Dear DR. Bengtson,

You manuscript has now been seen again by the two original reviewers. Both agree that you have made significant improvements and recommend publication after quite minor corrections.

I thus encourage you to re-submit a new, revised version following the comments by the reviewers.

Please answer all the reviewers comments in a letter and mark any changes and corrections made to the manuscript in the file. I look forward to seeing this new version of your manuscript.

Kind regards, Marit-Solveig Seidenkrantz

Dear Prof. Seidenkrantz,

Thank you for the invitation to resubmit a revised version of our manuscript. Please see below for our responses to each of the Reviewers' comments.

Kind regards,
Shannon Bengtson

Key for response to Reviewers:
Black text: Reviewers' comments
Blue text: Authors' response
Green text: Amended text

Reviewer #1:

We would like to thank the Reviewer for reading our revised manuscript and continuing to provide helpful suggestions. Please see below our responses to the specific comments.

The draft has significantly improved and the comments of the the reviewer have been well considered. I have only a few minor issues to be dealt with.

- Throughout main text, SI text and Fig 1: To address atmospheric carbon dixode concentration I believe it has to be addressed as „CO2" in „ppm" (not pCO2).

Yes, it should read CO2, not pCO2. This has been corrected.

- Intro:
- Please specify on which criteria the definition of your end of the penultimate glaciation and of the last glacial inceptions are based on. You give quite a number of references here, but only in Govin et al 2015 I already find 4 definitions, depending on the variable of choice. To me, it seems like 129-116 ka BP is based on sea level crossing the 0m line according to Dutton and Lambeck 2012.

The definition we give here is based on a multitude of records, with the beginning of the LIG starting between approximately 129.5 and 128.5 ka (Menviel et al., 2019). We have adjusted the references provided for our definition of the LIG. The sentence now reads:

The LIG began at the end of the penultimate deglaciation and ended with the last glacial inception (~129–116 thousand years before present, ka BP hereafter (Dutton and Lambeck, 2012; Govin et al., 2015; Masson-Delmotte et al., 2013; Menviel et al., 2019)).

- PI defined as 1850-1900: This is a problem for d13CO2, since the 13C Suess Effect is already well visible in this time-window, e.g. see Bauska et al. (2015), doi: 10.1038/ngeo2422

Thank you for drawing this to our attention. We agree that the Suess Effect would be an important consideration for d13C data during PI. However, our analysis does not include data from PI, since our reference Holocene period spans from 7-2 ka BP. In the introduction, we introduce PI as a very recent period of time, when we compare the climate conditions of the LIG to PI, however data from the PI is not used in our analysis.

- Holocene CO2: The plotted spline falls from 268 to 260 ppm (by 8ppm, the text mentions 5 ppm) in the early Holocene, before rising to ~277 ppm (by +17 ppm, not the mentioned +18ppm) at 2 ka BP. Or you stick to your rise by 18 ppm which would end at 278ppm, but change when this is reached. Maybe also spend a few words on the small-scale variability seen during the last 2 kyr or so.

We have changed the text to reflect the changes in the splines of the CO2 data. The sentence of CO2 changes during the Holocene now read:

... while during the Holocene CO2 first decreased by about 8 p.p.m. starting at 11.7 ka BP before increasing by ~17 p.p.m. to 277 p.p.m. at ~2 ka BP (Fig. 1a) (Köhler et al., 2017).

- CH4: I believe CH4 reached 700ppb in both HOL anf LIG.

That's correct. The sentence now reads:
CH4 and N2O peaked at ~700 p.p.b and ~267 p.p.b, respectively, during both the LIG and the Holocene (Flückigeret al., 2002; Petit et al., 1999; Spahni et al., 2005).

- Fig 1c. Please plot EDC dD on the most recent age model AICC2012 as the other ice core records, which is available (for example) here: https://doi.pangaea.de/10.1594/PANGAEA.824891. Note, that your calculation of temperature out of dD of EPICA Dome C ignores any necessary sea level correction, e.g. see Parrenin et al 2013, Eq 1-3 in the SI, doi: 10.1126/science.1226368

We have updated the data to be on the AICC2012 age model for both dD (as per Bazin et al. (2013)), and we have also adjusted the Jouzel et al. (2007) temperature estimates to the AICC2012 age scale.
For the temperature estimate, we use the published data from Jouzel et al. (2007) directly, which accounts for the isotopic changes in sea-water.

