I thank the authors for revising the manuscript. This manuscript has now developed into a nice paper that illustrates forcefully and convincingly the important imprint of air-sea gas exchange on upper ocean d13C.

It is nice to see that the mass balance of 13C is now treated correctly.

I have a few remaining comments that the authors may wish to consider.

We thank the reviewer for the appraisal of our work! We also appreciate the insightful suggestions made by the reviewer.

Main manuscript (line numbering refers to track changed MS)

1) l. 14 and other places: suggest to write: "a ~35ppm rise in atmospheric CO2" instead of "..pCO2" as ppm units are for a mixing ratio and not a partial pressure.

We have corrected the relevant text throughout the revised manuscript.

2) Line 487: "Since there is no 13C fractionation during CaCO3 formation in the LOVECLIM model, the last term on the RHS can be assumed to be zero (see Supplement)."

This holds for steady. The isotopic signature will change during the experiment and this change is carried downward by the CaCO3 flux (irrespective of the assumed fractionation). As described in the SI, the contribution of CaCO3 dissolution to DIC is small in the upper 1000 m and therefore the last term can also be neglected in transient experiments and the upper ocean. The authors may provide a more complete explanation.

Thanks for pointing this out. The relevant text has been changed to "Since the contribution of $CaCO_3$ dissolution is small in the upper 1000m (where GeoB17402-2 is located) in carbon cycle models (see also the Supplement), and there is no ^{13}C fractionation during $CaCO_3$ formation in the LOVECLIM model, the last term on the RHS can be neglected for the purpose of this study."

Supplementary Information:
1) SI: line 57: "From this we confirm, as expected, that DIC = DIC(pref) + DIC(Csoft), and d13C(DIC) = d13C(pref) + δ13C(Csoft)." I guess a qualifier is needed: "in the upper 1000 m" as CaCO3 dissolution may contribute significantly in the deep.

We admit this was not entirely clear in the text, and we intended this short paragraph to describe a simple numerical check carried out without $CaCO_3$ (and hence $\delta^{13}C_{(carb)}$) existing in the model. In the absence of any formation and dissolution of $CaCO_3$, the statement was correct. We have made this <u>much</u> clearer in the revised text and apologize for the ambiguity.

2) SI, line 70-90: In my opinion, it would be helpful to show here explicitly the equations how the different components are computed.
We now include equations for derived tracers #1-6 and have significantly expanded upon and hopefully further clarified all the tracer descriptions and applications.

3) Please include #7 and provide the equation describing how d13C_soft is computed from the explicitly simulated DI13C_soft and DIC_soft and DIC tracers.

We are not entirely sure what is being requested here. #5 (and newly added equation) describes how $\delta^{13}C_{(Csoft)}$ is estimated from AOU (and hence $DIC_{(Csoft)}$) together with $\delta^{13}C_{(Corg)}$ and DIC, while #6 (and newly added equation) describes how $\delta^{13}C_{(Csoft)}$ is estimated from the Csoft tracer ($DIC_{(Csoft)}$) together with $\delta^{13}C_{(Corg)}$ and DIC. The only other $\delta^{13}C_{(Csoft)}$ is the explicitly simulated numerical tracer described in an earlier section of SI.

We have revised and expanded upon the entire SI section and hope this implicitly addresses the request.

4) SI, line 93: "As is widely appreciated, AOU overestimates the consumption of oxygen through respiration as a result of incomplete equilibrium occurring between the ocean surface and overlying atmosphere." This explanation is only partly correct. It should also be mentioned in addition that the solubility of O2 is nonlinear. For example, mixing two O2 saturated water bodies with different temperatures will lead to an O2 concentration that deviates from the saturated concentration.

Thanks for pointing this out. In fact, we have greatly expanded on this discussion and also now include zonal mean fields of the AOU error itself for completeness.

Rereview of CPD manuscript 10.5194/cp-2020-95 by Shao et al.

