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Comment

Interactive comment on “Regional inversion of CO₂ ecosystem fluxes from atmospheric measurements: reliability of the uncertainty estimates” by G. Broquet et al.

Anonymous Referee #1

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This paper evaluates the methods of estimating/assigning uncertainties to the fluxes derived using inversion modelling. A posteriori fluxes are compared with those estimated from the flux tower network over the European domain. They find that the a posteriori uncertainties are somewhat large as estimated here and that interannual variations in fluxes cannot be determined statistically significantly at the European scale given the measurement network and inversion modelling systems employed for this work. The paper is well written, and addresses an important issue in inverse modelling. However, I have some reservations that their system is a bit too specific to be widely applicable for all inverse modelling systems in general. Please find my concerns below. I do not demand the authors to resolve any or all of the issues raised below, but

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at the least limitations of the approach should be clarified before publications in ACP.

Detailed comments: p.5771, l.15: I do not understand "respectively" as used here and elsewhere in this para.

p.5772, l. 1: "atmospheric CO₂ measurements" may sound better

p.5772, l. 6: What is the difference between inventory and climatology?

p.5778,~l.10: This is difficult to believe. I cannot say what is wrong, unless detailed values are given or the correlation lengths too large? In principle the prior flux uncertainty in NEEs should be large in the summer than winter. Can you comment on this? Or the system has large number of dipoles, when integrated over the whole domain the results look similar! How are the results at country scale, say, Germany or France or at site scale?

p.5780,~l.26: I am curious to see the results, if you make four divisions of the western Europe. Could you show a four column figure? Please provide the figure in your reply, if not in the main paper.

p.5782, l. 1: It is strange that all the figures and tables are cited before the results section p.5786, l.23: I thought that was one of the main targets of this paper? Have not such conclusions already well documented in published literatures?

p.5788, l.15: Whilst talking about the Europe wide fluxes, it may be good to use TgC/yr or /mon units?

p.5788,~l.25: For these conclusions that IAVs in monthly or annual fluxes have to be greater than a posteriori uncertainty for the flux IAVs to be significant, I think most of the model/data errors assigned to the sites are systematic, and only partly random, which would cancel out for sufficient number of model realisations. Much of the systematic components will keep the a posteriori uncertainty high, but the mean flux value will change due to the signals in atmospheric CO₂ data anomaly. As traditionally done in the CO₂ inverse modelling, one has to run sensitivity inversions to estimate the

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uncertainties for flux IAVs at monthly/annual scales.

p.5789, I.15: The "remarkable agreement" comes from the inversion setup, say, a priori dependence. A priori meaning not the a priori fluxes only, but also including the correlation lengths etc., which controls your inversion results Can you reduce the correlation lengths to a few forward model grids around the measurement sites, and perform the same analysis only for the grids of measurement sites? In such a system you will be handling mainly the a priori and and posteriori fluxes, I presume, constrained by CO2 measurements, without other external influences.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 5769, 2013.

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