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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 K. Crichton

Dear K. Crichton,

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

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The study presents developments of the DCESS earth system model, for vegetation zones and for a permafrost carbon pool. They present some validation for the vegetation zones, and then go on to perform and discuss the simulation of the last glacial termination. I focus on the permafrost module here.

The amount of carbon stored in the area defined as permafrost in the model is 30kg/m2, an approximation from present-day near surface soil organic carbon data in Schuur et al 2015. The approach to define where is the permafrost, is to use the latitude Lsnow (or Lice, whichever is lower) at the 0degC global temperature (page 9 line 6: is this a typo? Do you mean Lsnow is at the 0degC latitude? Perhaps this needs to be re-written).

To clarify we modified 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 assume that this extent is defined by L_{snow} , the latitude of 0°C global mean temperature".

Whilst this would indeed create a dynamic pool of carbon sensitive to changes in the area of Lsnow/ice, I am not convinced that this is a good representation of permafrost-carbon. If the concentration in this pool is fixed at 30kg/m2, then it cannot be properly taking account of the long time-to-equilibrium that would be seen in a permafrost-like carbon pool. Low accumulation and low decay rates means that the rate of change of area becomes important for soil carbon content. This is true for both release from "thawed" (i.e. no longer in Lice/snow) or newly permafrost (i.e. new Lice/snow) areas. They assume that this 30kg/m2 is instantaneous for new areas, and is instantly released in thawed areas (is this the case in the model?). As such it is entirely dependent on the parametrisation of Lsnow (and not soil carbon dynamics or decay rates). The mean value of 30kg/m2 also does not take account of the true spatial heterogeneity of carbon content in permafrost soils. For example, some areas which underlay

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peatlands contain on the order of 100kg/m2+ organic carbon contents, and others far less than 30kg/m2. The spatial location of higher or lower-than-mean carbon content soils would make a big difference to carbon release rates. However, this is not possible to treat in this model (due to how it is set up) but should be mentioned.

I understand that this is a reduced complexity model, but it is important to incorporate accumulation and decay rates into a permafrost carbon pool model. This is especially true if the aim is to consider changing climates. At least this needs to be discussed in the text. See Zimov et al 2009 for example of the dynamic response of a permafrost-carbon model soil.

In the simplified context of the DCESS model, mean values and zonal boundaries characterise each of our three new vegetation zones and for consistency this approach was extended to our treatment of permafrost. As shown in Zimov et al. 2009, carbon release rates from permafrost for warming are rapid with time scales on the order of 100 years. Such time scales are comparable to those of extra-terrestrial forest reoccupation of areas freed from permafrost, a process that we also take to be "instantaneous" in the model. Thus there is also internal consistency in this point. On the other hand, carbon buildup in permafrost during cooling is a much slower process (Zimov et al. 2009) but we start our simulations from LGM conditions following 80,000 years of cooling. Thus we feel that our permafrost approach, although very simplified, should be able to capture the first order effects of permafrost on carbon cycling during deglaciation, as reported in our paper (but see also below). We will bring a discussion of these points more up front in our revision.

The authors say they tuned their model to a last glacial climate state, but this doesn't appear to include the amount of carbon on land. Although the total change of -408GtC from LGM to PI is in alignment with recent estimates, the starting point at LGM from 1800GtC (fig 1) is far lower than Ciais et al 2012 estimate (at 3640+-400GtC). The authors do state p18 line10 that their permafrost pool is underestimated (compared to Ciais et al 2012), but they need to quantify this. It makes a big difference to the LGM simulation discussion. For example, with a far larger LGM permafrost carbon pool it is unlikely that regrowth of the terrestrial biosphere would compensate permafrost thawing for land carbon flux. It would also pull their LGM-PI land carbon change out. This needs to be discussed.

Ms. Crichton is mistaken in stating that our LGM starting point for the amount of carbon on land is 1800 GtC. Rather this is our result for our new biosphere but does not include our estimate for permafrost. When that estimate is included (see Fig. 5) the total amount of carbon on land is about 2800 GtC, much larger but still somewhat low compared to the Ciais et al. 2012 estimate of 3640+-400GtC. To address this and our apparent underestimation of the permafrost pool we will carry out an additional simulation as a sensitivity study whereby we will use a doubled permafrost content, i.e. 60 kg of carbon per square meter. For this case our total amount of carbon on land will be about 3800 GtC, in good agreement with the Ciais et al. 2012 estimate. We will include the results of this new simulation in our Figs. 4 and 5.

The authors have put a lot of effort into improving the land biosphere module for vegetation zones, but the permafrost pool representation is less well developed and not well explored, and is not validated. They need to consider whether representing permafrost carbon dynamics in this way, assuming an instant equilibrium with climate of soil carbon, is appropriate (I think it's not). And if not, then to instead develop a separate permafrost model for use with DCESS.

As follows from what we wrote above, we will put more effort into explaining, exploring and validating our treatment of permafrost.

Zimov, N. S., et al. "Carbon storage in permafrost and soils of the mammoth tundra-steppe biome: Role in the global carbon budget." Geophysical Research Letters 36.2 (2009).

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