Review- Future changes in North Atlantic winter cyclones in CESM-LENS. Part I: cyclone intensity, PV anomalies and horizontal wind speed

I have reviewed the paper the current manuscript by Edgar Dolores-Tesillos, Franziska Teubler, and Stephan Pfahl. In this manuscript the authors investigate cyclone-centered composites in an ensemble of CESM-LENS simulations. They perform a piecewise PV inversion to study the projected changes in upper and lower PV anomalies and associated winds of strong midlatitude cyclones. Overall, this is a well-written paper that presents interesting analysis. While most of the results are not really surprising (as other previous studies have already examined changes in storm-centered composites in other climate-change simulations), the detailed examination of the PPVI analysis is interesting and deserves publication, after some revision.

My detailed comments are given below

- Line 124: "15-day average"- should this be 30?

- Line 125: could the 2.5 spatial average hide some of structure at upper levels? For the full upper level PV field that should not matter, but for the upper level PV anomaly that spatial averaging around the center might involve averaging over positive (from the trough to the west) and negative (from the ridge to the east) values.

- In general, your references (when citing more than one in the same parenthesis) should appear in chronological order).

- In what ways was the PV inversion code modified by Teubler and Riemer (2016)? And do you use the modified version? Equations (2) and (3) are based on the original David and Emanuel (1991) and David (1992) papers, so this is not clear.

- Figure 1: the "lower" and "upper" decomposition should also contain a third "surface" contribution.

- Line 154: "30 days time slice"- 30 day time slice?

- Line 163: How is the separation defined? is it 600-200 hPa for the UPV, 600-875 hPa for the LPV, and 875hPa for the lower boundary? You should write this explicitly. Also, why do you take the 875 hPa as

the lower boundary for the PPVI? Can't you use the surface temperature (or T2m temperature)? You later mention that you interpolate to 1000-50hPa, so why not use the lowest level? This might influence the PPVI composites.

- Line 166: "Following (Davids and Emanuel, 1991)..."- Following Davids and Emanuel (1991),...

- Figure 3: The switching of the colormap from panel (a) to panels (c) and (e) is really confusing! Please use the same colormap, and the same color to denote the same sign (e.g., blue to denote negative values, and red to denote positive values).

- Lines 270-275: The RV850 changes (Fig.5b) essentially show a more SW-NE elongated low level feature. Could this be a signature of enhanced Anticyclonic Wave Breaking (AWB), which is projected in the future in the region?

- Figure 6: Why does PV increases everywhere in the troposphere? If these changes are due to enhanced LHR than I would expected positive PV changes below ~700-800 hPa, and negative PV changes above. This is not what you get.

- Lines 341-349 (about the upper level PV decrease): your findings about the upper level PV reduction are confusing. First, the spatial distribution of the changes (Fig.7d) are confusing- they are not similar to other studies (e.g., Michaelis 2017 see their Fig. 8g,h), who find a PV decrease to the northeast of the UPV center (opposite from what you find!). The regions to the northeast (e.g., where the low level PV increases, Fig.7b) is where I would expect to find a PV decrease at upper levels, so I am confused.

Could this reduction of PV that you find be just a signature of the upward lifting of the troposphere and tropopause? Can you plot vertical cross sections (without the spatial averaging) of PV and PV changes? Perhaps the peak of the PV maximum at upper levels has just shifted slightly upward?

- Lines 354-359: Again, not clear to me. The diabatic heating from LHR usually tends to amplify the upper level ridge downstream of the positive PV anomaly, and this is not what you get.

- Line 414: fairly way–> fairly well

- Figure 10a: I find it confusing that the strongest meridional winds are to the west (i.e., southward), as I would expect, due to the induced winds from the UPV, to find the strongest v' to the east (i.e., poleward). Do you get the same results in the 850 hPa composites for the ERAI reanalysis data?

- Figure 10d: Why is the wind velocity induced by the low level PV alone not more circular, as one would expect from the low level roughly circular PV anomaly?

- Figure 11: If you perform the PV inversion but not show the contributions from other layers, then this is not very meaningful (panels b and c are then just like taking a mean and perturbations). I think you should at least add the low level inversions to the SI.