Manuscript: WCD-2021-57

Moisture origin, transport pathways, and driving processes of intense wintertime moisture transport into the Arctic

by Lukas Papritz, David Hauswirth, and Katharina Hartmuth

Reply

We would like to thank the two reviewers for their evaluation of our study and their comments. We consider their suggestions very helpful and following teir recommendations we performed additional analyses and revised the manuscript as detailed below. The reviewer's comments are given in **blue** and our responses in **black**.

Overview over main changes to the manuscript and additional analyses

Following the reviewers' comments, the main changes to the manuscript and additional analyses performed are:

- For the events of *intense zonal mean poleward moisture transport* considered in this study we adopted the well-established term *moist-air intrusions* (i.e., following Doyle et al. 2011, Woods et al. 2013).
- We improved the writing as suggested by the referees. In particular, we expanded the introduction by including additional references and a more extensive discussion of what is known about large-scale drivers of moisture transport into the Arctic and the moisture origin, in particular emphasizing more the relationship between blocking and NAO+ as found by Luo et al. 2017, 2019. Furthermore, we clarified the methodology (identification of moist-air intrusions, definition of the integrals) and provide further explanation of the selection of trajectory starting points with the aid of an example cross-section for the sample case presented in section 3 (Supp. Fig. 1).
- We performed a sensitivity analysis of moisture uptakes wrt. the choice of moisture uptake threshold. The results show that the sensitivity is very small in terms of the moisture uptake contributions. Hence, and for the sake of keeping the manuscript as concise as possible, we decided not to include this analysis in the manuscript.
- Further, we investigated the contribution of moisture uptakes within and above the planetary boundary layer, which we believe provides interesting additional information. Hence, we included it as Supp. Fig. 2 with according reference in the main text.

- The month-to-month variability of the three clusters reveals an interesting shift from Cluster 2 (related to blocking) in early winter (mainly November) to Cluster 1 (related to storm track shifts) later in winter. We have included this analysis in section 4.5 (Fig. 9e).

In the following we present our detailed responses to each of the reviewer's comments.

Comments from Reviewer 1

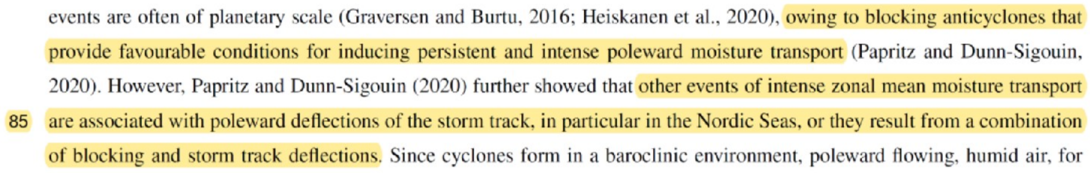
This study is an interesting topic. This manuscript examines the moisture sources of Arctic warming in different regions, whose findings help us improve an understandingof the occurrence of Arctic warming. However, there are some clarity and confusing issues in this manuscript so that I recommend a major revision. Especially, some results of this manuscript have been reported in other previous studies. However, the authors didn't make any comparison.

Major comments:

(1) Abstract is too long, which should be shortened to emphasize new findings different from previous results.

Reply: Thanks for this comment. We have shortened the abstract, emphasizing novel findings.

(2) In the introduction of this manuscript, some descriptions are misleading, which should be rewritten and re-organized. For example, please see the descriptions (yellow shading) below:



events are often of planetary scale (Graversen and Burtu, 2016; Heiskanen et al., 2020), owing to blocking anticyclones that provide favourable conditions for inducing persistent and intense poleward moisture transport (Papritz and Dunn-Sigouin, 2020). However, Papritz and Dunn-Sigouin (2020) further showed that other events of intense zonal mean moisture transport are associated with poleward deflections of the storm track, in particular in the Nordic Seas, or they result from a combination of blocking and storm track deflections. Since cyclones form in a baroclinic environment, poleward flowing, humid air, for

Before Papritz and Dunn-Sigouin (2020), Luo et al. (2017, ERL and 2019, CD) have examined the different roles of high-latitude European blocking (or Scandinavian blocking) and Ural blocking with positive North Atlantic Oscillation (NAO+) in influencing the poleward deflection of the storm track and producing persistent and intense poleward moisture transport toward the Barents-Kara Seas. The same issue also exists in other region of this manuscript. Maddonna et al. (2020) examined the control of atmospheric large-scale flows in cyclone variability over Barents-Kara Seas. Also see the review of Henderson et al. (2021). I think that the authors should cite the works of Luo et al. (2017, 2019), Maddonna et al. (2020) and Henderson et al. (2021) in the yellow shading region.

Reply: Thank you for the literature suggestions. We included Luo et al. (2019), Madonna et al. (2020), and Henderson et al. (2021) in the revised manuscript. Note that we already referenced Luo et al. (2017) in the original version.

