Review rebuttal for:

Mg/Ca and $\delta^{18}O$ in multiple species of planktonic foraminifera from 15 Ma to Recent

By Boscolo-Galazzo et al.

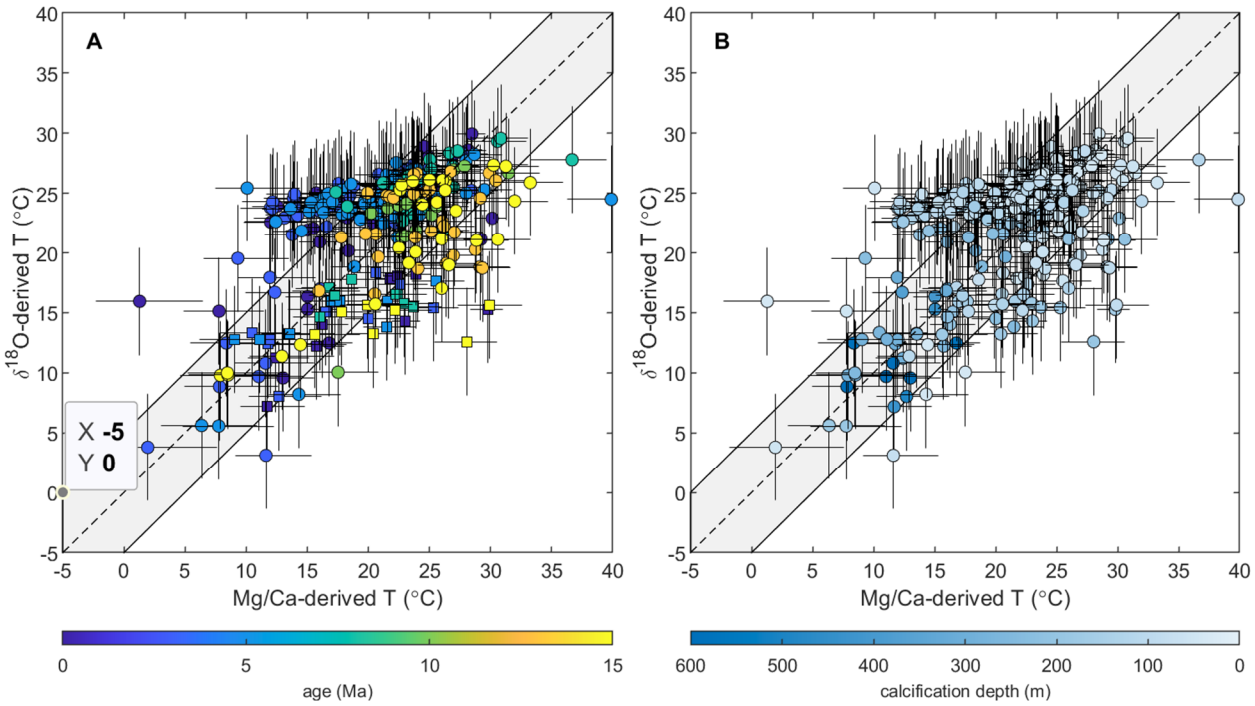
We thank the reviewers and editor for their comments and feedback on the manuscript. We agree with almost all of the points raised and have revised the text accordingly. Below we give a point-by-point response to the reviewers comments.

Reviewer 2

d18O-temperatures: I'm aware that the online tool of Gaskell and Hull (2023) allows for a limited number of d18Osw corrections and that Rohling et al. (2021) (R_21) is a perfectly valid option. However, not only is d18Osw beyond the Pleistocene (or even just the LGM) quite uncertain, but also a recent benthic d18O deconvolution from Rohling et al. (2022) (R_22 https://doi.org/10.1029/2022RG000775) suggests heavier d18Osw during most of the last 15 million years compared to R_21, with the independent model output from De Boer et al. (2010) sitting somewhere in between R_21 and R_22. Meanwhile, clumped isotope derived d18Osw suggests even heavier values for parts of the Miocene (e.g. https://doi.org/10.1029/2020PA003927). For these reasons, I wonder if it'd be possible to at least account for some of this uncertainty in your d18O-temperatures (larger vertical error bars?) and whether this might bring some of your "discordant" samples back into the uncertainty envelope of Figure 3.

