JAS-3700 Guimond
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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" by S. R. Guimond, M. A. Bourassa, and P. D. Reasor

Recommendation: Accept after **minor** revisions.

General Comments: The authors present an improved method to estimate latent heating associated with warm-rain processes (specifically condensation/evaporation) for a high-resolution four-dimensional airborne Doppler radar dataset from Hurricane Guillermo (1997). The improved methods include (a) analyzing the scheme using output from a numerical model, (b) documenting sensitivities using supplementary data, and (c) developing a parameterization for the precipitation storage term (which is often difficult to estimate from observations). The methods are shown to minimize biases and randomly distributed errors associated with strong vertical velocities. The manuscript is well organized and is effectively motivated by previous observational and theoretical studies. Salient results are generally well explained and compelling. However, the manuscript exceeds AMS length guidelines. Recommendations on how to reduce manuscript length (primarily remove the unnecessary EDOP material) and other minor concerns are outlined below. Overall, a revised manuscript should contribute to our understanding of latent heating in the hurricane inner-core. Thus, the manuscript may be suitable for publication in the *Journal of Atmospheric Sciences* after revisions and clarifications have been made.

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?

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.

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.
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.
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.
5. Page 22: For clarity, define total precipitation water content (q_p) as the sum of the liquid water content (LWC) and ice water content (IWC).
6. Page 23: Replace "…a composite high-altitude () dropsonde…" with "…a composite sounding derived from ten high-altitude () dropsondes…" for greater clarity.
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.
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.
9. Page 29-32: The summary and conclusions section could be streamlined to further reduce manuscript length.

10. Figure 1: Should be removed if all EDOP discussion is removed (see above).
11. Figure 4: What at the quasi-linear "spikes" in the scatter associated with high precipitation production for both warm and cold processes?
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.
13. Figure 14: Should be removed if all EDOP discussion is removed (see above).
14. Figure 15: My version is not very clear (it looks scanned), please contact the author and obtain an original.