Review of «An ERA5 Climatology of Synoptic-Scale Negative Potential Vorticity-Jet Interactions over the Western North Atlantic”,

by Alexander Lojko, Andrew C. Winters, Annika Oertel, Christiane Jablonowski, and Ashley Payne.

Summary: In this study, the authors provide a systematic analysis of interactions of 250 hPa negative potential vorticity (NPV) features with the extratropical jet in the North Atlantic sector. The authors first introduce an identification scheme for NPV–jet interactions and then go on to discuss climatological characteristics of NPV–jet interactions in the North Atlantic sector, such as frequency of occurrence, seasonal variability and ~20 year linear trends. Detailed composite analyses of three types of NPV–jet interactions then reveal common dynamical features and differences between NPV–jet interaction events of distinct classes, which are further illustrated based on three detailed case studies. These analyses lead the authors to the conclusion that the presence of NPV features near the jet systematically intensify the latter and are furthermore associated, systematically, with an increase in wave activity, with potential effects on the downstream development of synoptic-scale Rossby wave packets.

Overall, the writing as well as the figures are very clear, and the analyses have been carried out with greatest care. Very evidently, this is work that has been carried out with great attention to detail, which I highly appreciate. However, in its current form the study is somewhat vague regarding the scientific (and any potential societal) relevance of the research that is presented and at times it is not entirely clear why the authors choose certain concepts and analyses to construct their arguments. While I fully see the intellectual appeal in working towards better understanding NPV, I think the authors should invest some time and thought into the framing of their study, and they should state in a very explicit manner why we should all care about NPV. This would avoid conveying the impression that their work is a purely academic exercise (see major comment 1 below). Furthermore, I have a major concern regarding their composite approach, which, in my opinion, might compromise the degree to which conclusions regarding NPV-jet interactions can be drawn from their results (major comment 2). Finally, I wonder why the authors restrict their analyses to a single pressure level, rather than a seasonally varying isentropic level, and I ask the authors to provide more compelling arguments detailing why a single pressure level is the ideal choice (major comment 3).

Despite these concerns I believe the study will be a valuable addition to the growing NPV literature and, if framed more explicitly, it will likely be of relevance also to a broader readership, e.g., colleagues working on clear air turbulence or numerical weather prediction. I thus encourage the authors to revise their manuscript in line with my comments below before I can recommend acceptance of this study.

Major comments:

1) The motivation for this work and its scientific (and potentially societal) value needs to be stated more explicitly. In the abstract the authors motivate their work with (lines 2–5) “it is postulated that NPV may be relevant for the large-scale circulation as it has been observed

to …, accelerating jet stream winds and degrading numerical weather prediction skill". Similar statements can be found in the introduction. However, these statements presumably hold for any intense and diabatically influenced negative PV anomaly on the equatorward side of the jet, irrespective of whether the sign of its absolute PV values is negative or positive. In its current form, the manuscript thus does not yet fully convince me that NPV features are a subset of tropospheric negative PV anomalies that warrant special attention.

The authors could alleviate this concern by providing evidence (e.g., from previous studies) that NPV-jet interactions lead to more degraded forecast skills than interactions of benign negative PV anomalies (i.e., with low but positive absolute values) with the jet. Similarly, is there evidence suggesting that CAT is more vigorous/frequent for NPV-jet interactions than for benign "low PV"-jet interactions? If so, please state this more prominently. I acknowledge that, by theoretical arguments, NPV should not be stable and thus it is interesting intellectually to study NPV, which might help to further our theoretical understanding of PV dynamics. In my opinion this puzzling aspect of NPV is a perfectly valid reason for studying climatological characteristics of NPV, but it is not mentioned very prominently as a motivation for their work currently. Rather, the authors currently motivate their study with a need "to further understand how cloud processes impact jet stream dynamics" (line 28). If that is indeed the author's goal, then I believe they look at a subset of events that is unnecessarily narrow. I thus ask the authors to think again about why exactly climatological characteristics NPV–jet interactions in the North Atlantic warrant a dedicated study, and to be more explicit about the benefits and scientific value of this research.

