Review of

The vorticity budget of developing Typhoon Nuri (2008)

by D.J. Raymond and C. Lopez Carrillo

This review refers to the originally submitted discussion paper and does not take into account the authors' responses to previous review. It is likely that some of my comments are addressed by these responses.

In this manuscript, the authors use ELDORA radar and dropsonde observations collected during TPARC/ TCS08 to document the vorticity structure and vorticity tendency terms, as well as vertical mass flux profiles, during the development of what became Typhoon Nuri (2008). A brief comparison with a non-developing system is provided. To my knowledge, this is one of the most detailed vorticity analyses of a developing tropical cyclone (TC) from observations. The manuscript is of great scientific interest.
Section 3 and accompanying figures present the results of the vorticity analysis in a clear and concise manner. The vorticity analysis yields dynamical insight into the genesis of Nuri. The manuscript, however, could benefit considerably from a revision of the introduction, with respective modifications in other sections. The authors should also present a clearer and more complete discussion of how their results compare with, and are related to previous ideas of TC genesis.

I recommend accepting the manuscript after minor revisions.
With kind regards,

Michael Riemer (signed)

General comments

In accordance with most other reviewers, I cannot follow the authors' focus on Ekman balance, or the more general idea of vorticity balance in the boundary layer. Smith et al. 2009 demonstrate that the *im*balance of the frictional inflow layer plays an important role for the spin-up of the inner core of the TC vortex. It is of interest that Ooyama's (1969) TC spins up gradually even with an inflow layer in Ekman balance. Still, to me it does not seem to be a reasonable approach to focus *a priori* on the cancelation between terms in a tendency equation for a *rapidly developing* system. The motivation for this approach needs to be revised.

The authors do not assess the degree of Ekman balance in this manuscript but their focus is on the vorticity balance, equation (3). I find it unnecessary and confusing rather than helpful to introduce (3) via the concept of Ekman balance. As the authors mention, Smith and Montgomery (2008) have actually shown that Ekman balance is not a valid approximation in the inflow layer of a TC. No clear connection between Ekman balance and (3) is provided in this manuscript. I suggest that the authors introduce (3) simply as the competition between frictional spin-down and spin-up by stretching. It is these competing processes that are evaluated in this study. In my opinion, the reference to Ekman balance could be dropped completely.
I agree with the first paragraph on page 16594 in the sense that it is a crucial question how convection in a developing system is controlled/ organized. It seems, however, incorrect or at least incomplete that 'if vorticity balance does not hold, we must seek other mechanisms' (than frictional convergence) 'by which the convection is controlled'. For a mature, intensifying TC we do *not* expect vorticity balance in the frictional inflow layer, according to Smith at al. 2009. Yet it is hard to argue that the well-defined eyewall convection of mature TCs is not controlled by frictional convergence. It is unclear why frictional convergence requires vorticity balance to organize convection. The authors need to clarify their line of argument here.

References to ideas of Ritchie and Holland, and Bister and Emanuel on pg16592/ pg16593:

1) mid-level vortex as precursor: Later, you consider it curious to find the maximum of the circulation at low-level, as can be expected for easterly wave disturbances. Is it of importance/ interest for your analysis and results to refer to the 'mid-level vortex as precursor' idea? Why do you not refer to Dunkerton et al.'s "marsupial" theory at this point also?
2) Ritchie and Holland's mix of PV and vorticity thinking: To me, this is a loaded mix! If stratification were taken into account (PV thinking) then one needs to explain the formation of the low-level *PV* anomaly that forms during genesis, not merely the circulation associated with a PV anomaly by action-at-a-distance. Yes, a mid-level PV anomaly *may* induce a low-level circulation. Yet it is a long way to build the TC's PV monolith from there. Either PV thinking needs to be applied for all levels of the TC, or one simplifies the problem, assumes a barotropic vortex and thinks in terms of vorticity.
3) The Bister and Emanuel hypothesis needs more explanation because the idea that a cold anomaly may serve as the incubation region for warm-core development is counterintuitive. Furthermore, it would be helpful to point out that vertical advection of vorticity is not a viable concept (as indicated by the flux form of the vorticity equation).

I do not see how your manuscript benefits by reference to these ideas, other than for the sake of completeness. In the current version of your manuscript, dropping the respective paragraphs would make the paper more concise without loss of importance information, i.e. improve the manuscript. Alternatively, and preferably, I suggest that you refer to these ideas as valid hypotheses that can be partially tested by your analysis. In the discussion or conclusion section you should then evaluate the hypotheses based on your results.

