

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 δ¹³C). This we detail in the point-by-point responses below.

We will also frame the paper much more towards the novel respired δ¹³C 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 δ¹³C changes as well as error inherent in previously published approximation (from regenerated PO₄ to respired δ¹³C) 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.

We agree with the reviewer that winds can affect carbon partitioning through air-sea exchange as well as circulation changes. We realized this further $\delta^{13}\text{C}$ attribution decomposition into thermodynamic, kinetic, biological components was unnecessary and will be removed in the revision.

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 (Buitert et al., 2018); 2) “LH1-SO-SHW” matches some of the important observations (e.g. ice core record of atmospheric pCO_2 and $\delta^{13}\text{CO}_2$) 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 $\delta^{13}\text{C}$ 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 respired $\delta^{13}\text{C}$. 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 respired $\delta^{13}\text{C}$.

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) respired $\delta^{13}\text{C}$ can be estimated in model (in turn based on AOU). 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 $^{13}\text{C}_{\text{DIC}}$) tracer, which when leaving the surface can accumulate remineralized (regenerated) DIC (and $^{13}\text{C}_{\text{DIC}}$). In addition to this standard tracer, we include a preformed DIC (and $^{13}\text{C}_{\text{DIC}}$) tracer that indeed does leave the ocean surface initially with no regenerated component and only accumulates regenerated DIC (and $^{13}\text{C}_{\text{DIC}}$) 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 $\delta^{13}\text{C}$ 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 $\delta^{13}\text{C}$ 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 $\delta^{13}\text{C}$ 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 $\Delta\delta^{13}\text{C}$ breaks down into preformed and respired components, we will make some useful qualitative statements in the revision:

“ $\Delta\delta^{13}\text{C}_{\text{pref}}$ dominates the upper 1000m and could account for a 0.3-0.4‰ decline in marine planktic records during the early deglaciation, whereas $\Delta\delta^{13}\text{C}_{\text{soft}}$ becomes increasingly important at deeper depth”

Focus on initial deglacial $\delta^{13}\text{C}$ decline: It is confusing that in places the entirety of the deglacial $\delta^{13}\text{C}$ is discussed although boundary conditions and driving mechanisms might

differ throughout the deglaciation (e.g. 162-164). I recommend to remove these and instead exclusively focus on the early deglacial $\delta^{13}\text{CO}_2$ 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 pCO_2 around 16.2 kyr before present (e.g., 206-208).

Lines 206-208 refer to $\delta^{13}\text{CO}_2$ rather than atmospheric CO_2 at 16.2ka. We argue that the centennial negative $\delta^{13}\text{CO}_2$ excursion documented by the Taylor glacial record is part of the early deglacial $\delta^{13}\text{CO}_2$ change. If the atmospheric bridge is really efficient as we propose, this rapid negative $\delta^{13}\text{CO}_2$ 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 $\delta^{13}\text{C}$ anomaly. Thus, we would like to keep the discussion about the centennial change in $\delta^{13}\text{CO}_2$ around 16.2 ka in the revision.

Representation of foraminiferal $\delta^{13}\text{C}$ of true DIC $\delta^{13}\text{C}$ changes: It might be worthwhile to highlight in the manuscript that the one-to-one representation of seawater DIC $\delta^{13}\text{C}$ changes based on foraminiferal $\delta^{13}\text{C}$ 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 $\delta^{13}\text{C}$ change resembles atmospheric $\delta^{13}\text{C}$ 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 so that the readers are aware of the potential issues related to foraminiferal $\delta^{13}\text{C}$.

We will change the text to:

“The new benthic $\delta^{13}\text{C}$ record from the intermediate WEP documents a -0.3 to -0.4‰ decline during the early deglaciation (Figure 1c). We are aware that foraminiferal $\delta^{13}\text{C}$ can be complicated by temperature and carbonate ion changes (Bemis et al., 2000, Schmittner et al., 2017), and thus may not solely reflect seawater DIC $\delta^{13}\text{C}$ changes. Nonetheless, foraminiferal $\delta^{13}\text{C}$ changes (especially benthic foraminifera) are highly correlated with seawater DIC $\delta^{13}\text{C}$ changes (Schmittner et al., 2017).”

