Interactive comment on "Coupled climate-carbon cycle simulation of the Last Glacial Maximum atmospheric CO_2 decrease using a large ensemble of modern plausible parameter sets" *by Krista M. S. Kemppinen et al.*

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In their revised manuscript, Kemppinen and coauthors have made a strong attempt to address the many demands of the three reviewers. Overall, I am pleased with the revisions but am still left unsatisfied. I advocate for further revisions before publication.

The introduction is better, although still needs some work. The authors introduce many mechanisms in the Introduction that might affect CO2. I appreciate that the authors are trying to cover a lot of ground in their introduction, but to simply name a mechanism and then not explain to the reader how that mechanism or change works to affect CO2 is confusing and can be misleading to the reader. I realise that it is beyond the scope of this work to present explain how every mechanism in the biosphere/lithosphere can alter CO2, but I think it is equally poor to name all of them and then not give the reader an explanation of why they are important. For instance, the authors list many factors in the 3rd paragraph in just one sentence and fail to provide any conceptual clarity. Possibly, restricting this initial discussion of mechanisms to just a few important ones would be fine, so long as you detail the links to climate. Alternatively, a table/schematic that describes each mechanism you list in the introduction and takes the reader through its links to global CO2 and climate (i.e. its drivers, processes and effect on CO2) would be a helpful addition.

The methods are clearer.

The results section is clearer and more concise.

The abstract is much clearer, more interesting and conveys the major findings. I would advocate for one more sentence, however, that describes why an increase in TerrC and a decrease in OceanC is associated with a pCO2 drawdown. Something to the tune of "preserving, rather than destroying respired C buried under ice sheets and slower remineralisation rates in soils". I think this will be of interest to the field and improve the popularity (or notoriety?) of the study.

To this point, I am still confused about what the authors mean when they talk about ice-sheet carbon. My confusion stems from an ignorance of what processes are affecting carbon when the ice sheets grow. The authors state that "The increases in terrestrial biosphere carbon are predominantly due to our choice to preserve rather than destroy carbon in ice sheet areas. However, the ensemble soil respiration also tends to decrease significantly more than net photosynthesis resulting in relatively large increases in non-burial carbon". Great, I follow this. Currently, I understand that carbon is preserved in the soils as ice sheets grow over these areas, and that cooler temperatures globally also tend to increase carbon in soils where vegetation is present.

However, then the authors make statements like "It is the combination of our ice sheet carbon stocks increasing rather than decreasing when exposed to LGM climate, and our choice to preserve rather than destroy this carbon", and I am lost. Why does carbon *increase* under ice sheets? It is entirely possible that carbon is *preserved*, but I do not know what process enables carbon in soil under an ice sheet to increase, and the authors do not offer a process that explains this.

They then go on to say "If most of the carbon that was present in ice sheet areas at the end of the preindustrial runs had been lost to climate forcings, it would not matter much [to what? I assume pCO2] whether the remaining stocks had been destroyed or preserved.". I assume from this statement that the authors mean that carbon tends to accumulate in the terrestrial reservoir regardless of changes under ice sheets because soil respiration is reduced more than photosynthesis under cooler climates, and that this is the most important term. This makes sense when viewed through the lens of the metabolic theory of ecology [by the way it would be good to cite some work of the metabolic theory of ecology (Brown, etc.)]. However, this statement then seems to contradict a previous statement: "If the LGM burial carbon inventories were to be removed, DTERRC would be negative in 13 our of 16 simulations, despite the fact that terrestrial carbon also increases outside of the ice sheet areas in 15 out of 16 simulations." This suggests that burial of carbon from under ice sheets (?) is super important for gains in the terrestrial reservoir, while gains in non-ice sheet areas are not so important.

One of two things are causing confusion and make me sceptical of the results. One, the authors have not provided an explanation of the processes and assumptions of how they treat carbon under ice sheets. More explanation of the processes governing vegetation-soil-burial carbon stocks would be very useful. Such an explanation may be all that is necessary to resolve these apparently conflicting statements that make the results difficult to understand. Second, errors in the treatment of carbon under ice sheets were made in the initial runs, causing carbon to *increase* in non-physical ways, and the authors are trying to hide this, which causes confusion. The authors state in a response to reviewer 3, for instance, that "Our LGM burial carbon estimates include the initial preindustrial carbon inventories, plus carbon accumulated in response to glacial forcings". With all previous information, this statement makes me think that carbon is accumulating under ice-sheets, which is odd. Is this due to some undefined process with physical underpinning, or an unrealistic, non-physical process?

