Future changes in North Atlantic winter cyclones in CESM-LE – Part 2: A Lagrangian analysis
egusphere-2023-1382

The aim of this study is to investigate the processes leading to projected future changes in mid- and upper-level PV anomalies through a Lagrangian analysis of cyclone airstreams. The authors analyse changes in several variables along Lagrangian back trajectories initiated at different locations within the cyclone composites. They conclude that the majority of the PV tendencies occur within the last 24 hours before they reach their initiation point. They attribute the low-level PV tendencies to ascent in the WCB but cannot simply attribute upper-level PV tendencies to a cyclone airstream. The figures are well presented, and the structure of the paper is easy to follow. I enjoyed reading the paper.

The authors have attempted to link their previous Eulerian analysis to this Lagrangian analysis which is interesting, particularly the cyclone-centred composites of Lagrangian tendencies. My main concern about the analysis, is that the cyclone airstreams discussed are not explicitly identified. A cartoon of the airstreams is shown in figure 1, but the same airstreams are not identified with sufficient accuracy in the analysis of the data (see general comments below). As the aim of the paper is to link PV anomalies to cyclone airstreams, I think this needs to be addressed before the paper is suitable for publication.

Major comments

Figures 3 and 7 show cyclone-centred composites of Lagrangian tendencies and how they are projected to change in the future. These figures are nicely presented but I struggled to identify the cyclone airstreams in these figures.

On line 226 the authors link the northward, ascending flow in the cyclone's warm sector to the WCB. The region of maximum ascent is located close to the cyclone centre, but the region of maximum poleward displacement is located further north-east, what region specifically is linked to the WCB and how does this relate to the WCB illustrated in figure 1? Furthermore, the WCB is typically comprised of two branches, one ascending and turning anticyclonically at upper levels and another ascending and turning cyclonically at mid-levels. While there is evidence of the anticyclonic branch in figure 7b, there is no evidence of the cyclonically turning branch. Line 304-305 states that the reduced eastward transport in the WCB outflow region corresponds to an intensification of the WCB outflow that wraps around the cyclone centre, but the flow is still westward and hence not cyclonic. Is this because the cyclonic branch is located at a lower pressure level? If so, can cyclone-centred composites of Lagrangian tendencies at this lower pressure-level be shown. The cyclonic branch is also missing from the figure 1 illustration. Line 347 states that ascent in the eastern part of figure 8c is associated with the cyclonic WCB branch wrapping around the cyclone centre. Please can the authors present evidence of this cyclonic branch. Finally, line 450 refers to the cyclonic and anticyclonic branches of the WCB. More evidence is needed to support this conclusion.

Line 227 links the descending southward flow to the DI. Like the WCB, the DI is typically comprised of 2 branches, one turning cyclonically at low-levels and another turning anticyclonically near the surface (as stated on line 74). The anticyclonic branch is missing from the figure 1 illustration. While there is evidence of the cyclonic branch in figure 3b, there is no evidence of the anticyclonically turning branch. Also, in line 413 the authors state that some DI trajectories arrive to the west of the cyclone moving southeastward at low levels and others to the east of the cyclone moving northeastward close to the cyclone centre. Is this motion shown in figure 10a? I do not see any eastward motion in this figure, which shows pressure tendencies, or in figure 3b which shows longitudinal tendencies.

The authors state on line 71 that the CCB can produce PV anomalies in the lower and middle troposphere, but analysis of this airstream is entirely missing from the paper. They also state that the CCB consists of 2 branches (line 67) but only the cyclonic branch is shown in figure 1 for some

reason. Is this because no identification of the CCB airstream is possible from the data using the current latitude and longitude tendencies (figures 3a and b).

To address the points above, the authors should also show figures of the cyclone-relative tendencies of the trajectories. I.e., subtract the cyclone motion 24hr latitudinal and longitudinal tendency from the trajectory tendencies. This will illustrate the cyclone relative trajectory tendencies and will likely highlight the missing WCB and DI branches and the CCB.

Minor comments

1. Line 103. Should 'proving' be 'providing'?
2. Line 148. If averaging over the entire cyclone area leads to cancellation between ascending and descending airstreams, why is this analysis presented? They also have a very large spread (line 176) meaning that interpretation of the averages is difficult.
3. Figure 2. Is the shading around the present-day average the grey or red shading?
4. Line 187: In the 24 h before what?
5. Line 220. I suggest that the trajectories from the north have smaller absolute meridional displacement because the cyclone's themselves are typically travelling northwards enhancing to the airstream trajectory component in that direction (see major comments).
6. Line 223. I suggest that the relatively small region of westward displacement would be more significant if cyclone-relative longitudinal tendencies were plotted. This would give a better indication of cyclonic wrap-up of the air around the cyclone centre.
7. Figure 5 and others. I think the description of blue and red lines should also be in the figure caption.
8. Line 281.'Righ' should be 'right'.
9. Line 282. 'th' should be 'the'.