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 pCO_2 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. “Despite 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 pCO₂ increased by 80-100ppm from the last glacial maximum (LGM) to the Holocene (Marcott et al., 2014; Monnin et al., 2001). During the initial ~35ppm rise in pCO₂ between 17.2 to 15 ka, ice core records also document a contemporaneous 0.3‰ decline in atmospheric δ¹³C (Bauska et al., 2016; Schmitt et al., 2012) (Figure 1a, b, interval highlighted in grey). This millennial-scale trend was punctuated by an interval of more rapid change, with a 12ppm pCO₂ increase (Marcott et al., 2014) and a -0.2‰ decrease in δ¹³CO₂ (Bauska et al., 2016) occurring in an interval of just ~300 years, between 16.4-16.1 ka (Figure 1a, b, interval highlighted in red). Hypotheses proposed to explain these transient perturbations between 17.2 to 15 ka 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 leading to the atmospheric changes recorded in ice cores is not well understood.

Line 44. I do not think that a clear lead of a δ¹³CO₂ 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 δ¹³C decline . . .” needs to be revised as it is confusing. How can a δ¹³C 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 respired DIC will also affect preformed component as the latter is calculated as the difference between simulated DIC and estimated respired 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 $\delta^{13}\text{C}$ 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 $\delta^{13}\text{C}$ 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 $\delta^{13}\text{C}$ 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, DIC_{org} is depleted in $\delta^{13}\text{C}$ not $\delta^{12}\text{C}$. Line 121. Budget of what?

We will revise the paragraph to address these points.

Line 123: (D $\delta^{13}\text{C}_{\text{reg}}$) instead of (D $\delta^{13}\text{C}$)

Yes, our mistake (which will be corrected).

Line 124: Is $\delta^{13}\text{C}_{\text{org}}$ defined or simulated? Is DIC = DIC total, i.e. reg + preformed? How is $\delta^{12}\text{C}_{\text{org}}$ defined?

DIC = DIC_{total} = DIC_{reg} + DIC_{pref}.

In the revision, we will stick to ‘DIC’ and not additionally use ‘DIC_{total}’ to avoid confusion.

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

Line 129. 2 and 5 mg CaCO_3 .

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 $\delta^{13}\text{C}$ decline in the GeoB17402 and in atmospheric $\delta^{13}\text{C}$. Is this real or an artifact of the age model (i.e., variable reservoir ages)? I am surprised that there is no mention/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 $\sim 16.2\text{yr}$), 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 $\delta^{13}\text{C}$ anomaly in the upper 1000m of the ocean is dominated by the preformed $\delta^{13}\text{C}$ 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 $\delta^{13}\text{C}$ 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 $\delta^{13}\text{C}$ 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.

Goericke, R. and Fry, B.: Variations of marine plankton $\delta^{13}\text{C}$ with latitude, temperature, and dissolved CO_2 in the world ocean, *Global Biogeochemical Cycles*, 8(1), 85–90, <https://doi.org/10.1029/93GB03272>, 1994.

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.

Review of MS by Jun Shao et al.

Fortunat Joos

The paper forcefully demonstrates that air-sea gas exchange effectively acts to couple atmospheric and upper ocean $\delta^{13}\text{C}$. This point is, - from a modelling point of view and for all those monitoring the penetration of the anthropogenic perturbation into the ocean - rather trivial and not new. The timescale to bring the surface layer in equilibrium with a $\delta^{13}\text{C}$ perturbation in the atmosphere by air-sea gas exchange is of order 10 years as outlined by Broecker, Peng and others. Numerous measurements of CFCs, bomb-produced radiocarbon, DIC, and notably of $\delta^{13}\text{C}$ demonstrate that the atmospheric perturbation in these tracers is communicated by air-sea gas exchange to the surface layer and by surface-to-deep exchange to deeper layers within years to decades (e.g., (Heimann and Maier-Reimer 1996; Broecker et al. 1985; Eide et al. 2017)). Thus it is clear from an observational as well as from a modelling point that air-sea gas exchange is important and needs to be considered when addressing carbon isotopes. Unfortunately, the role of air-sea exchange is sometimes neglected in the interpretation of marine planktonic $\delta^{13}\text{C}$ records. It may therefore be appropriate to recall this point for the paleoceanographic tracer community.

