

Review of “Tornado-Scale Vortices in the Tropical Cyclone Boundary Layer: Numerical Simulation with the WRF-LES Framework”, by Liguang Wu, Qingyuan Liu, and Yubing Li.

General Comments:

In this study, the authors use WRF-LES to simulate a quasi-idealized tropical cyclone (in the environment of a real TC), for the purpose of investigating tornado-scale vortices. They find such vortices along the inner eyewall, concentrated in the left-of-shear region where convection is enhanced by the environmental vertical wind shear. Large horizontal gradients of wind speed are found in association with strong updrafts, and from the perturbation wind structure, the authors identify distinct vortices. The authors also argue that the tornado-scale vortices are related to horizontal roll vortices, and they suggest that the vortices may be related to the local large vertical wind shear that is present in the low-level eyewall.

Overall, this is an interesting study that contributes to our knowledge of intense small-scale vortices that are believed to be prevalent within the low-level eyewall of intense tropical cyclones. I have a number of minor scientific comments that are mostly related to requests for clarifications, but also include some areas where I’m not quite convinced that the authors’ analysis demonstrates what is claimed. I also have a few more significant concerns. First, I think it is possible that the use of a moving average to define the reference state for wind speed may result in an exaggeration of the gradients in the perturbation winds, and that the azimuthal mean (or azimuthal-mean + low-wavenumber flow) may be a better choice for this analysis. Second, a study (Stern and Bryan 2018) has recently been published, that also used LES to examine these eyewall vortices, and so I think (in revision) that this current study should include some discussion of how their results may relate to those of Stern and Bryan (though I recognize that the authors submitted their manuscript just prior to the appearance online of the earlier study, so I don’t mean this as a critique of this manuscript). Finally, it seems that a major result of this study is the finding that the eyewall vortices are related to pre-existing horizontal roll vortices within the boundary layer at and outside of the eyewall. I’m not fully convinced this is the case (though it may be), as the authors haven’t really objectively defined the horizontal roll vortices that they see, and the existence of an updraft/downdraft couplet in the tornado-scale vortex isn’t itself (in my view) necessarily a horizontal roll vortex (also see minor comments #25-26). Following revisions, I think that this study can be a nice contribution to the literature.

Specific Major Comments:

A. Use of a moving average to define the reference state

I think it may be problematic to use the 8-km moving average for calculating perturbation winds. This choice results in the much weaker tangential winds within the eye influencing the perturbation winds in the tornado-scale vortices, and vice versa. For example, in Fig. 3b, the perturbation flow within the eye is apparently anticyclonic, as the mean winds are much stronger than the local flow (because they include a region of the eyewall). This results in an

exaggerated characterization of the vortices, because the mean radial gradients are influencing the perturbation structure. I think a better choice would be to use the azimuthal mean (at a given radius) to define the perturbation winds. I see that the authors have examined something similar to this and they stated that they found similar results to their choice of the moving average. Still, I think the azimuthal mean is a more appropriate choice than the moving average.

B. Discussion of other recent related studies

With respect to observations of extreme local wind speeds and updrafts that are believed to be related to small-scale vortices, I think it would be worthwhile to discuss the recent study of Stern et al. (2016), who examined extreme updrafts and wind speeds observed by dropsondes. Also, Stern and Bryan (2018) very recently published a study using LES to investigate similar features as to what the authors examine in this manuscript. This was probably not available to the authors at the time that they submitted their manuscript, but given the similarity in some of the goals and methods of these studies, I think it would be worthwhile for the authors to add some discussion of how their results may relate to those of Stern and Bryan (2018).

<https://journals.ametsoc.org/doi/abs/10.1175/MWR-D-18-0041.1>

C. Results that are not shown

There are a fairly large number of results that the authors refer to that aren't actually shown (given in minor comments below). This can be ok, but they need to make clear when a claim isn't explicitly shown by a figure. Also, these results that aren't shown probably shouldn't be included in the abstract (e.g., that the in nearly all vortices, there is also a broad downdraft).

Specific Minor Comments:

1. P5 l117

“may be responsible for TC intensification” is too strong, I suggest changing to “may contribute to”. It also might be a good idea to note here (I see it is mentioned later) that other subsequent studies (such as Bryan and Rotunno 2009) have found that this mechanism is unimportant for intensification.

2. P6 l126-131

It's a bit unclear why a real case is chosen, but without any evaluation of the simulation in comparison to the observed storm. I'm guessing that the goal here is to examine a realistically sheared storm (and this may be easier to do in the real-case framework), but not to reproduce the evolution or structure of a specific real typhoon. This is ok, but the reasoning here should be made clearer. I note that Typhoon Matsa was not particularly intense, only an estimated 90 kt peak intensity, whereas it appears that the simulated storm here is stronger.

3. P6 l159-160

The authors state that they output 3-s data for a 22-min period at $t=29$ h. This output is only used briefly on page 11 (with no figures shown), and so I think it would be better to move the description here to be part of the discussion of where it is used on p11.

