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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Quantifying the UK's Carbon Dioxide Flux: An atmospheric inverse modelling approach using a regional measurement network" *by* Emily D. White et al.

Anonymous Referee #1

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The manuscript "Quantifying the UK's Carbon Dioxide Flux: An atmospheric inverse modelling approach using a regional measurement network" by E. White et al. presents an estimation of the UK's net CO2 fluxes over two years based on an atmospheric inverse modelling approach and a measurement network around and within the UK. They find that averaged over the two years the UK's annual net biosphere flux is close to zero, i.e. in balance, within the error bars.

The research in itself, i.e. atmospheric inverse modelling, is not new, however the focus on national scale is somewhat new and has raised considerable interest in the recent past because of the growing importance of national greenhouse gas reporting.

Since the reporting is based on bottom-up methods, inverse modellings as a top-down approach can be considered is a tool to evaluate the reporting. Another interesting aspect of the paper is the sensitivity study with respect to the underlying prior flux field and the approach to solve for gross fluxes instead of the net flux in the inversion. So, overall the manuscript addresses an important issue in the field of carbon cycle research linking to atmospheric measurements.

The manuscript is well written and structured and mostly easy to read and follow. There are no major revisions needed, however, a few minor points need to be addressed before the manuscript can be published.

A main point of critique is a missing validation or at least evaluation of the inversion results and posterior fluxes. This is of course not an easy task but at least some basic evaluation tests should have been performed. This could be done by comparing modelled CO2 vertical profiles using the posterior fluxes against aircraft measurements or, if not available, ground-based observations withheld from the inversion. Also, the resolution of the posterior fluxes might already be high enough to compare them directly with eddy covariance based observations. Such an evaluation is missing completely. It is therefore not clear how 'trustworthy' the posterior fluxes are and, also, which one of the two inversions based on different priors performs better than the other. This is a crucial point currently missing in the manuscript and should be added before publication.

Some additional points: L 28: Spell out negative.

L 43: Are flux measurements really localized down to centimetres? Probably not.

L 46: What do you mean by 'are driven by observational data to varying degree of detail'?

L 55/56: Indeed, inversions are a valuable tool, but they are also not free from errors. It would be good to mention here sources for uncertainties in inversions and put these into perspective.

L 62: This is of course not true: Using these measurements in an inversion framework is not an independent way of providing estimates GHG emissions because the inverse modelling system requires prior emission fields as an input. Hence it is not independent of bottom-up inventories. This sentence needs to be rephrased in the manuscript.

L 74: Why is it rarely the case that model uncertainties are well characterized? This is also related to the comment on L 55/56.

L 80/81: But using Gaussian PDFs is only a choice made by the user, there is no mathematical need for Gaussian PDFs, one can use any PDF to describe prior knowledge. So why is that a problem here?

L 83/84: Why is the size of the diurnal cycle a problem and how does it matter if you solve for monthly fluxes?

L 156/157: Shouldn't this 'surface-exchange' height be dependent of actual meteorological conditions and vary for instance with boundary layer height or the strength of vertical mixing?

Sec 2.2.2: Have you done some sensitivity tests on how to handle the boundary conditions? It would be interesting to see how the results change if you don't include the boundary conditions in the control vector. There is of course a trade off between getting the boundary conditions right and using as much of the observational information as possible to constrain the surface fluxes. In principle, the boundary conditions are nuisance variables, which obviously influence your results but are in themselves not very interesting.

L 235 and Sections 2.3.2 and 2.3.3: The wording is maybe a bit misleading here. First you say, that ocean and anthropogenic fluxes are subtracted, and thus treated as perfectly known. Then you explain that these are prior fluxes. Usually a prior flux is a flux that gets updated through the inversion yielding a posterior flux. But you do not do

that here. I suggest to reword Sections 2.3.2 and 2.3.3 and not use the word 'prior' for ocean and anthropogenic fluxes. Also, I wonder how well the ocean fluxes are really know in that area, especially if you take the Takahashi fluxes (representing open ocean fluxes) as an estimate of the UK coastal ocean fluxes!

Ll 293-295: Does that mean that only MODIS LAI is assimilated? That also means that you are assimilating model output (since MODIS LAI is not a measured or even observed quantity).

L 303: It seems that both biosphere models use MODIS LAI data in some way. How independent are then the estimates of JULES and CARDAMOM?

L 364: How did you determine the length of the burn-in period and does the number of iterations include the burn-in period?

Sec 2.4.3 and Eq (7): What is x and y here? How many basisfunctions do you have in total and how does the Jacobian look like? Maybe you can add an equation for the Jacobian: H= del . . ./ del . . .

L 393: The word 'tested' is not correct here, 'applied' would fit better. In any case it would be good to add a few sentences on testing you set up in e.g. an identical twin experiment.

L 414: How can soil and litter carbon stocks be fixed in JULES? I wonder a model with fixed carbon stocks can provide decent estimates of the actual respiration fluxes.

Section 3.1: This goes back to my main comment on evaluating the inversions. Can you say which result is more realistic?

L 480: Do you mean 'underestimating' the net summer flux compared to the true flux? And if so, how do you know the true flux?

L 516: Do you mean here the posterior fluxes from the inversions or the prior fluxes from the two different models? Maybe stick to a common notation/terminology for the fluxes, e.g. prior fluxes and posterior fluxes throughout the manuscript and not refer to them just by model name.

Sec 3.4: This section presents some posterior diagnostics of the inversions and presents a first step towards an evaluation of the inversions. What do the different fits to the data mean for the inversions and posterior fluxes?

Ll 588-590: Agricultural activities should somehow (implicitly) be accounted for by the biosphere models through the use of MODIS LAI, which should capture events such as harvest in the LAI.

Ll 622-626: Again, this hypothesis means that you trust your inversion results but it is not clear on which basis you trust the inversions. This hypothesis should be supported by a more substantial evaluation of the inversions.

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