Dear Editor Laurie Menviel, Dear anonymous Referee #1 and #2,

Thank you for your suggestions and comments on our revised manuscript from the 12th of December 2020. We have replied to your comments below (in italics), in the following order: 1) the comments from the Editor, 2) the comments from Referee #1, 3) the comments from Referee #2.

Yours sincerely,

Anne Morée and co-authors

Editor Comments

1) The simulated changes in oceanic circulation in the Atlantic Ocean seem relatively small, and there is no shoaling (or even a slight deepening) of NADW at the LGM compared to PI. The LGM ACC is stronger, does that mean the Southern Ocean upwelling is stronger at the LGM? In any case, given the text in section 3.2.1, it should be made clearer in the Abstract, Discussion and Conclusion, that stronger changes in oceanic circulation could also provide a better model-data agreement, and given little changes in oceanic circulation a Beff of 75% is needed.

The LGM-PI changes in ocean circulation are visualized in Fig. S6 (Atlantic stream function), as well as through the PO tracer Fig. 1a,b and Fig. S11a,b. Based on the Atlantic stream function, we find a 350m shoaling of the 0 Sv contour line at 30°S, and based on the PO tracer we see a more voluminous and northward reaching SSW mass. The ACC has strengthened by about 13% LGM-PI. The latter indeed goes along with a strengthened upwelling south of ~55 °S (see Fig 1 below).

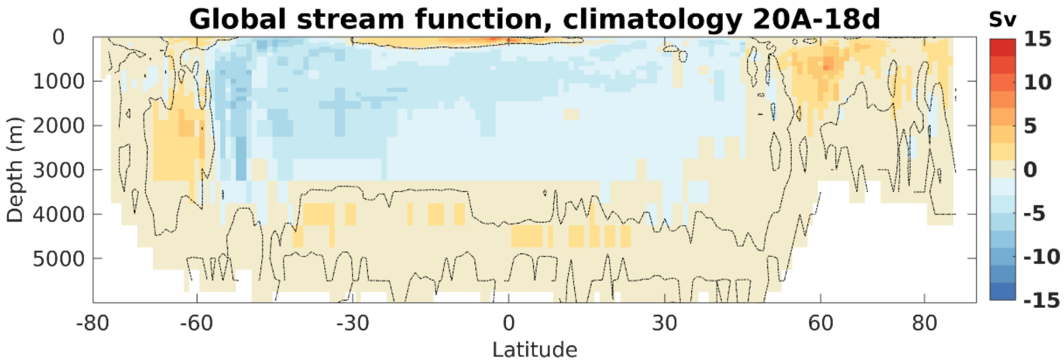

Figure 1 Global climatological mean stream function, difference between LGM-PI.

The LGM-PI changes in ocean are generally within the uncertainties of reconstructions (as described in detail in Section 3.2.1). We nevertheless agree that there is additional room for changes in ocean circulation that could improve the d13C model-proxy data agreement (as discussed in Sect 4.1). A reduction instead of an increase in Southern Ocean upwelling between the LGM and PI would be one candidate for improvement. Note however, that this does not contradict our approach to explore the BP_eff as a strengthening of the efficiency of the biological pump can also be obtained through 'pure' circulation changes as this are inherently related to the biological pump (see also the last sentence of Sect 2.4). Nevertheless, the doubling of the strength of the efficiency of the biological pump still leaves a model-proxy data error of 0.07 permil larger than the 0.19 permil data uncertainty, which can likely be improved through further changes in ocean dynamics (last sentences Abstract). In order to put more *stress on and clarify the possibility of ocean circulation changes reducing the model-proxy data error, we:*

- included the sentence 'The drivers of such an increase in the biological pump efficiency may be both biological as well as related to circulation changes incompletely captured by our model - such as stronger isolation of Southern Source Waters.' in the Abstract;

- include the sentences 'Last, we note that many of the biogeochemical processes mentioned here are closely related to ocean circulation. Therefore, changes in LGM-PI water mass configuration and overturning strength beyond those captured by our model are strong candidates for reducing model-proxy data biases.' At the end of (the new) Sect 4.2.;

- including the sentence 'This approximate doubling is likely driven by a combination of additional biological and physical changes, such as stronger isolation of SSW (as discussed in detail in Sect 4.1 and 4.2).' in the conclusion.;

-We add the sentence 'The strengthening of the ACC in our simulation goes along with a strengthening in upwelling south of ~55 °S (not shown)' to Sect. 3.2.1 to clarify the effect of the ACC strengthening on upwelling in our LGM simulation.

2) Some clarifications might also be needed regarding the LGM to PI shift in mean d13C: the mean oceanic d13C was about 0.34 permil lower at the LGM than PI (e.g. Peterson et al., 2014). Given that the LGM and PI atmospheric d13CO2 are relatively similar, this shift arises from a lower terrestrial carbon reservoir at the LGM, and potential imbalances between weathering and sedimentation. As the LGM oceanic state is forced with -6.5 permil d13CO2, this shift is not included, which is fine. However, caution has to be taken while comparing model and data d13C, as the model values will of course be higher than the data. I am saying this because I am worried that when adjusting the Beff, this was not taken into account, which would lead to an "over-adjustment" of the Beff.

