

Review of Karancz et al., “Glacial-interglacial contrasts in the marine inorganic carbon chemistry of the Benguela Upwelling System,” revised for *Climate of the Past*, by Jesse Farmer

I greatly appreciated the authors’ thoughtful and detailed responses, and largely agree with their revisions. At the same time, a closer read of the manuscript illuminates multiple areas that need refinement before publication. My recommendation is another round of major revisions here, but I’m hopeful that the revised manuscript will be publishable in *Climate of the Past*.

Major comments.

- (1) **Structure of Introduction section.** The current introduction section (L32-105) is rather challenging to follow as it covers massive scale differences, from global upwelling regions (L32-40) to the importance of alkenone physiology to pCO₂ reconstructions (L85). To help the reader, I’d recommend keeping the current L32-61, then L96-105 (plus “Here” sentence starting on L74) as the introduction. The details of the proxies themselves (L63-94) could be a separate subsection titled “Proxy Interpretation” in the introduction, as a separate section after “Oceanographic Setting” (better), or perhaps as a subsection of the Methods.
- (2) **Discussion Section 5.1.** Within this section, I find that the text does not accurately reflect the data, especially the poor correspondence between local temperature proxies (Mg/Ca, U³⁷N, d¹⁸O; e.g., L496-541), and their poor correspondence to high latitude temperature indicators (e.g., L488-494). It is completely fine that different temperature reconstructions do not agree; this happens all the time in paleoceanography. But the authors must be honest in their description. Moreover, when records do not correlate, a mechanistic discussion is not warranted because it is difficult to gain any insight on the underlying mechanisms (e.g., L509-516).

Overall, I believe the authors should greatly shorten this section, noting that some proxies (U³⁷N, d¹⁸O) appear to show a “classic” deglaciation pattern, while other proxies (Mg/Ca) do not. I think the major takeaway of this section should be that the dissimilarity within the different paleotemperature proxies at the site may reflect changing and complex upper water column temperature structure over the last 27,000 years, while the dissimilarity between these proxies and high latitude temperature proxies probably reflects changing and complex interactions between temperature at the core location and those in the high northern and southern latitudes.

- (3) **Section 5.4 needs to be overhauled.** Some ideas:
 - A. The authors should focus first on their primary observation that is shown in Figure 8. After that, they can speculate that *G. bulloides* might more faithfully reflect the properties of AAIW due to its subsurface depth.

- B. Please also draw an arrow for atmospheric pCO₂ on Fig 8.
- C. After this, they can discuss potential local influences on pCO₂ gradients (L749-755) and rule these out.
- D. Following ruling out the local processes, they can conclude by suggesting their data are evidence for greater CO₂ storage in mid-depth waters during the LGM, as has been previously suggested. They do not need to get into mechanistic explanations (e.g., local vs. remote iron fertilization) because their data do not speak to the underlying mechanisms. (Unless they also wish to add an iron flux record to this!)

Line-by-line comments/edits.

Abstract L18-19. Suggested rephrase: “but also on the efficiency of the biological carbon pump, which constrains the drawdown of atmospheric CO₂ in the surface waters.”

L59-61. I think it would be best to briefly summarize the caveats to this 1-dimensional approach here. Namely, ocean circulation is constantly working to “erase” the surface to deep gradients, while also importing waters with different d13C signatures due to the integrated histories of air-sea exchange and production/remineralization within those watermasses.

L71. Suggest adding paragraph break at “Here”, and removing the paragraph break on L77.

L85-86. Delete comma before “that”

L121. Grammar – change its’ to its (delete apostrophe); see correct usage on L133.

L124-125. Change “Corilois-force” to “Corilois force”

L129. Delete high, as the word “productive” implies high productivity

L145. Suggest change to “...year-round upwelling of varying intensity due to...”

L147. Suggest clarification to “Predominantly, the surface waters within the BUS act as a CO₂ source...”

L165. Here you should move up the paragraph presenting the L* data, currently on L414-421. When you move this paragraph up, please delete the clause “which together comprise a near-continuous time interval from ~5 to 27 ka BP” as you will not have discussed ages

yet at this point in the methods. Note also that light reflectance is typically signified as L* (capitalized L); this should be changed here and in Figure S2.

