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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.

Anonymous Referee #2

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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 termina-

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tion 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.

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).

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.

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.

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.

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?

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.

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.

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.)

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

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.

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

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

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.

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