Response to the comments from Anonymous Referee 2, Rune Grand Graversen, for the submitted ACP paper: ˝Bresson, H. et al. 2021: Case study of a moisture intrusion over the Arctic with the ICON model: resolution dependence of its representation. ˝

We thank the ACP handling editor, Farahnaz Khosrawi, as well as the Referee #2, Rune Grand Graversen, for this review. Please find below our response to the comments (in italics) from the Anonymous Referee #2.

Comments from Anonymous Referee #2:

Peer review of "Case study of a moisture intrusion over the Arctic with the ICON model: resolution dependence of its representation" by H. Bresson et al. The study concerns a spring moisture-intrusion event over the Kara and Barents seas extending into the Norwegian and Greenland seas, which occurred 5-7 June 2017. Reanalysis, analysis, and model: ERA5, ICON analysis, and ICON-LAM of different resolution and physics are compared to observations: satellite-based radiances and radiosonde profiles. The study is comprehensive in its comparison between model/analysis and observations, especially concerning the pseudo radiance images based on model output. It appears also to be perhaps the first work to present high-resolution model results of a moisture-intrusion event. The study finds higher skills in representing the moisture-intrusion event with the high-resolution regional model as compared to the lower resolution analyses. I am however a little skeptical to the presented evidence for this conclusion (see below). The work is an important contribution. The manuscript should be accepted for publication after the authors have taken some mostly minor points into account.

Response: We thank the Reviewer #2, Rune Grand Graversen, for his comments and suggestions, which will greatly improve this manuscript.

Major points:

1. The manuscript would benefit from being a little more nuanced in the conclusions regarding skills of models versus analyses. Lines 11-12 in the abstract, and lines 431-432 in conclusions seem to conclude that ICON-LAM represents the event with higher accuracy than do the global ICON model and the ERA5 reanalysis. However Fig. 3 shows that brightness temperature is better represented by ERA5 (Fig. 3p-r)) than by ICON-LAM (Fig. 3M-l). Also it is not clear from comparison with Ny-Ålesund radiosonde observations (Table 1 RMSE and MAE) that ICON-LAN performs better than in particular ICON global, but also ERA5.

Response: We agree that our wording in the abstract and conclusions would benefit from some rephrasing. In the main text we give all the details of where the ICON-LAM model shows a higher/lower skill than the reanalysis and global model, whereas in the abstract and conclusions we aim to highlight that example where ICON-LAM showed an advanced skill.
The according revised paragraph in the abstract reads now:

"One feature where the high resolution simulations demonstrated an advanced skill is the representation of the changing vertical structure of specific humidity and wind associated with the moisture intrusion passing Ny-Ålesund (western Svalbard)."
The according revised paragraph in the conclusions reads now:
"The results from this study showed that the ICON-LAM model is able to represent the spatio-temporal structure of the selected AR, and for specific features with a higher accuracy than the driving global ICON model and the ERA5 reanalysis. This was demonstrated in the more accurate representation of the AR's impact on the temperature, wind and humidity profile changes at Ny-Ålesund. However, an advanced skill for all aspects of the AR cannot be concluded."

Table 2 is for Shojna radiosonde observations, but here a comparison between model and observations seems to be missing.

Response: Actually for Ny-Ålesund, in Table 1, we computed the bias statistics of IWV using 3-hourly observations from HATPRO. Unfortunately, for the corresponding table for Shojna, namely Table 2, the only available observations for Shojna are from the coarse resolution (12-hourly) radiosondes. Hence, with only 6 profiles in total, we did not calculate the bias statistics for Shojna, as we thought it would not be meaningful.

Figure 8 shows better agreement for IWV between radiosonde observations and ERA5 than between observations and the high-resolution models. It is argued that the good agreement for ERA5 could be due to assimilation of the observations (Lines 321-322). This could be checked by comparing with ERA5 forecast fields (instead of analysis), similar to what was done in Tjernström and Graversen, Q. J. R. Meteo. Soc., 2009.

Response: As suggested, we checked the ERA5 forecast and compared it to the ERA5 analysis and the observations. Below we provide the associated figure for you. As seen, the ERA5 analysis (black diamonds) shows a better agreement with the radiosondings compared to ERA5 forecast (grey diamonds). Indeed, the IWV temporal representation of ERA5 forecast is closer to the ones of the ICON simulations than to the observations. This supports our statement and means that the assimilation of the observations does play a role. An interesting future work would be to compare the results of the current ICON simulations with new simulations which have assimilated the observations at those locations. Although interesting, this point will not be mentioned in the manuscript for clarity and brevity of the manuscript.

