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Climate of the Past Discussions

# *Interactive comment on* "Assessing the impact of late Pleistocene megafaunal extinctions on global vegetation and climate" *by* M.-O. Brault et al.

**M.-O. Brault et al.**

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Reviewer(s)' Comments to Authors:

Short Commentary by ALS Swann

" I enjoyed this paper and the continued discussion of land surface and thus climate impacts associated with changes in megafauna. The authors of this study attribute the climate impacts of changes in high latitude vegetation cover (mostly shrubs and some trees) to changes in albedo. I want to suggest that the authors also consider the possible impact greenhouse warming from changes water vapor content associated with higher transpiration rates.

The feedback mechanism of increased transpiration rates from a shift to deciduous



vegetation cover (trees and shrubs) leading to warming through the greenhouse effect has been considered in the literature (Swann et al. 2010, Lawrence and Swenson 2011, Bonfils et al. 2012) and found to be of the same order of magnitude as the impact of albedo change. In personal communication with authors of Doughty et al. 2010 it was acknowledged that they did not consider this effect, and looked only at the correspondence between albedo and surface temperature - so comparison with their findings would not acknowledge this mechanism.

From the figures provided in this manuscript it is difficult to evaluate the relative importance of albedo and water vapor changes. In figure 3c the climatology of temperature change is plotted showing a maximum in late June/early July at 60N (although the numbers on the contour lines are too small to read). The seasonality of such a temperature change is consistent with the expected seasonality in increased transpiration, and not necessarily with the expected changes in albedo. I suggest that the authors compare seasonality of changes in albedo, transpiration, column water vapor content, and if possible, changes in the residence time of water vapor in the Arctic atmosphere. It may be that this model results show only the effect of changes in albedo, but readers are unable to evaluate this from the information presented. "

>The atmospheric component in the UVic ESCM is very simple, and although water vapor concentration is implied in the model equations (see equation 5 in [Weaver et al., 2001]) we did not constrain relative humidity in a way as to investigate the water vapor feedback. In fact, we removed from an earlier version of this paper a plot of the changes in precipitation because a single-layer energy-moisture balance model cannot produce very accurate precipitation fields.

>Thus, while it would be worthwhile to examine the effect of the feedback mentioned here, the present model does not give us sufficient resources to carry out a thorough investigation. We have added a sentence in the paper (see p.19, line 1) to acknowledge the importance of water vapor feedback in the global system.

Anonymous referee #1

" Summary Brault et al. explore the impact of late Pleistocene extinction of large terrestrial fauna on climate using the University of Victoria Earth System Climate Model (UVic ESCM). They assume that with the extinction of megafauna, tree-grazing in northern mid to high latitudes ceases and open grasslands are replaced by shrubs and trees. These vegetation shifts result in changes in the biogeophysical characteristics of the land surface (e.g. albedo) and changes in carbon fluxes. Brault et al. find a global warming of about 0.2 C from biogeophysical effects alone in their Maximum Impact Scenario, which assumes the largest possible reforestation in the model. In a model simulation with free-evolving CO2 this warming is amplified by biogeochemical effects due to release of soil carbon and a slight increase in atmospheric CO2. Smaller but non-negligible temperature effects are also found in scenarios with less extreme assumptions. Brault et al. conclude that megafaunal extinctions at the end of the Pleistocene could have had a small effect on climate.

General comments This is an interesting paper, as it suggests that megafaunal extinctions could have resulted in non-negligible climate effects. The paper is well structured and clearly written. The methodology is sound and the conclusions are justified. My only criticism is that the paper provides little discussion of the robustness of results and differences/similarities with previous studies. Since only few studies exist that explore the climate effect of megafaunal extinctions specifically, the authors could draw on the body of literature about mid and high latitude reforestation/afforestation in the modern climate (e.g. Betts et al., Nature, 2000; Arora et al., Nature Geosc., 2011). How different are the results from those of these earlier studies? Are the discrepancies due to model differences or differences in boundary conditions (Pleistocene vs. modern)? "

>We now mention the two above papers in our discussion of temperature in the MIS (see section 5.2, third paragraph). Our discussion already includes some comparison of our results with earlier studies, but we have made these comparisons clearer. While our overview of climate impacts is fairly qualitative, we find results that seem to agree



with earlier studies on deforestation/afforestation. The main discrepancies between our results and those of other studies come from the difference in boundary conditions, mostly in regards to the distribution of vegetation, continental ice sheet configuration, and orbital parameters.

