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Comment on wcd-2022-56

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Referee comment on "Similarity and variability of blocked weather-regime dynamics in the Atlantic–European region" by Franziska Teubler et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-56-RC1, 2022

The paper investigates processes leading to the formation of blocked weather regimes in the North Atlantic region. Among the 7 identified weather regimes based on a year-round classification, 4 of them are dominated by an anticyclonic anomaly and can be defined as blocked regimes: (i) Atlantic ridge (AR), (ii) European blocking (EuBL), (iii) Scandinavian blocking (ScBL) and (iv) Greenland anticyclone (GL). The methodology is based on the decomposition of the Potential Vorticity (PV) equation into various tendency terms and to project them onto each blocked-regime PV anomaly to identify the terms and processes responsible for the formation of each blocked regime. It follows a similar approach as that done in some previous studies using the relative vorticity equation but is here applied to the PV equation and the decomposition of the terms is different as well. Another originality is that the decomposition of the weather regimes is here based on the recent year-round 7-regimes classification of Grams et al (2017) that provides more refinement than the classical 4-regimes classification of Vautard (1990). The methodology is also based on the decomposition of the blocked regimes onsets onto two types of clusters; one associated with a westward displacement of the anomalies (retrograde cluster) and another associated with an eastward displacement (upstream cluster). Even though it lengthens the paper and the number of figures, the most original part of the results comes out from that last decomposition. It shows that the upstream cluster is mainly dominated by linear Rossby wave dynamics while the retrograde cluster by non linear eddy interactions. Other processes are discussed: baroclinic processes, moist processes, dependence on the season or on the type of regime transition. Overall the paper is well written and well organized, figures are well chosen and clear, and the result on the difference between retrograde and upstream clusters makes the paper particularly interesting to better understand weather regimes formation and transitions. I do not have a major concern about any of the sections of the paper but my list of comments is a bit long and I recommend publication once the authors have adequately addressed them.

Major comments:

1) The introduction is well written but in my opinion it does not provide enough information on the various definitions of the blocking. A blocking is indeed a circulation pattern that blocks the mean jet but this might be created by a dipolar anomaly and not necessarily a blocking anticyclone (even though I agree that in the real world the most common situation is the domination of the high). Furthermore, the introduction gives the feeling that all blocked weather regimes, ie. where anticyclonic anomalies dominate following the definition of the paper, have low predictability or are less well represented than the other regimes (see lines 35-38). Is it really true ? Because in my opinion, the Greenland anticyclone, which sometimes refers to negative NAO, is not known to exhibit lower predictability than other regimes. Finally, defining GL as a blocked regime presents that regime as similar to the others. However, it has an effect on the jet which is very different than the ScBL, EuBL or AR. In GL, the jet is shifted to the south but is zonal whereas in the other three regimes, the jets are not straight and there is always an abrupt deviation in the westerlies latitude at some longitudes. I know the paper is quite long but since it is entirely relying on this definition of blocked regimes, some caution should be taken or some warning needs to be provided to the reader to explain that in the present paper blocked regimes refer to the dominance of anticyclonic anomalies.

2) Section 2.4: I think this section needs to be better inserted within the text. When I read it, I clearly did not undestand why there was this discussion on EOF and k-means clustering here. I thought it was a mistake and was there to describe the classification of weather regimes, which in fact was already present in the beginning of section 2.2. So reading section 2.4 was confusing. It is only in section 4.1 that I understood that it is applied to the PV anomalies to separate the weather regimes onsets onto two different clusters: retrograde and upstream. To be understood, section 2.4 needs to start by explaining why we want to do an additional claissification and precise to which variable it is applied. If I understood correctly, it is on PV but this is not said.

3) First paragraph of section 3 (lines 267-279). This paragraph is difficult to follow when we do not know what are the two pathways in question. Since the paper is already lengthy and this paragraph did not bring key information for me to understand the next subsections, my suggestion is to significantly reduce it. One or two general sentences to introduce the section would be enough.

4) Barolinic term. I struggled a lot to understand the signs of the baroclinic coupling terms. In all the reasonings I have made, I got the opposite signs. The baroclinic coupling term is -(v'_low.gradq_0)_L while the intrinsic wave propagation term is -(v'_up.gradq_0)_L. Since q_0 is a climatological mean, the baroclinic term is roughly -(v'_low_L.gradq_0) and the intrinsic wave propagation can be approximate by -(v'_up_L.gradq_0). Since the low-frequency anomalies are mainly barotropic equivalent we expect v'_low_L and v'_up_L to have roughly the same signs (even though there might a slight westward tilt with height). Hence, I would expect the baroclinic coupling term to be negative upstream and positive downstream of the anticyclonic anomalies but Figures 6a-d clearly show the opposite. Could you check the signs or show the circulation associated with v'_low, or show a section of the circulation across the negative PV anomalies ? I am pretty sure that the patterns of the baroclinic coupling term can be easily interpreted and should be presented in the paper. One possibility would be to draw a schematic to show in the paper.

