"A climatological characterization of North Atlantic winter jet streaks and their extremes"

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Recommendation: Major Revisions

Overview:

This study uses the ERA5 reanalysis to perform a climatological investigation of North Atlantic jet streaks during the winter months and compares their characteristics against a group of "extreme jet streaks". This study is particularly topical, since extreme jet streaks can have an influence on the aviation sector and lead to the development of high-impact weather events. Overall, I found the study to be very interesting, well-illustrated, and well-written. I also found that the authors were very thoughtful and thorough in considering their approach to identifying jet streaks and their characteristics. My review of the manuscript raised a few questions, however, that are largely methodological in nature and may be worthwhile for the authors to consider as part of a revision. I also feel that some of the results could be further bolstered via the application of statistical significance testing. For these reasons, I've recommended the manuscript undergo a round of Major Revisions prior to being considered for publication in *Weather and Climate Dynamics*.

Major Comments:

1. There are some aspects of the methodological approach that might benefit from further clarification:
    a. L103: The height of the tropopause on the equatorward side of the jet can often extend as high as 350K during the wintertime, especially in cases of polar/subtropical jet superposition (Winters et al. 2020). Is there a particular motivation for cutting the analyses off at 340K and are you missing anything by not considering isentropic levels above this level say up to levels of 355K? The secondary peak in Fig. 8 at higher potential temperatures partly motivates my concern.
    b. L124: In many cases the flow can be highly amplified and jet streaks can be meridionally oriented rather than predominantly zonal. How does the methodology handle this common occurrence?
    c. The selection of specific threshold values and percentiles for the analysis could benefit from more justification. For instance, the choice of 92.5 m/s for extreme peak intensities is not much different than the median of all jet streaks (i.e., its within one standard deviation according to Fig. 6). I find the extreme jet streaks category to be a bit more rigorous since there's an area criterion associated with it.
    d. L155: It is conceivable to me that the isentropic level corresponding to the maximum wind speed is likely to change throughout a single event. Is anything done to account for this as part of the analysis and is it necessary?

e. L195: Similar to one of my earlier comments, is any methodology performed to account for the different orientations of jets when compositing (i.e., a rotation of fields so that they are aligned with the jet axis)?

2. The results sections show interesting differences and statistics regarding the characteristics of jet streaks and extreme jet streaks, but it might benefit from the application of statistical testing to verify to what extent the differences are statistically significant.
   a. For instance, could a bootstrap test be applied to the mean wind speeds associated with the different jet regime categories in Table 1 to determine whether they are indeed uniquely different? A similar practice could be applied to the content of Fig. 12 or Table 1.
   b. In any of the composite analyses, a statistical bootstrap test could be performed to determine to what extent the characteristics of the near-jet environment during extreme jet streaks are significantly different compared to the composites of all jet streaks.

3. In some of the composites the raw fields are used rather than anomalies (i.e., PV, sea-level pressure). Would using anomalies potentially be more effective given that the climatology of these variables can vary substantially throughout the cool season and potentially bias the analysis, especially if the jet streaks are also located in different parts of the Atlantic Basin? In addition, I might have missed it, but are the composited fields weighted at all to account for smaller distances between grid points at high latitudes compared to lower latitudes? If not, treating the input maps equally will unfairly weight the composites towards jet streaks that are located at higher latitudes.

Minor, Specific, and Typographical Comments:

Abstract
L3–4: Consider specifying that this upper-level divergence pattern is only specific to Northern Hemisphere jet streaks, since the pattern reverses in the southern hemisphere. If looking for a more unified phrasing you could refer to the regions as "equatorward entrance region" or "poleward exit region".

L12: Consider using a different term rather than "deepening" when referring to the intensification rate of jet streaks, as this is more standard when referring to cyclogenesis rather than jets. Perhaps acceleration could be a suitable alternative term?

1. Introduction
L34: The Harnik et al. (2014) study largely considers merging of the two jets from a seasonal perspective, but this also can occur on synoptic time scales and lead to some of the extreme winds observed in this study (e.g., Winters et al. 2020). It might be worth highlighting this environment as part of the introduction discussion on jet streaks, as well.

Winters, A. C., D. Keyser, L. F. Bosart, and J. E. Martin, 2020: Composite synoptic-scale environments conducive to North American polar–subtropical jet superposition events. *Mon. Wea. Rev.*, **148**, 1987–2008, doi: 10.1175/MWR-D-19-0353.1

L69: Should the cyclonically curved case favor ascent beneath the left-exit region? I believe the effects of flow curvature and speed changes should theoretically cancel in the right-exit region.

2. Methods and Data

L127: Could you expand a bit more as to why this percentile threshold is chosen? Was it determined empirically, and how sensitive are the results to this chosen threshold?

L134: This wind speed threshold is very defensible, but it might help to offer some citations to other studies that have used comparable thresholds for the jet.

L223: I'm a bit confused by the terminology, "wind speed curvature". In particular, are you referring to the curvature of the flow that would be associated with troughs and ridges, or more so describing the gradient in wind speeds present within a jet. If the latter, I might recommend rephrasing to avoid any confusion in interpretation by a reader.

