Review of: Cold air outbreaks drive near-surface baroclinicity variability

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This paper investigates the role of CAOs for variability in near-surface baroclinicity at the entrance of the Northern Hemisphere storm tracks, along the Kuroshio-Oyashio and Gulf Stream Extensions, from the perspective of the isentropic slope framework.
The authors quantify the fraction of total variability in lower tropospheric baroclinicity (measured in the variability in the isentropic slope) explained by various intensities of CAOs (measured using a CAO index). They find that a substantial fraction of variability in the slope in the Gulf Stream region can be attributed to CAOs, whereby particularly strong CAOs account for a disproportionally fraction of variability in baroclinicity. For the Kuroshio-Oyashio Extension CAOs appear to explain a smaller fraction in total variability of baroclinicity, leading the authors to the conclusion, that the role of CAOs for determining baroclinicity in the storm tracks differs between the two study regions.
The authors complement these results with an analysis of the two contributing terms (tilting and diabatic term) in a phase space - to showcase how the two terms evolve in time, and contrast the CAO's contributions with the background contribution – as well as a synoptic perspective providing more context as to what the situations look like when the tilting term is at its maximum.

Overall, the study is innovative and sheds new light onto an important topic of the midlatitudes: The relevance of CAOs for the downstream storm tracks. The manuscript is carefully prepared, very-well written and features nicely prepared figures. I also enjoyed the synoptic perspective section, which can offer more concrete context to the more abstract phase spaces.

Main comments:

1. The isentropic slope framework, as I understand, quantifies baroclinicity as a means of the slope of isentropes. While the isentropes are slanted in the free troposphere, they are vertical in the convectively mixed boundary layers and may even be unstable in extreme situations such as marine CAOs (Vannière et al. 2017). During moderate to intense CAO events the marine boundary layer may well reach 825hPa, which is here chosen as the upper boundary. Therefore, I would be curious to know how this issue is treated in this study, and how grid points in which θ is constant throughout the column from 900-825hPa are handled.
2. Related to main comment 1, I'm also wondering about the representation of potential temperature in the marine boundary layer in ERA5. How well can we trust the profile of potential temperature within the boundary layer (especially in extreme situations such as marine CAOs)?
3. CAO indices smaller than 4K can hardly be considered CAOs. Though, it is very interesting to see by how much various intensities of CAOs contribute to variability, I think it is misleading to call regions with, e.g., CAO index > 2K, CAOs, as these regions cover a large fraction of the study region (see Fig. 1) and are often likely associated with cold sectors of cyclones rather than transport of cold air from the polar regions/cold continent associated with CAOs. Consider discussing this issue more explicitly for CAO indices smaller than 4K.

4. I understand that you exclude the Japan Sea, as this region is not directly connected to the storm tracks, yet this region features a lot of very strong CAOs (stronger than on the eastern side of Japan) and large baroclinicity due to the strong land-sea contrast. Furthermore, large baroclinicity in this region may still be important for cyclones developing at the entrance of the storm tracks. How would your results change if you included this basin? Would a larger fraction of the variability in baroclinicity be explained by CAOs then?

Specific comments:

- Line 40: Please add a space between troposphere and the parenthesis.

- Figure 1 caption: "23 January 2014 at 15Z". Do you mean 15 UTC?

- Line 62-63: I understand that you exclude the Japan Sea, as this region is not directly connected to the storm tracks, yet this region features a lot of very strong CAOs (stronger than on the eastern side of Japan) and large baroclinicity due to the strong land-sea contrast. Furthermore, large baroclinicity in this region may still be important for cyclones developing at the entrance of the storm tracks. How would your results change if you included this basin? Would a larger fraction of the variability in baroclinicity be explained by CAOs then?

- Line 74: Suggest change to "ADV represents adiabatic changes in the slope …"

- Line 75-76: If the magnitude of the advection term is comparable to the other two terms, a substantial fraction and potential source in variability of baroclinicity is neglected. How would this term be related to/affected by the occurrence of CAOs?

- Line 141 -146: I would argue that a large part of this larger extent is not the CAO itself but rather the remnants of a cold sector of a cyclone over warmer ocean surfaces (see main comment 2). From Fig. 1 it is evident that it is mainly the extent of the region where the CAO index < 4K, differs between the 2 regions. The extent of CAO index > 6K (as well as CAO index > 8K) seems to me of comparable size in the two basins. The current interpretation may be misleading to other readers. Consider discussing this matter more explicitly.

- Line 138-140: Tilting term → how trustworthy/significant is this is, as you say, a very noisy field, featuring a lot of small-scale dipoles due to variability of up and downdrafts within the Boundary Layer, and then average over a large spatial domain → don't we just get a residual of large values in dipoles?

- Line 152: Consider adding a reference to Fig. 2 after "than their spatial extent would imply".

- Line 155: I find this mismatch between the explained variability between the two regions very interesting. Do you have any explanation why this may be the case?

- Line 162: Add a comma after "Thus,…".

- Line 162-163: where is the starting point in the analysis of phase diagrams? How can one start by saying that "first" baroclinicity is depleted by tilting and "then" diabatically restored? Maybe this is explained in the cited reference, but a quick hint in this direction would also be beneficial to this manuscript.

- Line 170: Why is there a secondary circulation with a clockwise direction? Also, how would this exactly be explained by CAO dynamics, as stated in Line 171?

- Lines 182: what would this "noise" represent physically?

- Line 188: The described sentence appears to be different from what is shown in Fig. 4. Specifically, the authors speak of the presence of a cyclone indicated by the contour lines, yet from the figure caption I take that solid contours indicate positive anomalies (and thus an anticyclone).

- Figure 4: Why are there faint and solid blue and red lines? Also, how significant are these anomalies?

- Line 201-202: Same as in Line 188.

- Line 211: I'd rather say, there is persistent advection of cold continental air, resulting in climatologically higher CAO indices. Whether this is actually a CAO or not is debatable….