4. P8 l172-173

Note that the persistence of the open eyewall isn't shown.

5. P8 l178

The shear given here is 5.2 m/s, but the figure caption gives 7.0 m/s.

6. P8 l181

The period here is stated to be 11 hours, but above it is given as 10 h. Also, note that the RMW range is not shown.

7. P8 l187

Clarify that you are referring to the TC-scale shear-induced convective asymmetry here, not local enhancement of reflectivity around the tornado-scale vortices.

8. P9 l202 and Fig. 3b

The figure is somewhat confusing, because the labels for the identified vortices are not found where the actual features are. It would be good to add a dot/circle (or some other symbol) to indicate the specific locations.

Also, I think it would be better to describe the convention for numbering the vortices here (where they are first introduced), rather than later when referring to Table 1.

9. P9 l209-211, p10 l213-221

The authors refer to the features examined here as "tornado-scale" vortices, and they discuss this in the context of the studies of Aberson et al. (2006), Aberson et al. (2017), and Marks et al. (2008). But these prior studies did not refer to the features as "tornado-scale", and so this could be somewhat misleading. The more recent study of Wurman and Kosiba (2018) did use this terminology, and I think it can be an appropriate choice. But the authors should clarify how previous studies viewed these features.

10. P10 l217

Why is there a height threshold used in the definition here?

11. P10 l218

These thresholds are somewhat arbitrary. Also, we don't really know that these are vortices simply from the vorticity threshold, as a region of high vorticity isn't necessarily a vortex.

12. P10 l231-232

I agree that these vortices may be responsible for the strongest wind gusts in TCs, and this is also consistent with the results shown in Stern and Bryan (2018).

13. P10 I227-238

I'm of the belief that the small-scale vortices probably don't have a substantial effect on the overall intensity evolution. However, I don't think the analyses in this study can really answer this question one way or the other, since we don't know how this simulation would have evolved in the absence of these features.

14. P11 I245-246

I note that this relationship between the vertical wind shear orientation and the spatial distribution of extreme updrafts is consistent with what Stern et al. (2016) found from dropsonde observations in many storms.

15. P11 I246-251

It could be good to discuss Stern and Bryan (2018) and Wurman and Kosiba (2018) here, as these studies both estimated the period for which these vortices/updrafts could be tracked (and with similar time periods as the authors have found).

16. P11 I254-255

I think it would be good to make clear that the authors are not directly identifying "vortices", but rather are identifying strong updrafts that are collocated with strong vorticity, and they are inferring that these are likely vortices (aside from the specific example vortices that they more directly identify).

17. P11 I257-258

That the updrafts/vortices are often found inward of the mean RMW is somewhat consistent with Stern and Bryan (2018), though in their study, they found that the strongest updrafts tended to occur more nearly at the RMW.

18. P11 I258

Clarify that you are referring to the $w=20$ m/s threshold, not the $w=15$ m/s threshold that has also been examined in this study.

19. P12 I260-261

Note that this result is not shown.

20. P12 I271

Please clarify what is meant by "using the smoothed fields". In what manner is the smoothing done?

21. P12 I271

Please define the Richardson number.

22. P12 I274

It would be helpful for the authors to more clearly explain why they are examining the Richardson number, what they expect it to tell them, and why they believe it should be related to the existence of these vortices. I see that the authors do so somewhat at the end of this paragraph, but I think it would be good to include this reasoning here where they introduce their analysis.

23. P13 I293-294

I'm not sure what this sentence ("The altitudes of the maximum vertical motions generally increase when the inflow layer deepens outward") means. Perhaps the authors are saying that the height of the features tends to be greater when they are found at larger radii?

24. Section 6

It's a bit confusing to have the vortices split into categories without first defining what the categories are. I think the distinctions should be brought up at the beginning of the section, along with information on how specifically the vortices are assigned to a category (is it subjective?), and a discussion of the physical reasoning for these classifications.

25. P14 I312-313

In my opinion, it is hard to tell if the features discussed here are indeed "closely associated with horizontal rolls". Is there any more objective evidence the authors can provide that can better demonstrate this claim?

26. P14 I318

It seems that the authors are concluding that these are roll vortices because there is a updraft/downdraft couplet, and so this implies a transverse circulation and horizontal vorticity. But I don't think this is really the same thing as what is traditionally referred to as a horizontal roll vortex, which generally have an elongated quasi-linear region of weak updrafts/downdrafts. These eyewall vortices will naturally have local updraft/downdraft couplets and large horizontal vorticity, but I don't think this makes them *necessarily* related to horizontal roll vortices.

27. P14 I323-324

In my view, we don't actually know whether the high thetae layer is an indication of transport from the eye. There is high thetae further outward as well in this cross section, so the elevated layer of high thetae does not have to have originated within the eye, although it may have. I think a trajectory analysis is necessary to have confidence on the origin of this air mass.

