

Interactive comment on “Modal shift in North Atlantic seasonality during the last deglaciation” by Geert-Jan A. Brummer et al.

Anonymous Referee #1

Received and published: 6 May 2019

Brummer and co-authors present single specimen stable isotope measurements of polar species *N. pachyderma* and transitional species *G. bulloides* for core T88-3P in the northern mid-latitude North Atlantic. The authors deduce that two different populations of *N. pachyderma* existed throughout the last deglaciation and that, based on modern observations in the northern North Atlantic, these populations represent calcification during different periods of the year and thus under different environmental conditions. The study provides important new insights and merits publication in a journal like *Climate of the Past*. However, before the current manuscript could be accepted for publication, there are several points that need to be addressed/explained better and the inconsistencies in labeling etc. need to be corrected. So overall, I am recommending major revisions.

[Printer-friendly version](#)

[Discussion paper](#)



There are three major concerns that I have and which I will outline first.

1) Unimodal mode of *G. bulloides* and *G. bulloides* $\delta^{13}\text{C}$ values

The authors state that the single specimen isotope data of *G. bulloides* are unimodal, but give not reasoning for this statement. Subsequently, they use the unimodal distribution of *G. bulloides* as evidence that the two populations of *N. pachyderma* cannot be related to bioturbation (more on this in point 2). I would like to see some justification for declaring the *G. bulloides* data unimodal in the text. Whereas the $\delta^{18}\text{O}$ values show much less scatter than the *N. pachyderma* data, the respective $\delta^{13}\text{C}$ data show a range of 0.5‰ at some levels and I wonder, if this is not a reflection of more than one population. This statement is, however, only valid if the $\delta^{13}\text{C}$ values plotted in Figure 3 are actually correct, because *G. bulloides* $\delta^{13}\text{C}$ values should (mostly) be negative and the scale on the Figure is positive and has exactly the same range as for *N. pachyderma*.

2) Influence of bioturbation

Whereas I agree with the authors in the general sense that the occurrence of two populations cannot be explained by bioturbation, I would urge them to be more careful in those cases where one of the populations is presented by only 1 to 4 specimens. In this regard, it is essential to include an abundance record (which could be the *N. pachyderma* ratio record from Fig. 2) of both species in Figure 3. Since Figure 2 is presented vs. depth and Figure 3 vs. age, it is impossible for the reader to see where abundance minima of the respective species could have led to a "bias" in the single specimen isotope data (also in *G. bulloides* during periods of near dominance of *N. pachyderma*). For example, I do not perceive the argument of the unimodal mode of *G. bulloides* valid for the two specimens of population 2 in the third line of Table 2 [see note below on correcting column 1 of this table], if that level has already a low abundance of *N. pachyderma* and can thus be much more likely affected by –even if assumed minor, i.e. over 5 instead of 10 or 20 cm depth– bioturbation. In addition,

[Printer-friendly version](#)[Discussion paper](#)

Figure 3 should include a plot showing the variations in the sediment rates, so that the reader can see where low sedimentation rates might have increased the chance of bioturbational mixing. Including these plots might not change the story, but provides the reader with the option to judge him/herself in which levels bioturbation might have affected the single specimen data (and to what degree) or not.