- Section 2
- lines 100f: Here, LIG covers 130-118 ka and the HOL 8-2 ka, while the grey shaded bare in Fig 1 cover different time periods. Please explain here why a different time window is chosen. I believe this issue was already covered in the last round of review, so I am not sure, why I am still confused here. It might be enought to refer to section 3.1 for more details on the chosen time windows.

Apologies for the confusion. We did not intend L100 to read as though we are defining the LIG as 130-118 ka BP and the Holocene as 8-2 ka BP. Rather, this line refers to the periods which we use for compiling all the data, within which are the Holocene and the LIG periods (taken as being 125-120 ka BP and 7-2 ka BP, respectively). We include this data even though it is not part of the LIG and Holocene periods that we select because it appears in the time series presented in Fig. 4 and Fig. 6. We have changed L100 to make this clearer:

We present a new compilation of benthic δ13C covering the periods 130–118 ka BP and 8–2 ka BP. From these two sets of data, we select data pertaining to the LIG and compare it to data from the Holocene.

- Results
- Table 3: Please add information, if these regional summaries contain any contributions from the size of the regional water masses or if this is based on the pure point-data. Since volume-weighted averaging is done in section 3.2 this was probably not done here, so a brief notion (non-volume weighted) in the caption would be enough.

Yes, the values in this table are not volume-weighted. This is now clarified in the table caption: "Shown are the non-volume-weighted means…"

- Fig 6: Please use the same scale for the x axis for LIG and the Holocene, right now the Holocene time axis is half as large as that of the LIG (1.5 kyr in the Holocene are the same as 3 kyr in the LIG).

Agreed. We have updated Fig. 6 so that the x axis sizes are consistent between the LIG and the Holocene.

- line 271. Please refer to Fig 6 at the end of the sentence.

Added.

- Discussion

- line 318. Most recent paper on the geological CH4 release is missing (Dyonisius et al2020, doi: 10.1126/science.aax0504): This might replace the Petrenko et al 2017 paper.

We agree and have replaced the reference.

- line 320: d13C of CO2 from volcanism has been assumed to be -2 to -8 permil in Roth and Joos (2012), which is NOT close to zero as suggested here. Please revise the discussion of this issue.

Apologies for this mistake. We meant that the difference between atmospheric d13CO2 and d13CO2 from volcanic outgassing is small. L321 has been amended to:

Similarly, since the difference between the δ13C value of CO_2 from volcanic outgassing is close to that of atmospheric CO_2 (Brovkin et al., 2016) and modelling suggests volcanic outgassing likely only had a minor impact on δ13CO2 (Roth and Joos, 2012)...

Reviewer #2

We thank the reviewer for again providing encouraging and helpful feedback on our manuscript. Below we have outlined our responses to the specific comments:

