Response to the reviewers

We are grateful for all reviewer's insightful comments and suggestions and have tried to follow advice or, when not, given good arguments for our position. Below follow item-by-item responses in red to all the comments. The added or revised sentences in the manuscripts are highlighted by blue. Line numbers referred here is from the lines in the changes track file.

Submitted on 18 Feb 2022
Anonymous referee #1

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

I wish to thank the authors for their careful response to my comments and those of my co-Reviewer. The paper is now much clearer, and a more appropriate representation of the literature is presented.

I have just a few minor details which should be attended to. Once this is done I will be able to recommend acceptance of the manuscript in ACP:

Lines 349-351: Important to emphasise the role of blocking in these processes and how the Arctic region (and its warming) is intimately involved with this. Reinforce this message with references to
Luo, Chen, et al., 2018: Changes in atmospheric blocking circulations linked with winter Arctic warming: A new perspective. J. Climate, 31, 7661-7678, and
Luo, D., Y. Yao, and co-authors, 2017: Increased quasi-stationarity and persistence of winter Ural Blocking and Eurasian extreme cold events in response to Arctic warming. Part II: A theoretical explanation. Journal of Clim., 30, 3569–3587.

Thanks for the suggestion. We have added following text to the beginning of the conclusion, 'Warm Arctic in winter is always related with long-lived blocking (Luo et al., 2017b, 2018). To the west of these blocks, Warm-and-moist air is transported to the Arctic, greatly contributing to Arctic surface warming. In this research, we name these warm events as warm-and-moist air intrusions (WaMAIs).As the persistence of Arctic blocking increases (Luo et al., 2017b), WaMAIs could be more frequent and hence lead to more amplified Arctic warming in winter.'

Lines 428-434:
Please present correct author details and publication numbers for these three papers. They are …
Kim, B.-M., S.-W. Son, S.-K. Min, J.-H. Jeong, S.-J. Kim, X. Zhang, T. Shim and J.-H. Yoon, 2014: Weakening of the stratospheric polar vortex by Arctic sea-ice loss. Nature Communications, 5, 4646, doi: 10.1038/ncomms5646.
Kim, K.-Y., and S.-W. Son, 2016: Physical characteristics of Eurasian winter temperature variability. Environmental Research Letters, 11, 044009, doi: 10.1088/1748-9326/11/4/044009.
Kim, K.-Y., J.-Y. Kim, J. Kim, S. Yeo, H. Na, B. D. Hamlington and R. R. Leben, 2019:

Vertical feedback mechanism of winter Arctic Amplification and sea ice loss. Scientific Reports, 9, 1184, doi: 10.1038/s41598-018-38109-x

Thanks for pointing out these errors. We have corrected them in the paper.

Review 2

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

I am only partially satisfied with the replies of the authors, which in many cases state that something is appropriate without providing supporting evidence and often seem to seek to minimise the changes applied to the manuscript rather that actually replying to the reviewer comments. As a general suggestion I would also encourage the authors in the future to specify where the edits have been implemented in the text, as this would make the work of the reviewers much smoother.

1. In reference to my previous major comment no. 1, I think that the point the authors make on the temperature anomalies vs sensible heat flux illustrates why calling the moist intrusions ”warm” may introduce confusion. If the authors insist on calling them warm and moist, the explanation they present in their reply could fruitfully be integrated in Sect. 2.2.

Thanks for the comments. We have summarized the response to comment no. 1 to the manuscripts at lines 93-96, ‘A particular warm air intrusion may carry less moisture than a typical moist intrusion, but a typical moist intrusion will certainly carry warm air into the Arctic. We therefore name these events as ‘warm and moist air intrusions’, identify and quantify them with the vertically integrated northward moisture flux, f_w, …’

2. In reference to my previous major comment no. 2, and following the authors’ explanation, I believe that what is shown in Fig. 2a is not a correlation. As far as I know, correlation by definition is in the range [-1, 1].

Yes, you are definitely correct. From the definition of linear regression coefficient:

$$Y = a + bx$$

$$b = \frac{\sum_{i=1}^{n}(x_i - \bar{x})(Y_i - \bar{Y})}{\sum_{i-1}^{n}(x_i - \bar{x})^2}$$

In the case of figure 2a. x is sea ice concentration, Y is poleward moisture transport, f_w..If we calculate the linear regression directly with the above equation. The linear regression coefficient would be definitely in the range of [-1,1]. However, in this research we have standardized the sea ice concentration. Therefore, according to above equation, the unit of b would be same as the unit of f_w. The altitude will be not limited in the [-1,1]. Instead it indicates the general aviation of f_w from the climate mean during the sea ice decrease. Similar explanation also applies to figure 8, where x in above equation represents daily mean f_w. To make it more easily understandable, we have summarized above explanation to the captions of figure 2 (lines 546-548) and figure 8.

For figure 2, 'Note that the linear regression shown in figure 2a is calculated against standardized sea ice concentration. Therefore, its unit is same as that of f_w and its value represents the general variation of f_w from the climate mean during the sea ice retreat.'

For figure 8, 'Similar as figure 2a, the linear regressions here are calculated against standardized f_w. Therefore, the unit of the regression is same as the corresponding variables and the values represent the general anomalies from the climate mean during positive f_w'.

