

Interactive comment on “Nested atmospheric inversion for the terrestrial carbon sources and sinks in China” by F. Jiang et al.

Anonymous Referee #2

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This paper uses a modified regional mask and TransCom L3 inversion method for estimating CO₂ fluxes at regional scales. TM5 transport model is used for simulating interannually varying response functions for the period 2000–2009. The CO₂ fluxes and flux anomalies over China are given special attention, which is long overdue. However, the results are not so well presented. I have a strong feeling that the uncertainties in the inversion system is sometime overrated or underrated without sufficient rationale/explanations. Thus the results and discussions have minimal impact on our understanding of the CO₂ sources and sinks over China. I cannot recommend publication of this paper. Please find below detailed issues and concerns.

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Line 14–15: The analyses of carbon flux variability with climate change/variability have
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been a matter of intense study in the past decade. I strongly urge the authors to review earlier publications.

line 18: I understand there are large number of data collected by various organisations in china. Have you look for possibilities for collaboration with the observational groups?

Line 23–25: Mistake in units? And I am sure this reference is wrong for 10 PgC/yr fossil emission. May be you should cite CDIAC. Also for the next sentence.

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Line 11–12: Please follow sign convention carefully. Are both these values sinks? Note different signs.

line 15–20: Cannot see the links for these climate model simulations here for this paper

line 26–27: Interesting writing style! how these two error can be compared. Are we talking apple and oranges?

line 29: Did Denning et al. do an inversion?

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line 1: Should give some credits to the CSIRO scientists. Apparently you are using the code written by Enting, Rayner, Law group. (eventhough I find reference to their work later on)

line 13, title and elsewhere: This cannot be called a nested method, here or in rest of the paper. Please discuss otherwise making your point clear.

page 1181 —

Texts around line 15 : What is the justification of this fine region division? In the abstract you mention that there are very few or no data from China

page 1182 —

line 22-23: Please cite original source for fossil fuel emissions or give details how are the distributions made or the global totals scaled?

page 1183 ———

Please comment about the performance of PISCES fluxes compared to Takahashi ocean. Since you are giving very small a priori uncertainties you should be careful choose your prior fluxes.

page 1184 ———

line 1-3: change '...considered as 1 site...' to '...considered as independent site...' How did you account for the differences in calibration scales for the measurements from different institutes? I presume WDCGG archive data as supplied by the observational groups, without any correction.

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line 1: Did you use all hour data from the continuous sites or day/night only data are selected?

line 9-12: You can find papers focussing on carbon balance of south/southeast asia using inverse modelling. And why 'may be', can you not check the posteriori uncertainties from your inversion for the south, southeast and all other regions? A check on the uncertainty reduction is a must before interpreting the flux results in any case.

line 14-15: Why are the uncertainty for this inversion 1.48 is so different from that for Deng and Chen (0.49)? Apparently your prior uncertainties are very similar!

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line 5 and thereabout: You should consider looking through earlier papers and references therein for IAVs in CO₂ fluxes for different parts of the globe in relation with climate variations. In addition you might be aware that Gurney et al. used TransCom L3 regional basis functions, which do not include IAV in meteorology as well as fossil

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fuel emissions explicitly.

line 15 onwards: Please add quantitative explanations here. If the inversion setups are different, you need to specifically state how are they different, e.g., a priori fluxes, uncertainties, etc. in quantitative manner. Too many use of 'may be' is not good.

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para 1: Again please check the signs before the numbers. Given the uncertainties, are these numbers different? Is this consistent with your conclusions at the end of the abstract. The discussions here leave me with a feeling that your inversion is working perfectly.

para 2: So given the sparse measurement network and short lifetime of BVOCs, we will not be able to track the BVOC oxidation. Then it is perfectly fine to budget BVOCs as CO₂, in my opinion.

However, you can prepare a table by splitting the CO₂ sources and sinks budgets over China by accounting all the flux components and non-CO₂ species oxidation.

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first para: How good are these uncertainty estimates (too conservative, I feel), e.g., the national/regional CO₂ flux uncertainties for USA or Europe are of the order of 0.3-0.4 PgC/yr at best. Do these uncertainties include interannual variations?

para 2: These numbers are unbelievable given the data network used for this inversion. I do not think we know such details for USA or Europe with that kind of dense network of surface, tower and aircraft observational network.

last para: Again I would recommend you to read earlier papers on the role of biomass burning and climate anomalies for land-atmosphere carbon fluxes. China certainly behaves different than the Southeast Asia, for example, in response to ENSO cycle; what about the affect of NAO or AO cycles on climate over China?

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The results of correlation shown in Figure 5 aren't conclusive

Figure 6 should be replotted showing same regional mask for both temperature and CO2 flux

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