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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A two-step scheme for high-resolution regional atmospheric trace gas inversions based on independent models" *by* C. Rödenbeck et al.

C. Rödenbeck et al.

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Author Response to Review 2:

Ad 1(a): As Section 2.1 does not refer to models, but to the basic equation of motion, any comment concerning grid cells is not pertinent here. However, even if a diffusion term was necessary, the equation would still be a linear one, so the argumentation of Section 2.1 would be unchanged. To make this clearer, we added a footnote.

Ad 1(b): The regional field is of course identical to the global one within the DoI. We add this statement for clarity.

Concerning the formulation of the boundary conditions: The main purpose of the moti-

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vation section is to show that the linearity of the equation of motion allows the split of the mixing ratio field into two parts, only one of which is directly related to the structure of the local fluxes. Our mathematical formulation has indeed been a bit sloppy. We therefore reformulated this discussion, and added the link to the implementation of the "coarse regional" model.

Ad 1(c): See reply to 1(b).

Ad 1(d): If the reviewer feels that the notion of 'pathways' was not compatible with the continuity equation, how does he/she reconcile his/her suggestion of 'injected mass' with the continuity equation?

The 2nd paragraph of the comment is misplaced as the split of the mixing ratio field does not refer to models.

Regarding the reviewer's suggestion about an "iteration": We show already in our tests that the error is small.

Ad "false impression of mathematical accuracy": This is a weird comment, given that the section is entitled "Motivation", and that most of the discussion of the paper is about the approximations in the scheme.

Ad 2(a): The paper clearly states that TM3 is only taken as a test case. Though a test on the (higher) target resolution would definitely be desirable, it should be clear to the reviewer that no such test can feasibly be done.

Ad 2(b): The scheme itself makes no reference at all to any specifics of Eulerian models. The coupling between the 2 steps happens only via a vector of mixing ratios. There is no difference between Eulerian and Lagrangian models with respect to how the model is used in the scheme. We added these points to the discussion.

Ad 2(c): As we are inverting station data, the fluxes (no matter if calculated by an "ordinary" or nested inversion) will not be reliable at pixel resolution. Thus, considering pixels would not make sense. We rather present target quantities (=regionally

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integrated fluxes) that are commonly looked at in the inversion community. We added these statements.

Ad 3(a): Though TM5 is certainly a good (Eulerian) model, it is widely accepted that Lagrangian models offer the potential of much finer resolution. Moreover, the reviewer seems to forget that an inversion using a nested model is also just an approximation with respect to an inversion on fine resolution globally. For example, when using a nested model, how large is the impact of the imbalance in resolution within or outside the DoI? We are not aware of a published quantification of these errors, e.g. by a quantitative test similar to the one presented here.

Ad 3(b): The reviewer's suggestion would not work because in that case the second step could not be done with zero boundary conditions (i.e. one would need a nested model). Even more severe, it would not be a good approximation, because the fluxes f_1 are only available at the coarse resolution, and therefore not suitable as a part of the high-resolution result.

Ad 4: We disagree on the following statements by the reviewer:
- The theoretical basis of the scheme is the split of the mixing ratio field into contributions from different pathways, possible due to linearity of transport. Even if our formulation had been sloppy, none of the reviewer's comments would question this split.
- As said already above, the scheme offers the potential to use high-resolution Lagrangian models. The alternative mentioned by the referee would not be comparable in resolution.

Ad 'Minor comments':

1: 'is' referres to singular 'input'

2: to avoid ambiguities, we changed the designation of the mixing ratio contributions that did or did not cross the boundary into "trans" or "cis" contribution.

3: added.

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4: added.

5: URL added.

6: slightly reformulated. Of course, application of the scheme in another inversion system requires some specific consideration.

7: we do not think so.

8: All panels have the same y-range (shifted vertically according to the respective average values) and the same tick interval, for the following reasons: - The smaller temporal variations in SW, NW, and Northern Europe are only due to the smaller sizes of these regions, which thus have a smaller impact on atmosheric signals. Therefore, magnifying their y-ranges would unduely exaggerate the impact of the errors from these regions compared to the larger regions. Thus, we decided to have essentially equal y-ranges across all regions (the only exception being Central Europe in Fig 4 where the orange line dips below -2PgC/yr such that we added one more tic interval). - The only curve clipped is the blue one representing the result with large model errors - its only message is that it is far off from the known truth. This message can be easily read from the figure also with clipping. In contrast, extension of the range to avoid clipping would make the other curves considerably smaller and thus disguise the actual information of the figure.

9: The plots show the time series 'as is' (cmp. page 1732 lines 8-9 and page 1736 lines 21-22). Though the connecting lines would indeed not be suited to represent an interpolation of the mixing ratio, the purpose of the figures is to show the degree of agreement between the different lines. A plot with symols, dots, or steps would serve this purpose less well than the lines.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 1727, 2009.