We thank the reviewer for raising this important point.
As the reviewer points out, using an alternative sea level/deep ocean temperature record would potentially systematically shift the d18O-temperature reconstructions. For example, using the reconstructions of Miller et al. (2020) rather than Rohling et al. (2022) would result in d18O-derived temperatures 0.6-3.3°C warmer than our preferred scenario as presented in the main text, with the difference most pronounced for our 7.5 Ma samples, changing the overall proxy-proxy agreement from 62% to 64%. However, rather than include this source of uncertainty in the error bars of individual data points, we have accommodated this suggestion by including a supplementary version of main text Fig. 3 alternatively using Miller et al. (2020). The rationale for this is that, as the reviewer notes, many reconstructions are available, each with underlying assumptions or issues and it is not clear that all should be given equal weight in terms of application to paleo reconstructions. For example, in this specific case, we opt for the use of Rohling et al. (2022) over Miller et al. (2020) due to differing treatment of the benthic foraminifera Mg/Ca data used to deconvolve the benthic d18O record. However, given that our manuscript does not present any new benthic data and therefore cannot contribute directly to this debate, we do not feel it is the right place to critique individual approaches. Rather, we stress in the manuscript that these are the temperatures one would arrive at for a given underlying set of

assumptions, which are clearly noted, but that alternate choices have the potential to systematically shift the d18O temperature data in particular (lines 331-340). Encouragingly, while the choice of sea level record mentioned above can impact d18O temperature by up to ~3°C this has very little impact on our interpretation overall as the dataset is approximately equally distributed either side of the 1:1 line.



We propose to add the figure above as supplementary figure: it shows main text Fig. 3 except using the sea level and deep ocean temperature record of Miller et al. (2020) to derive d18Osw following the approach of Gaskell et al. (2023).

Alternatively, you could replot a few more versions of Figure 3 with different d18Osw corrections as offered in Gaskell and Hull (2023). Either way, I think it would be important to at least acknowledge these uncertainties in d18Osw before giving the reader the impression that a high degree of confidence exists behind the non-thermal corrections mentioned in the text. Moreover, this has implications for e.g. your hypothesis in Section 4.1.

Please see our response to the previous comment. We have done exactly what the reviewer suggests here (proposed supplementary figure above).

Technical corrections

Title: The title is too general, you could consider making it a bit more specific and refer to some of your main results.

We respectfully disagree with the reviewer on this point. In our view, the title accurately reflects what the study is about and, while we appreciate that it is often helpful to place a key result in the

title, it is difficult to do so in this case given that we demonstrate the existence of lineage-specific effects on Mg/Ca.

(Line) 220: The sea level record of Spratt and Liesecki only reaches 800 kyrs and doesn't go as far as 8 Ma as stated on line 221.

The reviewer is correct, we simply use Spratt & Liesecki to derive a scaling factor that is then applied back to 8 Ma. We have rephrased for clarity (line 223).

Also, I understand that in order to use the tool of Gray and Evans (2019) in samples older than 800 kyrs you probably had to modify the original code to include longer datasets for the salinity and and pH corrections (I only have experience with the R version so I'm not sure if the matlab version is a bit more flexible in this sense). If that were the case you could be more explicit with this in the text.

Yes, we did update the code. Upon acceptance we will place the revised version of the script on the MgCaRB github page (https://github.com/dbjevans/MgCaRB) and add a note to the text will be added.

221: Given that your choice of sea level/ benthic d18O deconvolution is Rohling et al., 2022 for the conversion of d18O into temperature (Line 259) I suggest using this same sea level record instead of Miller et al., 2005 for this step as well (from 800 kyrs up to 15 Ma, see comment for Line 220) to be as consistent as possible with the corrections applied to both proxies.

Yes, we agree. This has now been updated in the code and the figures changed but it has virtually no impact on the results as the salinity control on Mg/Ca is extremely minor.

229: This line suggests a very similar (or equivalent?) Mg/Casw than that one derived in Rosenthal, Bova and Zhou (2022) where they also combined Zhou et al., 2021 with the fluid inclusion data from Brennan et al., 2013. If it is then it might be more clear for the reader to cite the Rosenthal and Bova (2022) record directly (https://www.ncei.noaa.gov/access/paleo-search/study/36413).

Yes, we now cite this as suggested.

Figures 3B-8B: These plots colored by calcification depth are so interesting it would be nice if you could increase the color contrast a bit more so that the 200-0 m range is more visible.

Thank you, we tried and made it stand out more.

Line 626: It goes from ~4 mol/mol at 5 Ma to ~5.25 mol/mol at present (I'm looking at Figure 10 in Rosenthal, Bova and Zhou, 2022). did you perhaps mean 15 Ma?

Yes, corrected and reference to Rosenthal et al. (2022) added.