Response to reviewers comments

May 1, 2021

1 Reviewer 1:

1.1 General comments:

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.

We would like to thank the reviewer for supporting our manuscript and for providing constructive comments and suggestions that have extensively helped to improve it. Our response to your comments are interwoven in bold in the text below. Please note that any changes in response to reviewer comments are highlighted in blue and that some minor changes aimed at further clarification that were not in response to particular reviewer comments are included in black in the revised manuscript. These changes did not alter any qualitative or quantitative conclusions from the original manuscript.

1.2 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 themost appropriate for identifying blocking in general, but rather which is the most appropriate for the purpose of this study.

Thank you for this comment. We agree with the reviewer that it would be intriguing to see if the picture differs with use of blocking definition based on PV, given the significance of PV in our main conclusion. Nevertheless, we have decided to use what we believe to be one of the most robust blocking definitions currently available under the constraint that the CFSR dataset does not PV on pressure levels, which makes it difficult to implement the PV definition based on this dataset. Alternatively, we could release particles from a few PV-defined blocks based on ERA-Interim and compare the properties along these particles against those along our original particles to test if the same conclusions hold. However, mixing different datasets does not seem ideal either.

For these reasons, we have left the suggested point as a future study as mentioned from l.234 in the revised manuscript:

In the meantime, a closer look at the difference plots (right panels) highlights that moist particles tend to be situated poleward of dry particles for those particles released from the lower altitudes (5,000 m asl and 3,500 m asl). Previous studies showed that geopotential height anomalies associated with blocks tend to be located equatorward of the corresponding PV anomalies (e.g., Fig. 10 in Steinfeld and Pfahl (2019)), suggesting that if we repeat the analysis with the blocks being defined with PV anomalies, the signature of moist particles making up of the blocking cores may be more clearly seen. Confirmation of this speculation, however, should be a topic of a future 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.

   We appreciate your feedback on this point and your suggestions. Following the reviewer's comment, we have additionally released particles from ∼10,000 m a.s.l., which approximately corresponds to 250 hPa. In addition, we have removed those particles which reside in the stratosphere (with PV values exceeding 2 PVU) throughout the tracking period. Release of particles from a higher altitude revealed that, while the fraction of the moist particles decreased compared to those that are released from lower altitudes (updated Figure 6), the key conclusions remain the same (not shown). Furthermore, implementation of the new criterion to remove stratospheric particles resulted in further highlighting the relative importance of diabatic effect as can be seen in the following table which summarizes the result for the particles released from 7500 m asl.

Moist particle fractions before and after removal of stratospheric particles					
	dry particles	moist particles	Atlantic particles	Pacific particles	two-basin particles
before removal	63.6%	36.4%	22.8%	12.1%	1.3%
after removal	58.7%	41.3%	25.8%	13.6%	1.4%

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?.

   As Reviewer 1 points out, the current criterion essentially separates the particles that once resided in the PBL from those that did not, which is exactly the goal of the current study. PBL is where the oceanic influence can be directly felt by the particles via turbulent heat fluxes, and positive (upward) latent heat flux at the time and location when and where a particle is situated is a very good indicator for the moisture pick-up by the particle. Indeed, Yamamoto et al. (2015) applied the same technique and found that the supply of turbulent heat fluxes from the ocean defined in the same manner describes the potential temperature change along the particles pretty well (over 80%).

   We could instead classify particles into ascending and non-ascending categories as the reviewer suggests. However, we do not have any prior knowledge that those particles that undergo substantial ascent to become a part of blocks have to get fuelled by the ocean. Additionally, ascending particles may also include dry particles that undergo orographic lifting without any role for moisture content. For these reasons, we believe that our current definition adequately addresses the question of interest in a more direct manner. We have added the following sentence to further justify our methodology in l.139:

   Applying the same technique, Yamamoto et al. (2015) showed that up to 88% of the along-trajectory potential temperature variability released from the western Europe can be explained by the accumulated heat fluxes along the trajectories thus computed alone, indicating that this method adequately captures along-trajectory diabatic changes.

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.

   Thank you for this suggestion to apply the same technique as Pfahl et al. to the current study. Following the reviewer's comment, we have applied the technique to identify the moisture sources contributing to WCBs (Figure 11b in the revised manuscript). The analysis reveals that while the moisture sources for the WCBs are wide spread

over both the Atlantic and Pacific basins, both the Gulf Stream and Kuroshio do stand out as the primary moisture sources. The analysis further reveals that the two-basin particles retain some of the moisture gained over the Pacific basin (23.5%) to be lifted over the Atlantic basin. The descriptions about the method and the results of the diagnostic have been added starting from l.369.

