Dear editor,

Thank you for the insightful comments and for handling this manuscript. We greatly appreciate your effort. We will not list the suggested grammar or typo corrections, but we have corrected them in the revised manuscript. Below we respond to your main points.

Main Text:
line 27: I am not sure what you mean by "upper-deep" waters and in which region these upper-deep waters would be located. Be more specific

The text now reads
"The model results suggest that thermocline waters throughout the ocean as well as 500-2000m water depths were affected by this atmospheric bridge during the early deglaciation."

paragraph line 205: You say you used idealized glacial boundary conditions, but then you apply interglacial CO2 of 278 ppm. This is confusing. See also comment on the supplement below.

We are sorry for not being clear. Glacial CO_2 concentration is not included as part of the 'idealized glacial boundary conditions' in either Rae et al., (2020) or this study. We now clearly specify the 'idealized glacial boundary conditions' applied to cGENIE. The text now reads

"For this, we take a model configuration based on the idealized 'glacial' boundary conditions of Rae et al., (2020) (including increased zonal planetary albedo at high Northern Hemisphere latitudes and the orbital configuration at 21 ka). Note, we did not attempt to achieve a glacial-like atmospheric CO_2 value for this spin-up, instead, we prescribed atmospheric CO_2 = 278ppm, $\delta^{13}CO_2$ = -6.5‰. The spin-up was run for 10,000 years.

line 284: Here you say that that the Southern Ocean would be heavier by 0.1-0.2permille everywhere, but that is not true for the surface. Please be more specific

The text now reads
"In all sectors of the Southern Ocean below 400m depth, $\delta^{13}C$ increases by 0.1-0.2‰ due to stronger ventilation."

paragraph line 405: This needs some clarification. I read it that although the EEP thermocline waters are supersaturated with respect to atmospheric CO2, their d13C is controlled by the top-down signal. This refers also to the question by referee #2 related to the work by Martinez-Boti, showing a supersaturation of waters with respect to CO2 in the atmosphere. The argument is that isotopic equilibration doesn't require net gas exchange and you should say so in this paragraph to explain this "conundrum".

Thanks for the suggestion, we added the explanation to this "conundrum". The text now reads "The modeling evidence indicates that even though the EEP is the largest CO_2 outgassing regions (in terms of absolute ΔpCO_2, Figure S9) under an enhanced Southern Ocean upwelling scenario, its thermocline $\delta^{13}C$ is dominantly controlled by the 'top down' mechanism rather than the 'bottom up' mechanism as previously suggested (Martínez-Botí et al.,2015; Spero and Lea,

2002). The apparent conundrum can be explained by the fact that the air-sea balance of carbon isotopes is achieved through *gross* rather than *net* CO_2 exchange."

lines 424-425: Here you mention an early decline in d13C between 18.3 and 17 ky and that LOVECLIM would be able to simulate this. However, reading the text I wasn't sure I could pinpoint always to which process at what time you refer to. Please be more specific with the timing and clarify the text.

We are sorry for not being clear. We hope the revised text addresses the request.

"However, mid-depth (1800-2100m) benthic $\delta^{13}C$ records from the Brazil margin (~27°S) document a sharp decline of 0.4‰ at ~18 ka (Lund et al., 2019), while atmospheric $\delta^{13}CO_2$ did not decrease until ~17 ka (Bauska et al., 2016; Schmitt et al., 2012). Lund et al., (2019) argued that the lagging atmospheric $\delta^{13}CO_2$ decline seemed at odds with the idea that $\delta^{13}C_{pref}$ contributed to the early benthic $\delta^{13}C$ decrease at their site. The observed benthic $\delta^{13}C$ trend between 20-15 ka at these Brazil margin sites is well simulated by LOVECLIM (Figure 10), allowing us to explore this question further. Before atmospheric $\delta^{13}CO_2$ starts to decline in LOVECLIM at ~17.2 ka, changes in $\delta^{13}C_{DIC}$ at ~2000m depth at the Brazil Margin are dominantly controlled by excess accumulation of respired carbon (indicated by highly negative $\Delta\delta^{13}C_{soft}$, Figure S10b), itself a response to the weakened AMOC, while $\Delta\delta^{13}C_{pref}$ is relatively small (Figure S10c)."

Supplement:
line 4: Here you say that the model has only a EMBM but in the next point you say it has a dynamical sea ice model. I assume, you also have a prescribed (average) wind field? If yes, say so to clear up the contradiction

We are sorry for not being clear. Yes, the wind field is prescribed. We have added more details through the text below.

"The absence of a dynamical atmospheric GCM component then requires that (fixed, annual average) 2D fields of wind stress and speed are applied, which are re-gridded from observations, plus a zonally-average profile of planetary albedo is applied. Greenhouse gas feedback on climate is implemented by applying a top of the atmosphere anomaly in radiative forcing according to the relative deviation of atmospheric CO_2 from a reference value of 278 ppm. These three individual components, their coupling, plus details of the simplified atmospheric component and associated climate feedbacks, are described in *Marsh et al.* [2011] (and references therein). "

paragraph line 211: Here you explain that you do not really do a deglaciation run, but that you are only interested in the temporal evolution in a perturbed transient run. In the main text this does not become so clear, and I wonder whether you could mention that more prominently in the main text.

We were not mean to say the deglacial experiment presented in the main text is unnecessary here. We are sorry for the confusion. We hope the revised text clarify this point.

“However, although the warming approximately corresponds in overall magnitude to that associated with deglaciation and is additionally associated with reorganization of the Atlantic Meridional Overturning Circulation, it should also be noted that this idealized instantaneous-perturbation transient experiment is distinct from the deglacial-like experiment described and analyzed in the main text.”