We thank the reviewer for his very thorough and helpful review. We agree that the principle behind a dominant role of air-sea gas exchange on surface $\delta^{13}\text{C}$ is quite well established in theory and is 'well known' in the modeling community (e.g. Schmittner et al., 2013). We are very happy to revise the manuscript to include these points and better highlight the coupling between ocean and atmosphere. However, in the specific hypotheses we address, the situation is subtly different – although eventually the signature of isotopically depleted carbon release must reach the atmosphere and hence be re-transmitted to the entire global surface ocean (via air-sea gas exchange), the question is whether the observed $\delta^{13}\text{C}$ decline in specific planktic and shallow/intermediate depth benthic records reflect the 'end point' (following ocean invasion of the signature globally from the atmosphere), or whether they reflect a location on the pathway of carbon release from the ocean (and hence prior to invasion to the atmosphere and global surface re-equilibrium). We admit this was not fully clear in the original text and will discuss the competing hypotheses (plus sequence of events, and the inevitable role of air-sea gas exchange) much more clear.

Interesting is that the authors offer a quantification of the influence of preformed versus remineralized changes in $\delta^{13}\text{C}$. However, the method applied to separate changes in $\delta^{13}\text{C}$ into the contribution from preformed sources and sources from biogenic particles is unclear and may not be appropriate (see below).

In the cGENIE model the numerical tracer is exact by definition, but we agree that there are a number of caveats that we did not discuss when deriving regenerated $\delta^{13}\text{C}$ from other modelled metrics (as we do in LOVECLIM). We address this in response to the detailed Referee comments below.

Another interesting point, which deserves a bit more discussion, is the information on the change in $\delta^{13}\text{C}$ versus the change in atmospheric CO_2 in response to an increase in deep ocean ventilation (AABW, AAIW) forced by prescribed changes in salt and Southern Ocean wind stress.

I recommend major revisions. Specific comments:

1) Attribution of the $\delta^{13}\text{C}$ changes.

Section 2.3 The separation into preformed and remineralized $\delta^{13}\text{C}$ from the model output of DIC and $\delta^{13}\text{C}$ appears problematic. The assumption and simplifications of the approach are not explained to the readers

We will fully rectify this.

It would be preferable to simulate the preformed tracers online.

We do in cGENIE. But the historical/published nature of the LOVECLIM experiments means that we are unable to simulate the preformed tracers online for LOVECLIM (hence the use of cGENIE to elucidate the errors in the approximation we then must use).

1. a) The authors use the equations given in line 124 to estimate the change in $\delta^{13}\text{C}$ due to a change in remineralization. However, the equation is unclear. A new term is used in this equation: “ $^{12}\text{C}_{\text{Org}}$ ”. I guess $^{12}\text{C}_{\text{Org}}$ should read DIC_{Org} , the amount of remineralized carbon. Then the equation given by the authors reads:

$$\delta^{13}\text{C}_r = \delta^{13}\text{C}_{\text{Org}} * \Delta(\text{DIC}_{\text{Org}} / \text{DIC}) \quad (1)$$

We admit some sloppiness with the notation, which we will correct and much better clarify.

2. b) How is $\delta^{13}\text{C}_{\text{Org}}$ computed? I get the impression that a constant $\delta^{13}\text{C}$ signature of organic carbon ($\delta^{13}\text{C}_{\text{Org}}$) is assumed in the approach applied to distinguish the preformed and remineralized components in LOVECLIM.

As per earlier comments (and also subsequent replies) – we will extensively revise the text to more logically and explicitly detail: the role of cGENIE vs. LOVECLIM, how the preformed tracers are simulated in cGENIE, how respired $\delta^{13}\text{C}$ is estimated in LOVECLIM and what the error (based on cGENIE analysis) are.

c) Any perturbation in surface $\delta^{13}\text{C}$ is also transferred to newly formed organic matter and CaCO_3 and finally to remineralized carbon. In addition, changes in surface CO_2 affect the fractionation factor for organic matter formation and thereby again $\delta^{13}\text{C}_{\text{org}}$. Further changes in surface $\delta^{13}\text{C}$ also affect changes in the signature of preformed fluxes. It appears that changes in the isotopic signature of preformed and remineralized carbon fluxes are neglected. This seems an oversimplification.

We agree, and it was an omission not to discuss this in the original manuscript. (We note that the cGENIE preformed tracer analysis encapsulates *all* errors, but these were not broken down as highlighted by the Referee.)

d) The equation on line 124 used to compute the change in remineralized $\delta^{13}\text{C}$ needs to be properly derived. The mass balance should be considered in the separation of the different components. I distinguish preformed DIC (DIC_p ; index p) and remineralized DIC (DIC_r ; index r) and related fluxes (F_p, F_r).

