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

September 19, 2010

Reviewer's comments are inset. Replies are full width. Note that the reviewer's page references appear to refer to the page previous to that at which the respective comments are directed.

> Recommendation: Reconsider after major revision
>
> General comment
>
> This paper presents an analysis and interpretation of airborne Doppler radar data collected during the development of Typhoon Nuri (2008) in the Western North Pacific. The data give an unprecedented detailed picture of the fine-scale vorticity structures associated with deep convection during the development of Nuri and their analysis supports broadly the predictions of recent theoretical studies demonstrating the important of such vortical convective structures in the intensification process. These analyses are important and should be published.
>
> The main weakness of the paper is its focus on so called "Ekman balance" in the interpretation of the observations. In my opinion, the concept is not adequately explained in the paper and its relevance to interpreting the results is obscure (at least to me). I would encourage the authors to rethink the theoretical part of their paper in the light of my comments below.

General reply: We agree that the emphasis on "Ekman balance" is misplaced, in that it is an approximation to the full "vorticity balance", which is used in the diagnosis of data in the paper. There also seems to be considerable confusion among all the reviewers about the concept of vorticity balance as defined in this paper. The only approximations made in deriving vorticity balance are the steady state condition and the minor (in our case) approximation of ignoring the vertical component of the baroclinic generation of vorticity. In order to clear up misunderstandings about the concept of vorticity balance and related issues, we have included a new theory section on the flux form of the vorticity equation from which this concept is involved, and on the divergence equation written in a special form which highlights its relationship to the vorticity equation.

Regarding the broader question about the degree to which the flow is balanced (in the sense of vorticity balance) both in the boundary layer and in the free troposphere, We still believe that this is issue is worth documenting. The reviewer and his colleague Mike Montgomery have convincingly shown that most ordinary balance conditions are not well satisfied even in a steady-state hurricane boundary layer. Since no significant approximations are made beside steadiness in deriving the vorticity balance condition, this criticism does not apply to vorticity balance!

We also maintain that the quasi-steady assumption (i.e., gradient wind balance in axisymmetry above the boundary layer) is open to question, particularly for weaker systems such as tropical waves and tropical depressions. After all, such systems frequently intensify as a result of convective bursts, and the degree to which these systems maintain balance during these bursts seems to us to be an interesting issue. It certainly pertains to the question of whether gradient wind balance (in axisymmetry – nonlinear balance otherwise) is even approximately maintained *in the free troposphere* during rapid intensification. This gets to the heart of the dynamics of intensification. We did not address this question in the first version of the manuscript, but plan to do so in the revision. As it turns out, our results seem to indicate that imbalance above the boundary layer is not strong, at least in a system-wide average sense.

> Specific comments
>
> P16589 Eq. (1): Is the density not important in this equation? Whatever, the matter requires comment.

I assume that this refers to the baroclinic generation term, which we now include for completeness.

> P16590 L12: The statement that "Advection does not change the magnitude of vorticity in a parcel" is unclear. It appears that you are talking only about horizontal advection, in which the statement doesn't hold in general.

It is hard for me to see how this could be made more clear. For any quantity χ that obeys an equation like

$$\frac{d\chi}{dt} = \frac{\partial \chi}{\partial t} + \vec{v} \cdot \vec{\nabla} \chi = S_\chi,$$

the parcel derivative $d\chi/dt = 0$ when the source term S_χ is neglected, which means that parcel values of χ don't change. This is true whether the advection is 2-D or 3-D. Two-dimensional advection is assumed in our form of the vorticity equation because vertical advection is identically zero in this equation.

> L16: Ooyama (1969) assumes that the boundary layer is in gradient wind balance. This is different from an Ekman layer.

Agreed, and fixed in the revised theory section.

> L27: I would say that "clouded" is more appropriate than "finessed". The vorticity equation alone is not "equivalent to" the primitive equations. One needs to consider the divergence equation as well to get the big picture. Smith and Montgomery (2008) considered both radial and tangential components of the momentum equations.

Agreed; we now also discuss the role of the divergence equation.

> P16591 Eq. (3). There is a lot buried in the sole use of this equation. In an axisymmetric flow configuration, for example, it contains only information about the tangential momentum. In fact, it is just the radial derivative of the tangential momentum equation divided by the radius.

The vorticity and divergence equations together are equivalent to the momentum equation. However, to the extent that the flow can be approximated as being in some sense balanced (i.e., time derivative of the divergence can be ignored), the divergence equation becomes a diagnostic for the pressure – the full vorticity equation becomes the only prognostic equation in the system, and the "slow" parts of all variables, including the quasi-balanced parts of the divergence and vertical velocity (think Sawyer-Eliassen equation as an example), can be derived from it. Thus, there is some justification in considering the vorticity equation (or the potential vorticity equation) as being the prime equation in quasi-balanced cases.