I also have some concerns with regards to the discussion of $\delta^{13}C$ in the “Other paleo proxies” section. A strong reason for a lower terrestrial C reservoir under glacial conditions is because of the decrease in $\delta^{13}C$ recorded in benthic foraminifera recovered from glacial sediments. The decrease in $\delta^{13}C$ is thought to originate from the land, as terrestrial organic matter with very negative $\delta^{13}C$ signatures found its way into the atmosphere as the terrestrial reservoir diminished.

Carbon in the atmosphere then moved into the ocean, leading to an explanation of atmospheric CO2 drawdown and the lower oceanic δ^{13}C values. If the major result of this study is to hold, then the authors must present a new and plausible explanation of this decrease in the ocean that does not contradict their simulated increase in terrestrial C.

To do this, the authors invoke a reduced marine productivity, low sea surface temperatures, and greater sea ice extent. However, they do not go further to explain why these features of a glacial climate would decrease δ^{13}C in the ocean without requiring the loss of carbon from the land. My reasoning suggests that:

1. Low marine production would actually increase d13C, because less negative d13C would be transferred via organic matter to depth. NOT CONSISTENT.
2. Low SST would make δ^{13}C more positive because fractionation during outgassing of CO_2 (which is what you're invoking) would leave more ^{13}C in the ocean. NOT CONSISTENT.
3. Greater sea ice area would limit production, as you have stated, which would increase δ^{13}C. NOT CONSISTENT.

The authors then mention other studies that could change δ^{13}C over the glaciation and deglaciation (in the subsequent paragraph), and I suppose their aim here is to introduce uncertainty for the causes of the trends in δ^{13}C (and radiocarbon) to challenge the accepted wisdom. However, with the exception of the Lea 1999 study they cite, I am left unconvinced by their argument and therefore sceptical of their result. Once again, the authors seem to list off many studies without talking through in a conceptually clear manner why that response/mechanism could help to explain their results. Moreover, their mention of Hain et al (2011) is misleading, because this study did not discount depleted Δ^{14}C in a glacial ocean, but rather the "extremely" depleted values found in the Pacific by Stott et al (2009) and Marchitto et al (2007). An increase in the carbon reservoir of the ocean was not challenged by Hain et al (2011).

Major revisions are once again needed. Overall, I am of the opinion that many of my concerns could be allayed if only more effort was put into making the arguments clearer. So, if the authors can (1) improve the introduction by clarifying why a certain process affects CO2 or do not invoke that process (alternatively schematic/table for reader), (2) clarify the processes that allow carbon to increase under ice sheets over their LGM simulations, and (3) improve their discussion of their results against the carbon isotope and paleo proxy data sets, then I advocate publication.

I **strongly suggest** that the manuscript provides a more thorough description of how the model treats carbon under ice sheet growth. This may allay my concerns totally and I believe would focus and strengthen the later discussion of alternative processes to explain the carbon isotopes.

1 General comments

Some other general concerns that I had as I read the article:

- I think that the Results sections might be better if they were split into "Pre-Industrial conditions", "LGM ensemble conditions", and "Carbon cycling through terrestrial, ocean and lithospheric reservoirs". This would be a nice way to present the main features of the results, which are pretty cut and dry in the first two sections. The real meat of the paper lies in the consequences for carbon cycling, and an interested reader could flick between "LGM ensemble conditions" and "Carbon cycling through terrestrial, ocean and lithospheric reservoirs" sections to see changes in conditions and consequences for carbon, respectively.

