Anonymous Referee #1

Received and published: 14 September 2020

Shao et al. assess the mechanisms driving changes in the stable carbon isotopic composition of the upper ocean and in atmospheric CO2 (d13CO2) during the last deglaciation, focusing on the first major decline in d13CO2 observed in Antarctic ice core records around 17 kyr before present. Based on model simulations with LOVECLIM and GENIE, the authors test two hypotheses that may explain these trends: first, the upwelling of respired carbon (with a low-d13C signature) from the deep ocean, primarily in the Southern Ocean and its advection to the global ocean via the thermocline, and subsequent equilibration with the atmosphere (bottom-up scenario); and second, the sub-surface supply of respired carbon and strong equilibration with the atmosphere in upwelling regions (causing a decrease in d13CO2), and parallel transmission of the atmospheric d13CO2 signal to the upper ocean via air-sea gas exchange (top-down scenario). Through a carbon speciation analysis, the authors find a strong influence of the top-down process on global upper-ocean d13C records (including a new one from the western equatorial Pacific), confirming important proxy-based postulations made by Lynch-Stieglitz et al. (2019).

This paper is a timely model-study on the mechanisms of global d13C records, testing (opposing) inferences on the global carbon cycle made initially by Spero and Lea (2002) and more recently by Lynch-Stieglitz et al. (2019). It therefore merits publication in Climate of the Past.

We are grateful for the positive assessment of our work.

I do, however, have difficulties to follow the argumentation of the authors in places, see why different model approaches were chosen (transient vs. equilibrium, glacial vs. interglacial boundary conditions) and whether they are appropriate for the premise of the study (in particular, their combination). The study essentially confirms the proposition of Lynch-Stieglitz et al. (2019) but I see some scope to provide novel insights that would increase the impact of the study. I elaborate on these aspects and other minor ones below. I recommend major revisions of the paper prior to publication. I also want to sincerely apologize to the authors for the delay in providing my evaluation of their manuscript. I hope that despite the delay the authors find my comments useful in improving their study.

We will revise the manuscript to better elucidate the rationale for our approach and in doing so also accommodate recommendations of Referee #2, paying particular attention to how the models and associated experiments are justified and described, how the numerical tracers are defined, as well as expand on the more novel insights that arise (including evaluation of preformed $\delta^{13}C$). This we detail in the point-by-point responses below.

We will also frame the paper much more towards the novel regenerated $\delta^{13}C$ numerical tracer that we have implemented in cGENIE – this is the first time such an (explicit) analysis has been carried out to our knowledge, and enables us to shed novel insights into the different components contributing to observed $\delta^{13}C$ changes as well as error inherent in previously publish approximation (from regenerated PO_4 to regenerated $\delta^{13}C$) approaches.

Major comments:

Preformed and remineralized speciation in Introduction: The partitioning of ocean carbon into 'preformed' and 'remineralized' is central to the authors' study, but these important terms are not properly introduced in the study. A definition of these terms in the introduction are needed, and in particular how they are defined and what processes they are influenced by in the real world and in the model world.

In the revision, we will be more expansive on the description, justification, and application of the numerical/diagnostic tracers employed in the models.

The latter I find some- what incomplete: How do kinetic equilibration effects play into the partitioning process of carbon between the atmosphere and ocean, besides thermodynamic equilibration effects and primary production? Are surface wind effects considered as drivers of air- sea gas exchange in the model? Through the impact of surface wind stress on the piston velocity or gas transfer coefficient, winds have a strong influence of air-sea gas exchange in the real world (e.g., Wanninkhof, 1992). Also note in line 59, that changes in the residence time of water parcels at the surface can also lead to preformed carbon changes, simply by varying the time available for air-sea gas exchange. This statement needs to be revised accordingly.

We are grateful to the reviewer for highlighting what was a poor descriptive effort on our part, particularly given the importance of the tracer to the study. We will substantially improve and expand on the description in the revision.

With respect to the role of winds and air-sea gas exchange (and in addition to addressing requests of Referee #2 regarding a fuller description of the global invasion of isotopic signatures from the atmosphere), we will explicitly isolate the role of changing (Southern Ocean) winds – both as influencing only air-sea gas exchange and not circulation, and in influencing only circulation and not air-sea gas exchange – in an additional series of cGENIE model experiments that pick apart the changing controls on preformed vs. regenerated $\delta^{13}C$.

Line 83. Justification is needed why the simulation LH1-SO-SHW was chosen although Menviel et al. (2018) provide a number of other simulations with increase Southern Ocean ventilation, e.g. LH1-SO.

