Review of manuscript 2020-039 submitted to Weather and Climate Dynamics

Oceanic origins for wintertime Euro-Atlantic blocking

by Yamamoto et al.

This study investigates the origin of upper-level potential vorticity anomalies contributing to wintertime Euro – Atlantic blocking in reanalysis data by making use of backward air parcel computations. The authors find substantial contributions from ascending, diabatically heated air masses, thus contributing to the growing body of evidence for the importance of diabatic processes in blocking formation and maintenance. As such this is not completely novel, but this study additionally asks about the specific pathways and history of the involved air masses, as well as the source regions of moisture contributing to these air parcels, which to my knowledge has not been previously done in that detail. In particular, the authors find that (I) the oceanic frontal zones in the Atlantic and the Pacific are key for supplying moisture to these ascending air parcels, (II) an important portion of air parcels originates in the Pacific, where it ascends and then moves quasi-isentropically towards the block along with the jet, (III) a small but nevertheless interesting group of air parcels first ascends in the Pacific, then descends over North America, and picks up moisture in the western Atlantic from where it ascends again into the block.

However, it is these novel insights where the study is somewhat less convincing from a methodological point of view. This concerns, in particular, the classification of air parcels into dry and moist ones, the attribution of moisture uptakes to the Gulfstream and Kuroshyo ocean fronts, as well as the statistical analysis of air parcels (or more precisely the lack of climatological context against which the specific results obtained for blocking air parcels can be compared to). Hence, in order to convincingly support the novel insights of this study, some additional evidence needs to be presented – which I believe can be achieved based on the air parcel data set by some adaptations of the methodology for analysing them and with additional analyses. Some specific suggestions are given below. The study is mostly clearly written with occasional clarifications and improvements of the language needed.

In summary, I found this study interesting to read, revealing some novel insights into the history and moisture origins of air masses contributing to blocking. With improvements to the methodology and additional evidence presented, I believe that the study has the potential to become a valuable contribution to WCD.

Specific comments:

1. **Blocking identification:** The authors use a 500 hPa geopotential height based index to identify blocking. However, throughout most of this study a potential vorticity perspective is adopted. For example it is argued that ascending, diabatically heated air parcels inject low potential vorticity air into the upper troposphere and by that contribute to the formation / maintenance of blocks. From that point of view, a potential vorticity based blocking identification would seem more natural.

The methodology for identifying blocking also matters for determining starting positions of air parcels. Geopotential height anomalies associated with blocks tend to be located equatorward of the corresponding potential vorticity anomalies (e.g., Fig. 10 in Steinfeld et al. 2019). As a result, air parcels started from grid points with geopotential height anomalies may not be representative of the air masses with the strongest upper level potential vorticity anomalies in a block. This is all the more true since the blocking index is based on 500 hPa geopotential height, whereas air parcels are initialized at upper tropospheric levels.

Please note that I do not want to raise the cumbersome question of which method is the most appropriate for identifying blocking in general, but rather which is the most appropriate for the purpose of this study.

2. **Air parcel starting points:** As I understand from the methodology section, air parcels are randomly initialized at heights between 7000 m to 8000 m AGL. Is this sufficiently high to capture the low potential vorticity air masses underneath the - in the case of a block elevated - tropopause?

   In addition, how is it made sure that no stratospheric air parcels are selected? Some of the “dry” air parcels have fairly high potential vorticity values exceeding 2 pvu. This suggests that they are stratospheric air parcels that may not be related to the block. This in turn biases the conclusions regarding the differences between dry and moist air parcels in favour of a stronger influence of the moist air parcels. A simple remedy could be to select air parcels by requiring them to have sufficiently low potential vorticity at the starting time.

3. **Classification of air parcels into dry and moist categories:** I find the criterion for selecting dry and moist air parcels not very transparent. Air parcels are categorized according to whether they once have positive LHF along their path. As I understand, latent heat fluxes (LHF) are only considered when air parcels are within the planetary boundary layer (PBL). Since normally LHF is directed upward, this criterion simply separates air parcels that once resided in the PBL from those that didn’t. Why not simply classify them into ascending and non-ascending air parcels instead of dry and moist?

4. **Identification of moisture uptakes:** From the evidence presented (for instance Fig. 3), it remains unclear to what extent the moisture uptakes are linked to the Gulfstream and the Kuroshyo. For example, Fig. 3c shows a large region south-west of the blocking region in the Atlantic and another in the Pacific where the air parcels experience large positive changes of specific humidity. These regions extend quite far away from the oceanic frontal zones. Of course it appears plausible that an important portion of moisture originates from the oceanic frontal zones, simply because these regions have climatologically very large LHF. Based on the evidence, i.e., Fig. 3c, it is impossible to tell whether this is really the case.

Given that the analysis of moisture origin is one of the very unique and novel aspects of this study, I suggest the evolution of specific humidity along moist air parcels is analysed in more detail in order to support the claims. The study by Pfahl et al. (2014) on the moisture sources contributing to WCBs might give some ideas about how this could be achieved in a more quantiative way.