We have reorganized parts of the introduction for the sake of a clearer structure. In particular, we have extended the overview over previous works that explored the dynamical drivers of moisture transport events and thereby included the suggested references. Specifically, L49ff now reads (proposed references in bold):

Various large-scale circulation patterns and weather systems have been identified to drive poleward transport of warm and humid air in these regions (cf. ***Henderson et al., 2021****, for a comprehensive overview). For example, moist-air intrusions in the Atlantic sector are associated with a zonally aligned dipole of mid- and upper-tropospheric geopotential height anomalies (****Luo et al., 2017****; Messori et al., 2018). The negative geopotential height anomaly is linked to an enhanced frequency of cyclones along Greenland's east coast (Sorteberg and Walsh, 2008; Villamil-Otero et al., 2018; Messori et al., 2018). In fact, Fearon et al. (2021) established that 74 % of the annual moisture flux into the polar cap north of 70° N is related to poleward propagating cyclones. The positive geopotential height anomaly, in turn, is linked to blocks over Scandinavia and the Ural mountains (Woods et al., 2013; Liu and Barnes, 2015; Gong and Luo, 2017; Ruggieri et al., 2020), whereby blocks can directly cause a poleward moisture flux via their associated circulation or indirectly via the poleward deflection of cyclone tracks (****Madonna et al., 2020****; Papritz and Dunn-Sigouin, 2020). Moreover,* ***Luo et al. (2017)*** *and* ***Luo et al. (2019)*** *found poleward moisture transport in the Nordic Seas and towards the Barents Sea to be particularly efficient when blocking over Scandinavia or the Ural coincided with a strengthened North Atlantic storm track as reflected by the positive phase of the North Atlantic Oscillation (NAO+). Similarly, the interplay of synoptic-scale waves and blocking over Alaska has been shown to contribute to moisture transport in the Pacific sector (Baggett et al., 2016). These finding are in line with Papritz and Dunn-Sigouin (2020) who identified the most intense zonal mean poleward moisture transport events at 70° N to coincide with blocking over Scandinavia or Alaska and a pronounced poleward deflection of the mid-latitude storm tracks.*

The sentences highlighted by the reviewer have been removed from the revised manuscript as we felt they are too repetitive with respect to what has been said in an earlier paragraph in terms of the circulation patterns that drive moisture transport.

(3) Some descriptions of the integrals in M and Q are confusing. The authors should clearly describe the integrals.

Reply: Thanks for pointing out, we clarified the integrals.

(4) In section 3, the authors defined the layer near 700 hPa as the mid-troposphere is inappropriate. In general, the layer between 600 and 400 hPa is defined as the mid-troposphere, whereas the layer between 1000 and 700 hPa is defined as the lower troposphere. I suggest that the authors should calculate the moisture transports and their trajectories following the new definitions of the mid-troposphere and lower troposphere.

Reply: We do not agree with this comment. There is no generally accepted definition of the mid-troposphere in terms of pressure levels we are aware of. Given the wintertime depth of the mid- and high-latitude troposphere of about 700 hPa (i.e., the mean tropopause is located somewhere near 300 hPa, cf. ERA40 Atlas; https://sites.ecmwf.int/era/40-atlas/docs/section_D25/parameter_zmpttp.html), half of the tropospheric mass is below 650 hPa. Thus, partioning the troposphere into lower and mid-troposphere at the 700 hPa level appears well justified. Moreover, a partitioning into lower and mid-troposphere in which the layer between 700 hPa and 600 hPa is not included in any of the two seems problematic at the least.

Most importantly, our choice is guided by practical considerations. Since most of the moisture transport is confined to well below 700 hPa and the air transporting moisture poleward further aloft has a very different thermodynamic history than the air transporting moisture further below, the 700 hPa level provides a natural partitioning into lower and mid-troposphere in the context of this paper. We have added the following clarification on L180:

Note that we consider the 700 hPa level as mid-tropospheric since the moisture transport characteristics at this level are clearly distinct from those at lower altitudes (cf. Sect. 4).

(5) The results in section 4 are interesting. Some results of this section have been found in previous studies and some results are new. However, the authors didn't make any comparison with previous results. For example, in section 4.1 (Characteristics of moisture transport at 70N), some results are consistent with those of Zhong et al. (2018). The authors should at least compare their results to emphasize which results are new. In section 4.2 (Geographical distribution of moisture sources), some results are consistent with the previous findings. The authors should point out their difference with the previous results to emphasize which ones are consistent with previous results and which ones are new. For example, “a tongue of enhanced moisture uptake extends into the western North Atlantic along the warm side of the Gulf Stream front” has been found in Luo et al. (2017, ERL).