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 nonblocking 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?

   Thank you for this comment. Following the reviewer's comment, we have additionally released 20 particles when there is no blocking found within ±10 days from every 2° × 2° grid from 7500 m above sea level and the result is summarized in the table below.

Comparison between blocking and non-blocking particles					
	dry particles	moist particles	Atlantic particles	Pacific particles	two-basin particles
blocking	58.7%	41.3%	25.8%	13.6%	1.4%
non-blocking	62.9%	37.1%	23.0%	12.3%	1.2%

   Applying chi-square test, we found that these values associated with the non-blocking particles are all significantly different at the 99% confidence level from those associated with the blocking particles. We have added the descriptions about the non-blocking particles in the method section (l.131) and this result from l.159 and from l.255 in the revised manuscript as follows:

   The fraction of the moist particles for the non-blocking particles is 37.1%, thus slightly lower than the blocking case. We found that this difference is significant at 99% confidence level, indicating that blocking particles preferentially contain more moist particles compared to non-blocking conditions.

   In comparison, non-blocking particles consist of 23.0% for the Atlantic particles, 12.3% for the Pacific particles, and 1.2% for the two-basin particles out of the 37.1% of the total moist particles. While the differences seem small particularly for the two-basin particles, all of these fractions are significantly different from the blocking particles at the 99% confidence level.

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.

   Thanks for this comment. We have changed the title of the manuscript accordingly.

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.

   We appreciate this comment. As for the two-basin particles, we have added the following sentence in l.12:

   There is also a small fraction of moist particles that takes up moisture from both the Pacific and Atlantic basins, which undergoes a large-scale uplift over the Atlantic using moisture picked up over both basins.

   Also, following reviewer 2's comment, we decided to remove the section about Pacific blocks for this paper. As for the last point about the contribution of diabatic processes for the maintenance of blocks, as far as we know there is no prior study which explicitly underscored the moist origin of the maintenance processes, and we believe that it is

one of the important findings of the current study that is worth highlighting in the abstract. We have elaborated more on the last point as follows in the revised abstract:

A previous study identified a blocking maintenance mechanism, whereby low PV air is selectively absorbed into blocking systems to prolong blocking lifetime. As they used an isentropic trajectory analysis, this mechanism was regarded as a dry process. We found that these moist particles that are fuelled over the Pacific can also act to maintain blocks in the same manner, revealing that what appears to be a blocking maintenance mechanism governed by dry dynamics alone can, in fact, be of moist origin.

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}.

   We apologize for the confusion. The LHF values shown in Figure 3 signify the total LHF along the moist particles within each $2^o \times 2^o$ grid cell on those specific days, divided by the total number of moist particles, thereby these values indicate the average LHF contributing to the blocking per particle. We have amended the figure caption as follows:

   SHF and LHF are computed as the total SHF and LHF along the moist particles found within each $2^o \times 2^o$ grid cell on the indicated days, divided by the total number of moist particles, indicating the average LHF contributing to the blocking per particle

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?

   The range signifies the moist particle fraction at different release level, which, as the reviewer mentions, was not clearly stated anywhere in the previous version of the manuscript. The beginning of the second paragraph of the abstract now reads as follows:

   The analysis reveals that 28 - 55% of particles gain heat and moisture from the ocean over the course of 10 days, with higher percentages for the lower altitudes.

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.

    As also mentioned in the response to the reviewer's second comment, we have removed those particles that reside in the stratosphere throughout the tracking time and remade Figure 5 in the revised manuscript. The new figure reveals that even without the stratospheric particles the same conclusion remains true, such that the potential vorticity values are significantly different between the moist and the dry particles.

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?

    Thanks for this comment. The way it is constructed is such that LHF is "accumulated" for each $2^\circ \times 2^\circ$ grid cell and then divided by the number of moist particles for each category as in Figure 3, but it was not clearly stated as the reviewer points out. The figure caption is corrected accordingly as follows.

    Total LHF along the moist particles found within each $2^o \times 2^o$ grid cell, divided by the total number particles

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.

It is an interesting point and we speculate that it might be due to the fact that at this point Pacific particles are under the effect of PV mixing in the upper troposphere, which acts to increase the PV value of the Pacific particles. We have added the following sentence in l.308.

