REVIEW OF: A Latent Heat Retrieval and its Effects on the Intensity and Structure Change of Hurricane Guillermo (1997). Part I: The Algorithm and Observations

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RECOMMENDATION: Accept with minor revisions

This paper presents an algorithm for retrieving latent heating distributions from airborne Doppler radar. Using primarily a 2-km numerical model simulation to obtain many of its parameters, the paper does a very thorough job describing possible sources of error and uncertainties. It represents an advance over previous radar-based latent heating retrieval techniques, in particular facilitated by the use of the dynamically-consistent numerical model in its formulation, and it appears to offer promise for obtaining latent heating profiles in tropical cyclone inner cores.

In general I think this is a good paper. I just have a few questions and suggestions for improving the manuscript even more. For these reasons I am recommending that this paper be accepted after minor revisions have been performed.

General comments

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.

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.

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.

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.

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.

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.