5) The discussion is interesting but is quite long (2 pages) and centred on what the same team has published in the recent past. There is no really a discussion on what are the new aspects on blocked regimes the paper brings out and what are the results confirming previous studies.
a) There is only one sentence (in the summary 5.1) where the authors mention that GL is consistent with the traffic jam theory of Nakamura and co-authors while the other three regimes are not. I am a bit surprised that this is precisely GL that fits the traffic jam theory because it corresponds to a high-latitude blocking that cannot really stop the mid-latitude wave propagation. Because the other three regimes present a mid-latitude blocking, they would have been better candidates for the traffic jam theory.
b) Also since the paper highlights the linear vs nonlinear processes in driving regimes I would expect more discussions with regard to the results of Michel and Riviere (2011) who have also focused on the relative importance of linear vs nonlinear processes in the formation of weather regimes. For instance, Michel and Riviere (2011) found that linear processes first trigger the formation of WR and then nonlinear processes associated with wave breaking play an important role in reinforcing the WR. In the present paper, I would have expected the quasi-barotropic linear dynamics to be first dominant and then followed by the non linear eddy term. But figure 7 does not show it (I do not see any time lag betweenthe two terms) and this is puzzling for me. Is it because the quasi-barotropic linear term does not contain all the linear terms (it should be added to the baroclinic one) ? Is it because the non linear eddy term does not include all the non linear terms (it misses the term with divergence -v'_div.gradq') ?

Minor comments:
1) Line 144: The text says that a 90-day running mean is applied but in appendix a 30-day running mean is applied. Why is there a difference ?
2) Line 145: a reference for k-means clustering is needed and maybe a sentence describing it.
3) Line 160-169: the number of cases selected by the described procedure should be provided. How many cases or days correspond to the composites of Figure 1?
4) Line 185: please indicate here that the derivation of that equation is provided in Appendix A.
5) Line 229: why is the computation of envelopes of synoptic Rossby waves important here ? Since Fig.3 shows a simple composite, I would expect a simpler diagnostic of synoptic Rossby wave to be also relevant like EKE (eddy kinetic energy where eddy contains the synoptic part only). Is there a reason for using such a sophisticated diagnostic here ?
6) Line 235-241: as mentioned above, I would expect more details on the objectives of that subsection and also the variable(s) to which is applied the algorithm
7) Line 267-279: as said above, this paragraph is really too long and too general to be understood by the reader.
8) Figure 3: Could you explain the choice of the values for the magenta contours ? 16, 18 and 20 m/s are very close to each other and values a bit below 16 m/s could be relevant as well. For instance, why not 10, 15 and 20 m/s ?
9) Line 292-294: I am a bit suprised by the fact the northern branch could not be a continuous extension of the North Atlantic waveguide. for the transition from zonal to ScBL regime studied in Michel et al (2012, GRL), some continuity is shown between the western Atlantic storm-track and the northeastern edge. I see two explanations. One is the fact that the present study considers all transitions to ScBL and not only one. The other is that the northern branch can be seen by lowering the magenta contour interval in Fig.3
10) Line 297-298: I am surprised that GL comes out as the illustration of the traffic jam theory because, as said before, the whole jet is shifted southward and there is no interruption of the westerly wave guide in such a regime.

11) Figure 5: dashed blue contours are not visible.
12) Line 320-326: the discussion on the existence or not of wave trains as function of the WR sounds a bit unfruitful for me. EuBL, AR and ScBL have more zonally oriented anomalies while GL is structured with a meridionally oriented dipolar anomaly. So the PV tendencies reflect more the structures of WR they are building rather than the existence of wave trains in my opinion.
13) Line 335-338: The effect of the dipolar anomaly on the reduction of the mean PV gradient is difficult to appreciate because the mean PV gradient is not shown and the dipolar anomaly anomaly is tilted (see Fig5c). Is it really useful to say that here ?
14) Line 336-346 and Figure 6: the fact that a low-level positive temperature anomaly lies below the anticyclonic anomaly is consistent with the fact that the anomalies are barotropic equivalent and dominated by upper-levels. Indeed, an upper-level anticyclone with amplitude decreasing as it goes closer to the surface is associated with a warm anomaly over the whole column because the sign of the vertical gradient of the geopotential is the same as the sign of temperature following hydrostatic relation. As mentioned above, this gave me the wrong sign for the baroclinic coupling term. So I would need clarification on that aspect.
15) Line 385-386: usually in k-means classification, the choice of the number of clusters will dependent on the ratio between inter and intra variances. Does the number 2 appears as the most appropriate number ?
16) Line 393: Is the sum of retrograde and upstream clusters equal to the number of cases shown in Fig.1 ?
17) Line 425-428: Sentence is difficult to fully understand.
18) Line 430: the structure of the sentence in the parenthesis is a bit strange.
19) Line 491: the fact that baroclinic coupling contributes negatively is strange following the reasonings I have made (see above). I am probably wrong...but it needs clarification.
20) Line 493: What is meant when the authors say "located outside the regime pattern" ? Does it mean that the amplitude of the divergent tendencies are far away from those of the regime ? or does it mean divergent tendencies are in quadrature ?
21) Line 536: same question as in 20)
22) Line 554-555: the fact that GL (or negative NAO) is formed via local processes has been emphasized by other studies (see Feldstein, 2003; Benedict et al. 2004)

reviewed by Gwendal Riviere