JAS-3119
Reviewer A
2nd review of Guimond et al. (Hurricane Dennis)
August 2009

I appreciate the effort the authors put into their response. The authors obviously did a lot of work in this response, but the paper does not seem much more clear or convincing than it did before. I still recommend major revisions to the manuscript.

In the response to this review, please include page numbers where changes are made, and double check the figure numbers that are referenced. That will make it much quicker for me to scan through the revisions and hopefully recommend the paper for publication next time.

(1) Before going through the response to my review, I want to comment on Reviewer C's first point. I was also troubled by the use of the phrase "hot towers" in this paper, since that has traditionally implied undilute ascent from the boundary layer to the upper troposphere (from Riehl and Malkus 1958 Geophysica, with the "hot tower" terminology in Malkus and Riehl 1960 Tellus and subsequent papers). Recently the phrase gets used very loosely, to the point of having almost no meaning sometimes. That seemed to be the case in the first version of this paper. The current paper by Fierro et al. (2009 JAS), with Joanne (Malkus) Simpson as a co-author, now re-defines "hot tower" this way: "A tropical 'hot tower' should thus be redefined as any deep convective cloud with a base in the boundary layer and reaching near the upper tropospheric outflow layer." That is a *much* broader definition than what this paper by Guimond et al. is referring to. Instead of making another definition here based on peak updraft speeds, avoiding the phrase "hot tower" altogether and using a different descriptive phrase would make things more clear.

(2) Concerning the ~1425 UTC ER-2 pass being aligned tangent to the eyewall instead of through the center, the authors gave a good response (other than referencing Figure 15, which turns out to be the HAMSR figure!). But part of the strength of that response hinges on seeing similar structures in the later figures from the subsequent ER-2 pass. The reader does not know this while reading about the first pass. The descriptions of that ~1425 pass should assure the reader by at least alluding to the later pass's similar structures.

(3) Now I'm sorry I suggested looking at HAMSR! The response mentions raw HAMSR brightness temperatures with very little processing, and little help from the HAMSR team. It is a shame that they did not provide a quality-controlled product, particularly since a publication like this does make their instrument look useful. Do you have sufficient confidence in the data quality and calibration? From what is said in the response, it sounds like you do not. Please state something about this in the text, or remove the figure. Do the 55.50 GHz brightness temperatures match the 150 hPa model analyses away from the storm? If not, it is inappropriate to compute a temperature anomaly as (TB_HAMSR - T_Model). It might be appropriate to show (TB_HAMSR_eye - TB_HAMSR_environment) instead. Even with that, I would

wonder whether the calibration is sufficient to interpret the raw TB difference as equating to a temperature difference.

As for the specific HAMSR results, listing a temperature anomaly to the hundredths place (15.77 K, on p. 22) is completely unjustified. The grayscale in the figure is difficult to read, since darker shades are assigned to both cooler and warmer temperatures - a plot like this should have the contour values labeled. It looks like there is a 3-4 K increase along the center of the swath - maybe the increase would be greater if compared to a farther distance from the center. But a 15-17 K anomaly at 150 hPa is difficult to believe. Other studies (Hawkins' papers from the 60's and 70's, Halverson et al. from CAMEX-4) showed a sharp decrease in warm core strength above about 200 hPa.

(4) P. 15, 2nd ¶: "The region of low TBs on the eastern (downshear) side of the storm in each panel is observed to begin development during the first two overpasses (Fig. 6a and 6b). During this time period, the convection is disorganized and straddling the mean radius of maximum wind (RMW; 25 km) shown as a circle in each panel of Fig. 6. During the third ER-2 overpass (Fig. 6c), the low TBs organized into a thin band inside the mean RMW and dropped to ≤ 100 K in ~ 15 min,"

That "thin band inside the mean RMW" can also be seen in 6a and 6b, distinct from the more disorganized looking convection straddling the RMW. In the same location as the band that is prominent in 6c, 6a (~1341 UTC) has a band of yellow / light orange shades (TB ~ 220 K), with darker oranges on either side. This band becomes easy to see in 6b, with some blue shades ~150 K ~ 1407 UTC. The ~1428 UTC observation in 6c does have the most prominent signature and lowest TBs, but the description in the text gives the wrong impression - that something disorganized near the RMW in a and b suddenly became organized at a smaller radius with a much lower TB in ~15 minutes.

