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www.geosci-model-dev-discuss.net/8/C2335/2015/

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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General Comments
This study, as presented, has fairly narrow applicability. The finding that lateral boundary conditions of a regional model play a significant role for long-lived pollutants is obvious. That a global model is used for the lateral chemical boundary conditions of a regional model is nothing new.

Although this comment may sound rather critical, we do find it quite valuable, because it makes us realise that there are some important points that we did not explain sufficiently well in the original manuscript. We wish to strongly emphasise that the points the reviewer addresses are not the focus of our work. Indeed, we never

made any claims that the significance of lateral boundary conditions (LBCs) in regional models or the use of LBCs from a global model are among the novelties of our work; quite on the contrary, we have cited several relevant publications on these topics. Even more particularly, this is not a study on the significance of hemispheric transport at all; if it were, then we would have submitted it to ACP. Rather, this is a technical/development/evaluation paper, which falls within the scope of GMD; the main issue is to discuss the methodology that is being used for evaluation of LBCs. We will make all possible efforts to emphasise this more explicitly in the revised version in order to avoid any misunderstandings. Most importantly, we emphasise more clearly the novelties in this work:

- The primary focus of this study is the methodology for *evaluating* LBCs by use of satellite data, *not* to investigate their significance in regional modelling. LBCs are often evaluated by comparing in-domain model results to (satellite) observations. A direct evaluation at the domain boundary has previously only been reported in the recent GMD paper by Henderson et al. (2014) for the North American domain. No such studies for the European domain have been reported, to the best of our knowledge.
- The EMEP model, which we use as a global model for generating LBCs, is *the* European policy model. To the best of our knowledge, this is the first time LBCs obtained from the global version of EMEP are confronted with satellite observations. (The GMD paper by Henderson et al. (2014) was based on using GEOS-Chem. We forgot to mention this in our paper, and will correct this in the revised paper.)
- The work by Henderson et al. (2014) was limited to evaluating the LBCs at the domain boundary. Our work goes one step further by combining the boundary evaluation with in-domain model-run evaluation; i.e., we first evaluate smoothed EMEP model results at the boundary against satellite data, and then we use the

boundary conditions in another regional model and evaluate the in-domain concentration fields against independent satellite data from a different instrument, as well as against in situ ground observations. The results clearly confirm that LBCs evaluated by satellite observations at the boundary can be expected to provide accurate results in the free troposphere; however, they also reveal the limitations of the methodology for ensuring the accuracy of boundary-layer concentrations. This makes it clear that it is not sufficient to limit the evaluation to using satellite data, and it underlines the critical importance of monitoring ground concentrations near the inflow boundary.

- The reviewer mentions that it is obvious that LBCs play a significant role in regional modelling. However, our results indicate that for ground concentrations this significance may have been overestimated in previous studies. Even though we consider long-lived species, we find that the LBCs influence ground concentrations only at locations in close proximity to the inflow domain. (However, since this is a methodology-paper, this is only a corollary finding of our work.)

We intend to submit a revised manuscript with appropriate changes in the Introduction section and in the Conclusion section.

The analysis of EMEP-based boundary conditions for Swedish MATCH is only useful for those using Swedish MATCH, and potentially for other model users with a similar European domain. Can the authors think of a way to reframe this study to make it of more broader interest?

Again, this is an important comment, as it helps us realise that we have not explained certain facts well enough, because we simply took them for granted. There is a rather large number of regional models in Europe, probably at least one in every country (and probably more than in North America!). For instance, the operational European

air-quality forecasting system that has been developed during the EU-FP7 project MACC (https://www.gmes-atmosphere.eu) is based on an ensemble-forecasting approach using seven of the major European models. Among them are both EMEP and MATCH, but also CHIMERE, EURAD-IM, LOTOS-EUROS, MOCAGE, and SILAM. So one should not underestimate the size of the community that is among the potential benefactors of this study. Also, since EMEP has been originally developed as a regional European model, it is particularly attractive for regional modellers in Europe to use the global EMEP model for generating LBCs. We intend to make these important points much clearer in the revised version of the paper.

One thing that might be interesting is to add evaluation of another global model for use as LBCs for Europe (e.g., MOZART-4/GEOS-5 from NCAR).

As a matter of fact, during the work that resulted in this paper we *did* perform an extra evaluation of LBCs generated by MOZART. For the record, we show the results of this evaluation in the two supplemented figures.

As can be seen in these two figures, we do not find any striking differences as compared to the case of EMEP LBCs. However, this is not the reason why we did not include these results in the manuscript. Rather, the reason is that MOZART results are used as an *a priori* estimate in the MOPITT retrieval algorithm. Hence, a comparison of MOZART boundary fields with MOPITT retrievals would, as far as we understand it, not constitute a comparison of truly independent data sets. We do not have access to any global model fields other than MOZART or EMEP.

For the reason mentioned above, we would prefer to not include the figures above in the final paper, unless the editor insists that they should be included.

Specific Comments
In the discussion of underestimation of surface CO concentrations, it is recommended to reference Stein et al. 2014. (http://www.atmos-chem-phys.net/14/9295/2014/acp-14-9295-2014.pdf).

This reference is already cited on page 15, line number 23 in the discussion of underestimation of CO surface concentrations.

/colorblue p. 5770, starting at line 26. Please add a more detailed description of where the fixed lateral boundary conditions for MATCH (later referred to as "ORIG") come from. What are they based on?

