Atmos. Chem. Phys. Discuss., 9, S1993–S1998, 2009
www.atmos-chem-phys-discuss.net/9/S1993/2009/

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.

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Received and published: 7 May 2009

Author Response to review by P. Rayner

We would like to thank the reviewer for his comments, which helped us to improve the exposition of the scheme. Accordingly, we restructured and reformulated the discussion section, putting more emphasis on explaining how the scheme works and what the conditions of its application are.

this paper presents a method for efficiently and simply nesting high-resolution atmospheric tracer inversions inside the larger domain required to supply boundary conditions. these boundary conditions are themselves informed by an atmospheric inversion carried out in the larger domain (usually but not necessarily global). The problem is im-

portant given an upsurge of interest in regional-scale inversions (e.g. Peylin et al., 2005, Carouge et al., 2008 and Lauvaux et al., 2008, 2009). these inversions are constrained, on the one hand, by the need for high resolution to capture faithfully details of continental data (Geels et al, 2007, Law et al., 2008) and the computational demands of inversions at this resolution. Several solutions are available to this problem but they generally have limits e.g. to the difference of resolution actually possible or the flexibility to use different physical parameterizations at different resolutions. the solution outlined here can work around these problems at the cost of imperfect coupling.

We would like to comment that any nesting, such as an inversion with a nested model, or one where boundary conditions are estimated as additional degrees of freedom, represents an approximation to a hypothetical global high-resolution inversion as well.

The method is well described and the practical test to demonstrate it well framed and carried out us- ing a case where the high-resolution solution can be used as the benchmark. the main lack in the paper is some development explaining the requirements for the approach to work. I would probably have tried this with some simple mathematics and some very simple cases such as a 4-box model although there is no guarantee that would have helped.

We had indeed thought about a simple demonstration like this. However, we did not find one that would still be sufficiently similar to the real application to be useful, but at the same time add more insight over the mathematical motivation as of Sect. 2.1 and other argumentation given at various places.

even without this it is important for those who would apply this scheme to understand not only that it works but also how to live with its limits. When, for exam- ple, does the non-optimality of the two-stage solution really bite? I would imagine when there is strong coupling between the larger domain (the homogeneous part) and an ob- servation site in the inner domain.

A coupling between an inner site and the outside fluxes is actually not specifically

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problematic, because in step 1 the information from all the sites is fully available. More problematic would be the reverse, with a site just outside the DoI not directly available to step 2 any more, which therefore should be avoided. Some remarks concerning the choice of DoI w.r.t. the site locations and natural correlation breaks were given in page 1741 lines 4-14. We tried to present this more prominently now in a separate subsection (new Sect. 4.2), and added more discussion about how the scheme may be applied best. Of course, application of the scheme in different contexts would still require careful testing.

Are there any requirements on similarity between the models of the inner and outer domains?

This is an interesting question, even though it does not seem to have a general answer. One possibility to test it would be by repeating the numerical examples with a pair of very different models, such as a (coarse) Eulerian and a (fine) Lagrangian one. Of course, in this case, the benchmark (global Lagrangian) would not be feasible any more. As a potential work-around, one could do the inversion in three steps (global - larger DoI - smaller DoI), and try to perform the middle step both on coarse and fine resolution, thereby creating a partial benchmark for the third step. This is however out of scope of the present paper. Also note that the coarse model used here is rather poorly performing, making the presented test actually rather strong. In a real application, the model for step 1 would of course be choosen as fine as computationally feasible. We added more discussion of these points into the paper.

Is there any guidance for how much extra error is introduced by the approach?

In the numerical tests presented in the paper (Figs 3 and 4, right panels), the error of the scheme has been shown to be less than half the general error in retrieving the 'known truth', and smaller or much smaller than the signal size. This is one of the central statements of the paper (Sect. 4.1). Of course, as stated, these results are specific to the particular target quantities considered (three-monthly temporal variations of re-

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gional fluxes), but (1) these are quite usual target quantities of inversions of this type, and (2) the behaviour seems to be typical also for similar target quantities. If a very different target quantity was considered, the numerical test could also be evaluated in terms of it. We added a discussion of this point.

Does the subtraction to produce the homogeneous concentration field depend implicitly on the linearity of the large-scale transport model?

The split of the mixing ratio field does depend on the linearity of the transport equation, as stated. We therefore expect that non-linearity of the model does introduce some additional error. As TM3 is linear, there is no problem in the present case. How large the error from using a non-linear model would actually be, and whether it would be a problem, could be tested numerically using such a model.

there are also, I think, one or two things missing in the description of the method. What errors does one apply to the residual concentration for the second phase of the inversion?

The uncertainty intervals are the same, as implicitly stated in Sect. 2.4.3. This of course neglects that the observations and the 'remaining mixing ratio' have different statistics, but this effect is small, as discussed in Sect. 4.2. (page 1741 lines 4-14). We now discuss this point even more explicitly (new Sect. 4.1).

How does one propagate the uncertainty from the first step through to the second? I imagine this could come either through an uncertainty on the assumed zero boundary condition or, perhaps more simply, through considering the contribution of the uncertainty in the far-field fluxes to the uncertainty in the residual concentration.

A formal propagation of uncertainties would probably indeed work according to the suggestions of the referee. However, as we showed for the test example A (Fig 8), the uncertainties can be calculated quite exactly just from step 2 even when the error propagation from step 1 is neglected. As discussed, this is due to the fact that the local

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fluxes are mainly related to local mixing ratio gradients that are hardly affected by the global fluxes. We therefore did not consider the exact propagation worthwhile.

I also would like to see a little more quantification of the success. How different is the two-step solution from the benchmark compared to the posterior uncertainty of either?

Figs. 3 and 4 show a comparison between the errors arising from the scheme (deviations of the step-2 results from the benchmark) and the general errors of the inversion (deviations of the benchmark from the 'known truth'). These errors are given as amplitudes (temporal standard deviations of differences) with the intention that this directly quantifies the errors in a signal of practical interest, and they are in turn compared to the signal amplitude itself.

The changes I request are uniquely in the description of the results and discussion, I doubt the paper needs any fur ther experiments.

Specific comments: P1733L11 I don't understand the "uncorrelated" assumption here, it doesn't seem necessary; Bayesian statistics can handle correlations provided they are not between prior distributions of unknowns and data.

The parameters are uncorrelated just by definition, such that all information about the a-priori correlations of the *fluxes* is contained in the matrix **F** (compare formula in line 13).

P1733L14 replace "gradients" with "gradient"

Done.

P1734L5 replace "is re-entering" with "re-enters"

Done.

P1738L5 "good as possible" should be "well as possible"

Done.

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P1740 (bottom) I don't understand this comment. Do the authors mean that the model that calculates the homogeneous part at coarse resolution (for subtraction from the global calculation) should be the same as the global model?

Yes, this is what was meant. We try to improve the formulation.

P1741L20 and beyond. There Is a theoretical problem with the inversion as its set up here but I don't think it's an a priori correlation between the data and the first simulation but rather the lack of optimality in the two-stage process. A problem would arise if there was a correlation between the prior used in the DoI and the residual concentrations but there shouldn't be here.

We do not fully understand this comment on the exact nature of the theoretical problem, but in any case the relevant point is that (1) the fact that within-DoI fluxes mainly depend on within-DoI mixing ratio gradients means that both any suboptimality and the mentioned correlations are small, and (2) the numerical test examples are shown not to be much affected by the problem. We reformulated this discussion.

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