3. In reference to my previous major comment no. 3, point 2, I am not fully convinced by this argument because what happens along the trajectories will partly depend on where they "go". In other words, an error in the tracking will influence also what happens along the trajectory. This should at the very least be mentioned as a caveat in the methods section. More generally, when it comes to resolution and interpolation, it would be good if the authors good support their statements/choices of data with reference to past studies looking at the impact of resolution on tracking. I suggest a (very old) one below, but there are certainly other more recent studies on the topic.

Thanks for your comments. We have added following discussion into the manuscripts at lines 120-127 based on the paper you suggested. Unfortunately we did not find recent studies on this topic.

It is to be noted that both the temporal and spatial resolutions would influence the accuracy of the trajectory calculation (Stohl et al., 1995). However, the increase of only temporal or spatial resolution does not necessarily improve the accuracy of trajectory calculation (Draxler, 1987). In this study, we calculate the trajectories 2 days forward and backward, instead of calculating 4 days trajectory at once. Furthermore, we also calculate the ensembles of trajectories at different heights to decrease the error introduced by vertical interpolation from pressure level to geometric height.

4. In reference to major comment no. 3, point 3, another reference that may be useful in this discussion is Wang et al., (2019), primarily since it compares ERA5 to its predecessor ERA-Interim, although the comparison is limited to a small amount of variables.

Thanks for the suggestion. We have integrated this paper to line 81.

5. In reference to major comment no. 3, point 6, it would be good if the authors could provide some qualitative support to their answer, in addition to simply stating that they believe their method to be appropriate. E.g. how many trajectories do actually cross the pole

and are truncated/excluded? I am not suggesting the analysis is repeated after changing the trajectory definition, but rather that a check is performed on how the subjective choices of trajectory requirements affect the sample being analysed.

Thanks for the suggestions. To take Barents Sea sector as an example, we calculated how many trajectories are excluded by each requirements in the criteria: 1) the trajectory tracks continuously northwards and reach 85°N . 2) the terminal point of the trajectory is at least 5 degrees north of the sea ice edge. Totally 551 trajectories meet these two requirements.

As the reviewer has mentioned, it can be strict to set the requirement that the trajectory tracks continuously northwards till 85°N. Here according to our calculation, there are 45 trajectories tracking southward before they reach 85°N but finally still can reach 85°N. We think 45 is much fewer in comparison with 551 trajectories which meets the requirements.

We have also checked how strict the second requirement is, by calculating the number of trajectories which are all the way north and reach 85°N but the terminal point is less than 5 ° north of the sea ice edge. The results show that only 28 trajectories are in this case, which is far fewer from 551 trajectories which has pass the requirements.

Actually the most strict requirements is that the terminal point of the trajectories should reach 85 °N. 791 trajectories cannot meet this requirements but this requirement is necessary since we want to look at how the air column evolve on its way to the central Arctic over the sea ice.

6. In reference to major comment no. 3, point 7 l. 106. Again, ”believing” is not a good metric for scientific research. At the very least the authors should mention this in the study as a caveat of their methodology.

This is similar to comment 3. The interpolation would introduce errors of the trajectories, as we mentioned previously, the ensembles of the trajectories at different height could reduce the error. We have clarified this in the manuscript as we claimed for the comment 3.

7. In reference to major comment no. 3, point 8: reproducibility is essential, even in the presence of a semi-objective classification approach. Since the authors work with roughly 200 events, they could add a small data file as Supplementary Data with the dates and starting locations of the WaMAIs and their classification, or similar information, to ensure reproducibility.

We have added the launch time, location of each trajectories for each of the categories in the supplementary materials Table S1.

8. L. 287-289 The added text is quite unclear, please rephrase. For example, it is unclear what ”these” refer to, as the new sentence is the first sentence in its paragraph. Also, you could remind what category the ”typical for the Barents Sea” and “for the other sectors” are before making the point about the Kara Sea. Right now you first make this statement and only later explain what the ”typical categories” are.

9. In reference to minor point no. 5, did the authors add this explanation to the text?

We have added it to line 187-189.

10. In reference to minor point no. 7, this would be of interest to the reader (especially the reader trying to reproduce some of the results) and should be mentioned in the text.

WE have added into lines 266-272.

The boundary-layer energy-budget pattern plotted this way is very variable from case to case, mainly because the northward component of the advection is differently from case to case; also, the location of the ice edge is different from year to year. Hence, some trajectories are long but reach less far north while other are shorter and still reaches further north. In the vertical, the cases are also subject to different subsidence, affecting the PBL growth. We therefore have yet to come up with a workable normalization that would allow an ensemble average of all the cases.

11. I believe that the numbering of the SI figures is incorrect. Please also update references in the main once the numbering has been corrected, and ensure that all SI figures are referenced in the main text.

Thanks. I have corrected the numbering. Figure S1, S3, S5, S7 are referenced at line 163 and figure S2, S4, S6, S8 are referenced at line 174. Table S1 is referenced at line 286.

12. Caption to Fig. 2: ”dash line” ◊ ”dashed line”

Corrected. Thanks.

Stohl, A., Wotawa, G., Seibert, P., and Kromp-Kolb, H.: Interpolation errors in wind fields as a function of spatial and temporal resolution and their impact on different types of kinematic trajectories, J. Appl. Meteor., 34, 2149–2165, 1995.

Wang, C., Graham, R. M., Wang, K., Gerland, S., and Granskog, M. A.: Comparison of ERA5 and ERA-Interim near-surface air temperature, snowfall and precipitation over Arctic sea ice: effects on sea ice thermodynamics and evolution, The Cryosphere, 13, 1661–1679, https://doi.org/10.5194/tc-13-1661-2019, 2019.