

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-610

Anonymous Referee #2

Referee comment on "Warm and moist air intrusions into the winter Arctic: a Lagrangian view on the near-surface energy budgets" by Cheng You et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-610-RC2>, 2021

This study addresses how wintertime warm and moist air intrusions into the Arctic affect the surface/boundary-layer energy budget. The analysis is very thorough and highlights intriguing differences between fully ice-covered sectors and sectors which instead have open waters to the south. The analysis and conclusions provide an interesting addition to the literature and I believe that they should eventually be published. Nonetheless, I would recommend a number of revisions before the paper is accepted. I have some concerns on the methodology and on how this may affect the results, and I find the paper to be overall poorly written and with a large number of careless errors which suggest that no proof-reading of the text has been carried out prior to submission.

Major Comments

1. The authors call their intrusions "warm and moist", but they only use the "moist" part to define them (as far as I can tell, T only comes in for choosing where to initialise the trajectories, but there is no threshold imposed on it). One may argue that the two go hand in hand, but I would still favour calling these simply "moist intrusions". Other authors (e.g. Papritz, 2020), have shown that poleward transport of already warm air accounts for only a small part of the Arctic wintertime extremely warm airmasses. This perspective could be fruitfully integrated into the introduction, and Papritz et al. (2021) may also be relevant in this context. To avoid any misunderstandings, I would like to clarify that I am not Lukas Papritz.

2. The paper is poorly written and with a number of careless errors/typos, which suggest that no proof-reading has been performed prior to submission. The text needs a thorough review before it may be published. Below are a few examples, but this is by no means a comprehensive list:

Several "the" missing from the abstract.

l. 35 winter --> winters

l. 59 budget --> budgets

l. 88 "a WaMAIs" --> "a WaMAI"

l. 88 ". we" --> ". We"

l. 93 "blue lines" --> "blue line" (I only see a single blue line in the panel)

l. 120 "in" --> "on"

l. 130 Should this refer to Fig. 3 instead of Fig. 4? Or did the authors indeed mean to refer to the Beaufort sea but misplaced the figure reference in the sentence?

l. 153 "contributes" --> "contribute"

l. 167 "by composite the heights"

l. 190 "at a rate 1.6 times larger rate"

l. 208 "resulted" --> "resulting"

I. 217 "warmer and moister ocean surface" Warmer and moister than what? Also, it is hardly appropriate to refer to the ocean surface as "moist", since it is made of water.

II. 252-253 "applicable for" --> "applicable to"

II. 319-321 Rephrase.

I. 331 "is warmed" --> "are warmed"

II. 331-332 "before advected" --> "before being advected"

Fig. 11-14 Double-check the cross-referencing to other figures in the captions.

Fig. 2a The contours are labelled with numbers > 1 , so I assume this is not correlation as indicated in the caption; or perhaps they are multiplied by -10 instead of -1, or there are decimal points which are made invisible by the stippling.

3. I have some concerns on both the methodology itself and how it is explained.

- Was there a reason to exclude the ~ 20 degrees wide ocean sector immediately to the east of Greenland?
- Sect. 2.1 Doesn't the use of coarse-grained ERA5 data (at 1/3 of the spatial resolution available from ECMWF) significantly reduce the accuracy of the air mass trajectory calculations?
- I. 73-80 I agree with the authors that we need to make use of the data we have, even though it has known limitations. However, the discussion on this point should be qualified with references to the relevant literature. There are several different reanalysis products available for the Arctic region, and several studies have provided intercomparisons of how they perform. To clarify, I am not suggesting the use of additional datasets, but rather a more robust argument grounded in the existing literature as to: (i) why ERA5 is a sensible choice and how it compares to other data options that the authors could potentially have used; and (ii) what sort of uncertainty we may expect from the use of reanalysis data (are we talking about an uncertainty of the same order of magnitude as the actual values?).
- II. 88-90 This is a key part of the methods but the details are hard to follow. Please

revise the phrasing, specify what the 95th percentile is calculated for (all sensitive regions, each region in turn or other? One should not need to second-guess it from Fig. 2), specify that WaMAIs are >>continuous<< periods when f_{bar_w} exceeds 0, explain that a single WaMAI always contains at least one EMI, but may contain several, show the 95th percentile as a horizontal line in Figs. 2c, d etc. Also, the highlighted WaMAI in Fig. 2c does not seem to match the definition, as the portion of the line immediately preceding the marked WaMAI is still above zero, yet coloured in black.

