

Interactive comment on “On the forecast skills of a convection permitting ensemble” by Theresa Schellander-Gorgas et al.

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Review of “On the forecast skills of a convection permitting ensemble” by SCHELLANDER-GORGAS et al.

The authors like to thank the reviewer for the careful and thorough review, which is constructive and helps a lot to improve our paper. We have answered your comments, questions etc. one by one in below. We have either modified the manuscript and figures, or given a detailed explanation.

Best regards,

Theresa Schellander-Gorgas and co-authors, Sept. 12. 2016, Vienna

This paper compares the performance of two limited-area ensemble prediction sys-

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tems over central Europe. ALADIN-LAEF is a typical mesoscale regional ensemble at 11km resolution and the AROME-EPS a convective permitting model at 2.5km resolution. The focus of the study is on verifying surface weather variables, particularly precipitation, over the Austrian region during summer. The authors have access to dense observation datasets and are able to separate periods of weak and strong synoptic forcing. They are also able to compare model performance over mountainous regions and adjacent plains. The methodology and data analysis is good. The paper is reasonably well-written, with occasional spelling/grammar errors which are noted below. The suggestions I have are largely cosmetic and would not require further data analysis, so I am recommending accept with minor revisions.

Major points:

1. It took a while to understand how the AROME-EPS is set up. Is the AROME-EPS a direct downscaler of ALADIN-LAEF with the same initial time or is there a lag before the start of AROME-EPS? Maybe a diagram showing the EPS setup could help?

â€” You are right, AROME-EPS is a direct downscaler of ALADIN-LAEF, and it is run with the same initial time as ALADIN-LAEF. The set-up is as simple as possible, however, we agree that this information is not clearly stated in the text. Despite this, we decided not to add any additional diagram, as a large number of Figures is included in the paper already. Instead, we reworked the paragraphs which contain the relevant information, and explained the set-up more clearly. (Page 8, Lines 176-180; Page 9 f., Lines 198-203; Page 12, Lines 257-258).

2. Fig 4 is rather poor quality. Some of the axis labels have been cropped (well at least on my PDF viewer!) and the lines and legend appear rather faint. The y-axis should also include zero.

â€” We are sorry for this circumstance. The problem is likely caused by the graphical transformation of the underlying diagrams. We now tried to convert the original graphics in a different way, which brought, at least, a small improvement of the quality.

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Additionally we added 0 to the y-axis where it was missing. If the improvement of quality is not sufficient we can also try to rerun the generation of verification diagrams.

3. Section 4.1: Why is the verification of surface variables more significant than upper level variables? Is this because there are more surface observations? The text should explain this.

â€” For the evaluation of upper air variables we used the grid values of IFS-ECMWF analyses instead of observations as verification data. This was due to the low number of available (radiosonde) observations on these levels. Therefore, the lower significance on the upper levels results rather from the model set-up than from the verification strategy. Near surface and on lower levels AROME-EPS can add more information to the model simulation compared to ALADIN-LAEF than on higher levels. This can mainly be explained by the SURFEX soil scheme and the interaction between a refined representation of orography and the model physics schemes and dynamics. On the higher levels, however, there is less influence of the orography and the simulation resembles more the driving model. We added the information to the text (Page 24, Lines 500-509)

4. I'm not convinced that Table 2 adds much to the paper. Consider deleting it.

â€” Thanks for the suggestion. We agree that Table 2 contains a lot of information, which is not really needed to highlight the verification results. Our conclusion concerning model performance was drawn upon a broad variety of verification metrics. But only a small selection of results could be shown. The primary idea of Table 2 was to inform the reader about this circumstance. We now decided to remove Table 2. We think that the notes in the text should be sufficient to explain that we used more than the three presented point-to-point metrics for our verification. (Page 16, Lines 348-357)

Minor points:

1. Title: May be better to say "On the forecast skill of a convection...". - Done. Thank

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you for the advise. Indeed, it seems better to use the singular word skill as an overall term.

2. Fig 4 caption: Rather say which verification area separately “...August 15, 2011 of AROME-EPS (dotted line) and ALADIN-LAEF (solid line), both verified over the AROME-domain. ...” - Done.

3. Include “Strong/weak forcing” and “threshold” on axis labels in Figs 5-8. It is hard to follow which panel is which from the caption alone. - Done.

4. Typo on p12, line 13: “... on which rains was...” - Done.

5. P13, lines 5-6: “...which is of most interest to users of convection permitting...” - Done.

Please also note the supplement to this comment:

<http://www.geosci-model-dev-discuss.net/gmd-2016-191/gmd-2016-191-AC1-supplement.pdf>

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-191, 2016.

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