S. Pfahl, E. Madonna, M. Boettcher, H. Joos, and H. Wernli, 2014: Warm conveyor belts in the ERA-Interim dataset (1979–2010). Part II: Moisture origin and relevance for precipitation. J. Climate, 27, 27–40, https://doi.org/10.1175/JCLI-D-13-00223.1.

5. **Climatological context:** Some of the study's conclusions rely on statements that a certain category of air parcels constitutes a certain percentage of blocking air parcels. To properly interpret these fractions, they should be compared to climatology or non-blocking air parcels. For instance, the interesting (!) two-basin pathway is shown to constitute 1.3% of the analyzed air parcels when the air parcels are extended to 20 days backward in time. I would have been surprised if such air parcels would not exist if air parcels were extended sufficiently far backward in time. To what extent is the fraction higher for blocking air parcels compared to climatology?

6. **Title:** I find the title of the study somewhat misleading as it portrays the ocean as a direct cause of blocking. Instead I'd recommend a title along the lines "Oceanic moisture sources contributing to wintertime Euro – Atlantic blocking", which would be more in line with the content of the paper.

7. **Abstract:** It does not mention the two basin pathway nor the comparison of Atlantic and Pacific blocking, both of which represent rather novel aspects of the study. Also the final sentence of the abstract very generally underlines the importance of diabatic processes for the maintenance of blocks, which as such is not a particularly novel insight. Instead, I'd recommend to end the abstract on an emphasis of the implications of the novel findings.

8. The LHF values in the Figures and discussed in the text appear not plausible throughout the manuscript. For example, Fig. 3 states that LHF values are shown for 0.01 W m^{-2} ... whereas in the Gulf Stream region climatological LHF is on the order of 200 W m^{-2} and also over much of the oceanic basins the LHF reaches mean values on the order of 100 W m^{-2}.

9. What does the range of 36 – 55% of moist particles signify (abstract, as well as several mentions in the manuscript)? Are these the minimum and maximum found among all events?

10. Fig. 5 and L207: Given that many air parcels have stratospheric potential vorticity values

at the time of their release, it is no surprise that dry air parcels have higher potential vorticity values than moist ones: virtually all air parcels with stratospheric potential vorticity will automatically be attributed to the dry category as they cannot have ascended from the lower troposphere and, thus, be classified as moist. Hence, the different potential vorticity values of dry air parcels compared to moist ones is likely simply an artefact of how the air parcel starting points are defined.

11. Fig. 7: The caption states that the LHF is accumulated. Why then are the units in W / m^2? Are these mean values instead of accumulated?

12. Fig. 9: I am wondering about the increase of potential vorticity along the Pacific Pathway. The air parcels seem to gain potential vorticity while they are already located at upper levels. What is the reason for this increase? I would expect that potential vorticity would rather decrease once the air parcels are located above the heating maximum.

13. Fig. 10: I don't understand how the climatology is compiled. Why is it so "spiky" compared to the trajectory distribution?

14. Fig. 13: I am confused about the units in these plots. How are the probability densities scaled? Shouldn't units then be % / (g / kg) in (a) for instance?

Wording and typos:

L5: … to **~~an~~** atmsopheric reanalysis data.
L13: … while the remaining one third **originates** from the Pacific (?)
L26: blocking activities → blocking activity
L31: by the contribution of feedback → by feedbacks
L39: a blocking formation → the formation of a block
L64: … by giving off large amounts of heat and moisture …
L88: resolution (singular)
L117: The sentence is confusing as it is not clear whether blocking events for winter or for the whole years are considered.
L119: Here and elsewhere: interpolated to instead of interpolated at
L150: Replace "swifter" by "stronger"
L169: Replace "dehydrating" by "drying"
L190: Replace "arise" by "originate"
L220: "locally sourced" sounds awkward, please rephrase (here and elsewhere)
L245: Suggest to rephrase sentences such as "The Atlantic pathways tend to originate…" to "Air parcels following the Atlantic pathway originate …" throughout the manuscript.
L374: Comparison with **~~the~~** Pacific blocking
L407: remain~~s~~
L411: those **ascending** via the Atlantic and **those ascending** via the Pacific
L413: swift → rapid

Caption Fig. 1: sector**s**

Caption Fig. 4: suggest rephrasing “for moist particles above the marine PBL or within the PBL over land”

Caption Fig. 5: … supplied **to** the moist particles …

Caption Fig. 10: … interpolated **to** their…

Please check the appropriate use of “the” throughout the manuscript. In many cases it can be omitted, in others it should be added. Some examples:

L23: … association with ~~**the**~~ extreme weather events …

L51: … of ~~**the**~~ European blocking …

L60: … dominant type of ~~**the**~~ Rossby wave breaking …

L66: … amplify ~~**the**~~ wintertime Atlantic blocking …

L92: … by a reversal of **the** meridional geopotential height gradient