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Interactive Comment

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 #2

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Review of 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 Kubota et al. MS No.: cp-2014-23

Kubota et al. present new rather high resolution (although this is not specified) records of bottom water temperature (BWT, derived from benthic Mg/Ca), stable isotopes and using a paleotemperature equation computed d18O of bottom water from a sediment core at intermediate depth in the NW Pacific. An additional contribution from the manuscript is a local calibration of Mg/Ca to BWT for a morphotype of C. wueller-

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storfi. The records are used to infer changes in the circulation at intermediate levels (mainly, the variations on deep circulation announced in the title are only indirect) and speculate on the role of the tropical north Pacific in the outgassing of CO2 during the deglaciation. In this sense, the authors mostly use their records to support pre-existing hypothesis without presenting any important new idea or conclusion. The lack of new ideas leaves some feeling of emptiness, nonehteless, the authors discuss more or less appropriately (see below for details) their data within the existent state of knowledge, being the BWT record novel for the area (more effort should be done to prove its reliability) and I find the manuscript suitable for publication after major revisions.

Specific comments to the text

Title: It mainly reflects the content of the manuscript but not entirely because I do not see "at first" much of the variations in the "deep ocean circulation". Based on the data presented and the hydrography suggested (i.e. NPIW and NEqIW at the site) I would remove that part from the title, the data presented mainly allows inferences about intermediate circulation and only marginally about deep circulation by discussing existing hypotheses. Idem for the first line of the abstract and line 22 in pag. 1269. Nonetheless, I rather see the site under the influence of NPIW and PDW (see below for further details on this) which would then allow for some "deep circulation" inferences.

Abstract: To understand variations in intermediate and deep ocean circulation in the North Pacific, bottom water temperatures (BWT), carbon isotopes (δ13C) of benthic foraminifera, and oxygen isotopes (δ18O) of seawater at a water depth of 1166m were reconstructed from 26 ka to present. A new regional Mg/Ca calibration for the benthic foraminifera Cibicidoides wuellerstorfi was established to convert the benthic Mg/Ca value to BWT, based on twenty-six surface sediment samples and a core top sample retrieved around Okinawa Island. In addition, core GH08-2004, retrieved from 1166m water depth east of Okinawa Island, was used to reconstruct water properties from 26 ka to present.

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-> you say this at the beginning, you do not need to repeat it here. I suggest some rewording here, the sentences are too long.

To understand variations at intermediate depths in the North Pacific, bottom water temperatures (BWT), carbon isotopes (δ13C) of benthic foraminifera, and oxygen isotopes (δ18O) of seawater at a water depth of 1166m were reconstructed from 26 ka to present. A new regional Mg/Ca calibration for the benthic foraminifera Cibicidoides wuellerstorfi was established to convert the benthic Mg/Ca values to BWT. This was based on twenty-six surface sediment samples and a core top sample retrieved around Okinawa Island. During the Last Glacial Maximum (LGM), from 24 to 18 ka, BWT appeared to be relatively constant at approximately 2°C, which is ~1.5–2°C lower than today

During the Last Glacial Maximum (LGM), from 24 to 18 ka, BWT appeared to be relatively constant at approximately 2°C, which is ~1.5–2°C lower than today.

-> I think that the tick labels in Fig. 9b are moved, it is difficult to see whether the mean LGM value is 2°C or 1 °C. In fig. 8 it seems rather that the value 0.8 for Mg/Ca represents the LGM which would be 1°C. Can you check the tick labels and numbers please? See below for morenumbers difficult to reproduce for readers.

One of the prominent features of our BWT records was a millennial-scale variation in BWT during the last deglaciation, with BWT higher during Heinrich event 1 (H1; ~17ka) and the Younger Dryas (YD; ~12ka) and lower during the Bølling/Allerød (B/A; ~14 ka). The record of seawater δ18O in core GH08-2004 exhibited a rapid increase in association with the rapid warming of BWT at 17 ka, likely due to the reduced precipitation in the North Pacific in response to less moisture transport from the equatorial Atlantic as a result of the collapse of the Atlantic Meridional Overturning Circulation.

-> This increase is a single point excursion derived from a low value (single point) in benthic d18O. Besides the propagated errors associated with the computation of d18Osw (which are not mentioned for this manuscript) are usually large (+/- 1 per mil).

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I would be cautious with giving it so much importance. I suggest some rewording to be more conservative in the interpretations of d18Osw. The same change is not clearly seen for the YD and the same mechanism proposed here could occur at the YD (collapse AMOC, change in moisture transport from Atl. To Pacific. . .). Please present your actual propagated error of the calculation.

