

Interactive comment on “Constraining terrestrial ecosystem CO₂ fluxes by integrating models of biogeochemistry and atmospheric transport and data of surface carbon fluxes and atmospheric CO₂ concentrations” by Q. Zhu et al.

Anonymous Referee #2

Received and published: 3 October 2014

The paper presents a study attempting to join the bottom-up and top-down approaches to gain insight on the sub-continental-scale carbon balance for the year 2003. Flux measurements are used to constrain the parameters of a biosphere model (TEM) which is then used as a prior in a top-down flux inversion using GEOS-Chem.

In general the paper is clearly written and laid out, although it would benefit from a good proof-reading. (I marked down some minor corrections below, but it's by no means a comprehensive list.) After carefully reviewing the paper I still have several outstanding

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methodological questions which need to be clarified. For instance:

-Why was AIRS chosen, rather than a NIR sensor (GOSAT, SCIAMACHY) with greater sensitivity to the lower troposphere?

-What measurement and model/representativity uncertainties were assumed for the GLOBALVIEW and AIRS measurements used to constrain the fluxes?

-Are the prior and posterior fluxes only at monthly time scales? If so, why, and what effect might this have on the evaluation in concentration space?

-What timestep was the model running at?

-Why only 2003? This makes it difficult to compare to other flux estimates, in many cases, as the interannual variability can be quite substantial. Also, was there a spin-up or spin-down time with the atmospheric inversion? Or is this not an issue with the specific 4D-Var method used here? I would strongly recommend expanding the analysis to a longer time period, even just a few years, in order to provide a better basis for analysis and comparison. Data availability should not be a limiting factor.

The approach holds a great deal of promise, but this attempt is rather heavily weighted towards the bottom-up side of things. The analysis does little to inform me of the added value of using the surface stations and satellite measurements. For instance, in Figure 7, how different does this plot look when performing a forward simulation of the TEM and CASA fluxes? Perhaps it would be worthwhile to compare inversions using only the surface network with those using AIRS as well, to see what value the mid-tropospheric information brings to surface processes. Adding a few bars to Figure 5 would be a start, to better visualize the shift in fluxes brought in by the atmospheric data. Particularly for AIRS, it would be useful to see a plot (maybe histogram)? of residuals and bias for the modelled columns based on the prior and posterior fluxes, to get an idea of how much the inversion was able to fit the data.

If the section discussing the assimilation of atmospheric measurements is appropri-

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ately expanded and the presentation of the results is improved, then the paper might be considered for publication in ACP.

Minor comments and some corrections/typos:

p22591, line 12: to -> for

p22591, line 24-25: maybe reword, as "...is that the currently available CO₂ concentration data are insufficient." (Rather than implying that none of the measurements are useful.)

p22591, line 27: lands -> land, "its CO₂ network" -> "the CO₂ measurement network" (The CO₂ measurement network was not unique to the TransCom study.)

p22591, line 28-29: The flux uncertainties are lower over the ocean than over land largely because of less variability in the ocean fluxes (on the timescales considered), not because the surface-based measurement network measures atmospheric mixing ratios over the ocean fluxes so much better, as is implied here. Please clarify.

p22592, lines 23-26: This upscaling has been done, and appropriate references should be added (such as the work of Martin Jung and colleagues). Several other publications have used flux tower data to tune parameters in biosphere models (such as the diagnostic model VPRM, see Mahadevan et al., 2008). This discussion should be expanded to reflect the current state of the art.

p22593, line 6: a-> the

p22598, line 1: purpose->purposes

p22599, line 7: CONTRIAL->CONTRAIL

p22600, line 13: insert "and" before "INTEX-NA"

p22600, line 20: layers-> layer

p22600, line 21: height->heights

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p22601, line 14: parameters -> parameter

p22601, line 18: with -> as

Figure 3: A bit confusing to me. The values are normalized, but then why are they centered at zero and not at one? I think it's just a simple offset, but then the prior error bars of 40% of the value (which are plotted as 0.4, or 40% of 1) make one think of 40% of zero, and the same happens when comparing the optimized values. For instance, for tropical forests, the optimized values of CMAX and KI are ~0.6 and ~-0.6 respectively. Does this mean that they are 60% larger or smaller than the normalized (to one) value? To me at least, this would be more clear if the priors were centered at one.

p22603, line 25: change semi-colon to comma

p22604, line 9: "types of plant function" -> "plant functional type"

p22608, line 5: he->the

p22608, line 11-14: Given the uncertainty on the two priors, was it even possible for the posterior fluxes to agree?

p22608, line 16-19: Here you're adding the land-use change flux to surface fluxes constrained by atmospheric measurements to argue that there's good agreement. But in both cases (Peylin et al., 2013 and the current study) the atmospheric measurements have "seen" the land-use change signal, and this should be reflected (however erroneously) in the optimized fluxes, provided the prior uncertainty isn't too tight. Or am I missing something?

p22608, line 21: add "the" before "Integrated ..."

p22608, line 22: posteriors -> posterior

p22609, line 3: source-> sink

p22609, line 18: Stephen et al. (2008) -> Stephens et al. (2007)

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p22609, lines 19-22: Again, why do you think that your *inversion* (and thus the atmospheric measurements) can't see fluxes related to land-use and land-cover change? I can understand that TEM might not be able to reproduce this, but the flux inversion is another matter.

Figure 7: Why compare only monthly data instead of the full (albeit limited) resolution of the GlobalView data?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 22587, 2014.

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