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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new analytical inversion method for determining regional and global emissions of greenhouse gases: sensitivity studies and application to halocarbons" *by* A. Stohl et al.

Anonymous Referee #1

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General Comments

This paper extends an existing inversion method to estimate long-lived tracer fluxes, given a priori estimates of those fluxes and transport calculations using a Lagrangian particle dispersion model (LPDM). The method is then used to give a posteriori estimates of the emissions of three halocarbons that will become increasingly abundant and important for the radiative forcing of climate. The paper is very well written and the method and its application are rigorous.

The greatest limitation of the work so far is that the error in a priori emissions estimates is not propagated through to estimate uncertainty in the a posteriori emissions. This weakens the conclusions that can be drawn from the summary tables 4-7. In addition, it seems somewhat dangerous to quote the emissions estimated per country (Tables 5-7) and their change between 2005 and 2006 without obtaining a rigorous uncertainty estimate for each number. For example, can we really conclude that German HFC-134a emissions increased from 2005 to 2006 (Table 4) or are the figures indistinguishable given the uncertainty? Furthermore, as stated in the text, the attribution to each country is uncertain, especially when emissions are close to borders. In the case of Germany are the emissions really half the level derived by UNFCCC or are the a posteriori estimates for this country too uncertain to tell?

On p.19088, l.20 the authors subjectively estimate errors in the emissions. How were these numbers estimated? You need to justify them. I recommend quoting large uncertainties which you feel would safely encompass the true value and then use your future work to reduce the uncertainty and estimate it more precisely. These numbers need to be quoted in Tables 4-7 so that readers do not draw misleading conclusions.

I recommend publication of this excellent paper, subject to the revision suggested above. Some minor corrections are listed below.

Technical Corrections

1. At no point do you justify the use of 20 day backward simulations. What motivated this choice? Would you use longer simulations if you had the resources? At what point does trajectory length become irrelevant?

2. p.19069, l.20: Use either Ny-Alesund or Zeppelin.

3. p.19073, l.11: Why use a stepwise linear segments of 31 days long? Is there a relation to the 20 day trajectory length? What are $n_1 and n_2$?

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4. p.19073: Although you could refer to this method as Bayesian, once all the approximations to the PDFs (i.e., normally distributed, uncorrelated errors etc) have been made it looks like a 3D-VAR algorithm to me, boiling down to a least squares cost minimisation problem. It would be worth adding some references to the literature on data assimilation for numerical weather prediction (you appear to be using the standard notation of that literature).

5. p.19081: Why refer to the zero prior emissions assumption as Tikhonov regularisation? On its own this is a rather meaningless statement that would baffle unfamiliar readers unless connected to the relevant literature where Tikhonov regularisation is used.

6. p.19094, l.14: A time-lag between emission and reported consumption is used to explain unexpected results for HCFC-22. Why not adjust for a two-year time-lag and repeat the inversion to show that you can obtain more consistent results?

7. p.19095, l.1: You point to the large increase in emissions for Central America resulting from inversion in Fig.11. This also appears for the other two halocarbons, together with a strip of very high increases down the western flank of the Andes. Given that Fig.1 shows a very low SRR in this region, how much of this would you consider to be realistic? Is there a consistent problem with the method along the high orography of Central and South America?

8. p.19095, l.6: Expand LPDM here to help the readers.

9. p.19096, l.5: In my opinion, these are the most important conclusions to draw in this paper. Given that resources are limited, where would you set up new stations in the network (in priority order) and how much would they have the potential to improve emissions estimates?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 19063, 2008.

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