Response to the reviewer 1

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.

This is a well and clearly written submission which takes both Eulerian and Lagrangian perspectives to examining warm and moist air intrusions into the Arctic basin. It explores and compares the temperature impact caused by anomalies in surface fluxes and in thermal irradiances. The nature of this comparison is found to depend on the particular Sea within the Arctic under consideration. Special comparisons are conducted for the Barents and Beaufort Seas.

The submission has the potential to make a very significant contribution to the literature, but it is not quite there yet. Before I would be able to recommend acceptance, there are a number of issues which need to be addressed (including updates to the literature).

We thank the reviewer for the encouraging comments. We hope that the revised version has improved the clarity of the message.

Lines 31-32: Also add to the References the study of Screen, Bracegirdle et al., 2018: Polar climate change as manifest in atmospheric circulation. Curr. Clim. Change Reports, 4, 383-395, doi: 10.1007/s40641-018-0111-4.

Thanks. Added.

Line 34: Make reference here to recent analysis of Li et al. 2021: Trends and variability in polar sea ice, global atmospheric circulations and baroclinicity. Ann. NY Acad. Sci., doi: 10.1111/nyas.14673 who show the continuing dramatic decreases in the Barents in all seasons.

Thanks. Added.

Lines 35-36: Add to these references the works of Luo, Overland, et al., 2019: Weakened potential vorticity barrier linked to recent winter Arctic sea ice loss and midlatitude cold extremes. J. Climate, 32, 4235-4261, Rudeva and coauthors (2021) Midlatitude winter extreme temperature events and connections with anomalies in the Arctic and tropics. J. Climate, 34, 3733-3749, and Li, M. et al., 2021b: Anchoring of atmospheric teleconnection patterns by Arctic Sea ice loss and its link to winter cold anomalies in East Asia. Int. J. Climatol., 41, 547–558, doi: 10.1002/joc.6637.

Thanks. Added.

Line 44: Beneficial in this context to also cite the papers of

Tiina Nygård, Tuomas Naakka and Timo Vihma, 2020: Horizontal moisture transport dominates the regional moistening patterns in the Arctic. Journal of Climate, 33, 6793-6807, doi: 10.1175/JCLI-D-19-0891.1, and Luo et al., 2017: Atmospheric circulation patterns which promote winter Arctic sea ice decline. Env. Res. Lett., 12, 054017, doi: 10.1088/1748-9326/aa69d0.

Thanks. We have added the citation.

Line 49: These 2018 and 2020 papers (co)authored by Felix Pithan do not appear in the References. From the context I suspect the authors are here referring to …

Pithan, F., et al. (2018), Role of air-mass transformations in exchange between the Arctic and mid-latitudes, Nature Geosci., 11, 805-812, doi: 10.1038/s41561-018-0234-1, and Mubashshir Ali, and Felix Pithan, 2020: Following moist intrusions into the Arctic using SHEBA observations in a Lagrangian perspective. Quarterly Journal of the Royal Meteorological Society, 146, 3522-3533, doi: 10.1002/qj.3859.

Thanks. We have added these two citations into the References.

Lines 73-76: It would be valuable to present a few references here in connection with the overall quality of reanalyses, and particularly for ERA5in this data-sparse region of the world. Some examples are …

Michael Mayer, Steffen Tietsche, Leopold Haimberger, Takamasa Tsubouchi, Johannes Mayer and Hao Zuo, 2019: An improved estimate of the coupled Arctic energy budget. J. of Climate, 32, 7915-7934, doi: 10.1175/JCLI-D-19-0233.1,

Graham, R. M., L. Cohen, et al., 2019: Evaluation of six atmospheric reanalyses over Arctic sea ice from winter to early summer. Journal of Climate, **32**, 4121-4143, doi: 10.1175/JCLI-D-18-0643.1.

However, as the authors argue, reanalyses are the best tools that we have for this sort of investigation.