Reply: We agree that some of the results concerning the geographical origin of moisture are consistent with previous work. Thereby it is important to bear in mind that many previous studies investigating Arctic moisture sources have focused on subregions of the Arctic instead of the entire polar cap as we do (i.e., Sodemann et al. 2008, Luo et al. 2017, Zhong et al. 2018, Luo et al. 2019, and Schuster et al. 2021), thus complicating a systematic comparison between our study and previous works. Notable exceptions are Vázquez et al. 2016 and Singh et al. 2017, who also considered the entire polar cap.

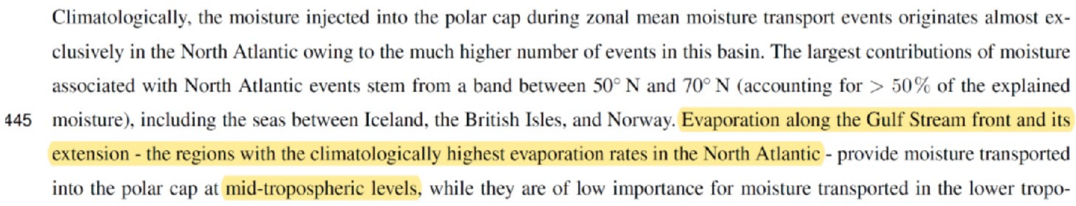
In section 5 (Discussion and conclusion) we have already included a discussion of our results in light of previous works, including the works by Zhong et al. (2018) and Luo et al. (2017) who focus on the Barents and Kara Sea subregions (see paragraph L479ff and entire section 5.2). We rather prefer to keep this order, i.e., we first present our results in section 4 and then discuss these in light of previous works in section 5. We believe this leads to a clearer structure of the manuscript than intermingling results and discussion in one single section.

(6) The definitions of $\Delta\theta+\Delta T-$, $\Delta\theta+\Delta T+$, $\Delta\theta-\Delta T-$, $\Delta\theta-\Delta T+$ are confusing. I think that the authors should revise the definitions. $\Delta\theta+\Delta T-$ should be changed to $\Delta\theta+/\Delta T-$ in order to avoid a misunderstanding.

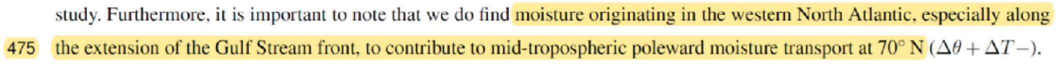
Reply: We prefer to stick to the original notation in order to be consistent with Papritz (2020) in which this notation was introduced. From the context it should be clear that this is a symbolic notation and not meant as the sum of a potential temperature and a temperature difference. For clarity, we have rewritten L272ff as follows:

This characterisation of trajectories allows for a straightforward classification based on the signs of ΔT *and* $\Delta\theta$ *into four categories. Following the symbolic notation introduced by Papritz (2020), the four categories are denoted* $\Delta\theta+\Delta T-$, $\Delta\theta+\Delta T+$, $\Delta\theta-\Delta T-$, $\Delta\theta-\Delta T+$, *where +/− indicates the sign of the respective term. Each of these categories represents a different type of air-stream characterized by a unique thermodynamic evolution.*

(7) The results in the yellow shading region were not new results, which have been also noted by Luo et al. (2017). In these regions, the authors should mention the results of Luo et al. (2017).



Climatologically, the moisture injected into the polar cap during zonal mean moisture transport events originates almost exclusively in the North Atlantic owing to the much higher number of events in this basin. The largest contributions of moisture associated with North Atlantic events stem from a band between 50° N and 70° N (accounting for $> 50\%$ of the explained moisture), including the seas between Iceland, the British Isles, and Norway. Evaporation along the Gulf Stream front and its extension - the regions with the climatologically highest evaporation rates in the North Atlantic - provide moisture transported into the polar cap at mid-tropospheric levels, while they are of low importance for moisture transported in the lower tropo-



study. Furthermore, it is important to note that we do find moisture originating in the western North Atlantic, especially along the extension of the Gulf Stream front, to contribute to mid-tropospheric poleward moisture transport at 70° N $(\Delta\theta + \Delta T-)$.

Reply: Thanks for this comment. Indeed Luo et al. (2017) find that moisture transported to the Barents and Kara Seas region partly originates along the Gulf Stream front. A direct comparison of our results with those of Luo et al. (2017) is, however, hampered by the fact that the latter focus on the Barents and Kara Seas, comprising a rather small – albeit important – sub-region of the polar cap. While some of the events considered in our study are associated with moisture transport into this region, most of the events inject moisture further poleward into the high Arctic. Hence, the events are in general not comparable.

The key point we make is that the Gulf Stream front is in fact **not** the dominant moisture source for the intense events of moisture transport into the *entire* polar cap. Instead, we find that the Gulf Stream front is relevant for providing moisture injected into the Arctic at mid-tropospheric levels. Mid-tropospheric moisture transport accounts for only about 10% of the total transport.

