Atmos. Chem. Phys. Discuss., 15, C6243–C6248, 2015
www.atmos-chem-phys-discuss.net/15/C6243/2015/

Atmospheric Chemistry and Physics
Open Access
Discussions

Interactive comment on "On the potential of ICOS atmospheric CO_2 measurement network for the estimation of the biogenic CO_2 budget of Europe" *by* N. Kadygrov et al.

Anonymous Referee #1

Received and published: 26 August 2015

This paper describes efforts to assess the impact of an expanded European in situ GHG network (under ICOS) on our ability to determine net terrestrial biospheric CO2 fluxes over Europe. In particular, so-called Observation System Simulation Experiments (OSSEs) are used, in which atmospheric CO2 inversions are performed on pseudo-data, under various model and data configurations. Overall, the paper is well written, the figures are clear, and the analysis, for the most part, is sound. The topic and quality are appropriate for ACP. However, there are some significant assumptions and/or missing elements that make me doubt that the experiments conducted are sufficient to answer the question of how well the eventual ICOS network will be able to

determine annual NEE over the ICOS European domain.

To my mind, the main issues that are not dealt with fully, but that can have a major impact on retrieving CO2 fluxes from CO2 data in a regional inversion are:

1) The CO2 (and secondarily, meteorological) lateral boundary conditions, especially how uncertainty (both bias and 'noise') in the boundary CO2 fields propagates into the flux solution; and 2) The time-dependent fossil fuel emissions inside the domain, and how uncertainties (noise and bias) will propagate into NEE flux retrieval.

For both boundary CO2 and fossil fluxes the issue is not simply one where the uncertainty of the NEE will increase as a result of propagating errors. But there is the major issue that biases in these fixed parameter fields will alias into NEE biases. In other words, the results of the study, at present, need to caveated by saying that "In the limit of perfectly known fossil fuel emissions and lateral boundary conditions the proposed ICOS network will be able to solve for NEE with such and such resolution."

Another issue that is never addressed in the paper is that of whether the absolute uncertainties produced by this system might be useful enough to meet the ICOS/EU/national objectives. All of the figures in the main text, for example, deal with relative uncertainty reduction. It is only in the Appendix (Fig. A2) that absolute uncertainties are shown at the country scale. Moreover, it's not clear to the reader whether these values, say 0.25 gC/m2/day, would be useful policy-wise. I don't mean to say that the paper needs to include a C policy analysis, but some guidance or reference point needs to be provided to interpret the absolute uncertainties.

Specific comments:

P14222, 12: Given my concerns on the absence of boundary CO2 and FFCO2 in the OSSEs, I don't think this is 'robust'. Also, strike final 's' from Experiments.

P14222, 25: Strike 'resp.' in two instances. Not necessary and makes one erroneously think 'respiration'. P14233, 18: Strike 'The' at the start of the paragraph.

P14225, 3: Insert 'are' at the beginning of the line.

P14225, 9: Strike 's' in performances.

P14225, 28: strike 's' in Experiments.

P14226, 15: change 'built' to 'build'

P14227, 6: Earlier, the study is described as 'state of the art', yet using 50 km resolution for meteorology for a regional European inversion hardly seems so. (I understand the need, however, to solve for fluxes at 50 km to reduce the dimension of the problem.)

P14228, 17: When using 'hourly averages', it's not clear if these are night and day or only daytime (or as in Broquet, 2011, do they change by site class/altitude). If using nighttime data, are the corresponding 'data' error values in R inflated to account for the likely inability of the model to accurately simulate nighttime boundary layer structure? Moreover, if using consecutive hourly data, although off-diagonal elements are not included in R to account for hour-to-hour correlated errors in the meteorology, are the diagonal elements inflated to account for this effect? This issue is important, because if the effective number of independent observations in the analysis is too high (i.e. uncorrelated errors for consecutive hourly averages), then the uncertainty reduction produced will also be too high (according to eq. 2 which defines posterior covariance). Some, but not all, of this information is available from Broquet, 2011. More explanation is deserved here.

