Reply to Reviewer Kevin Tory of "The vorticity budget of developing Typhoon Nuri (2008)" by D. J. Raymond and C. López Carrillo

September 20, 2010

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

> K. Tory (Referee)
>
> An analysis of the formation of Typhoon Nuri is presented using scanning Doppler radar and dropsonde data. The Typhoon formed despite a non-trivial sheared environment. The authors demonstrated that the storm developed in a protected environment consistent with the marsupial pouch theorem. However, in the sheared flow the closed circulations in the lower and middle troposphere were displaced horizontally, but retained some overlap. This overlap was important for the Typhoon development because it defined a deep protected region, within which the Typhoon core developed. The high spatial resolution data collected at three stages of the Typhoon development provided an excellent opportunity to investigate the intensifying circulation. A vorticity budget revealed the dominant system-scale cyclonic vorticity tendencies to be horizontal convergence in the low- to mid-troposphere, and tilting above. The authors used the vorticity budget to demonstrate that the Ekman balance assumption (balance between Coriolis, pressure gradient and friction forces) was inappropriate for the Typhoon boundary layer during a period of intensification. Comparisons were made with a non-developing system.
>
> General comments: The study is thorough and puts the available data to good use. While there is nothing surprising in the results, the study is very important for supporting or confirming theories arising from contemporary modelling studies (e.g., marsupial pouch, the role of deep convection).
>
> The conclusion regarding the appropriateness of the Ekman balance assumption during a period of strong development is also not surprising. If the BL circulation is intensifying, and the only term contributing to a cyclonic vorticity tendency is convergence, then the convergence must exceed the friction term. Perhaps there could be some commentary on the possibility of near balance at other times, for example Figs. 15 and 18 show the two terms are of similar order of magnitude.

The difference probably falls within the factor of two uncertainty range regarding the friction magnitude. Such balance would be more likely in a mature storm of steady intensity.

Figure 15 seems pretty convincing to me that there is spindown near the surface, a result that wouldn't be changed by a factor of 2 error in the friction term. I have added a comment that the circulation tendency in figure 18 is slightly positive, but indistinguishable from zero given the potential errors in the surface friction term.

Specific comments: Only the last three digits of the page numbers will be listed below.

1. P 590: Eq. 1 appears to be constructed in geometric coordinates (x,y,z), in which case the baroclinic term should probably be included for completeness, as well as a sentence or two to justify its neglect in the budget. Also it is probably worth mentioning which equation from HM87 was used to derive Eq. 1.

I have added a separate theory section in response to comments from Roger Smith and I have included the baroclinic term for completeness. HM87 do not discuss the geometric coordinate version of the vorticity equation, only the isentropic and pressure coordinate versions, so I cannot point to a specific equation. However, their paper clearly describes the substance of what we do here.

2. P 591, L17: When describing the Ekman balance it might be worth including the centrifugal force, given the tropical cyclone application.

In response to Roger Smith's comments I have omitted any discussion of the Ekman balance equation as he convinced me that it is tangential to what we are doing.

3. P 592, L1: Strictly speaking the "divergence of absolute vorticity" described here is the "horizontal divergence of the horizontal flux of the vertical component of absolute vorticity".

I agree that this was sloppy terminology. It has been superceded by the discussion in the new theory section.

4. P 593, L1-7: You could quote Tory and Montgomery (2006, IWTC-VI report) and/or Tory and Frank (2010, p76, Global perspectives on tropical cyclones Volume 2) in which this process is described and illustrated schematically.

My editorializing on Bister and Emanuel was criticized so heavily by Smith and Montgomery that I decided to omit it in favor of a much more limited statement on this paper.

5. P 599, L1-2: It might be worth quoting the logarithmic wind profile to justify the conclusion that using CBLAST results valid at 10 m height produces a slight overestimate for the surface stress at a level more than 60 times higher.

I believe that the logarithmic profile is an idealization which doesn't apply quantitatively in real, convective, possibly baroclinic boundary layers.

6. P 600, L 4: Has FNL been defined yet?

It is defined on p 598, line 8.

7. P 601, L 10: Just a comment on style. The sentence reads as if the authors were surprised that TCS030 did not develop after passing over warmer water. (Given the vorticity distribution I'm sure they would have been surprised if it did develop.)

I prefer the sentence the way it is, as it sets the stage for the later vorticity argument.

8. P 602, L 2: "..east and southeast sides of the disturbance." I found this location hard to understand at first. It might be less ambiguous if the enhanced vorticity location was described with respect to the 5 km circulation centre rather than the surface centre.

Done.

9. P 603, L 25: It's not immediately obvious what "this" at the end of the line is referring to.

I have resolved the issue by removing "in this case".

10. P 604, L 6: Begin the sentence with "Figure 10. . ." to avoid ambiguity.

Done.

11. P 604, L 7: Explain how the 7 m/s shear was determined. The shear in Nuri 1, 2 and 3, appears to be about 3, 7 and 9 m/s respectively.

