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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 (TCS-08) field experiment – Part 3: Dynamics of low-level spin-up during the genesis" *by* L. L. Lussier III et al.

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We wish to thank the Reviewer (Dave Raymond) for his many perceptive and constructive comments. We have done our best to address each of his substantive points below.

Review of "The genesis of Typhoon Nuri as observed during the Tropical Cyclone Structure 2008 (TCS-08) field experiment – Part 3..." by Lussier, Montgomery, and Bell David J. Raymond December 10, 2013

The authors make a very nice analysis of the first two aircraft missions into developing typhoon Nuri, employing both dropsonde and ELDORA radar data. This follows our similar analysis of this system, presented in Raymond and López (2011; RL11) and Raymond, Sessions, and López (2011; RSL11). Not surprisingly, their analysis is constructed to test their hypotheses about tropical cyclogenesis, which focus on the storm-relative circulation center near 850 hPa ($\approx$ 1.5 km). This differs from our analysis, which focuses on the vorticity evolution in the mid-troposphere ($\approx$ 500 hPa). I will not argue that one or the other of these hypotheses is correct in this review. However, I am concerned that the authors make certain allegations about our analysis and come to certain conclusions that are not supported by the data. I think that the paper is worthy of publication once these issues are addressed.

Major issues

1. The authors promote their axially symmetric analysis centered on the "sweet spot", i.e., the 850 hPa circulation center, as superior to our analysis. It is certainly true that analyzing circulation or tangential wind as a function of radius yields more information than a simple average over a single specified area. The reason for locating the center of the analysis where they did is clear in light of the authors' fundamental hypothesis. However, given the lack of symmetry of developing tropical cyclones, Nuri 1 especially, it is unclear what advantage a circular region of analysis has over one covering (say) the region with high values of vorticity or saturation fraction. The region near the low-level circulation center was particularly asymmetric, as there was heavy convection to the south and clear air to the north. In addition, since the 850 hPa circulation center was located near the northern boundary of the observed region, averages over circles centered on this circulation center encompass large areas void of data, especially for larger radii. This is clear from the insets of dropsonde azimuths in figure 4. In particular the analyses with radii greater than 2 degrees for Nuri 2 and greater than 3 degrees for Nuri 1 lack any data over such large azimuthal angles that it is hard to understand what the averages of tangential wind actually signify. This needs to be addressed in

the paper.

Reply: To a large extent we agree that there is a lack of symmetry in developing tropical cyclones, especially in terms of the distribution of deep convection, moisture, and vorticity. However, we have seen that even when asymmetries exist in the aforementioned mesoscale features, the synoptic-scale wave pouch can be relatively symmetric, as is the case of Nuri (see Montgomery et al. 2010, Nuri Part I; Fig 11). That is not to say that all pouches are symmetric. For instance, a case like Hurricane Sandy exhibited asymmetries in both the convection and vorticity distribution as well as asymmetries in the larger-scale pouch structure (Lussier III et al. 2014). So, for the case of an approximately symmetric wave pouch, as in Nuri, we feel that using circles of varying radii centered on the sweet spot position is defensible. In the case of a strongly asymmetric case, another methodology would be more suitable.

As far as the dropwindsonde distribution is concerned, Nuri 1 appears to be reasonably well sampled in our analysis domain out to the four-degree box (see Fig. 1). For Nuri 2 there is a sampling bias to the south, especially for the larger distances from the sweet spot center (see Fig. 1). Therefore, the tangential wind profiles for Nuri 2 at the outermost radius may not be representative of the broad circulation around the sweet spot (Fig. 4). Given our analysis technique, we somewhat disagree with the reviewer comments and feel that Nuri 1 is well sampled out to the four degree box. The only limitations are in the outer distances of the Nuri 2 data.

We have added the following statement to the pertinent text qualifying this fact:

“As illustrated in Fig. 4 the outermost radius in Nuri 2 has a sampling bias to the south of the sweet spot. While these data are shown for completeness, it is difficult to draw robust conclusions with respect to the system-scale circulation in the outer-most radius of Nuri 2.”