We have rephrased the statements marked by the reviewer to make our point clearer and discuss more extensively the differences of our findings wrt. the Luo et al. (2017) study. Specifically, we now write on L455ff:

Evaporation along the Gulf Stream front and its extension - the regions with the climatologically highest evaporation rates in the North Atlantic - provides only about 10 % of the moisture transported into the polar cap. In addition, the moisture originating in this region is only relevant for moisture transport at mid-tropospheric levels but not in the lower troposphere.

A discussion in light of the previous works including Luo et al. (2017) is then given on L482ff:

They contrast, however, with the results by Vázquez et al. (2016) and Luo et al. (2017, 2019) who find lower latitudes to be important. More specifically, they identify western boundary currents such as the Gulf Stream front and its *extension as the dominant moisture sources. Some of this discrepancy is likely because of the consideration of a different target domain, which hampers a direct comparison. For instance, Vázquez et al. (2016) include major areas south of 70° N in the definition of the Arctic domain, whereas Luo et al. (2017, 2019) focus on the Barents and Kara Seas, a sub-region of the Arctic polar cap. While some of the moist-air intrusions considered in our study are associated with moisture transport into the Barents and Kara Seas, most of the intrusions extend deeper into the polar cap. Finally, it is important to note that we do find moisture originating in the western North Atlantic, especially along the extension of the Gulf Stream front, to contribute to mid-tropospheric poleward moisture transport at 70° N* ($\Delta\theta+$ $\Delta T-$).

(8) In section 5.4, the authors should emphasize new finding points different from previous results.

Reply: We do discuss the novelties and results different from previous studies in sections 5.1 and 5.2. We don't think this needs to be repeated in section 5.4, which contains final remarks and an outlook. Accordingly, we have renamed this section to "Final remarks and outlook" for it to appropriately reflect its content.

Comments from Reviewer 2

This manuscript aims to identify wintertime moisture sources, airstream pathways, and primary large-scale flow features (i.e., cyclones, atmospheric blocking, and cold-air outbreaks) linked to moisture transport into the Arctic at 70°N. To identify these aspects, the authors use trajectory calculations with moisture-uptake tracking and flow-feature detection applied to the ERA5 dataset (1979-present) to compile a set of events which exceed the 90th percentile. Overall, the manuscript is interesting and has the potential to add valuable knowledge on moisture sources, transport pathways (including thermodynamic changes along the path), and large-scale flow configurations driving anomalous moisture transport into the Arctic. However, there are some details that are not described clearly enough for me to fully understand all of the methods amongst other major and minor points outlined below. Therefore, I can not recommend this study be published in Weather and Climate Dynamics at this time, but I do think the authors could improve after major revisions.

Major comments:

1. The abstract is too long. New results on airstreams and their linkage to large-scale flow configurations are not emphasized and are not distinguished well from previous results, such as the North Atlantic is the dominant transport gateway into the Arctic (e.g., Dufour et al. 2016 and others).

Reply: We agree regarding the length of the abstract and we have shortened it substantially. The second paragraph of the abstract emphasizes the air-streams and how they are linked to the driving large-scale circulation patterns. Specifically L12ff now reads:

Focusing on intrusions in the North Atlantic (424 intrusions), we find that lower tropospheric moisture transport is predominantly accomplished by two types of air-streams: (i) cold, polar air warmed and moistened by surface fluxes, and (ii) air subsiding from the mid-troposphere into the boundary layer. Both air-streams contribute about 36 % each to the total transport. The former accounts for most of the moisture transport during intrusions associated with an anomalously high frequency of cyclones east of Greenland (218 intrusions), whereas the latter is more important in the presence of atmospheric blocking over Scandinavia and the Ural (145 events). Long-range moisture transport, accounting for 17 % of the total transport, dominates during intrusions with weak forcing by baroclinic weather systems (64 intrusions). Finally, mid-tropospheric moisture transport is invariably *associated with (diabatically) ascending air and moisture origin in the central and western North Atlantic, including the Gulf Stream front, accounting for roughly 10 % of the total transport. In summary, our study shows that moist-air intrusions into the polar atmosphere result from a combination of air-streams with pre-dominantly high-latitude or high-altitude origin, whose relative importance is determined by the underlying driving weather systems (i.e., cyclones and blocks).*

In addition, we cannot reconcile the last statement of the reviewer concerning the North Atlantic as the dominant transport pathway. In the abstract we did not present this as a novel result, instead we stated that the asymmetry in the moisture sources is a consequence of the fact that the North Atlantic is the dominant transport pathway, which as such has not been shown in any of the previous studies we are aware of. For the sake of a shorter abstract, we have removed this statement.

2. The description of how poleward moisture transport events are identified and computed is not very clear. For example, in Section 2.2 Line 130: "We then select timesteps for the further analysis based on the exceedance of the so-obtained H_L anomalies of the 90th percentile, resulting in 597 intense poleward moisture transport events." How specifically are anomalies computed? Are these daily anomalies defined as H_L minus the long-term daily mean? Is an event detected when the daily anomaly value of H_L exceeds the 90th percentile?