28. P15 I329-331

It isn't clear to me why/how the downward motion at 500 m is responsible for the high thetae layer. It also is unclear to me why the low thetae layer near the surface should have lower

thetae because it is in inflow. It's true that the mean radial gradient tends to be negative (and so mean radial advection tends to yield a negative tendency), but this is generally outweighed by other tendencies (such as surface fluxes).

29. P15 l332-333

Again, I don't think we can know from the analysis here that the high thetae eye air is entrained into the eyewall.

30. P15 l342-343

It looks to me that "~65 m/s" should be "~60 m/s", and that "~90 m/s" should be "~95 m/s".

31. P15 l345

It isn't clear to me why the downward motion is "consistent" with the "strong wind speed jumps". What is the relationship here? I'm guessing that the authors are implying that strong wind gusts could be caused by vertical advection of higher momentum from above.

32. P16 l360-362

Is the structure described here for this particular feature believed to be generally true for other such features? It's unclear if there is a robust signature here, as the authors are showing a single example.

33. P17 l377-378

I think it is important to acknowledge here that this definition is somewhat arbitrary, and to reiterate that the frequency of these inferred vortices is very sensitive to the thresholds of this definition.

34. P17 l387

Here, the authors refer to updrafts stronger than 15 m/s, but their definition given above is for 20 m/s.

35. P17 l390

That the updraft is generally associated with a downdraft is not shown.

36. Fig. 1

Make the caption clearer by changing "instantaneous and azimuthal maximum" to "maximum instantaneous and azimuthal-mean".

37. Fig. 2

The red dots in 2a aren't defined in the caption. Change "solid circles" to "black circles". Give the height at which the RMW is evaluated here. Insert ", respectively" after "and the radius of maximum wind". Remove "(27h)".

38. Fig. 6

The “vertical slice” doesn’t look exactly vertical to me. Please clarify if it is “nearly” vertical”.

39. Fig. 7

Please clarify if this cross section is purely in the radial dimension (as opposed to projecting onto the azimuthal dimension as well).

40. Fig. 9

Please clarify if the vertical velocity shown here is also a perturbation quantity.

Technical Corrections:

1. P2 l40

“the favorable location” is vague and somewhat confusing. I suggest “the location along the inner edge of the eyewall”, or something similar.

2. P3 l66

“by now” should be “for now”.

3. P5 l109

“*f*” should be italicized.

4. P6 l134 and elsewhere

When expressing the lengths of a grid, the units should be “km”, not “km²”.

5. P6 l134

Insert “grid” before “spacing”.

6. P6 l136

“dimentional” should be “dimensional”.

7. P6 l142

Insert “for” after “except”.

8. P7 l145

Insert “of” after “temperature”.

9. P7 l160

Use “29 h” instead of “the 30th hour”.

10. P7 l162

“northern north west” should be “north northwest”.

11. P7 l163-166

“instantaneous and azimuthal maximum” should be “maximum instantaneous and azimuthal-mean”, with “maximum” presumably applying to both metrics. The following sentence “The instantaneous output...” can be removed, as it is redundant.

12. P8 l167, l169

I think it would be better to put the smaller number first for these ranges.

13. P8 l176

Insert “is” after “shear”.

14. P8 l183

“France” should be “Frances”.

15. P9 l194

“filed” should be “field”.

16. P9 l198

“wind” should be “window”.

17. P9 l209

Aberson et al. (2016) should be Aberson et al. (2017).

18. P10 l233

Insert “some” before “previous”.

19. P10 l235

“Montgomery” should be “Montgomery”.

20. P10 l235

“azimuthal” should be “azimuthal-mean”

21. P12 l272

Insert “vortex” after “scale”.

22. P12 l274-275

Stern et al. (2016) is cited here, but it doesn’t appear in the list of references.

23. P14 l305

Move “inward” from end of sentence to right after “kilometers”.

24. P14 l318

“frank” is a typo here. I think that the authors mean “flank”. Also, “rolling” should be “roll” (also where it is used elsewhere).

25. P15 l327

Replace “To the right” with “Outward”.

26. P15 l328

Change “the category” to “this category”, and insert “of vortices” after “category”.

27. P15 I328

It’s ambiguous which feature “the lower-altitude high thetae layer” refers to. Please clarify.

28. P15 I329

Change “does not” to “is not”.

29. P15 I337

I think that the authors may mean Fig. 8a and not Fig. 8b. Also, I think they may mean Fig. 3b and not Fig. 2b.

30. P16 I359

“Figures 10a” should be “Fig. 10a”.

31. P16 I367

Insert “with” after “associated”. Insert “the” before “complicated”.

32. P17 I371

Change “nesting” to “nested”.

33. P17 I382

There is an extra period after “layer”.

34. Fig. 4

“500-km” should be “500-m”.

35. Fig. 8

Fig. 8b has “400 m”, but the caption says “500 m”.

36. Fig. 10

The last sentence of the caption is a duplicate.