" Specific comments: p. 441, l. 11, “It is almost impossible to overlook”: This phrase sounds awkward. Replace with “It is necessary to consider” or something similar. "

>We have corrected the wording as suggested by the reviewer.

" p. 441, l. 13: Please clarify which processes do you refer to as “carbon sequestration”. "

>Here, carbon sequestration refers to the uptake of atmospheric carbon by plants via photosynthesis. Hence an increase in plant cover results in an increase in carbon sequestration. We have clarified this in the paper.

" p. 443, l. 22 and 28: The “2” in CO2 appears superscript instead of subscript. "

>We have corrected this.

" p. 446, l. 17: Does “tree growth” include growth of shrubs? Please clarify. "

>Yes, it does include shrubs. We have changed “tree growth” by “growth of trees and shrubs” in order to clarify.

" p. 446, l. 23: “annual spread of surface albedo anomalies”. It is unclear what you mean by “spread”. Variation? Same comment applies to p. 448, l. 20. "

>We have deleted the above awkward phrase and have rewritten the discussions of Figs. 2c and 3c.

" p. 448, l. 9, “temperature response is not fully linear”: linear with respect to which variable? "

>Linear with respect to albedo changes, since this is what drives the warming. We

have clarified this in the paper (see p.13, lines 21-23).

" p. 451, l. 23-24: Which processes account for the reversal in atmospheric CO2? "

>In the original manuscript we stated in brackets that vegetation carbon sequestration was responsible for the reversal in atmospheric CO2. This cannot be true, as carbon sequestration from vegetation would only increase during the vegetation expansion, which occurs during the first 150 years of the simulation. The reversal is likely due to carbon uptake by other reservoirs such as the ocean. The offending sentence has been modified accordingly.

" p. 452, l. 7: Is ocean carbon changing in response to the vegetation changes in the free-CO2 experiment? "

>Yes. The experiment is set up so that all changes in the climate system are a direct or indirect response to the change in vegetation. That includes a perturbation to any component of the carbon cycle.

" p. 452, l. 11: I am not sure this is a pertinent reference. In Cox et al., soil carbon release is associated with the reverse process (replacement of trees by grasslands)! "

>We have now replaced the reference with [Meissner et al., 2003] which is more pertinent to the discussion in this section.

" p. 454, last sentence, "we recommend a more thorough investigation of land surface processes: : :": Which kind of investigation do you have in mind? Please be more specific. "

>Here, we refer to the surprising results obtained with the free CO2 simulation. A more thorough investigation would involve testing the free CO2 setting at various boundary conditions to know whether the land carbon emission always occur during the transition from grass to shrub PFT, and not just a late Pleistocene phenomenon. It would also be a good idea to compare with other dynamic vegetation models that could produce similar results. We have clarified this point in the paper (see p.19, lines 3-4).



" Fig. 1 caption, l. 2: There is an extra "both". "

>We have eliminated one of the "both".

" Figs. 2C and 3C: These figures are hard to read in the printed version of the manuscript. "

>We have remade the figures in order to display larger numbers. It should now be easier to read the figures.

" Fig. 6: Panel A: I suggest to state in the figure caption that the panel displays global SAT anomalies. Panels C and D: I doubt the units (PgC) are correct? "

>We have clarified the figure caption for panel A. The units for panels C and D are kg and not Pg. This has been fixed.

Anonymous referee #2

" Brault et al. investigate the potential impact of the extinction of mammoths and similar megafauna after the last glacial on the climate system. Assuming a boreal ecosystem where the growth of trees has been suppressed by large animals, they describe both biogeophysical and biogeochemical consequences of the regrowth of trees after the extinction of the megafauna using an earth system model of intermediate complexity.

The extinction leads to a northerly expansion of shrubs and trees into the areas originally kept tree-less by megafauna, modifying the surface albedo, which leads to a warming. In addition, soil carbon is released, leading to an increase in atmospheric CO2, amplifying the warming.

Overall, it is a very good manuscript showing well-described original research. However, there are a few issues which warrant attention. I therefore recommend publication after minor revisions.

