

**SC #1 Comments:  
June 23, 2011**

**Thank you for the comments on our manuscript, please find our responses in bold font below.**

**Ben Poulter**

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This paper by Poulter et al. quantifies the influence of different climate and land cover data sets on the carbon balance and CO<sub>2</sub> airborne fraction as modelled by a global vegetation model. The authors used 4 different remotely sensed land cover maps and three different climate datasets. The land cover datasets were translated to plant functional types using the CRU climatology and integrated into the LPJ dynamic global vegetation model. Carbon fluxes were simulated based on the different combinations of input data and compared with a standard model run with dynamic vegetation and a reference dataset. The results show that the climate datasets have a larger impact on carbon fluxes than the land cover maps. All different model simulations show an increasing trend in the NEE residuals after 1998, which could be an indicator for an inappropriate climate sensitivity of the model. The selection of the input data affects also the airborne fraction. This analysis is in general a good idea because differences in land cover and climate data sets are known but their impact on bottom-up carbon flux estimates not investigated in detail. The impact of different model structures, land cover and climate data on gross primary production in Europe was already investigated by Jung et al. (2007). Although this manuscript by Poulter et al. is based on one model (LPJ) only, it is innovative because it considers also heterotrophic respiration and net ecosystem exchange and it is applied globally. It also tries to assess the impact on the CO<sub>2</sub> airborne fraction. Because of these innovations the paper should be published after revisions.

**We appreciate this summary of our manuscript. One minor point, as we clarify below (& update in the text of the manuscript), the land cover datasets were not developed with CRU climate data, but with the Global Historical Climatological Network v2.0.**

The use of a dynamic global vegetation model (DGVM) is slightly unfavourable for the aim of this study. Because DGVMs normally don't use land cover maps as input, it should be explained why a DGVM was used to assess the impact of land cover maps on carbon dynamic estimations. For the aim of this study the usage of a model with prescribed vegetation would be easier. Thus, the study is partly an evaluation of the carbon balance as caused by the dynamic vegetation in LPJ in comparison to observed PFT distributions. But nevertheless, such an analysis with a DGVM is of course allowed and ambitious. The title of the paper implies more general results about data differences on carbon balance estimations, but it is a case study with one specific model. At the end it remains unclear if the results have more general implications or if they are specific for the used model. These questions should be discussed more in detail.

**We have clarified in the text why it is important to evaluate the uncertainty from PFT distributions using a DGVM model. While DGVM models were developed explicitly to account for non-equilibrium vegetation dynamics, it is not that uncommon for DGVM models to have a static / prescribed vegetation mode, see ORCHIDEE DGVM (Krinner et al., 2005; Jung et al., 2007) or SHEFFIELD DGVM (Quaife et al., 2008). We have developed a static vegetation mode for LPJ for this study, which has been used in static vegetation mode in other studies (Beer et al., 2003; Jung et al., 2007).**

One important preparatory work for this analysis is the translation of different remotely sensed land cover maps to plant functional types (PFT). For this work a manuscript currently under review in another journal (Poulter et al. 2011) is cited. Thus, this important step is at the moment not clear for the reader and should be explained with some more details. Could it be possible that the model results driven by CRU climate perform better because the PFT datasets are also created based on CRU?

**We have now published the manuscript detailing the development of the PFT datasets in Biogeosciences Discussions (Poulter et al., Submitted). The manuscript details the process of combining the land cover datasets with biome types - and provides an ftp site for downloading the PFT data. The climate data used to develop the PFT datasets were based on the Global Historical Climatological Network v2.0 and so were independent of CRU.**

Page 1621, line 3-5: “We modified a coupled biogeography-biogeochemistry model, the LPJ DGVM (Sitch et al., 2003), which includes updated hydrology and land-use schemes (Gerten et al., 2004; Bondeau et al., 2007)” Because of these references, one could assume that probably LPmL (managed lands) was used in this study. Please indicate the proper name and version of the model.