P14228, 25: As mentioned earlier, assuming that errors in fossil fuel emissions are "negligible" compared to transport errors is a big assumption, and one I doubt without good evidence to the contrary, which is not provided here. The paragraph goes on to say that ICOS sites are "relatively far from large urban centers", but it's not clear what "relatively" means in this case. Even if "relatively" here means that ICOS sites have in their 50x50 km cells one or two orders of magnitude less emissions than urban grid cells, the "local background" levels of FFCO2 will still be impacted. In short, there

may well be bias in the FF product used, including potential (missing) covariances between the temporal FF patterns and transport (see e.g., http://www.atmos-chem-phys-discuss.net/15/20679/2015/acpd-15-20679-2015.html). The bottom line for me is that especially in Europe with high emissions density, there needs to be a careful analysis of how these errors propagate into NEE estimates. If the error in NEE due to fossil fuel emissions is low, this would be a great result, but I think it needs to be demonstrated, not assumed.

P14229, 8: While I agree that it would be possible to correct much of the boundary condition bias through careful examination of 3D global model CO2 fields and upwind CO2 observations, I still think it is very important to propagate the random uncertainty from the boundary into the posterior flux estimates. This could be done in a number of ways mathematically, all the way from solving for one boundary value per observation in the state vector x (along with uncertainty), to simply inflating elements of R. Because the distance between the western boundary and the majority of the sites is of order 1-3 days PBL travel time, the boundary CO2 uncertainty, if taken into account could substantially inflate the NEE uncertainty.

P14230, 7: 'image' is confusing and unusual terminology here. Please clarify.

P14230, 23: This view of eq. 2 (i.e. posterior cov. A) is overly optimistic. Sure, the equation tells you that there's no sensitivity to fossil fluxes or the boundary, but that's a limitation of the equation, not a reflection of reality.

P14231, 13: It's not true that the dimension of the problem precludes an analytical solution (thus requiring 4DVar and the like). The system of Yadav and Michalak (GMD, 2013), allows for the relatively easy inversion of large matrices, with no loss of accuracy.

P14231, 28: change 'these' to 'the'.

P14232, 9: What are the potential impacts of a 500 mb ($\sim$ 5 km) ceiling for the model? For example, what if vertical transport (storms in the winter and convective lifting in summer) were to transfer surface signal into the upper troposphere? Is all this ok as long as there are no observations above this height? I'm not sure of the implications, but I would be more confident of the study if this issue was addressed.

P14232, 10: Fill in the missing section number after 'section'.

P14233, 3: Regarding edge effects, is a three day buffer at the end of the inversion period sufficient to capture all upwind fluxes ending on day 14 of the main period? Consider observations on the eastern part of the domain: fluxes from the western side of the domain may not have travelled all the way across (assuming westerly flow). Thus these fluxes may not be as well constrained as fluxes during the middle of the study period.

P14233, 16: Strike 'months' at the end of the paragraph.

P14234, 7: Please specify what the range of the scaling factors on Rh is?

P14236, 5: (see also final comment p14248): The authors may also want to cite Bousserez et al, 2015, Quarterly. J. Royal Met. Soc. concerning the number of ensemble members required for a given degree of accuracy of the posterior covariance matrix.

P14238, 16: Insert 'the' before 'south', otherwise this refers to Africa!

P14238, 25: It is not clear why 'there is generally a larger uncertainty reduction in July'. Please explain more.

P14239, 22: Change 'shows' to 'show'.

P14240, 9: Please explain the last sentence more. Why does this occur?

P14240, 28: This comparison with CT-EU is hard for me to understand. First, how are annual scale uncertainties from CT being compared with uncertainties just for two weeks from the present system? Second, CT uses a five week window in its ensemble Kalman smoother and only produces covariances at these time scales. Any annual

covariance from a system like this is not reliable in the first place.

P14241, 3: Change 'error temporal correlations…' to 'temporal correlations between uncertainties'.

P14241, 21: Figure 5 seems to have more spatial considered than just the 5 grid scales listed in the text.

P14247, 11: Delete 'the' before 'wind speed'

P14247, 17: Change 'results' to 'result'

P14248, 27: I understand that more iterations may be required for convergence with more observations, but would more ensemble members be necessary for accurate Monte Carlo uncertainties? Please see Bousserez, 2015.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 14221, 2015.