With great interest I have reread the revised manuscript of Shao et al. and their response to my comments on the first submitted draft of the manuscript. The authors have responded efficiently and satisfactorily to many of my earlier points of criticism. The use of the LOVECLIM simulation LH1-SHW-SO is much better motivated, the logic behind a comparison of LOVECLIM and cGENIE output data is much clearer, and the paper benefits from the additional error analysis (although those should be part of the discussion rather than just a bunch of figures in the supplement). I am further glad to see that new simulations were performed with cGENIE and that the paper now focusses on the initial deglacial d13CO2 decline, which makes it clearer and more streamlined.

Thanks for the supportive comments. We also appreciate the insightful suggestions made by the reviewer.

However, on some aspects, as outlined below, the authors made, in my view, an insufficient effort to improve the impact and clarity of the study. I also noticed one other weakness, namely in the results section that includes a number of discussion elements and does not give a full account of the model results and observations that are relevant for the study.

Encouraged by the reviewer, we have expanded the results section such that the description of d13C anomaly as well as its decomposition for all ocean basins (see the new Figure3-5) are now presented for both models.

The referred 'discussion elements' previously in the result section have also been moved to the discussion section. See our response to the comments below.

Earlier I have raised concerns regarding the premise of the paper, the distinction between preformed and regenerated ocean carbon in an early deglacial transient simulation with LOVECLIM, when this model version does not simulate those species explicitly. This has been echoed by the second reviewer, who even recommended to rerun the simulations with an explicit tracer scheme for preformed and regenerated carbon. I accept the authors notion of going forward with the paper setup as is,

We appreciate that the reviewer agrees with our approach.

but I recommend to specify in line 175-177 the restrictions of the AOU approach. This is important and acknowledges that the authors approach for carbon partitioning of the LOVECLIM data is imperfect. I suggest to clearly outline the reasons why AOU (as difference between in-situ and calculated oxygen) can in some instances not be a faithful representation of regenerated carbon, and may overestimate it, as discussed in the mentioned literature.

Agree, we have changed the text to "The AOU approach to estimate respired carbon content assumes that the oxygen content of surface waters always reach equilibrium with the overlying atmosphere. However, studies have shown that this is not the case, particularly for water masses formed in high latitudes (Bernardello et al., 2014; Ito et al., 2004; Khatiwala et al., 2019, Cliff et al., 2021). As a result, AOU likely overestimates respired carbon content in the deep ocean. Additional errors associated with the AOU approach may result from the non-linear solubility of O_2 and respiration that does not involve O_2 consumption (i.e. through denitrification or sulphate reduction) (Shiller, 1981; Ito et al., 2004)."

We have also greatly expanded on this discussion in the SI and also now include zonal mean fields of the AOU error itself for completeness.

I am somewhat disappointed by the authors response to my suggestion to better carve out the relative contributions of top-down and bottom-up processes on oceanic d13C, because of the fact that this "may very much depend on the models used". The authors would have two models at hand to discuss this.

In this study, the 'top-down' and 'bottom-up' refer to two potential pathways of light d13C transport in the upper ocean. In the expanded result section, we now describe the $\Delta\delta^{13}C_{soft}$ and $\Delta\delta^{13}C_{pref}$ patterns in detail for both models. Both models show that $\Delta\delta^{13}C_{pref}$ dominates in the upper 1000m of the global ocean, which points to a top-down control. The models also show some $\Delta\delta^{13}C_{soft}$ and $\Delta\delta^{13}C_{pref}$ signals in the deep and abyssal ocean. However, the signals are more related to $\delta^{13}C$ changes in the source waters, water mass mixing ratios and ocean ventilation state. As those changes are not the focus of this study, we chose to only briefly describe those features in the main text. Nonetheless, $\Delta\delta^{13}C$, $\Delta\delta^{13}C_{soft}$ and $\Delta\delta^{13}C_{pref}$ in both models are documented in the revised plots.

I cannot follow statements in the paper such as "Subsequently, air-sea exchange dominates the δ13C decline in the global upper ocean. (line 253)" when the cause of the d13C decline is outgassing in the Southern Ocean (LOVECLIM model). Should it say in the global upper ocean outside the Southern Ocean?