"LH1-SO-SHW" was picked from Menviel et al, (2018) for several reasons: 1) recent ice core records also suggest enhanced SO westerly winds during Heinrich stadials (Buitzert et al., 2018); 2) "LH1-SO-SHW" matches some of the important observations (e.g. ice core record of atmospheric pCO_2 and $\delta^{13}CO_2$) better than the other scenarios presented in Menviel et al.,(2018); 3) the stronger SO windstress in "LH1-SO-SHW" leads to an increased transport of AAIW to lower latitudes, which could have impacted the intermediate depths of the global ocean, including the site of our new benthic $\delta^{13}C$ record.

Offline calculations of carbon species in LOVECLIM: I find it striking that the authors' "approach requires accurate representation of the preformed and remineralized components" (line 62), but that the LOVECLIM model does not simulate them explicitly. The authors need to discuss what types of errors might affect their offline calculation based on the LOVECLIM and how large these errors might be. For instance, why does AOU overestimate true oxygen utilization? (line 182).

This goes to the heart of our '2-model' methodology (also see replies to Referee #2), in that we are re-analyzing an existing model experiment (LOVECLIM 'LH1-SO-SHW') and that the particular published experiments we are interested in lack the specific (and unique) numerical tracer we need. For this reason, we employed the 'cGENIE' Earth system model of intermediate complexity to explicitly evaluate metrics derived from the LOVECLIM model experiment – AOU to regenerated phosphate and hence to regenerated $\delta^{13}C$. Furthermore, rather than evaluate derived metrics such as AOU in the context of the modern (preindustrial) state, we will conduct additional experiments employing glacial-like boundary conditions in cGENIE and carry out the evaluation in that context. This will all be significantly expanded upon in the revised manuscript, including discussion of errors inherent in the approximations.

With respect to the Referee's specific question – it is well known that AOU likely over estimates the true oxygen utilization, and thus DIC_{org}, particularly in water masses formed in high latitudes (Bernardello et al., 2014; Ito et al., 2004; Khatiwala et al., 2019). To this, we will provide illustrative maps of the AOU error to give the reader a better sense of where (and why) the AOU approximation breaks down. We will present a similar analysis for the step to regenerated $\delta^{13}C$.

I find the sensitivity experiments made in cGENIE to alleviate the problems associated with the necessity of an offline calculation not convincing, because the experimental setup, forcing and boundary conditions are very different. This leads to my next point of criticism.

Comparability and suitability of LOVECLIM and cGENIE simulations: How do the cGENIE and LOVECLIM simulations support each other, when they are so different? Is it correct that wind changes are not considered in the cGENIE simulation (which they are in the LOVECLIM simulation)? If correct, this should be clearly stated. In that case, would this call for the use of LH1-SO instead of LH1-SO-SHW?

Firstly, we agree that the 2-model methodology was not made clear from the outset. We propose an extensive revision of the text that separates out the cGENIE-based assessment of how (and how reliably) regenerated $\delta^{13}C$ can be estimated in model (in turn based on AOU) before moving onto the analysis of the LOVECLIM experiment. We will include explicit graphical illustration and discussion (also addressing comments by Referee #2) that supports what will be a much more transparent and logical methodology.

Secondly, we agree with the reviewer that since we employ cGENIE to evaluate the method we use to attribute the isotope changes simulated in LOVECLIM, that the experimental design for cGENIE should be as close as possible to that of LOVECLIM. Hence, for the revision, we will carry out a revised series of tracer diagnostics and analysis using cGENIE simulations run under recently published and more 'glacial-like' conditions that account for a different planetary albedo due to expanded continental ice sheets as well as the radiative forcing from the lower glacial greenhouse gas concentration (Rae et al., 2020). To better compare with LH1-SO-SHW, we will also include transient varying wind stress forcing over the Southern Ocean in the cGENIE experiments, in addition to the salt/freshwater flux that is already applied in the original simulations.

How preformed nutrients or carbon are simulated in cGENIE is unclear, in particular given the statement in line 98 to 99. If preformed tracer values are reset to the full tracer value (what is this?) at each model step, does this skew the outcome to a dominance of preformed changes? I believe some more explanation is required here, as this suggests that all water masses leaving the surface ocean, e.g. in the Southern Ocean, have no remineralized tracer component.

The cGENIE model still carries a DIC (and $^{13}C_{DIC}$) tracer, which when leaving the surface can accumulate remineralized (regenerated) DIC (and $^{13}C_{DIC}$). In addition to this standard tracer, we include a pre-formed DIC (and $^{13}C_{DIC}$) tracer that indeed does leave the ocean surface initially with no regenerated component and only accumulates regenerated DIC (and $^{13}C_{DIC}$) subsequently. We will make this much clearer in the revised text.