Changes in $\delta^{13}\text{C}$ can arise due to changes in the carbon fluxes, but also due to changes in the signature of the carbon fluxes.

Let us consider a single box of Volume V and inorganic C concentration DIC with an isotopic signature $\delta^{13}\text{C}$. The fluxes of preformed DIC entering and leaving the box are denoted $F_{p,i}$, with i an index for the different fluxes covering all fluxes by diffusion, advection, and convection entering or leaving the box. Their signature is $\delta^{13}\text{C}_{p,i}$. For simplicity, we consider one flux of remineralized carbon entering the box, F_r , with the signature $\delta^{13}\text{C}_{r,\text{in}}$. Mass balance is then given by:

$$V \cdot \frac{d}{dt}(\text{DIC}) = \text{Sum}(F_{p,i}) + F_r \quad (2a) \quad V \cdot \frac{d}{dt}(\text{DIC} \cdot \delta^{13}\text{C}) = \text{Sum}(F_{p,i} \cdot \delta^{13}\text{C}_{p,i}) + F_r \cdot \delta^{13}\text{C}_{r,\text{in}} \quad (2b)$$

Subtracting steady state fluxes and considering the change (Δ) over one time step of length Δt , we get with $\Delta(\text{DIC}) = \Delta F/V \cdot \Delta t$:

$$\Delta(\text{DIC}) = \Delta(\text{DIC}_p) + \Delta(\text{DIC}_r) \quad (3a) \quad \Delta(\text{DIC} \cdot \delta^{13}\text{C}) = \Delta t/V \cdot \Delta(\text{Sum}(F_{p,i} \cdot \delta^{13}\text{C}_{p,i}) + F_r \cdot \delta^{13}\text{C}_{r,\text{in}}) \quad (3b)$$

Linearising (3b) and using again $\Delta(\text{DIC}) = \Delta F/V \cdot \Delta t$, we get:

$$\Delta(\text{DIC}) \cdot \delta^{13}\text{C} + \text{DIC} \cdot \Delta(\delta^{13}\text{C}) = \Delta(\text{DIC}_p) \cdot \delta^{13}\text{C}_{p,\text{in}} + \Delta(\text{DIC}_r) \cdot \delta^{13}\text{C}_{r,\text{in}}$$

$$\Delta t/V \cdot \text{Sum}(F_{p,i} \cdot \Delta(\delta^{13}\text{C}_{p,i})) + \Delta t/V \cdot F_r \cdot \Delta(\delta^{13}\text{C}_{r,\text{in}}) \quad (4)$$

The first two rhs terms in (4) describe the change in isotopic mass due to the addition of carbon by the perturbed preformed and remineralized carbon fluxes. The last two rhs terms describe the change due to the change in the signature of the preformed and remineralized carbon fluxes. Equation 4 has many unknowns ($\Delta(\delta^{13}\text{C}_r)$, $\Delta(\delta^{13}\text{C}_p)$, $\delta^{13}\text{C}_{r,\text{in}}$ and $\delta^{13}\text{C}_{p,\text{in}}$...). Thus, it

seems not possible to attribute the change in $\delta^{13}\text{C}$ to preformed and remineralized components in an exact way without carrying a separate preformed $\delta^{13}\text{C}$ tracer in the model.

(Indeed, hence the inclusion of the new preformed $\delta^{13}\text{C}$ tracer in cGENIE.)

Perhaps it is justified to make approximations. We may assume that

$$\Delta(\text{DICp}) * \delta^{13}\text{C}_{\text{p,in}} \ll \Delta(\text{DICr}) * \delta^{13}\text{C}_{\text{r,in}}. \quad (5)$$

This is probably o.k. as $\delta^{13}\text{C}_{\text{p,in}}$ is close to zero permil and $\delta^{13}\text{C}_{\text{r,in}}$ is about -20 permil for organic material.

