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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:

Before the last paragraph: The reviewer wonders about the utility of the numerical machinery we developed in the case of hourly observations at dozens of sites. We should notice that the form of the inversion we use (Equations 1 and 2) only imply 1) huge matrix multiplications that can be done out of core by storing the intermediate steps into files and 2) the inversion of a matrix of the size of the observation space which is still affordable in the case of surface measurements but not in the case of satellite products. Note that any attempt to account for error correlations in data may

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require a similar matrix inversion even in a variational approach. These statements have been clarified in the new version of the text.

Specific Comments:

1. We indeed have postponed a thorough analysis of the different tradeoffs imposed by the choice of the inverse set up, to a subsequent paper using pseudo-data. This paper is in preparation.

2. The suggestion made by the reviewer is very good and we have extended a short paragraph in the introduction that was only briefly describing the study's intended role (''In this work we make a first attempt to .....''). We now explicitly mention and also justify the use of real observations to test a new methodology and to reveal the caveats and limitations of these inversions in the context of real observations. The paper both explores some methodological issues and their application to a real case. It is indeed critical to define pertinent diagnostics in the context of real observations and these diagnostics might be quite different in the case of ''pseudo-data''.

3. The point raise by the reviewer is indeed crucial when using low elevation continental towers in an inverse procedure. The day-to-day variability of the night time CO2 build up within the stable boundary condition is critical and we know that current models still have difficulties in properly representing this effect (see Geels et al., 2005). However with LMDZ and the November period, we believe that this effect does not impact the major outcomes of the paper. First, the use of the zoomed grid in LMDZ with a resolution of 50-km over Europe helps to better account for the local topography compared to standard global models. Finally the problem is also only likely to be severe for the HUN tower and Westerland site. Our test of the fit of the prior model to the data also suggests that the problem is serious but not disastrous. We agree that this issue was not discussed adequately and we have modified the text to explicitly mention these potential biases (paragraph 3, section 2.1). However in our first attempt to invert continuous measurements, we chose not to address in detail this issue and we only suggest the

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use of daytime only concentrations in order to avoid the problem.

For the last part of the comment (about the model's vertical structure), we have improved the text section 2.2 (first paragraph). We discuss in more detail the vertical characteristics of the model and how mountain sites are treated. These modifications are discussed below as they directly refer to point 6 of the reviewer's comments.

4. We have prescribed the daily mean fluxes from TURC to the LMDZ model and not the full diurnal cycle of these fluxes. We choose that solution because 1) the fluxes in November do not show a large diurnal cycle over Europe and that such diurnal cycle is still crudely modelled in TURC, and 2) the implementation of diurnally varying fluxes would complicate the use of the ''retro-plumes" without any substantial gain. We nevertheless mention in section 2.2 how to sample the retro-plumes in order to account for a given time distribution of the fluxes. We followed the suggestion of the reviewer and added one sentence p 1654 (paragraph 3) to explain our choice.

5. At the time of the study, there was no available information about the true variability of the fossil fuel fluxes in Europe. The situation has improved slightly with partial data available for a few Western European countries. We will use what data is available in follow-up studies. This is recognized as a major knowledge gap.

6. We agree that the description of the LMDZ model properties was quite succinct, especially about the vertical structure. We have included some additional information to explicitly mention the vertical discretisation in the PBL (3 to 4 levels), the convective scheme (Tiedke et al.) that is used, the treatment of mountain sites, and references that cover these aspects in more details. We have substantially increased the first paragraph of section 2.2 to cover these aspects. However, we have not discussed in detail some aspects raised by the reviewer such as the influence of mountain-valley circulations, frontal lifting, and stable boundary layers. We are aware that these features become crucial when using continental observations in an inverse procedure but a thorough analysis of their influence is beyond the scope of this paper. We only point

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towards some references that recently investigated these aspects with LMDZ.

7. Ok

8. We have improved the description of the treatment of prior flux uncertainty (section 2.4) in order to clarify the assumptions that were made to distribute a total uncertainty in space and time.

9. The size of the background covariance matrix is on the order of (217000 x 217000). As mentioned by reviewer D. Baker, it is the ability to project to the full spatial dimension line by line, rather than the block form of Pb that simplifies the inverse computations. The block form does make it possible to construct the matrix at all however. The line-by-line multiplication allows the extension of our inverse approach to a much larger network and even to hourly observations. All matrix multiplications can be done out of core and the critical point is only to be able to invert a final matrix that has the size of the number of observations. We have clarified these aspects in the text and we also provide the size of Pb.

10. The very low reduced chi-square value indeed indicates that for the 500 km length scale the fit to the data is very good. Such agreement results directly from our choice of large uncertainties for the data. We could have reduced them to get a chi-square value closer to one, but given that there are potentially large model biases we kept these errors relatively large. We made the conservative choice of large data uncertainty in order to avoid strong influence of any particular datum on the flux solution. The resulting chi-square values can thus not help with the selection of the correlation length scale L. Using tighter errors on the data would indeed lead to a different choice of L, if we only use the reduce chi-square diagnostic and the targeted value of one.

11. We slightly change this paragraph to emphasize the tradeoffs between an accurate specification of the initial conditions and the treatment of their uncertainty and also changed the conclusion (point 4 of the major outcomes) to highlight this aspect. In particular one would hope that a better observed initial condition would be closer to

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that which really affected the stations during the inversion period and would allow us to use smaller uncertainties, hence reducing the impact of the initial condition on the retrieved fluxes.

12. We reworded the last two sentences of the paragraph in order to clarify the point.

13. We agree with the reviewer that the use of correlation with distance is a crude simplification of reality and that this correlation plays an important role in the spread of the information from individual stations. The use of biogeochemical or geographic knowledge to define these spatial correlations would certainly change the results and the spread of the information from the data. However, realistic spatial patterns for the correlations are more likely to decrease the power of isolated concentration observations compared to simple length scales. Indeed the corrections of the initial fluxes are usually limited in space according 1) to the spatial extension of the synoptic weather events and 2) to the differential response of each ecosystem to climate forcing. These limitations are likely to be stronger than when using a large correlation length such as the 2000 km case. We did not explore this aspect of the inverse set-up as it is a new and complex field of investigation that is beyond the scope of this paper.

However, we change the paragraph ‘‘clear result from the difference .....” in order to moderate the conclusion as already pointed out by the first reviewer.

Technical corrections:

P1652: changed

P1656: footnote: Geels et al is still not published and we thus keep the footnote.

P1660: changed

P1662: changed

P1663: changed

P1665: changed

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P1666: As the manuscript ‘‘Dargaville et al.” is still not submitted we dropped this reference in the text.

P1668: changed

P1668: changed

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