

Interactive comment on “Variations in intermediate and deep ocean circulation in the subtropical northwestern Pacific from 26 ka to present based on a new calibration for Mg/Ca in benthic foraminifera” by Y. Kubota et al.

Anonymous Referee #1

Received and published: 2 May 2014

Review of the manuscript "Variations in intermediate and deep ocean circulation in the subtropical northwestern Pacific from 26 ka to present based on a new calibration for Mg/Ca in benthic foraminifera" by Y. Kubota, K. Kimoto, T. Itaki, Y. Yokoyama, Y. Miyairi, and H. Matsuzaki.

This manuscript presents a new core top calibration of Mg/Ca vs calcification temperature for the benthic foraminifera *C. wuellerstorfi* and uses this calibration and stable oxygen and carbon isotopes to reconstruct a downcore record to 26 ka of northwestern Pacific intermediate/bottom water variability off the island of Okinawa. The tempera-

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ture record suggests up to 2°C lower temperatures during the LGM followed by a rapid increase into H1 along with a strong decrease in salinity which is linked to a decrease in North Pacific precipitation. The dataset presented in this study is very interesting, but I think that the focus needs to change. Basically, the study contains two stories, one on making a new Mg/Ca vs temperature calibration, and one on a downcore reconstruction. Currently my impression is that both parts deserve more discussion. Making a new Mg/Ca vs calcification temperature for benthic foraminifera is not as simple as it is presented here. The carbonate ion effect is hinted at but not further included and dissolution is assumed not to be a problem. This is a problem as the downcore record mostly covers those temperatures which are normally accepted as being in the interval where the carbonate ion effect is most significant, i.e. <4°C. The interpretation based on the downcore record is making an effort to explain large scale changes in the northern Pacific over the deglaciation which is not always based on the analyses themselves. For example, changes in upwelling are a major theme in the discussion, but it is never really clear what is meant here, upwelling in the equatorial Pacific (east Pacific records are included), in the North Pacific (PMOC developed during the glacial opposite to today), or at the core location itself (but then, how to reconstruct this with benthic Mg/Ca and $\delta^{18}\text{O}$)? To conclude I think that the potential is there, but suggest that the authors focus more directly on their own data, i.e. what is affecting their new Mg/Ca calibration and base their downcore results on what their recorded changes would mean at their core location. In a final section some implications of their results for the general idea of deglacial changes in the North Pacific in general can be mentioned. In summary, I recommend that this study would fit well in the special issue "western Pacific paleoceanography" in Climate of the Past after major revisions have been made. See below for more detailed comments.

Major comments Mg/Ca calibration: the influence of the carbonate ion effect at temperatures <3–4°C on benthic foraminifera is well-known, apart from the cited papers also see for example Yu and Elderfield, 2008 and Raitzsch et al., 2008. The data which are newly presented here and used to make a new calibration vs calcification

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temperature should be discussed in much more detail. The mentioned references notably say “very weak temperature sensitivity, probably 0.03 mmol/mol per °C, on *C. wuellerstorfi* Mg/Ca” (Yu and Elderfield, 2008). So, to discard this effect on the current core tops based on the reasons mentioned in p.13, lines 18-20 is not enough. The authors do mention that their values are different from previous published values (lines 2-3), so why not show the newly generated data vs published calibrations? Also, plotting Mg/Ca vs (CO₃⁼) and water depth would be very interesting. In section 6.2.1 it is mentioned that dissolution does not have an impact in this part of the Pacific because the lysocline is deeper than the core location. For Mg/Ca it is well-known though that dissolution of biogenic carbonates (based on planktic forams) starts well above the lysocline (at (CO₃⁼) <20-30 μmol/kg). A recent study by Regenberg et al. (2014) shows for samples from western Pacific that this means dissolution starts around 1 km water depth. So is there a potential dissolution effects on the core top results?

Downcore reconstruction: The calculated temperatures are mostly <3°C, which illustrates why it is so important to include the carbonate ion effect into the calibration. As pointed out before I think that the downcore reconstruction would be much better when it focuses on the results and what that would mean for the area with in the end possible implications for the larger picture. The basis is presented by the oceanographic introduction which gives clear numbers how to distinguish the different water masses. Another example of over-interpretation is the d18O_{water} record. Like on p.15, lines 15-17: “large-amplitude millennial fluctuations”: the number of data points excludes any conclusions that there are significant millennial fluctuations. Additionally, adding an error bar to the d18O_{sw} values will show that there is hardly any significant change at all. Due to the uncertainties involved with d18O_{sw} common error bars are on the order of + 0.5 permille (Rohling, 2000). So, I would be careful putting too much value on “large-amplitude” changes which involve only a few data points. Upwelling: it is not clear how to interpret the upwelling, is it upwelling in the equatorial Pacific (east Pacific records are included), in the North Pacific (PMOC developed during the glacial opposite to today), or at the core location itself (but then, how to reconstruct this with

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benthic Mg/Ca and d18O)? In the introduction the different intermediate water masses are brought which bath the core location. Mixing is mentioned as one of the factors which can change the local benthic d13C signal. How and/or where then is upwelling coming into the story?

