Response to reviewers comments

June 27, 2021

1 Reviewer 1:

I thank the authors for their detailed and convincing responses. They have substantially revised and improved the manuscript and extended it with additional analyses, alleviating most of my concerns raised previously. However, I do have two minor suggestions as detailed in the following.

We would like to thank the reviewer once again for supporting our manuscript and providing us with another round of constructive comments. Our response to your comments are interwoven in bold in the text below, and any changes in response to reviewer comments are highlighted in blue in the revised manuscript.

- One of the main points I raised concerned the way moisture sources are identified. They are now using the diagnostic by Sodemann et al. (2008) to identify moisture uptakes along the trajectories, which also takes intermittent precipitation events into account. While this diagnostic is used to identify the moisture sources contributing to diabatic ascent (Section 4.3), the previous method is still used for stratifying trajectories according to moisture origin (Section 4.1). Why not use one method throughout? I leave it to the authors to decide whether they want to change this. At the very least, they should provide an explanation in the manuscript for not just using one method.

  Thank you for this comment. While we agree that using the diagnostic by Sodemann to also partition moist particles might be a good idea, we still think that defining different moist particles according to their oceanic moisture origin and then diagnosing the moisture necessary for the large-scale ascent for those particles is ideal for our purposes. This is because we do not know *a priori* whether particles that are under the influence of the ocean necessarily ascend as WCB trajectories or not. For this reason we keep the methods as they are. To further clarify these points, we have added the following sentence from L377:

  Thus, while partitioning of moisture particles into different moisture origins (Section 4.1) has been based solely on the location where the particles are exposed to LHF from the ocean without a prior knowledge that they undergo a rapid large-scale ascent, application of this method identifies the moisture sources necessary for a rapid large-scale ascent for each of these moist particles.

- Please reference the study where the moisture source diagnostic was first introduced (now you are refering to Pfahl et al. 2014, who employed this diagnostic).

  H. Sodemann, C. Schwierz, and H. Wernli, 2008: Interannual variability of Greenland winter precipitation sources: Lagrangian moisture diagnostic and North Atlantic Oscillation influence. J. Geo- phys. Res., 113, D03107, doi:10.1029/2007JD008503.

  Thank you for this comment. We have added the reference accordingly.

Other than these two points, I do not have any further concerns. I believe that this study is a valuable contribution to WCD and I recommend its acceptance.

2 Reviewer 2:

2.1 General comments:

This is my 2nd review of this paper. The authors have responded to my previous comments satisfactorily apart from one that I have revisited below (comment relating to line 352 of the paper) and the language glitches that were present have been mainly removed. I have one remaining more major comment, but the rest are minor. This is an interesting paper on a topical subject and will be a nice addition to the WCD journal.

We would like to thank the reviewer for supporting our manuscript and kindly providing us again with detailed comments that have helped further improve our manuscript. Our response to your comments are interwoven in bold in the text below, and any changes in response to reviewer comments are highlighted in blue in the revised manuscript.

2.2 Major specific comments:

- Section 4.3 In this section the proportion of the moist particles that satisfy the ascent criterion for being within a warm conveyor belt is calculated and the moisture sources of these particles determined. I don't find this section very useful and it is potentially misleading because the constraint that all the particles must end in a block (they are back trajectories from blocks) seems to have been forgotten. As an example, figure 11 is labelled as showing WCB occurrence and moisture sources. However, it more precisely shows the occurrence and moisture sources of the WCBs that terminate in blocks. This will be a subset of the full set of WCBs as, although most WCBs ascend into developing ridges, most of these ridges are unlikely to meet the blocking criteria used. Also, as noted in the text, unlike in Madonna et al. (2014) the particles identified as belonging to WCBs have not been constrained to be near cyclones meaning that some of the particles are likely to have been mislabelled as being in WCBs. Due to these limitations this section could be removed. At the very least the fact that Fig. 11 is only representative of a subset of WCBs (and also may include some particles that are not within WCBs) needs to be clearly pointed out both here and where the result is revisited in the conclusions (L448).

  We appreciate this comment. While we are very aware that these WCB trajectories in our analyses are only those that are specifically associated with blocks, we agree that, as the reviewer points out, it was not explicitly written out. Now this point has been clarified in both main text and the figure captions. We opt for keeping the section, since here we highlight the importance of the large-scale ascent and the properties associated with it, where these properties seem fairly analogous to WCBs in terms of both ascent criteria and property, despite that we do not impose here the criterion that they have to be associated with extratropical cyclones. We agree with the reviewer's point on comparing the percentages with Madonna et al. (2014), and we have removed the specific sentence from L349 in the previous manuscript, and modified the sentence starting from L448 as follows:

  These regions also coincide with the most frequent starting points of large-scale ascent similar to warm conveyor belts associated with extratropical cyclones (Figure 11).

2.3 Minor specific comments:

- Introduction, L45 It might also be worth mentioning that block onset has been shown to be sensitive to the forecast location and intensity of upstream cyclone (https://doi.org/10.1175/MWR-D-18-0226.1) which is linked to the importance of cyclones WBCs for block onset.

  Thank you for this suggestion. A sentence has been added mentioning this point as follows starting from L46:

  A recent study has further demonstrated that forecast skill of the location and intensity of a cyclone with a WCB tends to influence the subsequent forecast of blocking onset downstream (Maddison et al., 2019).

