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.

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 15 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.

3. Next, we note that the mass flux profile in figure 15 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.

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.

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.

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.

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.)

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.

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.

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.

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.

4. Page 26801, lines 5-9: Contrary to the argument about rotation changing the character of the convection, see 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.

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

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.

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

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

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

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.

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

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

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

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.

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

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?

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

References

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