In the meantime, it is notable that PV along these Pacific particles increases as they ascend approximately from day -5, which is the opposite to the expectation that PV decreases above the heating maximum. Although further investigation is necessary, we speculate that the reason behind this PV gain may be that they are under the influence of PV mixing with the higher upper-tropospheric PV near the tropopause (e.g., Hoskins 1991).

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

    Thank you for this comment. We have remade the figure by constructing the climatology by applying 30-day low-pass filtering to the PV field. The revised Figure 10 now exhibits much more smoothed climatological PV.

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?

    Thank you for the comment. The unit has been corrected accordingly.

1.3 Wording and typos:

We would like to thank the reviewer for pointing them out. We have made the corrections accordingly.

2 Reviewer 2:

The authors present an innovative study on the contribution of moist diabatic processes to blocking in the Euro-Atlantic sector. By means of diagnostics based on Lagrangian particle dispersion model calculations, they highlight the link between ocean basins and different pathways to support the low-PV core of blockings. The study fits well into the scope of WCD, is overall well-structured and well written. There are some specific aspects as detailed below. Since a reconsideration of the method and discussion part, and revision of figures could induce substantial changes to the manuscript, I recommend major revisions.

We thank the reviewer for the insightful comments, which helped us improve the manuscript. We have addressed each of the concerns, including the one started in this introductory comment, in bold text below and in the manuscript. Please note that any changes in response to reviewer comments are highlighted in blue and that some minor changes aimed at further clarification that were not in response to particular reviewer comments are included in black in the revised manuscript. These changes did not alter any qualitative or quantitative conclusions from the original manuscript.

2.1 Major comments

1. Provide a better connection on the literature regarding moisture origin diagnostics, and method description for the Lagrangian particle calculations. Stohl and James (2004, 2005) have previously used FLEXPART to study moisture sources from specific humidity changes along trajectories. Sodemann et al. (2008) and later studies introduced modifications to this approach to quantiatively estimate final contributions to precipitation at an arrival domain, taking the sequence of moisture increases/decreases into account. In their approach, the authors now consider all location with positive LHF during at least one time step as a moisture source, irrespective of the sequence of events thereafter. How likely is it then for example that evaporation identified in the Pacific still contributes substantially to water vapour when airmasses also enter the boundary layer over the Atlantic? I think the approach of the authors does at present not allow to clearly conclude on this question. A more balanced discussion of the results regarding the two-basin pathway is therefore needed.

   Thank you for this constructive comment. Following the reviewer's advice, we have applied moisture analysis following Pfahl et al. (2014) as was also suggested by the other reviewers. The result indicates that the moisture of Pacific origin does contribute to the uplift over the Atlantic basin by 23.5% and 41.6% for the two-basin particles for the 10-day and 20-day tracking, respectively, as can be seen in Figure 11b in the revised manuscript. We have added the description about the moisture analysis and the result starting from l.369.

2. Improve presentation of several figures, as detailed in the Minor Comments below.

   Thank you for the comment. We have modified the figures accordingly.

3. Improve writing regarding use of English grammar (in particular regarding the article "the").

   We have gone through the manuscript carefully, especially being mindful of how we use articles in the revised manuscript.

2.2 Minor comments

- L. 7 (also L. 63): massive amount: consider to specify how large, or moderate statement
- L. 64: swift, massive, large: consider to specify these qualitative statements further,

  Concerning these two comments, since we specify them quantitatively in the later parts of the manuscript when necessary, we would refrain from going into too much

details quantitatively in introduction and abstract. We have thus decided to keep these statements as is.

- L. 139: and/or: unclear which one is valid, please rephrase

  The corresponding part has been rewritten as follows in l.146, where we are still keeping the expression of "and/or", since we do not know at this point which hypothesis is true:

  We speculate that the difference between our and Pfahl et al. (2015)'s results might arise from the differences in the blocking definitions, Lagrangian tracking methods, and/or reanalyses used in two studies.

- L. 140: immature/onset: unclear which one is valid, please rephrase

  We have modified the corresponding part following other comments we have received, and this sentence is deleted in the revised manuscript.

- Figure 2: A drawback of the approach in this study is that number densities are displayed, rather than actual contributions of the latent heat flux to the blocking. This limitation should be included in Sec. 6 (Discussion). (It may be that Fig. 7 contains such information, but it is not clear from the writing).

  An advantage of showing number densities such as Figure 2 is that we can visually see where the particles are situated. The actual contribution of moisture as identified by moisture diagnostic is now included in Figure 11b in the revised manuscript.

