Geosci. Model Dev. Discuss., 6, C338–C343, 2013
www.geosci-model-dev-discuss.net/6/C338/2013/


**Geoscientific Model Development Discussions**

# *Interactive comment on* "On the parallelization of atmospheric inversions of $CO_2$ surface fluxes within a variational framework" *by* F. Chevallier

**F. Chevallier**

frederic.chevallier@lsce.ipsl.fr

Received and published: 18 April 2013

I thank the reviewer for her/his careful reading and for her/his constructive comments. I have addressed them all. They are copied hereafter with my answers inserted where appropriate.

> This paper describes a novel approach to parallelize the very computing-intensive running of the forward model in a CO2 flux inversion. It divides the multi-year window into many smaller windows with some overlap to account for the proper mixing of the various flux signals. This method allows much more efficient running of flux inversions, which can be used to increase for instance the spatial or time resolution. It also appears to be



> much more stable than the very long window inversions. I think this paper is very suitable for GMDD and I recommend publication after my comments below have been taken into account.
>
> Main comments:
>
> Page 46, lines 14-18: This is a very worrying comment that deserves more emphasis. It means that long window inversions that have been done in the past can have very significant errors that have not been accounted for. It would be important to know how these errors grow as a function of window length. Has this been properly documented elsewhere?

Back in 2010, I lead the publication of the results of an inversion with a 21-yr window. The evaluation of this dataset with independent measurements (presented in this 2010 paper) showed no sign of numerical instability. A few months later, I tried to extend the inversion to a period of 22 years and I found unstable results (unpublished), while still being able to reproduce the previous stable results on 21 years. The instability was worrying indeed, but clearly appeared in the evaluation phase, like it clearly appears in the present study, so that no flawed results have been circulated. Such numerical artefacts are usually not documented in the peer-reviewed literature. However, as an example of numerical instabilities, it is well known that the orthogonality of the eigenvectors of the cost function Hessian needs to be artificially reinforced for the Lanczos algorithm to properly converge (Paige 1970), which we do.

Reference: Paige C.C. 1970, Practical use of the symmetric Lanczos process with re-orthogonalization, BIT Vol. 10, pp183-195.

> Section 6: The method is formulated for both global and regional inversions and Section 6 discusses the implications for regional inversions. Did you test the regional set-up at all? Does it indeed perform as well as the global inversions. If you want to include the regional set-up, there should be more material documenting its performance.

The results for the global inversion actually confirm previous knowledge about global mixing timescales (e.g., Bruhwiler et al. 2005) and are shown here for illustration purpose only. Adding regional ones would not add more information and would be very limited by their necessarily subjective set-up (choice of domain location in the world and choice of domain boundaries).

Regional inversions do not pose any challenge for the method proposed here because advection rapidly wipes away the initial state within the domain. The importance of horizontal advection actually challenges the extraction of the flux signal by the inversion (Goeckede et al. 2010). For instance, in the case studied by Lauvaux et al. (ACP. 2009) in the South West of France, only 1% of the tracer particles had stayed within the 300x300 $km^2$ domain after 15h of transport. With slower zonal winds or larger domains, longer spin-up periods are needed, but the 3-month mixing time used here at the global scale provides a very generous upper limit for regional inversions.

References:

Bruhwiler, L.M.P., Michalak, A.M.. Peters, W., Baker, D.F., and Tans, P.: An improved Kalman smoother for atmospheric inversions, Atmos. Chem. Phys, 5, 2691-2702. 2005.

Goeckede, M., Turner, D. P., Michalak, A. M., Vickers, D., and Law, B. E.: Sensitivity of a subregional scale atmospheric inverse CO2 modeling framework to boundary conditions, J. Geophys. Res., 115, D24112, doi:10.1029/2010JD014443, 2010

Lauvaux, T., Uliasz, M., Sarrat, C., Chevallier, F., Bousquet, P., Lac, C., Davis, K. J., Ciais, P., Denning, A. S., and Rayner, P. J.: Mesoscale inversion: first results from the CERES campaign with synthetic data, Atmos. Chem. Phys., 8, 3459-3471, doi:10.5194/acp-8-3459-2008, 2008.

> Minor comments:
> P38, line 4: I suggest to rephrase this sentence slightly to "...the ensem-



> ble or analytical approaches."

I will do this.

> P38, line 24-25: I find this a strange sentence (..., but it is rigorously guided by the Bayesian paradigm.) The Bayesian framework forms just one way of solving inversion problems, making use of prior information. It sounds here as if it is the only way to do this.

Bayes' therorem is a truism: it directly stems from the definition of conditional probability. It therefore can benchmark any inversion method, even when the method does not explicitly refer to it. The role of the prior probability density function in Bayes' theorem for flux inversion manifests the limited information content of atmospheric measurements in terms of surface fluxes (the limitation comes from non-reversible atmospheric mixing and from observation sparseness): prior information is always needed, in whatever form (e.g., assuming that prior fluxes are constant in space and/or in time).

> Also the next sentence ("It requires...") is not really embedded in this paragrapgh; it comes a bit out of the blue. Please consider rephrasing this paragraph.

I will suppress the sentence.

> P39, line 21: I am not sure if “mid-00s” is a correct English expression.

I will replace mid-00s by mid-2000s.

> P43, line 10: I suggest to replace “gathers” with “comprises of”.

I will do this.

P43, line 11: The “a” before “software” should be removed

I will do this.

P43, line 18: Which ECMWF analyses have been used? The operational or those from the reanalysis. If it was the former, would there be an impact of the changing output quality over the years 1979 - 2010?

I have used the ERA-40 reanalysis, but this is of no importance for the demonstration of the parallelization method.

P44, lines 14 and 19: I suggest using "length scale" instead of "length".

I will do this.

P45, lines 7-8: Could you please define "observation uncertainty" more carefully here. It stands for the observation-model differences and this might not be clear to all readers.

I will use a more classical vocabulary by referring to the observation errors that are assigned in the inversion system.

P47:, lines 1-2: Could you explain the shown patterns a bit more?

The patterns correspond to the areas of large prior uncertainty (Chevallier et al. 2010, Fig. 1a). I will insert this indication.



Figure 1: This figure is very hard to read and needs to be of better quality

The high resolution of the original figure does not seem to be exploited by the on-line version. I will check this with the technical editor.

---

Interactive comment on Geosci. Model Dev. Discuss., 6, 37, 2013.