Relative contributions of top-down and bottom-up processes: The authors suggest that air-sea gas equilibration leaves a strong imprint on upper-ocean d13C records, while also acknowledging that bottom-up processes cannot be neglected, more so in some regions over others (e.g., line 174-179). However, the authors focus a lot on the top-down process, while in my view they would be in the position of *quantifying* what the relative contributions of these different processes in *different regions* are (and provide a global map accordingly). This would significantly increase the impact and value of the study, in particular for those researchers working with proxy data. I hence encourage the authors to consider performing these analyses. The study should also better highlight the finding that upper-ocean d13C are ultimately affected by both (top-down and bottom-up) processes but with strongly varying proportions in different regions.

We thank the reviewer for the suggestion. Indeed, showing a relative contribution of net $\delta^{13}C$ anomaly of preformed versus regenerated component would be very helpful for paleo tracer community. However, such a quantitative 'map' for the early deglaciation may very much depend on the models used, boundary conditions and forcing applied. This can already be seen by comparing the LOVECLIM and cGENIE simulations provided in the present study. Nonetheless, based on the zonal sections of the Pacific that show how the net change in $\Delta\delta^{13}C$ breaks down into preformed and regenerated components, we can make some useful qualitative statements such as: "$\Delta\delta^{13}C_{pref}$ dominates the upper 1000m and could account for a 0.3-0.4‰ decline in marine planktic records during the early deglaciation, whereas $\Delta\delta^{13}C_{reg}$ becomes increasingly important at deeper depth" and which we will expand on in the revision. We will also provide comparable zonal sections for the Atlantic and Indian Ocean basins and thereby provide something equivalent to a 'map' (one broken down into zonal sections).

Focus on initial deglacial d13CO2 decline: It is confusing that in places the entirety of the deglacial d13CO2 is discussed although boundary conditions and driving mecha- nisms might differ throughout the deglaciation (e.g. 162-164). I recommend to remove these and instead exclusively focus on the early deglacial d13CO2 change.

We will now focus on the early deglacial part of the record as suggested by the reviewer.

The same (somewhat) applies to the centennial change in pCO2 around 16.2 kyr before present (e.g., 206-208).

Lines 206-208 refer to $\delta^{13}CO_2$ rather than atmospheric CO_2 at 16.2ka. We argue that the centennial negative $\delta^{13}CO_2$ excursion documented by the Taylor glacial record is part of the early deglacial $\delta^{13}CO_2$ change. If the atmospheric bridge is really efficient as we propose, this rapid negative $\delta^{13}CO_2$ excursion should have had a strong influence on the global upper ocean, although a centennial marine signal is not likely to be captured by most of the sedimentary records. The LOVECLIM simulation illustrates nicely that such a centennial marine signal can be visible in the simulated global upper ocean water mass, supporting a highly efficient atmospheric bridge in transporting $\delta^{13}C$ anomaly. Thus, we would like to keep the discussion about the centennial change in $\delta^{13}CO_2$ around 16.2 ka in the revision.

Representation of foraminiferal d13C of true DIC d13C changes: It might be worthwhile to highlight in the manuscript that the one-to-one representation of seawater DIC d13C changes based on foraminiferal d13C is imperfect, more so for planktonics than for benthics (e.g., Bemis et al., 2000; Schmittner et al., 2017). It might be hence useful to clarify whether the trends and/or the magnitude of benthic d13C change resembles atmospheric d13C changes, e.g., in line 261, and whether both can be linked without reservations.

We thank the reviewer for this helpful suggestion. We will add some relevant descriptions in the introduction so that the readers are aware of the issues related to foraminiferal $\delta^{13}C$ records.

The paragraph will be changed along the following lines of:

"Here we term this scenario 'bottom up' transport. In this scenario, the upper ocean at lower latitudes acts as a conduit for ^{13}C-depleted carbon to enter the atmosphere. As a result, benthic foraminifera upper intermediate depths of low latitude oceans should have also recorded such an early deglacial $\delta^{13}C$ decline. We are aware that benthic (Schmittner et al., 2017) and planktic (e.g. Bemis 2000) $\delta^{13}C$ can be complicated by temperature (planktic) and carbonate ion changes (both). Thus foraminiferal $\delta^{13}C$ changes at different parts of the upper ocean may not totally reflect seawater DIC $\delta^{13}C$ changes. Nonetheless, foraminiferal $\delta^{13}C$ changes (especially benthic foraminifera) are highly correlated with seawater DIC $\delta^{13}C$ changes."