3) Age model and 14C calibration

The authors made the effort to test different approaches to establish an age model, but in the end the reader does not know, which age model/age control points were used to produce the record of the data vs. age as shown in Figure 3. So please, specify this and provide either in the main manuscript or in the supplementary material a table listing the final age control points. Did you combine? If yes, did you then discard some calibrated ages? Issues with the text and information in Table 1 regarding the 14C calibration: Table 1 and section 2.4 and supplementary material: your measured age should be the same as the conventional age, i.e. the raw 14C concentration converted into an uncorrected 14C age (using the Libby half-life). If you calibrate with Marine13 this uncorrected age would be the one used to calibrate. So I do not understand how your Table 1 can list conventional ages that are 400 years higher than the measured age –which to me looks like a reservoir age correction going into the wrong direction! And I am not sure, which age –measured or conventional– was actually calibrated! If you analyze marine material like foraminifera the measured/conventional age needs to be corrected for the reservoir effect, i.e. transferred to "atmospheric 14C levels" by subtracting the reservoir age (such as 400 yr), if you want to calibrate with atmospheric level calibration data like Intcal13. Since you are calibrating with Marine13 you do not use a fixed reservoir age (of 400 years)! During the Holocene (0-10.5 cal ka BP) section the reservoir age is provided as outcome of the ocean-atmosphere box diffusion model and varies "significantly" over time –see for example Figure 4b in Hughen et al. 2004 on Marine04. In the glacial section, where a fixed reservoir age is used, the value is 405 years and not 400 years (see p. 1877 in Reimer et al. 2013). Inconsistency

[Printer-friendly version](#)[Discussion paper](#)

between p. 3 line 30, supplementary material: you state that the deepest/oldest 14C age was not used/excluded; so why it is then shown and used in Figure S2?

While correcting the 14C calibration will change the age model, this will not affect the general conclusions of the manuscript.

Additional comments:

Main manuscript p. 3 abundance counts: please specify a) how the % IRD was calculated; b) why a Ratio of NPS was calculated and not the more commonly used % N. pachyderma.

p. 3 Stable isotope section: please mention a) the resolution at which the single specimen measurements were done (4 cm?); b) if the N. pachyderma specimens were encrusted; c) which are the international carbonate standards used during the stable isotope analyses?

p. 3 core stratigraphy (besides comments above on 14C calibration): may be specify that you follow Reimer et al. (2013) when using ΔR of 0 ± 200 yr. line 29-30: if you keep the sentence, specify which sample was excluded (do not assume that every reader will read the supplementary material in detail). line 31-32: how many specimens of *G. bulloides* and *G. glutinata* were analyzed for the "bulk" analyses? line 35: include that the tuning was done to the $\delta^{18}O$ record of NGRIP, which, I assume, is presented on the GICC05 chronology. If you used NGRIP on GICC05, did you remember to correct the GICC05 b2k ages to BP ages (by subtracting 50 years) to make the tuned ages compatible with the calibrated 14C ages? line 36-37: you are providing information on temporal resolution and not sedimentation rates. I do not find this very informative and would like to see a figure showing the variations. Also, the sentence in its current phrasing is incomplete.

p. 4 line 4: what does IFA stand for?

p. 4 line 20: year missing for Jonkers and Kucera reference

Printer-friendly version

Discussion paper



p. 5 line 14-15: what about within glacial mixing/bioturbation?

p. 6 line 35: *N. pachyderma* $\delta^{18}\text{O}$ data not shown in Figure 2.

Table 1: following the recommendations of Stuiver & Reimer " Users are advised to round results to the nearest 10 yr for samples with standard deviation in the radiocarbon age greater than 50 yr".

Table 2: first column: please correct; what you are listing are not or incomplete depths. since the data itself is not shown vs. depth, it would be good to have an age column as well. Reduce the number of decimal places in the Prob and Mean columns, so that the numbers become easier to read.

Figure 3, 4, S1 etc.: in all the axis label referring to the NGRIP $\delta^{18}\text{O}$ data, replace the "SW (sea water ??)" by "ice". Provide reference for NGRIP data in figure captions.

Figure 3: as mentioned already above under point 1, correct the $\delta^{13}\text{C}$ scale for *G. bulloides*.

Figure S4: the right panel does not show the filtered NGRIP record = tuning target. Why is the SPECMAP error applied and not the GICC05 errors?

Supplementary material text: line 24 insert $\delta^{18}\text{O}$ before ice core and mention that the NGRIP record is on the GICC05 time scale.

line 27: provide more information on the "simple filter". for which frequencies did you filter and why?

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-144>, 2019.

Printer-friendly version

Discussion paper