The authors mention that generation of vertical vorticity by tilting on the vortex scale is of secondary importance. The segregation of vorticity dipoles that have been generated by tilting on the *convective* scale, however, has been proposed previously to be important for TC genesis (e.g. Montgomery et al 2006). I wonder how well your budget, at the resolution available, is able to capture this segregation process. The authors might want to comment on this point.

I have a hard time following your ideas about the vertical structure of the disturbance as summarized in Fig. 20. You propose that the circulation center at low and mid-level emerge by the "vector sum of the system-relative ambient wind and the induced circulation associated with the wave-scale region of positive relative vorticity". You emphasize that this mechanism is different from that of a tilted vortex (references to Jones on pg 16609). Your "explanation depends on the existence … of background vorticity … over the region of the wave".
The latter statement is particularly unclear to me. It seems to me as if you postulate a wave-scale, coherent circulation and associated vorticity that extends from low- to mid-levels. Do you assume that this circulation is associated with the precursor wave? Or is it the signature of the vertical expansion of the circulation as one stage towards genesis, similar to the evolution of pre-Karl in the Atlantic this year?
Either way, why should the broad (wave-scale) vorticity pattern not interact with vertical shear in a similar way as proposed by Jones, and also by Reasor and Montgomery (2001) and Reasor et al. (2004)? Reasor and Montgomery (2001), in particular, considered broad, weak vortices in vertical shear; therefore their theory should be valid during the genesis/tropical depression stage of a TC. From their results, and also from Jones and from Reasor et al., one would expect that the broad circulation tilts to the left of shear with height and forms the circulation pattern that is observed in Nuri. While you present a new explanation based on flow *kinematics*, I would claim that the vertical structure could readily be explained by previous concepts considering the *dynamics* of broad, weak vortices in vertical shear. More justification/ motivation is needed as to why a new conceptual model should be necessary in this case.
You suggest that the "overlap" region of the low- and mid-level circulation constitutes the preferred region of genesis. Regardless of the reason for the left-of-shear displacement of the mid-level circulation: How is this proposed genesis location related to the ("marsupial") theory of Dunkerton et al 2009 that predicts that genesis should take place at the intersection of the critical latitude and the trough axis of the precursor wave?

Further comments

Abstract:
1) typhoon intensified rapidly → disturbance developed rapidly

2) unclear what is meant with “convective sources of boundary layer vorticity”

L20, pg 16590: “clarify budget”? Do you mean “calculate”?

L9, pg16592: Zehnder (2001): The reader might wonder what these implications are

Pg16591/pg16592: at this point it is not clear why it should be valid to ignore the tilting term

L1, pg16598 please define a2

L6, pg16599: chosen *to* represent

L24, pg16601: shown *in* Fig.

L25, pg16601: Nuri 1, please define (also Nuri 2 and Nuri 3)

L16-L19, pg16603: I do not understand this statement

pg16603/ pg16604: Vertical wind shear on Nuri, Figure 10 and discussion in text:
As the authors note, the vertical shear derived from the data in the vicinity of the developing system may represent shear due to a vertical tilt of the system (“environmental flow contaminated by the storm generated winds”). Is there evidence in the FNL data that there was indeed *environmental* vertical shear on Nuri during the period under consideration?

Pg 16609, last paragraph: mass flux profiles:
These wind profiles are derived from a snapshot of the vertical motion field in a part of the recirculation area. The profiles depend on the phase of the life cycle during which the individual convective elements are sampled. I wonder how representative these profiles are for the respective stage of the development. Can you comment?

References

Reasor, P. D. and Montgomery, M. T.: Three-dimensional alignment and corotation of weak, TC-like vortices via linear vortex Rossby waves, J. Atmos. Sci., 58, 2306–2330, 2001.
Reasor, P. D., Montgomery, M. T., and Grasso, L. D.: A new look at the problem of tropical cyclones in vertical shear flow: Vortex resiliency, J. Atmos. Sci., 61, 3–22, 2004.
Smith RK, MontgomeryMT, and Nguyen SV. 2009 Tropical cyclone spin up revisited. *Q. J. R.Meteorol. Soc.*, **135,** 1321-1335.