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Climate of the Past
Discussions

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

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Response to comment by Colin Prentice: *Interesting simulations, but what about the real world?*

Thank you to Colin for his detailed comments on the LGM carbon aspect of the paper. We note that none of his comments detract from the main point: the balance of biogeophysical and biogeochemical climate effects from the terrestrial biosphere over the glacial-interglacial cycle. After the formal reviews, we will be happy to make some adjustments to the text.

For clarity, we include the comments in italics and our response in standard text below.

The Abstract severely overestimates the uncertainty in estimates of the change in land

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carbon storage between the last glacial maximum (LGM) and pre-industrial time. The impression is given that the biogeochemical consequences of land-ocean transfer are small, and even of unknown sign. Hence the last sentence of the Abstract, which implies (unjustifiably in my view) that land biogeochemical feedbacks can be neglected on these time scales.

An abstract is always a delicate balance between giving the most amount of information possible, fairly representing the results, providing appropriate context, and showing the results most likely to interest potential readers. We freely acknowledge we may not have struck the correct balance. We will be guided by the reviewers on whether it would be better to amend the last sentence of the abstract. However, given this is a research paper rather than a review, we feel it is justified to report the novel results and their possible implications, rather than reiterating existing understanding.

The problem arises as soon as the range of modelled values for the LGM – PI difference in land carbon storage is given as –440 to +37 PgC. In fact, only the value of –440 PgC is defensible. The other three values calculated assume either that there was no vegetation on the exposed continental shelves, and/or that pre-existing vegetation and soil carbon was not transferred to the ocean but somehow remained in situ.

The two ‘no carbon on expanded land area’ scenarios assume that soil and vegetation carbon on exposed continental shelves is not returned to the atmosphere when the land is inundated, not that it doesn’t exist at all or that it isn’t transferred to the ocean. The same is true for the ice-sheets. We agree that this is not made sufficiently clear and will clarify the text accordingly.

The main text makes clear that the authors do not, in fact, consider these as likely alternatives; so they should not be given equal weight.

We agree the most likely scenario should be given more weight, and consequently we use the -440 value for further calculations throughout the paper. As we discuss in the paper, the fate of carbon under ice-sheets and after inundation is currently not fully

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known. Therefore, we feel it would be disingenuous to dismiss this range all together. Ultimately, our responsibility as researchers is to present the results we found, including the uncertainties, and explain them as best we are able.

The problem is compounded by a superficial treatment of the literature on observationally based estimates of this difference. The current text gives equal credibility to attempts made two decades ago to estimate carbon storage "bottomup", either via manual interpolation of sparse pollen records (e.g. Adams Faure 1998, Crowley 1995) or based on climate and biome model simulations (e.g. Prentice et al. 1993) A review of this topic (Prentice Harrison 2009) noted the unreliability of all of these methods, which (a) assume a constant carbon density per biome and (b) disregard the effect of CO2 concentration on plant productivity and, therefore, carbon storage

While we agree that a comprehensive assessment of the biogeochemical, biogeophysical, and vegetation literature for the whole of the 120 ka period would be ideal, space constraints mean we have picked a representative selection. In the introduction we provide context in the form of older papers, and in the discussion we cite a variety of newer papers as comparators. Our intention is to highlight areas of similarity and difference to other research across the several topical and temporal areas we cover, rather than provide a detailed review of any one aspect.

Ciais et al. (2012 – nb, this should be 2011)

We believe this misunderstanding originates from the fact that the paper was published online November 2011, whereas the journal itself was published in January 2012. Following the citation provided by Nature Geoscience, we use 2012.

is cited for the large range of previously published values that is summarized there. But Ciais et al. more importantly provided the most comprehensive analysis of benthic δ13C data to date, and a complete isotopic mass balance calculation, arriving at a best estimate of –330 PgC. Ciais et al. also attributed the discrepancy between this observationally based estimate and several larger, model-based estimates to the *counterbalancing effect of a large inert carbon pool (putatively stored in permafrost) at the LGM. This idea is further supported by the recent work of Crichton et al. (2016) showing that the δ13C record of atmospheric CO2 over the deglaciation can be well explained by a substantial permafrost carbon contribution to the deglacial CO2 rise.*

We primarily use the -440 PgC value, which is fairly consistent with Ciais et al. (2012) and Crichton et al. (2016). A best estimate is, however, not 'the truth' and does not invalidate exploration of the source and scale of uncertainties. We feel it is important to present the results as we found them, and that it would be wrong to exclude results which do not exactly conform to current thinking.

