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## ***Interactive comment on “Impacts of land cover and climate data selection on understanding terrestrial carbon dynamics and the CO<sub>2</sub> airborne fraction” by B. Poulter et al.***

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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

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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.

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.

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

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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?

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.

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.

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?

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

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.

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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

#### 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.

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

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

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.

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

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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.

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