

Interactive comment on “What can we learn from European continuous atmospheric CO₂ measurements to quantify regional fluxes – Part 1: Potential of the network” by C. Carouge et al.

Anonymous Referee #1

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General Comments

This paper, which is the first of a pair, evaluates the possibility of determining CO₂ fluxes from observed concentrations for Europe on a scale which is as fine as possible. The methods are state-of-the-art, and (especially for Europe) they have rarely if ever been applied on such a fine scale. The results are somewhat disappointing at first hearing (even for a perfect simulation of the transport, useful results can be obtained only on a relatively coarse scale, of order 1000 km and 10 days), which is however not the fault of the authors.

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When reading the methods section, I noted a number of passages (specified below) which I found difficult to understand. I would enjoy it if the authors could explain these points during the interactive discussion. I believe that this section is very important, and that some pages deserve a major revision to make them intelligible to a wide readership.

Further I would recommend an extension of the conclusions, which can easily be done.

Specific Comments

Overall there is some confusion about the grid distance of the model, a value of 50 km is given in the abstract, but 40 km on page 18596 and page 18605. The whole paper should be scrutinized on this point.

Title: Specify the network. It should be clear from the title at least that it concerns the (relatively limited) state of 2001.

Abstract: Here and elsewhere, the observation network should be specified better, by mentioning the names of institutions or frameworks or projects, as far as necessary.

Section 2: The section describes NEE computations but (as far as I can see) is silent about antropogenic emissions. Have they been neglected? If so, could this cause complications when the present results are applied to real situations? The paper should be more explicit on these points.

page 18597, line 5: I do not agree with the term “fundamental time symmetry”: Properly, there is no symmetry since a plume spreads out in forward simulation, but it should be contracted in backward simulation. This is apparently not what happens to the retro-tracer, which starts with a pulse. How is the problem of non-symmetry avoided? The

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answer is probably found in the literature to which the authors refer (which I did not study), but a brief explanation would be in place in the paper.

page 18599, line 29: The reason for choosing 1000 km is not well explained. The preceding text says that the observed correlation lengths are smaller.

page 18600, line 8: The way in which $0.5 \text{ gC m}^{-2} \text{ day}^{-1}$ is found should be explained better.

section 2.5.3: The exposition of this important section should be made more clear. Is it correct that lines 18-19 mean that correlations are zero unless the fluxes have either a common location or a common time? Using the word “separate” suggests independence, which is something different (e.g. correlation dependent on location-difference without considering time-difference). Am I right that the correlation between two different locations at the same time has maximum 1/2 ? If so, is this a logical construction?

page 18601, lines 21-22: “This product can be decomposed”: how?

page 18602, line 4: “we keep the middle month”: I do not understand what this means (consider it as final result?). More general, the important subject of how the time windows are fitted deserves a better exposition.

line 11: “offset”: explain.

lines 13-15: linearly decreasing the prior flux errors: requires a better explanation. E.g. “prior” is ambiguous since it is also used for fluxes which have already been corrected

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at a previous step. The nomenclature should be made more precise, as well as the numerical values.

page 18603, use of R and NSD: This seems appropriate. However, the text should include a warning for the pitfall: if R is much lower than 1, a decrease of NSD is an ambiguous result since it may be caused by noise reduction but also by a spurious compression of the signal amplitude. In the applications, this is usually but not always recognized (see e.g. line 14 on this page).

page 18603, line 12: $R_{APO} = 0.42$: Here there is a difference with Table 2 which has $R_{APO} = 0.43$. In the following, slight differences between the text and Table 2 occur time and again. The authors should scrutinize the text and Table 2.

line 15: “the inverted fluxes”: The term reoccurs several times. I do not understand what is precisely meant here. The procedure of deseasonalizing is applied to both prior and posterior fluxes.

page 18607, lines 24-26: It would be interesting to explain why the initial agreement is much better for North than for West Europe.

Conclusions: The conclusions already contain the most important points. However, they should be extended with a recapitulation of the inversion method. Herein the authors should emphasize the essential aspects of the method which have contributed to the partial success which they have obtained (e.g. setting up error matrices, dealing with the problem of too large matrices etc.).

Table 2: Check the numerical values, since there is not always correspondence with the numbers in the text.

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Technical corrections

page 18594, lines 10,12: “information content of observing system”: this should be reformulated, an observing system contains no information by itself.

page 18601: line 21: “2700000 x 3650” replace with “3650 x 2700000”.

page 18604, line 27: “deseasonialied” replace with “deseasonalized”.

page 18605, line 26: “both”: replace with “combined” or something like that, otherwise it would sound as if separate treatment would work too, which is not intended.

page 18607: References to figures 5.1, 5.2: the figure is itself divided into 5a-d.

Table 2: The results for the SP pixel should be placed after the results for Western Europe, in accordance with the ordering in the text (starting with the SP pixel is very confusing to the reader).

Figure 5: See remark at page 18607.

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