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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 again 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 together with were you can find the changes in the revised edition (which is added as supplementary .pdf). Changes have been done regarding all the points. For changes according to your comments and suggestions in the revised edition, see the text marked with red.

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.

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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 have added further details regarding the expansion to the global model, on page 8 and line number 1-2 in the revised edition.

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

The months of January and August are chosen to represent the winter and summer seasons, which corresponds to low and high ozone levels, respectively. This has been added to the revised edition on 10, line number 27-28.

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. This has been specified in the revised edition on page 17 and line number 27.

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. We have expanded the explanations in the revised version to justify our choice more clearly, see page 19 and line number 1-5.

GMDD

8, C2408–C2410, 2015

5) Is Table 1 caption missing CO?

Yes, the caption was missing; this is corrected in the revised edition, see table 1.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/8/C2408/2015/gmdd-8-C2408-2015-supplement.pdf

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Interactive comment on Geosci. Model Dev. Discuss., 8, 5763, 2015.