**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.

**Responses to Reviewer B**

**Major Point and Recommendations:**

1. The current manuscript exceeds the AMS length threshold by roughly five pages. The most obvious way to reduce the length is to remove all sections related to the EDOP radar. The authors introduce and discuss the EDOP radar, but the latent heating retrieval (as best as I can tell) was developed primarily from the Bonnie numerical simulation and the Guillermo radar data (supplemented with Katrina liquid water contents and high-altitude dropsonde data), with no contribution by the EDOP data. Then, at the very end of Section 4a, latent heating profiles are presented for the EDOP data. At this point, showing the EDOP data is a distraction that raises more questions than it answers (or clarifies). For example, if the EDOP has trouble estimating the horizontal winds, how reliable are the heating profiles obtained using Equation (2) – which clearly contains a horizontal divergence term?

In order to reduce the number of pages, we have removed the EDOP material. The paper was not dependent on this information. The paper will still be a few pages greater than the AMS length threshold, but it takes a bit more space to present a new algorithm and convince the reader of its merit including error characteristics.

**Minor Points and Recommendations:**

**1.** Pages 8-9: If the EDOP material is retained (see above), please clarify your definition of a hot tower. For example, do they represent the strongest updrafts observed at any altitude, or for a deep-layer average? Also, you provide a hot tower definition for this study, but never use the definition anywhere else in Part I. Rather, you only use the 5 m/s threshold. If hot towers are exclusively discussed in Part II, then move the definition and background to Part II.

EDOP material removed.

**2.** Page 12, Figure 3: I recommend adding a sentence or two to the caption of Figure 3 stating that these convective-scale updraft events were defined after a scale separation method was applied. As a result, the total vertical velocity can be negative when a convective updraft is superimposed upon a stronger mesoscale downdraft.

Added the sentences the reviewer suggested.

**3.** Pages 13-18 and Figure 6: Each time I read through this section and view Figure 6, the same question arise: how are the precipitation water contents obtained from the radar reflectivity field? Granted, your method is described in detail later, but at this stage the reader may be confused. Thus, I recommend inserting a sentence or two at the end of Section 3a (and possibly a box Figure 6 – see below) stating that these methods will be discussed in Section 4a.

Added a sentence clarifying the application to radar observations at the end of section 3a and put a sentence in the caption of Figure 6.

**4.** Page 16 and 23: Please clarify what you mean by “...magnitude of saturation”. I think you are implying a spectrum ranging from sub-saturation through super-saturation, but one could easily be confused since the term “saturation” often implies a single state described as 100% relative humidity.

We added this clarifying sentence to page 16…

“Put another way, the algorithm is only dependent on knowing if precipitation is being produced not on the precise value of precipitation production.”

Also added some clarification to a similar sentence on page 23.

**5.** Page 22: For clarity, define total precipitation water content (qp) as the sum of the liquid water content (LWC) and ice water content (IWC).

Done.

**6.** Page 23: Replace “...a composite high-altitude () dropsonde...” with “...a composite sounding derived from ten high-altitude () dropsondes...” for greater clarity.

Done.

**7.** Page 23 and Figure 13: I find the 3D imagery difficult to interpret, much less compare to Figure 2. In particular, the altitudes of the heating/cooling maxima are nearly impossible to determine, and the azimuthal distributions are not very clear. I recommend converting these to top-down images that show the horizontal structure of the vertically averaged heating/cooling. You could then add a second field to each panel denoting the altitude of the heating/cooling maxima at each grid point.

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.

**8.** Page 25: Your retrieval method assumes a horizontally uniform density profile, yet the eyewall contains strong thermodynamic (and thus density) gradients. Did you test the retrieval’s sensitivity to such density gradients or just different horizontal uniform density profiles? Please clarify in the text.

Density only appears in the computation of saturation and perturbations from the composite sounding have a small effect. We added a parenthetical insert to acknowledge this, “(perturbations to the density profile in all directions had a small effect on calculations)”.

Temperature and pressure appear in the calculation of the magnitude of latent heat release but do not require horizontal gradients. Sensitivity to the thermodynamic information was small for the magnitude calculation. This is discussed extensively in the manuscript.

**9.** Page 29-32: The summary and conclusions section could be streamlined to further reduce manuscript length.

We streamlined the summary and conclusions section a bit, but this section is only 2 – 2.5 pages anyway. Removal of the EDOP information will help with length.

**10.** Figure 1: Should be removed if all EDOP discussion is removed (see above).

Done.

**11.**Figure 4: What at the quasi-linear “spikes” in the scatter associated with high

precipitation production for both warm and cold processes?

This is more obvious in the ice phase precipitation and is probably associated with some feature of the microphysics scheme (there may be quite a few conditional statements in the code that act to organize the output into quasi-linear clusters). Either way, we focus on warm rain processes so it’s not a big issue.

**12.** Figure 6: You may wish to add a box between the Doppler grid volume and Equation (2) showing the conversion of radar reflectivity to liquid/ice water content.

We added a sentence to the caption and in the manuscript. See comment 3.

**13.** Figure 14: Should be removed if all EDOP discussion is removed (see above).

Done.

**14.** Figure 15: My version is not very clear (it looks scanned), please contact the author and obtain an original.

I actually made that plot. It should look clearer in encapsulated postscript or pdf format for the final production.