The paper indicates that the shear was taken between the surface and 6 km and re-examination indicates that 7 m/s is a reasonable estimate for Nuri1 and Nuri 2. The shear is greater for Nuri 3, but we now indicate that this estimate is unreliable due to the strong influence of the cyclone itself.

12. P 604, L 17-19: Haynes and McIntyre recommend not splitting the advective flux into these terms, due to cancellation between the terms. So it might be worth explaining that while mathematically more correct, it is physically impractical, because the advection contribution can be very noisy, particularly when you have numerous vorticity anomalies in the circulation.

We have expanded our statement slightly in response to this comment: "The stretching tendency $-\zeta_z \vec{\nabla}_h \cdot \vec{v}_h$ is shown rather than the total tendency because stretching is the main mechanism by which parcel values of vorticity are increased, at least in regions of small tilting tendency. The vorticity advective tendency $-\vec{v}_h \cdot \vec{\nabla}_h \zeta_z$ exhibits complex patterns which are irrelevant to the parcel increase in vorticity since advection simply moves parcels around without changing their vorticity. Of course the advective contributions are needed to compute the overall circulation tendency given by (XXX) since they can move parcels in and out of the circulation domain."

> 13. P 605, L 8-9: The lack of PBL stretching could also be due to a lack of vorticity to be stretched. However, in this case the PBL stretching tendency is negative in the vicinity of the convection, which suggests that the mass flux is downward, giving rise to vortex "squashing" rather than "stretching".

A re-examination of the TCS030 case indicated that our statement here was inaccurate, and it has been omitted. By the way, in some of the PREDICT dropsonde flights we see clear evidence of "squashing".

> 14. P 605, L 15: The discussion on closed vorticity flux lines is useful and a nice way to illustrate the vorticity containment by the system. But I do not think the figures contain enough of the circulation to conclude with certainty that the vorticity flux lines are closed for the Nuri examples and open for the non-developer. A few more sentences would be required to justify the conclusion that the curved field of TCS030 PBL vectors is open and that of Nuri 2 is closed.

We have revised the statements regarding closure of the vorticity flux circulation as follows: "Circulations of vorticity flux in the PBL in Nuri 1 and 2, as seen in figures 12-14 [new figure numbering] appear to be closed, but the observed regions are too restricted to say for sure. At 5 km the Nuri 1 circulation also suggests closure. For Nuri 2 at 5 km and Nuri 3 at both altitudes the vorticity flux circulation is clearly closed. These results suggest that regions of strong vorticity created by stretching are not exported from Nuri during its growing stage. The pattern of vorticity flux in TCS030 in the PBL may be closed, but observations do not extend far enough to the north to verify this. At 5 km there is no hint of a closed circulation in TCS030, allowing export of generated vorticity at this altitude."

> 15. P 605, L 24: Mention the different scales between Figs. 11-14.

Done.

> 16. P 605, L 25-27: The "However" in this sentence implies some connection with the previous sentence, which I think I am missing.

This sentence has disappeared as a response to previous comments.

17. P 606, L 9-10: It might be worth splitting this sentence into two, because only the latter half relates to the next sentence, which is connected by the conjunctive adverb "thus".

This discussion has been modified.

18. P 606, L 20: Add "comparatively" after ". . .of tilting is". The tilting term is actually larger at low levels in Nuri 2 than 1.

Done.

19. P 608, L16: Replace "explains" with something like "gives a possible explanation for".

Done.

20. P 608, L 27-28: The conclusion here and in the abstract is that the overlapped pouch is important because it gives a full column deep protected region from the PBL to 5 km. It might be worth noting that this is also where convection is likely to be favoured, due to the isentropic upslope flow tendency towards the down-tilt direction of a tilted vortex.

We have strong doubts as to whether such quasi-geostrophic effects are significant at these latitudes, so we prefer to omit this comment.

21. P 610, L 5: While the conclusion that TCS030 did not spin up because it did not have a closed circulation is probably true, one could also argue that there just wasn't enough vorticity in the vicinity.

Of course, these two arguments are linked, in that there may have been a closed circulation were there more vorticity.

22. P 617: What does the colour scale refer to? Replace "ar" in the caption with "as".

The color shading indicates the number of pixels at the given temperature and time. A statement to this effect was added to the figure caption. Typo fixed.

23. P 619: Are the units correct?

Do you mean for the absolute vorticity? Yes, I believe they are – inverse time, kiloseconds to the minus one.

24. P 622: What are the vorticity units in Fig. 7?

The vorticity units are kiloseconds to the minus one. We have added this to the figure.

25. P 626: Remove "in" from the fourth line of the caption.

Done.

26. P 630: It might be worth commenting on the negative mass flux at low levels. Also how is the mass flux normalized?

Comment made. The mass flux (mass/time) has been divided by planetary circulation (area/time) along with the other plots in the figure. The detrained mass flux (mass/height/time) and the detrained volume flux (volume/height/time) have been treated similarly.

27. P 633: Change caption to read ". . .over the 2 degree square box illustrated in Fig. 6 centered. . ."

Done.