Yes, the relevant text has been changed to
"We show that in this scenario the isotopic signal is first transmitted to the atmosphere through strong outgassing in the Southern Ocean (Figure 6). The atmosphere then transmits the $\delta^{13}C$ signal to the rest of the global surface and subsurface ocean through air-sea gas exchange."

I am slightly confused, and was hoping for more clarification on the regions where outgassing occurs and where the water column is overprinted. In other words, the trigger for an atmospheric d13C bridge must be outgassing in parts of the ocean, where upper ocean d13C must see the regenerated d13C and DIC from below.

Exactly, the trigger for an atmospheric d13C bridge is through outgassing in the Southern Ocean in both LOVECLIM and cGENIE. And the upper Southern Ocean 'sees' the regenerated d13C and DIC from below.

In section 4.1, we now describe the strong outgassing in the Southern Ocean right before 'atmospheric d13C bridge' is introduced. We also revise the abstract so that this point is clearly conveyed to the readers.

"Here we present modeling evidence to show that rather than respired carbon from the deep ocean propagating directly to the upper ocean prior to reaching the atmosphere, the carbon would have first upwelled to the surface in the Southern Ocean where it enters the atmosphere. In this way the transmission of isotopically light carbon to the global upper ocean was analogous to the on-going ocean invasion of fossil fuel CO_2."

The water masses are likely overprinted by the atmosphere (essentially by their own signal), but it is not clear by how much. I find explanations around this issue confusing, e.g. in line 316-319: why would the atmospheric d13C_CO2 signal be compensated by the upwelling d13C_CO2 signal from the deep, when both should have a similar negative signature? Are we talking in the EEP about a gas exchange of water masses from the sub-surface (with a regenerated signal) that equilibrates with an already decreased atmospheric d13C_CO2 (acquired somewhere else)? Or has that sub-surface signal itself acquired a negative preformed signal through atmospheric CO2 overprints outside the EEP?

This is a great point. The negative d13C signal in the surface EEP can come from the gross gas exchange with an already decreased atmospheric d13C-CO2 and/or sub-surface waters that acquired a negative preformed signal at other parts of the global surface ocean. We clarify this point in the revision in the main text in section 4.2:

“On the other hand, the EEP thermocline is also shallow enough to record an atmospheric $\delta^{13}C$ signal, either directly through gas exchange at the surface or indirectly through a preformed signal acquired from other parts of the global surface ocean.”

In general, I believe the paper would benefit from a more careful and in-depth explanation of the processes at play in the different ocean region, with model data support in the form of informative figures/maps. The authors mention the study of Martinez-Boti et al., (2015) which show an pCO2 oversaturation with respect to the atmospheric of 80 ppm at the beginning of the early deglaciation. This is evidence for the fact that the water mass has not fully equilibrated with the atmosphere. How can this be reconciled with the authors notion that isotopically it has acquired its signal from the atmosphere?

The isotopic equilibrium between the surface ocean and the atmosphere is achieved through gross gas exchange rather than net gas exchange. Therefore, even if the surface pCO2 in the EEP is not in equilibrium with the atmosphere in either the LOVECLIM simulation (now showed in the new Figure S9)or in the proxy reconstruction that the reviewer is referring to, it does not necessarily imply that d13C of DIC in surface water has not acquired a signal from the atmosphere.

I further recommend that the results section is not mixed with discussion elements. At its current stage, the entire Results section below line 236 (to 260) is actually discussion.
We have now moved those elements to the discussion section as the reviewer suggested

The only observation on carbon partitioning that is described and that would qualify for the results section is that from the North Pacific, which is incomplete. Even more worrisome is that none of the results of the cGENIE benchmark test has been described properly in the results section. This needs to be revised. Model papers have the luxury of presenting a wealth of data. The reader will appreciate the distillation of the main observations of the model output data.

As mentioned above, the description of the model outputs in the result section is now expanded to cover all ocean basins.