It is much less clear whether also the terms with the changes in the isotopic signatures in eq. (4) can be neglected. The changes in $\delta^{13}\text{C}$ may be small, but they are multiplied with the total carbon fluxes (F_r , $F_{p,i}$) and not just with the perturbations in the carbon fluxes. Therefore, these terms may be very significant. Nevertheless, let us assume for the moment these two terms are negligible. In this case, we get:

$\Delta(\text{DIC}) * \delta^{13}\text{C} + \text{DIC} * \Delta(\delta^{13}\text{C}) \sim \Delta(\text{DICr}) * \delta^{13}\text{C}_{\text{r,in}}$ and the solution for $\Delta(\delta^{13}\text{C})$ is:

$$\Delta(\delta^{13}\text{C}) \sim \Delta(\text{DICr}) / \text{DIC} * (\delta^{13}\text{C}_{\text{r,in}} - \delta^{13}\text{C}) - \Delta(\text{DICp}) / \text{DIC} * \delta^{13}\text{C} \quad (6)$$

Eq. 6 is somewhat similar to the eq. (1) above and given on line 124 in the MS, when setting $\delta^{13}\text{C}_{\text{org}} = \delta^{13}\text{C}_{\text{r,in}} - \delta^{13}\text{C}$. This difference in isotopic signatures of the material remineralized and of the isotopic signature of DIC should be considered. In particular, in the upper ocean $\delta^{13}\text{C}$ of DIC is different from zero.

In addition, it seems that the parentheses are not properly set in (1) and eq. 1 should rather read $\delta^{13}\text{C}_{\text{r}} = \delta^{13}\text{C}_{\text{org}} * \Delta(\text{DIC}_{\text{org}}) / \text{DIC}$.

Further, the second rhs term of (6) is neglected in (1). The second rhs term in eq. 6 may be small as typical source signatures are between 0 and 2 permil in the upper ocean. However, it seems easy to account for in the evaluation of $\Delta(\delta^{13}\text{C})$.

In conclusion, the calculation of the change in $\delta^{13}\text{C}$ attributable to organic matter remineralization and to preformed fluxes must be revised. It remains the task of the authors to demonstrate that changes in the isotopic signature of the preformed and remineralized fluxes can either be safely neglected (as done to get eq. (6)) or otherwise to properly account for their influence.

We intend to address this via a full attribution analysis of the factors influencing $\delta^{13}\text{C}$ in the ocean. Using cGENIE, we will start by elucidating the error terms involved in making the step (employed in LOVECLIM): $\text{AOU} \rightarrow \text{regenerated PO}_4$, and in the context not only of pre-industrial steady-state conditions, but under transient deglacial-like boundary condition changes. We will then do similarly (using cGENIE) for the step: $\text{regenerated PO}_4 \rightarrow \text{respired } \delta^{13}\text{C}$. To fully break down the error terms (as outlined above by the Refree), we will carry out a series of cGENIE experiments in which we should be able to explicitly elucidate all the contributions to

changing ocean $\delta^{13}\text{C}$ and hence in respect in reconstructing regenerated $\delta^{13}\text{C}$ from AOU in LOVECLIM, not only what the net error is (which we included in the submitted manuscript), where how large and from where the contributing terms arise (will be included in the Supplement).

My recommendation is to explicitly include preformed tracers in LOVECLIM and then to repeat the simulation shown in figure 1 with the preformed tracers enabled in this model of intermediate complexity.

In an ideal world, yes. However, implementing new tracers in LOVECLIM and then repeating a previously published experiment is not practical and would require several months of work and run-time. We see our approach as analogous to the CMIP/PMIP series of model inter-comparison experiments, where published experiments are subsequently ‘mined’ and reanalyzed (and typically without re-coding and re-running). Our methodology is somewhat aligned with this workflow.

The simulations have been published before and the separation of $\delta^{13}\text{C}$ changes into preformed and remineralized components is the main point of this paper. Thus, this separation should be done properly to make this manuscript publishable.

Accepted. See above for the additional cGENIE modelling that we propose to fully elucidate the different sources of error involved in approximating regenerated $\delta^{13}\text{C}$. The advantage of this approach over a single model run presenting only an explicit (numerical tracer) preformed $\delta^{13}\text{C}$ tracer based analysis is that it gives us the chance to evaluate how different processes control $\delta^{13}\text{C}$ distributions and changes in the ocean (as outlined by the Referee above) together with the uncertainties involved in published approaches of approximating based on AOU or preformed phosphate tracers.

