Author response to the Editor: Perez et al., A new characterization of the North Atlantic eddy-driven jet using 2-dimensional moment analysis, submitted to Weather and Climate Dynamics

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Editor comments in black. Author response in blue.

I'd like to thank the authors and both of the reviewers for their work in improving this paper. Reviewer 2 has a few minor outstanding points to be addressed but Reviewer 1 has declined to provide a recommendation. Following the advice of Reviewer 2, the decision as to whether this is a sufficient advance over existing methods can be left for future users to decide. The method is clearly described and tested in the revised manuscript, which both reviewers noted is considerably improved. This paper is a useful contribution to the literature, so long as it is presented in the context of the diversity of metrics available which emphasise different aspects of the flow.

We thank the Editor and both reviewers for reading the revised manuscript and to the Editor and Reviewer 2 for offering further suggestions for improvements. We have implemented these as detailed below. We are disappointed with the attitude of Clemens Spensberger who appears unwilling to engage constructively with the peer review process despite our efforts to take on board his suggestions.

The paper could be accepted following consideration of the following minor suggestions:

1. The enhanced introduction is useful but could go further to highlight that both types of approach have advantages and applications. As potential examples, the lower-tropospheric metrics provide an integrated view of the flow, connecting well to patterns such as the NAO as shown (which ultimately explain a large fraction of the variance) and have also been found to relate clearly to the zonal momentum budget. Equally, the upper tropospheric metrics can reveal details associated with individual synoptic systems, and relate better to diabatic processes as mentioned.

   Thank you for the comment, the following sentences have been added to the introduction:

   "Upper tropospheric jet metrics are particularly useful for connecting the jet with synoptic systems (e.g., Spensberger et al., 2017) and elucidating the influence of diabatic processes on the jet (e.g., Auestad et al., 2024) "

   On lines 23-24.

"Lower tropospheric metrics have been shown to have close links to large-scale modes of variability such as the North Atlantic Oscillation (e.g., Barnes and Hartmann, 2010; Woollings et al., 2010) and the zonal momentum budget in the mid-latitudes (e.g., Simpson et al., 2014)."

On lines 31-33.

2. The effects of time filtering have been a particular topic of interest. The additional sensitivity tests have certainly helped here. I think it's also possible that the nature of the new method is somewhat scale-selective, with a focus on the larger scales. So although no explicit spatial filtering is applied, some may be implicit, hence the relative lack of sensitivity to time filtering. This could be considered and discussed if relevant.

   Thank you for highlighting this interesting point. We do have explicit spatial filtering incorporated through L^* and L^*_λ, so this likely reduces the sensitivity to time filtering by removing small EDJOs that may be detected in unfiltered data. It is less clear to us what role there is for implicit spatial filtering. In the absence of explicit spatial filtering, any implicit filtering would be connected to the choice of U^*_{850}. If U^*_{850} is sufficiently small to only identify large EDJOs in unfiltered data, then it could act as an implicit spatial filter. However, this would be offset against identifying smaller regions of weak westerlies. For higher U^*_{850}, the EDJO spatial scale would get progressively smaller and would identify small areas of localised strong wind in unfiltered data, which would not be removed in the absence of explicit spatial filters. However, we expect that in practice the relative lack of sensitivity to time filtering is most strongly influenced explicit spatial filtering through L^* and L^*_λ.

   The following sentence has been added to address this:

   "We note this result is likely to depend on the inclusion of explicit spatial filtering through L^* and L^*_λ, which remove small EDJOs that might be more frequently detected in unfiltered data."

   On lines 393-394.

3. The role of the length scale in reducing the impact of Greenland tip jets is clear in the revised manuscript. It might be worth mentioning as a caveat another common limitation of lower-tropospheric metrics is that different modelling systems have different approaches for treating below-ground regions of the 850hPa surface, so there can be some sensitivity to this.

   Thank you for your comment. We have added the following sentences to the conclusions:

   "One limitation affecting lower tropospheric jet metrics is that different approaches are used for interpolating below the surface over high topography, e.g. Greenland, which can affect the input wind fields. Therefore, care is required to check the influence of, e.g. missing data over Greenland for lower tropospheric metrics."

   On lines 371-374.

4. The authors should check that statements they have made about the Spensberger method are correct. In general, it might help to highlight positive aspects of other indices, such as that by Barriopedro et al, as well as aspects that they are trying to improve on.

   Statements regarding the method of Spensberger et al. (2017) have been checked and corrected. We have also added the following sentence regarding the metrics proposed by Barriopedro:

   "Barriopedro et al. (2022) also extend the measures of the EDJ to better characterise its variability by introducing measures of longitudinal position and sharpness; however, the implementation

of their measures relies on similar underpinning assumptions as Woollings et al. (2010) and Messori and Caballero (2015)."

On lines 59-62.

5. The comments made by Reviewer 2 should, of course, be addressed.

   The changes suggested by reviewer 2 have been implemented in the manuscript. The descriptive statistics given on line 3 in the abstract have been removed as we agree that it's not clear how these contrast with the JLI. The sentence on line 3 now reads:

   "the distribution of the daily winter $\overline{\phi}$ is unimodal which is in contrast to the trimodal distribution of the daily Jet Latitude Index (JLI) described by Woollings et al. (2010)."

   The change from **noise** to **higher persistence** has now been made on line 5. We apologise for not doing this the first time.

   We have corrected line 21 from **vertical** to **quasi-horizontal** when discussing the work of Spensberger et al. (2017).

   Thank you for this comment, we agree that this is an added benefit of the method and the following sentence has been added to lines 71-73 of the manuscript:

   "Further, issues with the identification of orographic features are mitigated by incorporating a minimum EDJ length, which is shown to reduce the occurrence of jets located at high northern latitudes."

   The change to line 83 has been made.

References

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