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Interactive Comment

Interactive comment on "Satellite-inferred European carbon sink larger than expected" *by* M. Reuter et al.

P. Rayner (Referee)

prayner@unimelb.edu.au

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This paper presents a new look at the European carbon balance from the viewpoint of satelite measurements of xCO2. Its result, if correct, is striking indeed, carrying a strong reminder of the controversy following the publication of the Fan et al. paper in 1998. Controversial, of course, does not mean wrong but, as with the Fan paper, there are enough simplifications here to worry about.

Another way of stating the paper's conclusion for me is that in situ and remotely-sensed measurements of CO2 suggest very different things about the European net sink. The use of the Carbontracker posterior as a prior for this inversion is one way of assessing this consistency but I think it has some problems.

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Interactive Discussion

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the Carbontracker posterior estimate is a reasonable starting point for an inversion like this. As the authors point out, it could allow a stepwise inversion of surface and xCO2 data. for this, though, the uncertainty from Carbontracker must be correctly fed through from the observations and must, itself, be correctly derived from the observations for the new step of the inversion. I am not confident of this for a couple of reasons. Firstly the Ensemble Kalman Filter used for Carbontracker is a fine technique but a weakness is the specification of the posterior uncertainty. Limited ensemble size and the finite assimilation window make this difficult. furthermore, aggregating the uncertainty to the region used for this study is doable but not trivial ... hopefully it was directly generated from the ensemble members rather than from the estimated posterior covariance. There are also likely to be temporal uncertainty correlations among the estimates from month to month since the influence of observations in the Carbontracker system is not limited to month boundaries. correlations have been added but the choice of correlation structure is not very clearly motivated. Certainly doing so to regularize an inversion which is already regularized and constrained by a previous inversion is difficult to justify.

In summary I think the problem of using the Carbontracker posterior estimate as a prior is harder than it might appear. I even wonder why the authors did this? their formalism makes it fairly easy to start from the same prior as Carbontracker but use both the in situ and xCO2 measurements in the inversion. this would directly test the consistency of the measurements.

Another concern is the role of horizontal boundary conditions in conditioning determining inversion results. I understand that the use of a global bias removes the impact of the absolute value of the boundary conditions but I don't believe the sensitivity studies rule out a major role for the east-west difference in boundary conditions in determining the integrated flux. this is especially important when there might be considerable uncertainty in these conditions. I recommend carrying out an ensemble of inversions with an ensemble of prior flux and boundary conditions from Carbontracker.

Finally I am not convinced that the sensitivity study to region size is sufficient to rule out aggregation error. the regions involved are still quite large and it may well be true that gradients near the edges of the region provide most of the constraint.

Finally I offer one caution from the Fan et al. controversy. In trying to understand this result many of us spent a long time looking for the one key point of their inversion which explained the large difference between their results and others. Finally gurney et al. (2002) showed there was no such smoking gun. Instead there were several differences which, one at a time, made a considerable but not striking difference. Considered together we could finally explain the difference. Nothing I have said suggests the current result (or indeed the Fan result) is wrong but the authors are best placed to understand what aspects of their inputs explain the large differences. It could well be the data alone. The quoted results of Basu et al. offer important corroboration. I think more exploration is required however.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 21829, 2014.