I very much applaud that the authors look into the transient changes in biogeochemical and biogeophysical feedbacks, which may show quite a different response from the changes in quasi-equilibrium states after all. The one drawback is that it is by no means clear what the final quasi-equilibrium state of the system will be like. Therefore I am missing one crucial experiment in the manuscript, an entirely unperturbed experiment in which tree cover was never suppressed in the first place. Since CTL, according to the paper, was spun off from a transient run from LGM to present day, this should actually be available to the authors. "

>The run which this reviewer refers to is indeed available to us. It is a background run showing simulated climate change during this period, with no perturbation to vegetation. The reviewer makes a point that it would be a good idea to compare the results of the MIS with the unperturbed run, which would allow us to see what difference remains between the quasi-equilibrium (unperturbed simulation) and our simulation. In Fig. 3a, we have added a timeseries of the temperature difference between the background run and the CTL run, representing the quasi-equilibrium state of the system. The MIS is always a little colder than the background run (where trees were not removed in the first place), but the two curves converge towards each other with time, showing that the climate system gradually returns to its quasi-equilibrium state.

" I would like to see this for comparison because it is not clear from the paper whether the vegetation has reached a quasi-equilibrium state after the 500 yr model experiments. Judging from the figures, the affected areas are first covered by shrubs, and after about 300 years the shrubs begin to be replaced by trees. In year 500, the end of the experiments, tree coverage seems to still be increasing. This could either be because glaciers are still receding or because the tree cover has not yet reached quasi-equilibrium with the boundary conditions. If the latter, effects may actually be larger than described in the paper. "

>The vegetation response in the model is fairly quick due to the height-biased competition scheme in TRIFFID. Most of the vegetation recovery due to the mammoth extinction is complete after approximately 150 years. Since we compare an “extinction” run with a “no-extinction” run where no vegetation is allowed to grow, the difference



plot also registers any subsequent vegetation change due to ice sheet melt and global warming. For example, the appearance of trees toward the end of the simulation (year 12000 BC) is predicted by the model even without the perturbation to vegetation cover.

" With regard to the biogeophysical effects of the regrowth, I generally find the paper convincing, though there are two questions left open:

The paper convincingly shows the transient changes in tree coverage and the corresponding changes in albedo and temperature. However, the reader is left wondering how close to equilibrium with respect to boundary conditions the system is at the end of the experiments, i.e., whether further would have to be expected after the end of the experiment. It would be great if the authors could discuss this aspect of the temporal dynamics. "

>Since this is a transient simulation, the final state is not very important; rather, we are interested in the evolution of climate parameters over the course of the simulation. As we already mention in the discussion (p.12, lines 22-24), we would expect the temperature difference between the CTL and EXT runs to become even greater with time because the ice sheet melt and global warming increase the difference between the two runs in terms of tree cover. In our case, a state of equilibrium is never really reached.

" In addition, it is not quite clear from the manuscript under which conditions the Weddell Sea temperature anomaly occurs. I am especially wondering whether it also occurs in the “later extinction” scenario, but also whether it would occur in a scenario where no suppression of tree growth had ever taken place. "

>The Weddell Sea temperature anomaly is a recurring feature in the UVic model which is consistent with other studies with this model (see, for example, Matthews et al. (2012) on how the UVic model deals with the Weddell Sea). The anomaly is caused by a see-saw effect in the oceanic circulation which causes the Southern Ocean to become cooler in response to warming in the Northern Hemisphere. The effect is am-

plified in our simulation, which is why the anomaly appears despite our plots showing the difference between the CTL and EXT simulations. The anomaly is also shown to be insensitive to timing, so it definitely appears in the “later extinction” scenario.

" When it comes to the biogeochemical effects, I find the manuscript not quite as convincing. It is entirely plausible that grassland soils may store substantial amounts of carbon that might partially be lost under vegetation change, but whether the particular formulation of the TRIFFID soil carbon model is realistic in this respect warrants further discussion.

