

Reply to Comment by Zhuo Wang on “The vorticity budget of developing Typhoon Nuri (2008)” by D. J. Raymond and C. López Carrillo

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Reviewer’s comments are inset. Replies are full width.

Z. Wang

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Recommendation: accept after minor revision

This study examines the vorticity dynamics of the tropical cyclone formation using radar data and dropsonde data. The vorticity budget diagnosis identified the spinup and spindown processes at different stages of the storm evolution, and illustrated the important role of a closed Lagrangian circulation: In the precursor disturbance of Nuri, a closed circulation retained vorticity inside and spawned a tropical cyclone; in TCS30, the vorticity flux was exported outside and this wave failed to develop. This is consistent with Dunkerton et al. (2009), Montgomery et al. (2010) and Wang et al. (2010), which showed that the Kelvin cat’s eye within the wave critical layer provides a favorable environment for vorticity aggregation for tropical cyclone formation.

One question that is not answered in this study is how the “pre-existing tropical-depression-scale circulation” forms within the wave disturbance. It was stated in section 4: “Intensification of a tropical cyclone in shear thus takes on the character of a “chicken and egg” problem; in order for spinup to occur, there must be a pre-existing broad distribution of relative vorticity through the low to middle troposphere. However, creation of this vorticity anomaly is difficult without protection against ventilation by the environment.” I would like to point out here that the formation of a meso-alpha scale closed circulation, or the so-called wave “pouch” (Dunkerton et al. 2009), does not rely on the convective processes. Or in other words, a wave critical layer is a kinematic structure of the wave due to the nonlinear interaction between the wave and the ambient flow, instead of being generated by convection (although convection can enhance the cat’s eye circulation). The dry idealized simulations in Montgomery et al. 2010

(ACP, in press) showed that the cat’s eye circulation can develop even without moist processes. In the real atmosphere, the cat’s eye circulation provides a favorable local environment for convection development and is also enhanced by convection.

The reviewer is correct in her comments regarding the non-adiabatic origin of the pouch in the Dunkerton et al. picture. The paragraph in question has been changed to:

“Superficially, the pre-Nuri tropical wave observed during Nuri 1 was similar to the wave seen in TCS030. Both cases appeared as tropical waves in large-scale analyses and both had similar values of shear. However, the TCS030 system did not exhibit a closed circulation at 5 km in our data and the existence of one in the PBL was doubtful, even in system-relative coordinates. This probably explains why TCS030 did not spin up. According to Montgomery et al. (2010), Nuri exhibited a long-lived and durable “pouch” (region with a closed circulation) in the terminology of Dunkerton et al. (2009), whereas the pouch for TCS030 was shallow and marginal at best (Michael Montgomery, personal communication 2010). Thus, Nuri was able to retain its moist core and vorticity over an extended period while TCS030 could not.”

The authors also discussed the displacement or vertical tilt of the tropical-depression-scale circulation induced by the vertical shear. In the schematic in Fig. 20, three motions of different spatial scales are displayed: the system-relative ambient wind, the synoptic-scale wave motion (associated with the broad region of positive relative vorticity), and the tropical-depression-scale circulation. An implicit assumption is that the wave structure and propagation speed do not change with height, which is a reasonable assumption for many waves. On the other hand, some waves do have different structures and propagation speeds at different vertical levels. As shown in Dunkerton et al. 2009, the vertical shear of the ambient flow may help bring a tilted structure to vertical alignment.

Actually, only two motions are displayed, the ambient wind and the wave-scale motion. We have tried to improve the discussion of this point in the revisions we have made to the paper, including a subsection in the new theory section, since this idea has confused so many people. We believe that if the wave-scale vorticity pattern were deforming in response to environmental shear, then the tilt would not be normal to the shear as observed.

The authors also suggested that the overlapping area (in Fig. 20) is likely the location for TC genesis because it is protected from environmental intrusion between the PBL and 5 km. I agree that a closed circulation of sufficient depth, for example, extending from the mid-to-lower troposphere to the PBL, is likely a necessary condition for tropical cyclone formation, but I picture the wave pouch as a material entity of certain depth, which is resilient to moderate vertical shear and can still protect the moist air within even if it has a vertical tilt. When convection becomes more active, the wave pouch will become more vertically aligned, and the center of the closed circulation is likely to fall in the overlapping area, where the vorticity centroid of the (3D) wave pouch is.

We understand the reviewer’s view of the pouch as tilted under the effects of shear. However, if it is tilted so much that there is no protected vertical column, then the convection will penetrate unprotected air at some level and will likely not have the properties needed to intensify the cyclone.