During the interval from 17 to 15 ka, the bottom water temperature tended to decrease in association with a decrease in the carbon isotope values of C. wuellerstorfi, likely as a result of increased upwelling of the older water mass that was stored in the abyssal Pacific during the glacial time.

-> Note that this decrease is only 0.1 per mil and the existence of an older water mass in the abyssal Pacific is still under debate. The formulation of this sentence could therefore be more speculative, e.g. "a decrease in the carbon isotope values of C. wuellerstorfi, plausibly as a result of increased upwelling of an old water mass that is proposed to have been stored in the abyssal Pacific during the glacial time..."

The timing of the increased upwelling coincided with the deglacial atmospheric CO2 rise initiated at ∼17 ka, and suggested that the increased upwelling in the subtropical northwestern Pacific from 17 to 15 ka contributed to the carbon release from the Pacific into the atmosphere.

-> The CO2 increase starts at 20 ka (Fig 9g) when the d13C is actually at Holocene levels (when mean ocean changes are taken into account).

Introduction:

Line 5 pag. 1269: The formation of NPIW was expanded or shifted to the Bering Sea during H1?

Lines 13-14 pag. 1269: Lack of connection between the sentences. Suggested rewording: Both models simulated subthermocline and intermediate water warming in the subtropical Pacific Ocean and deep water formation as a response to the collapse

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of the AMOC, although there was a difference between the two simulations in how much deep water formed.

Oceanographic setting and water mass tracers:

Lines 10-12 pag. 1271: I do not agree with this statement and I would either remove it or tone it down. Komatsu et al. 2004 and Yasuda, 2004 find only marginal evidence for presence of a high oxygen water mass at intermediate levels that could have been influenced by AAIW, they do not say this would be AAIW. Qu et al. 2004 on the contrary show evidence that AAIW do not make it northwards of 15°N.

Line 21 pag. 1271: Are you sure that the increase salinity below 700 m indicates inflow of NEqPIW? Could it not simply be PDW? The salinity of PDW (ca 34.68) also fits with the Itaki CTD data (mixing of NPIW+PDW). See also fig. 7 in Bostock et al. 2010 and some ODV plots I have made below.

Line 23 pag. 1271: Then the site would be mainly under the influence of a mixture of NPIW and PDW? and I see little chances for AAIW to be having a strong influence here (according to Qu et al. 2004). If you were to indeed be at the boundary between NPIW and PDW, then you can indeed speak about variability of intermediate and deep circulation from your record.

Surface sediment samples and core GH08-2004:

Line 23 pag. 1272: Was there any reason for not including the core top of GH08-2004 in the calibration?

Line 8 pag. 1273: “The core material was sample at 2.2. cm intervals”. Samples at 2.2 cm intervals for a 273 cm long core would mean ca 124 data points. I only count 61 data points in Fig. 9. Do you mean 2.2. cm thick samples were taken? It seems so from the depths of the radiocarbon samples. And if there are 61 data points in 273 cm, then the sampling interval would be 4.5 cm?

Line 12 pag. 1273: . . .and because it records d13C in equilibrium with ambient water.

Lines 24-25 pag. 1273: you are kind of repeating what you said above, you can save these lines, i.e.

C. wuellerstorfi type B was more abundant than C. wuellerstorfi type A in most of the surface and core samples, and the Mg/Ca temperature calibration equations were generated for C. wuellerstorfi type B because of their higher abundance and continuous occurrence in core sediments.

C. wuellerstorfi type B was more abundant than C. wuellerstorfi type A in most of the surface and core samples, and therefore the Mg/Ca temperature calibration equations were generated for type B.

Analytical methods:

Lines 15-16 pag. 1274: Please match the order of descriptions, i.e. ...clay materials, Mn–Fe oxides, and organic matter were removed using a clay removal, reductive and oxidative procedure (Boyle, 1994). Did you keep this order in the cleaning, i.e. reductive before oxidative? See Rosenthal et al. 1995.

Lines 19-21 pag. 1274: this is a standard procedure and you do not need to describe it, you could save these lines.

Lines 26 to 28 pag. 1274: I do not get what you did with the set of 21 samples that "was prepared to improve reliability", do you mean you measure Mg/Ca also on those? Where are those results?

Line 2 pag. 1275: Please explain what Sc is, is it a commercial standard?

Lines 17-20 pag. 1277. I do not find very satisfactory the discussion of carbonate ion effect. This is mean to affect below 3°C and this temperatures cover most of the downcore record. More effort should be made here.