> L4: The concept of "vorticity balance" introduced here is incomplete and, I would argue, misleading, without considering the role of the radial momentum equation. The latter cannot be ignored in discussing vortex boundary layers. Indeed, it is equivalent to the divergence equation in axisymmetric geometry. Vogl and Smith (QJ, 2009) carried out a scale analysis for the vortex boundary layer and showed that the linear approximation to the boundary-layer equations terms is poor in the inner core region of a tropical cyclone. The concept of "vorticity balance" as applied here needs to be justified in terms of a similar scale analysis.

Yes and no. Please see the previous discussion concerning the vorticity equation. Also, the vorticity balance condition is an exact steady state equation and needs no scale analysis aside from one indicating how weak the time tendencies have to be to justify a quasi-steady assumption. This we have added to the theory section.

> L8. The relevance of vorticity balance to the problem at hand needs to be explained in detail.

Again, see above comments.

> L16: What, precisely, does "the initiation of the cyclone heat engine" mean? Emanuel's (1986) paper, which is cited here, is a steady-state theory. It does not discuss "initiation".

We have eliminated this slightly contentious statement.

> L27-28: The authors cite Bister and Emanuel's idea "that downdrafts associated with the Mesoscale rain areas advect the mid-level vortex downward, thus increasing the low-level vorticity", but it is not clear whether they subscribe to this view. From a vorticity perspective, vortex lines would be compressed also, an effect that would oppose the advection. Axisymmetric dynamics would tell us that low-level divergence would lead to a weakening of the surface vortex because of the generalized Coriolis force.

This paper seems to excite strong reactions, and it is hard to know what to say about it as the authors' statements may either be viewed as consistent with GFD or not, depending on how they are interpreted. We now limit our statement to "Bister and Emanuel argue that the development of a cool, moist environment resulting from stratiform rain serves as the incubation region for the formation of a low-level, warm-core vortex.", omitting any reference to their somewhat controversial interpretation of their numerical model results. We believe that this hypothesis of Bister and Emanuel is valid and interesting.

> P16592 L25: I think what you are saying here is that you can't obtain a complete picture of what is going on without invoking the divergence equation (or radial momentum equation). Nevertheless, a more detailed discussion of the limitations of "vorticity balance" is called for to make the results of the paper intelligible.

No, I am simply invoking Stokes' theorem.

> P16592 L8-9: The question is: are there any good reasons to believe that "vorticity balance" might be a valid approximation? Is it even worth testing? What is the basis to assume that boundary-layer convergence might be predicted "by this approximation", by which I assume the authors mean that the boundary-layer inflow might be predicted using the tangential momentum equation and not the radial momentum equation. Is this idea worth testing? At least a scale analysis should be carried out to show this.

There is no need for a scale analysis, as vorticity balance is obtained from the steady state primitive equations with no approximation. The question about the validity of vorticity balance is really a question of whether the steady state assumption is justified.

> L10: Do you mean by "other mechanisms" that radial convergence might control the convection? What other mechanisms would be conceivable?

If convection controls convergence rather than the other way around (as we deem the more satisfactory approximation in most tropical conditions, excluding the core of mature cyclones), then alternate mechanisms must be sought. An extended discussion of this is beyond the scope of the paper, but surface fluxes, saturation fraction, and convective inhibition are likely candidates.

> P16592 L13: "took off" might be better than "launched". The P3s aren't space ships. Also "returned" might be more accurate than "recovered"!

We have changed to "departed Guam" and "returned".

> P16597 L1. I couldn't find where a2 is defined, but it needs to be.

We don't want to go into details of the analysis here, but we have included the statement "(a measure of the quality of the geometry for dual Doppler analysis)" in addition to the Lopez-Raymond reference, which should become available shortly and which explains this parameter in detail.

> P16598 L6: How is this average "depth defined"?

Here is a slightly expanded statement in the paper regarding the depth of the boundary layer: "...a scale height of $z_s = 1.25$ km is chosen represent the average depth of the planetary boundary layer (PBL) in tropical regions. The postulated scale height is consistent with the idea that surface friction is mixed through the full PBL, i. e., the layer containing boundary layer clouds as well as the sub-cloud layer, via turbulent eddies. Unfortunately, not enough is known about boundary layers topped by convective clouds in developing tropical storms to justify a more refined estimate of the vertical distribution of the drag force resulting from the surface stress. Given these uncertainties, $\vec{F}$ is probably known to within only a factor of two. However, this accuracy is sufficient to draw some significant conclusions, as noted below." Note that we are not talking about the boundary layer of a fully developed tropical cyclone.

> P16598 L2: Have you taken into account that friction is not Galilean invariant?

Yes, we have used the earth-relative velocities to calculate the surface stress.

> P16600 Nuri 1 should be defined the first time that it is used.

I have defined Nuri 1-4.

> P16603 L21: Why are the patterns of vorticity advective tendency irrelevant to the parcel increase? What about the vertical advection?

