Atmos. Chem. Phys. Discuss., 13, C655–C657, 2013
www.atmos-chem-phys-discuss.net/13/C655/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on “Improved simulation of group averaged **CO**$_2 surfaceconcentrationsusingGEOS-ChemandfluxesfromVEGAS''byZ.\ H.\ Chenet\ al.$

Anonymous Referee #2

Received and published: 18 March 2013

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 modeled and observed CO_2 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 CO_2 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 CO_2 emissions and a new land surface model results in improved atmospheric CO_2 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 CO_2 lost to atmosphere during biomass burning)?

- In Section 4 you explain the fluxes of CO_2 to the atmosphere, I think these should be introduced sooner for the benefit of readers not familiar with CO_2 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

ACPD

13, C655–C657, 2013

Interactive Comment

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.

ACPD

13, C655–C657, 2013

Interactive Comment