The authors' result hinges on the carbon release from SOM decomposition happening faster than the uptake of carbon during the forest regrowth. Unfortunately the authors do not discuss the final state the system would attain once forest regrowth is finished. How large is the total carbon stock (biomass+soil) before and after tree regrowth has finished? If the total stock is smaller after regrowth, the carbon release would occur anyway, no matter whether the particular time scales implemented in the TRIFFID model are correct. If total system carbon in the final stage is larger than before forest regrowth, on the other hand, the carbon release would be a completely transient effect depending on the exact model formulation, especially with regard to decomposition and forest succession times scales. An extended discussion of the size of the C pools, as well as the temporal dynamics, is required for the results to be credible. "

>One thing is clear according to our results: carbon is massively released from the soil when a transition from grass to shrub PFT occurs. Since tree regrowth is finished approximately 150 years after the extinction, the vegetation change results in a net CO2 increase of 15 ppm in the atmosphere. The subsequent decrease in atmospheric CO2 is likely due to sequestration of this carbon in other pools, such as the ocean. In the paper we attribute this decrease to the increase in vegetation carbon sequestration; this is false, since no significant expansion of the terrestrial vegetation occurs during the time during which atmospheric CO2 decreases. We have modified this section in the manuscript to clarify our discussion of the temporal dynamics (see p. 16, lines



18-23). See also our response to reviewer 1, page 451.

" Finally, Zimov et al. (see appended references) argue that the productivity of the mammoth steppe environment, as they call it, was very high due to grazing pressure and the fast cycling of nutrients if biomass is decomposed at mammal body temperature, as opposed to ambient temperature. While it is clear that the authors cannot actually model these hypotheses since MOSES and TRIFFID contain neither permafrost nor nutrients, a short discussion of the potential effects of nutrients and megafaunal extinction would further improve the manuscript. "

>As mentioned by the reviewer, the UVic model is missing crucial elements such as nutrient cycling and permafrost dynamics. However, we argue that these missing elements do not necessarily invalidate our results, since we do not explicitly model the presence of mammoths in the model. Rather, we simulate the extinction as an increase in vegetation cover, thus avoiding the topic of the impact of mammoths entirely. It is interesting to point out the potential role of these factors, but discussing those would be beyond the scope of this paper. However, we have added a reference to the work of Zimov et al. (p.19, line 2)

" Some minor things: p. 443, l. 22: CO2 should have the 2 as subscript, not superscript "

>This has been fixed.

" p. 452, l. 11: Cox et al. is the model description, a Hadley Centre technical report, not a peer-reviewed publication. This is not the best citation for showing that model behaviour is realistic. "

>We have now replaced the reference with [Meissner et al., 2003] which is more pertinent to the discussion in this section.

" Fig. 1, 2c, and 3 are very difficult to read (printed manuscript). A higher resolution version would improve matters, I believe. "

>We have enlarged the contour numbers in Figs. 2c, 3b and 3c in order to make them easier to read.

" Fig. 6: Are units correct? A release of 1.4x10ËĘ14 PgC seems an awful lot... After all, most models have roughly 1500PgC stored in soils globally. "

>The units should read kg instead of Pg. This has been corrected in the manuscript.

Editor comment

" The referees suggest acceptance of the manuscript with minor revisions. Perhaps their comments are a bit heavier than calling just for minor revisions. Nonetheless, I am glad to agree with the referees in principle and to encourage the authors to submit a carefully revised manuscript.

When submitting the revised manuscript, the authors should reply to the referees' comment and the comments by Abigail Swann point by point. Specifically, I suggest considering the point raised by referee 1 and 2 regarding the robustness of their model results. What is the quasi-equilibrium state of the model? Has vegetation dynamics and carbon cycle dynamics reached a final equilibrium? What about the Wedell sea anomaly? How robust is the carbon stock in TRIFFID? What about the (neglected) effect of nutrients? Perhaps, it is just a rumour, but does UVIC consider changes in cloud cover?

Furthermore I agree with referee 1 to consider earlier studies on biogeochemical and biogeophysical effect of land cover change in modern climate. In addition to the papers mentioned by referee 1, I suggest reading the papers listed below.

Whether it is possible to isolate the effect of albedo changes and water vapour changes on climate differences, or the effect of dry versus wet greenhouse gas changes, as suggest by Abigail Swann, is hard to say. Such an analysis would require a complete factor analysis using 2 to the power of n separate simulations in the case of n feedbacks. The authors might wish to comment on this point as well.



Finally, I would like mention that Otterman was one of the first who considered the effects on non-tropical forest on climate. "

>We have added references to [Otterman et al., 1984], [Claussen et al., 2001] and [Bathiany et al., 2009] in the literature review section.

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Interactive comment on Clim. Past Discuss., 9, 435, 2013.