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Geoscientific Model Development Discussions

Interactive comment on "Evaluation of lateral boundary conditions in a regional chemical transport model" *by* E. Andersson et al.

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Thank you for taking your time and effort in reviewing our paper. Your comments have been valuable and will help to improve our paper. Regarding your comments/questions, please see the response below. Changes will be done regarding all the points, where we will clarify the text regarding the point 1-4 and add caption text for point 5. In the author comments, we refer to pages and line number from the downloaded version on the GMDD webpage.

1) As the global version of the EMEP model is used to provide the dynamic boundary conditions to the MATCH model, more should be written on the global EMEP model in the methodology part, rather than the regional version of the EMEP model.

The global EMEP model is based upon the regional model and expanded to a larger region. It may therefore seem like we describe the regional model instead of the global one. We will add some further details regarding the expansion to the global model, to make it clearer.

2) Why only January and August are used for validation?

In the paper, on page 5773, line number 3, we try to motivate our choice, but this will be further specified in the revised edition. The months of January and August are chosen to represent the winter and summer seasons, which corresponds to low and high ozone levels, respectively.

3) Page 18, line 8: What is meant by the long term average here? The whole simulation mean?

Here, by long term average, we refer to the average of the seven year period (2006-1012) at the ground level from the measurement station Mace Head, for both CO and ozone. We will specify this further in the revised edition.

4) Why the authors prefer not to smooth model data for the 500 hPa analyses in section 3.3?

This is a noteworthy comment. There are primarily two reasons as to why we did not smooth the model data. First, smoothing a data set increases the reliability of the vertical distribution, but we are only interested in one particular pressure level. Second, the chosen 500 hPa level is the level at which the satellite retrievals are least dependent upon the a-priori. Thus the a priori has very little impact on the retrieval result at that level. These reasons were mentioned on page 5781, line 10–15. We will expand the explanations in the revised version to justify our choice more clearly.

5) Is Table 1 caption missing CO?
Yes, the caption was missing; this will be corrected in the revised edition.

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