Reply: The computation of the climatology with respect to which anomalies are computed is described on L123ff. This follows a standard procedure (see for instance Messori et al. 2018 and Papritz 2020) that takes both the seasonality and long-term trend into account, which we wish to remove from the original timeseries. Specifically, for computing the climatology, we first apply a 21-day running mean filter on the timeseries of H_L^*, yielding a smoothed timeseries. Subsequently, we average this timeseries over 9 years. Having defined the anomalies, we then compute the 90th percentile of the anomaly timeseries. Moist-air intrusions are defined as timesteps exceeding the 90th percentile. We have clarified this in the revised manuscript and also included a reference to Messori et al. (2018) who introduced this approach. The relevant section now reads:

Finally, we remove the seasonality and the long-term trend from H_L^* *by subtracting a transient calendar day climatology. Following Messori et al. (2018), the transient climatology for a given day and year is obtained from a smoothing of* H_L^* *with a 21-day running mean filter and subsequent centred* *averaging over 9 years. At the beginning and end of the timeseries the climatology is kept constant across years. This is to ensure that neither the seasonality nor the long-term increase of poleward moisture transport bias the selection of moist-air intrusions based on a fixed percentile threshold towards the warmer (and more humid) extended winter months or the later years in the study period. This is important since our goal is to gain insight into the dynamical mechanisms linking moisture sources and the Arctic, whereas seasonality and long-term trends of poleward moisture transport are not our focus. We then select all 597 timesteps for the further analysis for which* H_L^* *anomalies exceed the 90th -percentile. From here on, these timesteps will be refered to as moist-air intrusions.*

3. It is not very clear how trajectory starting points are chosen as described in Section 2.3, beginning on Line 138: “Among all grid points, we then select the smallest subset of grid points as trajectory starting points, which accounts for 50 % of the total poleward moisture transport. With this approach, we select the grid points that contribute most to the poleward moisture transport.” Is the subset of starting grid points selected by rank in terms of qv and their contribution to the daily moisture flux at 70°N? Would it be possible to create a schematic/visual for one timestep showing the positions of the starting trajectory grid points for an event?

Reply: This is a very good suggestion that helps clarifying the methodology used for identifying trajectory starting points, thank you. We included the vertical cross section for the exemplar case discussed in section 3 of the manuscript as Supp. Fig. 1 (also shown below as Fig. R1).

And you are right that our explanation of the approach was not sufficiently clear. It is true that potential trajectory starting points are ranked by the meridional moisture transport. Then the highest ranked points are selected such that together they account for 50% of the zonally and vertically integrated poleward component of the meridional moisture flux on the day of the event. Note that contrary to how it was stated in the original manuscript, this is done using instantaneous meridional moisture transports including all three hourly timesteps on the day of the event. We have corrected this in the revised manuscript and clarified the text as follows (L134ff):

In the first step, potential trajectory starting points at 70° N are defined every three hours on the day of the event (from 00 UTC to 21 UTC) on an equidistant grid with spacing of 50 km in longitude and 20 hPa in the vertical, ranging from 10 hPa to 610 hPa above ground-level. This yields 8 × 274 × 31 potential trajectory starting points per moist-air intrusion. The instantaneous *meridional moisture transport $q \cdot v$ is then interpolated to these points (see Supp. Fig. 1 for an example) for all three hourly timesteps and points are ranked according to this transport. Finally, the highest ranked points are selected as trajectory starting points such that together they account for 50 % of the integrated poleward moisture transport on that day (red circles in Supp. Fig. 1b). With this approach we ensure that selected trajectories represent the most intense poleward moisture transport and they explain a substantial portion of the zonally and vertically integrated flux of moisture into the polar cap on the day of the intrusion.*

