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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Inversion of long-lived trace gas emissions using combined Eulerian and Lagrangian chemical transport models" *by* M. Rigby et al.

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The study presents revised yearly emission estimates for SF6 based on AGAGE atmospheric data. It uses a matrix inversion technique, with pulse response functions calculated by a combination of fine-scale Lagrangian transport modelling around some measurement sites, and coarser global Eulerian transport modelling. Improvements of the estimates due to the higher resolution of transport are discussed. I find it an interesting contribution. I would like to recommend the study for publication, after revision of the following points:

The authors put much emphasis on the method, and on presenting it as more suitable than the two-step scheme of Rödenbeck et al (2010). However, the presented method is only applicable to problems where fluxes can be decomposed into a relatively small number of emission pulses (a few hundred in the presented SF6 example with yearly pulses). In contrast, typical regional CO2 inversions, for which the Rödenbeck et al (2010) scheme was designed, have at least daily flux resolution, and many more observation stations around which high spatial resolution would be required in the one-step method. While I agree that a global one-step solution has conceptual advantages, I expect the computational burden of the one-step method to be prohibitive in cases like the example of Rödenbeck et al (2010) which has 30000 degrees of freedom (where the two-step scheme minimizes the cost function in well less than 100 model runs). As a further disadvantage, the spatial and temporal disaggregation of emission pulses has to be fixed in advance, while no choices have to be taken and full flexibility remains in the two-step scheme except for fixing the domain of interest. One the other hand, regarding the efford of implementation, it does not seem to me that the presented method is any simpler than the two-step scheme. In summary, if the authors feel the methods comparison important, they need to revise it to include all relevant aspects in a balanced way.

It is claimed several times that the method avoids aggregation errors. While I fully agree that higher resolution can generally be expected to reduce spatial aggregation errors, the study does not substantiate or discuss this in any detail. No attempt is made to actually show how large the remaining aggregation errors are for the chosen geometry of emission pulses, and that they are now indeed "small". More material is needed here to support the claims. It should also made clear that reduction of aggregation errors just comes from higher resolution, not from the specific implementation method introduced here.

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Interactive Comment