2) It is unclear how much of the signals shows in Fig. 6 (one of the central figures in this manuscript) are due to NPV-jet interactions and how much of these signals simply result from the difference in the large-scale flow during NPV-jet interactions and the climatology. Can the authors exclude the possibility that the composite anomalies in Fig. 6 would look very similar if one considered (instead of the NPV-jet interactions) time steps with just a large-scale flow situation similar to that during the actual NPV-jet interactions? If not, then the key conclusions about the effect of NPV-jet interactions on the upper-level dynamics, e.g., on lines 15–16 "The results show that NPV-jet interactions can in-situ strengthen the mid-latitude jet stream and could be dynamically relevant in enhancing downstream development, …" do not seem justified.

   The key question for me is how much of the signals depicted in Fig. 6 would be retained if the authors compared the variables depicted in Fig. 6 during NPV–jet interactions with time steps with a similar large-scale flow pattern occurring without NPV-jet interactions? I think this question needs to be addressed in some form, as otherwise it is very much unclear how much we learn about NPV-jet interactions from the authors composite analysis. As an inspiration, the authors could consider the study of Pohorsky et al. (2019), where these authors faced essentially an analogous problem when examining the interaction between recurving tropical cyclones (TCs) and the jet, which preferentially happen in an amplified flow situation, that, by itself already features considerable upper-level PV and IVT anomalies compared to climatology. Pohorsky et al. addressed the problem by comparing composite fields during instances of actual TC-jet interactions with a climatology that was constructed from days without TC-jet interactions, but with similar large-scale flow configurations than during the actual TC-jet interactions (i.e., flow analogues). Please consider whether such an approach could be useful here too.

3) I agree that (line 98) “using only one particular isentropic level can miss NPV features”, but using only one pressure level has similar caveats. Since you anyways refer to the Röthlisberger et al. (2018) study: Why not choosing a seasonally varying isentropic level that follows the isentropic level of the jet? In that case you would have all the benefits of analyzing PV on isentropic levels whilst still analyzing data from only one level.

Minor comments:

1) Line 7: This may be a naïve question, but how sure are you that these synoptic-scale NPV bands in ERA5 are real and not an artefact of the IFS model or assimilation procedure employed in ERA5?
2) Line 9: Not yet clear what the 1.2% mean here. Maybe rephrase to “occur at >1.2% of all time steps at particular grid-points”?
3) Line 34: “causing it to perturb polewards” -> “causing it to move polewards”
4) Line 47: Delete “spatially”
5) Line 63: “stream” instead of “steam”
6) Line 74–75: Related to major comment 1. I don’t see why this is an interesting research question. By the PV invertibility principle it is clear that if you move an intense negative PV anomaly (e.g., a NPV feature) towards large positive PV values then the flow in-between accelerates. How could it be any different? Please rephrase this “objective” of the study once you have clarified how you would like to motivate your work.
7) Line 115: How exactly is the major axis length-scale calculated?
8) Comment on Section 2.2.1: I find the description of your algorithm very clear and understandable!
9) Line 172: What is the distance metric that you use in the K-means clustering?
10) Lines 167–188: Why exactly do you use the full PV field for the K-means clustering? This gives a lot of weight to stratospheric PV features (e.g., trough intensity, TPVs etc.), as they simply have larger PV values than tropospheric features. Alternatives focusing more on the large-scale flow pattern rather than stratospheric PV features exist: You could do the clustering on the natural logarithm of the PV field, whose gradients are linearly related to wind speeds (Martius et al., 2010) or you could consider a binary (stratospheric = 1, tropospheric = 0) field to emphasize the geometry of the large-scale flow, as in Pohorsky et al. (2019). Note: I’m not suggesting you need to do that, but perhaps discuss why you chose the full (latitude weighted) PV field for the clustering.
11) Line 186: Delete “enhanced”.
12) Line 203: What is the |U-vector| exactly? I assume it is |U-vector| = |(U, V)|, i.e., the magnitude of the base state wind. Currently you just write “|U-vector| is the wind speed”. Please clarify.
13) Line 205: How are derivatives computed here?
14) Line 207–208: What does “negligible change to PVU” mean exactly? Do you mean that the PV value of these features doesn’t change much?

