

Interactive comment on “Extreme lowering of deglacial seawater radiocarbon content is recorded by both epifaunal and infaunal benthic foraminifera” by Patrick A. Rafter et al.

Anonymous Referee #2

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Marchitto et al 2007 record of anomalous deglacial radiocarbon depletions (as recorded in infaunal benthic foraminifera) in the subtropical eastern North Pacific (at intermediate depths) set up an exciting debate regarding the Pacific Ocean ventilation and its role in deglacial atmospheric CO₂ rise. While some subsequent studies provided support for the reliability and the interpretation of Marchitto et al 2007, many studies raised critics to it. These critics relate to the reconstructions (chronology imperfections) and the interpretation (the source and the spatial extent of the recorded radiocarbon anomalies). In addition, bioturbation, diagenetic alterations and reworking are potential biases for any foraminiferal ventilation record. Rafter et al by using a different (and novel) approach for establishing the age model and dating monospe-

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cific benthic foraminifera (one epifaunal and three infaunal) from nearby records show similar deglacial large radiocarbon depletions providing a strong support to the fidelity of Marchitto et al 2007 record (while the source of radiocarbon anomalies remains an open and puzzling question). This important contribution largely rules out that the reconstructed deglacial radiocarbon depletions in Baja California are age model or foraminiferal artefacts. The paper is clearly written and the data are well presented and reasonably interpreted. I look forward to seeing it published after moderate, though critical, revisions.

First, the authors need to elaborate on their approach for constructing the age model, both advantages and complications. Given that the marine deposition of wood is very fast, the host sediment age = wood age – the time from wood growth to marine deposition. The premise is that the time from wood growth to marine deposition is more or less negligible. Are there any criteria that could be used to test that the dated wood have not been stored for long at land (i.e., the time from wood growth to marine deposition can be ignored)? For example, Zhue and Keigwin (2018, Nature Communications) used the presence of bark layers as a criterion that their wood samples are not redeposited old remains. A check to whether the wood age is younger than the age of co-deposited benthic foraminifera is employed to test the reliability of wood ages and accordingly 5 wood dates were omitted (as they showed older ages). Then, the ‘test-passed’ wood ages should be taken as the oldest possible ages for the host sediments (and reconstructed ventilation ages are the minimum possible ages) as the authors stated (page 6, line 25). While this issue does not affect the overall conclusion of this paper in terms of the validation of Marchitto et al record, the authors used the observation that their ventilation age estimates are not way older than those in Marchitto 2007 to partly rule out the effect of sedimentary redeposition (page 10, line 18) and diagenesis (Page 11, line 29). I think some consistency is needed here. What foram species has been compared to the wood; is it *P. ariminensis* (described as the ‘preferred one’), or the species with the youngest age from each sample? Can long period from wood growth to marine deposition be (partly) masked (when ages from coeval wood and foram are compared)

due to the generally decreasing atmospheric ^{14}C during the time interval of interest?

Second, in some places in the paper one might get the impression that the inference that the extreme deglacial radiocarbon anomalies are not species-specific is applicable to everywhere e.g., the paper title (the study area should be stated there); page 3, line 19; page 10, line, 29, referring to Thornalley et al 2011; the first paragraph of the 'conclusions' section in general e.g., mentioning the North Atlantic (page 12, line 5). As the authors stated in other places this is not the case e.g., the deglacial extreme aging from the subpolar North Atlantic and the southern Norwegian Sea seems to be *Pyrgo*-specific, away from the fact it's not yet clear whether the interspecies age differences from these areas are due to short-term hydrographic changes or foram issues (Ezat et al., 2017).

Third, what do the authors think about the interspecies benthic ^{14}C age differences in their records? Do they reflect real hydrographic changes? Can they be explained in the light of species abundance data (bioturbation effects)? I think these interspecies ^{14}C age differences are the meant by '...-with important caveats-... ' in the abstract (line 12), and so they deserve more attention in the discussion. While the average values from abundance maxima for *P. armininesis*, *U. peregrine*, *T. bardyi* (but not *Bolivina* spp.) are similar, the range of variability is still large and deserves more attention in the discussion. Can $\delta^{13}\text{C}$, $\delta^{18}\text{O}$ records of these different benthic species be helpful here (if they are measured)?

Fourth, the higher values that form the middle of the 'W' shaped anomaly in Marchitto et al 2007 (from 14.4 to 13.6 ka) is based on 1 mixed, 2 *Uvigerina* and 1 *Bolivina* dates. The adopted explanation here to explain the absence of the middle of 'W' in the Gulf records would have been plausible if the middle of 'W' is based on *T. bradyi*, but this is not the case. Can the lower sedimentation rates in the Gulf records be the reason for the absence of this feature in the 'Gulf' records? Notably, however, the highest delta ^{14}C value in this part is based on mixed benthic (so, it would be interesting to check with the authors of Marchitto et al 2007 if this mixed sample includes *T. bradyi* in a

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significant amount (if 'detailed' picking notes were taken for this sample).

Fifth, I suggest plotting the isotope data from Lindsay et al 2015, 2016 in Figure 6, which I think will make the comparisons between 'Undercurrent' and 'Gulf' records clearer to the readers (page 9, line 15). Related to this, can d18O records be used to align the 'Undercurrent' and 'Gulf' records? If this is feasible, you may put the 'Undercurrent' records on your wood age model and calculate the 'Undercurrent' ventilation ages accordingly?

Minor comments

- Page 4, line 6, what size fraction used for foraminifera abundance? How is the error in abundance (as shown in Figure 4) calculated?
- Page 3, line 1, it's not clear for me what the sentence about 'planktic foraminifera' means. I think people usually try to account, though very difficult sometimes, for the vertical migration of planktic foraminifera by assigning an average (or a range of) calcification depth representative for the whole life period based on modern observations (e.g., from plankton tow, surface sample studies) (e.g., Sarnthein and Werner, 2017, Marine Micropaleontology).
- Page 4, line 27, for 0.7mg and 0.1 mg, I think those values are for carbon mass. Please specify to avoid confusion with carbonate weights.
- Page 5, line 7, 'sediments' or 'sediment cores' instead of 'sediment'?
- Page 5, line 8, '30-to12 kyr' instead of '30,000-to12,000 kyr', or change 'kyr' to 'years'?
- Page 8, line 15, as the Holocene is just tuned to be equal to modern (in terms of intermediate ventilation age), I think the deglacial values should be just compared to modern.
- Page 11, line 18, it's mentioned (in page 4, line 18) that tests with and without the HCl pretreatment yielded identical results for the investigated records.

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- Page 12, line 15, '2015' is repeated.

- Figure 5e (or any other figure where all species-based ventilation ages are shown), why not giving each core a different colour and each species a different symbol? And if it's not going to be very messy, this can be done for Marchitto et al 2007 record as their supplemental table 1 includes the required information to do so?

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