Review of MS by Jun Shao et al.

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The paper forcefully demonstrates that air-sea gas exchange effectively acts to couple atmospheric and upper ocean d13C. 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 d13C 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 d13C 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 d13C records. It may therefore be appropriate to recall this point for the paleoceanographic tracer community.

Interesting is that the authors offer a quantification of the influence of preformed versus remineralized changes in d13C. However, the method applied to separate changes in d13C into the contribution from preformed sources and sources from biogenic particles is unclear and may not be appropriate (see below). Another interesting point, which deserves a bit more discussion, is the information on the change in d13C versus the change in atmospheric CO2 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 d13C changes.

Section 2.3 The separation into preformed and remineralized d13C from the model output of DIC and d13C appears problematic. The assumption and simplifications of the approach are not explained to the readers. It would be preferable to simulate the preformed tracers online.

a) The authors use the equations given in line 124 to estimate the change in δ13C due to a change in remineralization. However, the equation is unclear. A new term is used in this equation: “12Corg”. I guess 12Corg should read DICorg, the amount of remineralized carbon. Then the equation given by the authors reads:
   δ13Cr=d13Corg * Δ(DICorg /DIC) (1)

b) How is δ^{13}Corg computed? I get the impression that a constant δ^{13}C signature of organic carbon (δ^{13}Corg) is assumed in the approach applied to distinguish the preformed and remineralized components in LOVECLIM.

c) Any perturbation in surface δ^{13}C is also transferred to newly formed organic matter and CaCO3 and finally to remineralized carbon. In addition, changes in surface CO2 affect the fractionation factor for organic matter formation and thereby again d13Corg. Further changes in surface d13C 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.

d) The equation on line 124 used to compute the change in remineralized δ13C needs to be properly derived. The mass balance should be considered in the separation of the different components. I distinguish preformed DIC (DICp; index p) and remineralized DIC (DICr; index r) and related fluxes (Fp, Fr).

Changes in δ13C 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 δ^{13}C. The fluxes of preformed DIC entering and leaving the box are denoted Fp,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 δ13Cp,i. For simplicity, we consider one flux of remineralized carbon entering the box, Fr, with the signature δ13Cr,in. Mass balance is then given by:

$$V * d/dt\,(DIC) = Sum(Fp,i) + Fr \qquad (2a)$$
$$V * d/dt(DIC * \delta^{13}C) = Sum(Fp,i * \delta13Cp,i) + Fr * \delta13Cr,in \qquad (2b)$$

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

$$\Delta\,(DIC) = \Delta\,(DICp) + \Delta\,(DICr) \qquad (3a)$$
$$\Delta\,(DIC * \delta13C) = \Delta\,t/V * \Delta\,(Sum(Fp,i * \delta13Cp,i) + \Delta\,(Fr*\delta13Cr,in) \qquad (3b)$$

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

$$\Delta(DIC) * \delta13C + DIC * \Delta(\delta13C) = \Delta(DICp) * \delta13Cp,in + \Delta(DICr)*\delta13Cr,in$$
$$\Delta\,t/V * Sum(\,Fp,i * \Delta(\delta13Cp,i)) + \Delta\,t/V * Fr * \Delta(\delta13Cr,in) \qquad (4)$$

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. Equations 4 has many unknowns (Δ(δ13Cr), Δ(δ13Cp), δ13Cr,in and δ 13Cp,in …). Thus, it seems not possible to attribute the change in δ13C to preformed and remineralized components in an exact way without carrying a separate preformed d13C tracer in the model.

Perhaps it is justified to make approximations.

We may assume that

$$\Delta(DICp) * \delta13Cp,in << \Delta\,(DICr)*\delta13Cr,in. \qquad (5)$$

This is probably o.k. as δ13Cp,in is close to zero permil and and d13Cr,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 δ13C may be small, but they are multiplied with the total carbon fluxes (Fr, Fp,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:
$\Delta(\mathrm{DIC}) * \delta 13\mathrm{C} + \mathrm{DIC} * \Delta(\delta 13\mathrm{C}) \sim \Delta(\mathrm{DICr}) * \delta 13\mathrm{Cr,in}$ and the solution for Δ(δ13Cr) is:

$$\Delta(\delta 13\mathrm{Cr}) \sim \Delta(\mathrm{DICr})/\mathrm{DIC} * (\delta 13\mathrm{Cr,in} - \delta 13\mathrm{C}) - \Delta(\mathrm{DICp})/\mathrm{DIC} * \delta 13\mathrm{C} \qquad (6)$$

Eq. 6 is somewhat similar to the eq. (1) above and given on line 124 in the MS, when setting $\delta 13\mathrm{Corg} = \delta 13\mathrm{Cr,in} - \delta 13\mathrm{C}$. 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 δ13C 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 $\delta 13\mathrm{Cr} = \mathrm{d13Corg} * \Delta(\mathrm{DICorg}) / \mathrm{DIC}$.
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 Δ(δ13Cr).

In conclusion, the calculation of the change in δ13C 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.

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.
The simulations have been published before and the separation of d13C 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.

2) Line 149-158: The authors separate surface ocean δ13C 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 PO4 by upwelling and consumption of PO4 by export leads to a positive PO4 anomaly in the SO surface ocean (Fig 3d). Correspondingly, the balance of upwelling and export alone leads to a negative δ13C anomaly in the SO ocean surface. It is unclear to which extent incomplete air-sea exchange contributes to this residual component.

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 δ13C and CO2 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 δ13C 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 d13C 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.

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 d13C, but that this well-established fact is sometimes neglected in interpretation of planktonic d13C records. It would be appropriate to recall the typical equilibration time of 10 yr for d13C 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.

L51-52: I do not understand the conclusion that atmospheric CO2 is not affected. If atmospheric CO2 (and d13C) varies/is perturbed, e.g., by outgassing in the Southern Ocean, then the CO2 perturbation will like the d13C perturbation enter the upper ocean.

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?

L109: “The atmosphere is held constant ..allowed to evolve freely” This text is unclear. Do you mean in the experiment “fix” atm. CO2 and d13C is kept constant and in exp. “free” CO2 and d13C evolve freely?

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