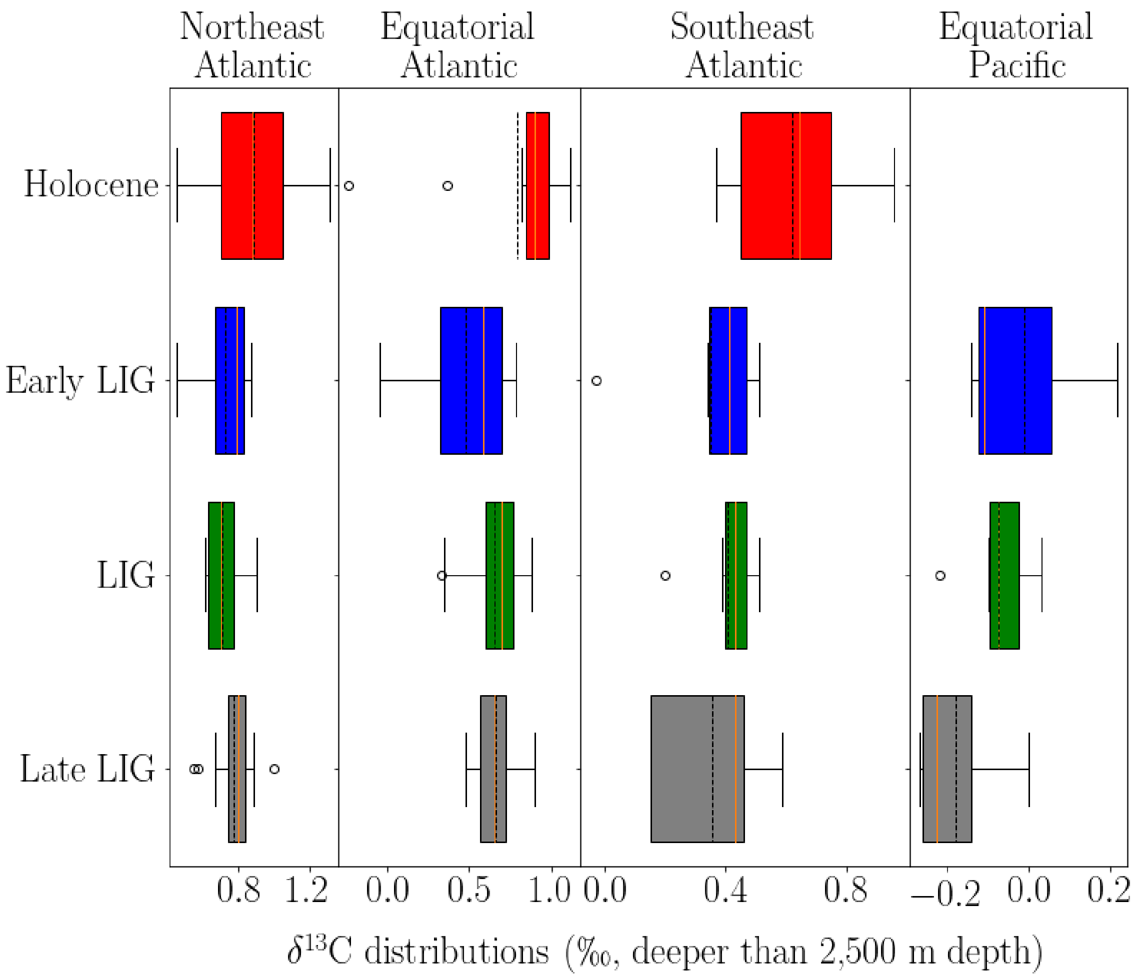
The revised version of the manuscript is a major improvement on the initial submission, which itself was already a solid contribution. Overall, the authors have done a superb job addressing the initial round of concerns, particularly those regarding: 1) age model consistency, 2) the lack of a d13C mass balance estimate, and 3) the depth coverage of the compiled records. While the last point remains an issue given the very few records below 3 km depth in the Pacific, it is now much easier for the reader to assess this limitation given the addition of Figure 3. It is also very useful to have an estimate of the amount of terrestrial organic matter that would be required to yield lower mean oceanic d13C during the LIG, even with the assumption of closed system behavior.

Thank you.

Two other issues, regarding AMOC variability and the driver of lower oceanic d13C, were also addressed but in a less complete way. In regard to the first issue, the authors use a temporal and spatial averaging that make it difficult to see any differences in AMOC between the early and late LIG, despite clear evidence for differences in subpolar North Atlantic d13C. For example, Figure 7 of Hodell et al. (2009) shows a ~1 per mil offset between early and late MIS 5e d13C records at depths shallower than 2 km. Such a large change points to marked shift in the endmember d13C of NADW, which was likely associated with weakened AMOC early in MIS 5e. Note that this is not just centennial-scale variability but rather changes in d13C that span several millennia. In Figure S2 of the revised manuscript, however, there is little apparent difference between the early and late MIS 5e results. It appears that part of the problem is that the mean d13C values in Figure S2 are based on an average of all d13C results from the Atlantic basin, including light d13C data in the South