L164. Delete space before end parenthesis

L170. Change to “relatively high abundance”

L173-184. This is a step in the right direction, but the revised version is misleading. I interpreted this new text to mean that the authors have picked from a narrow size fraction for C and O isotopes, because ontogeny matters for these isotope systems, but they picked over a broad size fraction for B isotopes, because ontogeny doesn't matter for symbiont-barren foraminifera in the B isotope system. The problem is that we do not know whether this is true; the variation in the B isotopic composition of *G. bulloides* size fractions has not been tested (pending publication of the Buisson et al. results). There are reasons to suspect that different *G. bulloides* sizes may be living in different depths in the water column (e.g., Jonkers et al., 2013; Osborne et al., 2016; but cf. Metcalfe et al., 2015), which could influence what environmental pH they record. As a result, there is an uncertainty here that the authors must acknowledge even if they cannot quantify it.

To address this, I'd recommend the following roadmap:

1. (As the authors currently do) acknowledge that the larger sample size requirements of $\delta^{14}\text{C}$, $\delta^{11}\text{B}$, and El/Ca measurements required picking *G. bulloides* from the 150-425 μm size fraction
2. (Also as the authors do) note that there are observed size fraction $\delta^{11}\text{B}$ differences in symbiont-bearing planktonic foraminifera.
3. Note that while symbiosis should not be a concern for *G. bulloides*, uncertainty in the environmental conditions reflected by different size fractions could lead to biasing of the $\delta^{11}\text{B}$ and El/Ca results. In addition, previous studies looking at *G. bulloides* $\delta^{11}\text{B}$ have worked over narrow size fractions (Martínez-Botí et al., 2015; Raitzsch et al., 2018).
4. They can then cite Buisson et al. (in review) here to discount this effect, and/or note that future work would be needed to evaluate any size fraction-specific influences.
5. It is fine to say that, for the sake of this manuscript, you assume that the $\delta^{11}\text{B}$ and El/Ca results in *G. bulloides* reflect average conditions.

Jonkers, L., S. van Heuven, R. Zahn, and F. J. C. Peeters (2013), Seasonal patterns of shell flux, $\delta^{18}\text{O}$ and $\delta^{13}\text{C}$ of small and large *N. pachyderma* (s) and *G. bulloides* in the subpolar North Atlantic, *Paleoceanography*, 28, 164–174, doi:[10.1002/palo.20018](https://doi.org/10.1002/palo.20018).

Metcalfe, B., W. Feldmeijer, M. de Vringer-Picon, G. J. Brummer, G.J., F. J. C. Peeters, and G. M. Ganssen (2015), Late Pleistocene glacial–interglacial shell-size–isotope variability in planktonic foraminifera as a function of local hydrography, *Biogeosciences*, 12(15), 4781–4807.

Osborne, E. B., R. C. Thunell, B. J. Marshall, J. A. Holm, E. J. Tappa, C. Benitez-Nelson, W.-J. Cai, and B. Chen (2016), Calcification of the planktonic foraminifera *Globigerina bulloides* and carbonate ion concentration: Results from the Santa Barbara Basin, *Paleoceanography*, 31, 1083–1102, doi:[10.1002/2016PA002933](https://doi.org/10.1002/2016PA002933).

L191. Change “sonification” to “sonication”

L193. Change to “removal of”

L209. Change to “exetainer”

L226-227. Please specify whether the reported $d^{13}C/d^{18}O$ values are for the NFHS-1 standard; it is unclear as written.

L286-297. As you already had to calculate past salinity for the carbonate chemistry calculation (L294-295), you should evaluate the effect of changing salinity on Mg/Ca-derived SST. With very little additional work, you can quantify the assertion that the effect of salinity on temperature is “relatively minor” (L290).

L312-314. In order to solve for CO₂, you need to specify a second variable in the carbonate system. Which variable did you specify, and how did you estimate its value? See e.g. L668 far later down – there needs to be detailed methods to define your approach.

Section 4.1. Please report sedimentation rates in cm kyr^{-1} instead of cm yr^{-1} .

L443. Most of the rapid changes in $d^{18}O$ predate the deglaciation; e.g. the variance between 25 and 20 ka occurs during the LGM (dated to 26.5 – 19 ka, e.g. Clark et al., 2009, cited in manuscript).

