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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 #1

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General comments

The paper presents a new terrestrial carbon cycle module within a the DCESS Earth System Model. Specifically, the authors expand the model, by accounting 3 vegetation zones, that can expand and contract, depending on global mean temperatures. The authors show, that the inclusion of these zones indeed creates a vastly different total vegetation carbon pool for glacial/interglacial transitions. They then couple the model with ocean and atmosphere content to evaluate the evolution of DELTA 14CO2 delta 13CO2, as well as CO2 concentration in the atmosphere during the last deglaciation.

I appreciate the work showing that indeed expanding and shrinking of areas of plant growth does have a significant effect and can indeed cause differences in the overall

response to global temperature change. However, the authors seem to be caught in a conundrum when applying their model to glacial-interglacial change. On the one hand, they try to discuss the degree with which the change in atmospheric carbon proxy can be reproduced by their model, but they have to deal with the result that these changes are mostly the imprint of ocean dynamics and ocean carbon cycle. As a result, there is much back and forth in the paper between discussing the terrestrial biosphere module and the entire model – leading to some confusion. Perhaps a way to remedy the whole thing is to organize the results from the DCESS without model improvement, talk about what how the transition is set up and carry out glacial-interglacial simulation in absence of a terrestrial module. Having this out of the way the focus can remain on the terrestrial system. Thus, first focus is glacial interglacial change with ocean/atmosphere boundary/initial conditions, and perhaps run a simulation without any vegetation change. In a next step one can then compare against this null model with the crude DCESS terrestrial module (no vegetation zones) against the improvement in the land model. The comparison may also not just discuss the outcome of carbon cycle and its impact on the prediction (and feedback) of temperature, but it could also include albedo effects due to the different biomes as well (and methane? - The authors mention also a wetland module at one point). The focus would then not be so much on the degree with which the DCESS model can reproduce CO2, but how the land parameterization affects DCESS dynamics.

It is important to note that the terrestrial biosphere likely gains carbon. Thus it works against the accumulation of CO2 in the atmosphere. In a way, your model improvement makes the situation worse. I think the author should point that out more clearly (despite the fact that the paleoclimate community is very well aware of). It also shows an important conundrum in modeling: Namely, even if you improve a model (and you know it), the outcome gets worse – which does not mean your model took a turn to the worse. In fact, such work help to foster continued model development.

Overall, I think the development and application of an improved terrestrial module in a reduced complexity Earth System Model is a worthy endeavor. These types of model can offer great insight since they can easily be modified and interpretation is much more straightforward. I am sure the presentation can be modified that the paper achieves this goal by focusing on how the modification affect the overall Earth system dynamics.

Specific comments: Is methane considered as radiative forcing (methane emissions from terrestrial systems are briefly mentioned)? Also, is there a specific climate sensitivity applied to the model?

One of the limitations of the Gerber et al., 2004 study was, that they did not incorporate ice sheet, nor did they calculate potential effects of a reduced sea level. In particular, the reduction of sea level caused an additional and significant storage of carbon because of the expansion of the land mass. This may be worthwile discussing – see e.g. Joos et al., 2004 for applications with LPJ – against which the comparison here has been made.

It seems to me central parameters to glacial/interglacial change are lambda_Q (the Q10 factor), and fCO2 (the CO2 fertilization factor). Would it be worthwile to test the sensitivity of these in the DCESS outcomes? Method section: Parts of it seems to be result: I believe the simplistic model behavior should be juxtaposed with the improvement in the model section. In particular figure 1 should not appear in the method section, but be actually part of the results.

Equations 1 and 2: This is a 5th degree polynomial, is there a justification to use 5 degrees, can the extent not adequately represented by 3 degrees? I think it may be important to keep the number of parameters low in a reduced complexity Earth system model.

Equations 14-18: It seems these equations do not balance the carbon flux e.g. 10/60 of the leaf loss is unaccounted for. Also I don't understand equation 18: the units seem to be off and I don't see where the 45/55 comes from: In my understanding equation

18 should be the sum of equations 14-17.

I have trouble understanding how you calculated permafrost release. There are 2 numbers, one considers a permafrost storage of 30 kg m-2, but what is the 0.33 kg m-2? And how is this number linked to the isotope ratio? This may be my limitation, but perhaps there are ways to clarify this.

Figure 6: It is not clear what the production rate is in the ALL_TF simulation (red line).

Discussion of transient simulations: A great deal of this discussion focuses on ocean carbon cycling, which is not surprising given that the ocean dominants the carbon cycle on this time scale. However, there is little support to the items raised in the paper. Where is it detailed out, how much each of the radiative forcing contributes to the temperature increase (dust etc.), and how this affect isotopic distributions. In some instances, it may be sufficient to point to the appropriate figure/text in the supplementary material, but perhaps it is also worthwile considering additional plots. And again, I suggest some restructuring to better set apart the overall mechanisms of glacial/interglacial carbon cycling from the discussion of the improved vegetation dynamics.

P9L23. Starting from "As is,. . ." until the end of paragraph, this seems to be misplaced.

P10L4: Please also state what the initial global temperature is (14 degree C?)

P2L12: Check abbreviation for extratropical forest

Reference Joos, F., S. Gerber, I. C. Prentice, B. L. Otto-Bliesner, and P. J. Valdes. 2004. Transient simulations of Holocene atmospheric carbon dioxide and terrestrial carbon since the Last Glacial Maximum. Global Biogeochemical Cycles 18:GB2002.

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