(5) P. 15, bottom: "Around 15-20 minutes later, Fig. 6d..."
The references to time intervals in this paragraph make me wonder if the times listed in the figure are correct. The time listed for 6c is 14:21:54 - 14:33:43. For 6d, it is 14:50:11 - 15:02:01. That puts the times across the eye ~28 minutes apart. For a W-E pass followed by an E-W pass, the times across the eastern eyewall would be a little closer (~25 minutes), and the times across the western eyewall a little farther apart (~30 minutes).

(6) slope of eastern eyewall inferred from AMPR: The statement about the "peak of the eastern eyewall" being vertically erect is based on collocation of the 85, 37, and 19 GHz scattering signatures. But when you consider *only* the scattering signatures, that gives much less profiling information and less ability to discern anything about the slope. The large particles responsible for scattering the 19 GHz radiation would also scatter the 37 and 85 GHz radiation, whether or not there is an erect tower above those particles. A better indication that the upper part is not sloping far outward is the narrow width of the 85 GHz depression. A strongly sloped eyewall should have low 85 GHz TB collocated with the 19 and 37 GHz minima *and* extending outward from there.

There are healthy emission signatures just inward of the scattering signatures - no need to dismiss them as coming from "hydrometeor debris (or shallower cloud)". I do not see a reason to treat this separately, referring to it in the text as being "outside the HTs". I am not arguing that it is strongly sloped, but there is some noticeable slope from the low level (liquid rain) part to the upper level (graupel) part.

(7) previous comment #18: I never doubted the existence of the mid-level downdraft in this cross section, which is what the response addressed. Based on the response, and what is said near the bottom of the current p. 21, I doubt that you even intended to make the point I was objecting to. The phrasing in the paper makes it sound like you are describing a single, vertically coherent updraft that has a downdraft separating its upper and lower branches. But it can't be a single feature with that up-down-up structure - air from the lower branch would not get through the downdraft to reach the upper branch. I do not think you intend to give that impression, but the phrasing (now on p. 18, with a "core updraft of the HT" separated into upper and lower sections by a downdraft) does give that impression. The phrasing toward the bottom of p. 21 is much more clear on this topic.

(8) p. 21, last paragraph:
it is not appropriate to call Figures 9b and 14 b two *random* cross sections. Coming from the same part of the same hurricane, tens of minutes apart, they are not random at all!

(9) "Based on the vertical momentum equation, significant local buoyancy (such as latent heat release) must be present to produce such strong updrafts." Not necessarily - this could be driven by dynamic effects, similar to dynamically driven vertical motions in supercells or tornadoes. I suspect local buoyancy is a major contributor in this case, but as you mentioned in the response, the measurements are simply not there. Either way, this distinction is not necessary for the points this paper does show. Regardless of whether the updrafts are driven by dynamic forcing, buoyancy, or some combination, they do induce subsidence that would necessarily warm the eye.

(10) "As far as we know, this is the first time that hot towers (or bursts of convection) have been shown (i.e. quantified) to organize the warm core (at least on this scale) from observations." Is that really shown here? I hope the paper does not make such a strong statement. We see strong convective towers during a period when the warm core strengthens and the TC intensifies. Of course that is an over-simplification on my part, but I do not see a lot of the dots being connected by this analysis (other than by inference and speculation). The speculation is plausible, but the measurements (more importantly, a lack of more complete measurements spatially and temporally) do not seem sufficient for going beyond speculation.

(11) Figure 18: This figure is extremely difficult to read without color or without values labeled on the contours. I can tell what the values are from the color version in the original submission. Even with the color version, for me it is more confusing than informative (it just leaves me wondering what those AMSU plots look like!). Based on the text, its point seems to be that the warm core (within ~250 km of the center) becomes

more asymmetric due to the Cuban landfall, then becomes more symmetric during and after the ER-2 flight. Maybe this would be simpler to see with a line plot that just takes the 50 km value, or the mean within 250 km, or whatever distance is appropriate.