

Interactive comment on “An improved Kalman Smoother for atmospheric inversions” by L. M. P. Bruhwiler et al.

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The paper entitled “An improved Kalman smoother for atmospheric inversions” presents an alternative approach to infer sources and sinks of CO₂ from inversion of atmospheric transport, compared to the classical Bayesian synthesis inversion. The paper is mostly a methodological paper. It is particularly clear, well structured and pleasant to read. After an introduction presenting previous studies, the paper presents the Kalman smoother (section 3) and its comparison with synthesis inversion results (section 4). Section 3.1 does not bring original material but it is a very nice summary of the formalism of inverse methods. It might be put in annex, but this not an obligation, as the paper is not too long. Section 3.2 develops the “smoother” aspect of the

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methodology. Section 4 is interesting for modelling community because it shows that limiting transport information to a 6-month period is enough to get most of the inverted signals. Section 5, presenting the propagation of covariance, is the most interesting and original part of this work and makes, by itself, the paper very valuable for the CO2 inverse community. Thus I would recommend this paper for publication in ACP after accounting for the comments below :

- I find the presentation of previous studies and the references to synthesis inversions a bit partial and incomplete. In a CO2 inverse problem, surface sources and sinks are optimized against atmospheric observations. Two spaces are considered, the flux space and the observation space. On a computational side, two steps are the most limiting : the calculation of the response functions and the inverse procedure. For first inversions, using coarse transport model, annual fluxes and few remote observation sites, fluxes were aggregated annually over very few large regions (e.g. Fan et al., 1998). Solving for climatological seasonal cycles requested the development of the cyclo-stationary approach. Dealing with inter-annual variations over large regions on a monthly basis increased a lot the size of the problem, but was still feasible on present-day machines. A computational size problem occurred in inverse procedure when addressing the aggregation error in the flux space. Aggregation error occurs when an inverse procedure only solves for one scalar factor for an ensemble of model cells in space (making a region) and/or in time (Kaminski et al., 2001). Then if the space and/or time pattern is wrong, the inversion may scale the region source/sink for wrong reasons because of this hard constrained put on aggregated model cells. A solution to limit the aggregation error is to largely increase the number of regions solved for, and to provide soft constraints in order to regularize the inverse problem (Enguelen et al., 2002). Such soft constraints can be error correlations in the flux space. The calculation of response functions is much more efficient, in this case, by using the adjoint of the transport model, as long as the number of observations is not too large. But it is difficult to apply the classical synthesis formalism over inter-annual inversions because the size of the flux space is the number of grid cells of the transport model at

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the surface times the number of time steps of the inversion. The method proposed in this paper allows to address this problem, although CO₂ fluxes are solved over large regions only and not at model resolution. If the number of stations increases greatly (when satellite retrievals are available for instance), the size of the observation domain also increases greatly. Then the calculation of response functions really becomes a limiting factor of the inverse problem (at model resolution), because adjoint technique is less efficient when the number of stations is large. In this last case, only variational methods, deeply coupling transport and inversion thru 4D-VAR formalism can probably be applied. I would like the part of the introduction presenting previous studies to be re-written in this direction (p 1893,119—> p1894, 117). It can be also put in the discussion or conclusion. It should be said clearly that this studies brings elements to improve inverse procedures efficiency but without addressing the problems of aggregation error and of the calculation of response functions when solving for model cells individually.

- p1893 - l25 : Bousquet et al, (1999) solves for annual fluxes but used monthly observations. Thus they scaled prior seasonal fluxes over each region with one annual coefficient. You may also quote Bousquet et al., (2000) for inter-annual inversions.

- p1893 - l28 : I would not write that “more complex” is the main feature of cyclo-stationary inversions. It is just that when one wants to infer climatological monthly fluxes, the construction of response functions has to account for the dilution past emissions.

- p1894 - l5-10 : you should not suggest that cyclo-stationary inversions requires a lot of computational time. It negligible compared to inter-annual inversions.

- p1894 - l15 : I would develop the use of adjoint models after Rodenbeck reference (see my 1st comment)

- p1904 - l8-9 : To my knowledge, there were not 88 CO₂ stations active in 1980-85. Does it mean that extrapolated globalview values were used ? If yes, it is a bit strange. Why not using actual data ? I understand that this is a methodological paper and that

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it is thus not a critical point but it can lead to an artificial smoothing of the observations that could influence your results. Please comment on this.

- p1905 - l8-9 : TM3 is a widely used model. There is probably evidences in previous studies of a weak vertical mixing. Pleas add a reference here about this

- p1907 - l5 : typo error : "We the" ??

- p1909 - l10 : "... 3 months of transport has generally has the highest ..." . Why is this not always true ? ex: Jan, Feb and Mar over Amazonia ?

- p1911 : l9 : "Estimates... would be virtually free of prior assumptions ..." I do not agree with this sentence for 2 reasons :

1-The posterior estimates of the first time step depends on prior values of the fluxes. Using them as priors for the second time step still maintain an indirect link with prior estimates; and this link is propagated thru the different time steps.

2-Fluxes are solved for large regions over which a prior patterns in space is specified to calculate the response functions. Solving fluxes at model resolution would suppress this issue. I think this sentence should be removed.

- On the colors of lines in figures : please make as distinct as possible the different lines of your plots. It is often hard to distinguish the different cases (e.g. : fig 3). Captions of the different figures are often very close, making difficult to see what part of the text the figure is illustrating. I suggest to be more precise, specifying if it is made with the smoother only or with the correlation propagation.

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