2. A fundamental issue underlying the analysis in this paper is the quality of the variational reconstructions of the flow and thermodynamic fields in Michael Bell’s Samurai

system. In general, the reconstructions produced by Samurai and our 3D-VAR system appear similar, and Bell’s approach using splines may be technically superior. However, one issue in particular that differentiates the two analyses is the differing vertical profiles of mass flux computed for Nuri 1 and Nuri 2. Figure 16 shows the vertical mass fluxes "calculated exactly as in RL11". First of all, what this means needs to be defined; in particular, the area covered by the analysis needs to be specified.

Reply: “Calculated exactly as in RL11” has been edited in the text to read as follows:

“For the vertical mass flux calculation, we broadly reproduced the results of RL11 using our independent 3D-VAR analysis scheme. Specifically, we integrated Equation (27) in RL11 over the observational area as defined in RL11 at each level to produce vertical mass flux profiles (Fig. 16).”

3. Next, we note that the mass flux profile in figure 16 of the current paper for Nuri 1 at low levels is significantly different from those in RL11 and RSL11. Our results in the latter papers show slight downward motion in the lower troposphere. However, Samurai shows upward motion there, albeit weaker than in Nuri 2. Vertical mass flux is a difficult quantity to get right in Doppler radar analysis, so some differences should be expected. However, one bothersome aspect of the Samurai mass flux profiles is the discontinuity in slope at 500 m, i.e., at the top of the first grid cell. Given the smoothness of the rest of the profiles, this suggests a possible analysis artifact in the Samurai system near the surface, produced perhaps by a switch from centered differencing aloft to off-centered differencing in the lowest layer. If there is an analysis error here, it could significantly affect the conclusions drawn in this paper. In particular, since the vertical gradient of the vertical mass flux profile controls the mass convergence (assuming that mass continuity is precisely satisfied) it could impact the conclusion that the vorticity tendency in Nuri 1 was positive near the surface, as opposed to near-zero (RSL11) or slightly negative (RL11). The origin and effect of the kink in the mass flux curves at low levels needs to be investigated.

Reply: The discontinuity in the gradient (and not in mass flux, itself) is just a plotting artifact due to the linear interpolation between averaged discrete levels. Note that the same behavior is evident at 4.5 km altitude in the red line if one looks carefully. There is no change from center to one-sided differencing near the surface. By their nature, the spline analysis and first derivatives are mathematically continuous, and the W=0 boundary condition is enforced by the splines themselves, such that the vertical velocity gradient can be very steep in this region. Analytical tests of SAMURAI (not shown) indicate that the software can correctly diagnose the low-level divergence/convergence to high accuracy given perfect data. The steepness of the mass flux gradient in a real data analysis depends on the amount of horizontal and vertical smoothing and error specifications, which is likely responsible for the differences near the surface between the analyses presented here, RSL11, and RL11. We note that the analysis produces similar results to that of RL11 when averaged over the same observational domain (see Fig. 1 below).

4. The authors go to great lengths to emphasize the differences between their results for the circulation tendency of Nuri 1 at low levels and the results of RL11 and RSL11. The results of RL11 and RSL11 differ in this regard in that the former finds spindown at low levels while the latter finds near-zero circulation tendency there. We note that the RL11 analysis assumed a westward propagation speed of 5 m s−1 while RSL11 assumed 7 m s−1. In retrospect I believe (in agreement with the authors of the paper under review) that the latter assumption is the better one. Results dependent on spatial derivatives can be sensitive to the assumed propagation speed because the relative positions in the moving frame of data obtained at different times change as the propagation speed is changed. The bottom line is that small differences between circulation tendencies in slightly different analysis schemes can occur, and therefore details of these tendencies are not robust. In spite of the uncertainties near the surface in Nuri 1, RL11/RSL11 show that the tendencies aloft in this case were robustly positive.

Reply: Once again, we agree to a large extent with the reviewer's comment. Differ-

ences in phase speed and analysis schemes can lead to small differences in circulation tendency values. We have added a qualifying footnote (#2) along these lines.

We agree also that the circulation tendencies aloft are robustly positive (as seen in RL11, Raymond et al. 2011, and the paper under review). However, understanding the true sign of the tendency in the lower-levels is paramount to understanding how Nuri formed. This topic is discussed in further detail below.

