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

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Final author comments

We would like to thank the two reviewers for their evaluation of our study and their comments. We consider their suggestions very helpful and will perform additional analyses and revise the manuscript as outlined below. The reviewer's comments are given in **blue** and our responses in **black**.

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.

Thank you.

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 will shorten the abstract.

(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: Thanks for the literature suggestions. We will include them in the revised manuscript. As suggested, we will incorporate a more extensive review of the literature discussing the role of blocking for deflecting the storm track poleward.

(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 will clarify 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 fully agree with this comment. There is no generally accepted definition of the mid-troposphere in terms of pressure levels we are aware of. 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.

We do agree though, that 700 hPa is somewhat low as the lower bound of the mid-troposphere. However, this choice is guided by practical considerations. Since most of the moisture transport is confined to below 700 hPa and the air transporting moisture poleward further aloft has a very different thermodynamic history than the air transporting moisture further below, we think the 700 hPa level provides a natural partitioning into lower and mid-troposphere. We will discuss the considerations that led to this choice in the revised manuscript.

(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, 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 consider the entire polar cap.

In section 5 (Discussion and conclusion) we have already included a discussion of our results in light of the works by Zhong et al. (2018) and Luo et al. (2017), who focus on the Barents and Kara Sea subregions (see L460ff and entire section 5.2). We will expand this discussion in the revised manuscript. However, we rather prefer to keep the overall structure of the manuscript, i.e., we first present our results (section 4) and then discuss these in light of previous works (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.

We are happy to hear from the editor if he considers $\Delta\theta+/\Delta T-$ to be the better choice than $\Delta\theta+\Delta T-$. In case he suggests to keep the original notation, we will add additional explanation to limit the potential for confusion.

(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 deeper into the polar cap. Hence, the events are not in general 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 will rephrase 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.

(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. We will rename this section to “Final remarks and outlook”.

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 will shorten it where appropriate. However, we would like to stress that we already clearly mention the linkage between the airstreams and the large-scale flow configurations. Specifically, we write (highlighting the sentences emphasizing the link between airstreams and large-scale flow configurations in bold):

"Focusing on events for which 75% of the zonal mean moisture transport takes place in the North Atlantic east of Greenland (424 events) reveals that lower tropospheric moisture transport results predominantly from two types of air-streams: (i) cold, polar air advected from the Canadian Arctic over the North Atlantic and around Greenland, whereby the air is 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 dominates the moisture transport during events associated with an anomalously high frequency of cyclones east of Greenland (218 events), whereas the latter is more important in the presence of atmospheric blocking over Scandinavia and the Ural*** *(145*

events). *A substantial portion of the moisture sources associated with both types of air-streams are located between Iceland, the British Isles, and Norway. Long-range moisture transport, accounting for 17% of the total transport,* ***is the dominant type of air-stream during events with weak forcing by baroclinic weather systems*** *(64 events).”*

We don’t feel that emphasizing this aspect more would lead to a clearer abstract. Also it would be at odds with the (justified!) suggestion to reduce its length.

In addition, we cannot reconcile the last statement of the reviewer concerning the North Atlantic as the dominant transport pathway. In the abstract we do not present this as a novel result, instead we say 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. Specifically, the abstract states (L5ff)

“The bulk of the moisture transported into the polar cap during these events originates in the eastern North Atlantic with an uptake maximum poleward of 50° N. This asymmetry between ocean basins is a direct consequence of the fact that most of the moisture transport into the polar cap occurs in this sector.”

For the sake of a shorter abstract, we will consider shortening this part.

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. Events are defined as timesteps exceeding the 90th percentile. We will clarify this in the revised manuscript. Also, we will include references to Messori et al. (2018) and Papritz (2020) who used an analogous procedure to compute anomalies.

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, thank you. We will include a vertical cross section for the exemplar case discussed in section 3 and we will mark the starting points.

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 will perform a sensitivity analysis using thresholds of 0.01 g/kg/h and 0.05 g/kg/h and include the results in the supplementary material.

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, thank you. In Figs. 3 and 4 the moistening above the planetary boundary layer is included. We agree that such a decomposition might be informative and we will perform this analysis.

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 slightly more than 100 events only. However, we will include a panel showing the number of events per month in Fig. 9.

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: This is a very good suggestion, which we will adopt throughout the manuscript. Thank you!

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: Yes, we are referring to the asymmetry of the moisture uptake. We will revise this also in the spirit of a shorter abstract.

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 will rephrase “zonal mean transport” to “zonal mean meridional moisture transport”, which is definitely clearer.

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 and vertically integrated mass flux into the polar cap. We will clarify this.

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: Thanks for the above suggestions! We will rephrase accordingly.

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

Reply: Yes, indeed SST labels are missing. Thanks for pointing out.