Please see the above statement about advection. In the flux form of the vorticity equation there is no vertical advection. (There is the tilting term, but its effects are small at low levels in weaker cyclones.)

> P16604 L8: What, exactly, do you mean by "TCS030 lacks PBL stretching"?

A second look suggests that there is some stretching near (but not exactly co-located with) the referenced convection, so this statement has been eliminated.

> L13-14: This is exactly what happens in the numerical simulations of Nguyen et al. (QJ, 2008. See p571). A reference to this connection might be appropriate.

Done.

> L21: I don't understand what you mean by saying " ... allowing vorticity maxima in the PBL to be exported from this system." It would help if you were to clarify this whole sentence.

I decided to remove the word "maxima" and simply say that vorticity can be exported from the system of the vorticity flux vectors do not form a closed circulation. This statement seems clear to me.

> P16605 L2: Why "primarily"?

One could imagine that frictional stress could be exported to levels above the boundary layer by moist convection.

> L6: Why "Curiously"? Also, what do you mean by "maximum" in this context?

I have removed "Curiously". It seemed somewhat curious to me in that the initial circulation maximum did not occur at middle levels, as it does in African easterly waves. However, I don't want to get into a discussion of this here. I mean "maximum" in the context of the cited figure.

> L14: I would insert a comma after "level".

Done.

> P16606 L1-3: I don't understand what you are trying to say in this sentence. What is the significance of the remark?

I am not sure what the issue is.

> L10-14: I don't follow these arguments!

Ditto.

> P16607 L3: What, exactly, do you mean by the vorticity distribution broadened? How is the distribution defined/calculated?

This is based on a simple visual interpretation of the figure. The means by which the distribution is calculated is stated in the paper.

> P16608 L13-15: This is exactly what happens in the numerical simulations of Nguyen et al. (QJ, 2008. See p571). A reference to this connection might be appropriate.

Paper cited earlier.

> L20: To what does "This" refer?

It is referring to the previous sentence, and the statement seems clear to me.

> P16609 L12-13: You say that: "A particularly interesting aspect of Nuri's evolution is that vorticity balance in the PBL was far from satisfied." The question is: Why is this result interesting? Indeed, why might you have expected it to be satisfied to make all this effort to verify that it is not?

It seems interesting to me, since some simplified models still assume something close to gradient balance in the boundary layer, an assumption that the reviewer himself has discredited in the case of mature cyclones.

> L13-15: You say that: "In Nuri 1 and Nuri 3 (full observed region) the frictional spindown tendencies slightly exceeded the spinup tendencies due to vorticity convergence." The devil might say "so what"? Why is this theoretically important? How does it help us to understand the dynamics of spin up? Or should I say, how can it tell us much without a knowledge of the radial motion in the boundary layer? The same remarks apply to the next sentence.

I think that the subject of this comment is covered in previous responses.

> L18-21: You say: "Thus, the Ekman pumping hypothesis, in which low-level convergence implied by Ekman (or vorticity) balance is assumed to control deep convection, appears problematic in this case, at least in the phases preceding tropical storm strength." What do you mean here by "is assumed to control deep convection"? How can you make that assessment by a global constraint on the so-called "pumping"? I would expect that the effect of "pumping" on convection would be a local one within the domain of areal averaging and would require knowledge of the forced boundary layer convergence (i.e. you would need to consider a radial momentum equation or its equivalent, the divergence equation".

Charney and Eliassen as well as Ooyama made the "crunchy" assumption (words of the Economist magazine!) that the vertical motion at the top of the boundary layer equals the mass flux into convection. Subsequent authors have backed off and made the "mushy" assumption that boundary layer convergence "modulates" or "has an effect on" convection without being more specific. This is tantamount to saying nothing useful. We are simply trying to figure out what actually does control the convection in these situations. To do this we first have to clear away the mush.

If balance is not satisfied globally, then it certainly isn't satisfied locally.

L21-22: You say that: "The effects of tilting are generally insufficient to change these qualitative results, at least at low levels." Aren't you talking about the areal average of the tilting? The dynamical significance of this remark is unclear to me also.

Yes, the areal average. Its significance is that one can ignore it in most cases at low levels (at least in the systems studied here), which makes the interpretation simpler. In strong cyclones tilting in the boundary layer probably cannot be ignored beyond a very crude zeroth order approximation.

L6-7: You need to state what definition you use for the boundary layer, perhaps with a reference to Smith and Montgomery (QJ, 2010), where the various definitions are discussed.

This is discussed in response to an earlier comment.

L26: It would be worth commenting on the fact that frictional force is not Galilean invariant and explain the consequences of this fact for the analysis.

We now indicate that the wind used in the bulk flux formula for the surface stress must be in the earth's reference frame. We haven't investigated the particular effects of this on cyclogenesis, though it is clear that fluxes will be stronger in the northern half of a westward-moving disturbance.

Signed Roger Smith