Thanks. The results from these two papers have been integrated into this paragraph.

Line 96: The ‘sea-ice edge’ should be defined. I presume this refers to the usual definition of where SIC exceeds 15%, but this should be made explicit.

Yes, the reviewer is definitely right. We clarified this on line 107 in the new manuscripts.

Line 123-132: I strongly suggest indicating regions over which the composite anomalies in Figures 3 and 4 differ significantly ($p = 0.05$) from zero. The Z500 anomalies show a very strong and simple structure, and this would be worth a comment. However, it is still important to demonstrate statistical significance.

Thanks for this suggestion. We have conducted the significant test and plotted the region where the difference passes the test in Figure 3-5 and Figure S1-S6.

Line 174: To avoid any possible confusion (with temperature) I suggest replacing ‘degree**-1’ with ‘(degree latitude)**-1’ here and throughout the text. Also to make a similar change to the label of the x-axes in Figures 9 and 10 (to make it clear that this distance is measure in the meridional direction).

Thanks. We have changed degree**-1 to (degree latitude)**-1 in the text. However, we did not change the unit for x-axis in figure 9 and 10 where the label ("Distance from…") makes it quite clear what we mean, and we clarify it in the caption.

Line 248: Change '(2019b)' to '(2019)' – there is only one 2019 paper of relevance here.

Thanks. Corrected.

Line 251: Neither of these two papers of the second author are presented in the References. Please correct this. I would guess the relevant papers are …

Tjernström, M., and R. G. Graversen, 2009: The vertical structure of the lower Arctic troposphere analysed from observations and the ERA-40 reanalysis. Quart. J. Roy. Meteor. Soc., 135, 431-443, doi: 10.1002/qj.380.

Tjernström M., Birch C. E., Brooks I. M., Shupe M. D., Persson P. O. G., Sedlar J., Mauritsen T., Leck C., Paatero J., Szczodrak M. and Wheeler C. R. (2012) Meteorological conditions in the central Arctic summer during the Arctic Summer Cloud Ocean Study (ASCOS). Atmos. Chem. Phys. 12, 6863-6889, doi: 10.5194/acp-12-6863-2012.

Thanks. Corrected.

Line 252: Another citation which does not appear in the References!

? Ian M. Brooks et al., 2017: The turbulent structure of the Arctic summer boundary layer during the Arctic Summer Cloud-Ocean Study. Journal of Geophysical Research, 122, 9685-9704, doi: 10.1002/2017JD027234 ?

Thanks. We have added this citation.

Lines 315-346: The Conclusions of this fairly complex study are presented clearly and offer valuable insights. Particularly interesting are the findings of the relative importance of downward long-wave anomalies and surface fluxes as contributors to Arctic warming, and the dependence on which ocean (and its characteristics) are being considered. This has direct relevance to the analyses and discussion in the papers of Screen et al. (2010), The central role of diminishing sea ice in recent Arctic temperature amplification, Nature, 464, 1334-1337, and Lee, Feldstein, and coauthors, 2017. 'Revisiting the cause of the 1989-2009 Arctic surface warming using the surface energy budget: Downward infrared radiation dominates the surface fluxes', Geophys. Res. Lett. 44, 10,654–10,661. In this summary part of the manuscript, it would be very valuable to refer to these papers, and comment on how the present submission adds new light on the issue.

Thanks for this input. We have integrated these citations into the discussion.

Lines 406-407: This journal article does not appear to be referenced in the paper – please adjust.

Thanks. We have deleted it.

Line 448: 'sic' should be 'six'

Thanks. Corrected.

Line 455: months

Thanks. Corrected.

The captions for Figures 11 thru 14 seem to be messed up:

Line 540: ‘figure 13’ should be ‘Figure 11’

Line 545: ‘figure 10’ should be ‘Figure 11’

Line 549: ‘figure 10’ should be ‘Figure 11’

Thanks. Corrected.

Response to the reviewer 2

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.

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.