Age model:

What was the time resolution achieved, maybe 409 yr? Please, specify. Ages were

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interpolated assuming SR of 10-15 cm/kyr between 26 and 7.3 ka…Why do not you interpolate ages between 14C dates (and extrapolate for depths beyond the last date)? It will not change much your age model but it will read more precise than assuming 10-15 cm/kyr (where 10, where 15?) between 26 and 7.3 (when 10, when 15?).

Results:

Line 25 pag. 1277: lowest LGM value 0.8 mmol/mol? In fig. 8 it looks more like 0.6 or 0.61 as you say for the range of values (line 23). Or do you mean the "lowest values"? 0.8 mmol/mol looks like an average value for the LGM which as a matter of fact would yield a BWT of 1°C and not of 2°C as reported in the abstract and in line 17 of pag. 1278. Also, if 0.61 is the minimum value at 22 ka, then, I am getting a BWT = (0.61-0.66)/0.14 = -0.36°C as minimum BWT and not -0.22°C (line 16 pag. 1278)??? LGM, BWT 2°C? from Fig. 8a the mean value looks more like 0.8 mmol/ mol -> BWT = 1°C (which appears to be the tick label in Fig. 9b, please checked if these moved). Additionally, you give a value for the BA of 1.8°C and indeed the BA is warmer (Fig. 8a, 9b) than the LGM. Also, in line 23 pag. 1278, the low peak at 6.5 ka is given a value of 2°C while I read a Mg/Ca of ca 0.83 that would be 1.25°C? Please clarify these numbers to me. If needed, revise the discussion on d18O and temperature effect and leave it for section 6.2.2, you are anticipating in section 6.2.1 (lines 17-19 pag. 1278) what you later repeat in 6.2.2.

Lines 5-8 pag. 1278: Wording, there are not negative or positive peaks in the Mg/Ca record, all values are positive. You probably mean a high/low peaks.

Lines 10 pag. 1278: Wording,

Mg/Ca of foraminifera tests can be lowered by carbonate dissolution (Mekik et al., 2007), but the potential dissolution effects are unlikely to affect the changes in Mg/Ca values in our record, as the core site was located well above the carbonate lysocline depth depth (2000 m in the subtropical northwestern Pacific; Feely et al., 2004). You could move this paragraph to line 24 pag. 1277, i.e. before you start the full description

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of the record. Here it appears disconnected.

Section 6.2.2 is quite confusingly written. Lines 5-7 pag. 1279. Wording, the way it is written it seems that this is the only existing equation and there are more. Suggested rewording:

We use the paleotemperature equation of Shackleton (1974) that was derived from the original equation of isotopic fractionation between calcite and water established by O'Neil et al. (1969) to compute variations on d18O of bottom water (d18Ow). The d18Ow is obtained in PDB scale and it is converted to the VSMOW scale as follows: (d18Ow(VSMOW) = d18Ow(PDB)+0.27‰ $Hut, 1987)$.

I think you mean here d18Ow (VSMOW) = d18Ow (PDB) +0.27‰ and not d18Ow (VSMOW) = d18Oc (PDB) +0.27‰Ẏou are not converting between d18Ow and d18Oc, only changing the scale of your computed d18Ow.

Subsequently, the ice volume offset (Waelbroeck et al., 2002) is subtracted from d18Ow, yielding the residual d18Ow (Dd18Ow) (Fig. 9e).

I would favor to call this the "local d18Ow" or the "ice volume corrected d18Ow", these terminologies are more commonly use and therefore clearer.

Line 12 pag. 1279: Is this average really representative of modern conditions, i.e. which samples did you use to compute it? (n=5?? i.e. the upper 5 values of plot 9e??); I bet that the std dev. is very large since you have a very positive value in your youngest sample and quite negative values between 1 and 2 ka (values 4 and 5?). Could you specify which samples you used? I would be careful using your youngest benthic d18O value; there is quite a different between morphotypes A and B for this sample (fig. 8) and you are using the value of B for this calculation, the result would be quite different using the value of A (which I know you do not use for consistency). Can you get a third measurement to check which value is more reliable? If not, do not rely much in that value.

Line 23 pag. 1279: I am still not fully convinced that the core site lies within NEqIW, in my opinion it is too far north for being under a prominent influence of that water. A mixture between NPIW and PDW could maybe as well explain the HOL d13C values. Unfortunately our knowledge of d13C values of modern water masses in this region is quite poor. I include here WOCE line P10 that is probably the best approximation to your site (while far offshore from your location). In the plot you can see the negative NPIW (flowing South) above the more positive PDW.

See also S, ox, nutrients below. Your site may as well be affected by the relative high oxy water coming out from the South China Sea (Qu et al. 2004)?

