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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric CO_2 inversions at the mesoscale using data driven prior uncertainties. Part2: the European terrestrial CO_2 fluxes" *by* Panagiotis Kountouris et al.

Anonymous Referee #1

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The study presents an atmospheric inversion over Europe using surface tower measurements of atmospheric CO2. The methodology was described in the first part of the study, whereas the application of the method is presented in the second part. The inversion results are compared to previously published estimates for different years and to eddy-flux measurements for the same year. Overall, the inversion results show a better agreement with independent flux measurements and fall within the range of continental flux estimates. The inversion system uses an unprecedented number of observation sites and provide a fairly robust assessment of the inverse estimates with the posterior uncertainties.

The one and only criticism is related to the initial objective in comparison to the final

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Discussion paper

results. The regional inversion aims at improving the spatial and temporal distributions of the fluxes but I am concerned by the mismatch in the seasonal cycle of the fluxes and unexpected spatial features in the inverse fluxes. Considering the seasonality, both crop and non-crop sites start with a correct timing for the maximum uptake but are shifted by one month after inversion. Why is the inversion unable to capture the time of maximum uptake? Is there an error from the global inversion that shifts the peak? If the inversion is supporting ecosystem modeling activities, it is important to understand the potential causes of discrepancies between top-down and bottom-up approaches. A shift of one month shows that both approaches possibly disagree in the processes even if the annual and monthly estimates are closer the observed eddy-flux measurements.

Concerning the spatial distribution, the comparison to Meesters et al. (2012) is a first step towards a more complete assessment of the inverse flux estimates at the country scale. However, looking at the maps in Figure 3, several spatial features are difficult to explain. For example, the largest (or among the largest) sinks are located over the Alps (i.e. Switzerland and northern Italy), western Czeck Republic, The Netheralands, Belgium, and England. Some of these areas, e.g. the Alps or England, are not extremely productive areas in terms of vegetation, unless recent reforestation took place there. One would expect that the most agricultural regions would represent the largest uptake of CO2. At the opposite, large sources are visible in eastern Europe (e.g. Ukraine), western Atlantic of France and Poland, where vast areas of arable land are being cultivated. Are these signals only due to the lack of observations? The argument is used for England but sites over the Alps should constrain central Europe fairly well. The comparison to existing inversions and inventories seem straightforward and would provide an better overview of the sub-continental fluxes, more than comparing annual estimates over Europe. These flux signals are the most fundamental part of the regional inversion system if one claims that higher resolution is able to improve the current inverse fluxes from global inversion systems. More thoughts and analyses should be dedicated to demonstrating the accuracy of the inversion at these scales

(i.e. national scales) to confirm that the deployment of new atmospheric sites will help improve our understanding of the European carbon balance, or if regional systems are still too uncertain to provide a clear answer to bottom-up differences. One could argue that these analyses are beyond the scope of a single inversion, which is a fair argument assuming that some errors are difficult to diagnose. Nevertheless, each inversion system should be able to produce reasonable flux distributions that provide information to bottom-up estimates. More results and comments on that matter would be beneficial to the science community and to the use of continental scale inversions in general, and possibly argue whether or not higher resolution is helpful.

Finally, the last comment about this study is a possible conclusion from your results, at least a point that I have in mind looking at the magnitude of the flux components. As you discuss in Section 4.3.2, the magnitude of carbon sources and sinks at sub-continental scales depends on the uncertainties of the fossil fuel emissions. Because both components and their uncertainties are similar, errors in anthropogenic estimates may impact the biogenic fluxes after inversion. The magnitude of total emissions can suffer from large errors but even more singificantly the spatial and temporal distributions as well. I want to conclude here that fossil fuel emissions should not be prescribed here but instead optimized in a joint optimization framework. Comments about that statement and possibly a discussion would help. Futhermore, indications on whether or not future inversion efforts should address this issue would be welcome.

Specific comments:

page 3-line 3: "...have been applied using..." Please re-phrase ("assimilate"?)

page 3-line 5: Add references

page 3-line 6: Rephrase "focus of interest".

page 3-line 10: "makes difficult" Explain more clearly what you mean.

page 3-line 12-14: This example is very specific, out of place for an introduction, and

not supported by a reference. Delete or move this example.

page 3-line 18: ... not seen by the ground network...

page 3-line 25: Modeled dry air mole fractions

page 3-line 25: in vertical mixing

page 3-line 27: ...biases in concentrations due to transport model errors are translated...

page 4-line 1-2: When and where are these errors applied? The numbers have to be explained here.

page 4-line 10: Does it mean that the resolution of the state space is 0.5 degree? If so, what does 0.25 degree resolution correspond to?

page 6-line 13: For hourly concentrations, 1ppm changes seem fairly small. Why discarding these data? What fraction has been removed using this filter?

page 7-line 8-21: The codes used for the various inversions, i.e. B1, S1, S1a,... are difficult to remember and confusing for the readers. Short but self-explanatory codes would be easier to track in the figures and the results section.

page 8: Dependence to ecosystem types has not been considered here whereas previous ecosystem model assimilation studies often compute model parameters based on pft. Is there a reason to describe flux error correlations only based on distance?

page 10-line 5: Remove "Figure 1".

page 10 and Table 3: the use of the goodness of fit is a simple weighted total mismatch divided by the number of unknowns. The actual Degree of Freedom of the System would be more informative as it describes also the weigths of the observations compared to the prior errors. It will indicate if the solution is over- or under-constrained by the atmospheric data.

page 12-line 1-3: Are these very large positive corrections realistic? The discussion is very brief here.

page 12-line 16: Comments on the peak mismatch are important here (cf. general comment). Why is the inversion systematically shifting the maximum?

page 16-line 23-27: the argument is valid but applies to both crop and non-crop eddy-flux sites. Clarify that this problem is common to all the ecosystems.

page 17-line 22: To study the representativity of the flux sites to constrain a pft, a Leave-One-Out cross-validation would help evaluate if the optimization applies to land classes, or if each eddy-flux site is specific to its own area.

page 21-line 15-25: This example of country-scale flux evaluation provides a first assessment of the inverse fluxes at higher resolution. Is the agreement representative of any European country?

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