5. The authors' broad assertion that the results of RL11/RSL11 differ significantly from their own results remains unproven because their calculations of circulation tendencies shown in figure 12 are taken over different areas than ours. That the areas that they chose for circulation tendency calculations are somehow "better" than ours depends on their hypothesis that cyclogenesis occurs at the low-level circulation center, an assertion that remains to be proven. The authors are free to analyze the data as they see fit, but comparison of "apples" to "oranges" should not be made.

Reply: One of the initial results we produced during the early stages in our mesoscale analysis of Nuri's spin up sequence was an attempt to exactly replicate Fig. 16 from RL11 (called Fig. 1 below). As illustrated in the below Fig. 1, for the same observational domains the results are unequivocally similar between Fig. 1 and Fig. 16 in RL11: both exhibit a mid-level vorticity maximum and both exhibit an absolute circulation tendency that is nearly zero near the surface. We have performed this calculation to set a baseline comparison between our two different methodologies and to illustrate that our analysis is similar to that of the Raymond group.

By extracting a vertical profile of circulation tendency at one distance from our Fig. 12 (2.0 deg, see Fig. 2 below), we produce a profile very similar to Fig. 16 from RL11. However, as Fig. 2 and our Fig. 12 demonstrate, vertical profiles of circulation tendency at smaller distances from this location produce distinctly different profiles. These results raise the legitimate question of whether a single profile provides the necessary amount of information to describe the cyclogenesis process.

In the present work, we have chosen to analyze the observational data centered on the low-level sweet spot of the parent easterly wave disturbance. The reason for choosing the low-level sweet spot as the key reference point is that the sweet spot is the favored location for minimal shearing deformation and is where local sources or sinks of momentum and entropy are greatly amplified owing to the fact that fluid parcels spend a considerably longer time in such regions compared to those well outside the wave's critical layer (Dunkerton et al. 2009). Evidence supporting these predicted properties has been presented in the current paper, in Montgomery et al. 2010, in Montgomery and Smith 2012, and in Lussier et al. 2014. These works strongly support the hypothesis that cyclogenesis occurs in the region of the low-level sweet spot. By examining the SAMURAI data at variable radial distances from the sweet spot position, we have not only provided insight that is unavailable in the Raymond analysis, but have provided insight that is consistent with several recent papers illustrating the importance of differences in thermodynamic and dynamic structure at radii near the pouch center versus radii farther from the pouch center (e.g., Wang 2012; Davis and Ahijevych 2012). Because Raymond et al. uses only one area for analysis, these differences are not represented in these papers.

Raymond et al.'s analysis is limited by the assumption that their subjectively chosen areas encompass the area where the relevant processes for cyclogenesis occur. If this assumption is not true (which we believe is the case), then they have not provided a complete picture of the cyclogenesis process. Thus, although our comparison may not be a precise "apples to "apples" comparison, our methodology provides new and useful information that is important to understanding Nuri's genesis process, information which is omitted in Raymond et al's papers.

We have added a footnote in Section 4 summarizing these points.

6. The authors challenge our statements in RSL11 that a cold core existed at low levels in Nuri 1. We define this cold core relative to the virtual temperature of the undisturbed tropical environment, as illustrated in figures 2-7 of this paper. This environment was

characterized in RSL11 by the mean sounding from the TCS030 case, a very weak tropical wave, but this sounding is almost identical to that in Hagupit 2 (another weak wave) and to the upstream conditions on the west side of Nuri 1. The core of Nuri 1 was of order 1 K cooler than these undisturbed tropical conditions. We agree that Nuri 2 was not much colder than Nuri 1 at low levels except very near the surface. We also agree with the authors' observations that Nuri 2 was of order 1 K warmer than Nuri 1 in the upper troposphere. However, the difference between Nuri 1 and undisturbed tropical conditions is as important to our analysis as the difference between Nuri 1 and Nuri 2.