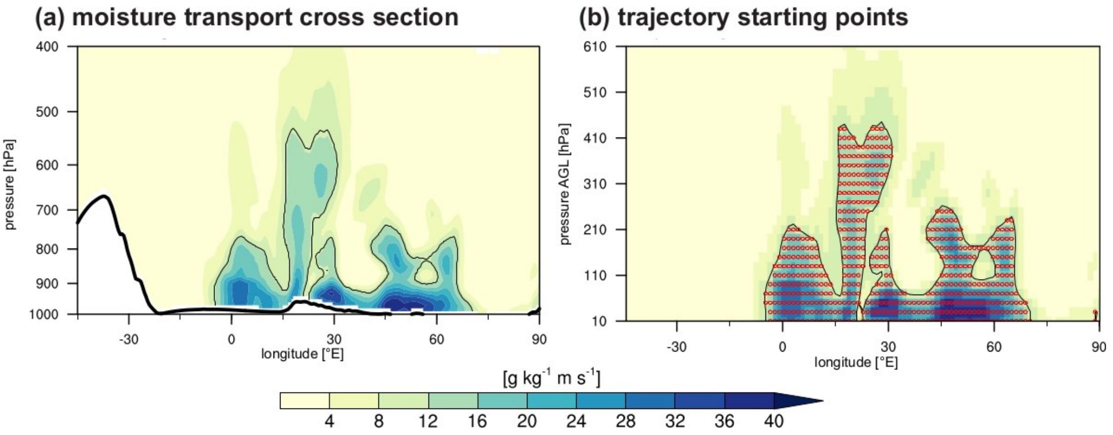


Figure R1: Vertical cross-sections at 70° N showing meridional moisture transport $(v \cdot q)$ at 12 UTC associated with the moist-air intrusion on 17 January 1995. In (a) the moisture transport is shown as a function of longitude and pressure, while in (b) it has been remapped to an equidistant (50 km vs. 20 hPa) grid. The thin black contour indicates the threshold on the meridional moisture transport for selecting trajectory starting points. Selected trajectory starting points are shown in (b) by red circles.

4. Line 150. Regarding the detection threshold for specific humidity of 0.025 g/kg/3h, have the authors explored the sensitivity of this choice, and if so, does it significantly change the spatial pattern of moisture uptake as shown in Fig. 3? The threshold used in the Sodemann et al. (2008) study was 0.2 g/kg/6h. Is the smaller value choice in this study due to temporal and spatial resolution differences in the data (relative to Sodemann et al. 2008) or because vapor above the boundary layer is incorporated or other? Does Fig. 3 significantly change if a threshold of 0.1 or 0.2 g/kg/3h is used?

Reply: Note that the threshold used in this study is not 0.025 g/kg/3h but 0.025 g/kg/h (see L150), which corresponds to a threshold of 0.15 g/kg/6h when 6-hourly data is used. Hence, it is only moderately smaller than the threshold of 0.2 g/kg/6h used by Sodemann et al. (2008).

In response to the reviewer's comment, we have performed a sensitivity analysis using thresholds of 0.01 g/kg/h and 0.05 g/kg/h. Figs. R2a,c show the moisture uptake contributions for the two new thresholds and Fig. R2b is identical to Fig. 3 (i.e., using a threshold of 0.025 g/kg/h). Comparing the three panels reveals that the geographical distribution of moisture uptakes is virtually insensitive to the choice of threshold. Very small differences can be seen in the regions with generally low contributions (i.e., < 1 % (10^6 km^2)$^{-1}$) such as over continents.

Nevertheless, choosing a different threshold leads to a systematic pattern when relative changes are considered (Figs. R2d, e). Choosing a lower threshold (Fig. R2d) leads to an increase in the contributions over land (e.g., Scandinavia) and relatively cold waters (e.g., Labrador Sea, Greenland Sea) at the expense to relatively warm ocean areas. The spatial pattern is effectively reversed in the case of a higher threshold (Fig. R2e). The reason for this pattern is that over cold ocean areas or over land, the moisture increments are generally weaker as compared to the warm ocean areas – likely owing to the Clausius-Clapeyron relationship that limits surface evaporation – and, hence, they more often fall below the threshold and are disregarded by the moisture source diagnostic.

We would like to stress, however, that these changes are very small in absolute terms (Figs. R2a-c). Accordingly, we believe that this sensitivity analysis does not provide important additional insights and we decided not to include it in the revised paper or the supplement.