Discussion:

It needs better organization and rigor on citation and statements of previous authors. One becomes the impression that there is some mixing between the alternative hypothesis for CO2 out gassing to the atmosphere and how the data supports one or the other. I am taking here the BWT at face value (see comment about above carbonate ion effect).

In section 7.1 you are jumping back and for between periods and comparing events between records which are not coetaneous and you rely a lot in model results which are only for H1. This makes it hard to follow and to think in how the model could be used for other time periods.

One of the prominent features of our records was millennial-scale variation in BWT during the last deglaciation. The deglacial pattern of BWT at site GH08-2004 with warming and cooling in association with H1 and B/A, respectively, was opposite to that of the sea surface temperature (SST) in the mid-latitude northwestern Pacific (Sagawa and Ikehara, 2008) as well as the marginal seas such as the East China Sea (Sun et al., 2005; Kubota et al., 2010) and the South China Sea (Kiefer and Kienast, 2005) that show a warming synchronous with the B/A warming in Greenland ice cores (Fig. 9). In contrast, -> there is not contrast because you did not speak about H1 of the previous

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records

An SST record based on planktonic foraminifera Mg/Ca from the same core as our benthic record showed an approximately 1°C increase at 17.5–17 ka (Lee et al., 2013), which coincided with the rapid warming of the intermediate water. However, the continued rise of SST in core GH08-2004 toward the B/A (Lee et al., 2013) is opposite to our BWT record that indicated a decreasing trend. Recent modeling results based on both an earth model of intermediate complexity, LOVECLIM, and a CGCM, MIROC, simulated subthermocline and intermediate warming in the Pacific basin between 30°N and 30°S as a result of the collapse of the AMOC and consequent establishment of the PMOC during H1 (Chikamoto et al., 2012). The mechanism involved in the warming of intermediate depths in the tropical and subtropical Pacific would be related to the PMOC intensification, due to a decrease in the PDW return flow and reduced upwelling from the abyssal Pacific would contribute to the warming of tropical intermediate waters (Okazaki et al., 2010, Chikamoto et al., 2012). The BWT warming estimate of âĹij1°C at the beginning of H1 found in core GH08-2004 was consistent with the simulation results. In addition, a small increase in d13C from 19 to 17 ka in core GH08-2004 also suggested decreased upwelling of the deep water

-> Note that deep waters are today more d13C enriched than NPIW, you need to add some info about why the deep waters would be d13C depleted (thus capable of reducing your d13C signal) during the glacial. -> Alternatively, and you mentioned this in section 7.2, a deeper penetration of well-ventilated NPIW (e.g. Okazaki et al 2010) (you could add here Jaccard and Galbraith, 2013) can also increase the d13C.

However, another BWT record based on Mg/Ca Uvigerina akitaensis from core GH02-1030 off northern Japan (lat., lon., 1212m water depth) exhibited a pattern opposite to our records, with warming at the beginning of the B/A, and subsequent cooling in the YD (Sagawa and Ikehara, 2008; Fig. 9c). This discrepancy was unlikely due to the uncertainty of the age models, because the age differences in the temperature peaks between the two cores were much larger than errors expected by 14C measurements.

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-> This paragraph (and the next one) does not fit here, previously you were speaking about H1, this record and this comparison refers to BA. It matches better after line 4 pag. 1282.

Because the LOVECLIM H1 simulation showed an asymmetric intermediate water response with cooling in the subarctic and warming in the subtropical North Pacific (Chikamoto et al., 2012), it was likely that the subarctic and subtropical waters showed different temperature responses also under BA conditions? -> The simulations were for H1, the BA is mean to have an ocean circulation equivalent to the present one. Today there is contrast in BWT between your site and GH02-1030 site in which this last is colder. It seems that the Japan record is warmer in the BA (although because of the issue with the tick labels it is not easy to read). Any further ideas to explain this?

In contrast, The rapid decrease INCREASE!! in Dd18Osw in association with the rapid BWT warming at 17 ka in core GH08-2004 was likely due to the reduced precipitation in the North Pacific in response to less moisture transport from the equatorial Atlantic as a result of the AMOC collapse (Okazaki et al., 2010; Chikamoto et al., 2012). The intensified PMOC would be maintained as a positive feedback,

-> I think that here the PMOC is rather the positive feedback mechanism for maintaining high salinity in the North Pacific. "The intensified PMOC would act as a positive feedback in accordance with the. . ."

in accordance with the Stommel feedback scenario (Stommel, 1961) as intensified PMOC supplied the subtropical saline water to the high latitudes (Okazaki et al., 2010; Chikamoto et al., 2012). At that time, more heat was transported to the high latitudes in the North Pacific while less heat was transported in the North Atlantic (Okazaki et al., 2010). The large-scale surface cooling in the subarctic North Pacific

-> There is not strong evidence for a large scale subartic cooling, only in the MIROC simulation there is large scale cooling, in LOVECLIM of both, Okazaki and Chimamoto, there is an east-westt gradient with warming in the east, cooling in the west (their figs.