Reply: First, we are not strictly evaluating whether the presence of a cold core existed in the low-levels in Nuri 1. We are, however, questioning the assertion of Raymond et al. (2011, Pg. 12) that the ". . . cold core at low levels was signiïňĄcantly enhanced in Nuri 2 compared to Nuri 1". The azimuthally-averaged virtual temperature cross-section presented in our Fig. 14b, along with the work of Montgomery and Smith (2012), certainly provide enough evidence to question the credence of this claim in RSL11.

Second, we do not understand why the comparison with TCS030 is critical. We would argue that a tropical wave (i.e., TCS030) is not an undisturbed tropical condition (see Smith and Montgomery 2012 and Montgomery and Smith 2012 for supporting reasoning).

Third, we agree that there is some weak stabilization on the pouch-scale between Nuri 1 and Nuri 2. However, for reasons discussed in our manuscript, Smith and Montgomery (2012) and Montgomery and Smith (2012), the dynamical role of the stabilization is not clear to us.

Finally, we have shown that low-level stabilization does not exist near the sweet spot (consistent with the findings of Wang, 2012). Furthermore, we have shown that there is low-level spin-up in this region.

Summary: We believe that the foregoing evidence provides a legitimate basis for questioning the necessity of the stabilization in the cyclogenesis process of Typhoon Nuri.

7. The authors discount without supporting evidence the possible importance of small changes in the virtual temperature profile on the character of tropical convection. They also discount the cloud model results of Raymond and Sessions (2007), asserting that the neglect of rotation invalidates these results in the context of tropical cyclogenesis. Perhaps the authors are unfamiliar with the results of Wissmeier and Smith (2011), who show that the effects of rotation on convective dynamics are relatively small for effective Coriolis parameters up to mean values seen in tropical depressions. Assertions about the lack of importance of small temperature perturbations to the character of convection need to be backed up by evidence, especially as we have provided significant observational and numerical evidence to the contrary. (See also Gjorgjievska and Raymond, 2013.)

Reply: As requested by the reviewer, we now provide "supporting evidence" that is distinct from the current work to support our viewpoint that the low- to middle-level cooling observed during Nuri2 may be incidental to the development process. The evidence can be found by consulting Figures 9 and 17 from Braun et al. (2010). These area-averaged analyses provide evidence that the low-level (3-5 km) cooling and upper-level warming (above 400 mb) in the perturbation potential temperature (Figure 9e) only appears when the system-scale circulation is already spinning up. During these times in the developing Gert simulations, there also develops a broad middle-level cyclonic PV anomaly with a weak cyclonic PV anomaly near the surface. In fact, the thermal wind balance constraint for the azimuthally-averaged flow around the sweet spot would suggest that the emergence of a balanced elevated cyclonic PV maximum requires such a cold-core thermal structure! This deduction raises the possibility that the Raymond "theory" may in fact be putting "the cart before the horse" so to speak and misinterpreting an effect of the development for a cause of the development.

The observational evidence we present for Nuri 1 is consistent with the foregoing re-

sults. The salient finding of our work is that the pre-Nuri disturbance was already spinning-up in the low-levels during Nuri-1. Given the cited work by Braun et al. (2010) and the observational evidence presented herein, we question the necessity of the stabilization effect for the formation of Typhoon Nuri.

We discuss the potential issues with the numerical simulations of Raymond and Sessions (2007) in the manuscript.

In response to the reviewer's statement that "the importance of the thermal stabilization has now been established observationally", we would argue that the observational results of Gjorgjievska and Raymond (2014) do not provide significant observational evidence of the importance of small temperature perturbations to tropical cyclogenesis. Specifically, we think that the statistical analysis presented in Fig. 1 of Gjorgjievska and Raymond (2014) examining low-level circulation tendency versus instability index is inconclusive. The reasons for this are the following: i) there is no evidence linking observed stabilization to observed spin-up in this plot. ii) there does not appear to be a way to distinguish between developers, non-developers, or tropical cyclones in this plot. As a result, we do not think this plot reveals the necessary ingredients for cyclogenesis.

As far as the individual case studies presented in Gjorgjievska and Raymond (2014) are concerned, the work of Bell and Montgomery (2014; in preparation) illustrates the cyclical nature of stabilization in the pre-tropical cyclone environment. That work suggests that the changes in stabilization are tied to the convective cycle, which has a diurnal component and regulated by a boundary layer "recovery time" necessary to initiate another round of convection. In our view, the Bell and Montgomery (2014) analyses raise new and interesting questions regarding the role of stabilization in the tropical cyclogenesis process.