15) Line 234 and Fig. 3. You report >12% occurrence frequencies of NPV features in the subtropics. I find this number surprisingly high, given that these features should not be dynamically stable and, theoretically, should decay very rapidly. I acknowledge your comment regarding the MCS observations, but I still think that more discussion of the magnitude of these numbers is warranted here, including some explanation or hypotheses as to why they are so frequent. Is our theoretical understanding so poor or are they perhaps partly artefacts of the IFS/data assimilation underlying ERA5?
16) Line 238: Brackets around the "Thompson et al (2018)" citation are missing.
17) Line 245 and Fig. 3b: Is it possible that we just see the occurrence frequency of jets in this panel? I'd appreciate some additional panels in Fig. 3, for instance (a) one showing the frequency of jet occurrence, (b) the conditional probability to observe a NPV-jet interaction provided a jet is close by, or (c), conversely, the conditional probability of observing a NPV-jet interaction provided an NPV feature is there.
18) Line 251 and Fig. 3c: Along the same lines as above: Do we see these anomalies in Fig. 3c just because, by design, NPV-jet interactions need a jet streak, i.e., strong winds? Instead of the current Fig. 3c, I'd be more interested in seeing whether wind speeds are stronger during NPV-jet interactions than during instances when "only" a jet is nearby.
19) Line 255: Apologies for potentially misunderstanding something here, but I find this hypothesis test not very convincing. The null-hypothesis you are testing is that there are no wind speed differences between climatology and instances of NPV-jet interactions. However, by definition of NPV-jet interactions, there have to be strong winds (due to the jet). Thus, the result of increased wind speeds compared to climatology is expected by design.
20) Figure 4: Please add a panel on jet frequencies, such that we can see to what extent the results in Fig. 4f simply result from jet frequency variability.
21) Line 272: That's an interesting finding. Could you speculate about why there is larger interannual variability in NPV area in winter compared to summer?
22) Fig. 5 and lines 300–301: What do you mean with the statement "does not satisfy the false discovery rate"? Do you maybe mean "is not significant at alpha=xx, which corresponds to a maximum false discovery rate of 0.1 in Fig. 5b"? Bear in mind that the FDR test is nothing else than a tool to determine on which significance level one should reject the null-hypothesis under scrutiny (see also next comment).
23) Caption to Fig. 5: I find your description of how you determine the significance somewhat confusing. I appreciate that you employ the FDR test, but bear in mind that the FDR test (with e.g., maximum allowed FDR = 0.1) is just a tool to find an appropriate significance level so that less than 0.1*100% of the "discoveries" will be erroneous. That is, the FDR gives you a p-value threshold based on which you deem your results significant or not. Therefore, I think your statement " ... are statistically significant to the 98th percentile" makes no sense. Rather, you should state that you determine the significance level (I called it alpha in the comment above) based on the FDR test with a maximum false discovery rate of 0.1. Ideally, you would also state what that resulting significance level is for panels (a) and (b).
24) Figure 5c,d: Are there reasons to believe that the trends you observe are forced by increasing GHG concentrations or do you believe that they are part of internal variability? Some comments on that important question (also in the text) would be highly appreciated.
25) L316: "patterns" instead of "pattern".

26) Discussion of Fig. 6: Related to major comment 2. Please make it very clear whether you consider the anomalies shown in Fig. 6 as related to the NPV-jet interaction events or whether they are merely features of the composite large-scale flow structure during these events.
27) L350: Do you mean “upstream” and “downstream” part of the ridge or really “poleward or equatorward flank of the ridge” as you state currently. If the latter is the case, then I don’t understand this sentence.
28) Figure 6g–l: Please choose the length of the reference vectors such that we can see the vectors in these panels better (i.e., make them larger).
29) L 372–375: I think there is a verb missing in this sentence. As it is now, I don’t understand this sentence.
30) Fig. 8d-I and discussion thereof: I appreciate that the authors went through the struggle of applying the WAF diagnostic, but please provide more framing here. Why exactly is this the right method to better understand how NPV features affect wave amplification and propagation here? What exactly do we learn from these analyses in terms of physical and mechanistic understanding?
31) Lines 617–618: Related to major comment 2: I’m not convinced your results allow drawing such a “causal” conclusion. The enhancement of wave activity propagation along the jet does not necessarily have to be related to the NPV features. It could well be that NPV features just preferentially occur in large-scale flow situations with amplifying waves, without the NPV features being causally related to the amplifying waves.

References:

Pohorsky, R., M. Röthlisberger, C. M. Grams, J. Riboldi and O. Martius (2019), The climatological impact of recurving North Atlantic tropical cyclones on downstream extreme precipitation events, Mon. Wea. Rev., 147, 1513–-1532, doi:10.1175/MWR-D-18-0195.

Martius, O., C. Schwierz, and H. C. Davies (2010). Tropopause-level waveguides, J. Atm. Sci., 67(3), 866–879, doi: https://doi.org/10.1175/2009JAS2995.1.