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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 #1

Dear Anonymous Referee #1,

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

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.

We appreciate these suggestions and are now restructuring the paper to accommodate them, thereby putting focus squarely on the new vegetation model. As part of this we now will compare the following six simulations of the last glacial termination in one or two of our figures: Simulation 1 (S1)$\mapsto$a nul model with no vegetation change but all other prescribed forcings, S2$\mapsto$S1 but using the old DCESS veg model, S3$\mapsto$S1 with the new DCESS veg model but no veg albedo effects nor permafrost, S4$\mapsto$S3 but including new veg albedo; S5$\mapsto$S4 but including permafrost (30kgC/m^2) and S6$\mapsto$S4 but including increased permafrost (60kgC/m^2; see also answer to K. Crichton's comment). This creates the desired shift of focus towards the effects of the land parameterisation on DCESS model dynamics. The discussion section will be restructured accordingly.

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.

We will discuss these points in more detail in the paper. As the data show and our model simulates, the terrestrial biosphere by itself likely gains carbon across the last termination. However, as shown in our Fig. 5, when permafrost is included there is an overall carbon loss that will increase for our new simulation with enhanced permafrost.

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Discussion paper

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?

Yes, methane is considered as radiative forcing. As described in Shaffer et al. (2008), our old land biosphere model was tuned to emit the methane required to balance atmospheric oxidation while achieving observed pre-industrial atmospheric methane concentrations. We have adopted this approach for our new land biosphere model too but we found that this simple extension of our earlier approach led to values for LGM methane considerably higher than observed in ice cores, about 500 ppb compared to 350 ppb. So we decided to use prescribed methane (and nitrous oxide) concentrations from ice core observations for our radiative forcing calculations in our last termination simulations. However, inspired by the reviewer's comment we are revisiting our methane calculations with the idea to only allow methane production in the “wet“ vegetation zones (tropical or tropical/boreal). Depending on our new results we may then use simulated methane (rather than prescribed) for our radiative forcing in the simulations and then, of course, include a new short section describing our new

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methane approach.

In our simulations we use a climate sensitivity of 2.5°C for a pCO_2 doubling as was explained and motivated in section S6 of our Supplement.

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.

In our work we do in fact include the ice sheet area effect, albeit in a simplified way. On the other hand we did not include possible effects of sea level change and associated land exposure. In Joos et al 2004, this effect was found to be considerably less important than the ice sheet area effect or the climate/CO_2 change effect. However, we will discuss this point as suggested.

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?

These are of course central parameters in any land biosphere model but here we prefer not to go into sensitivity studies based around variations of these parameters. We feel that that would carry us too far afield. Furthermore, the values for the parameters we now use have proven to give comparable land biosphere results in recent intercomparison studies of past and future warming and pCO_2 change (Eby et al 2013, Climate of the Past; Zickfeld et al 2013, Journal of Climate). When we participated in these studies we found that the original DCESS model fertilisation factor (0.62; Shaffer et al, 2008) appeared to be too high. By reevaluation of this we arrived at a lower value (0.37) that we used in the 2013 studies and continue to use today.

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.

We agree that the section of land biosphere albedo, including Fig. S1, should have been included in the main text and in the revision we will do so. As mentioned above, we will also do a sensitivity study of the effect of land biosphere albedo.

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.

The answer to this requires a more in-depth description, both here and in our revision, of how we arrived at these curves. Our point of departure was the total tree cover frame of Fig. 4 of Gerber et al. 2004, Global Biogeochemical Cycles. On that frame we read off, at 2°C intervals from -10 to 10°C deviation from pre-industrial global mean temperature, the latitudes in the Northern Hemisphere of 50% tree cover both above and below the subtropical zone of lower tree cover. Each of these two sets of 11 points formed the basis for our curve fitting. We found that 5th order polynomials provided good fits to each of these sets whereas 3rd order polynomials did not, in particular for the tropical/grassland boundary. In our revised manuscript we will plot the individual "data" points in Figure 2.

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.

There is a mistake in Equation 16. The fraction just to the right of the equal sign should be 35/60 (not 25/60), explaining the imbalance pointed out by the reviewer, This is an error only in the written equation of the manuscript; the model code uses the correct equation. As stated in Shaffer et al, 2008, "...NPP is distributed between leaves and wood in the fixed ratio 35:25, all leaf loss goes to litter, wood loss is divided between litter and soil in the fixed ratio 20:5, litter loss is divided between the atmosphere (as CO_2) and the soil in the fixed ratio 45:10. Soil loss is to the atmosphere as CO_2...". This helps to explain the other fractions in equations 16 and 17. Unfortunately as pointed out by the reviewer, Equation 18 is in error as written down in the manuscript (but not in error in the corresponding model code equation). In the manuscript Equation 18 should read

$$F_{CO2} = \sum_{i=1}^{3} -NPP^i + \frac{45}{60} NPP^i_{PI} \cdot \lambda^i_Q \frac{M^i_D}{M^i_{D,PI}} + \frac{15}{60} \cdot NPP^i_{PI} \cdot \lambda^i_Q \frac{M^i_S}{M^i_{S,PI}}$$

As pointed out by the reviewer, Equation 18 should be the sum of equations 14-17. This was so in the model code and now is also the case in the manuscript equations. Furthermore, Equation 19 is now also being corrected in the same manner in the manuscript.

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.

The number 0.33kg·m^{-2} (actually 0.329....) is the amount of ^{13}C needed to yield a δ^{13}C of -24 in permafrost soil, given the assumption of a constant 30kg·m^{-2} carbon in permafrost. It is calculated using Eq. S11 of the Supplement. For clarity we will include this information in the revised test.

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

The ALL_TF simulation uses the production rate from Hain et al. (2014), as stated in the figure caption.

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.

Through the above described restructuring of the paper, the discussion of ocean carbon cycling is now put more into the background. Furthermore, we now clearly state where individual items raised in the discussion are supported, basically that is Figure

S7 and Table S3. However, to maintain the focus of the paper on land carbon cycling, we refrain from adding more plots on these topics to the manuscript.

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

We may not have made clear enough why we include this text here. The model land fraction in this latitude range of changing permafrost extent is important since we want a global permafrost estimate but work with a model that represents the Earth with one generic hemisphere. If the mean land fraction of North and South Hemisphere (in that latitude range) was very different from the model land fraction, some scaling for the permafrost effect would be required. We now underline the importance of this point by revising the paragraph to read:

Land area uniformly covers 25% of the globe from the equator to 70 degrees latitude in the one hemisphere, DCESS model. For our model last glacial termination, permafrost affects latitudes between 47° and around 54° (see Fig. S3 in the Supplement), and is estimated as a two hemisphere mean. Across these latitudes, the land fraction averaged over both hemispheres is around 30% (see e.g. Matney, 2012). Thus we did not feel it to be necessary to further scale the permafrost effect due to global mean land fraction.

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

The initial global temperature is $15°$C and is now included in the text.

P2L12: Check abbreviation for extratropical forest

Corrected to EF in the text. Thanks again to the reviewer for careful reading of our manuscript.

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