

**Interactive comment on “Improved simulation of
group averaged
CO₂ surface concentrations using GEOS –
Chem and fluxes from VEGAS” by Z. H. Chen et al.**

Anonymous Referee #2

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In this paper, the authors demonstrate improved atmospheric CO₂ concentrations in certain regions due to new land surface fluxes in the GEOS-Chem transport model. Three model results are shown: GEOS-Chem with original emission inventories and CASA (ORI), GEOS-Chem with new emission inventories and CASA, and with new emission inventories and Vegas. The use of the Vegas model results in improved atmospheric CO₂ concentrations in several regions, compared to regionally-averaged observed CO₂ concentrations. The result is larger net CO₂ emissions (4.5 PgC/yr in Vegas compared to 4.1 PgC/yr in CASA) due to less uptake by the land. The paper also demonstrates the usefulness of grouping atmospheric CO₂ observations for model evaluation. The model with the Vegas land surface produces smaller RMSD between

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modeled and observed CO₂ concentrations, due to large and important differences in the land fluxes compared to CASA.

Specific Comments My main comment is that after reading the paper, I was unsure of the point. A majority of the paper focuses on division of observations into certain regions, and describing the observed CO₂ seasonal cycle in these regions. This detracts from what I think the main idea of the paper actually is: That using a new set of CO₂ emissions and a new land surface model results in improved atmospheric CO₂ and enhanced carbon emissions from land. Less time should be spent on describing seasonal cycles in every region, and more focus should be put on when, where, and why the new model produces different and improved results. In particular, revisions of the Introduction and Conclusion should make the overall picture of the paper more clear.

Some other general comments for the paper: 1) In regards to nomenclature for land carbon fluxes, take care to be consistent, and be clear about the sign convention. Define NEP, NBP etc.

2) I think the Introduction needs to end with a better explanation of what is being done in the study. Things that need to be included are:

- A summary of the models compared, and justification for replacing CASA with Vegas. Is it because of the inclusion of biomass burning in Vegas? What is the expected impact of including this (for example what are the estimates for CO₂ lost to atmosphere during biomass burning)?

- In Section 4 you explain the fluxes of CO₂ to the atmosphere, I think these should be introduced sooner for the benefit of readers not familiar with CO₂ inversion studies. Then you can also explain that in the Vegas experiment, the NEP flux into GEOS-Chem is changed from CASA to Vegas.

3) The methodology is not 100% clear (at least not the motivation behind the modeling

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methods). For example, Section 2.2 should start with a clear description of what the two land surface models represent. Explain right away that CASA simulates NEP as GPP minus ecosystem respiration, while Vegas simulates NBP, which is the NEP minus CO₂ lost from biomass burning. Then it should also be explained if there are other differences in the models that will affect the results: ie how do they calculate photosynthesis and respiration differently? Also I think an explanation of the big picture would be helpful. The GEOS-Chem transport model requires net fluxes of CO₂ from the land in order to predict global atmospheric CO₂ concentrations. It usually uses NEP from CASA but now you are using NBP from Vegas instead. Finally, I think the Appendix should be in this section. It is very relevant to the model differences and the results of the overall simulations.

4) I think that the grouping of CO₂ observational sites is best described as “regionally averaged”, rather than “group averaged”. What is different in these divisions as compared to the TransCom study? In the abstract and Section 3, you refer to grouping based on atmospheric mixing regimes, but I think this is a misuse of the term. To me this refers to stability of the atmosphere. It would be better to say “seasonal cycles” or just “seasonality”.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/13/C655/2013/acpd-13-C655-2013-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 2243, 2013.