Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-141-RC1, 2019

Geoscientific Model Development Discussions
Open Access

Interactive comment on "Verification of the regional atmospheric model CCLM v5.0 with conventional data and Lidar measurements in Antarctica" *by* Rolf Zentek and Günther Heinemann

Anonymous Referee #1

Received and published: 23 October 2019

This paper is a valid contribution to the scientific literature. It assesses the performance of the CCLM v5.0 for the Weddell Sea region. I have one general and a couple of specific concerns regarding the paper. I recommend the paper to be published only after these concerns have been adequately addressed.

Using Re-analyses data as reference in the validation is problematic. A recent paper by Gossart et al., (2019) for example shows strong warm biases in the interior of the continent in the different re-analyses products. It would be much better if only the observations were used for model validation. I recommend to remove the discussion

Printer-friendly version

Discussion paper

of the re-analyses and remove fig 3, 4 and 5 from the paper. If the authors feel strongly about keeping the re-analyses in their paper, it needs to be framed differently than is done now. From a comparison with observations – it can be investigated whether there is an added value in CCLM compared to the (driving) re-analyses. This can - for example – be done by extending figure 7, 8 and 9 and include the re-analyses here – if you think plots become too busy, you can differentiate winter and summer.

Related to that, I recommend to restructure the paper: 1) statistical analysis with station data, 2) comparison with Halley, 3) comparison with AWS3 buoys, 4) comparison with radiosondes 5) comparison with lidar

The methodology describing the sea ice is not completely clear: Is a fractional sea ice cover used in the model? This is particularly relevant when studying atmosphere-ice-ocean interactions – a goal that the authors have in mind. Can one grid box have sea ice classes of different thickness? Please clarify and also state the limitation associated with the assumptions made in the model.

The reduction of minimal diffusion coefficients for heat and momentum does indeed improve the performance in the interior, but deteriorates the performance on the ice shelves. Esp. in Fig 7 there is a strong increase in RMSE in winter over the east coast (and southern peninsula). This should be stated more clearly in the abstract and conclusions (in esp. the sentence 'Differences in other regions were small' is somewhat misleading). Do the authors have any idea how to improve the performance over the ice shelves? Is the albedo of the ice shelves correctly represented in the model and might deficiencies in albedo play a role?

Related to the previous point: Some information on the snow module should be included in the paper. Are albedo variations taken into account? How is the snow profile initialized and is this realistic? Even though this is a run in forecast mode, I assume that the surface is freely evolving. Is that right? Are snow temperatures drifting away from the forcing or is this not the case.

I am not sure if the forecast mode is the best when studying atmosphere ice ocean interactions – the sea ice cover in the driving re-analyses can be different than the observed cover and in that way processes related to atmosphere ice ocean interactions can be destroyed. A discussion on this topic in the conclusions / future work would be welcome. Moreover, it should be clearly indicated in abstract and conclusions that the model is used in forecast mode.

At the end of page 3 you describe you have a sea ice thickness of 0 m when the sea ice cover is 0-15%. I am not sure what this means – does it means that sea ice is simply ignored for these small fractional coverages? Although I did not dive into the reference, the value of 0.1 m for fractions between 15-70% seems very low to me. Can you somehow extent the argumentation on these values in the paper. Again this is quite relevant for the application that the authors have in mind.

Page 5 line 27 – you compare hourly averaged observations with grid box average instantaneous model output. You have to motivate this better – what is the typical advection speed and to which horizontal length scale does a time period of one hour correspond? Is it still possible to compare models and re-analyses with different resolutions if an evaluation is performed in this way. This point definitely needs more attention and a solid methodology needs to be presented and executed.

Figure 7, 8, and 9 are key figures to the paper, but difficult to interpret for the reader. Consider remaking them by plotting the box plots on a map, so that the reader directly knows to which station the comparison belong and is facilitated in the interpretation.

Consider switching Fig 11 with Fig 12.

Fig. 15 and 16: to facilitate the visual comparison, please remove the parts that are not measured with the lidar.

For the last part with the lidar comparison, also an evaluation of higher resolution integrations is added. Since sensitivity to resolution is small, I recommend to leave out this

Printer-friendly version

Discussion paper

comparison. It is sufficient to just make a note saying that decreasing the resolution to 5 or 1 km does not affect the wind patterns at the location of the lidar.

I suggest to merge the summary and conclusion and outlook section as there is some redundancy.

Reference:

Gossart, A., Helsen, S., Lenaerts, J.T M., Vanden Broucke, S., van Lipzig, N.P M., Souverijns, N. (2019). An Evaluation of Surface Climatology in State-of-the-Art Reanalyses over the Antarctic Ice Sheet. JOURNAL OF CLIMATE, 32 (20), 6899-6915. doi: 10.1175/JCLI-D-19-0030.1

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2019-141, 2019.