Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-95-RC1, 2020

Interactive comment on "The Atmospheric Bridge Communicated the δ^{13}C Decline during the Last Deglaciation to the Global Upper Ocean" *by* Jun Shao et al.

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 LOVE-CLIM 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. 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.

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. The latter I find somewhat 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. 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.

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). 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? 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.

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.

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 mechanisms might differ throughout the deglaciation (e.g. 162-164). I recommend to remove these and instead exclusively focus on the early deglacial d13CO2 change. The same (somewhat) applies to the centennial change in pCO2 around 16.2 kyr before present (e.g., 206-208).

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.

Minor comments:

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

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. “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 ...”

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”

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 . . .”

Line 63. Specify what components.

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

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.

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

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).

Line 120. A full sentence is needed here. Also, DICorg is depleted in 13C not d13C.

Line 121. Budget of what?

Line 123: (Dd13Creg) instead of (Dd13C)

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

Line 129. 2 and 5 mg CaCO3.

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 mention/discussion of this lag in the study.

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

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

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

Line 149. Which model?

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.

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

Line 172. “evolution” instead of “pathway”

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.

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

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.

References

Bemis, B.E., Spero, H.J., Lea, D.W., Bijma, J., 2000. Temperature influence on the carbon isotopic composition of Globigerina bulloides and Orbulina universa (planktonic foraminifera). Mar. Micropaleontol. 38 (3), 213–228. https://doi.org/10.1016/S0377-8398(00)00006-2

Lynch-stieglitz, J., Valley, S.G., Schmidt, M.W., 2019. Temperature-dependent ocean–atmosphere equilibration of carbon isotopes in surface and intermediate waters over the deglaciation. Earth Planet. Sci. Lett. 506, 466–475. https://doi.org/S0012821X18306836

Menviel, L., Spence, P., Yu, J., Chamberlain, M.A., Matear, R.J., Meissner, K.J., England, M.H., 2018. Southern Hemisphere westerlies as a driver of the early deglacial atmospheric CO2 rise. Nat. Commun. 9 (1), 2503. https://doi.org/10.1038/s41467-018-04876-4

Schmittner, A., Bostock, H.C., Cartapanis, O., Curry, W.B., Filipsson, H.L., Galbraith, E.D., Gottschalk, J., Hoogakker, B., Jaccard, S.L., Lisiecki, L.E., Lund, D.C., Martínez-

Méndez, G., Lynch-Stieglitz, J., Mackensen, A., Michel, E., Mix, A.C., Oppo, D.W., Peterson, C.D., Repschläger, J., Sikes, E.L., Spero, H.J., Waelbroeck, C., 2017. Calibration of the carbon isotope composition (δ13C) of benthic foraminifera. Paleoceanography 32, 512–530. https://doi.org/10.1002/2016PA003072

Spero, H.J., Lea, D.W., 2002. The cause of carbon isotope minimum events on glacial terminations. Science 296 (5567), 522. https://doi.org/10.1126/science.1069401

Wanninkhof, R.H., 1992. Relationship between wind speed and gas exchange. J. Geophys. Res. 97 (92), 7373–7382. https://doi.org/10.1029/92JC00188

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