- Sec. 3.1: The text leading through Fig. 3 and 4 is hard to follow, are all 12 panels in both figures really needed? Maybe a comparison with histograms comparing above/within boundary layer points could be a clearer illustration of the property changes?

  Our main question in the current study is to identify *where* the particles pick up necessary moisture, and we believe these figures provide useful information.

- Figure 6: Grey contours are hardly visible. Maps missing coordinates and labels. Could the information for two of the three altitudes be moved to the appendix?

  Thanks for this comment. In the revised figure, we now shade only the regions where the difference is significant at 95% confidence level. We believe that it is worthwhile to keep all the release levels in this figure to show the robustness of our finding that the moist particles tend to make up of the blocking cores at different levels. As for the map coordinate we have added the description about the meridians and parallels in the caption to avoid a clutter.

- Figure 7: Please add missing coordinates to maps. It is not clear from the writing how this result has been obtained.

  The beginning of the figure caption has been clarified as follows:

  Total LHF along the moist particles found within each $2^o \times 2^o$ grid cell, divided by the total number particles (colour shading)

  As for the map coordinates, we have added the description about the meridians and parallels in the figure caption as in the revised Figure 6.

- Figure 8: Please add missing figure labels. Please add a legend to the figure.

  A figure legend has been added and the description about the plotted meridians and parallels have been added in the figure caption.

- Figure 9: The visual presentation of this figure is poor. Please consider showing regular lat-lon, height-lon, height-lat plots instead, and provide information about the spread. Maybe the figure is not needed at all? Missing panel labels.

  We believe that the figure provides a valuable information as to where the mean trajectories are located. We have also created lat-lon, height-lon, and height-lat plots

as suggested by the reviewer, but we found that the 3D figure is more preferable, as we end up trying to make 3D trajectories out of 2D plots in our mind anyways. Instead, we have improved Figure 9 in the revised manuscript to for the better visual presentation. Due to the large variability in the particle location, visually illustrating the spread is not quite feasible. We believe that the standard deviation indicated on Figure 8 should suffice.

- Figure 11: Please add missing coordinates to the maps.

  Figure caption has been updated, which now includes the description about the plotted meridians and parallels.

- Figure 12: Color pattern masks trajectory locations, please redesign figure, if the figure is really needed. Maybe it can be included as supplementary material?

  We believe that this figure is essential in providing the information in what could possibly give rise to the different moist particle pathways. The figure has been updated to improve the visual presentation.

- Figure 13: Increase contrast of grid lines and size of figure text.

  We are not sure what the reviewer means by "contrast of grid lines", but the figure has been updated, in order to improve the visual presentation, which hopefully resolved the issue.

- Figure 14: Include labels with all panels, consider splitting in two figures. The Pacific perspective does not fit seamlessly with the title of the manuscript, consider relocating to supplementary material, changing the title, or motivating more clearly.

  Thank you for this comment. We agree with the reviewer that the Pacific perspective may not entirely fit with the scope of the current study and we have decided to keep it for another short paper.

3 Reviewer 3:

This paper presents an analysis of the moisture source regions of winter Northern hemisphere blocks, with a focus on those in the Euro-Atlantic region. While several previous studies have highlighted the importance of diabatic processes for blocking, the moisture source regions have not previously been characterised. It does not seem surprising that some of the air entering Euro-Atlantic blocks actually ascended in warm conveyor belts over the Pacific several days previously. However, the role of this source region in supplying low PV air to blocks is interesting.

The paper is generally easy to follow although there are many very small English language errors. Overall my comments are relatively minor.

We would like to thank the reviewer for supporting our manuscript and for helpful comments which have helped us improve the manuscript. Our responses to each of your comments are interwoven in bold in the text below. Please note that any changes in response to reviewer comments are highlighted in blue and that some minor changes aimed at further clarification that were not in response to particular reviewer comments are included in black in the revised manuscript. These changes did not alter any qualitative or quantitative conclusions from the original manuscript.

3.1 Major specific comments:

1. Abstract, L15 You say that "While the particles of the Atlantic origin swiftly ascend just before their arrival at the blocking, those of the Pacific origin ascend additional few days earlier, after which they carry low PV in the same manner as dry particles." However, Fig 8 indicates that the PV for the dry particles is much higher than that for any of the moist particles.

