General Comments

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. This paper presents interesting and important results but has several problems. I think the study should be published but there is significant work that needs to be done first.

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 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. I think it would be better to say "seasonal cycles" or just "seasonality".

Introduction
Page 2245:
Line 4-5: Remove "Piao's results show that"
Lien 6: Larger than what?
Line 8: Second half of sentence should read: "sinks in every part of the globe." Also might be good to reference satellite data, which does not tell us all the sources and sinks but do give a good idea of distributions of CO2.
Line 13-21: The second half of this paragraph (starting with "The mean annual meridional/") is unclear. What exactly was learned in these studies that is relevant to the current work?

Page 2246:
Line 4-6: This is true but does not explain what can be learned from comparing the concentrations in ocean regions since as the authors point out later in the paper, neighboring land often influences the CO2 concentration above oceans.
Line 8-9: Sentence beginning "It is significant for using" is confusing.
Line 9: What about fluxes from land cover and land use change?
Line 16: Begin sentence with "Nassar et al. (2010) …" and then remove reference at the end of the sentence. Also, this sentence explains the differences between the "original" and "new" CASA-based fluxes in Figure 1, but it is easy to forget the reason for these differences as the reader goes through the paper. A reminder to the reader in the results section would be helpful: Reiterate that the difference between the new CASA and original results is the inclusion of updated fossil fuel inventory, shipping and aviation emissions, and atmospheric production of CO2. Finally, I had to read the abstract of Nassar et al. to understand that CO2 shipping is really emissions of CO2 from ships.
Line 20-26: What is meant by balanced? Also define NEP.
Lines 23-26: The models used in this study need to be better explained. I have several questions:
- What is "it"? (It is available for the simulation of global CO2 concentration …")
- Is the DGVM you reference Vegas?

- I don't think the last sentence really adds much to the discussion.
- If you have replaced on non-DGVM with a DGVM, does this mean you are now predicting vegetation coverage? Or is there a prescribed land surface map for each model? Are these the same?
-Why was CASA replaced with Vegas?
- I don't know what you mean by "All DGVMs are consistent with the global carbon land budget"

Page 2247:
2 Data
Line 10: I think this section should begin with the description of what GLOBALVIEW-CO2 is, rather than where it comes from. How many stations are in the dataset? It's not a globally gridded product, is it?
Line 11: Remove "While"
Line 21-23: I don't know what this means.

Page 2248:
2.2 Vegas data
Line 1: This section is really about the land carbon cycle models/fluxes, not just Vegas. Maybe rename it "Modeling the land carbon fluxes"
Line 1-2: Not just DGVMs simulate NEE, any land surface model will do this. Is CASA a DGVM?
Lines 3-4: Are the sink numbers from Vegas? CASA? Also which DGVMs simulate a greater uptake?
Line 8: What is the source for the driving meteorology? Are there other drivers needed to run Vegas (like shortwave/longwave radiation, winds, humidity)?
Line 9: Define NBP? Again, be consistent and clear with nomenclature and sign conventions.
Line 12: What does CASA stand for?
Line 14: Here is a good place to say that: 1) A positive flux indicates a flux of CO_2 from the land to atmosphere and negative is uptake by the land; and 2) The CASA flux is always higher than the Original flux because of the added emissions from Nasser et al. (2010).
Line 15: Delete "It is evident that". Do you mean temperate North America instead of boreal South America? Are all of the model differences attributable to the fact that Vegas includes biomass burning?
Line 18-19: Would be easier to say the difference is distributed over tropical land.

Page 2249:
Section 3.1
Consider renaming Section 3 "Regional CO_2 observations", which would also mean changing the title of the paper.
This section could be made more clear. Basically, you have used 108 sites that are broken

into 11 land groups and 15 ocean groups based on the regions in the TransCom experiments (why do you have 4 more ocean groups than TransCom?). 72 sites are in the ocean, and 36 are on land. It would be helpful to explain this clearly upfront, before getting into the details of how you chose the regions etc. Also in general the description of the ocean grouping is very hard to follow.

Line 4: Delete "regional characteristics of"
Line 5, 6:
Lines 12-14: What do you mean by "except the boundary of two land regions"? And the latitude definition is confusing.

Section 3.2

General comment: It would be more helpful to give the names of the regions in the discussion rather than just the numbers.
Line 25: Remove "we can show that" and "from Figure 5". Also mention that the annual mean has been removed.

Page 2250
Line 6-9: Can you give an example of where these comparisons can help with the understanding of surface fluxes?

Section 3.3

General: The last paragraph on this page seems to be more of an overview, while the previous paragraphs in the section were more specific. Consider rearranging this section to have this paragraph first.
Lines 23-24: What is meant by "next to each other"? The meaning of this sentence is unclear.

Page 2251:
Are regions O4 and O5 dominated by air-sea exchange, since they are so far from land?
Line 3: By special trend do you mean that they have two minima? It would be better to be specific about what is special/unique about the trend.
Line 10: I think you mean "north of 35S"
Line 12: delete "totally"
Lines 15-16: It looks like O10 and O11 do show a very weak seasonal cycle.
Lines 21-22: I think the word 'variations' is too vague – do you mean the seasonal anomalies of CO2?
Line 23: remove "such as O15" since this is the only group south of 35S anyway.
Line 25-end: The discussion of the Southern Ocean is confusing, please be more specific.

Page 2252:

Section 4

General comment: the first paragraph is a good overview of the study but I have a few questions/comments:
- Reiterate the difference between the original and new inventories. Then you can move

the sentence on Line 12 ("A detailed description …") to Line 7-8.
- What is the residual annual terrestrial exchange? Is this the extra land flux needed to get the correct atmospheric [CO2] in GEOS-Chem?
- How is the ocean flux determined?
- Line 13: Which variable is spun-up?
- When the model is compared to a region with just one observation station I do not think broad conclusions can be made about the sources/sinks of CO2 over the entire region. The results in this section will be more robust if the model comparison focuses on regions with multiple stations.

Page 2253:
Section 4.1
Line 4-5: Another possibility is that the sources are too high.
Line 6-13: This paragraph does not read well, I suggest rewording/rearranging some of the sentences that describe the model-obs differences.

Section 4.2
General: At the end of this section, it is not clear what has been learned (in the big picture sense) from the ocean inventories. Do they elucidate any specifics about land surface processes? If so this should be more specifically addressed.
Line 20: Name the region with the largest discrepancy.
Line 23: But there is only 1 point in O11 and it is right next to South Africa, and the stations in O5 are very far from S. America. Given the predominant wind directions (westerly) in these regions are there other explanations for the improvements?

Page 2254:
Line 1: Why the simulated April minimum?
Lines 2-3: Reword to "The observed seasonal trends are very complex in the South Indian Temperate ocean region."
Lines 6-7: Isn't it probable that the bias is due to the high bias of CO2 over the NA land?
Section 4.3
Line 22: remove "It is clear that"

Figures
Figure 2-3: Are these Vegas fluxes minus CASA? It's not clear from the caption. Also switch 2 and 3 since you discuss the differences in July first.
Figures 4-7: Give region names in the caption or on the figures themselves.
Figures 7-8: Show the spread from the observations. For example, L11 has a large model-obs mismatch but also a large spread in the observations, so it's hard to tell how far off the model really is.
Figure 9: Add a legend.