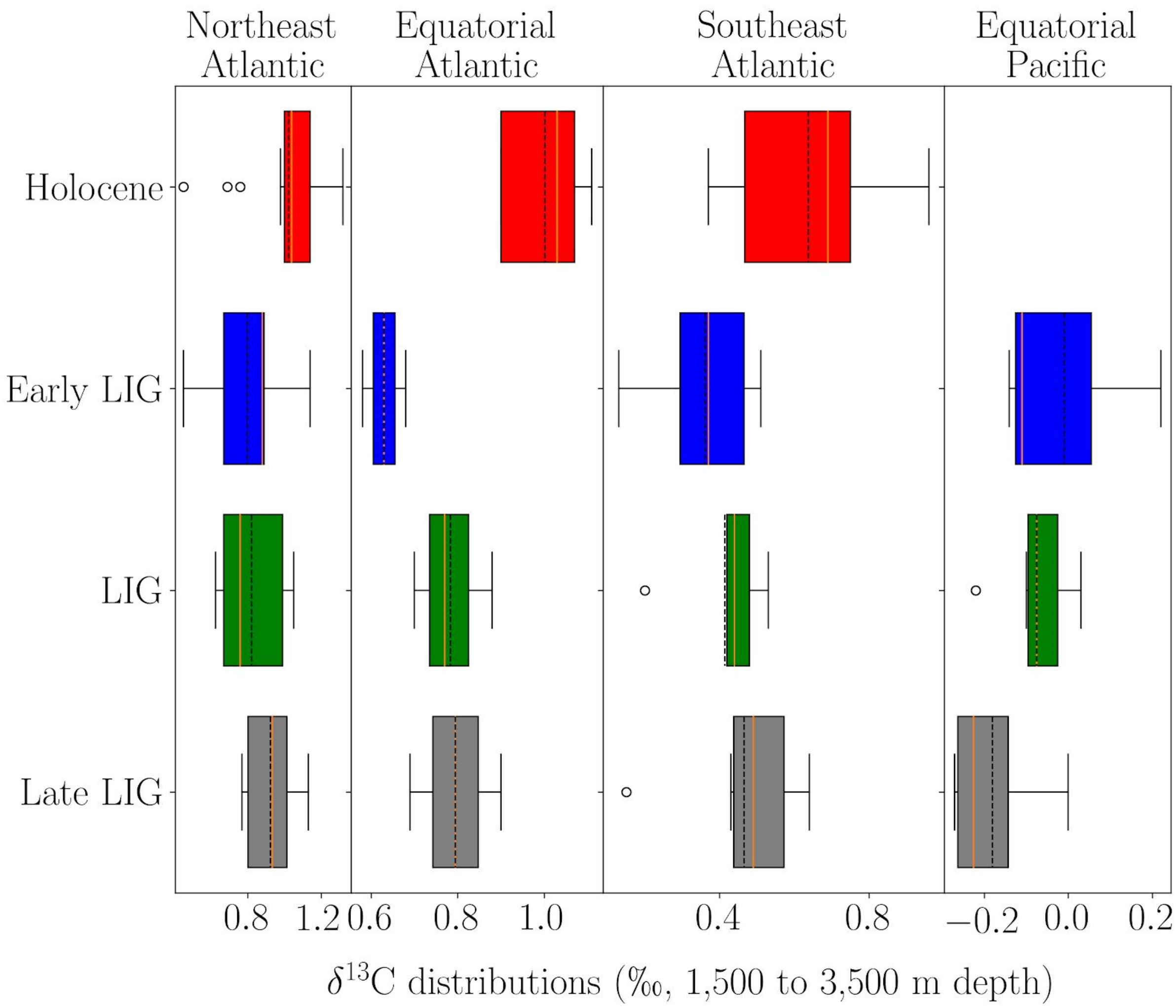
Atlantic that reflect the influence of southern source watermasses. The limited difference in mean d13C between the two time intervals is also likely due to the use of overlapping windows (125-120 ka and 128-123 ka). If the spatial domain were instead limited to the North Atlantic between 1.5 and 3.5 km, for example, and the time windows were non-overlapping, would the resulting mean d13C values still be the same? It is also important to note that most of the LIG data are from the eastern Atlantic, where AMOC perturbations to the d13C tracer field will be less obvious than in the western Atlantic. Given these issues, it would be helpful if the authors used different spatial and temporal constraints for their analysis, or if they were to add caveats to the existing results to alert the reader to potential issues.