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3c, Okazaki, 4b Chimamoto).

in the subarctic North Pacific would then intensify the Aleutian low in winter (Okumura et al., 2009) and lead to anomalous westerly winds in the mid-latitude North Pacific (Chikamoto et al., 2012). The intensified subtropical gyre, in accordance with the intensified subarctic gyre, could enhance the Kuroshio Current leading to greater heat transport to the high latitudes. Approximately 1°Cwarming in SST at 17.5–17 ka in core GH08-2004 (Lee et al., 2013) could be explained by the intensified transport of warm water from the equatorial western Pacific. After the rapid warming of the intermediate water, the gradual cooling in BWT from âĹij 17 to 15 ka in core GH08-2004 occurred in association with a negative shift in the benthic d13C, which suggested an increase in the upwelling of a potentially old, d13C- negative deep water. The gradual shift in Dd18Osw towards more negative values indicated freshening of the intermediate water that was likely due to an increasing ratio of precipitation to evaporation in the North Pacific.

Here you can talk about the different pattern of GH02-1030 as you are talking about this time interval.

The BWT began to increase again at 12.5 ka and reached the warmest temperature at 11.5 ka during the YD, approximately 1kyr earlier than the surface water warming that ended at 10.6 ka. At the beginning of the YD, the benthic d13C increased as well. Although the pattern of the benthic d13C did not match perfectly to that of the BWT during the YD, both records suggested a temporal reduction of the deep water upwelling at the beginning of the YD or a similar deeper penetration of NPIW similarly to H1?. (would this be an option?)

Section 7.2 some lines of discussion are opened here that are related to topics of section 7.1 and both could be connected.

During the glacial time, the water mass corresponding to the modern NPIW was thought to be thicker and deeply penetrated into the North Pacific, the so-called Glacial

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North Pacific Intermediate Water (GNPIW) (Matsumoto et al., 2002; Okazaki et al., 2012). During a severe cold interval during the deglaciation, such as H1, its main source area was probably in the Bering Sea (Ohkushi et al., 2003; Horikawa et al., 2010; Rella et al., 2012), and spread south to the California margin in the eastern North Pacific (e.g., Keigwin and Jones, 1990; Behl and Kennett, 1996; Tada et al., 2000; Hendy and Pedersen, 2005; Okazaki et al., 2010). Okazaki et al. (2010) suggested that the GNPIW extended to âĹij 2500m and flowed southward along the western margin of the North Pacific during early H1.

Please include in this discussion Jaccard and Glabraith, 2013 (GRL). They do partly support a deeper penetration of NPIW, to depths between 1400-2400 m, they do not support Okazaki in saying convection to 2500 m

On the other hand, the an increased meridional export of AAIW at âĹij18–17 ka is suggested based on a Nd isotope record of thermocline-dwelling planktonic foraminifera from the eastern equatorial Pacific (Fig. 9f; Pena et al., 2013). Evidence from the South China Sea also suggests that the an increased influence of the AAIW peaked at âĹij16 ka during H1 based on the Nd isotopes of bulk sediment (Huang et al., 2012). Considering the location of relative contributions of the water masses to site GH08-2004, both the increased contribution of the GNPIW and the AAIW could have compensated for the reduced upwelling of deep water during early H1.

They could also be “the cause” as a stronger penetration of one or the other (likely NPIW) would displace deep water. Is this what you mean by “compensating”?

This relationship was repeated again at the beginning of the YD and reversed during the B/A. The influence of the intermediate water originating from the Southern Ocean began to decrease and the contribution of the PDW had been increasing to increase in the eastern equatorial Pacific from 17.5 to 13 ka (Pena et al., 2013). This corresponded to a decreasing trend in BWT and d13C in core GH08-2004. The increase in upwelling that was interpreted from the intermediate cooling and low d13C was likely due to the

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reorganization of Pacific circulation that was related to reduction of the AAIW export to the tropics and less ventilation of GNPIW.

(any ref. to support this or any mechanism proposed to link both, less AAIW and more PDW?)

Our interpretation is consistent with Okazaki et al. (2010),

-> this should be Okazaki et al. 2012

who claimed that, in the North Pacific, there was a shift from a stratified glacial mode during H1 to an upwelling interglacial mode during the B/A.

they propose the stratified mode during the glacial, deeper convection during H1 and establishment of modern circulation during BA.

