JAS-3119 Guimond
Multi-scale Observations of Hurricane Dennis (2005)

Accept after major revisions

This is an exciting case of hurricane re-intensification during a period with strong eyewall convection after Hurricane Dennis emerged back over water after crossing Cuba. The paper presents many interesting observations, especially focusing on EDOP radar and AMSU temperature retrievals. It makes inferences about mechanisms for the strong convective towers causing an increase in warm core strength and symmetry. While I suspect the inferences are generally on target, I do not find the paper very convincing in this regard. That is not so much a fault of the authors, but a limitation of the available data. The tone of the paper should probably reflect less certainty in the inferences.

General comments and suggestions:

1) The use of the AMSU temperature retrievals is troubling for a few reasons. Most or all of these are listed separately in the specific comments. First, the paper uses "raw" AMSU temperature retrievals instead of correcting for hydrometeor contamination in the brightness temperatures. The EDOP and AMPR observations strongly suggest that hydrometeor contamination would be a problem in this case, leading to inadequate retrievals of teh warm core. Second, issues with the AMSU grid geometry and footprint size are clearly noted in the text, but they complicate interpretation of the figures. The figures should be modified to better account for these. Third, there are multiple comments in the text about limitations of AMSU horizontal resolution. The NASA ER-2 carried two temperature profilers - HAMSR and MTP - during this mission. This type of case study seems like the perfect place to include those measurements. I don't recall ever seeing them used in a study like this, even though NASA has flown them in most (all?) of its recent hurricane experiments. Is there something with the measurements from those instruments? If so, maybe it is worth mentioning. If not, they seem like a perfect fit for addressing some of the limitations in this study.

2) There are a few references to a suspected "mesovortice". First, I am not familiar with that being an accepted spelling instead of mesovortex. More importantly, I saw no observational evidence for the existence of this feature. NOAA P-3 data are included in the study; the tail radar should be sufficient for providing evidence for or against the presence of a mesovortex. Or perhaps the Key West WSR-88D was close enough. Without some supporting evidence, this "suspected" feature should not be mentioned in the abstract and conclusions. I can imagine another

author skimming those sections and then citing this paper as showing an example of an eyewall mesovortex!

3) The ER-2 leg in Figures 7-10 missed the low level eye and instead was tangent to the southern eyewall. This makes it improper and confusing to interpret the cross sections in terms of radial motions, transverse circulations, and convergence / divergence. Seeing the velocity data (Figures 8 and 10) presented that way can unnecessarily hurt the paper's credibility with a reader (it did on my first reading of the paper!). The subsequent leg in Figures 11-14 are well suited for those interpretations instead. The discussion of Figures 7-10 should be kept to a minimum, perhaps even deleting Figure 10.

Specific comments:

1) do a search and replace for mesovortice / mesovortex

2) P. 4-5 looks like a "data" section for instruments that are not even included in the study. I think your point is that EDOP provides superior resolution in its nadir view, compared to NOAA and NCAR radars. To make that point more directly and concisely, you can give the brief EDOP description and then state the NOAA and NCAR resolutions for comparison. But this belongs in section 2.

3) P. 5, near bottom: "Only recently has new information about deep convective bursts in TCs been uncovered"
That is an odd statement to make here, given how many studies can be referenced over the years. Several have been referenced already in this paragraph, and many more could be referenced (but no need to give an exhaustive list). People have been uncovering new information on this topic for decades!

4) p. 8-9: Either the hydrometeor-corrected temperature fields should be used, or a better / more complete justification for *not* using them needs to be presented. It looks like your complaint about the Demuth et al. (2004) method is only a complaint about the Cartesian grid and associated smoothing - not the hydrometeor correction itself. Why not make the corrections on the native swath, or on some grid that you do find acceptable? Ice scattering in convective cores (such as those in this Dennis case) can substantially reduce the AMSU brightness temperatures, and lead to a poor temperature retrieval if not accounted for. If you want to argue that the raw temperatures are acceptable in this case, you should show plots of brightness temperature and retrieved temperature, and make the case that hydrometeor contamination does not affect your results.

5) In that same paragraph:
a) "larger discrepancies (~2.5 K) in the warm anomaly": which was warmer, the raw or corrected fields?
b) the interpolation procedure "may be the reason for the discrepancies mentioned above." Isn't hydrometeor contamination the main reason for the discrepancies? If not, show us!

6) P. 12, 3rd line should reference Fig. 3a, not 2a; same mistake in first line of next paragraph

7) P. 12, near middle: "define rapid intensification" should be "predict rapid intensification"

8) P. 13, first line: "AMSU captures much of the evolution of Dennis…" That looks like an overtstatement, given how choppy those lines are in Figure 4.

9) P. 13, middle and Figure 4: "if this overpass is removed from the time series…"
The text on p. 13 successfully (and inadvertently?) makes the case that some of the data points in Figure 4 should NOT be shown (e.g., those with resolution > ~55 km). At the very least, the line should not be drawn connecting all these data points. Please remove the low-resolution points, or use separate lines to connect the low-resolution points and the higher-resolution points. Using separate lines would make it easier for the reader to see why the text cites temperature changes for two overlapping time periods (0829-2321 and 1947-1144).

10) Figure 4: caption should have a comma after "overpasses". Does the figure show the maximum temperature anomaly regardless of height, or is it for a particular height?

11) P. 13, "Temperature measurements at a resolution finer than that of AMSU…"
Why not show a HAMSR or MTP temperature retrieval? It seems odd to exclude HAMSR from this study - isn't this topic one of the main reasons HAMSR has been on the ER-2 for the NASA hurricane field programs?

