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Interactive comment on "An improved land biosphere module for use in reduced complexity Earth System Models with application to the last glacial termination" *by* Roland Eichinger et al.

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Reply to:
Interactive comment on "An improved land biosphere module for use in reduced complexity Earth System Models with application to the last glacial termination" by Roland Eichinger et al. from Anonymous Referee #2

Dear Anonymous Referee #2,

thank you for your comments and suggestions. Please find our answers (in blue) to your comments (in black) below:

General comments:

The authors present an improved land biosphere module which is then used in a reduced-complexity Earth system model to simulate the last glacial termination with a focus on carbon cycle changes. Although the processes causing the CO2 rise during the last glacial termination are far from being understood and contributions to increase this understanding are highly welcome, I have some major concerns about this paper.

I'm not very convinced by the structure of the paper. The paper focuses on the description of an improved land biosphere component on the one side and on the coupled Earth system model response during the last glacial termination on the other side. Much of the changes in the global carbon cycle in the model during the last termination are due to changes in the ocean carbon cycle resulting mostly from prescribed transition functions. It is therefore not very clear what the message of the paper is supposed to be. Focusing only on the carbon cycle changes driven by the land and how the improvements to the land model affect the simulated land carbon response during deglaciation would probably result in a more straightforward message being delivered to the readers.

In the revised version, the paper is being restructured to put the focus squarely on carbon cycle changes driven by the land and how land model improvements affect carbon cycling during deglaciation. We now start the results and the discussion from an additional simulation with constant land biosphere, followed by the old and the new biosphere model transition simulations and the discussion on permafrost impact. Thereafter, the evaluation of the impacts of individual transition functions for the transient simulation starts. This creates a shift of focus towards the role of the land biosphere on climate and carbon cycling and the discussion of ocean carbon cycling is now moved more into the background. Please also see also our answers to reviewer 1.

The authors claim that they have improved the land model but the improvements are discussed only in a very qualitative manner. Several quantities could be compared with observed or reconstructed (also model-based) values, e.g. permafrost area (both present day and LGM) and permafrost carbon content (present day), NPP (LGM).

We do in fact compare our calculations for land and permafrost carbon storage change between pre-industrial and LGM to other estimates (Sections 2.4 and 3.2). In the revised version we are extending these comparisons to present day and LGM carbon stocks as well. For example, we now state that our total LGM amount of carbon on land is about 2800 GtC (land biosphere plus permafrost), which is somewhat low compared to the Ciais et al. 2012 estimate of 3640±400GtC. We discuss how this can lead to an underestimation of the permafrost effect in the transition simulation and carry out an additional sensitivity simulation with a doubled concentration for permafrost carbon (also see our above response to K. Crichton on this).

A great advantage of a simple model over more complex models is the lower computational cost. This strength could be exploited to perform some parameter sensitivity analysis which would help to understand how robust the presented results are to changes in unconstrained parameter values. I'm a bit disappointed that this has not been done in the paper.

As discussed in our response to reviewer 1, we prefer to retain our values for central parameters like Q_{10} and f_{CO2} since the values we use have proven to give comparable land biosphere results to those of other models in recent intercomparison studies of

past and future warming and pCO_2 change. Also as discussed above we are now doing additional sensitivity studies, for example using a doubled concentration for permafrost carbon.

It is questionable if a progress in understanding the role of land carbon changes during glacial termination can be attained by using the extremely simplified model described in this study for several reasons:

1) In the model, permafrost carbon reacts instantaneously to changes in the snow-ice line. This seems a quite crude parameterization and neglects the long time scales associated with permafrost carbon dynamics. The assumption of a uniform permafrost carbon concentration of 30 kgC/m2 is not supported by observations which show large spatial variations in permafrost carbon over Siberia. At least a sensitivity analysis to this value would be appropriate.

For this see the detailed response to K. Crichton on DCESS model structure, very different time scales for uptake and release of permafrost carbon and consistency with the treatment of the land biosphere. Furthermore, as mentioned several times earlier, we will be carrying out an additional sensitivity simulation using a uniform permafrost carbon concentration of 60 kgC/m^2.

2) The Northern Hemisphere ice sheet extent at LGM is strongly dependent on longitude, with the Laurentide ice sheet over North America extending as far south as 50°, while Siberia was ice-free. The implications of this asymmetry, which can not be considered in a zonally averaged model, should at least be discussed in the paper.

In fact the Laurentide Ice Sheet extended as far south as 38°N during the LGM. To account for this and the lack of an ice sheet in large parts of Siberia and within the constraints of our zonally-averaged model, we prescribed the southernmost ice sheet extent to be 47° and used ice sheet reconstructions to estimate a time series of ice sheet extent retreat (see Section 3.1). We feel that this is a meaningful approach and the best that can be done to be consistent with the degrees of complexity in the other model modules. We will supply some more discussion on this in the revision.

