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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling representation errors of atmospheric CO_2 concentrations at a regional scale" *by* L. F. Tolk et al.

L. F. Tolk et al.

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We thank the reviewer for his comments and the constructive review. The analyses proposed in the review returned some interesting results which are included in a new version of the paper. The regression analysis and the separation between the different sources of the RE as suggested revealed the importance of convective structures. This factor of RE (in Gerbig et al. (2003 a,b) referred to as 'sampling error') did not get the full attention it deserved in the previous version of our paper and is emphasized in the new version. This was also the reason for the discrepancy mentioned in the review between the conclusion in this work and from Gerbig et al. (2003). Besides, we added a paragraph and two new figures that address the relative contribution of different components to the representation error. We have acknowledged the reviewer for his helpful comments at the end of our manuscript. Below we copied the remarks

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from the review and answer them further individually.

What would be interesting to see is if the simulated fields can be used to determine the connection between flux variability on unresolved scales and concentration variability. For example one could imagine a functional relationship that relates subgrid variability (or RE) of atmospheric CO2 to the RE of surface-atmosphere exchange fluxes of CO2. This would allow specifying the temporal and spatial dependence of RE, at least the part that is not dominated by mesoscale transport patterns, to be used in inversions using coarser models. If this can be done using the existing framework, I would regard this as an important contribution to add to the existing paper.

Response: In order to find a functional relationship for the RE, we studied the relationship between the flux variability and the RE during the day. We did not find a clear relationship between the sub grid flux variability and the RE. At high resolutions small scale meteorological variations due to convective structures overshadowed the relation between the fluxes and the RE. At 50 km and especially 100 km resolution, where the influence of the flux variability on the RE is most pronounced relative to the small scale meteorological variability, the number of coarse grid points within our domain was too limited to draw robust conclusions about a functional relationship.

Regarding the overall approach, the following should be discussed: Subgrid variability is often caused by a simple gradient or a step change across the gridcell, e.g. as obvious from Figs. 2 and 4. However, the concept of a standard deviation is valid in general only for a statistical sample with a sufficiently large number of elements. By using single time steps and single grid cells to derive both the temporal and spatial patterns of the RE one is obviously restricted in the size of the statistical sample. Note that in the measurement based approach of Gerbig et al., 2003 and Lin et al., 2004 we did not have to make this assumption due to a large number of profile measurements within each group.

Response: In our study the size of the statistical sample ranges from 25 (at 10 km

ACPD

8, S3359–S3364, 2008

resolution) to 2500 (at 100 km resolution) elements. We assumed these samples are large enough for the statistical analysis. We added this to the methods section. There the difference with the Gerbig et al., 2003 and Lin et al., 2004 regarding the statistical sample is stressed as well.

A related issue that should be discussed is to what degree simple gradients across large scale grid cells contribute to the RE. Simple linear interpolation in the coarse model would allow accounting for this fraction.

Response: We added a calculation of the RE based on interpolated coarse resolution values to see how much simple gradients across large scale grid cells contribute to the RE. The calculation is described in the paragraph 2.1. In the new figure 6 the influence is shown separately for the coast and the eastern (land) part of the domain. The interpolation appeared particularly important where land-sea and other surface cover contrasts cause large scale gradients in the CO2 concentration. We added a new paragraph to the manuscript that includes a description of the influence of linear interpolation (paragraph 3.6) and it is added to the discussed section.

Specific comments: Abstract, page 3288 line 13: The term "careful up-scaling" is not very clear in this context; it is also not mentioned in the rest of the paper.

Response: We reformulated this sentence.

Page 3290, Representation error calculation: It needs to be stated that this is done for different model levels independently. Also, it should be specified at what temporal resolution this is done.

Response: The suggestions are added to the text in paragraph 2.1

Page 3294, line 5: "the deep boundary layer was stationary over the forest": what is meant with stationary boundary layer, that there are no winds? I would suggest to reformulate this.

Response: This sentence is reformulated.

ACPD

8, S3359–S3364, 2008

Page 3295, line 8: Deep convection would not stop at 3 km. This event should be described as shallow convection.

Response: This is reformulated.

Page 3295, line 20: Since there really is no other influence on RE than CO2 gradients, the term "caused" instead of "influenced" seems more appropriate.

