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Interactive Comment

Interactive comment on "Daily CO_2 flux estimates over Europe from continuous atmospheric measurements: 1, inverse methodology" *by* P. Peylin et al.

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General comments

The paper describes an atmospheric transport inversion against continuous concentration data at six European sites, with the background field constrained by 64 flask sampling sites from the GLOBALVIEW CO_2 network [GLOBALVIEW-CO_2(2002)].

The paper explores the interpretation of continuous records in the framework of a

Bayesian synthesis inversion [Enting et al.(1995)Enting, Trudinger, and Francey]. This is highly significant, as it is a priori not clear whether an approach that proved successful for interpretation of background monitoring sites on global scale can be transferred to finer spatial and temporal scales.

The study focuses on methodological aspects and presents new concepts. Overall, I think, the authors are using the right approach. The following list contains a few suggestions that might be helpful.

Passing information from the first inversion step to the second

I agree with the authors that the inverse problem has to be tackled on global rather than on continental scale, as the continental budget depends strongly on the lateral boundary fluxes. The authors choose to solve the inverse problem in two steps: In an initial inversion step over a one year period, GLOBALVIEW flask sampling data [GLOBALVIEW-CO_2(2002)] are used to constrain fluxes over fixed big regions at monthly resolution in the temporal domain. In a second inversion step using a zoomed model version, the information from the six European sites (continuously sampling) is added. Since transport is linear, this is equivalent to a single step inversion that includes both sets of observations simultaneously *provided that* their uncertainties are uncorrelated and that posterior flux uncertainties from the first step are passing the information from the first step the second step.

In the present setup, the second assumption is violated, as only the diagonal of the covariance matrix of the first step's posterior flux uncertainty is included in the second inversion step. We know that the integral atmospheric constraints induce strong negative correlations on posterior uncertainties. Dropping these correlations make the posterior uncertainties from the first step appear artificially small. Passing the correlations with the diagonals would only require a small modification of the setup of the

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second inversion step: Those flux components from the first inversion which lie in the zoom region can be expressed as averages of components of the small scale flux field, i.e. there is some linear function A mapping the small scale flux field X_{fine} on the large scale flux field X_{coarse}:

$$X_{coarse} = AX_{fine} . \tag{1}$$

In the paper's equations (1) and (2) for the second inversion, the data vector Y^O could be extended by X_{coarse}, the covariance of its uncertainty extended by a block containing the posterior uncertainties from the first inversion step, and, finally, the transport matrix H by a block containing A. The prior uncertainties for the fluxes in the zoom region can then be selected on the basis of all available prior knowledge.

Initial Concentration for second inversion step

It is a good and very efficient approach to use the SVD of the sensitivity of the continuous observations to the initial concentration field in order to restrict the attention to the directions in the initial concentration field that are resolved by the data. Yet V is not orthonormal. Also in eq. (10) the transposed must go from the second V to the first.

Apart from this, it is not clear to me, why we should expect only positive correlations. After all the transport is mass conserving: Any transport error in the first inversion that results in a shift of a tracer plume from its true path, gives rise to negatively correlated uncertainties for the simulated concentrations used as initial concentration field for the second step. Negative correlations among posterior flux uncertainties from the first step will propagate into negative correlations in the uncertainty of the concentration field.

The large impact of the initial concentration is very tightly related to the selected setup. If one had chosen to start the simulation period on the fine grid on month prior to the first measurement (which would be comparable to the setup of [Rayner et al.(1999)Rayner, Enting, Francey, and Langenfelds]'s time dependent inversion, the initial concentration would have only a small impact. This dependence on the setup does not come across well in the abstract and the conclusions, where authors summarise their findings.

Adjoint

In the conclusions authors highlight three new features in their inversion approach, the third of which is the use of the retro plume approach. Yet Jacobians of fluxes at the transport model resolution have been employed for CO_2 inversions before, e.g. by [Kaminski et al.(1999)Kaminski, Heimann, and Giering, Rödenbeck et al.(2003)Rödenbeck, Houweling, Gloor, and Heimann], i.e. this feature is not new.

Whether a Jacobian is computed by a) many transport model runs, b) an adjoint that operates on the level of the statements in the code (Automatic Differentiation) or c) on the level of the process implementations (as for LMDZ) does not make any difference for the transport inversion, as long as the Jacobian is correct.