However, it should be noted that our results also showed a temporal reversal to the relatively stratified mode at the beginning of the YD.

YD looks more like H1 than like LGM; during LGM BWT are lower, d18Osw suggest furthermore the intermediate waters were fresher.

In section 7.3 you do not jump between periods but between ocean basins and "alternative" locations for the old glacial reservoir and the reader lost connection and wonders how should come that the abyssal water that may have been stored in the Southern Ocean is "in contrast" not seen in the north Pacific. . .I suggest the following re-organization.

Atmospheric CO2 concentrations rose approximately 40ppm during H1. This CO2 is thought to have been stored in the abyssal Pacific or in the Southern Ocean and released into the atmosphere due to changes in ocean circulation (Broecker and Barker, 2007, e.g. Skinner et al., 2010; Burke and Robinson, 2011). So far, there is still debate on the mechanism by which the old carbon was released into the atmosphere. Work by Okazaki et al. (2012) compiled d14C records from several parts of the northwestern

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Pacific and concluded that there was no sign of massive mixing of old carbon from the postulated Pacific abyssal reservoir during the glacial to deglacial period. On the other hand, eastern North Pacific records indicate injection of 14C-depleted intermediate waters at âĹij16–15 ka off the west coast of Baja California (Marchitto et al., 2007a) which these authors link to intrusion of Southern ocean intermediate waters carrying a signal of an old water mass stored in the Southern Ocean. There are studies that find evidence for the existence of a large, old, CO2 rich water mass in the abyssal Southern Ocean during the last glacial period (Skinner et al., 2010; Burke and Robinson, 2011). Evidence for the upwelling of 14C-depleted water was not found off the margin of Chile (De Pol-Holz et al., 2010) or off New Zealand (Rose et al., 2010)

Please, include Siani et al. 2014, Nature communications, in this discussion; they do see old waters off Chile after correcting the reservoir ages.

or the Drake passage either (Pahnke et al., 2008). -> Pahnke et al. 2008 do not present 14C nor are her records in the Drake Passage.

-> You need to link here the 14C story with the d13C minima story. It appears disconnected. For example:

Spero and Lea (2002) also suggest the intrusion of intermediate and mode waters from the Southern Ocean into the eastern equatorial Pacific based on d13C minima of thermocline-dwelling foraminifera at âĹij16–15 ka (core TR163-19). Pena et al. (2013) find the same d13C minima at ODP site 1240 and using εNd linked it to AAIW and SAMW. Considering the enhanced PDW signal (decreasing εNd) from 17.5 to 13 ka in the Nd isotope record in ODP site 1240 in the eastern equatorial Pacific (Pena et al., 2013), the simultaneous decreasing trend in d13C of the thermocline species in the eastern equatorial Pacific could be interpreted as increased upwelling of DIC-rich deep water (Figs 9d and 9f).

-> Note i) Fig 9d does not show the thermocline d13C, ii) that the d13C minima of Pena et al expands from H1 till the end of YD, i.e. it starts with increased penetration of

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AAIW, continues with increase PDW. So, upwelling of DIC rich water may not be the explanation for that core.

In the western side of the equatorial Pacific, boron isotope data suggests that the interval from âĹij18 to 13.8 ka is marked by surface water pCO2 values that substantially exceed atmospheric pCO2 (Palmer and Pearson, 2003). The surface water pCO2 began to increase at âĹij18–17 ka and was highest at âĹij15 ka, which is in good agreement with the negative peak in our d13C records (you only have one) in the western subtropical and that in eastern equatorial Pacific (Spero and Lea, 2002; Pena et al., 2013). The upwelling in the tropical and subtropical Pacific and subsequent release of older carbon to the atmosphere likely contributed to the rise of atmospheric CO2 during the time interval between 17 and 15 ka.

Conclusions:

Line 19: remove d13C from here, this refers to Mg/Ca and d18Osw. Line 23: elaborate a little bit more so that readers get from the conclusions all the important information, e.g. “and establishment of a PMOC”.

Figures Fig. 1: Bostock et al. 2010 do not show a northward flow at intermediate depths between 0° and 20°N in the W Pacific. Kaneko et al. 2001 draws a northward recirculation flow of NPIW, do you mean this one with these arrows? If so, please correct the fig. caption to include that ref. and if you stick to Bostock et al. 2010 circulation scheme, then correct the arrows. Fig. 3: Please add longitude scale to left figure and increase the font size of lat. and lon. for the right figure. You could also combine this fig. with fig 1. Fig. 5: could morphotype B be C. cf wuellerstorfi, also known as C. dispars? See Hayward et al. 2010 Recent New Zealand deep-water benthic foraminifera: Taxonomy, ecologic distribution, biogeography, and use in paleoenvironmental assessment.

