identifying uncertainties and possible sources of error. One area that did not seem to be specifically addressed, however, was the fact that much of the retrieval algorithm is based on output from a numerical model run at 2-km grid length. What impact would higher-resolution models have on the relationships derived? For example, the model output is used to confirm the assumption (based on flight-level data) that updrafts > 5 m/s are saturated, while weaker updrafts may or may not be saturated, thus requiring the estimate of Qnet. Is it reasonable to assume that a simulation with 1 km, or 100 m, grid length may have a lower updraft threshold for assuming saturation? Would that impact your resulting algorithm? What about if different microphysical parameterizations (e.g., double-moment or bin schemes) were used? I’m not saying you need to rederive the retrieval algorithm using these alternate model configurations, but I think it would be illuminating to at least discuss these possible uncertainties.

Yes, we believe that changes in model resolution and microphysics would probably change the vertical velocity saturation threshold. At a resolution finer than 2 km, entrainment and turbulence start becoming resolved which will affect updrafts and saturation. However, in the manuscript, we focus on observations of vertical velocities and relative humidity, which do not suffer from the modeling issues discussed above. In addition, the aircraft flight level data is collected at high resolution (~ < 1 km). The model data is used only in a supportive role to test the latent heating algorithm. One drawback of the observations is that there is not a very large sample size. This is mentioned in the paper. We have added a sentence describing the modeling sources of uncertainty in the manuscript.

2. Another question I have deals with the fact that only the latent heat of condensation/evaporation is considered in this algorithm, i.e., it only extends up to about 8-10 km altitude. However, other observational and modeling studies (e.g., Zipser 2003, Fierro et al. 2008, 2009, Kelley 2010) have shown a high-altitude updraft peak that is thought to be driven by latent heat of fusion. What impact could neglecting that term have on resulting latent heating calculations, in particular in the upper troposphere? The magnitude of the latent heating may be low, compared with in the lower troposphere, but it should alter the vertical profiles at least.

We only consider warm rain microphysics for several reasons. (1) warm rain heating is easier to understand and compute than mixed phase heating (2) the total latent heat budget is dominated by warm rain processes (this was discussed in the manuscript and the work of Tong et al. 1998 was referenced. Their study focused on Florida deep convection, which is probably not too different than deep convection in a hurricane. The hurricane modeling study of Zhang et al. (2002) was referenced that also came up with a similar conclusion). Including mixed-phase heating would, of course, add some heating in the upper-levels but this would be of secondary importance. As far as the dynamic response of the different profiles in hurricanes, warm-rain heating will have a greater impact because it is coupled to the high vorticity in the mid to lower levels. Shifting the peak heating in altitude does have an impact in balance models as described in Hack and Schubert (1986). We added a sentence in the introduction clarifying the results of the Zhang et al. (2002) study and mentioning that mixed phase processes will alter the vertical heating profile somewhat.

3. Figure 13 could be improved considerably. What about, instead of showing isosurfaces, you show vertical cross sections of latent heating alongside the same sections showing reflectivity from the tail Doppler radar? At the very least it would be good to provide more labeling on the figures, including cardinal directions. Plotting the shear vector would be helpful too. Also, what about including additional contour levels other than just ± 100 K/h? Of course you couldn’t do isosurfaces then, but it’d be helpful to see additional contour levels, which again raised the prospect of showing vertical cross sections rather than isosurfaces.

Figure changed due to similar suggestions from the other reviewers. Figure now shows the following: vertically averaged horizontal field of latent heat along with the azimuthal mean vertical profile of heating at the RMW for each pass. Text is updated to reflect new figure and a discussion was added.

4. Would there be any utility in showing examples of latent heating distributions from satellites? You discuss it a little in the introduction – what is gained by the Doppler retrieval compared with satellite (e.g., TRMM)-based retrievals? Is it only resolution? I know that there was no TRMM satellite during the Guillermo flights (or it was just launched), but you would at least show an example plot from TRMM or discuss in more depth what is gained by using airborne Doppler.

It would be interesting to show examples of latent heating retrievals from satellites but that is for another paper. We mention the vast array of problems with satellite retrievals in the introduction and this should convince the reader that these current retrievals are less than optimal. References are given for the reader to find satellite examples. TRMM does not have Doppler capability and thus no winds (a major disadvantage for latent heat retrievals).

5. It would be interesting to discuss a bit more what the limitations are in terms of the retrievals using a 2-km grid of Doppler data. You’re essentially only capturing the largest up- and downdrafts, and are presumably missing a significant portion of the spectrum where weaker vertical velocities may reside. What if you were to apply your algorithm to ELDORA data, with resolutions on the order of 0.5 km? And can you apply it to EDOP profiles, other than the mean EDOP hot tower profile shown in Fig. 14? Again, this suggestion is intended to stimulate discussion in the manuscript, rather than generating a whole new set of results and plots.

The wind retrieval is performed on a 2 km horizontal grid and then a LaPlacian filter is applied to the data. Thus, the effective resolution of the data is on the order of 5 – 10 km, which represents only the largest vertical velocities as the reviewer mentions. This can be seen clearly in Fig. 13. The large updraft pulse is captured well, but the smaller-scale oscillations are smoothed out. This is discussed in the manuscript. Applying the algorithm to ELDORA data would be interesting (application to other radars is currently being done). We have added a sentence that highlights the importance of applying the retrieval algorithm to other radar systems.

6. Finally, while the article is generally well-written, I do have one sylistic quibble – there too many parenthetical inserts. For some paragraphs nearly every single sentence has such an insert. This can serve to disrupt the flow of the reading. If you could go through the manuscript and try to remove these inserts whenever possible it would help the flow of the paper.

I agree. These were added from the many edits completed before submission. I thinned some of these out now.