Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-827-RC1, 2016

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Impact of vertical wind shear on roll structure in idealized hurricane boundary layers" *by* Shouping Wang and Qingfang Jiang

K. Gao (Referee)

kun.gao@noaa.gov

Received and published: 21 November 2016

This manuscript presents a well-designed and well-conducted study, which investigated the characteristics of rolls (large eddies) in the hurricane boundary layer under various sheared flow scenarios based on Large Eddy Simulations. The authors constrained the simulated mean (horizontal-averaged) flow by nudging it toward the prescribed wind profiles, making sure the simulated mean flow has characteristics consistent with observations. This study highlighted the importance of the radial wind shear in affecting the turbulence characteristics. Besides supporting some previous findings, the authors also found a few aspects of rolls that were not reported previously, such as the impacts of radial shear strength on the roll horizontal scale and the inflection

point height on the turbulent spectra. This manuscript is overall well written and the key points are clear. I would therefore recommend the manuscript for publication after the authors address the following comments.

Main comments

1. A few comments on the target wind profiles

i) The authors fixed the target tangential wind and only varied the radial wind in the two groups of experiments to explore the impact of the radial wind shear. However, in physics-based models one cannot vary the radial wind without changing the tangential wind as the two wind components are intrinsically coupled. Since the authors aimed to explore the turbulence characteristics in an idealized setting, I think their purpose justified their choice. But the authors are recommended to explicitly state this in their experiment design.

ii) There is no detailed description for how the target wind profiles are obtained. Are these profiles derived from a dynamical model or observed profiles?

iii) The authors only showed the LES results under the gradient wind speed of 45.5 m/s. Are the results shown in this manuscript representative for a range of gradient wind speeds that the authors have investigated? Or is the choice of 45.5 m/s somehow arbitrary? Either way, the authors should make it clear.

2. The model produced much higher mixed layer depth (Fig. 2a) than typically observed (Zhang et al., 2011, MWR). This is very likely because some important processes that stabilize the hurricane boundary layer, such as radial advection and diabatic effect (Kepert et al., 2016, JAS), were not considered in this study. The unusual high mixed layer has important implication for the large eddy characteristics. As discussed by Gao and Ginis (2014 and 2016), the height of mixed layer has critical impact on the roll characteristics and their coupling with internal waves. The authors are recommended to add some discussion on this.

3. The authors did nice quantitative analysis showing that the simulated rolls have a quasi-two-dimensional structure with the two velocity components (u', w') of the overturning circulations 90 degree out of phase. While their analysis help understand why the correlation between u' and w' is poor and the cross-roll momentum flux w'u' is weak, there is still lack of a fundamental explanation for the vertical tilting of the convergence zone (Fig. 6). One possible explanation is that, as shown in Foster (2005) and Gao and Ginis (2014), the roll streamlines tend to tilt vertically to efficiently extract the kinetic energy from the mean shear flow. The tilted convergence zone and the negative cross-roll momentum flux result from the tilted roll streamlines. The authors are suggested adding some discussion on this and revising the 4th point in the summary of section 3 accordingly.

4. The LES results are of central importance for the development of turbulent flux parameterizations under conditions where in-situ observations are difficult to obtain. This work presents important LES results under high wind shear conditions. The authors are thus encouraged to strengthen their discussion on the turbulent flux distributions, which may provide guidance for further effort on turbulence parameterizations under hurricane conditions. There are a few aspects worth attention. The authors are not asked to add a whole new section; one paragraph or two would be sufficient.

i) It would be interesting to apply the flux decomposition method to analyze the results from other experiments and compare the large-eddy-induced fluxes under the scenarios in which the single-mode roll structure is dominant or not.

ii) The vertical distributions of the roll-induced radial and tangential momentum fluxes presented in this study seem largely consistent with Gao and Ginis (2016), which investigated the correlation between the roll-induced momentum fluxes and the mean wind shear based on 2-D model results. It would be of interest to apply the same method and see if the 3-D LES results qualitatively agree with Gao and Ginis (2016).

Minor comments

Page 2 line 33: Suggest changing "Others neglect the effects by assuming . . ." to "Others neglect the horizontal advection effect on the HBL wind profiles by assuming . . ."

Page 3 line 4: Change "Morrison and Bussinger (2005)" to "Morrison et al. (2005)"

Page 4 line 7: The Charnock relationship gives a monotonic increase in the drag coefficient for increasing surface wind speeds, which was found not valid under high wind condition (surface wind greater than $\sim$ 30m/s). Did the authors put any constraint on the surface roughness length (drag coefficient) under high wind in this study?

Page 6 line 5: Foster (2005) used a linearized dynamical model to obtain the wind profiles and did not use observed wind.

Page 6 line 24: The fact that experiment H3 has the highest mixed layer is likely partially due to the strongest nonlocal mixing effect of rolls/large eddies, which have largest vertical extent in this experiment, not only due to the strongest turbulence intensity.

Page 7 lines 30-32 and Page 10 line 1: It is not clear why the authors say H3 has the most vigorous rolls. Is this based on the maximum w' or the domain-integrated kinetic energy? The turbulence statistics shown in Fig. 7 suggest that the maximum w' in L3 (which has strongest radial shear) maybe larger than H3.

Page 8 line 17: This sentence needs to be revised since it is somehow counterintuitive by saying "downward transport driven by vigorous upward motion".

Page 15 line 10: It is not clear how the tangential momentum flux w'v' affect the turbulence intensity.

Page 17 line 23: While it is true that rotation terms have no significant direct impact on rolls generated by the shear instability, Foster (2005) suggested that at small radii (insider of the radius of maximum wind) another type of instability associated with the rotation terms (the parallel instability) may be the dominant mechanism for roll formation.

Figures 3 and 4: At what time are these snapshots selected?

Figure 4: There is no black line in (c). Also, the caption for (d) should be w'u'.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-827, 2016.