- My initial suggestion regarding overlay of the red (ENS-16) over the yellow (ENS-104) bars in the histograms still stands. This would reduce a lot of unnecessary replication in the figures. The authors could use a transparency setting to ensure that the red and yellow are easily seen if they have the same number of experiments (frequency), and possibly use a break in the vertical axis to emphasise the lower frequency ENS-16 experiments. I note that reviewer 3 also suggested this.
- Another suggestion RE figures is that you use a bimodal colour scheme to present changes in your spatial plots. Reds for positive, blues for negatives and centre the range around zero so places with no change are clearly seen. It is misleading to readers assessing your results to present unbalanced colour schemes when discussion change.
- Once again, the writing needs some attention. There are too many adjectives, unnecessarily difficult acronyms, long subordinate clauses, and double negatives in some instances. Please make it easy for the reader. My native language is english, so I mostly understand with a repeat reading of sentences, but many scientists are not.

2 Specific comments

Abstract

Introduction

- Paragraph 3 is one long sentence. Please break it into more sentences to make it easier to read.
- In my opinion, the paragraphs 5 and 6 of the introduction are unnecessary. You cover these points in the methods and abstract. The content could be reiterated in a small paragraph under the main Results heading.
- The final paragraph is unnecessary, but I understand that reviewer 1 thought it was a good idea. Up to you.

Methods

- Page 4, line 16 - What is EFPC? you do not define it. I see you mention it only twice. Just spell it out.
- Page 7, line 23 - You mention that the model requires a detrital flux field that is specific to the ocean component. I strongly suggest you explain what this detrital flux does and how it affects your carbon cycling in the ocean between LGM and PI experiments. It might be explained more thoroughly in Ridgwell and Hargreaves (2007), but I would like clarification here.

- Page 8, line 10 - “If one expects...” Please rephrase this sentence. It took me three times over to make sense of it.
- Page 8, line 24 - “In the latter case,...” Please make it more clear what you mean by this sentence. I logic is not clear.

Preindustrial simulations

- Page 10, line 7 - “deemed not uncontrovesially implausible” is a double negative. Make it easy for the reader. “deemed plausible.”

LGM ensemble simulations

- Page 12, lines 9-12 - can you provide changes in mean salinity in units of psu alongside your percentages?
- Page 21, line 4 - You still use PGACF acronym here despite using ENS-315 elsewhere. Also, PGACF is not defined.
- Page 23, line 17 - But why would ice sheet burial of carbon constitute and *increase* in carbon over that of an active boreal forest, for instance? This needs to be clarified.
- Page 23, line 22 - The increase in terrestrial carbon under ice sheets as a result of LGM climate forcings needs to be clarified both here and up front in the Intro/methods.
- Page 24, line 3 - After reading this sentence many times, I think I now understand what you mean. You mean that if all carbon under ice-sheets were to have been destroyed then TerrC would have decreased under LGM scenarios. Could you make this clearer by being simpler?
- Page 25, line 1 - Why would vegetation carbon increase under an ice sheet?
- Page 25, line 11 - “Here, the increase in terrestrial biosphere carbon both inside and outside of the ice sheet areas, are presumed to reflect the decrease in soil respiration rate due to colder SATs exceeding the decrease in photosynthesis rate due to lower CO2, SAT and precipitation, as they are mainly driven by soil carbon increases.” Again, how could an increase in soil carbon occur under an ice sheet that should not have any vegetation providing a carbon input?
- Figure 11 - It would be helpful to see the outline for where the ice sheets were placed in the simulations.
- Page 26, line 11 - “The highly negative LGM terrestrial carbon changes...” This is an example of too many adjectives. “The loss of terrestrial carbon under LGM conditions” is easier to read.
- Page 27 - Table 5 is not simple to read. Perhaps you could represent this in a better way? Maybe a figure? It just takes a long time for me to look at it and understand what is going on given the headings and the lists of numbers.
- Page 29, line 6 - Just for your information, the Buchanan 2016 study did more than just make estimates of POC export during a glacial climate, and integrated global climate changes in physical variables including temperature, sea ice, circulation.
- Page 30, line 7 - again PGACF is used.

Conclusion

- The first paragraph is basically the same as what is in the introduction. Simplify and reduce.
- Page 36, line 14 - "equally plausible" of what. Once again, I really advocate for the authors to specify what they mean.
- And you cannot say "broad agreement" until there is a better argument for why your results could be compatible with a lower δ^{13}C in the ocean.

3 Technical corrections

1. Page 3, line 14 - replace "gets" with "is"
2. Page 5, line 18 - replace "which" with "those that" in both instances.