The authors give too little information about the land biosphere model that they used. In particular, no information is given about the formulation of the CO2 effect on primary production.

The dynamic vegetation model used in HadCM3, TRIFFID, is well established, having been first published in Cox et al., (2000), and is the core of the terrestrial carbon cycle in JULES (Clark et al., 2011). TRIFFID has been used in many publications (e.g. Armstrong et al., 2016; Booth et al., 2012; Cox et al., 2004, 2000; Davies-Barnard et al., 2014; Falloon et al., 2012; Friedlingstein et al., 2006; Good et al., 2012; McCarthy et al., 2012). In this paper, we provide five references for the land surface scheme; a summary of the key features of TRIFFID; and a separate reference for the model configurations we use. We feel this is sufficient guidance for interested readers to read further, whilst not overburdening the paper with detail.

This effect is critical given the large variations in atmospheric CO2 that occurred during the period studied. The plant functional type maps show unrealistically extensive tropical forests at the LGM (in comparison to pollen data, see e.g. Prentice et al. 2011; and offshore n-alkane δ13C measurements, see e.g. Bragg et al. 2013). This suggests that the model underestimates the effect of low CO2 on global biome distribution and also may mean that the strength of the biogeophysical feedback – a key point of the

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paper – has been underestimated.

You're right when you say that the extent of tropical forest does not perfectly fit the observations. Our main concern, however, is whether the error is of an order of magnitude that would affect our overall findings. The biogeophysical impact at the LGM in these simulations are consistent with those found by other modelling studies. Were we to assume the biogeophysical effect is underestimated in our model because of underestimating the effect of low CO2, it doesn't damage our conclusions, because if the biogeophysical effect is larger, proportionally any biogeochemical effect is smaller. Therefore, although we agree our model is not perfect, we don't think that invalidates our conclusions.

A minor point concerns the East Siberian ice sheet. According to the cited reference, and most other recent treatments, there was no such ice sheet during the last glacial period.

As we say in the text, we exclude the East Siberian ice sheet entirely. It is generally agreed that there was no East Siberian ice sheet at the LGM, but there is more uncertainty in the earlier part of the glacial period (see for instance Ehlers et al., (2011). We agree our phrasing doesn't clarify that we refer to uncertainty in the earlier parts of the glacial period, and we will amend the text accordingly.

Finally, although the paper makes much of the limited contribution of changes in terrestrial carbon storage to the long term course of atmospheric CO2 concentration in part due to compensating oceanic mechanisms, this is not a new finding. For example, the analysis by Joos et al. (2004) – of which Paul Valdes was a co-author – accounts for the CaCO3 compensation mechanism and indicates a more than six-fold "dampening" of the effect of terrestrial carbon storage changes on atmospheric CO2 over multimillennial time scales.

The main point of this paper is the assessment of the net (biogeophysical and biogeochemical) terrestrial biosphere contribution to the climate. This does necessitate covering both the biogeochemical and biogeophysical aspects, and as you point out, of course these have already been done to some extent (usually at lower resolution, as is the case for Joos et al. (2004).). In covering these aspects, we think it's useful to highlight interesting features of our simulations. Inevitably, we will use our new data to make some points similar to those made elsewhere. However, that does not diminish the fact that the key novelty of this paper is assessing both the biogeochemical and biogeophysical perspectives together to understand the total influence of the terrestrial biosphere on the climate over the glacial-interglacial period.

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