As to the specific mass balance calculations, we will modify the relevant method section as:

We assume the following carbon isotopic mass balance:

$$\delta^{13}\text{C} * \text{DIC} = \delta^{13}\text{C}_{\text{pref}} * \text{DIC}_{\text{pref}} + \delta^{13}\text{C}_{\text{soft}} * \text{DIC}_{\text{soft}} + \delta^{13}\text{C}_{\text{carb}} * \text{DIC}_{\text{carb}} \quad (1)$$

where DIC, DIC_{pref} , DIC_{soft} , and DIC_{carb} , are the dissolved total inorganic carbon, the preformed, respired organic matter (‘Csoft’), and dissolved (calcium) carbonate carbon pools, respectively. $\delta^{13}\text{C}_{\text{pref}}$, $\delta^{13}\text{C}_{\text{soft}}$, and $\delta^{13}\text{C}_{\text{carb}}$, are the corresponding isotopic signatures (as ‰) that contribute to the $\delta^{13}\text{C}$ signature of DIC. It is changes in the $\delta^{13}\text{C}$ of DIC that we assume foraminiferal records reflect.

Any given observed $\delta^{13}\text{C}$ anomaly in the ocean can then be expressed as:

$$\Delta\delta^{13}\text{C} = \Delta(\delta^{13}\text{C}_{\text{pref}} * \text{DIC}_{\text{pref}} / \text{DIC}) + \Delta(\delta^{13}\text{C}_{\text{soft}} * \text{DIC}_{\text{soft}} / \text{DIC}) + \Delta(\delta^{13}\text{C}_{\text{carb}} * \text{DIC}_{\text{carb}} / \text{DIC}) \quad (2)$$

The terms on the RHS represent the contribution of the preformed, respired, and dissolved (carbonate) components to the overall $\delta^{13}\text{C}$ change, respectively. Since there is no ^{13}C fractionation during CaCO_3 formation in the LOVECLIM model, the last term on the RHS can be assumed to be zero (see SI).

We use AOU to estimate respired carbon and its contribution to the $\delta^{13}\text{C}$ changes: $\Delta(\delta^{13}\text{C}_{\text{soft}} * \text{DIC}_{\text{soft}} / \text{DIC}) = \Delta(\delta^{13}\text{C}_{\text{soft}} * \text{AOU} * R_{\text{C:-o2}} / \text{DIC})$, where $\delta^{13}\text{C}_{\text{soft}}$ is estimated by the $\delta^{13}\text{C}$ of export POC in the overlying water column, $R_{\text{C:-o2}} = 117\text{:}-170$.

This leads to:

$$\Delta\delta^{13}\text{C} = \Delta(\delta^{13}\text{C}_{\text{pref}} * \text{DIC}_{\text{pref}} / \text{DIC}) + \Delta(\delta^{13}\text{C}_{\text{soft}} * \text{AOU} * R_{\text{C:-o2}} / \text{DIC}) \quad (3)$$

The anomaly, defined as the difference between 15 and 17.2 ka, can be expanded as:

$$\delta^{13}\text{C}^{15\text{ka}} - \delta^{13}\text{C}^{17.2\text{ka}} = \delta^{13}\text{C}_{\text{pref}}^{15\text{ka}} * \text{DIC}_{\text{pref}}^{15\text{ka}} / \text{DIC}^{15\text{ka}} - \delta^{13}\text{C}_{\text{pref}}^{17.2\text{ka}} * \text{DIC}_{\text{pref}}^{17.2\text{ka}} / \text{DIC}^{17.2\text{ka}} + \delta^{13}\text{C}_{\text{soft}}^{15\text{ka}} * \text{AOU}^{15\text{ka}} * R_{\text{C:-o2}} / \text{DIC}^{15\text{ka}} - \delta^{13}\text{C}_{\text{soft}}^{17.2\text{ka}} * \text{AOU}^{17.2\text{ka}} * R_{\text{C:-o2}} / \text{DIC}^{17.2\text{ka}} \quad (4)$$

It is well known that AOU likely overestimates the true oxygen utilization, and thus DIC_{soft} , particularly in water masses formed in high latitudes (Bernardello et al., 2014; Ito et al., 2004; Khatiwala et al., 2019, Cliff et al., 2021). However, to what extent these biases will affect the relative contribution of preformed and respired carbon pool on $\delta^{13}\text{C}$ anomaly in a carbon cycle perturbation event has not to our knowledge previously been evaluated. To address this, we conducted a benchmark test with cGENIE, which explicitly simulates the contribution of respired carbon to $\delta^{13}\text{C}$ of DIC. Specifically, we performed a similar deglacial transient simulation (see section 2.4) and then applied equation (4) to the cGENIE output; the results are then compared with the values that are explicitly simulated by cGENIE. We also conducted a simplified (modern configuration based) analysis of steady state and transient error terms, which we include in SI.