Specific points:

1. P16591: “Postulating a steady state flow in which the tilting term is ignored in the vorticity equation leads”. Is it worth mentioning that this assumption is most likely to be valid in the PBL, where convergence (and thus the stretching term) is strong and vertical wind shear (and the tilting term) is relatively weak?

Actually, since we have calculations for the strength of the tendency due to tilting, we don’t need to make any assumptions in this regard.

2. P16592: “In any case it is this balance, when evaluated in a storm-relative frame, which applies to a steady or nearly steady tropical cyclone if vortex tilting is insignificant.” Can this balance be applied to tropical cyclone development, which is not a steady process?

That is the real question isn’t it? We have added some evaluation of the validity of the steady-state assumption in the PBL and the free troposphere. Balance is approximately satisfied in the free troposphere, but mostly it is not in the PBL.

3. P16593: “As we shall see, this may be a plausible assumption in the very earliest stages, but it is incorrect in the tropical storm stage.” I appreciate the first author’s professional attitude to criticize his own work.

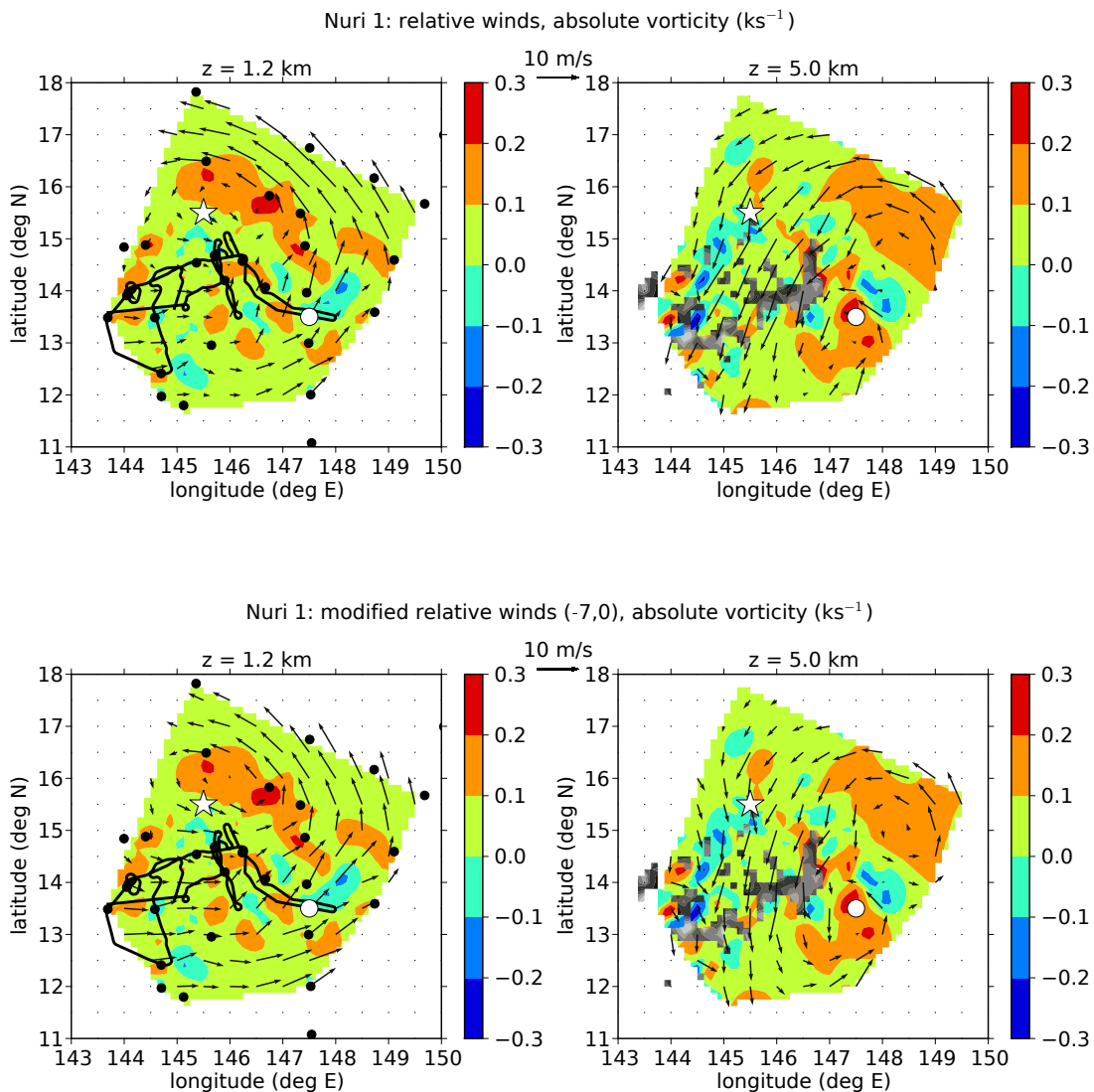
Thanks.