Fig. 9b: The scale seems to be moved, i.e. tick labels are not in place, I think 0 should be at the lower tick label, 2 at the third one etc. Check also the scale of 9e. Also in 9e,

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salinity increases towards higher values, i.e. the other way around as displayed by the arrows.

Technical comments In general I find that the text is quite well structured, except for the discussion which will benefit from some order in the description of events and processes (see above). Regarding the language, I think there are several missuses of prepositions, articles, plurals and sentence connectors (e.g. “however”, “in contrast” are sometimes used without need): e.g. 1 samples were used to calibrate benthic Mg/Ca to (rather than “with”) bottom water temperature e.g. 2 line 6 pag. 1268: Thus, the reconstruction of. . . e.g. 3 line 24 pag. 1268: In the paleoceanographic field. . . e.g. 4 line 21 pag. 1273: overgrown surface of the type B results from secondary calcite (rather than “the secondary calcite”) e.g. 5 Table 1. List of surface K-grab samples. . . e.g. 6 section 3.1: Surface sediment samples (rather than surface sediments) e.g. 7 Oceanographic setting (rather than settings) Please, get that checked throughout the text.

Please remove the “dots” after vs, you put a dot when you shorten a word (e.g. Prof., lat., lon.) but not when you create a new short word to substitute a longer one (e.g. Dr, vs, ca).

Section 2. It reads to me more correct to say: Oceanographic setting and water mass tracers

Currently the use of units for salinity is officially discourage (e.g. Millero, 1993) because salinity is defined as a ratio and is therefore dimensionless. Line 15 pag. 1270. The formation of AAIW is linked to the Subantarctic Mode Waters and occurs mainly in the southeast Pacific off southern Chile (Talley, 1996; Tsuchiya and Talley, 1998; Hanawa and Talley, 2001). (not Tally) Line 13 pag. 1271: This ref. should be Qu et al. 2000 while I suggest you use Qu et al. 2004 who shows that the maximum northward extension of AAIW is 15°N. Line 29 pag. 1271: Reference, please be so kind to give credit to the original authors on the use of d13C as water mass tracer.

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Lines 1-2 pag. 1274: washed through a 63 ïA■m mesh sieve and dried in the oven at 50°C. ("for one day" is not needed). Lines 17 pag. 1274: The d18O and d13C were measured... (idem line 19) Line 11 pag. 1275: Radiocarbon analyses were carried out by accelerator mass spectrometry (AMS) on approximately 6-12 mg of the planktonic foraminifera... Line 15 pag. 1275: according to Table 2 you extracted foraminifera for 14C from 6 "horizons" (since 4 14C dates are from Itaki). Line 24 pag. 1276: a Mg/ca (instead of "an Mg/Ca"). Line 26 pag. 1277: Refer here to Fig. 8. Line 12 pag. 1281: INCREASE in d18Osw Line 10 pag. 1283: Okazaki et al. 2012 not 2010 Line 26 pag. 1282: Huang et al. 2013 Line 18 pag. 1288: Capital letter for "Hydrographic".

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/10/C455/2014/cpd-10-C455-2014-supplement.pdf