2) Line 149-158: The authors separate surface ocean $\delta^{13}\text{C}$ change into a thermodynamic equilibrium component and a residual component. I am puzzled by the interpretation offered by the authors. The authors state that the residual component mainly reflects changes in primary productivity. This is not demonstrated but only inferred from simulated changes in productivity. The balance between the input of PO_4 by upwelling and consumption of PO_4 by export leads to a positive PO_4 anomaly in the SO surface ocean (Fig 3d). Correspondingly, the balance of upwelling and export alone leads to a negative $\delta^{13}\text{C}$ anomaly in the SO ocean surface. It is unclear to which extent incomplete air-sea exchange contributes to this residual component.

It's true that in the model, even though the surface productivity increased, the overall efficiency of the biological pump decreases when deep ocean overturning rate increases. It's also true that incomplete air-sea exchange is somewhat ignored in this separation. To focus on the main point of this paper, we will remove this part in the revision. With hindsight, this decomposition was one step in $\delta^{13}\text{C}$ attribution too far and wholly unnecessary (and confusing as also remarked upon by Referee #1).

3) The LOVECLIM simulation is forced by prescribed changes in wind stress and salt fluxes (Fig. 1a,b). This triggers a change in Southern Ocean upwelling and deep ocean ventilation (as e.g., reflected by AABW changes in Fig. 1c). One may then ask which part of the early deglacial CO₂ rise may be explained by such a change in deep ocean ventilation.

The ratio between the change in atmospheric $\delta^{13}\text{C}$ and CO₂ is interesting as this ratio can be directly compared with ice core data as done in previous work.

The change in deep ocean ventilation and Southern Ocean upwelling enforced by prescribed wind stress and freshwater forcing causes $\delta^{13}\text{C}$ to decline by 0.35 and CO₂ to increase by 25 ppm in the LOVECLIM simulation (Fig 1c). This yields a ratio of 7 ppm per 0.1 permil decline. (Tschumi et al. 2011) performed similar idealized simulations where Southern Ocean overturning was changed by prescribed changes in boundary conditions. They found a ratio of 13 ppm per 0.1 permil decline. The ice core data suggest an increase in CO₂ of 35 ppm and a decrease in $\delta^{13}\text{C}$ of 0.3 permil during the early deglacial period. This corresponds to a ratio of 12 ppm per 0.1 permil decline. Tschumi et al. suggested that the entire increase in CO₂ of 35 ppm during the early deglacial was due to enhanced Southern Ocean upwelling. The LOVECLIM results suggest a smaller contribution of SO upwelling to the early deglacial CO₂ rise. According to LOVECLIM only 25 ppm of the deglacial CO₂ rise are attributable to the prominent Southern Ocean upwelling hypothesis.

This issue should be discussed in section 4.

The different $\Delta\text{pCO}_2/\Delta\delta^{13}\text{CO}_2$ sensitivity the reviewer is referring to can be mainly explained by different initial conditions in the two studies - Tschumi et al., 2011 applied pre-industrial conditions while the transient LOVECLIM simulation analyzed in this study started from a LGM state, that was benchmarked against benthic $\delta^{13}\text{C}$ data. Deep ocean $\delta^{13}\text{C}$ is $\sim 0.6\%$ lighter at the LGM than the Holocene (Peterson et al., 2014). Therefore, for the same magnitude of atmospheric pCO₂ increase through enhanced SO upwelling, $\delta^{13}\text{CO}_2$ decline in LOVECLIM is larger. Appropriate initial conditions are necessary to investigate the carbon cycle perturbation that led to a negative excursion in $\delta^{13}\text{CO}_2$ during the early deglaciation.