Summary. The specific request by Dave Raymond to provide independent evidence has been taken to heart. We have provided plausible evidence questioning the necessity of the stabilization in the cyclogenesis process, but we have not provided sufficient evidence to disprove the role of stabilization. We have adjusted the wording in areas of the paper (P 26820, Line 2; P 26821, Line 28) to reflect these thoughts.

8. The authors' hypothesis asserts that the favored location for cyclogenesis is at or near the above-described sweet spot, and that this is produced by vorticity concentration by deep convection. However, no deep convection was observed at the sweet spot during Nuri 1; the closest convection was about 100 km to the south. Whether convection developed subsequently over the sweet spot subsequently is unknown. An alternative hypothesis might be that the heavy convection to the south caused a new low-level center to develop there and that the old one decayed. Given the modification of the flow, such a development might even have moved the sweet spot to the south. The authors could perhaps look at the time series of geosynchronous satellite infrared data (or other satellite source) to resolve this issue.

Reply: For reasons given below, we believe the reviewer's statement of “Whether convection developed subsequently over the sweet spot is unknown” is unfounded. The convective organization around the sweet spot was demonstrated by Montgomery et al. (2010, Nuri Part I; their Figs. 11 and 13). (This convective organization around the analysis-inferred sweet spot can be demonstrated by viewing any satellite loop from the time period).

[The alternate hypotheses that Raymond presents of a low-level circulation associated with the southern convective burst has been already hypothesized in the literature (Park and Elsberry 2013). However, there has been no published evidence of a robust circulation in the low-levels in the 3D-Var analysis results of RL11, Raymond et al. 2011, or in the currently reviewed work. There is also no clear indication of such a localized southern circulation in the dropwindsonde data presented by Montgomery et al. 2010 or the work currently under review. Lussier III and Montgomery are preparing a short manuscript that addresses the issues of convective organization and the hypothesized presence of a southern circulation that became the locus of Nuri's formation. (We will

be happy to share this mss. with Dave Raymond if he expresses interest.) In our short manuscript (to be submitted), we also address the question of whether the sweet spot moves south to this location. Although the Montgomery et al. (2010) analysis suggested that the sweet spot moves slightly south at 12 UTC 16 Aug (likely a response to the stronger convection in the southern quadrant of the wave pouch), the sweet spot location is relatively robust and does not jump several degrees from one convective burst to another.]

Other issues:

1. Page 26796, lines 20-22: RL11 does not suggest that Nuri 1 as a whole was spinning down. The assertion was made that the boundary layer circulation exhibited a spindown tendency at the time of observation. (This conclusion was later modified in RSL11.) The observed differences in vorticity profiles between Nuri 1 and Nuri 2 suggest that strong spinup was occurring at most levels, with a maximum near 5 km. The ratio of the total circulation to the planetary circulation at the surface between Nuri 1 and Nuri 2 remained unchanged at about a factor of 2, suggesting that the results of RSL11 that friction nearly balanced vorticity convergence at the surface are reasonable.

Reply: Added PBL to sentence.

2. Page 26798, lines 1-3: What evidence favors the assertion that a moistened column "favors a predominantly convective type of heating profile"? López and Raymond (2005) show just the opposite.

Reply: The hypothesis that column moistening favors a predominantly convective type of heating profile was presented in DMW09. The authors have conducted preliminary work using TRMM data that supports this hypothesis (not shown). Additionally, Wang (2012) presents data illustrating that the center of the pouch is more favorable for convective instability. Since this idea is presented in the manuscript as a hypothesis, and the above references do provide some support, we do not feel that a change in wording is necessary.

3. Page 26799, lines 21-23: The absolute circulation at the surface decreased between Nuri 1 and Nuri 2, but this doesn't reflect spindown, as the area enclosed by the circulation loop decreased between the two cases. The key is to look not at the circulation per se, but at the ratio of total to planetary circulation in the figure, which is a measure of average absolute vorticity.