Minor comments:

Line 23. Specify the depths that relate to "from depths that are potentially affected by the atmosphere".

We will specify the depths (i.e. upper 1000m) as suggested.

Line 28. I find that the statement "The mechanisms and the chain of events that were responsible for this pCO2 are not well understood" neglects a large body of literature, a large number of existing hypotheses and a wealth of proxy-data in support of some of these. I recommend some more nuance and adjustments to reflect this. E.g. "De- spite xxx, the mechanisms ..." or "Although the leading hypothesis for millennial- and centennial-scale pCO2 rise was suggested to be xxx, the chain of events ..."

The paragraph will be changed along the following lines:

"Atmospheric pCO_2 increased by 80-100 ppm from the last glacial maximum (LGM) to the Holocene (Marcott et al., 2014; Monnin et al., 2001). During the initial ~35ppm rise in pCO_2 rise between 17.2 to 15 ka, ice core records have documented a 0.3‰ decrease in atmospheric $\delta^{13}C$ (Bauska et al., 2016; Schmitt et al., 2012). This millennial-scale trend was punctuated by a rapid 12ppm pCO_2 increase between 16.3-16.1 ka (Marcott et al., 2014) and a 0.2‰ decrease in $\delta^{13}CO_2$ (Bauska et al., 2016). Leading hypotheses that have been proposed to explain the early deglacial carbon cycle perturbation includes increased Southern Ocean ventilation (e.g. Skinner et al., 2010, Burke et al., 2012), poleward shift/enhanced Southern Hemisphere westerlies (Toggweiler et al., 2006, Anderson et al., 2009, Menviel et al., 2018) and reduced iron fertilization (Martínez-García et al., 2014). However, the chain of events is not well understood."

Line 44. I do not think that a clear lead of a d13CO2 decline can be or was documented. I hence recommend removing "initially occurring in the atmosphere"

This statement will be removed.

Line 51. The statement "and the subsequent d13C decline . . ." needs to be revised as it is confusing. How can a d13C decline contribute to pCO2 variability? I recommend changing it to "is a reflection of the evasion of oceanic carbon to the atmosphere, contributing to . . ."

This statement will be removed.

Line 63. Specify what components.

Errors in estimated regenerated DIC will also affect preformed component as the latter is calculated as the difference between simulated DIC and estimated regenerated DIC. We will explicitly clarify this in the revision.

Line 70. "To our knowledge, the origin .." this sentence is confusing and seems out of place. Please revise.

The text will be removed.

Line 72. It is entirely unclear at this stage why a new benthic d13C record has been obtained. This sentence should be moved or the premise of these analyses should be introduced.

One of the main findings of our study is that this fast equilibrium $\delta^{13}C$ route through the atmospheric bridge compared to ocean transport actually affects not only the top layers in the ocean (i.e. where planktic foraminifera live), but also the water column down to perhaps 1000m.

The motivation for presenting a new benthic $\delta^{13}C$ record from upper intermediate Pacific will be clearly described in the Introduction. We will also improve the structure of the paper earlier on to better justify and explain how the new data fits in with the overall methodology.

Line 87. Insufficiencies of the models in representing sub-grid processes are unquestionable. This statement should not be phrased as if they were not.

Our apologies – this is not what we intended to say. The sentence will be changed along the following lines:

"Due to its relatively coarse resolution, the model could mis-represent the high southern latitude atmospheric or oceanic response to a weaker NADW. Enhanced AABW could have occurred due to a strengthening of the SH westerlies, changes in buoyancy forcing at the surface of the Southern Ocean, opening of polynyas, or sub-grid processes."

Line 108. It is entirely unclear why the forcing is limited to the Pacific sector of the Southern Ocean. Please specify. Here for consistency, I recommend changes a similar forcing to Menviel et al., (2018).

In a revised series of experiments, we have now applied salt flux forcing to the entire SO in cGENIE experiments and hence to better align with the LOVECLIM experiment.

Line 120. A full sentence is needed here. Also, DICorg is depleted in 13C not d13C. Line 121. Budget of what?

We will revise the paragraph to address these points.

Line 123: (Dd13Creg) instead of (Dd13C)

Yes, our mistake (which will be corrected).

Line 124: Is d13Corg defined or simulated? Is DIC =DIC total, i.e. reg + preformed? How is 12Corg defined?

DIC=DICtotal=DICreg+DICpref.

In the revision, we will stick to 'DIC' and not additionally use 'DICtotal' to avoid confusion.