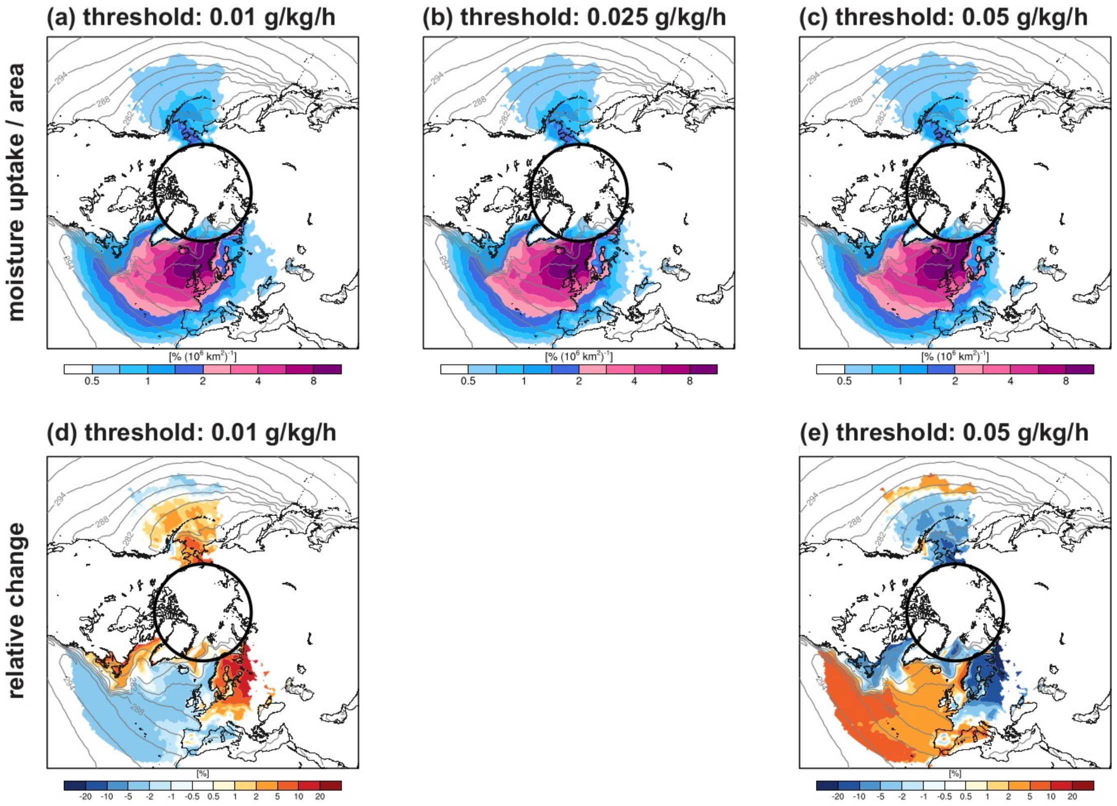


Figure R2: (a-c) As Fig. 3 in the manuscript for moisture uptake thresholds of (a) 0.01 g/kg/h, (b) 0.025 g/kg/h (baseline), and (c) 0.05 g/kg/h. (d-e) Change of moisture uptake contributions using moisture uptake thresholds of (d) 0.01 g/kg/h and (e) 0.05 g/kg/h relative to baseline of 0.025 g/kg/h.

5. Line 153. Are instances of moistening above the planetary boundary layer included in the spatial pattern shown in Fig. 3 and 4? If so, it might also be interesting to see the spatial patterns of surface versus elevated uptake on separate maps, as differences/positioning might be informative in relation to moisture sources?

Reply: This is a very good point. Indeed, in Figs. 3 and 4 no distinction is made between moistening within and above the planetary boundary layer.

Fig. R3 shows the separate contributions of moisture uptakes within and above the planetary boundary layer. As can be seen from Fig. R3a, most of the moisture uptakes occur in the planetary boundary layer, mostly related to surface evaporation. Nevertheless, a notable amount of moistening occurs above the planetary boundary layer (Fig. R3b). This is likely related to convective overturning, which can transport moisture from the planetary boundary into the free troposphere, as well as evaporation of hydrometeors.

We believe that this figure adds interesting information for some of the readers. Therefore, we have included Fig. R3 in the supplement as Supp. Fig. 2 and added a reference to the figure on L231.