- l. 93 Do the authors mean that they select the single point along the latitude circle within each sector with the highest T850 on the day when a WaMAI is selected and initialise the trajectories only from there? This needs to be rephrased to clarify what the procedure actually is.
- l. 95 Isn't this a very restrictive criterion? Airmass trajectories penetrating the Arctic basin may have a strong zonal component and in some cases even have short sectors of their tracks with a southward component. Could the authors quantify how many events they exclude which have a predominantly northward component but track southwards for 1 or 2 timesteps during the 4 days considered?
- l. 97 ERA5 data is typically used on pressure levels (with some variables also being available on sigma levels), yet here the authors seem to imply that they initialise their trajectories at fixed geometric heights. Does this mean that interpolation is not only used to retrieve the vertical profiles of the trajectories but also to determine the starting points of the trajectories? If so, does this not significantly degrade the performance of the tracking algorithm?
- l. 106 Why use a 0.5 interpolation from data at 0.75 degrees when higher-resolution ERA5 data is available directly from ECMWF?
- Sect. 3.3 I am not sure that the description of these four WaMAI boundary-layer energy-budget classes allows for full reproducibility of the results. Could the authors add a short section in the Methods or an Appendix where they describe in detail how each class is defined, how border-line cases at the cross-over between different classes are treated etc.?

4. The authors mention on l. 84 that the Kara sea, like the Barents sea, also experiences some wintertime sea-ice variability. However, this aspect is never picked up again in the analysis, and the Kara Sea seems to be missing altogether from Table 2 (at least according to the table caption). Could the authors include a discussion of the Kara sea in their analysis and comment on whether it is a special case (intermediate between the Barents Sea and the other sectors) or follows one of the two patterns already discussed (open ocean vs. land-locked ice)?

5. Sect. 3.2 There are some very interesting results in this section, but I struggled to read it. Right now it reads more as a point-by-point description of the figures rather than as a description which highlights the key results from the figures. I would encourage the authors to try to distil the relevant information provided by the figures, and communicate it more effectively in the text. This also applies to a few other passages in Sect. 3.3, but is most evident here.

Minor Comments

1. I am always in favour of short titles, but in this case this has perhaps been taken a step too far. Adding some reference to the energy budget may provide a better idea of what this study is about.

2. I. 32 The study by Francis and Vavrus (2012) has been heavily criticised in the literature and the methodology it adopted is at best debatable. I would recommend that the authors refer to later studies where a more robust analysis framework was adopted. Also, the study in question focussed on Arctic-mid latitude interactions, so it seems inappropriate to cite it in reference to accelerating arctic warming.

3. I. 87 Do the authors mean "winter trends" here or "winter variability"?

4. II. 131-132 Is this really the case? For example, is there such a well-defined Z500 dipole for all basins? It may be worth showing the corresponding plots for the other basins in a Supplement or Appendix. Several Arctic ocean sectors have been analysed but the reader only gains information about two.

5. I. 155 Please describe in the text how this statistical significance is computed.

6. I. 160-161 I may have misunderstood what the authors are doing here. If they use the full \bar{f}_w timeseries for the regression, would this not imply "a similar relationship for all days" rather than "for all WaMAIs"?

7. l. 227-228 Would composites over all events of a given category in a given sector show some features similar to those shown for these individual events, or are the boundary-layer characteristics so variable as to average out in the composite? This is a point that may be of interest to readers. I would be grateful if the authors could show a composite figure in their reply (even if they decide not to include it in the manuscript) to get a feel for what the variability of these events actually is.

8. Fig. 2b, d The blue dot is very hard to view. Perhaps use a vertical line to mark the 95th percentile?

9. Fig. 6 To ease interpretation, you may want to specify that the range of the colourbars is different from Fig. 5.

10. Fig. 9 "Note that this is not necessarily the distance travelled, since WaMAIs need to travel due northward." I thought the reason for this not being the distance travelled is that WaMAIs need to track northwards but can have also a zonal component to their path?

11. Figs. 9, 10 and 16 Mixing red and green is not a good idea as colour-impaired readers will not be able to distinguish the different curves.

12. Fig. 10 Specify in the caption that panels (a), (b) refer to the Barents Sea.

References

Papritz, L. (2020). Arctic Lower-Tropospheric Warm and Cold Extremes: Horizontal and Vertical Transport, Diabatic Processes, and Linkage to Synoptic Circulation Features, *Journal of Climate*, 33(3), 993-1016.

Papritz, L., Hauswirth, D., and Hartmuth, K.: Moisture origin, transport pathways, and driving processes of intense wintertime moisture transport into the Arctic, *Weather Clim. Dynam. Discuss.*, in review, 2021.