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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.

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General comments:

The paper addresses an important issue, namely the representation error (RE) that arises when atmospheric transport models of a given (insufficient) horizontal resolution are used to simulate atmospheric CO2 distribution. Mesoscale transport processes and surface flux variability are discussed as the causes for this representation error. The method uses a statistical analysis of CO2 fields simulated with a mesoscale model. The paper is certainly within the scope of ACP and should be published.

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.

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.

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.

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.

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.

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

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reformulate this.

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

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.

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.

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?

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.

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.

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 checker-

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board 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.

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.

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?

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

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