12) Section 4 in general: It seems like this section would be more effective if the IR, AMPR, and EDOP descriptions were integrated together, describing the measurements of a particular convective feature all at once. That might cause more harm than good - just consider it.

13) P. 14, last sentence of first paragraph: You are hypothesizing that symmetric distribution of cloud tops and development of a clear eye are aggregate effects of the convective burst episodes. I have no problem

with that as a hypothesis, but the phrasing on p. 14 looks more like an unsupported assertion of fact than a hypothesis.

14) P. 14 and Fig. 5: Since the color scale only goes to 215 K (no warmer), please confirm to the reader that this was indeed a clear eye (not just a warm spot that looks clear with that color table). How warm was the IR eye?

15) P. 14, first line of last paragraph: "ER-2 flight *segments*".

16) P. 15, 2nd line: Cold $T_B$s are already well inside the RMW in Figure 6b, and I even see a light orange shade at the same location in 6a. It looks like convection is strengthening at a location well inside the RMW in this sequence, but not noticeably contracting.

17) P. 15, last 3 sentences: I have major problems with these interpretations of Figure 7.
a) When discussing the brightness temperatures to the south of the flight track, keep in mind that parallax should exaggerate the apparent slope. For a hypothetical vertically erect column away from the flight track, the low-altitude signal (10 and 19 GHz) would be projected to a location closer to the plane than the high altitude signal (85 GHz). Figure 1 is somewhat effective for visualizing this. I'm not suggesting the southern eyewall in this case is vertically erect, just that parallax exaggerates the slope.
b) "eastern eyewall is vertically erect, shown by the near collocation of the 85 GHz and 19/10 GHz signatures" - No… the inner portion of the low-level eyewall (defined by high $T_B$ at 10, 19, and 37 GHz) is clearly to the left (west) of the upper scattering core. This is easiest for me to see in the 19 GHz, but also in the 37 GHz - for both channels, the TB max (from liquid rain) is located inward from the TB depression (from convective ice).

18) P. 16: The 2nd paragraph notes that EDOP only allows 2-d cross-sections through 3-d updrafts, but the bottom paragraph seems to lose track of that point. The "core updraft" separated by a downdraft between 6-8 km altitude can't be a single feature, unless it has spiraled all the way around the eye while ascending from the lower section to the upper section. Is that what you are suggesting? (The same comment applies near the bottom of p. 19)

19) P. 17, middle: "downdraft… at an altitude of 5-6 km was likely enhanced by cooling of air due to melting hydrometeors":
But it looks like that downdraft is centered slightly above the radar bright band… a melting-induced downdraft should be below that.

20) P. 17, 2nd from last sentence of first paragraph: How is it known that errors in fallspeed would not significantly change the structure observed? There is a big difference between liquid and snow fallspeeds - if liquid particles are mis-classified as snow, that would "create" strong downdrafts.

21) P. 17-18, Figure 10: The discussion of zonal wind for this ER-2 pass is troubling because the track is significantly off-center, mixing the tangential and radial components of the huricane's flow. Because of this, some of the diagnosis of converging or diverging airflow and inflow or outflow is dubious. The pass shown in Figures 12 and 14 is effective for this instead. The analysis would be more convincing of you stick to Figures 12 and 14 for this topic, and omit Figure 10 entirely.

22) P. 21, near middle: The eye-eyewall interface is mentioned in the text, but it's hard to identify in these figures - just where should we be looking?

23) P. 21, near bottom: "It is possible that EDOP is sampling the rotational portion of a mesovortex..." Is there evidence for or against this from the other aircraft? Was there a P-3 with a scanning radar that could be checked? If not, was this too far away from the Key West 88D?

24) P. 22, 1st sentence of 2nd paragraph: as mentioned before, this is not a truly vertically oriented eyewall (it is less sloped than the other side, but not vertical)

25) P. 22, last sentence of 2nd paragraph: Nothing was shown to specifically support *neutrality* to slantwise moist ascent. The transverse circulation does not require that. Similarly, some might also argue about the "significant local buoyancy", since no measurements of temperature or density were shown.

26) P. 23, equation 1: Wouldn't an asymmetric component be artificially imposed, simply by having the AMSU footprint and the storm-relative grid mis-aligned from each other? If not, why not? I think this is addressed at the bottom of the page, but it should be addressed when the procedure is first introduced.

27) P. 23, bottom: "Spatial plots of the warm core (not shown)..." I imagine it would be more effective to show a few spatial plots of the warm core instead of Fig. 15. Please consider replacing that figure with the spatial plots.

28) P. 24, 1st paragraph: "This subsidence, *coupled with* the cyclonic rotation of the initial down-shear downdrafts..." How is the cyclonic

rotation of those downdrafts different from what was cited in the previous sentence?

29) P. 24, about Figure 16 should note the HT location (~20-25 km from center).

30) P. 25, bottom: Was the aircraft coverage fairly symmetric, and was the wind field based on those aircraft passes basically symmetric?

31) P. 26, 7th line from bottom: EDOP does not give a "direct measurement of vertical velocity" of the wind. It gives a direct measurement of Doppler velocity of the scatterers, relative to the plane. There is fallspeed uncertainty and removal of aircraft motion needed to retrieve vertical wind.

32) Figure 3: Please add words "solid" and "dashed" to the caption to identify the lines.

33) Figure 6: Is the color scale the same for all? The numbers in a and b are different than c and d.