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Interactive comment on "Quantifying the Influence of the Terrestrial Biosphere on Glacial-interglacial Climate Dynamics" *by* Taraka Davies-Barnard et al.

Anonymous Referee #1

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In this study the biogeochemical and biogeophysical effects of vegetation on the climate system are analysed separately over the time span of the last 120 000 years. Spanning the last ice age inception, last glacial and the latest deglaciation, 62 "snapshot"-type (i.e., under constant forcing) simulations were integrated, distributed over that period, with HadCM3, a fully coupled atmosphere-ocean GCM with interactive vegetation. In addition, 5 transient simulations were integrated over the whole period with various versions of the cGENIE model. These simulations were based on terrestrial carbon fluxes diagnosed from the HadCM3 simulations. The authors conclude that the biogeophysical effects of vegetation account for additional mean cooling during the glacial and, in some cases, substantial regional cooling. The biogeochemical effects are smaller and of opposite sign. The authors also emphasize that different timescales are involved for these two effects on climate.

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I have enjoyed reading the paper. It is well written and presents interesting results. I do have three major comments and a long list of other comments and recommend that this paper might be suitable for publication after all of my comments are addressed (i.e. major revisions).

Major comments:

1. One underlying hypothesis with this approach is that the feedbacks considered (ocean, vegetation, atmosphere, sediments, biogeophysical, biogeochemical) add up linearly (i.e. there is no non-linear interaction between the biogeophysical and biogeochemical feedbacks). This assumption needs to be clearly stated and discussed.

2. Under which boundary conditions cGENIE was spun up? Am I right to assume that 120kaBP boundary conditions were used? Am I also right to assume that the diagnosed terrestrial carbon fluxes (calculated based on changes in terrestrial carbon) from the HadCM3 snapshot simulations were interpolated as "emission" forcing time series and applied to cGENIE? And that these "emission" forcing time series were the only changing boundary conditions during the simulations (i.e., no additional imposed CO2 changes, no continental ice sheet changes, etc)? If I understood this right, there is another assumption made by this approach: that the climate sensitivity is constant and independent of the climate state. This assumption also needs to be clearly stated and discussed.

3. As shown in Figure 3b and described on Page 8 (lines 11-15), the vegetation changes trigger a change in ocean circulation. While I agree that this is a model-dependent result and not part of the core results (although certainly influencing these core results), it would be interesting to see time series of AMOC for the static and dynamic simulations. In case there is a simple feedback that can be easily crystallized (such as the one mentioned in the text), it would also be good to analyse the results further, verify that this is indeed the feedback and play and describe this feedback in more detail.

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Other comments:

* Permafrost and wetlands are not (cannot be) resolved correctly. Both play important roles in terrestrial carbon feedbacks. While the Discussion briefly mentions the lack of permafrost related feedbacks in the simulations (Page 14, lines 16-23), it might be interesting to add a discussion about wetlands (changes in wetlands in the tropics, but also changes from permafrost to wetlands or vice-versa at high latitudes). Can you please broaden this discussion, including some key references, and, if possible, add an uncertainty range to your results in the Discussion section.

* Coarse resolution and numbers of PFTs (page 5, lines 27-32): The representation of vegetation (and therefore associated feedbacks) is crude in TRIFFID (only 5 PFTs, coarse spatial resolution, crude representation of terrestrial nutrient cycles). While this is not any worse than in most other coupled models, the implications should be discussed in more depth in the discussion section.

* It strikes me in Figure 1 that there is almost no change in tropical rain forest cover. Is that realistic? Would it be possible to include a validation of these results (present day bias + comparison to pollen data from LGM for example)?

* Table 1: I am certainly misunderstanding something here. . . Why does the vegetation C change differ for all 4 set-ups? Shouldn't vegetation C only be affected by new land (especially because the atmosphere does not "see" the carbon released from under the ice)? In which case the two ELE simulations should be very similar, if not the same (same for the two ELI simulations)?

* Page 13, lines 5-8: this is an interesting (although neither surprising nor new) result that feeds back into the discussion about climate sensitivity over long timescales. I would suggest adding a paragraph to the discussion about the different timescales involved and the implications on climate sensitivity. It would also be good to remind the reader, which of these feedbacks are usually incorporated in the state-of-the-art IPCC models (i.e. expand the second paragraph in Discussion).

* HadCM3 simulations: it is my understanding that the HadCM3 simulations are run under constant external forcing, initialized from the previous MOSES 1 simulation, then run for 300 years with equilibrium TRIFFID (50 years of TRIFFID for each 5 years of the climate model run), and finally integrated 300 years with dynamic TRIFFID (where TRIFFID is called every 10 days). I am puzzled by this approach – I would assume that TRIFFID is in equilibrium after the first 300 years of "equilibrium setting". I would also assume that atmospheric conditions are fairly close to equilibrium after these first 300 years, so TRIFFID in "equilibrium setting" saw internal forcing with little drift. Why integrate TRIFFID in dynamic setting for the last 300 years, if the forcing that TRIFFID "sees" is more or less constant in a climatological sense and if TRIFFID is already in equilibrium? This does not change the results presented here, it just seems weird (e.g. why not run TRIFFID in equilibrium over the whole 600 years of constant boundary conditions?). See also text on Page 11, lines 15-16 "the model is run for sufficient length of time for the soil and vegetation carbon to reach equilibrium".

* Set of simulations with static vegetation based on PI simulation (page 4, lines 11-14): I assume that the vegetation in this simulation is masked out under continental ice sheets (adjusted for the ice sheet extension of the period of interest). Can you please add this here.

* Why are the EPICA anomalies halved? Is that to get a representation of global temperature changes (versus changes in local temperature)? Can you please clarify this in the text?

* I do not understand what is shown in Figure 4a. Is it the globally integrated (or globally mean) albedo anomalies? If this is the case; why don't they add up? Or are these means over land/ocean versus global mean? Can you please clarify?

* Figure 5: can you please show these plots for simulations that treat carbon under ice sheets the same way? These plots should only show the difference in C and albedo due to changes in vegetation and soils away from ice sheets. The large purple areas

in 5e and f take the attention away from the results.

* cGENIE simulation TRCE needs to be described in more detail in Section 2.2.

Editorial changes:

* Page 4, line 6: I would prefer to go forward in time: 120 – 0 kaBP. * Page 4, line 21-24: again, please go forward in time; e.g. 120 to 80 kaBP,. . . * The filled points are hard to see in Figure 2a * Figure 4: I find it hard to discern the different lines – would it be a good idea to plot these time series in different colours? * Page 9, line 6: "is" missing * Figure 6: the red and pink lines are hard to discern * Figure 7: Figure caption should read GCI_ELE and not GLI_ELE

Comment on Colin's comment: I agree with the authors that this is a sensitivity study, showing the whole range of possible scenarios (including unlikely ones). I would not recommend reducing the numbers of scenarios shown and discussed in the text. However, I agree with Colin that it should be made clear in the abstract that the higher terrestrial carbon loss scenarios are more likely (this is the one the cGENIE time series are shown for in Figure 7). Side note: a newer estimate of total terrestrial carbon glacial/interglacial change based on benthic d13C data is given by Menviel et al. (2017).

Menviel, L., Yu, J., Joos, F., Mouchet, A., Meissner, K.J. and M.H. England, 2017: Poorly ventilated deep ocean at the Last Glacial Maximum inferred from carbon isotopes: a data-model comparison study. Paleoceanography, 32, 2-17.

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