We note there is some confusion regarding the setup of our atmospheric carbon isotopes. Our LGM oceanic state is not forced by a -6.5% permil $\delta^{13}C^{atm}$, instead the box atmosphere lets it freely evolve. We include a sentence at the end of Sect 2.2 'Note that atmospheric $\delta^{13}C$ can freely evolve in our setup due to the inclusion of a prognostic box atmosphere.' as well as including '…, after which these are allowed to freely evolve,' in Sect. 2.3 in order to make this clearer.
The effect on atmospheric and marine $\delta^{13}C$ due to vegetation loss on land is indeed not included in our study (as stressed in Sect 3.2.2). Nevertheless, we see we have not reported the actual LGM-PI atmospheric $\delta^{13}C$ change as simulated by our model nor the change in mean marine $\delta^{13}C$. Mean marine $\delta^{13}C$ changes +0.2148 ‰ (from 0.5403 ‰ in the PI to 0.7551 ‰ in the LGM). We have added these numbers to Sect 3.2.2, as well as for atmospheric d13C.
Indeed, if our model would have contained the input of low $\delta^{13}C$ from the land to the atmosphere and ocean, this may have contributed to the mean marine $\delta^{13}C$ shift. We think that the mean marine shift of 0.34 ± 0.19 ‰ as estimated by Peterson et al., (2014) is driven not only by lower terrestrial carbon reservoir at the LGM and potential imbalances between weathering and sedimentation (and the atmospheric shift which is small): The stronger LGM marine $\delta^{13}C$ gradient represents a voluminous deep ocean with a negative signature, thereby contributing to lower mean marine $\delta^{13}C$. The effect of sedimentation-weathering imbalances plus lower terrestrial carbon on mean marine $\delta^{13}C$ was estimated in Jeltsch-Thömmes et al. (2019) at a relatively uniform deglacial change of ~0.4 permil (i.e.

-0.4 permil LGM-PI) for a change in the land biosphere carbon inventory of 890 Gt C – within the uncertainty range of the Peterson et al. (2014) estimate. Nevertheless, the 0.4 ‰ mean $\delta^{13}C$ shift also leads to a change in $\delta^{13}C^{atm}$ of similar size (Jeltsch-Thömmes et al., 2019), which is not seen in e.g. ice core records. Several processes thus need to be considered simultaneously to estimate their effects on $\delta^{13}C^{atm}$ and $\delta^{13}C$_DIC – and no consensus is reached here yet. Our model simulated a ~0.2 ‰ increase in LGM-PI mean δ^{13}C *in absence of some of these processes (most importantly, a land and sediment model). In summary, we note that the quantitative contribution of each of the drivers of the shift in mean marine* δ^{13}C *is still under debate and that our model setup only captures some of these drivers (as no land or sediment model is included). We agree that it is important for the reader to get an impression of the potential effects of a mean $\delta^{13}C$ shift on our analysis (as it indeed can cause an overestimation of LGM BP_eff). Therefore, we decided to include an estimate of the potential effects of a shift in mean d13C on our analysis: If we redo the analysis done to make Fig. 5 but including a -0.4 (or -0.2 permil) shift in the LGM results before comparing to the proxy data, the Figure would look as in the new Fig. S19, with a best-fit BP_eff of ~55 % (~65%). We included a new paragraph on this in the end of Sect 4.4 as well as in the Summary and Conclusions to discuss these points.*

Besides the above:
- While adding the data on the atmospheric carbon isotopes to the manuscript, we noted two small errors in the reporting of the LGM-PI pCO2 changes as well, which is 20.3 ppm exactly (corrected in the revision).
- For radiocarbon, all data are calibrated after the model simulation to an atmospheric value of 0 ‰ (last sentence Sect 2.1) to facilitate comparison with other studies.
- We included the words 'up to' in front of the 75 % when describing the conclusion of our analysis on BP_eff where appropriate throughout the text.

3) Previous LGM model-data comparisons, and particularly the ones using d13C, are completely ignored (e.g. Tagliabue et al. 2009; Hesse et al., 2011; Gebbie 2014; Schmittner & Somes, 2016 (this one is discussed a bit); Menviel et al., 2017, Muglia et al., 2018; Menviel et al., 2020). A paragraph in the Introduction would have been nice, but a few sentences putting your results in the context of these studies definitely need to be added in the discussion.

We had included and discussed the papers by Gebbie (2014) (p. 8, l. 36; p. 9, l. 9; p. 10, l. 27;), Schmittner and Somes (2016) (p. 15, l. 2-9), Menviel et al. (2017) (p. 5, l. 8), and Muglia et al. (2018) (p. 5, l. 13; p. 9, l. 1) in our revised version together with other publications on LGM model-data comparisons. It is unclear to us how the impression could arise that these "are completely ignored". In order to extend the discussion with respect to previous work, we have included further text sections and references in the introduction and discussion (See changes in Introduction Sect. 1 second paragraph, beginning Sect. 4 and Sect. 4.3).