We agree that the description of the original boundary conditions was too brief. We used the same monthly or seasonally varying boundary conditions as in Andersson et al. (2006), which were partly based on large-scale model runs reported in Näs et al. (2003), and partly on measurements. In particular, monthly varying top boundary concentrations for CO and ozone were based on sonde data from Ireland, the United Kingdom, and Norway, averaged over the years 1996–2001 (Andersson et al., 2006). The corresponding monthly varying lateral boundary values are based on back-trajectory analysed measurements for 1999 from EMEP stations near the model-domain boundaries; this analysis was performed by Solberg et al. (2005). A complete table of the boundary values is given in Andersson et al. (2006). In the revised manuscript we have added this information as well as citations to Näs et al. (2003) and Solberg et al. (2005), see page 9 and line number 10-14. The paper by Andersson et al. (2006) was listed in the discussion paper; however, the year was incorrectly given as 2007 instead of 2006.

Figures 2 & 4. Include in these plots what the "ORIG" lateral boundary conditions look

like.

We agree with the reviewer that it will be helpful to provide the original boundary values, even though they can be found in Andersson et al. (2006). However, we also think that the plots are already quite busy as they are; adding yet another line would make the plots quite unintelligible. We therefore prefer to add the requested information in the supplementary materials as a table for CO and O_3.

Section 2.4. The authors should justify their choice of a very small subset of ground-based stations. Why not use all of the EMEP network? Or all rural background stations in the Airbase network?

We agree with this comment in principle. It is characteristic of these types of studies that there are always numerous ways in which one could do extra work, and it is also characteristic that the reviewers will, inevitably, point out some of them. We had, in the course of this work, numerous discussions about the scope and extent of the work that should enter into this study, where we tried to weigh the amount of extra work with the expected insights such work could offer. Many of those discussions did materialise and were included in the paper. Extending the number of observation stations was one of the options we had discussed. We also consulted with colleagues with extensive experience in the use of ground observations for evaluating chemical transport models. In our case, we found that the LBCs do impact free tropospheric concentrations, but they make a significant impact on ground concentrations only close to the inflow boundary, where ground observations are extremely sparse. We therefore decided that the extra work we could invest into extending the number of ground stations in our analysis is out of proportion to the prospective benefits. We will amend the text of the revised paper to clarify and justify our choices, as suggested.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/8/C2335/2015/gmdd-8-C2335-2015-supplement.pdf

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

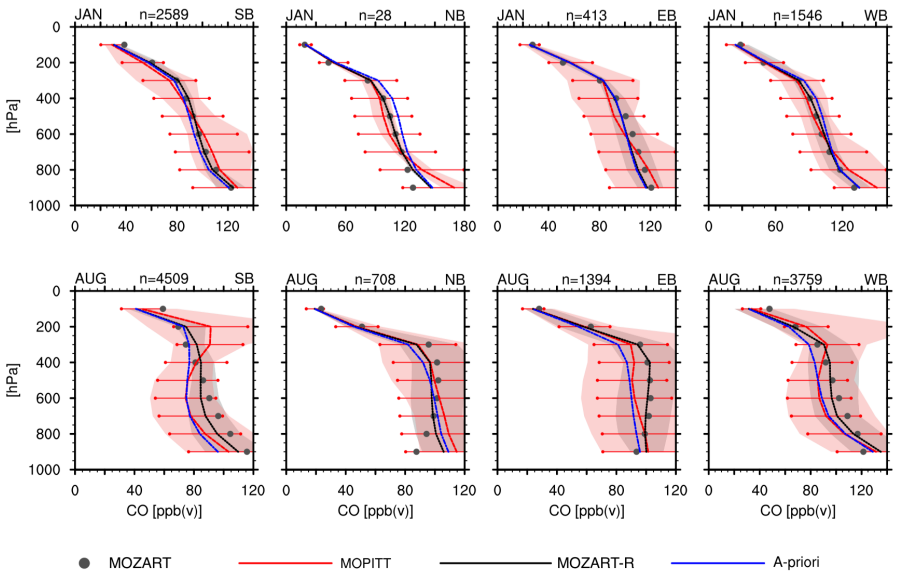

Fig. 1. Carbon monoxide mixing ratios for January (first row) and August (second row) at the four cardinal boundaries (denoted SB, NB, EB and WB).

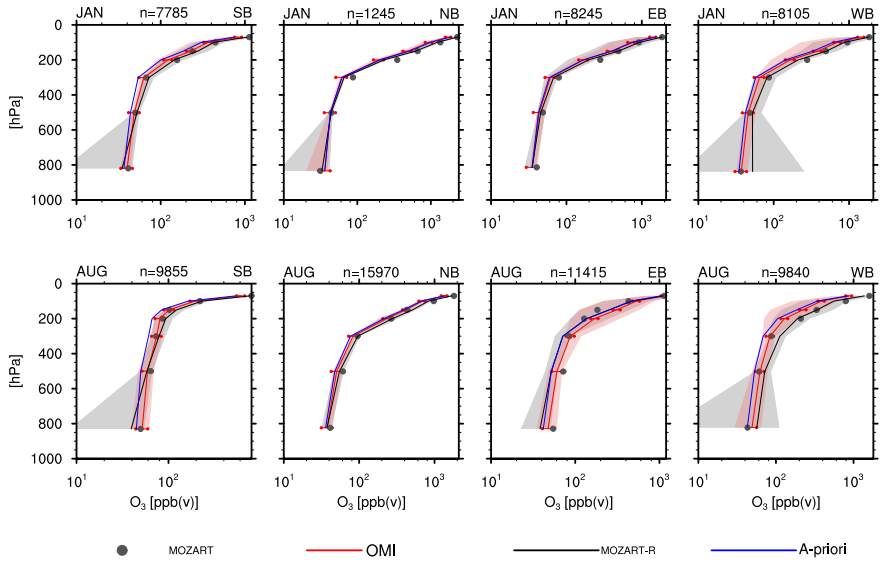

Fig. 2. Same as Fig. 1, but for ozone and for OMI satellite retrievals