   The fact that the PV value is significantly lower than the dry particles along the Pacific particles is indeed one of the key findings of the current study. What we mean by "the same manner" is, as described in Section 4.2 of the manuscript, after ∼day 4 the Pacific particles travel in the upper troposphere, undergoing radiative cooling, just as dry particles. We have clarified this point in the revised abstract as follows:

   While the particles of Atlantic origin swiftly ascend just before their arrival at blocking, those of Pacific origin begin their ascent a few days earlier, after which they carry low PV air

2. Fig. 3 and 4 Changes in potential temperature, q and pressure are plotted in these figures for sets of particles but it's not clear how these changes are calculated. Are the values at each location mean values for changes over a timestep (of one hour as hourly data is available?) of all particles in that location at at exactly 2, 5 or 9 days before arrival in a block? Please can you clarify.

   Thank you for this comment. As indicated by the unit on the figure's color bar, the changes are computed for every 6 hour. We have explicitly added the units to the figure caption

3. L199 You point out that previous studies have found that air ascending in WCBs tends to increase in PV below the heating region and then reduce in PV above this, entering the outflow region with similar PV values to those in the inflow. However, while your results that the WCB air arrives in the outflow region with low PV values, they don't show this increase and then decrease. Why?

   We believe that the signature is smeared out due to the large number of particles released. Each one of these particles has its own timing of uplift and different characteristics that the small feature such as this is averaged out when we take a climatological view. At the level of individual particles, one can see this increase and decrease of PV similar to Figure 6 of Madonna et al. (2014) but with much more noise. We have added the following sentence in l.221:

   This PV increase and decrease, however, are not visible in Figure 5 after averaged over millions of particles, as the timing of ascent differs for individual particles.

4. Section 3.3 How do the results shown in your fig 5 and 8 compare to those shown in Fig 1 of Pfahl et al. (2015)? Your results show that, for air travelling into a block, there can be a substantial

proportion of it with rather low values of PV (your moist particles) 7 days previously. This air warms substantially (by 10 K). Pfahl et al. show that air that undergoes substantial heating has PV values close to the local seasonal climatology (within 1 PVU). Given that you have the climatological PV values (shown in Fig 10), it would hopefully be straightforward to produce a plot of the same format as in Pfahl et al. The difference between the plots would presumably be mainly due to the different trajectory tools you've used (Lagrangian kinematic trajectory calculation compared to using an atmospheric dispersion model in your study) although the definition of blocks might also have an impact. You motivate your decision to use an atmospheric dispersion model by its representation of turblence effects in section 2.3 so it would be interesting to see this comparison.

Thank you for this comment. It is certainly an intriguing point and would be ideal to compare our results as those of Pfahl et al. (2015) by conducting the same analysis. However, conducting the same analysis is hindered by the constraint that the CFSR dataset provides no PV on pressure levels but only on discrete isentropic levels.

5. Section 4.4 I don't think we learn much in this section in addition to what is revealed by Fig 8 in section 4.3. Hence, the authors could consider omitting it.

   We appreciate this comment. However, we believe that there is some value in including this section. In particular, while Figure 8 exhibits the mean properties along the particles, it does not answer the question of what kind of meteorological conditions are responsible for giving rise to the different pathways, which is the aim of Section 4.4. We have further clarified this point in l.383 in the revised manuscript as follows:

   Thus far we have shown distinct properties along different pathways of moist particles and how they evolve differently from one another. The question, however, remains as to what kind of synoptic conditions possibly give rise to the different pathways that these moist particles are likely to take.

6. L369 The result from Fig 13d that stronger low pressure systems are associated with stronger ascent doesn't look particularly strong. That is likely to be because WCBs tend to be strongest in strongly intensifying cyclones — deep, mature cyclones often have weak WCBs. You might find it interesting to look at this paper, https://doi.org/10.1175/JAS-D-15-0302.1, on the relationship between WCBs and cyclone intensification.

   Thank you very much for enlightening us on the relationship between WCBs and cyclones. We have conducted the same analysis but for the SLP tendency. However, we did not find any significant difference among different ascent levels either as can be seen in the updated Figure 13. We have added the following sentence in the revised manuscript in l.414:

   In contrast, no such clear relationship is discernible with the sea-level pressure (SLP; Figure 13b) or with the SLP tendency (Figure 13c) found at the base of the particle ascent, despite that a moderate to strong relationship has been reported between cyclone intensification and WCB strength by a previous study (Binder et al. 2016).

7. Section 4.3 Given that most moist particles arriving in blocks are identified as WCBs given a generous definition of WCBs, there is an obvious link between your work and that of Pfahl et al. (2014, https://doi.org/10.1175/JCLI-D-13-00223.1) on the moisture origins of moist conveyor belts. Please consider adding this reference.

