Review of: "Multi-scale observations of Hurricane Dennis (2005): The effects of hot towers on rapid intensification

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SUMMARY:

The authors present an interesting view of deep convection and very detailed analysis of updrafts and downdrafts within the eyewall of a hurricane. The analysis, for the most part, presents very well. There are a few major concerns, some of which relate to persistent use of the term "HTs" and to the inertial stability arguments. Other major comments relate to the lack of citations from previous research that shows a strong relationship between net core convection and intensity change. Some further analysis is also necessary.

Recommendation: Accept for publication pending major revision.

MAJOR COMMENTS:

1) I question the fixation on HTs vs. just stating that deep convection drives intensification. The amount of deep convection present near the center is an excellent predictor of future intensity (e.g., Cecil and Zipser, 1999). Are the authors claiming that there is something special about HTs that's not true of deep convection in general? Also, the definition of a HT relies strongly upon the "undiluted" core. No observations of theta-e are shown anywhere in this paper, so strong use the term 'hot tower' doesn't seem to be entirely justified.

2) Related to major comment 1, this study makes no mention of the wealth of studies that have found that the net amount of convection (latent heat release) near the center of a TC is very well correlated with its future intensity. For example, the Cecil and Zipser (1999) study mentioned above shows that observationally. Rao and MacArthur (1994) had a similar finding. Other modeling studies, such as the series by Tory et al [starting with Tory et al. (2006)] strongly suggests that *net* heating drives tropical cyclone intensification. Finally, Sippel and Zhang (2008) and their follow-up study, Zhang and Sippel (2009) showed in a model that intensification is strongly correlated to net precipitation. There are numerous other studies that have also shown this, and considering that a key finding is: "We believe that the growth and organization of Dennis' warm core was due to the outbreak of HTs <i.e., deep convection> …", the aforementioned studies are highly relevant.

3) Throughout the paper "upshear rotation" is mentioned as a mechanism by which convection intensifies in the western eyewall. For example, on p. 24 you say: "As the HTs ignite on the down-shear side of the storm, their rotation upshear

stimulates regions of strong vertical motion". The implication is that convection is reaching the western eyewall solely via advection. However, nowhere in the paper is the advective timescale from the eastern to the western eyewall calculated. Is the amount of time that it takes for convection (or a HT) to reach the western eyewall consistent with the tangential velocity and distance/time, etc where convection occurs in the eastern eyewall? Couldn't another process also be acting to ignite convection in the western eyewall?

4) The term "convergence" is often used to describe what is essentially only radially converging air. This leaves the other half – tangentially converging air – uninvestigated. Instead of leaving the reader in the dark on this, the authors should use the vertical profile of w to *show* where horizontal convergence is occurring (i.e., of dw/dz accompanying the plots of zonal and vertical winds would go a long way in this study!). Then, "convergence" can be more correctly used to describe what's going on.

5) I see that inertial stability arguments are presented to relate convection to warming and warming to intensification, but a much simpler approach that gets you to the same place is to look at voriticty. The local time tendency of vertical vorticity is proportional to the amount of vorticity present (ref. the stretching term in the vorticity tendency equation). If you ignite convection in a strong vortex, vorticity *will* grow quickly. The warming and vortex intensification *must* happen in lockstep if the mass field is to be balanced with the momentum field. In my opinion, getting into inertial stability just makes things more complicated than they really need to be, but at the very least the authors should present the momentum (vorticity) framework alongside the mass (temperature/inertial stability) framework. I would also consider shortening or removing the inertial stability framework altogether.

REFERENCE:

Cecil, D. J., and E. J. Zipser, 1999, Relationships between tropical cyclone intensity and satellite-based indicators of inner core convection: 85-GHz ice-scattering signature and lightning. *Mon. Wea. Rev.,* **127,** 103-123.

Rao, G. V., and P. D. MacArthur, 1994: The SSM/I estimated rainfall amounts of tropical cyclones and their potential in predicting the cyclone intensity changes. *Mon. Wea. Rev.,* **122,** 1568-1574.

Tory, K. J., M. T. Montgomery, and N. E. Davidson, 2006: Prediction and diagnosis of tropical cyclone formation in an NWP system. Part I: The critical role of vortex enhancement in deep convection. *J. Atmos. Sci.,* **63,** 3077-3090.

