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Atmospheric Chemistry and Physics Discussions



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

**P. Peylin et al.**

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General Comments:

We agree with the reviewer that the use of "pseudo-data" would have allowed us to examine several critical issues not present in the paper. However there are other limitations only visible when using real data. With ‘‘pseudo-data" the transport is usually supposed to be perfect. We expect weaknesses in the transport model to be a major limitation in the use of such data and so we wanted to include this somehow. Adding random noise on the data to simulate transport errors is not sufficient as most of the transport errors are likely to be more systematic than random. We thus choose to

investigate the case of real data. A following study will include the benefit of using simulated data in the context of a full year inversion.

In his general comment, the reviewer suggests the use of pseudo-data to separate the impact on the actual errors in the estimates due to the transport model error and to the use of an approximate adjoint with the ‘‘retro-plume” approach (his point 3). We agree with this overall but see the previous response for reasons to use real data. Note also that it is not straightforward to decide whether the ‘‘retro-transport” equations give numerical results that are closer or further from the reality than the results of the ‘‘direct-transport” equations. Hourdin et al. (2005b) detail these aspects and we only briefly mention this point page 1657 (‘‘Note, however that in the numerical world....Transport Experiment.”). We slightly changed this paragraph in order to clarify this point.

Specific comments:

Pg2, col 1, par 1, li 6-7: No the temporal smoothing does not change the ability to constrain the fluxes inside the continents. This sentence makes an additional point.

Pg2, col 1, par 1, li 7-9: We agree with the reviewer and we have thus clarified the text by adding the considered time scale (as suggested).

Pg2, par2, last 2 sentences: We fully agree with the reviewer that the resolving power of the continuous data is greater only if we assume that the variations in fluxes are known on time scales equal to or longer than the sampling frequency. The reviewer suggests replacing the word ‘‘predictable” in the last sentence by ‘‘assumed” or ‘‘specified”. We believe that ‘‘predictable” is better suited as it includes the case of an inversion using a process-based model to compute (or predict) the fluxes. The carbon community is increasingly performing the direct optimisation of biogeochemical models (i.e., the parameters of the model). We thus wanted to be general enough to include this case. The overall meaning of the sentence with the proposed words ‘‘assumed” or ‘‘specified” would then be too narrow.



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Pg2, par5, li2: We removed the notion of ‘‘synthesis inversion” as it was initially design by Enting et al. (1995) to make the distinction between former ‘‘mass-balance” approaches and new probabilistic approaches. We replaced the word ‘‘synthesis” by ‘‘matrix” as it refers to the current split of the inverse approaches into the ‘‘variationnal” and the ‘‘matricial” techniques.

Pg2, col 2, par 3, li 10-14: We changed the last sentence of this paragraph to include the idea that the errors associated with the use of no diurnal cycle are considered as part of the data uncertainty.

Pg3, col 1, par2: The references Hourdin et al. 2004a,b are now in press in QJRMS.

Section 2.2 describes succinctly the ‘‘retro-transport” approach and directly refers to those references for all details requested by the reviewer. We decided to keep the description relatively short as the ‘‘retro-transport” concept is not central to the message of the paper.

The reviewer also asks about the precision of the numerically-implemented version of the ‘‘retro-transport” versus the exact adjoint of the forward tangent linear model. As we already mentioned in the response to the general comments, such question is slightly misleading. It supposes that the exact adjoint of the forward tangent linear model is a better approximation of the transport characteristics than the ‘‘retro-transport” approach that we use. However, the differences between the two approaches 1) only arise in the numerical world as explained in section 2.2 and 2) can not be related to any physical transport properties. It is thus not straightforward to decide which approach best matches reality. The choice depends on the use that will be made of the results. Some problem could arise if we combine the results of the ‘‘retro-transport” approach with forward model simulations, because the ‘‘retro-plumes” do not exactly correspond to the forward tangent linear model. This is not the case in this study as we define all response functions with the ‘‘retro-plumes”. We are also not using an iterative approach where differences between forward and reverse modes can confound



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optimization techniques. In order to clarify this point, we slightly modified paragraph 3 in section 2.2.

Pg3, col2, par2, li 1-5: We changed the text to mention that the 6 European sites were not included in the global inversion.

Pg3, col2, par3, li 13: we changed the text to ‘‘hourly concentration averages”.