Referee #1

Line16 (Abstract) : "presents a realization of" --> "discusses"
Author Response and change in the manuscript: Corrected as suggested

Line23 (Abstract) : " (offline)" --> removed; The meaning of the word "(offline)" is not very clear.

Author Response and change in the manuscript: Removed as suggested. We also included an explanation of the word offline in Sect. 2.4 and made small adjustment throughout the manuscript where the word 'offline' is used.

Line28 (Abstract): ", on which we include a detailed discussion. " --> removed (or the content of discussion should be described explicitly)
Author Response and change in the manuscript: Removed as suggested. We have also rewritten the last few sentences of the Abstract to better stress the potential role of ocean circulation changes in improving the model-proxy data bias.

Referee #2

Abstract: remove the last statement "on which we include a detailed discussion" as it is unnecessary here.
Author Response and change in the manuscript: Removed as suggested. We have also rewritten the last few sentences of the Abstract to better stress the potential role of ocean circulation changes in improving the model-proxy data bias.

Page 5, lines 30-31: it would be appropriate to cite at least one earlier reference and especially one that is based on the analysis of the sedimentary record, rather than only numerical simulations - e.g. Kohfeld et al. (2005). Kohfeld, K.E., Le Quéré, C., Harrison, S.P. and Anderson, R.F., 2005. Role of marine biology in glacial-interglacial CO2 cycles. Science, 308(5718), pp.74-78.
Author Response and change in the manuscript: We agree that there are certainly other references that could be listed here. We included several more from both modelling- and sedimentary record-based reconstructions.

Page 7, line 20: I still don't find "theoretical approach" the right definition here and a somewhat confusing way to call this. These are "offline calculations of the biological pump efficiency" or something along these lines.
Author Response and change in the manuscript: We have replaced this sentence with your suggestion. Additionally we made revisions throughout the text where the use of the terms 'offline' or 'theoretical approach' may cause confusion.

Page 7, lines 30-32: this sentence is a little long. Break up or at least add a comma between "mechanisms" and "obtained".
Author Response and change in the manuscript: We broke up the sentence in two and made some small adjustments to improve readability.

Page 8, line 1: figure should be sequential, but here you reference figure 7. But this is probably fine if you at least state that this figure is in Appendix A (here and everywhere else in the text afterward). That said, I am not sure that you need both a Supplementary Material and an Appendix?
Author Response and change in the manuscript: We have included 'Appendix A' to every mention of Fig. 7. We like to keep both a SM and Appenix on the Bern3D model as otherwise we find the main text becomes too long. Details are now in the SM, while more general information on the approach and model are in the Appendix and main text.

Page 11, line 12: I just find this reference to a "theoretical framework" unnecessarily confusing again here. All you need to say really is something like: […] and discuss the LGM-PI changes by exploring the efficiency of the biological pump (Sect. 3.3). Similarly in other parts of the text, as noted before.
Author Response and change in the manuscript: We assume the reviewer means page 8 here. See also our reply to your comment on Page 7, line 20.

Page 9, Line 28: "weaker" rather than "slower".
Author Response and change in the manuscript: Changed as suggested

Page 10, line 3: the meaning of the first part of the sentence ("Besides radiocarbon aging of in particular SSW") is unclear, so please rephrase.

Author Response and change in the manuscript: We shortened this first part of the sentence to improve clarity, and added the reference to the relevant Atlantic section figure (Fig. 1c)

I think that supplementary Figure S2 is useful, but it doesn't seem to be referenced anywhere in the text.
Author Response and change in the manuscript: Fig. S2 is only referred to in SM text 1, as is Fig. S1. We have now added a direct reference to Fig. S2 in our reference to SM text 1 (in Sect. 2.2).

Page 15, line 7: as well as
Author Response and change in the manuscript: Thank you for noting this mistake, we added 'as' here.

Page 14 onwards: the current "Discussion" session (4) is clearer as a self-standing section, but a little heavy as it is and could use some breaking up into subsections from line 15 onwards. I would suggest, for instance, renaming the whole section something like "LGM model-data biases" and add a couple more subsections to break this up a little. So the current subsection 4.2 could keep the same title but become 4.4 and before that the physical/water mass changes could be highlighted in one subsection (4.1) and then separate the biological mechanisms and DIC discussion from page 15 line 8 (4.2). Or split it in a different way if you think it is more appropriate, but add at least two additional break points here. I am saying this because this discussion is interesting and useful in highlighting several important results from these simulations, but it is hard to follow and the reader would struggle with it as is.
Author Response and change in the manuscript: Thank you for your suggestions to improve the readability of this discussion section. We now have 4 subsections none of which are more than a page long, which we hope helps the reader navigate through this discussion.

Page 18, line 5 onwards: it may be useful to refer to, as another example, this newly accepted paper: Zhu et al. (2021), GRL, Assessment of equilibrium climate sensitivity of the Community Earth System Model version 2 through simulation of the Last Glacial Maximum. Where the LGM simulation is used to show biases that would also affect simulations used for future projections with the same model (even though the focus is on equilibrium climate sensitivity and cloud feedbacks).
Author Response and change in the manuscript: Thank you for pointing us to this new article. We have included its reference here.