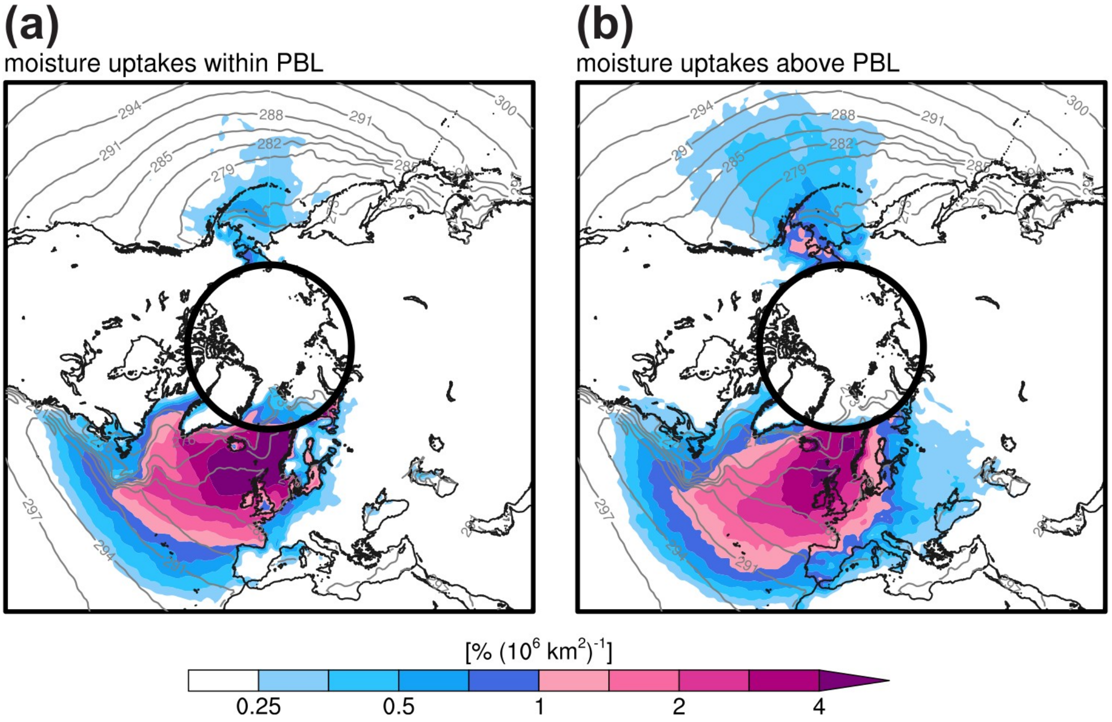


Figure R3: As Fig. 3 but for moisture uptakes taking place (a) within and (b) above the planetary boundary layer distinguished according to the boundary layer height provided by the ERA5 reanalyis. The fields are scaled such that the sum of (a) and (b) yields Fig. 3. Further note the different colorscale compared to Fig. 3.

6. Regarding the clustering of North Atlantic events and their relationship to cyclones, blocks, and cold-air outbreaks, the authors have shown interesting and convincing results in Section 4 for the combined months of NDJFM. Have the results been evaluated in the same framework except for individual month? Can the authors comment on the month-to-month variability?

Reply: Stratifying events by months and then performing the clustering analysis separately for each month would result in a poor statistics as each month contains on average less than 100 events. However, we have included a panel (see figure R4 below) showing the number of events per month as panel (e) in Fig. 9. Furthermore, we have added the following discussion (L405**)**:

The month-to-month variability in the number of intrusions is modest (Fig. 9e) with the highest number in December (nearly 2.5 intrusions per 30 days) and the lowest in March (about 1.8 intrusions per 30 days). However, the distribution of intrusions across clusters shows pronounced changes throughout winter. About 50 % of the intrusions in November are related to cluster 2, whereas cluster 1 accounts for the majority of intrusions during the other months with a peak in January. Cluster 3, finally is slightly more frequent in early than in late winter.

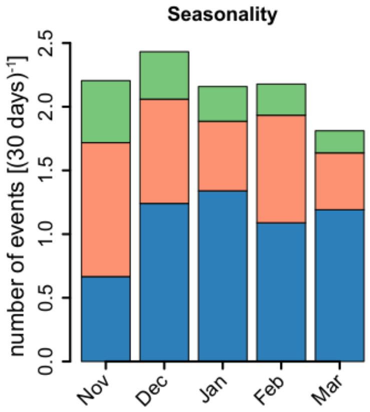


Figure R4: Mean number of intrusions per 30 days (cluster 1: blue, cluster 2: red, cluster 3: green).

Minor comments:

1. Line 1 in abstract and again on Line 41. “Poleward moisture transport occurs in episodic, high-amplitude events with strong impacts on the Arctic”. I realize the authors are interested in high-amplitude events, but moisture transport into the Arctic does occur in association with weaker cyclones or flow configurations even though the impact on the Arctic is less. This sentence should be rephrased perhaps with the caveat of “Intense poleward moisture transport occurs in episodic, high-amplitude...”. In addition, since the primary focus in this study is on transport events which exceed the 90th percentile, the authors may want to consider using the nomenclature “moist-air intrusions” introduced by Doyle et al. (2011) and Woods et al. (2013) to describe intense poleward moisture transport into the Arctic.

Reply: Thanks for this suggestion. We have adopted the term “moist-air intrusion” throughout the manuscript. Furthermore, we clarified that we mean the meridional moisture transport wherever we felt it might not have been clear in the original manuscript.

2. Line 8 in abstract. “This asymmetry between the ocean basins...” The asymmetry in the moisture uptake? Atlantic versus Pacific basins? Please clarify phrasing.

Reply: For the sake of a shorter abstract, the sentence in question has been removed.

3. Line 84 and throughout the manuscript. “Intense zonal mean transport event”. Should this be revised to “intense poleward moisture transport event”? Please use caution with the phrasing zonal mean transport. The zonal mean has been computed on the meridional flux? Line 81 shows other uses of this phrasing.

Reply: We have rephrased “zonal mean transport” to “zonal mean meridional moisture transport” and included the word poleward wherever we refer to the poleward component of the meridional transport.

4. Line 122. M “... is the mass flux into the polar cap”. Should this be “Is the average mass flux into the polar cap”?

Reply: M is the zonally integrated (not averaged) mass flux across 70°N. Following a comment of Reviewer 1 we have clarified the integrals and the definition of M.

5. Line 131. “so-obtained”. Consider rephrasing.
6. Line 223. “are spatial highly unevenly distributed”. Consider rephrasing.
7. Line 318. “which du to” revise to “which due to”

Reply: We have rephrased all of the above.

8. SST contour labels are needed in Fig. 1, 3, and 4.

Reply: Yes, indeed SST labels are missing, we have fixed this. Thanks for pointing out.