Pg4, col1: The reference Hourdin et al. 2004 is now in press in QJRM.

Pg4, col2, par3, li 5-7: We added the sentence ‘‘Note that each pulse is run back separately from the others”.

Pg 4, col2, par3, li8: We changed the sentence to include the precision, ‘‘across the full 24 hours”.

Pg 5, col1, equation 10: Changed.

Pg6, col 1, last par: The block form of the matrix Pb is due to the fact that we do not have temporal error correlations in this study. If we thus order all fluxes day after day in the matrix Pb we have only blocks of spatial correlations. The separation between the initial concentration field and the fluxes is only a minor simplification, given the limited number of initial conditions (180).

However, the reviewer is right that this block form of Pb is not the most important part of the cost saving and that the ability to project the full spatial dimension line by line is the critical property of the form we use for the inversion (equations 1 and 2). We reformulated this paragraph in order to be more precise and to mention explicitly such important properties of our inverse formulation.

Pg6, col2, par4, li 4: We replaced ‘‘altitude” stations by ‘‘high-altitude” stations throughout the manuscript.

Subtitle 3.2: We changed the subtitle to the reviewer’s suggestion.



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Pg7, col1, par3, li 8-15: We believe that our conclusions include the fact that daily fluxes depend strongly on the decay of the initial conditions and the lateral boundary fluxes from the coarser regions into the domain. This is stated in the 4th point of the major outcomes in the conclusion section: ‘‘For campaign-style inversion studies, ... in the inversion”. We have checked that the fluxes do not critically depend on the time span considered but they depend on the grid that is used, as is the case for most transport models. However this latter dependence is beyond the scope of this paper, as we do not intent to study the role of the grid size and of the zoom geometry in the retrieved fluxes.

Pg7, col2, par3, li 6-7: We deleted the reference to Dargaville et al. as the paper is still not submitted.

Pg8, par1: We agree with the reviewer that the statement ‘‘one clear result ... to constrain regional fluxes” was too strong and overstated. However, we believe that the shape of both flux correction and reduction of error are sufficiently different between the 500km and the 2000 km correlation length cases to draw some conclusions. In the 2000 km case, we are close to the case of previous inversions with large regions. Given that the error reduction in that correlation length case extends across all Europe including eastern regions with the use of only 6 western European sites, we believe that the power of isolated concentration measurements might have been overestimated in previous large-region inversions. We thus reformulated the last sentence of that paragraph in order to soften the conclusion and to only suggest the potential pitfall of large-region inversions.

Pg8, col1, par2, li 3: We agree with the reviewer that the reduction-of-error statistic only indirectly measures the contribution of the measurements regarding fluxes at each pixel. Because it depends on the prior flux error, the mean value of this quantity, percentage-wise, can be misleading. However, the relative difference between each pixel of the error-reduction statistic indicates the potential of the network in terms of spatial resolution of the fluxes. Moreover, we do not agree with the reviewer that the



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measurement information content, given by HTR-1H (or even its inverse) is a better quantity to represent the measurement constraint. This quantity does not account for the flux error correlations that are crucial in our inverse problem to spread the sparse information supplied by a few measurements sites. In the case of a ‘‘pixel-based” inversion with only few observations, it is indeed crucial to account for the structure of the flux error correlations that is used to properly discuss the potential of the network. With the measurement information content quantity, we would only get small areas around the sites with positive values and nearly zero elsewhere. Such a diagnostic does not tell us the potential of the network in the context of all the information that enters the inverse procedure and seems thus much less useful to us than the quantity that accounts for the full prior flux error correlations. Overall, we kept our statistic of error-reduction, but slightly changed the beginning of the paragraph to mention that it is only an indirect measure of the information added by the measurements.

Figure 3: We clarified the label of the different lines in the caption of the figure. The distinction between plain and dotted lines was already stated.

Figure 4: We initially said ‘‘monthly fluxes” and ‘‘monthly error reduction” in the caption. In order to be more precise we changed to ‘‘monthly mean fluxes” and ‘‘monthly mean error reduction (mean across all daily error reductions)”

Technical corrections:

Title: we chose ‘‘Daily CO2 flux estimates...”

All other suggestions proposed by the reviewer have been taken into account.

We apologize for spelling and grammar errors in the original manuscript and thank the reviewers for their corrections. We hope this version is more polished.

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1647, 2005.