Thanks for this feedback. The reviewer is correct; we define WaMAIs using poleward transport of moisture, and had the reviewer been Lukas Papritz this comment would still be relevant.

The choice of method is based on the hypothesis that a major factor for the surface and boundary-layer energy budgets comes from cloud formation. Since the Arctic boundary-layer relative humidity is typically high, high moisture transport is essentially impossible without transport of sensible heat; hence the choice of name: “warm-and-moist”. A warm air mass may be less than moist, but a moist air mass must also be warm and we only consider the strongest moisture fluxes. This argument is now more clearly discussed in the revised text.

Thanks for the suggestion of these two papers. We have integrated these two papers into the introduction. However, we need to comment that we do not intreprete the Papritz (2020) results the same way as this reviewer. First of all, the Papritz (2020) study is based on temperature anomalies and not fluxes of either moisture or sensible heat. They find that positive temperature anomalies are often due to transport of already warm air from somewhere else; roughly half the cases in winter and almost all in summer, when subsidence is a main contributor. Our paper is not about temperature anomalies. Instead it deals with what happens to moist (and warm) air as it enters in over the Arctic sea ice. Therefore, although interesting in many ways, we do not find the Papritz results contradictory to our view.

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"

l. 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.

ll. 252-253 “applicable for” --> “applicable to”

ll. 319-321 Rephrase.

l. 331 “is warmed” --> “are warmed”

ll. 331-332 “before advected” --> “before being advected”

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

Thanks for the help; it should be clear from the names that neither the lead author nor the coauthors are native English speakers. We have revised the manuscript according to suggestions from this and one other reviewer and we hope that it is much improved.

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.

Thanks for the question. The linear regression is between northward moisture transport and the normalized sea ice concentration (with a change in sign to avoid cluttering the plot with minus signs); not the absolute sea ice concentration. The normalized sea ice concentration is defined as the deviation from the mean divided by the standard deviation. Therefore, the linear regression represents the magnitude of poleward moisture transport related to the decrease of sea ice concentration and could be larger than unity. This is now explained in the revised text and in the figure caption.

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

1. Was there a reason to exclude the ~20 degrees wide ocean sector immediately to the east of Greenland?

   Thanks for this question. In this paper, we are more interested in how the warm air mass evolve over the sea ice meridionally during WaMAIs. However, to the east of Greenland, sea ice exists only at the places surrounding Greenland and the warm air-masses can hardly penetrate continuously over the sea ice there. Therefore, we decide to focus on other sea sectors excluding this region.

2. 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 airmass trajectory calculations?

The resolution of input data can definitely affect the trajectory calculations. But we use rather short trajectories; only two days long which limits this error. However, the most important argument is that we interpolate the energy budget terms along the trajectories wherever they may go. We are hence not primarily interested in the exact path of the trajectories, but in what happens along all of them. On average, which is the third argument; by looking over many trajectories and taking the average this error is minimized, assuming it is Gaussian.

3. l. 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?).

   We do believe that ERA5 provides the best reanalysis product available and base this on evaluations suggesting that ERA-Interim was the best, and that ERA5 in general represents an improvement. However, there are only very few evaluations of ERA5 available and they all rest on very limited data, especially for the central Arctic Ocean. We have modified the text on this and added the following three citations:

   Mayer, M., S. Tietsche, L. Haimberger, T. Tsubouchi, J. Mayer and H. Zuo, 2019: An improved estimate of the coupled Arctic energy budget. J. of Climate, 32, 7915-7934, doi: 10.1175/JCLI-D-19-0233.1,

   Graham, R. M., L. Cohen, et al., 2019: Evaluation of six atmospheric reanalyses over Arctic sea ice from winter to early summer. Journal of Climate, **32**, 4121-4143, doi: 10.1175/JCLI-D-18-0643.1.