Reply: We have changed the indicated sentence to read as follows: "Contrarily, RL11 show that the vertical profile of absolute circulation in the second Nuri flight (hereafter Nuri 2) revealed a distinct maximum at the 5 km level."

4. Page 26801, lines 5-9: Contrary to the argument about rotation changing the character of the convection, see Wissmeier and Smith (2011).

Reply: With all due respect, we believe that the reviewer is misinterpreting Wissmeier and Smith (2011).

5. Page 26801, lines 26-28: For the record, the thermodynamic control hypothesis was put forth by RSL11 and was not mentioned in RL11. Also, one cannot evaluate any non-trivial hypothesis in meteorology based on one case study.

Reply: We have removed RL11 in this discussion. The thermodynamic control hypothesis is based on physical processes and we have evaluated their applicability for the case of Typhoon Nuri. This hypothesis is not a statistical theory and for this reason we do not feel the need to conduct a statistical analysis with many cases, etc.

6. Page 26807, line 8: Shouldn't the "v" be bold, as it represents a vector?

Reply: Yes, this has been corrected.

7. Page 26807, footnote: The point presented by the authors here is well taken. However, the circulation tendency at a particular radius due to friction is still negative even when the vorticity in parts of the interior is increasing.

Reply: We thank the reviewer for the comment. We do not dispute the strict statement that spin-up in the boundary layer only occurs when convergence of absolute vorticity is larger than the tendency to spin down the flow due to friction. We have clarified the footnote to state that the dynamics of the frictional boundary layer do not act solely as a detriment to spin-up.

8. Page 26809, lines 4-8: This statement is confusing; figure 2 seems to indicate an eastward tilt of the circulation at all elevations.

Reply: This was an unfortunate typo. It has been changed to 'eastward'.

9. Page 26810, line 19: "differentials" –> "derivatives".

Reply: Done.

10. Page 26810, footnote: "data is" –> "data are".

Reply: Done.

11. Page 25814, lines 26-28: "...Nuri 1 is not spinning down, but spinning up in the low-levels." This conclusion depends on the particular area chosen. The area chosen by the authors is different (not necessarily wrong, not necessarily right, just different) than that chosen in RL11 and RSL11. Making such an unqualified statement is therefore not justified.

Reply: There are more areas and larger magnitudes of low-level spin-up up than spin-down in our analysis domain. On the basis of our results, we feel our statement is justified and defensible.

12. Page 26815, lines 23-25: Again, this statement (about low-level spinup) needs to be qualified by the center chosen for the analysis.

Reply: We have added a caveat stating that the Nuri 1 system is "broadly" spinning up.

13. Page 26816, lines 11-13: This statement represents a mis-interpretation of our

results, as noted above.

Reply: Thank you for pointing this out. It was not our intention to misinterpret the reviewer's scientific results. We have reworded our sentence as follows:

"The main difference between these studies is that RL11 show that the circulation tendency below approximately 1.25 km is negative and thus, they conclude that the pre-Nuri circulation is decaying in the PBL during the Nuri 1 mission."

14. Page 26816, line 19: As noted above, the tangential wind calculations from dropsondes for large radii are problematic due to poor azimuthal sampling.

Reply: Please consult our response to Major Point 1 above.

15. Page 26817, lines 21-23: We also interpret the upper level warming (and the lower level cold core) as a balanced response to the spinup (see Raymond, 2012). This does not diminish the potential effect on convection.

Reply: On the contrary, the data we present in Fig. 14 does not indicate the presence of a lower-level cold core. In other words, we do not find evidence that the cold core was "significantly enhanced" between Nuri 1 and Nuri 2.

16. Page 26819, line 18: "thermodynamically" –> "thermodynamic".

Reply: Done.

17. Page 26819, line 18: I am not sure what "thermodynamic processes described in DMW09" are being referred to here, aside from the assertion that moistening results in convection more favorable to cyclogenesis. What is the basis for this assertion?

Reply: We have stated this in the manuscript as a hypothesis. As in response #2, we are working to present evidence to test this hypothesis.

18. Throughout the manuscript: There is a technical problem in rendering accented char- acters that occur in Spanish names, e.g., López, Marín.