Sippel, J. A., and F. Zhang, 2008: A probabilistic analysis of the dynamics and predictability of tropical cyclogenesis. *J. Atmos. Sci.,* **65,** 3440-3459.

Zhang, F. and J. A. Sippel, 2009: Effects of moist convection on hurricane predictability. *J. Atmos. Sci.,* in press. (Available in early online release form from the AMS website).

MINOR COMMENTS:

1. Mesovortice should be mesovortex – multiple locations in paper.

2. P3, ¶2, Sentence 1 – comma should go between "research" and "particularly"

3. P5, ¶2, "seemingly important features" – it's not clear that hot towers themselves are important. It's clear that the net amount of deep convection is important for TCs, but saying the features are important implies there's something crucial about the features that's not true of other deep convection. See major comment 1.

4. P9, last sentence of first partial paragraph – magnitudes of what?

5. P10, ¶2 – Some elaboration is needed regarding the expected patterns that convection produces in TBs at the different channels. An explanation of why those patterns are observed would also be nice. As is, the casual reader is left somewhat in the dark. It appears some attempt is made to do this on pgs. 14 and 15, but it would make more sense to dedicate more time/space to a more full explanation in the methods section with perhaps a reminder later when the results are displayed.

6. P10, ¶2, sentence 2 – "85 GHz channel *to* 2.8 km" should be "85 GHz channel *and* 2.8 km"

7. P12, end of first partial paragraph – again, what's so special about HTs aside from the fact that they're a manifestation of deep convection?

8. P13, first whole paragraph, sentence 6 and Fig. 4 – This sentence is a little confusing because you're essentially comparing two different resolutions at two different times in the same sentence. To facilitate comparison from one time to the next in Fig. 4, you should consider adding another plot to the figure that shows the temperature anomaly weighted by a factor proportional to the resolution.

9. P13, first whole paragraph, sentence 8 – There is insufficient evidence to make the claim that a higher resolution sensor would observe the same difference in temperature. Regardless, this claim doesn't even seem necessary to the paper, so it should be deleted.

10. P14, paragraph 2 – elaboration would be nice here… see minor comment 5

11. P15, first two sentences – in Fig. 6a and c, the lowest TBs are just *inside* the RMW, not *outside*

12. P15, second sentence: "the TBs" should probably be "the low TBs"

13. P15, ¶2, sentence 3: insert comma between "depressions" and "providing"

14. P17, last sentence of first paragraph: More elaboration is needed here. What is the implication of the subsidence observed in Heymsfield et al. (2001)?

15. P17, ¶2, sentence 3: the current of air could also be natural inflow seen just under the anvil of many TCs… this might be part of the basic-state circulation w/o vertical shear

16. P18, last partial paragraph, sentence 1, suggest rewording to: "In the western eyewall, the 85-GHz low-TB signature is narrower than in previous overpass, and it has little displacement from the 19-GHz maximum." Also, your wording implies that the narrower signature indicates that the eyewall slopes less than in the previous overpass. Is this what you intend? If not, then perhaps you should remove mention of the width of the signature.

17. P20, 1st sentence: I think more examples are needed to make the statement that this is a characteristic of many of the HTs in Dennis at this time. You may be seeing the same updraft twice, or you may be seeing two different updrafts… the distinction is never made in the paper. Regardless, it's not clear that this structure isn't anomalous unless you have more examples that you haven't shown.

18. P20, ¶2. Again, what's the implication of the resemblance to Heymsfield et al. (2001)?

19. P20, ¶2, sentence 4: Where are you suggesting the TB field is uniform? Along the track of the plane there is very strong variance, as the plane goes right through deep convection…

20. P21, third to last sentence: Can you provide any more evidence or explanation regarding why you think this may be a mesovortex? This is warranted, especially since you mention it twice in the conclusion.

21. P24, first whole paragraph: Vortex Rossby waves (VRWs) are cited as being a critical part of the dynamics, but no evidence is given to support this. In fact, the last sentence states that the details are different from the VRW study of Montgomery and Enagonio (1998). Some softening of tone is necessary here.