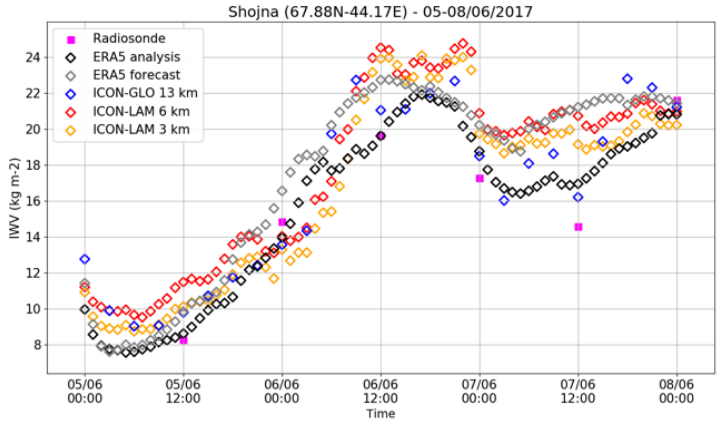

Figure: Time series of integrated water vapour (IWV, kg m^{-2}) at Shojna from radiosonde observations, ERA5 analysis and forecast, and ICON simulations from 5th June 2017 00:00 UTC to 8th June 2017 00:00 UTC. The model and ERA5 results are based on the station nearest grid point.

2. Clarity of display items should be considerably improved: Fig. 1: MSLP and IWV anomalies are difficult to see. The dark contours almost disappear into the dark shading background of IWV. Similar issues are found in many of the other display items. Also for example titles of plotting frames are providing unnecessary information, such as in Fig. 6.

Response: As suggested, the plots from Figure 1, Figure 2a, Figures 4a and 4b, Figure 7a, Figure 10a, Figure A1, Figure C1 and Figure D1 have been changed so that colormaps and isolines are more visible.

Also for example titles of plotting frames are providing unnecessary information, such as in Fig. 6.

Response: As suggested, the unnecessary titles from Figure 6 and Figure 9 have been removed.

Minor points:

1. ICON-LAM seems to have relatively low vertical resolution, 70 model levels with first at 20 m, whereas e.g. ERA5 has 137 levels with the first level at 10 m. This difference in vertical resolution should be more clearly stated, and, preferably, implications of this difference on the results should be discussed.

Response: We extended in line 128 "… below 5 km, and the lowest level at 10 m),…"
We included a new sentence in line 433 (after "… at Ny-Ålesund."): "The models and reanalysis differ in both the horizontal and vertical resolution, and assessment of this overall implication is complex. Although ERA5 has the highest vertical resolution, it does not show an advanced skill in the AR signature in the vertical profiles of temperature, humidity and wind at Ny-Ålesund, where the simulations with the ICON high horizontal resolutions show a better skill. This indicates that a certain horizontal model resolution is of particular importance for an effective comparison with station observations near coast and/or complex topography."

2. Please indicate more clearly the role of ICON-global in the regional model runs. How "free" are the regional models? Is the reinitialization occurring only at the boundary or is the regional models also nudged within the domains? What is the contact (if any) between ICON-global and the regional models in the 30 forecast hours after the initialization?

Response: The ICON-Global data is used for the initial conditions and lateral boundary conditions. For the initialization of the first run (i.e. 04/06/17 18:00 UTC), the global data are used for the entire domain. The ICON-LAM simulation is then performed for 30 hours (until 05/06/17 23:59 UTC) and 3-hourly boundary conditions from the ICON-Global data are input

to prevent departure in the LAM simulation. The two reinitializations (i.e. 05/06/17 18:00 UTC and 06/06/17 18:00 UTC) also make use of the initial conditions for the entire domain, and the lateral boundary conditions. The simulations are only one-way nested, hence the ICON-LAM simulations receive information from the ICON-Global data but no information from the LAM simulations are given back to the ICON-Global data. Concerning the nudging, a nudging is applied but only at the lateral boundaries (there is no top down nudging). A sentence has been added in the manuscript: "Furthermore, no additional forcing is applied (i.e. the model evolves 'freely' in the inner part of the domain up to the model top)."

3. Fig. 1: Why is some areas, such as Greenland, having no shading? Is there no IWV over these areas in ERA5?

Response: Indeed, there was a problem with the IWV calculation over the regions of high topography. This was corrected in the new plots.

4. Line 260: Text says that specific humidity observations in the lower atmosphere is typically smaller than 1 g kg-1. However Fig. 6a shows values of rather 3-4 g kg-1.

Response: With "the lower atmosphere", we meant the atmosphere above 500 m of altitude (but below 5 km), where the specific humidity is indeed lower than 1 g kg^{-1}. On the contrary, below the dry layer inversion (in the first 500 m of altitude), the atmosphere has higher specific humidity values. The word "lower" has been removed to avoid confusion.