In the introduction the authors state that the former approach to adjoint code construction (b) "can become a tedious task", whereas via the letter approach (c) an adjoint "can be easily constructed". Given that adjoint code generation can meanwhile be automated, which also simplifies maintenance and given that the process-level approach also has its pitfalls, this is misleading.

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Uncertainty in continuous concentrations

I understand that observational uncertainties are specified by computing the standard deviation of the hourly average concentrations. It is not clear to me, whether this captures the uncertainty in the observational process and in the model.

Additional diagnostics

Showing the fit to the flask sampling sites both over Europe and remote prior to the first inversion step and posterior to the second would be an interesting check of the consistency of the two-step inversion.

The continuous data can only resolve 30×6 directions in flux space. It would be interesting to look at correlations among posterior flux uncertainties from the second inversion step and show a few directions in flux space that are well resolved and a few that are not well resolved.

Identical twin experiments

The present inversion method is new and the paper lists many difficulties, especially when it comes to assigning the uncertainties that determine the weights in the cost function. This is confirmed by the very low χ^2 value of 0.09.

The authors stress that the aim of the studies is methodological. It would have increased confidence in the method, if it had first been presented in an identical twin (or pseudo-data)-experiment. This is common in the data assimilation community, where from prescribed values of the "unknowns", a synthetic data set is generated by the model and the inversion method used to infer the "unknowns". To be as realistic as

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possible, the uncertainty in the pseudo observations can be extracted from the real observations used here, and a random noise added to the synthetic observations before inversion.

Particular formulations in the text and typos

Presentation should be improved. Some issues are indicated in the following.

p 1648 l 26: use $\mapsto$ used

p 1649 l 18: "The alternative to regularisation is to solve for only in a greatly reduced solution space": I consider this as an extremely crude form of regularisation rather than an alternative.

p 1650 l 11: "the synoptic variation in flow acts as a differential sampling tool": Is "act' the best word here? Would "is employed as a component of" be better?

p 1652 l 5: [Peylin et al.(2000)Peylin, Bousquet, Ciais, and Monfray] have addressed the problem in the temporal domain.

p 1652 l 26: "...construction of an adjoint model ... requires only one run ...": It is not the construction (which is code development) but the execution of the adjoint

p 1653 l 2: easely $\mapsto$ easily

p 1655 l 5: "There is no consensus about which resolution should be used for a particular data frequency". Isn't the consensus that we always go for the highest resolution that is computationally feasible?

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p 1658 l 15: Does it really only take one year for an emission to be uniformly distributed?

p 1658 l 23: “almost all combinations of these values are unobservable by our chosen network, since it takes observations only over one month and a limited domain”: I don’t see the impact of the limited domain. One would not observe more than 180 directions in source space even if the domain in which the observations are taken was not limited.

p 1662 l 6: About spatial correlations of prior uncertainty: “if the inversion requires a flux correction along the path of the retro plume, it is likely that the effect should effect the neighbouring pixels”: It is conceptually misleading to base prior information on atmospheric transport considerations or, even worse, on the desired outcome. One is only allowed to include information from sources other those to be used in the inversion.

p 1662 l 18: on correlation length: “...is probably smaller, as daily flux patterns are usually less homogeneous than monthly flux patterns” Does the way the argument is written down mix up features of the underlying quantity with features of its uncertainty?

p 1662 l 25: On solving the inverse problem in observation space: If authors wanted to make a link to NWP, where some DA systems are also implemented in observation space, they could refer to the one at NASA-GMAO.

p 1663 l 15: “degrees of freedom” sounds much like a concept based on integers, is there a word the better captures the continuous nature here?

p 1663 l 17+21: reduce $\mapsto$ reduced

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p 1663 l 17+21: correlations $\mapsto$ correlation

p 1664 l 23-25: This short paragraph is difficult to understand. An equation could help.

p 1665 l 28: on the impact of size and geometry of the network: “probably” is a bit too weak: We know that more uncorrelated observations will deweight the other terms in the cost function.

p 1667 l 21: “values masked on Fig. 4 by station symbols”: just take a different symbol instead of the big star!

p 1668 l 8: are you sure you mean percent here? In Germany we have both “percent” and “percentage point”, and they mean different things.

p 1668 l 17: what do authors mean by “we clearly loose the spatial pattern of the major ...”?

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