We thank the reviewer for raising their concerns regarding the spatial and temporal domains that we used in our analysis of the early, mid and late LIG. Accordingly, we have repeated our analysis of Fig. 5 using time periods of 3 ka which do not overlap: Early LIG: 128-125 ka BP, LIG: 125-122 ka BP, Late LIG:122-119 ka BP, Holocene: 6-3 ka BP. Since modelling of AMOC d13C suggests that grouping the data between 1,500 m and 3,500 m could mask any changes in AMOC (e.g. Fig. 5 of Menviel et al., 2015, DOI: 10.1002/2015GB005207), in this analysis we look at the distributions of the d13C data below 2,500 m.



Here, the analysis does not show any differences in the north Atlantic between early LIG and LIG (125-122 ka BP), however the equatorial d13C data at the early LIG are indeed lower than at the LIG. This is not consistent with a weaker AMOC at the early LIG as this should lead to a d13C decrease in the north Atlantic below 2,500 m depth (e.g. Menviel et al., 2015). As these results are consistent with the conclusions of our paper, we decided to not include this new analysis in the new manuscript.

The Reviewer also specifically asked us to group the data between 1,500 m and 3,500 m using the temporal definitions of the early, mid and late LIG as above. The result of this analysis is shown below. There is still no significant difference in d13C in the north Atlantic.



Additionally, we analysed the d13C data in the northwest Atlantic below 3,500 m during the early LIG compared to the mid LIG. While there is a small difference in the d13C between the two periods (~0.1 permil), the spread in the data is large. Given that the AMOC should recover from HS11 sometime between 129 and 128 ka (e.g. Menviel et al., 2019), and that a centennial-scale weakening of the AMOC has been suggested at around ~127 ka BP (e.g. Galaasen et al., 2014, Tzedakis et al., 2018), making inferences on the mean state of the AMOC between 128 and 125ka seems difficult.

Lastly, the authors have added a substantial amount of discussion regarding the driver of lower mean ocean d13C during the LIG. The additions regarding ocean-atmosphere gas exchange and terrestrial carbon storage are very helpful, in particular the details about peatlands and permafrost. Yet after walking through the various possibilities, the authors return to their original statement about a long-term imbalance between isotopic fluxes to and from the lithosphere. Do the authors believe the LIG vs. Holocene contrast is simply part of a longer term trend that these two time windows happen to capture? If this were the case, we should expect to see earlier interglacial intervals to have even more depleted mean ocean d13C that MIS 5e. Is there evidence to support such a trend?

To date, the only evidence that carbon exchanges with the lithosphere might attenuate d13C excursions over glacial-interglacial timescales come from modelling studies (e.g. Jeltsch-Thömmes et al., 2020). Nonetheless, because of the magnitude of the fluxes in and out of the lithosphere, and therefore their potential impact on the exogenic d13C reservoir, we do not believe that we can attribute changes in mean oceanic and atmospheric d13C to changes in the terrestrial biosphere alone.

The first author is working on a comparison between the Last Glacial Maximum and Penultimate Glacial Maximum for his last thesis chapter and finds a similar anomaly of ~0.2 permil in mean oceanic d13C (not published yet). This offset was therefore likely established at some point during the last glacial period. Based on the available data for our study, we cannot comment if this is part of a long-term trend or not.

And a couple final details…

Line 144: 'Intermediate' typically is reserved for depths less than 1000 m, consider using 'deep' instead.

L144 has been changed to "generally found at depths between ~1,500 and 3,000 m"

Line 260: Check the slopes listed here, they seem to be reversed from those in the figure.

Sorry, the Holocene slope had not been properly updated with the new time window. This has been corrected. L260 now reads:

However, the meridional δ13C statistical model gradients are not very different for the LIG (0.0036 ‰ ^{o}latitude^{-1}) and the Holocene (0.0030 ‰ ^{o}latitude^{-1}) (Fig. 8a), suggesting a similar southward penetration of NADW.

Line 321: The d13C of mantle carbon is ~ -7 per mil, which is quite different than zero. Do you mean that volcanic CO2 has a d13C similar to the atmosphere?

Yes, you are right, it is the difference between the d13C values of atmospheric CO2 and CO2 from volcanic outgassing that we are referring to here. L321 has been amended to:

Similarly, since the difference between the δ13C value of CO2 from volcanic outgassing is close to that of atmospheric CO2 (Brovkin et al., 2016) and modelling suggests volcanic outgassing likely only had a minor impact on δ13CO2 (Roth and Joos, 2012)...