5. Figure 6 and Lines 272-274: I wonder of the spread across the four closest grid points is a fair comparison of accuracy between the models. The models have obviously different resolution, so variability is compared in different spatial scales across the models. It seems as a fair comparison would regard a fixed spatial scale. Hence for instance ERA5 data could be interpolated into the same grid as the high-resolution model before the comparison.

Response: Indeed, the across-grid-point spread is related to the horizontal resolution of the dataset chosen. We commented this in the lines 271-274. The aim of Figure 6 is to compare the nearest grid point with the observations, but to give in addition a measure of uncertainty, by indicating the across-point-scatter. We do not further interpret this (besides lines 271-274). Our final model-observation comparison statements are robust and are not affected by this scatter.

6. Figure 7: The humidity anomaly seems to occur first at around 1000 m before it occurs at the levels just above and below. However early occurrence of the humidity anomaly is also found at 4000 m. Why is that so? Please mention and briefly discuss this issue.

Response: Indeed, the humidity anomaly seen in Figure 7a occurs first at around 1000 m of altitude, as well as at around 4000 m. This is also seen in the vertical profiles of humidity in Figure 6b for both the observation and the models. As clarified in the text related to Figure 6, the first maximum (at around 1000 m) is driven by the passage of the AR while the second

maximum (at around 4000 m) seems co-located with the top of the dry layer, present above the AR (between 2000 m and 4000 m). These also correspond to the maximums of wind speed seen in Figure 6d. As explained in the text, these dry layers are frequently observed above ARs.

7. Lines 316-317: What is meant by "IWV evolution at Shojna before and after the AR passage is higher"? Please clarify.

Response: By IWV evolution we mean the temporal evolution of the IWV value. We clarified this and give the dates now. It reads now"… before (i.e. before 05/06/17 12:00) and after (i.e. after 07/06/17 00:00) the AR passage".

8. Lines 339: ".. moisture intrusions, which travel over the sea ice into the Arctic". Perhaps the point is that the moisture anomaly is advected over the stable Arctic boundary layer.

Response: We revised the according sentence. It reads now "… suggesting an "upward and poleward upgliding of the humid air parcels" of moisture intrusions over sea ice into the Arctic, because the cold air dome (built by the cold boundary layer capped by strong temperature inversion) blocks the intrusion (Komatsu et al., 2018)."

9. Figure 11: Indicate whether fluxes are positive downward or upward.

Response: This has been added in the text and in the caption of Figure 11.

10. Some other works may be relevant: Woods et al., J. Climate, 2017; Rydsaa et al., Q. J. R. Meteo. Soc., 2021; Nygård et al., J Clim, 2019; Naaka et al., Int. J. Clim, 2019.

Response: Thanks for pointing us to other relevant literature. We took these suggestions into account in the manuscript.
We add to line 38 "… in the polar regions (Woods et al., 2017; Martin et al., 2018; …"
We added to line 23 "… poleward transport of heat and moisture (Naaka et al., 2019; Rydsaa et al., 2021), which contributes…."
We added to line 26 "…Nygard et al., 2019)."

Rydsaa, JH, Graversen, RG, Heiskanen, TIH, Stoll, PJ. Changes in atmospheric latent energy transport into the Arctic: Planetary versus synoptic scales. Q J R Meteorol Soc. 2021; 147: 2281– 2292. https://doi.org/10.1002/qj.4022
Naakka, T, Nygård, T, Vihma, T, Sedlar, J, Graversen, R. Atmospheric moisture transport between mid-latitudes and the Arctic: Regional, seasonal and vertical distributions. *Int J Climatol*. 2019; 39: 2862– 2879. https://doi.org/10.1002/joc.5988

Woods, C., Caballero, R., & Svensson, G. (2017). Representation of Arctic Moist Intrusions in CMIP5 Models and Implications for Winter Climate Biases, Journal of Climate, 30(11), 4083-4102, 10.1175/JCLI-D-16-0710.1
Nygård, T., Graversen, R. G., Uotila, P., Naakka, T., & Vihma, T. (2019). Strong Dependence of Wintertime Arctic Moisture and Cloud Distributions on Atmospheric Large-Scale Circulation, Journal of Climate, 32(24), 8771-8790, doi:10.1175/JCLI-D-19-0242.1

Text suggestions:

L1: Add "of a similar size" to the end of the sentence.

Response: The suggestion has been added.

L12 "compared" -> "when evaluated against".

Response: The sentence has been amended.

L14: Remove "Namely" and set a colon at the end of the previous sentence. Comma after "layer" and "is best" -> "are the best".

Response: The sentences have been corrected.

L16: Comma after Wm-2. The last sentence of the abstract seems not finish.

Response: To add clarity, the last sentence has been cut into two sentences.

L385: Apostrophe in "AR's".

Response: The apostrophe has been added.