Response: We replaced the term.

Page 3295, line 24: "because of the lack of residual boundary layer ": It is not a lack of residual layer. Only the spatial patterns in CO2 caused by the sources and sinks from the previous day are not present.

Response: We reformulated this.

Page 3297, line 4: "During daytime, the REs simulated with a spatial homogeneous flux are only half as large as those of the standard simulation.": Can this be used to estimate the relative contribution of flux variability and mesoscale transport features to the RE, e.g. by decomposing RE into two parts RE(flux) and RE(meso), and calculating RE as the geometric sum?

Response: Two new figures are added in which we attempt to decompose the RE into the relative contributions of RE(meteo), RE(flux) and RE(gradient). Figure 4 gives an indication of the contribution of the sea breeze front to the total RE, relative to the background RE away from the front. At 10 km resolution this is about 2 ppm. We added new figures with the relative contributions of the different sources as figure 6 and 7 (instead of the previous figure 6 which showed the nocturnal CO2 concentrations). We discuss them in paragraph 3.6 and section 4.

Page 3297, line 19: This paper shows that the RE is not a constant number. However, if a constant number can be used in inversion studies or not has not been shown in this paper. I would expect that for a conservative (sufficiently large) uncertainty estimate the inversion could still retrieve unbiased results.

ACPD

8, S3359–S3364, 2008

Response: Theoretically a conservative constant estimate could indeed be used. However, a conservative constant estimate that also includes the nocturnal and mesoscale circulation REs would be so large that it would cover most of the signal, even at locations and times where this is not necessary. Therefore we recommend the use of a time and space dependent uncertainty estimate of the RE. We corrected our formulation in section 4.

Page 3297, line 24: It should be noted that the Gerbig et al. (2003a, b) study estimated the RE for many different areas within the US based on both, experimental and theoretical evidence. The Les Landes area studied here in comparison is certainly special with regard to its often occurring sea-breeze circulation, probably causing an increased RE. It might be interesting to show in Fig. 3 the curves from van der Molen and Dolman as well as from Gerbig et al. for comparison.

Response: We included the curves from the previous RE studies in figure 3. The suggested remarks are included in the discussion section where figure 3 is discussed further.

Page 3298, line 3: Table one specifies that only the vertical gradient (boundary layer vs. free troposphere) was changed. This by itself is not proof that results do not change with different initial concentration patterns. I would suggest to either test a checkerboard initialisation, or argue on theoretical grounds that previous days patterns are mixed spatially and do not contribute significantly to patterns observed on the current day.

Response: In paragraph 3.3 we included a discussion about the influence of the variations of the previous day. We argue that on days like 6 June the previous day patterns do not contribute significantly to the RE on the current day.

Page 3299, line 28: Can a more specific advice be given, e.g. what scale of topography is allowed, what is not? Otherwise, almost all monitoring sites will fail, since there is always some topography due to rivers and hills.

ACPD

8, S3359–S3364, 2008

Response: The enhanced RE due to topography is simulated near modest topography. Besides, the signal is advected and will also influence areas downwind of the topographic features. Therefore, we advice to take in account a conservative estimate of the uncertainty due to RE during the night, unless the terrain is extremely flat. This is reformulated.

Page 3300, line 21: "the largest gain is obtained when the resolution is increased to finer scales than 10 km": This is in contrast to the results found in Gerbig et al. (2003a, b), where we suggested to use a resolution of 30 km, at which the representation error equals the measurement error (i.e. at larger scales RE dominates). Is there an explanation for this disagreement?

Response: The discrepancy between the two conclusions disappeared when we considered the importance of convective structures. Previously, we did not consider the inability of high resolution models to correctly simulate the exact locations of the updrafts and downdrafts. Taking this in account we conclude now that the RE is not necessarily reduced when the resolution is increased to scales below the size of convective structures. This is in agreement with the conclusion in Gerbig et al. (2003a, b). We changed this in section 4 (last paragraph).

Technical corrections: Page 3299, line 13: Papers in preparation are hard to access for the community. Looking at the references list, it actually looks like it is in press

Response: The paper is indeed in press, changed in the text.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 3287, 2008.