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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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We would like to thank Dr Rodenbeck for his thoughtful comments. We respond to each criticism below. Dr Rodenbeck's comments are italicized.

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 inter-

esting contribution. I would like to recommend the study for publication, after revision of the following points:

We thank Dr Rodenbeck for this positive assessment

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).

We would like to emphasize that this work is not intended to 'supersede' the Dr Rodenbeck's work. Rather, as we mention in the original submission it is presented as an alternative approach which may be more appropriate in some instances. However, we feel that the conceptual simplicity of the proposed approach does have some advantages. For example, as we demonstrate, global and local emissions can readily be derived simultaneously using one observational data set in a single step using any number of established techniques (Bayesian inversion, Monte-Carlo, least-squares, etc.). The one-step approach means that these techniques can be applied without needing to use observations twice, and without having to modify the a priori correlation structure in the inversion.

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 Rodenbeck 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).

We do not agree with this assessment. The computational burden of the proposed method is not as large as implied in the above comment. 30,000 degrees of freedom is certainly not unreasonable. This is because of the approximations we make about the influence of local emissions on the background mole fraction. For example, in Section 3 of the paper, emissions are solved for $\sim$50 sub-regions within the European domain (although pixel-resolution could also be used, if desired). We assume that the influence of these regions on the global background is the same for every one of the sub-regions. Therefore, in this case, the Eulerian model only needs to be run twice in order to simulate the influence of perturbed European sub-regions on the global background (once to simulate the perturbation to the background and once to remove the 'local' influence from the Eulerian simulation at Mace Head). Of course, if it was felt that the local region should be sub-divided in order to better represent the change in the background then this is entirely possible too with a few more runs of the Eulerian model. In light of this comment, we have clarified in the paper that the entire local emissions region (or some appropriate sub-division) should be perturbed, rather than each local sub-region, we have also modified the schematic figure (1) to try to illustrate this.

The modest setup used in section 3 required $\sim$100 hours CPU time for the Eulerian model (which is, when tracers are run in parallel, equivalent to $\sim$10 single tracer forward runs in terms of CPU time) and a single run of the Lagrangian model (the footprints from which can then be used for many species).

Although we present annual emissions in the paper, monthly or daily perturbations could also be solved for with the proposed method by tracking 'pulses' for short periods in the model, albeit at much higher (but by no means prohibitive) computational cost. Appropriate assumptions would need to be made about the long-term fate of these pulses (e.g. an exponential decay as in Rigby et al., 2010, or by modifying the covariance matrix in a Kalman smoother as in Bruhwiler et al., 2005).

Similarly to the two-step approach, multiple measurement stations could readily be

included in each local region, simply by combining Lagrangian model footprints within each region.

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.

We agree that the temporal disaggregation needs to be fixed beforehand. Also, similarly to the two-step approach, the domain(s) of interest need to be fixed. However, also similarly to the two-step approach, the spatial disaggregation within the domain can be changed after the fact.

Given the efficiency of the system, we do not see the lack of 'temporal flexibility' as a problem, as the system can be re-calculated for a reasonable computational burden.

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.

We do not claim that the implementation of our system requires less effort than any other. However, as mentioned in the introduction and above, the conceptual simplicity of the proposed method makes it readily usable with many established inverse methods.

Dr Rodenbeck is correct though, that the implementation can be quite difficult. To aid future researchers, we are very happy to share the code used to perform the inversion upon request.

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.

To clarify, when the reviewer refers to the influence of 'resolution' on the aggregation error, we assume that he means the scale on which the emissions field is resolved, rather than the resolution of the meteorological fields, which will have no influence on the aggregation error. To illustrate the influence of aggregation error on our results, we performed an inversion where all of the grid cells within each local region were aggregated into single regions. Therefore the inversion used only 10 regions (6 non-local and 4 local), compared to more than 100 in the inversion in the paper (6 non-local and $\sim$ 100 local). The posterior de-trended squared correlation coefficient (R^2) at MHD, THD, GSN and CGO was 0.26, 0.14, 0.41 and 0.53 respectively in this case compared to 0.48, 0.11, 0.52, 0.53 in the inversion presented in the paper. Significant improvements in the model's ability to reproduce the observed variability is found at MHD and GSN if the local emission field was broken down in to many regions.

References

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