Weather Clim. Dynam. Discuss., referee comment RC2
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Comment on wcd-2021-73

Anonymous Referee #2

Referee comment on "Future changes in North Atlantic winter cyclones in CESM-LE – Part 1: Cyclone intensity, potential vorticity anomalies, and horizontal wind speed" by Edgar Dolores-Tesillos et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-73-RC2, 2021

Review of manuscript submitted to Weather and Climate Dynamiscs

Manuscript identification number: **wcd-2021-73**

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

Authors: Edgar Dolores-Tesillos, Franziska Teubler, and Stephan Pfahl

Recommendation: Minor revision

General Comments

The authors analyse the projected changes in cyclone intensity, PV anomalies and wind speed for North Atlantic cyclones in an 10-member ensemble of CESM-LENS climate

simulations for the historical period (1990-2000) and late XXI Century following the RCP8.5 scenario (2090-2100). With this aim, a composite analysis is performed to evaluate the characteristics of the (most) extreme cyclones and how these are affected in a warmer climate. The main novelty of this study is the use of piecewise PV inversion to evaluate the relative contributions of PV changes at different levels to changes in low level winds, which in my opinion is a very promising approach (also to evaluate other cyclone features). The manuscript is well written and fits well into the scope of the journal. Moreover, it surely includes interesting and publishable material. Still, some aspects should be strengthened before the paper can be accepted for publication. I largely see these comments as "minor". Please find detailed comments below. If needed, I would be willing to review the paper again upon resubmission.

Main Comments

a) The main shortcoming in the present study is the limited discussion with the available literature, particularly with the "conclusions" section. This may have been postponed for the "part 2 manuscript", but as it is the manuscript has a bit of an unfinished feeling. For example, it would be helpful to clearly stated in how far the present manuscript provided new insights compared to recent review papers (notably Catto et al. 2019, also co-authored by S.P.)

Moreover, some more detailed discussion about the caveats of the selected approach would be helpful. Some statements are made within the results chapters (e.g. lines 200-202; 418-424), but these should be properly stated and discussed in the conclusions. This should include a) single model approach b) single tracking method c) selection of vertical levels d) PPVI decomposition

b) The second main shortcoming is a limited quantification of uncertainty regarding the PPVI decomposition. While the uncertainty within the 10-member ensemble is shown in the previous sections and figures (e.g. line 343-344 regarding Fig. 7d), this is not the case for Figs 9-11. I wonder if this aspect could be enhanced (also in connection with lines 418-424).

Minor Comments

1) Lines 2-3: I would not say that "changes in cyclone strucuture and dynamics are unclear", but rather that "**SOME** changes in cyclone structure and dynamics are unclear", in the lines of the discussion presented in Catto et al. 2019. Please enhance.

2) Line 23-24: Please add Klawa and Ulbrich (2003) as a reference, other also possible

3) Line 46: Please add the review paper Ulbrich et al. (2009) as a reference

4) Line 63-64: Please add Donat et al. (2010) as a reference, others also possible

5) Lines 116-118: I do not thing that using rotated or non-rotated composites would make a strong difference for the 10% stronger cyclones, but this could make a difference looking at the 1% strongest ones (which should follow a more northward tilted track) … wonder why rotation of the composites have provided less clear results …. It should be the other way around … did you also produce this S1 figure for the 1% strongest ones?

6) Line 180: Please add Neu et al. (2013)

7) Line 200-202: Please add the information that Zappa et al (2013) was using the Hodges scheme, and add that the sensitivity of the climate change signal of cyclones to the choice of tracking method was analysed in detail in Ulbrich et al. (2013).

8) Lines 241-249: please compare the climate change also to other manuscripts than only Zappa et al. (2013). For example, Pinto et al (2009) found a similar spatial pattern – but slightly shifted southward (cf. Fig. 14) - when analysing the 10% strongest cyclones in a ensemble of ECHAM5 simulations.

9) Line 475: Please add the review article from Ulbrich et al. (2009) and others.

10) The colour scale in Fig 2a and 4a should be changed, as it is quite misleading.

References:

Catto et al. (2019) – already in the reference list

Donat et al. (2010) https://doi.org/10.3354/cr00853

Klawa and Ulbrich (2003) https://doi.org/10.5194/nhess-3-725-2003

Neu et al. (2013) – already in the reference list

Pinto et al. (2009) https://doi.org/10.1007/s00382-008-0396-4

Ulbrich et al. (2009) – already in the reference list

Ulbrich et al. (2013) https://doi.org/10.1127/0941-2948/2013/0420