4. P16594: “This supports the hypothesis of Dunkerton et al. (2009) that the region of the wave at the critical latitude is favored for development due to the weak wave- relative winds and the lack of import of dry environmental air at this latitude.” The closed circulation within the wave critical layer is also a region of weak deformation, which favors vorticity aggregation for tropical cyclone formation.

Words to this effect (concerning deformation) have been added to the manuscript.

5. What time does the track start in Fig. 1? At the TW stage or during the first mission, the track shows many zigzags, and the uncertainty of the propagation speed seems larger than $\pm 1\text{ms}^{-1}$. Also note that the propagation speed during mission 1 (table 1) is much smaller than those in the following three missions. If a larger propagation speed were used in Fig. 4, the low-level circulation may be better defined and the 5-km circulation center would shift northward to the more convective region.

The blue dots in this figure are at 6 hr intervals, so working backwards from the first 00Z magenta dot on 16 Aug. puts us at 06Z on 14 August. Two (somewhat) independent tracking methods, IR satellite imagery and FNL vorticity centers yielded similar westward propagation at about 5 m/s for Nuri 1, but I agree that the uncertainty is probably larger than 1 m/s for this first case. We have redone the vorticity plot figure for a propagation velocity of $(-7,0)$ m/s and both figures are shown below. (The white star and circle's locations have not been changed.) The change in the propagation velocity moves both centers to the NNE about 0.6 deg, and in some ways the locations of the new centers provides better correspondence with vorticity features. However, the conclusions drawn about these data are not significantly changed, and we prefer to leave the plots in the manuscript the way they are, but with an expanded discussion, as it is bad practice to adjust data to fit preconceptions.



6. “The 1.2 km circulation centers are listed in Table 1 and shown Fig. 1.” Add period before this sentence.

Done.

7. Fig. 10: As shown in Fig. 4-6, the closed circulation is displaced with height, and the circulation centers are also off the domain center. The storm-generated wind would thus contribute to the averaged wind in the domain. The vertical shear may be overestimated.

Yes, we now mention this in the paper.

8. P16605: “The pattern of vorticity transport in TCS030 is not closed, allowing vorticity maxima in the PBL to be exported from this system.” Fig. 14 actually indicates the presence of a closed circulation at 1.2 km, but a closed circulation is absent at 5 km. This suggests the important role of a closed circulation above the PBL, which can protect the moist air within from dry air intrusion.

We can see the possibility of a closed circulation at 1.2 km, but the northern half of it is missing, so we cannot conclude positively that the circulation is closed at this level. We agree with the reviewer’s comment about the effects of a non-closed circulation at 5 km, and believe that this is key to the lack of development of TCS030.

9. In Nuri 3 (Fig. 17) the stretching term weakens in the lower troposphere compared to Nuri 2. Fig. 2 shows that M2 took place after a convective burst. The weakening of the low-level stretching is probably due to the contribution of the stratiform processes. M1 and M3 also took place during the cloud top warming period. If the flight missions had taken place during an active convection phase, we should see a larger contribution from the stretching term.

We agree, and we believe that the inability to monitor developing systems continuously is a serious observational weakness. Efforts were made in the planning stages of TCS08 to consider night flights, but this was overruled on safety grounds.

10. “In the tropical storm stage (Nuri 3) the PBL inflow was less strong overall, resulting in a net spindown tendency in the PBL” The authors attributed this to a decrease of the spatial scale during the TS stage. However, as shown in Figs. 16-18, area-averaged vorticity keeps increasing. The spindown may be temporary and associated with the time of mission 3 (the decay phase of a convective burst). Again, if the flight missions had taken place during an active convection phase, we should see a larger contribution from the stretching term. The question is how typical the vorticity budget in Fig. 17 is for a tropical storm stage?

We don’t really know the answer to this question, but we have modified the comment about the decrease in scale, indicating that this may just be the result of a temporary change in the character of the convection in the system.