In the original submission, $^{12}C_{org}$ was defined as -21‰ that matches the observed modern global mean $\delta^{13}C$ of POC (Goericke & Fry 1994). However, depending on the choice of parameterization, the modelled $\delta^{13}C$ of POC can be different from -21‰ (Dentith et al., 2020). In the revision, to be self-consistent, $^{12}C_{org}$ will be defined as the simulated global mean $\delta^{13}C$ of POC in each model. We thank the reviewer for catching this.

Line 129. 2 and 5 mg CaCO3.

Fixed.

Line 131-132. What suggests that there is no evidence for invariable surface ocean reservoir age changes over the deglaciation? It is not enough to say that. I believe it has to be justified. Also Figure 4 shows a marked lag between the onset of d13C decline in the GeoB17402 and in atmospheric d13CO2. Is this real or an artifact of the age model (i.e., variable reservoir ages?)? I am surprised that there is no men- tion/discussion of this lag in the study.

We now use the new Marine20 calibration curve that incorporates potential reservoir changes to update our age model. However, the lag the reviewer was referring to still exists and we attribute it to a relatively large age model uncertainty below 154cm (median age ~16.2yr), up to 1-2 kyr (2SD)

Line 133. Remove "Once the calendar ages were established the results were plotted vs depth."

Removed.

Line 140. Remove "will be archived in Pangaea" and add URL to appropriate section Data availability.

We will obtain an URL, which will then be added into our revised manuscript.

We will also make the cGENIE experiment configuration files (and instructions for running the experiments) available on GitHub and generate a DOI for this.

Line 142-143. Remove "Below.. " I don't find this helpful here, and the structure of the manuscript can be reflected in the headings.

This sentence will be removed.

Line 149. Which model?

The LOVECLIM model. We will better clarify this in the text.

Line 152-154. I am surprise to see a discussion of entirely new carbon species/terms (Dd13Cthermo and Dd13Cres), which haven't been introduced or mentioned earlier. They need to be properly introduced, otherwise this analysis is entirely confusing, and not very helpful for the reader. They also appear not to be of relevance throughout the remainder of the manuscript, which somewhat questions whether this analysis is needed. It is difficult to follow the statements in the following lines 154- 157: What is meant here with Dd13C? What does the preformed signal reflect? Dd13Cthermo? Please clarify.

This section will be removed for clarity. We were over complicating things unnecessarily with 'Dd13Cthermo and Dd13Cres'.

Line 165. It should be pointed out clearly what observations lead to this major finding.

The $\delta^{13}C$ anomaly in the upper 1000m of the ocean is dominated by the preformed $\delta^{13}C$ signal leads to this finding. We will be more specific in the revision. As we will regarding the novelty of the creation of an explicit preformed $\delta^{13}C$ tracer in an Earth system model.

Line 172. "evolution" instead of "pathway"

We will change the wording as suggested.

Line 188. The d13C decline in the upper 1000 m (where? Does Figure 6 show a global ocean mean?) is also dominated by the preformed signal (everywhere?). Also some more help and explanation with regards to Figure 6 is needed, as it shows four panels.

Figure 6 are the zonal mean Pacific plots simulated by cGENIE, we will make it clear in the caption and in the associated main text.

Line 215-217: Reference to a figure is required.

We will add this.

Line 277-280: Please specify what time interval you refer to here. This also seems like an add-on that is not properly analyzed, and I hence wonder how useful this is. The authors would be in the position to test the different hypotheses of why the Atlantic and Pacific anomalies are so different, but that is entirely glossed over at this stage.

17.2-15ka is the interval. This paragraph is really about how benthic $\delta^{13}C$ records from 2000m of the South Atlantic can be re-interpreted with the insight from the transient simulation. So the Pacific-Atlantic difference is indeed an unnecessary add-on. We will remove the vague discussion in the revision.

References:

Dentith, J. E., Ivanovic, R. F., Gregoire, L. J., Tindall, J. C. and Robinson, L. F.: Simulating stable carbon isotopes in the ocean component of the FAMOUS general circulation model with MOSES1 (XOAVI), Geoscientific Model Development, 13(8), 3529–3552, https://doi.org/10.5194/gmd-13-3529-2020, 2020.

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Rae, J. W. B., Gray, W. R., Wills, R. C. J., Eisenman, I., Fitzhugh, B., Fotheringham, M., Littley, E. F. M., Rafter, P. A., Rees-Owen, R., Ridgwell, A., Taylor, B. and Burke, A.: Overturning circulation, nutrient limitation, and warming in the Glacial North Pacific, Science Advances, 6(50), eabd1654, https://doi.org/10.1126/sciadv.abd1654, 2020.