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Discussions

Interactive comment on "Role of eyewall and rainband eddy forcing in tropical cyclone intensification" *by* Ping Zhu et al.

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Review of: Role of eyewall and rainband eddy forcing in tropical cyclone intensification

by Ping Zhu, Bryce Tyner, Jun A. Zhang, Eric Aligo, Sundararaman Gopalakrishnan, Frank D. Marks, Avichal Mehra, and Vijay Tallapragada

Summary and Evaluation: This is a potentially interesting and potentially useful study for the scientific community devoted to improving our understanding and numerical weather forecasts of damaging tropical cyclones threatening populated coastal communities throughout the world. However, the current manuscript suffers from limitations involving a lack of clarity, poor scholarship in some places and multiple instances of muddled scientific writing. I was very disappointed to discover this state of affairs, given the large number of co-authors (7), including several senior (& expert) co-authors.

The authors begin their presentation by trying to argue that improvements in the forecast of rapidly intensifying storms will follow: a) once eddy momentum and eddy heat flux processes are properly accounted for in the eyewall and rainband regions; and b) once in-cloud turbulent mixing parameterizations are developed for the eyewall and rainband regions. These are certainly plausible points of view (but see point 2 below for an opposing view). The authors then propose a simple revision of the sub-grid-scale (SGS) turbulence closure scheme for the Hurricane Weather Research Forecast (HWRF) model. The proposed scheme recognizes the prevalence of turbulence in deep (presumably) rotating convection in tropical cyclone vortices. The revised closure is elementary and consists of redefining the height of the boundary layer in deep convective regions, such as the developing eyewall or rainband region, to a height of approximately 5 km altitude based on a model-derived reflectivity threshold of 28 dBZ (Figs. 4e, 4f). This revised boundary layer height definition is called the 'turbulence layer' (TL) and is an extension of the current HWRF scheme (based on pioneering work of Larry Marht and colleagues, and subsequent work in 1996 by Hong and Pan, etc.).

HWRF model simulation experiments invoking the new turbulence parameterization appear to be significantly improved over the standard Hong and Pan (1996) scheme that uses a gradient Richardson-number to define the boundary layer height (typically 1 km). Although the eddy momentum and heat flux divergent tendencies diagnosed from the new simulations are shown to be approximately five times greater than the corresponding SGS tendencies (pg. 13, bottom paragraph, Figs. 10, 11), the authors argue that the revised SGS tendencies are ultimately responsible for the improved forecasts. The authors appear to base their assertion on some mysterious coupling between the turbulence closure scheme and the cloud microphysical processes, and its corresponding coupling to the latent heating rate field associated with the aggregate of deep (presumably) rotating clouds in the inner-core region of the developing vortex. I am certainly willing to entertain the scientific possibility of a subtle nonlinear feedback involving the SGS tendencies and the microphysics, but the proffered feedback mechanism should be clearly articulated in this manuscript to help support the empirical evidence of the HWRF experiments in real cases. It is unclear, for example, which is most important: the revised turbulence closure scheme for momentum, heat, or moisture?

An alternative (and simpler) hypothesis might be that the structure of the resolved eddy forcing between the control and updated experiments might be more important in accounting for the improved forecasts. This alternate hypothesis originates from a cursory examination of Fig. 11 wherein the resolved eddy forcing of the mean tangential velocity tendency equation in the TL-HWRF experiment is more spatially concentrated and of higher intensity than the DEF-HWRF experiment. (Of course, the different eddy forcings are in part the result from the different SGS formulations, but the larger magnitude of the resolved eddy forcing seems to be a more plausible agent for influencing the spin up process.)

Finally, throughout the manuscript, I was disappointed to find that the authors never asked the basic question of whether a down-gradient turbulence closure for all predicted quantities is indeed appropriate in the rotating, convective turbulence region that pervades a rapidly intensifying tropical cyclone vortex? (see, e.g., Persing et al. 2013, their section 6.)

There are other substantive issues that need to be addressed by the authors and these issues are noted below.

Recommendation: Major Revision. The paper requires substantial improvement in several areas (listed above and below) before I can consider recommending the paper for acceptance in this journal.

Major comments:

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Discussion paper

First and foremost, the entire manuscript needs to be read carefully by the native English speaking co-authors. I have come across multiple instances of ambiguous or inaccurate statements that need attention. I highlight some of these instances below. I have not provided an exhaustive list however.

1. The first sentence of the Abstract typifies the lack of clarity that occurs in the manuscript:

“The fundamental mechanism underlying tropical cyclone (TC) intensification may be understood from the conservation of absolute angular momentum, where the primary circulation of a TC is driven by the torque acting on air parcels resulting from asymmetric eddy processes, including turbulence.”

If the fundamental mechanism underlying TC intensification can be understood from the material conservation of absolute angular momentum (AAM), why, then, are eddy torques being invoked in the SAME sentence to explain how the primary circulation is driven by the torque acting on air parcels resulting from asymmetric eddy processes, including turbulence? While I might be called out for singling out one sentence of the paper, it is the first sentence of the Abstract. Sentences like this abound in the manuscript and portray an alarming state of confusion concerning the mechanisms of tropical cyclone intensification.

2. How come the CHIPS model (Emanuel et al. 2004) is never mentioned in this paper? How come the latest Emanuel (2012) theory for tropical cyclone intensification is never mentioned in this manuscript?

The reason I am asking these questions is that some have suggested that the CHIPS model currently beats all deterministic forecast models (see Jonathan Vigh’s talk from the recent AMS conference on Hurricanes and Tropical Meteorology in Ponte Verde, Florida, April 2018). In that context, some have advocated that the turbulence closure problem examined here is a red herring. How will the authors address these questions in the revised manuscript?