3) How the polynomial relations for the latitude of the borders between vegetation zones are derived from Fig. 4 in Gerber et al. (2004) is not very clear. Since this is supposed to be a technical paper, some more details could be given. On what quantity is the separation between vegetation zones based? What is the justification for using a 5-th order polynomial? Also, what is the zone north of the snowline considered to be?

The first two of these questions are dealt with in detail in a response to reviewer 1, including new additions to the revised manuscript. In our simplified approach the zone poleward of the snowline (0°C) is taken to be permafrost. In our revised version we will mention this up front in Section 2.1 to avoid confusion.

For the LGM cooling relative to preindustrial the IPCC gives a very likely range of 4-7°C cooling, while a value 0f 3.5°C is used in the model based on Shakun et al. 2012. This is only one example where a sensitivity analysis would be appropriate. I would expect the choice of global temperature at LGM to have a large impact on the simulated land carbon storage at LGM.

Recent estimates based on much improved temperature data have shown LGM cooling of 3.2 - 4°C (Shakun et al. 2012; Schmittner et al., 2011, Science; Annan and Hargreaves, 2013, Climate of the Past). Earlier studies based on much less data found

considerably greater LGM global mean cooling (e. g. Schneider von Deimling et al., 2006; Geophysical Research Letters). Thus we feel that the choice of a 3.5°C LGM cooling is well motivated. Furthermore, our simulations use a modest climate sensitivity of 2.5°C based in part on the well-constrained LGM cooling we use (see Section S6 of our Supplement). Otherwise, our Fig. 1b provides a sensitivity analysis for our new land biosphere model like that proposed by the reviewer. The figure shows only a reduction in land biosphere carbon storage of about 50 GtC for a 5°C LGM cooling compared to a 3.5°C cooling. Furthermore, as shown in our Fig. 2, the model snowline does not extend equatorward of 47° for a 5°C cooling such that our prescribed ice sheet extent continues to form our equatorward permafrost extent. In the model this translates to no change in permafrost carbon storage for a 5°C LGM cooling compared to a 3.5°C cooling. Some of the above will be included in the revision.

Specific comments:

The last part of the last sentence on page 2 would fit into the abstract. I would suggest moving the discussion of Figure 1 (sentences on page 3, lines 25-27 and 31-34) to section 2.4.

These are good suggestions and are being adopted in the revision.

In the caption of Table 1, 'globally averaged for one hemisphere' should be replaced with e.g. 'integrated over one hemisphere'. (And why not give the global values instead of hemispheric values? That would make the values more easily interpretable.)

We also follow the reviewer's suggestion here and are providing global values in Table 1 in the revision. The table caption is being revised to read: Table 1. Pre-industrial distribution of carbon storage among model land carbon pools as well as model net primary production for the three vegetation zones (see Chapin et al., 2011, and citations therein).

Page 5, line 4: what do the authors mean by 'latitude of 0°C global mean temperature'?

In our model, a meridional temperature distribution in the atmosphere is obtained by fitting a second-order Lagrange polynomial to the atmospheric temperature of the low-mid latitude and high latitude boxes (see Shaffer et al, 2008). For clarification, in our revision we are modifying this text to read "During interglacials when ice sheets retreat poleward, the poleward boundary of this zone is taken to be the equatorward extent of permafrost. For simplicity we take this extent to be the latitude of our model equatorward snow cover extent, Lsnow , defined by the latitude at which global mean atmospheric temperature is 0°C in our zonally-averaged model".

In section 2.1, first the separation of vegetation zones should be described and only afterwards Table 1 should be discussed. The total area of each vegetation zone should also be given together with the values of biomass reservoirs and NPP in Table 1.

We are also adopting these reviewer suggestions in our revision.

Page 9, line 4 and 7: 'BF' -> 'EF'

Thanks, done.

Page 12, line 3: 'agree well WITH other estimates'.

This has now been corrected.

Page 12, lines 5: I can't see how the authors can say something about improvements in the 'timing' of carbon exchanges between land and atmosphere based on the results presented in the evaluation section

With the new vegetation scheme, a better quantitative description of the respective land biosphere pools is possible, including their individual reaction to temperature now calculated separately for each zone. As shown in Figure 3 for example, the TF zone vegetation reacts rapidly to temperature change since it is dominated by above-ground vegetation. Such differentiation was not possible with the old scheme and hence the timing of changes could not be represented as accurately as with the new scheme. We will try to make this clearer in the revised manuscript text.

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