   Thank you so much for letting us know of this paper. We have added the reference and also implemented the same moisture analysis as can be found in l.369 and Figure 11b in the revised manuscript.

3.2 Minor specific comments:

- L37 "This transport of low PV anomalies" - it is actually the low PV valued air that is transported leading to negative PV anomalies.

  Thank you for the comment. The corresponding part has been amended accordingly.

- L137 You need to point out that the "30–40%" range is for particles in the 3 days before their arrival in the blocking system (whereas the "60–70%" range is for the 7 days before arrival).

  Done.

- L138 Sentence beginning "We speculate" does not make sense - please re-write. Does "when the direct diabatic effect tends to maximise" refer to the "immature/onset stages of blocking"?

  Yes, "when the direct diabatic effect tends to maximise" was meant to refer to "immature/onset stages of blocking", though the latter part has been deleted in the revised version. The corresponding parts have been rewritten as follows starting in l.156:

  We speculate that the difference between Pfahl et al. (2015)'s results and ours might arise from the differences in the blocking definitions, Lagrangian tracking methods, and/or reanalyses used in two studies.

- L173 Please avoid this style of writing that compresses information using brackets ("an increase (decrease) in potential temperature is concurrent with a decrease (increase)...". It saves a few words, but it is very difficult for the reader (me at least!) to follow.

  The sentence has been changed as follows:

  In stark contrast, the dominant tendency for moist particles when they are situated above the PBL (Figure 4) is such that an increase in potential temperature is concurrent with a decrease in specific humidity, which is accompanied by their ascent, while also the opposite is true (Figure 4d).

- Fig 5 Here the shading around the mean is 0.5 standard deviation. I suggest instead using the standard error (the standard deviation divided by the number of particles) as a better representation of the likely error in the mean.

  Thank you for this comment. We have computed the standard error but given a large number of particles released, the standard errors are so small that they are not visible in any of the panels, indicating that what we show in the figure as mean values are fairly close to the real mean values. The computed standard error values are instead mentioned in the figure caption in the revised manuscript.

- Fig 6 What are you normalising by to calculate the normalised difference?

  Thank you for pointing it out. The figure caption of the corresponding part now reads as follows:

  their difference after being normalized by the total particle number of each category

- Fig 7 The red contours mark the "origin locations for each grid cell". I was confused by this initially as these are backwards trajectories and previously red contours were used to indicate the particle release locations. Perhaps this description could be reworded as "particle locations at (a) 10 and (b) 20 days prior to their arrival in the blocks" or similar?

  Thank you for this suggestion. We made the correction accordingly and we agree with the reviewer that the new description is much clearer than the original version.

- L299 Why is the Pfahl et al. reference cited here?

  We cited Pfahl et al. there as they refer to those particles as dry particles, but we agree that it is confusing and the citation is omitted.

- L321 Here you say that the difference between your finding that 10% of particles arriving in blocks are in WCBs and Madonna et al.'s finding that 0.36% of trajectories released in the lower troposphere are WCBs indicates that WCBs preferentially outflow in blocks. This conclusion doesn't follow from the two previous statements. The WCBs identified by Madonna et al. were all associated with cyclones. Thus some of those WCBs will outflow into blocks, but others (probably the majority) will instead just outflow into ridges ahead of the cyclones.

  We agree that it is not totally a fair comparison given that we did not strickly follow Madonna's definition of WCBs, despite that we visually checked that majority of the

WCB particles we identified and checked are associated with cyclones as well. The sentence starting from l.351 has been modified as follows:

While this difference in the fraction of WCB trajectories from the climatology may stem from different definitions used in detecting WCBs, it may also suggest the possibility that WCB trajectories are preferentially channeled towards the blocking systems.

- Fig 12 What period do the trajectories marked in these panels cover and when does the block occur for each case? Should we be able to see the block in the panel for the final time for each sequence?

  In this figure the plotted trajectories are the ones that belong to the same cluster and appear on those specific days which is indicated on the title of each panel. Thus, they do not necessarily correspond to the same period with respect to the arrival at blocking, despite they are in the similar range. The block is not quite visible in this plot, since the geopotential height field is high-pass filtered, as described in the figure caption.

- L396 Strictly you're giving percentages rather than fractions.

  This part of the manuscript is omitted for this manuscript, following the suggestion by Reviewer 2.

3.3 Technical errors:

Thank you for all these suggestions. We have made corrections accordingly.

References

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