3. Results

L278–280: Could more detail be provided as to why this wind speed threshold is chosen for an extreme jet streak? Why not a much larger wind speed given that this speed seems rather close to the mean of all jet streaks?

L282–286: These trends certainly do align with the results of Shaw and Miyawaki (2023), but how much of this result can be attributed to more observations over the North Atlantic in more recent decades compared to earlier in the dataset? Discussing potential uncertainties in this result, or evaluating the significance of trends, may benefit the text.

L295: The bimodal distribution in the central isentrope potentially motivates extending the search range for the core isentrope to higher isentropic levels that cover the entire troposphere.

L302: This trend also might relate to the strongest jet streaks being associated with superpositions of the polar and subtropical jet streams, which will feature characteristics of both polar and subtropical jets and strong wind speeds commensurate with those associated with extreme jet streaks.

L313: This is true for all subsets except for the jet streaks with peak intensities exceeding 105 m/s, if I am reading Fig. 9a correctly. Consider a revision to the text accordingly.

L386–390: These are interesting statistics, but would it be possible to perform some type of significance testing, such as a bootstrap test, to evaluate the extent to which these differences in frequencies during jet streak periods are indeed significantly different than climatology?

L395: Prior to discussing the transitions, it might be worth emphasizing initially that the predominant observation is that jets tend to persist in their genesis regime before discussing the transitions which are secondary in their frequency.

L428: I am having a bit of difficulty locating the figures associated with this discussion in Section E, but they do seem associated with appendix F. References to the pertinent appendix figures to support the discussion in L428–460 would be helpful, as well, to guide the reader.

L433: It might be worth emphasizing here, or elsewhere in the manuscript, that this result is not particularly surprising from a theoretical standpoint, given that supergeostrophic flow is expected at the apex of upper-tropospheric ridges from consideration of gradient wind balance.

L501–503: I found this text rather repetitive with that at the end of the previous paragraph. Consider whether this small paragraph might be deleted without any loss of content.

L515–517: I am not so sure I agree with this conclusion, as the surface cyclone is much stronger beneath the left-exit region in the extreme subset compared to the non-extreme subset. I would presume the strong pressure gradient in the extreme subset stems from the greater intensity of both the cyclone and anticyclone relative to the non-extreme, zonally-oriented cases.

Appendix A:
L633: The reference to "westward" in this sentence appears to be incomplete and might need another word or two to complete the sentence.

L635–640: This result might also highlight the M-regimes as featuring a greater likelihood of polar and subtropical jet stream superpositions, which would also align with the greater likelihood of extreme jet streaks in this category.

Figures and Tables:
Table 2: Is there a particular reason why the cluster numbering starts at 0 rather than 1? Also, might it be useful to use more descriptive names for each cluster that are tied to their respective characteristics rather than numbers?

Fig. 1: In panel (a), it is a bit confusing that there is one red arrow that points away from the diagram at the lowest isentropic level. What does this red arrow correspond to and how is it different from the one at the level of maximum wind speed?

Fig. 2: The solid black and dashed black lines that correspond to the mean and median, respectively, are a bit difficult to see against the grey histogram bars. Could different colors be used for these quantities? Similar considerations may also apply to other figures.

Fig. 9: Some of the dashed lines and solid lines are a bit difficult to see in panel (b). Could they be made a bit thicker? Could the hours associated with lifetimes also be included along the x-axis in panel (a). It is a bit difficult to compare the box and whisker plot values against values along the y-axis on panel (b) – far distance for the eye to travel.

Fig. 11: I understand why the authors ordered the panels the way they have, but it seems a bit counterintuitive that the first timestep of the evolution corresponds to panel (c) for all jet streaks rather than panel (a). Could the panel labeling conventions be changed to match the temporal evolution of the jet streak evolution? The panel labels are also incorrect for the extreme jet streaks and should be (e-h). I noticed similar errors in a few other figures, as well.

Fig. 12: I might recommend a revision to the caption to not refer to the presence of a low-level jet, which can have a much different definition than the jet features considered in this study. Namely, low-level jets often correspond to isolated low-level wind speed maxima that decay in intensity by some amount with altitude. I don't believe that is the case for the jets considered here. Instead, I might just recommend referring to times in which the jet resides in the S, M, and N regimes.

Fig. 13: I like this figure a lot, but it is a bit counterintuitive to me that the N regime is on the bottom and S regime is on the top. Could these regimes be reversed in their position on the plot so that the display of these jet regimes matches their characteristic location on a map?

Fig. 16*:* Might it be possible to include a contour that corresponds to the position of 2-PVU? That way the position of precipitation and surface sea-level pressure anomalies will be easier to link to the upper-level jet structure.

Fig. E1: Should the time of maximum intensification in this four-panel plot correspond to panel (b) rather than (a), since (a) is described as the start time later on in the caption?

Fig. F1: It appears the panel labels might be incorrect within this figure, since (a-d) is duplicated for both sets of 4 panels.

Appendix F Composite Figures: For each group of four panels that correspond to a cluster, could a label be added to more clearly identify which group of panels corresponds to a cluster (i.e., similar to what is done at the top of Fig. 15)