Reply: The accent characters have been fixed in the Word Document.

References cited here that are not cited in our manuscript Bibliography:

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Lussier III, L. L., Rutherford, B., Montgomery, M. T., Dunkerton, T. J., and Boothe, M. A.: Examining the roles of the easterly wave critical layer and vorticity accretion during the tropical cyclogenesis of Hurricane Sandy. Mon. Wea. Rev., Accepted with revisions, 2014.

Park, M. S., and Elsberry, R. L.: Latent Heating and Cooling Rates in Developing and Nondeveloping Tropical Disturbances during TCS-08: TRMM PR versus ELDORA Retrievals. J. Atmos. Sci., 70, 15-35, 2013.

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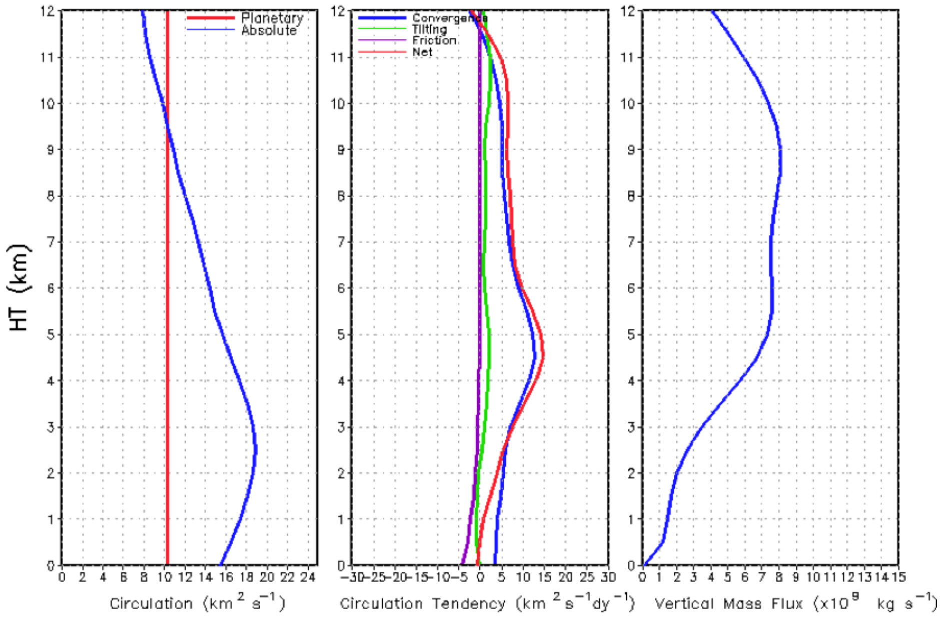

Fig. 1. Three panel plot of circulation (left; planetary [red], absolute [blue]), circulation tendency (middle), and vertical mass flux (right) from the Nuri 1 SAMURAI analysis. The layout of the figure is co

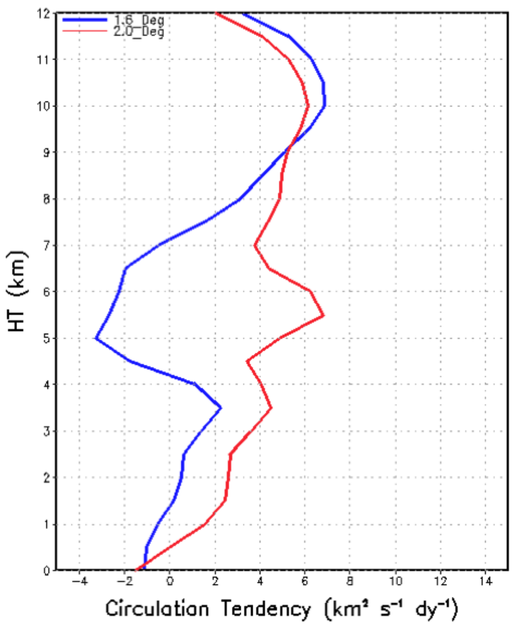

Fig. 2. Profiles of absolute circulation tendency extracted from our Figure 12 at 1.6 and 2.0 degree distances from the sweet spot center.