Reply to Review by Michael Riemer on The vorticity budget of developing Typhoon Nuri (2008) by D. J. Raymond and C. Lopez Carrillo

September 23, 2010

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

> 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.
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> 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.
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> 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.

The introduction has been extensively revised and a new theory section has been added, as reviewers have universally found the original presentation to be obscure. There is surprisingly little previous work on TC formation. We have tried to discuss all work in this area that we know about.

> 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 imbalance 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.

We have removed all reference to Ekman balance and have rewritten and expanded our description of vorticity balance and related ideas. Everybody was confused by our presentation in the first draft! We still believe that more needs to be understood about the control of convection in developing (as opposed to mature) cyclones. However, this topic will be covered in our next paper.

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?

What an embarrassing oversight! A discussion of Dunkerton et al.'s work is now added. The curiosity here derives from the conventional wisdom that (at least) African easterly waves come off of Africa with the strongest circulation at middle levels.

> 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.

Agree.

> 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 no 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.

We will have more to say about Bister and Emanuel in a later paper about thermodynamic issues in tropical cyclogenesis. Since there are some confusing issues here, we wish to keep our commentary to a minimum at this point.

> 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.

My discussions with Michael Montgomery at this point suggest that this segregation process is secondary in importance to bulk vorticity convergence. Nevertheless, our analysis will pick up this process if the vorticity dipoles are resolved by the 0.125 degree grid. We have not tried to separate out this segregation process, but my visual impression is that it is not very important in the Nuri case.

> 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?

Yes.

> 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.

The problem with Reasor and Montgomery (2001) is that the calculation is totally adiabatic, and therefore not very applicable to a highly convective tropical wave. Somehow these waves manage to maintain their structure and the convection probably plays a significant role in this process. We have added clarifying material in the new theory section of the paper.

> 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.

We have tried without success to extract a simple bottom line about the relationship between tilt and shear from Reasor and Montgomery (2001). Reasor and Montgomery's later paper about stronger vortices says that tilt is left-downshear, which is different from just left. However, this result is unlikely to apply to early Nuri stages. The results of Jones are similarly complex. Our simple model is relatively easy to understand (at least with the new theoretical discussion) and gives the right answer for Nuri.

> 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?

The two ideas are closely related. Our work adds support to the Dunkerton hypothesis as we indicate in the last line of the paper.

Further comments

Abstract:

1) typhoon intensified rapidly –> disturbance developed rapidly

Done.

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

How about "As Nuri developed, convective regions of boundary layer vortex stretching became fewer but more intense, culminating in a single nascent eyewall at the tropical storm stage."

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

This wording has vanished in revisions.

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

The reference to Zehnder has also vanished.

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

This has changed in the revision process. However, we have to make no assumptions about the tilting term as we determine it from observations.

L1, pg16598 please define a2

This has been addressed in response to other reviewers.

L6, pg16599: chosen to represent

Fixed.

L24, pg16601 shown in Fig.

Fixed.

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

Done.

L16 L19, pg16603: I do not understand this statement

We are not sure what needs clarifying.

> 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?

We haven't looked directly at the FNL, but the forecasters in Monterey during the project certainly thought that there was significant shear, based on model output. The computed shears for Nuri 1 and Nuri 2 probably aren't too distorted, as the system was weaker and the average covered the system more symmetrically than for Nuri 3.

> 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?

Given the possibility of the rapid evolution of the convection field, this is a non-trivial concern. However, the fact that the changes in the circulation profiles from one day to the next are roughly what is expected from the observed mass flux profile snapshots is comforting. The circulation is the long-term memory of the effects of convection on system!

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.

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Smith R K, Montgomery M T, and Nguyen S V. 2009 Tropical cyclone spin up revisited. Q. J. R. Meteorol. Soc., 135, 1321 1335.