   Lindsay, R., Wensnahan, M., Schweiger, A. and Zhang, J.: Evaluation of seven different atmospheric reanalysis products in the arctic, J. Clim., 27(7), 2588–2606, doi:10.1175/JCLI-D-13-00014.1, 2014.

4. ll. 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.

   Thanks for this input. The 95th percentile of $\overline{f_w}$ is calculated respectively over each ocean sectors. In our definition, each WaMAI only includes one EMI and the onset and terminal of each WaMAI determined by the nearest regionally minimum of $\overline{f_w}$. Therefore, the positive signal in Fig. 2c as mentioned by the reviewer is not considered WaMAI. These above information have been included in the section 2.2 WaMAI detection.

5. 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.

Yes, the reviewer is correct; note that this starting point is unique for each WaMAI. We have clarified in the article.

6. 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?

   Yes, it is somewhat restricting but it greatly simplifies the analysis of the progression of the physical processes along trajectories, which is the purpose of doing these in the first place. We believe still capture a large enough number of cases for stable statistics.

   Note that there can still be a considerable zonal component in the track of a trajectory as long as its north/south velocity does not change sign. The most severe restriction is probably that it prohibits following a trajectory across the pole; however, in many cases this would mean quite long trajectories which may also be problematic.

7. l. 97 ERA5 data is typically used on pressure (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?

   The reviewer is correct; for the starting point of each trajectory we interpolated physical heights to pressure and the impact on the accuracy is minimal. The reason for using physical height is that physical processes, especially turbulence, are a function of height above the surface and not the pressure. Moreover, for each subsequent time step along the trajectory, the winds will have to be interpolated in 3D space; interpolating also the starting point makes very little difference.

   l. 106 Why use a 0.5 interpolation from data at 0.75 degrees when higher-resolution ERA5 data is available directly from ECMWF?

   We believe this is sufficient for the purpose of this study.

8. 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 Metods 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.?

   These four categories have been described in line 255 ~ 262 and more details is also available in the sections for each category (section 3.3.1, 3.3.2, 3.3.3, 3.3.4). Obviously, we are using the most clear-cut examples to illustrate the differences between the categories, and there are also less obvious cases. This manual classification is unavoidably subjective.

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)?

The main differences here are if there is sea ice all the way to the coast or if there is any open ocean. Barents Sea is an example of the latter, and largely all the other sectors are of the first category. However, for the Kara Sea some WaMAIs are similar to the Barents Sea and others similar to the remaining sectors. The purpose here is not to give a detailed description of each sector separately. A few words on this was added in the revised manuscript.

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 highligths 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.

We have read through these two sections and revised them. Hopefully, it gets improved this time.

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.

We appreciate this feedback from the reviewer and the title has been modified to

Warm and Moist Air Intrusions into Winter Arctic: A Lagrangian view on the near-surface energy budgets

2. l. 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.

Thanks for this feedback. We have deleted this citation.

3. l. 87 Do the authors mean “winter trends” here or “winter variability”?

It should be ”variability”; thanks, we have corrected it.

4. ll. 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.

Yes, there are very distinct dipole patterns for all ocean sectores, however, slightly differently centered on the sector. The plots for other ocean sectors have been added into the supplement.

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

In table 1, if the mean value of these energy-budget terms are positive and greater than their standard deviation, then we consider they are statistically significantly positive.

6. l. 160-161 I may have misunderstood what the authors are doing here. If they use the full f_bar_w timeseries for the regression, would this not imply “a similar relationship for all days” rather than “for all WaMAIs”?

Yes, you understand is correct and we have replaced ‘all WaMAIs’ with ‘all days’.

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.

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.

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

We have changed the dot to blue dash line.

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

We have added this to the caption of figure 6.

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

This was incorrect and has been changed in the revised text. Trajectories do *not* have to travel due north; they do have to travel with a northward component larger than zero.

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.

We have changed red to magenta and green to cyan.

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

We have specified it.

References

Papritz, L. (2020). Arctic Lower-Tropospheric Warm a nd 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.