L455, L469 and throughout. Either “last 6 kyr” or “since 6 ka BP”. Kyr is a block of time (6,000 years), ka BP is a specific date (6,000 years ago).

L500-504. I find this meltwater hypothesis difficult to fathom – at face value, one would expect that increased meltwater would reduce the formation rate of AAIW due to buoyancy gain (see, e.g., Starr et al., 2021), and thus the low- $d^{18}O$ signature of meltwater would not be upwelled in the Benguela region. Has this meltwater hypothesis been suggested anywhere previously? I’d definitely recommend adding references and/or expressing greater uncertainty in this explanation.

Starr, A., Hall, I.R., Barker, S., Rackow, T., Zhang, X., Hemming, S.R., van der Lubbe, H.J.L., Knorr, G., Berke, M.A., Bigg, G.R. and Cartagena-Sierra, A., 2021. Antarctic icebergs reorganize ocean circulation during Pleistocene glacials. *Nature*, 589(7841), 236-241.

L496-540. Why is the discussion of Mg/Ca left separately?

L541. To emphasize the point above, the Mg/Ca reconstruction from *G. bulloides* is not “generally in line with the [alkenone] and d18O record” and does not “confirm overall trends”. In fact, the *G. bulloides* Mg/Ca-temperature only looks like alkenone and d18O records during the warming between about 23 and 16 ka. Outside of that, this is a completely different record.

L542-558. This is a good discussion of potential depth offsets and should be kept in a revised and shortened Section 5.1.

L595. Change “a” to “their”

L609-611. It is unclear what the stated d13C values here (0.3-1.0‰ and 2.4-2.6‰) refer to – are they the magnitudes of d13C corrections per process? The sensitivity of d13C to each process? Please specify or remove these values.

L619-626. I believe this is out of order – on L619 the sentences “We here applied... The offset of...” should come first, then the results from the correction application should follow: “Still, when corrections...”

L628-646. For comparing to other sites putatitvely in AAIW, there are more recent publications than Curry and Oppo (2005). Please compare your results against these.

Lacerra, M., Lund, D.C., Gebbie, G., Oppo, D.W., Yu, J., Schmittner, A. and Umling, N.E. (2019), Less Remineralized Carbon in the Intermediate-Depth South Atlantic During Heinrich Stadial 1. *Paleoceanography and Paleoclimatology*, 34: 1218-1233.

<https://doi.org/10.1029/2018PA003537>

Umling, N.E., Oppo, D.W., Chen, P., Yu, J., Liu, Z., Yan, M., Gebbie, G., Lund, D.C., Pietro, K.R., Jin, Z.D., Huang, K.-F., Costa, K.B. and Toledo, F.A.L. (2019), Atlantic Circulation and Ice Sheet Influences on Upper South Atlantic Temperatures During the Last Deglaciation. *Paleoceanography and Paleoclimatology*, 34: 990-1005.

<https://doi.org/10.1029/2019PA003558>

L643. Delete repeated “values”

L667. See point above about specifying your approach for calculating pCO₂ from d11B in the methods.

L668-670. Please delete phrase “which is likely related to AMOC intensity” as there is no mechanistic connection presented as to why reduced AMOC should lead to pCO₂ equilibrium calculated from d11B. (Alternatively, you can bring the reader through the mechanistic connections that would create this expectation).

L680. Delete “remarkably” – as you point out in the next sentence, there’s a ~65 ppm offset during about half of the Holocene. That is 2/3rds of the glacial-interglacial difference in CO₂, so it is certainly a significant offset.

L682. “this remained more or less in equilibrium” – what is “this”??

L695-704. The argument presented here is that using Ba/Ca as a constraint on phosphate concentration provides better estimation of the *b* factor than assuming constant phosphate. But the authors have not presented data to back this case up. How does calculated pCO₂ compare when using Ba/Ca vs. assuming constant phosphate? This comparison must be presented to justify the conclusion given on L699-704.

L788-789. Again, delete “remarkably”; perhaps say “while surface values of pCO₂ reconstructed from d¹³C of alkenones generally track atmospheric pCO₂”