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Comment on acp-2021-501

Rune Grand Graversen (Referee)

Referee comment on "Case study of a moisture intrusion over the Arctic with the ICOsahedral Non-hydrostatic (ICON) model: resolution dependence of its representation" by H el ene Bresson et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-501-RC2>, 2021

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.

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- alesund radiosonde observations (Table 1 RMSE and MAE) that ICON-LAN performs better than in particular ICON global, but also ERA5. Table 2 is for Shojna radiosonde observations, but here a comparison between model and observations seems to be missing.

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. Meteor. Soc., 2009.

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.

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.

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?

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

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.

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.

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.

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

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.

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

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

Text suggestions:

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

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

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

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

L385: Apostrophe in "AR's".

Good luck with it, nice work!

Rune Grand Graversen