**This update has been made.**

Page 1621, line 12-16: It remains unclear how “PFT fractions were prescribed directly to the maximum annual FPAR variable in LPJ” and how “PFT-specific bioclimatic thresholds [ . . . ] were modified”. The authors should give more details about this prescription of the land cover because this is a key step in this study with a DGVM.

**This information is detailed in the manuscript describing the development of the PFT datasets (Poulter et al., Submitted). As stated in the manuscript, bioclimatic limits that determine a PFT range were ignored so that prescribed PFTs could establish anywhere - and the FPAR variable, which describes the fractional coverage of a PFT, was replaced with the prescribed PFT fraction. We clarify this in the revised text.**

Page 1622, line 2: “Fire dynamics were implemented for natural PFTs”: What happened in the model with prescribed vegetation after fire was combusting vegetation?

Was a regrowth of vegetation possible or was the vegetation after a fire the same like before? Please add some more information about post-fire vegetation changes and if they were considered in the prescribed model. What is the effect of an implemented or the non-implemented, respectively, post-fire vegetation change on post-fire NPP and how does it influence the overall carbon dynamics?

**We clarify in the text that fire had the effect of reducing the number of individuals in a population and that the fire module remained unchanged. Regrowth also remained unchanged, i.e., dynamic, but the FPAR remained fixed to the prescribed PFT fractions. Following fire, FPAR would remain unchanged and NPP unaffected, but the number of individuals, which determines the scaling of NPP, and eventually, heterotrophic respiration, would be reduced.**

Page 1623, line 28 – page 1624, line 2: This description of the reference data set should be in the “Data and methods” section.

**The estimation of the residuals from the carbon budget accounting is intended to be considered in an ad hoc manner relative to the model and data set up and quantification of uncertainty.**

The paper wants to address impacts of different data on the CO<sub>2</sub> airborne fraction but this is given far too little attention in the text and figures (see chapter 3.3). In the text only residual errors of the simulated NEE to the reference data set are mentioned. The airborne fraction should be really calculated, visualized and discussed from the reference data set and the model simulations. These analyses are expected from the paper title.

**The aim of the paper is to quantify the contribution of terrestrial carbon cycle uncertainty and to illustrate that the uncertainty here propagates to closing the global carbon budget, with implications for the airborne fraction. We discuss the implications to the airborne fraction in a qualitative manner in order to emphasize the forcing uncertainty - more of this qualitative discussion is added to the text.**

The structure of the “Results and Discussions” section is not really logical. One can expect from the paper title the following order: first a discussion of the differences in the climate and land cover data sets, second a discussion of the impact on the carbon dynamics and thirdly a discussion of the airborne fraction. The differences between the land cover datasets are not shown. Additionally, the results are ending with “Evaluation of forcing data” (page 1626, line 19), which seems more like a model evaluation. To improve the compelling nature of the paper, I would suggest the following structure in the “Results and Discussions” section:

3.1 Comparison of forcing data

3.1.1 Climate data (including Fig. 1)

3.1.2 Land cover data

3.2 Impacts on carbon dynamics (including Tab. 3, 4, 5 and Fig. 2, 3, 5)

3.3 Impacts on CO<sub>2</sub> airborne fraction

**Because the Results and Discussions section mostly follows this recommended structure, we have left Sections 3.1 and 3.2 as is. But to try to clarify the logical sequence, as recommended, we have modified the subheading of Section 3.3 to clarify its intended description on the role of the forcing dataset combinations in reducing residual error on the global carbon budget.**

Minor remarks

The control model run with dynamic vegetation should be named the same. Now the words “Hyde”, “Dynamic”, and “Dynamic vegetation” are used. Please use the same name for it in the text, in Tab. 3, 5 and in Fig. 3, 5.

**We have updated the labeling in the Tables 3/5 and Figure 3/5**

Page 1619, line 11: One point (.) too much.

