

Interactive comment on “Ventilation changes in the western North Pacific since the last glacial period” by Y. Okazaki et al.

Anonymous Referee #2

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Okazaki and colleagues present new radiocarbon measurements from paired planktic and benthic foraminifera allowing reconstructing subsurface ventilation changes in the northwestern corner of the subarctic Pacific across the last glacial termination. The addressed topic is certainly timely and of large interest to the community. The pathways through which deeply sequestered carbon was released to the upper ocean and subsequently to the atmosphere during the deglaciation are still poorly constrained and largely debated. The data presented in this submission supports previous observations from the region. This contribution while not necessarily innovative, adds a new piece to the puzzle, especially considering the relative paucity of North Pacific records below 2000m. The data presented in the manuscript is nonetheless robust. I would thus recommend publication, provided the authors address a few - relatively minor - issues raised below.

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Major comments:

- Fig. 5. In addition to the comparison to ODP 887 (Galbraith et al., 07), I would suggest the authors to compare their results to recently published data from W8709-13PC (41°N, 125°W, 2700m) by Lund et al., 2011 (Nature Geoscience; <http://www.nature.com/ngeo/journal/vaop/ncurrent/full/ngeo1272.html>). - l.2, p. 2725. Based on the data compilation shown in figure 4, I am not convinced that D14C does increase substantially at 17kyrs. While this assertion seems reasonable when considering Fig. 5, I would urge the authors for caution since the interpretation is based on a single measurement. The new measurements presented here do indeed support a more strongly stratified water column during the glacial, consistent with previously published data. It does also support a better mixed water column since the Bolling. The evolution between the LGM and the Bolling is more ambiguous that what the authors would like to think. I would thus ask the authors to moderate their argumentation a bit more.

Minor comments:

- l.14, p. 2730. $^{231}\text{Pa}/^{230}\text{Th}$ at site GGC5 in the western Atlantic (McManus et al., 04) does not support total shutdown of the overturning circulation, but does instead suggest a strong reduction of the AMOC. - l.21, p. 2730. Does subsurface water extending to a depth of < 2500m in the Pacific Basin, which is more than 5500m deep qualify as “deep”? Overall I find the authors are being a bit loose with this characterization.

Interactive comment on Clim. Past Discuss., 7, 2719, 2011.

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