Minor comments regarding language and clarity of the text.
Line 39-40: I’d suggest to acknowledge the existing large body of literature on this. Despite extensive research efforts over the last decades, the chain of events leading to the atmospheric changes recorded in ice cores remains incompletely understood.
We have added the acknowledgement of previous research effort as suggested.
Line 57: influencing
changed as suggested.
Line 62: move “until recently (Lynch-Stieglitz et al., 2019)” to the end, otherwise this sentence makes no sense

Moved as suggested.
Line 88: I am confused by the word “theoretical”. I believe that the model calculates oxygen saturation directly, as function of ocean temperature and salinity etc. Also, “at every grid point in

the model" can be dropped.

Thanks for the suggestion. The sentence now reads "Because the LOVECLIM transient experiment does not explicitly simulate either preformed or respired carbon as additional numerical tracers, the respired carbon is instead estimated by apparent oxygen utilization (AOU) – the difference between oxygen saturation and simulated [O_2] (see section 2.4)."

Line 92: it is better to write "have used the separation of DIC species into regenerated and preformed as starting point/basis to study […]" because Khatiwala et al. for instance use a much more sophisticated framework also considering disequilibrium effects.

We agree that Khatiwala et al., 2019 is not a suitable citation in this context, now the sentence reads

"The carbon partitioning framework is not new - previous studies have used this framework to study the mechanisms that lead to lower glacial atmospheric CO_2 (Ito and Follows, 2005; Ödalen et al., 2018; Khatiwala et al., 2019) and processes that control $\delta^{13}CO_2$ and marine carbon isotope composition (Menviel et al., 2015; Schmittner et al., 2013)."

Line 98-101: this sentence needs to be revised. E.g.: Here, we use the Earth system model of intermediate complexity cGENIE with a comprehensive diagnostic tracer framework (including for the first time a respired organic matter δ13C tracer) in order to fully evaluate the AOU-based off-line approach made based on LOVECLIM model data.

This sentence has been changed to "However, new here is the application of a 2nd Earth System model (cGENIE (Cao et al., 2009)) to fully evaluate the AOU-based off-line approach against an explicit respired organic matter $\delta^{13}C$ tracer."

Line 117: Define abbreviation VPDB
We added the definition as suggested.

Line 140: first mention of NADW, acronym needs to be defined

We added the definition as suggested.

Line 151: AOU has been defined earlier.
The repetitive definition has been removed.

Line 167: This may seem trivial, but in my view it should be mentioned how d13C_pref and DIC_pref was calculated.
We have revised the description of the cGENIE model and tracer framework in the mode text and moreover, greatly expanded and now describe in much more detail, the tracer framework in the SI.

Line 179-181: This goes against the statement in the introduction whereby the cGENIE model is employed to analyse a simulation with numerical tracer scheme.

The conflict is not clear to us. In carrying out a thorough revision of the text, we hope this has now been resolved.

Line 185: in the supplement.
We've changed the text as suggested

Lane 207: it should read Rae et al., (2020). Does that mean that the model was tuned to Pacific LGM data only? If yes, this should be explicitly stated.

Thanks for catching the citation issue; we have made the correction.

We should also clarify that the LGM configuration we use is not tuned to the North Pacific LGM data; all the boundary conditions we applied are already stated in the text. There are two reasons for not tuning cGENIE to the North Pacific LGM data in this study: 1) in our experiments, the perturbations are mainly applied to the Southern Ocean; 2) the LOVECLIM LGM state was not tuned to the North Pacific LGM data either.

Line 275: Why should this signal be seen first in the STGSP? Some explanation is needed here. Can you clarify what nature the d13C anomaly has, of regenerated or of preformed nature?

We are sorry for not being clear. The text has been changed to
"On the other hand, if the 'bottom up' scenario is true, a large negative $\delta^{13}C$ anomaly (of respired nature) should first appear in the South Pacific subtropical gyre (STGSP), as STGSP lies on the pathway between Southern Ocean water masses and those at lower latitudes."

Line 303: Reference to figure is needed. Or in fact a description in the results section.
Reference to the relevant figure in the result section is added.

Line 317: where the δ13CO2 signal […] is / Line 319: atmospheric overprint.
Changed as suggested