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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The genesis of Typhoon Nuri as observed during the Tropical Cyclone Structure 2008 (TCS08) field experiment – Part 2: Observations of the convective environment" *by* M. T. Montgomery and R. K. Smith

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Journal: ACP Title: The genesis of Typhoon Nuri as observed during the Tropical Cyclone Structure 2008 (TCS08) field experiment. Part 2: Observations of the convective environment Author(s): M. T. Montgomery and R. K. Smith (hereafter MK) MS No.: acp-2011-674 Comment by reviewer Ed Zipser, University of Utah, USA

This is a worthwhile contribution to the important problem of tropical cyclogenesis, using analysis of the well-observed case of Nuri 2008 in the west Pacific as a case in

point. The authors' main finding is that their analysis shows little change in mean virtual temperature in the low troposphere during genesis, and they use that finding to counter some of the recent thermodynamic control ideas of Raymond and collaborators. This manuscript should be published with minor revisions, after the authors address some of my concerns about their analysis and the generality of their conclusions.

As this comment was being prepared, Raymond submitted his own comment. His major caveat, with which I agree, is that the difference between concluding that the low troposphere is about 1°K cooler near the center, and MK's conclusion of little change, can easily be explained by the different choices of specific analysis domains. Raymond is the right person to continue the discussion with MK about the validity of the Raymond and Sessions (2007) thermodynamic hypothesis, and I will not comment further on that.

My principal concern is with the reliance upon large-area averaging of data with respect to the system-relative center, or pouch. I have no problem whatever with the pouch concept. My problem is that all dropsondes should not necessarily be weighted equally. In Nuri 1, large areas are devoid of significant precipitation, and there are about 9 dropsondes in such areas (Fig. 2a), while in Nuri 2 all dropsondes seem to be in locations with significant precipitation (Fig. 2b). It is not fair to make detailed comparisons without considering the potential differences. In Raymond and Lopez-Carillo (2010) their analyses were constrained in part by the availability of Doppler radar data from the NRL P-3, less available away from precipitation. So it is hardly surprising, as Raymond's comment states, that small differences in choice of analysis regions can lead to somewhat different results.

My principal disappointment with this paper is that it introduces some of the main issues in cyclogenesis but studiously avoids addressing them directly. Near the end of the introduction, they hypothesize that a key "ingredient for genesis is the recirculating flow as it will tend to protect the convectively-generated vorticity seedlings within the critical layer and harbour a favourable environment for vorticity aggregation and moisturization by deep cumulus convection". Yes! How indeed does deep cumulus

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convection moisturize the pouch? Despite one of the best airborne Doppler radar in existence, none of the papers on Nuri have addressed this issue. They are silent on the specific nature of the deep convection and its mesoscale organization, in spite of teasing us in the first paragraph of section 2 that the NRL P3 is capable of documenting this. Instead, they report on area-average properties of the environment, as if "all convection is alike". One obvious change between Nuri 1 and 2 could at least be mentioned; the greater percent coverage of precipitation in all quadrants with respect to the pouch center.

Some more minor comments follow.

It is impossible to see sufficient detail in Fig 3 to distinguish variability of thermodynamic parameters between Nuri 1 and 2. In Fig. 4, the larger variability in RH in Nuri 1 vs. Nuri 2 is apparent, but one may speculate that the 9 soundings taken outside precipitation in the former may account for much of that difference. The same comment applies to the higher RH and theta-e in Nuri 2 in mid-troposphere shown in Fig. 5.

In my opinion, Nuri 3 and 4 comparisons with Nuri 1 and 2 are irrelevant to the purpose of this paper, and inappropriate because they are an obvious and inevitable aspect of the difference between a depression and a strong TC or typhoon. Let me put this strong statement in context. The best track data on Nuri (which would be a useful addition to this paper) show slow strengthening between Nuri 1 and Nuri 2. However, marked intensification is already underway during Nuri 3 that meets several of the criteria for rapid intensification starting between Nuri 3 and Nuri 4. How could there NOT be greater variability among a group of soundings at a range of distances from the cyclone center that has a marked warm core?

The streamline analyses shown in Fig. 2a-c are not very useful because they show only relative wind direction and not speed. So they cannot be compared with similar analyses in papers such as Raymond and Lopez-Carillo (2010).

The footnote on 31119 pours salt into the wound of one of my pet peeves by equating

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organized convection with thunderstorm activity. Not so! The existence and variability of electrification within tropical cyclones is a worthy topic of research, and we should not perpetuate common misconceptions like that one.

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