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Interactive Comment

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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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 important 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. The method is well described and the practical test to demonstrate it well framed and carried out using 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. 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 example, 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 observation site in the inner domain. Are there any requirements on similarity between the models of the inner and outer domains? Is there any guidance for how much extra error is introduced by the approach? Does the subtraction to produce the homogeneous concentration field depend implicitly on the linearity of the large-scale transport 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? 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. 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? The changes I request are uniquely in the description of the results and discussion, I doubt the paper needs any further 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

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unknowns and data. P1733L14 replace "gradients" with "gradient" P1734L5 replace "is re-entering" with "re-enters" P1738L5 "good as possible" should be "well as possible" 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? 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.

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