**Made change in revised text.**

Tab. 3: A row with the carbon fluxes from the reference data set would be helpful.

**The first row of each climate forcing refers to the dynamic vegetation and Hyde pasture land cover forcing - this model run refers to our reference case. In the text, we refer to independent estimates of component fluxes and net ecosystem exchange from a range of studies, which can also be considered reference datasets, but inconsistent with the inclusion and content in Table 3.**

Tab. 5: An additional row with the mean sensitivity (averaged over land cover data) and a column with the mean over climate data sets for a better illustration of the overall impact of climate or land cover data could be helpful.

**This partitioning is described in the text Section 3.2.**

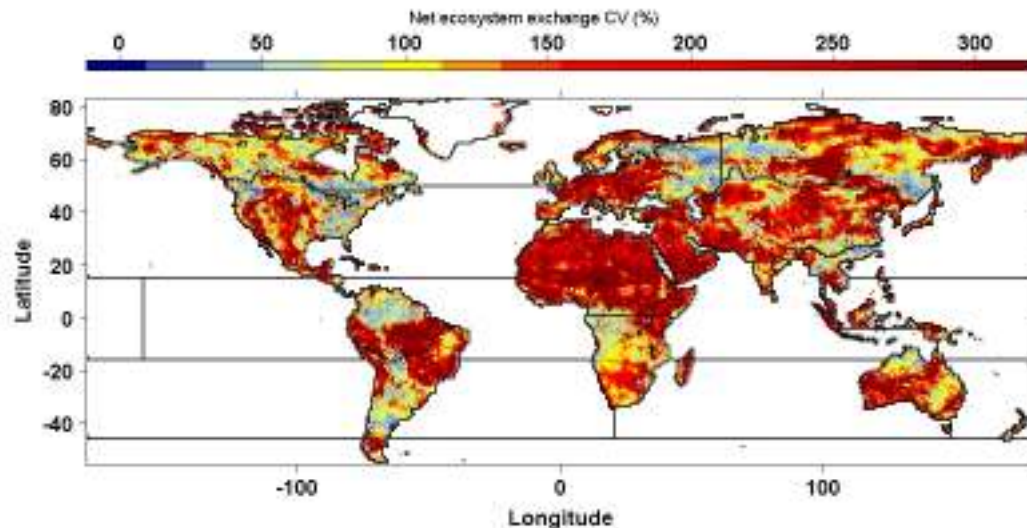
Fig. 1: Figure is hard to read and the regional borders are confusing in these small figures.

**We have updated the clarity of the Figures for the final version.**

Fig. 2: Because the standard deviation depends on the absolute values (as mentioned on page 1623, line 22), it could be better to use the coefficient of variation in this map to show the deviations in NEE between the different data sets.

**We intentionally show the uncertainty of NEE in terms of absolute values, rather than CV, because this statistic helps identify where improvements in forcing agreement could have the highest reduction in carbon flux disagreement. Plots of the coefficient of variation highlight less productive regions, as shown in the figure below:**

**Figure 1: Coefficient of variation for net ecosystem exchange: standard deviation of NEE divided by the mean of NEE multiplied by 100, for all 15 forcing combinations.**



## References

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- Krinner, G., Viovy, N., de Noblet-Ducoudré, N., Ogeé, J., Polcher, J., Friedlingstein, P., Ciais, P., Sitch, S., and Prentice, I. C.: A dynamic global vegetation model for studies of the coupled atmosphere-biosphere system, *Global Biogeochemical Cycles*, 19, GB1015, doi:10.1029/2003GB002199, 2005.
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- Quaife, T., Quegan, S., Disney, M., Lewis, P., Lomas, M. R., and Woodward, F. I.: Impact of land cover uncertainties on estimates of biospheric carbon fluxes, *Global Biogeochemical Cycles*, 22, doi:10.1029/2007GB003097, 2008.