- L126 Here you have added that "An additional criterion has been applied such that PV along the particles must remain below 2 PVU . . . in order to exclude those particles that stay in the stratosphere.". I don't think this is quite what you mean as it implies that there should be no particle PV values exceeding 2 PVU at any time. In Fig. 5 it is clear that PV does exceed 2 PVU for some particles. I think what you mean, based on your response to reviewer 1, point 2, is that have removed particles for which the PV exceeds 2 PVU throughout the tracking period.

  Thank you for this comment. Yes that is exactly what we have meant and the sentence has been modified as follows in L128:

  We then remove particles whose PV exceeds 2 PVU (potential vorticity unit; 1 PVU $\equiv 1.0 \times 10^{-6}$ m^2 s^{-1} K kg^{-1}) throughout the tracking period to exclude those particles that remain in the stratosphere.

- Consistency between Figs. 3, 4 and 7 In Fig. 3 the contours of heat flux values are very small (maximum 0.1 Wm-2) whereas the heat flux values in Fig. 4 are about 40 Wm-2. From the explanation that has been added to the caption in Fig. 3 this seems to be due to the way the heat fluxes have been calculated in Fig. 3, particularly the division by the number of moist particles. I don't understand why the values are so small though. Fig 7 also shows total LHF along the moist particles (although split according to oceanic basin origin). Here the values are up to 9 Wm-2. The explanation of the relationship between the LHF values in these three figures needs to be improved.

  Thank you for this comment. It is true that the interpretation of these fluxes was quite confusing. While in Figure 3, SHF and LHF for the first time step of each day (1 time step of 6 hourly output) was considered for all moist particles, divided by the total number of moist particles, in Figure 7 for each moisture particle class, LHF for all 10 days (40 time steps) are considered, divided by the number of particles belonging to each moist particle class. We have amended Figure 3 such that all 4 steps equivalent to 24 hours are considered, such that the difference between Figure 3 (for 4 time steps = for 1 day) and Figure 7 (for the whole 10 days) is a factor of 10. Figure 5, on the other hand, is a simple mean and standard deviation of turbulent heat fluxes along moist particles at each time step. We have modified the figure captions in order to clarify these differences.

- Fig. 9 The three-dimensional averaged trajectory locations show a maximum pressure (at the start of the averaged trajectory) for the Pacific and Atlantic particles of about 800 hPa. Fig. 8 also shows this result. Presumably the reason that the averaged trajectory doesn't get down to into the boundary layer is because the particles are in the boundary layer at different times along the trajectory. This would be worth mentioning.

  Thank you for the comment. We have added this point starting from L299 as follows:

  The lowest altitudes of these averaged trajectories do not necessarily get lower in the boundary layer, reflecting the fact that the individual particles reside in the boundary layer at different times along the trajectories.

- L352 You have added some text here in response to one of my earlier comments. However,it doesn't address my point that in comparing with the Madonna et al. (2014) you are not comparing like-with-like. Madonna et al. started forward trajectories at "all" lower tropospheric locations and found that a tiny (but important) 0.36% could be identified as WCBs. You are instead calculating what percentage of the moist particles that end up in blocks at 7000-8000 m asl can be identified as WCBs. Hence you have already inbuilt an ascent criteria by requiring the particles to travel from the boundary layer where there is positive LHF to this altitude (and also made it likely that many of these particles are likely to be in WCBs by requiring that they end in a block). It simply doesn't make sense to compare values.

  As mentioned as a response to the major comment above, we have removed this part.

- L424 At the start of this conclusions section it would be helpful to readers to clarify that when you say that the particles are "released" from blocks you are calculating backwards (rather than forwards) trajectories. Also when you say that "with the moist particle percentages decreasing with altitude" you mean the altitude that the particles release altitude.

Thank you for these comments. We have clarified the specific points accordingly.

- L160 The additional calculation of the dry and moist particles percentages for particles that travel ascend to the release altitude (n.b. backwards trajectories) from the boundary layer but are not associated with blocks is useful. The differences in the percentages compared to those for particles that are released in blocks is small although the statistical test indicates that they are different. Presumably the reason that the differences is small is due to the fact that may of these non-blocking particles are ascending into ridges that don't meet the blocking criteria. So what this study is telling us is the development and maintenance of blocks, rather than non-stationary ridges, is more likely if the particles that end up within them pick up a lot of moisture from the oceans. I suggest pointing out that distinction between blocking and non-blocking particles will be dependent on your blocking criteria and that non-blocking particles may well be ending up in a ridge that doesn't quite meet your blocking criteria (so leading to the relatively small differences in the percentages of blocking and non-blocking dry and moist particles).

  We appreciate this point. We have rewritten the sentence starting from L160 of the previous version of the manuscript as follows:

  The fraction of moist particles for the non-blocking particles is 37.1%, which is only slightly lower than the blocking case, but nevertheless we have found that this small difference is significant at the 99% confidence level. This result indicates that blocking particles preferentially contain more moist particles compared to non-blocking conditions, whereas non-blocking particles may well be ending up in a ridge that does not quite meet the blocking criteria used in this study.

2.4 Technical errors:

- L130 "we" should be "as".
- Fig. 4 caption This seems to repeat itself. You say that it shows information for "moist particles above the marine PBL or within/above the PBL over land, including those moist particles that are located within the PBL over land".
- Caption fig. 3 Change "indicating the average LHF contributing" to "indicating the average SHF or LHF contributing"
- L221 Change "averaged" to "averaging".

We greatly appreciate that you have pointed these minor errors out. They have been all corrected accordingly.