3. Pg. 2, Line 11: What is the mechanism underlying TC intensification? Surely, the material conservation of M above the BL (the ice skater model, i.e.) is an essential element of the spin up process above the frictional boundary layer. But what is the mechanism that supports the continued spin up of the vortex? You have not articulated the mechanism(S), other than a passing reference to CISK or cooperative intensification. I believe this is an inadequate state of affairs that needs to be corrected.

4. Pg. 2, lines 19-20: Re eddy forcing in BL:

"In the PBL, eddy forcing ðİŘźðİIJĘ + ðİŘźðİŚăðİŚŤðİŚă_ðİIJĘ is negative definite, meaning that it always slows down the motion; thus, it physically represents the frictional force in the tangential direction. "

". . . is negative definite . . . always slows down the motion" seems to be an assertion without proof of substantiation. Is this a property of the three-dimensional Navier-Stokes equations? (Ans: No.) Is it based in observations? Please give references.

5. Pg. 2, line 29: "In other words, the evolution of the primary circulation of a TC vortex must be (emphasis mine) accompanied by a secondary overturning circulation."

This sentence is insufficiently precise. Purely asymmetric motions can cause an evolution of the mean vortex without mean secondary (overturning) circulation (e.g. a barotropic nondivergent vortex Rossby wave packet and its accompanying wave, mean flow and wave-wave interaction).

Pg. 6, line 1: "Physically, this overturning circulation is induced by (emphasis mine) friction within the PBL and diabatic heating of convection."

Again, this statement should be sharpened. A moving, inviscid, baroclinic vortex on a beta plane will cause asymmetries, which will generally induce an overturning circulation and mean vortex evolution even without friction and diabatic heating (see, e.g., Flatau et al. 1994, JAS).

7. Pg. 3, line 10. Re WISHE and Emanuel 1986. This is a misleading and inaccurate

description of scientific history. Emanuel 1986 is a steady-state hurricane theory (!) and not an intensification theory. The WISHE acronym was not introduced until 5 years later by Yanno and Emanuel 1991. The WISHE feedback mechanism of intensification was articulated by e.g. Emanuel (2003). Following the credible scientific challenges of Montgomery et al. (2009, 2015), WISHE has now been re-defined (Zhang and Emanuel 2016) to mean just the formula for the wind-dependent moist enthalpy flux at the air-sea interface.

Continued:

Line 14. Potentially misleading. Smith and Montgomery were well aware of the limitations of the boundary layer definition used in the hurricane community and noted as such in Smith and Montgomery (2010).

Line 16: Inaccurate. Smith and Montgomery did not assume that the vertical velocity was zero in the boundary layer! (If a slab boundary layer model was being used, then there would be no vertical advection of AAM out of the boundary layer assuming the boundary layer was well mixed in AAM. This is hardly the same as assuming that the vertical velocity is zero within the boundary layer!)

8. Pg. 4, Lines 14-15. Inaccurate. Montgomery and Kallenbach 1997 and Persing et al. 2013 did not root their interpretation of eddy spin up on upscale energy cascade. These authors used a momentum-based approach, which hinges on the eddy vorticity flux (in the barotropic nondivergent case) and the eddy vorticity flux and eddy vertical advection of eddy tangential momentum in the 3D cloud-representing configuration (see Persing et al. 2013, their section 6). The difference is subtle because the eddies can act locally to spin up the maximum mean tangential wind and radial inflow/outflow even while consuming energy from the system-scale mean vortex.

Continued:

Line 26: “multiplication by density first” is missing.

9. Pg. 5, Lines 4-5: “In numerical simulations, Eq. (6) is the equation that governs the azimuthal-mean overturning circulation of a TC vortex.”

This statement is physically misleading. The azimuthal mean overturning circulation in a legitimate 3D forecast model such as HWRF is governed in part by the radial and vertical momentum equations. Equation (6) is merely a constraint that must apply at all times and does not “govern” the overturning circulation.

Continued:

Line 6: “In classic TC theories”. Citations please.

Lines 7-8: This statement is incorrect. The eddy forcing terms can be zero, but acceleration terms may still be nonzero.

Line 10: Why is Equation (8) to be time differentiated? Please explain.

10. Pg. 7, the text pertaining to Equations (9) and (10). My reading of this text is as follows: the HWRF model uses this two-component formulation for Km (i.e. the Hong and Pan closure in the BL/TL (Eq. 9) and the Smagorinsky closure with stability modification by Lilly above the BL/TL (Eq. 11), respectively) in the vertical and horizontal mixing terms for momentum, heat and moisture. Is this summary correct? Please clarify.

Bryan and Rotunno (2009) and Bryan (2012) use a much higher value of Km for horizontal diffusion than employed here. How do the authors explain this difference in model formulation compared to Bryan and Rotunno?

11. Pg. 14, Lines 1-2. “ . . . since the large energy-containing turbulent eddies are not resolved at the current model resolution of 2 km.” What are these large energy-containing eddies in these simulations and in real-life tropical cyclones?

12. Figures 10 and 11. The resolved eddy forcing tendency for the azimuthally-averaged tangential velocity tendency equation is plotted in cross section and on several horizontal height surfaces. In these panels the units are displayed as inverse

seconds. Shouldn't the units be that of acceleration (if instantaneous tendencies averaged over some finite time interval) or meters per second (if integrated over some time finite interval)? Please clarify here and elsewhere.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-610/acp-2018-610-RC1-supplement.pdf

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