Minor comments Title and also throughout the text: depth of the core is 1166 m and the authors also acknowledge the issue if the reconstructions are intermediate or deep water. I suggest to change this to “bottom water variability”, which could then when necessary be linked to “intermediate water masses”. Additionally, I would remove “a new calibration for Mg/Ca” from the title and replace with something like “Mg/Ca and stable isotopes. . .”.

Section 1 Lines 3-4, delete or move backwards “of benthic foraminifera”, i.e. all three proxies are based on benthics. Lines 12-13, remove “s” from records; using “millennial scale variation” (also later in the text) is a bit presumptuous when only 15 data points are covering this part. I would change this to mention that the data suggest changes that seem to follow Heinrich, BA, and YD. Lines 18-25, this is an example of the over-interpretation. How can this be concluded based on just the one, new downcore record which is presented? p.4, Line 24: Okazaki et al. 2011 is missing from the references, which include Okazaki et al. 2010 and 2012. p.5, line 14: delete “In paleoceanographic field” p.6, line 4: add References to the text. p.6, line 18: this is a very sudden jump from model results (is it relevant for this study that two models show different numbers for N-Pacific deep water?) to stable carbon isotopes on forams. The introduction can be more focused on the northwestern Pacific. p.6, line 21: add “stable” before oxygen and carbon.

Section 2 – Oceanography The oceanography part can be condensed significantly. For example, location and pathways in the South Pacific of the AAIW is not necessary for this study, only that AAIW probably contributes to local water. On the other hand, this section gives a very clear definition of the different intermediate water masses and their signatures. This would be a perfect basis to interpret any downcore variations in

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temperature, salinity, and d13C. p.7, line 1: “the study area is. . .”; line 4: add water to depth; Line 8: “a total flow”; line 17: delete “the”; lines 19-20: “Salinity increases.”; lines 21, 24: rephrase “bottom of the site”.

Section 3: Line 17: Grab sampler: how were samples taken from these? 0-1 cm? Also, have any surface samples been dated to show that modern samples were taken? p.9, line 8: 2.2 cm intervals? Line 21: add references for both types of wuellerstorfi; “overgrown surface” suggests a diagenetic overprint instead of something which is really part of the foram.

Section 4: this can be part of section 3.2 p.10, line 15: delete “clay materials”, these should have been removed already in the previous step. Line 16: “d18O and d13C were measured. . .”. Delete the bit in-between. Line 16-26: move this part to the end of the section, i.e. after the analytical part for the Mg/Ca. p.11, was Al/Ca also measured to monitor clay contamination? ; lines 6-7: “most” of the samples were under 65. So, a samples with 66 was discarded? And how many were then discarded? I think it is better to define outliers, e.g. samples which were more than 2sd away from the average, then a specific value.

Section 5 I suggest moving this up to between sections 3.1 and 3.2. Lines 13-14: is mixing shallow and deep living forams for 14C not going to give skewed results?

Section 6: Mg/Ca values as high as 3.1 mmol/mol from a waterdepth of just 300 m make me wonder if this is really wuellerstorfi? This section should be written in the present tense; values and temperatures are not smaller but lower. p.13, lines 26-27: remove “millennial scale changes”. Results and figures in general: add error bars. For example, p.13, line 27: “appeared to be negligible”, give statistics here.

Section 7 Start: rephrase, see comments on millennial variations before. p.16, line 14: only Lee et al. 2004 is in the References. Line 17: define subthermocline vs intermediate. Line 24: see also before, how significant is a 1°C warming in a range which is probably affected by the carbonate ion effect? Line 26: “suggests upwelling”,

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where? p.17, lines 12 and on: rapid changes in BWT and d18Ow lead to a very wide interpretation. I would bring this more carefully. p.18: would it not be more logical to show the Nd record from Huang than the one from Pena from the east Pacific?

Fig.1 and 3 can be combined into one. Delete the depth contours from Fig. 3b. Fig.2: it may be more helpful to show profiles covering the site location. The figure can then be combined with Fig. 4

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