Review of the paper "The vorticity budget of developing Typhoon Nuri (2008)" by D. J. Raymond and C. Lopez Carrillo, submitted to Atmos. Chem. Phys.

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.

Specific comments

P16589

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

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.

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

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.

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.

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.

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

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.

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.

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.

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

P16592

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

P16597

L1. I couldn't find where a_2 is defined, but it needs to be.

P16598

L6: How is this average "depth defined"?

P16598

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

P16600

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

P16603

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

P16604

L8: What, exactly, do you mean by “TCS030 lacks PBL stretching”?

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.

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.

P16605

L2: Why “primarily”?

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

L14: I would insert a comma after “level”.

P16606

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

L10-14: I don’t follow these arguments!

P16607

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

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.

L20: To what does “This” refer?

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?

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.

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”.

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.

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.

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.

Signed
Roger Smith