Menviel et al. 2015 present the atmospheric pCO₂, $\delta^{13}\text{CO}_2$ and oceanic $\delta^{13}\text{C}$ responses to changes in oceanic circulation in LOVECLIM and the Bern3D under pre-industrial conditions. As seen in their figure 4, for changes in Southern Ocean buoyancy forcings, $\Delta\text{pCO}_2/\Delta\delta^{13}\text{CO}_2$ ratio is +16ppm per 0.1‰ decline in LOVECLIM and +8.3ppm per 0.1‰ decline in Bern3D; for changes in SH westerlies, $\Delta\text{pCO}_2/\Delta\delta^{13}\text{CO}_2$ ratio is +10ppm per 0.1‰ decline in LOVECLIM.

As noted by the reviewer (and unfortunately not included in Menviel, et al. 2015), changes in SH westerlies lead to a 12 ppm CO₂ increase for a 0.1‰ $\delta^{13}\text{CO}_2$ decrease in the Bern3D as per Tschumi et al., 2011. The (slight) difference in sensitivity between the models could come from differences in the initial oceanic $\delta^{13}\text{C}$ distributions with a negative $\delta^{13}\text{C}$ bias in the equatorial regions at intermediate depth in LOVECLIM (see figure 3 of Menviel et al., 2015).

We think changes in atmospheric pCO₂ and $\delta^{13}\text{CO}_2$ during the early part of the deglaciation represent the integrated signal of different processes: a significant AMOC weakening, an increase in SO ventilation, a decrease in SO sea-ice cover, an increase in globally averaged SST,

and changes in the terrestrial biosphere. While idealized simulations of changes in SH westerlies provide information on the probable $\Delta p\text{CO}_2/\Delta\delta^{13}\text{CO}_2$ change, the actual integrated response is more complex.

Although this topic is of interest, it is more related to published work rather than the main point of this paper - $\delta^{13}\text{CO}_2$ decline during the early deglaciation can account for most of the marine planktic and shallow/intermediate benthic $\delta^{13}\text{C}$ decline in the global ocean. Given the structure and emphasis of the paper, we don't think such a discussion fits in the manuscript.

Minor comments:

Intro: It is suggested to change the framing of the introduction. It should be clearly pointed out that it is very well established by the modelling community and by those addressing the anthropogenic carbon perturbation that air-sea gas exchange influences $\delta^{13}\text{C}$, but that this well-established fact is sometimes neglected in interpretation of planktonic $\delta^{13}\text{C}$ records. It would be appropriate to recall the typical equilibration time of 10 yr for $\delta^{13}\text{C}$ in the surface layer by air-sea gas exchange and the typical decadal timescale of surface-to-thermocline transport as revealed by observations of anthropogenic tracers.

We are happy to make appropriate changes to the Introduction as part of framing the competing hypotheses more clearly and the role of ocean surface / atmosphere equilibrium.

The revised text has been provided in our response to the reviewer's major comments above.

L51-52: I do not understand the conclusion that atmospheric CO_2 is not affected. If atmospheric CO_2 (and $\delta^{13}\text{C}$) varies/is perturbed, e.g., by outgassing in the Southern Ocean, then the CO_2 perturbation will like the $\delta^{13}\text{C}$ perturbation enter the upper ocean.

The relevant text will be changed to:

“The ‘top down’ scenario has very different implications from ‘bottom up’. Firstly, a negative $\delta^{13}\text{C}$ need not be associated with enhanced nutrient supply to the upper ocean (on the principal that nutrients are trapped in some deep ocean reservoir along with isotopically depleted carbon). Secondly, a ‘top down’ scenario does not require a specific initial path of carbon to the atmosphere. Outgassing to the atmosphere could occur anywhere at the ocean surface, with a negative $\delta^{13}\text{C}$ signal then propagating globally through air-sea gas exchange – akin on-going fossil fuel CO_2 emissions and the propagation of its isotopically depleted signal down through the ocean (Eide et al., 2017).”

L57: This is a somewhat odd description of the preformed component. The preformed component reflects the balance between all tracer sources and sinks in a surface grid cell. Upwelling and exchange with the deeper layers are generally equally important as air-sea exchange and export production. Why highlighting the terms thermodynamic equilibrium and primary productivity? Would it not be more appropriate to mention air-sea gas exchange and new or export production as well as physical tracer exchange between surface and deeper layers?

This part will be removed (and as per our reply above).

L109: “The atmosphere is held constant ..allowed to evolve freely” This text is unclear. Do you mean in the experiment “fix” atm. CO₂ and δ¹³C is kept constant and in exp. “free” CO₂ and δ¹³C evolve freely?

Yes exactly. We will better clarify this in revision.

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