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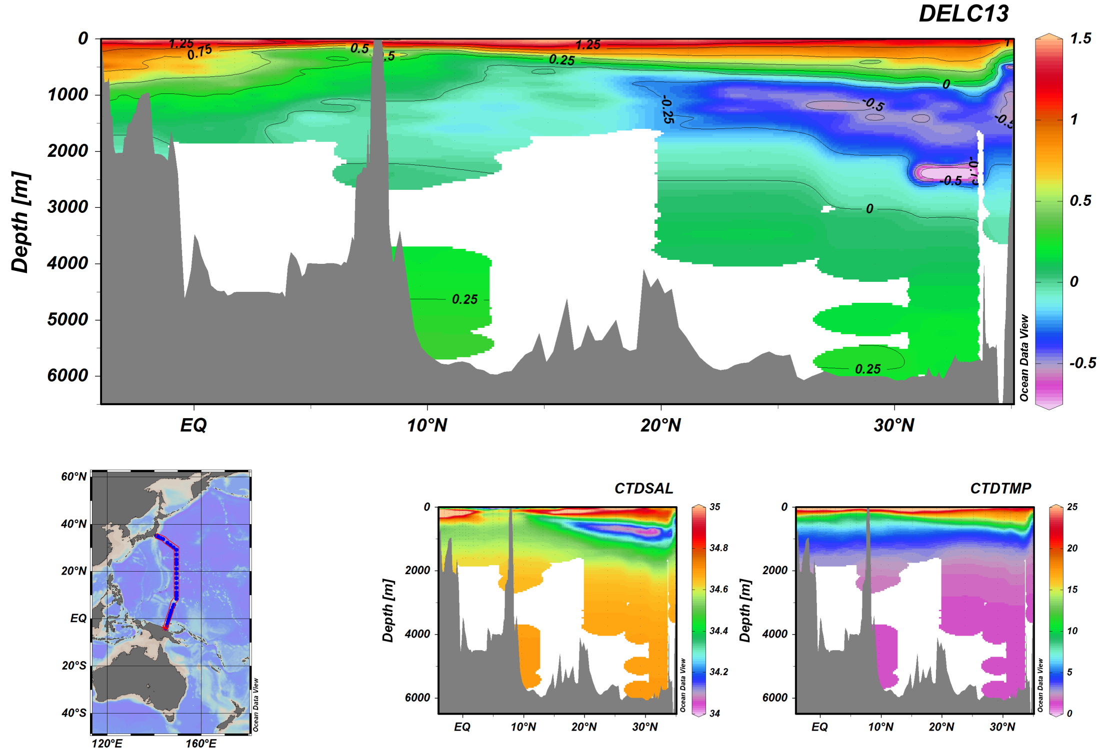
Interactive comment on Clim. Past Discuss., 10, 1265, 2014.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Depth [m]
EQ
10°N
20°N
30°N
Ocean Data View
CTDSAL
CTDTMP
60°N
40°N
20°N
EQ
20°S
40°S
120°E
160°E

Fig. 1.

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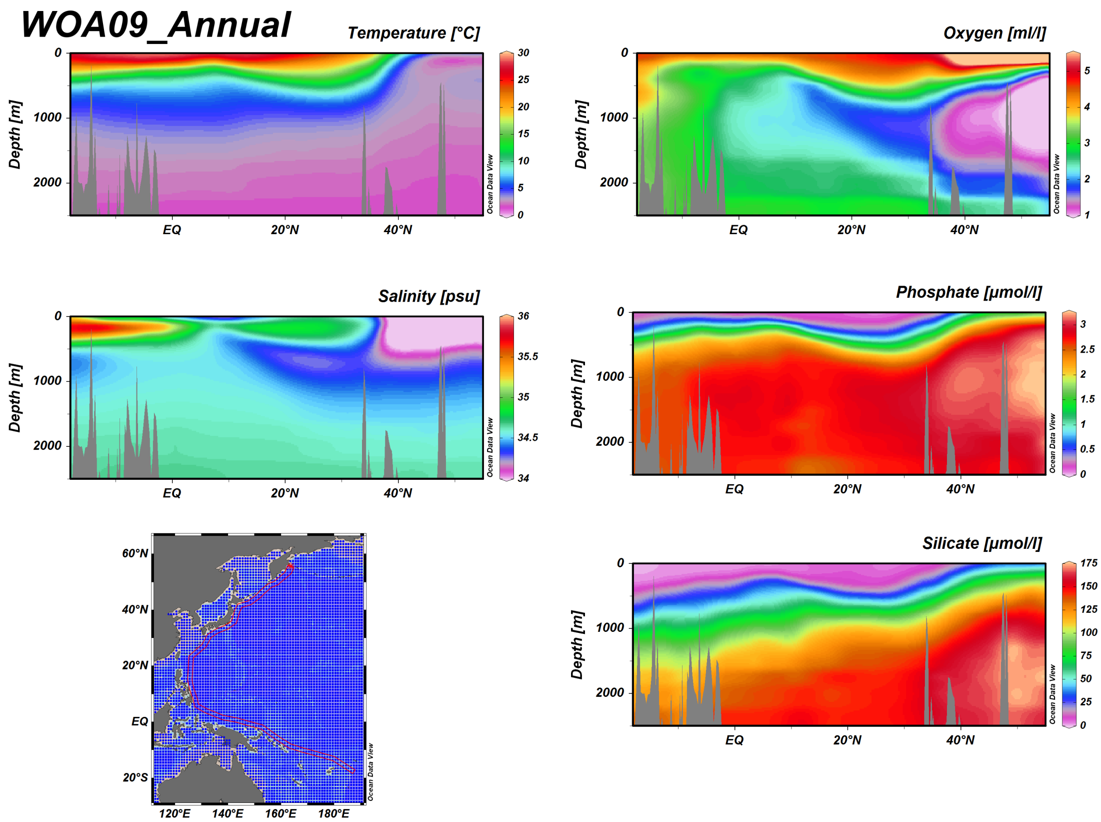

Fig. 2.

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Interactive Discussion

Discussion Paper

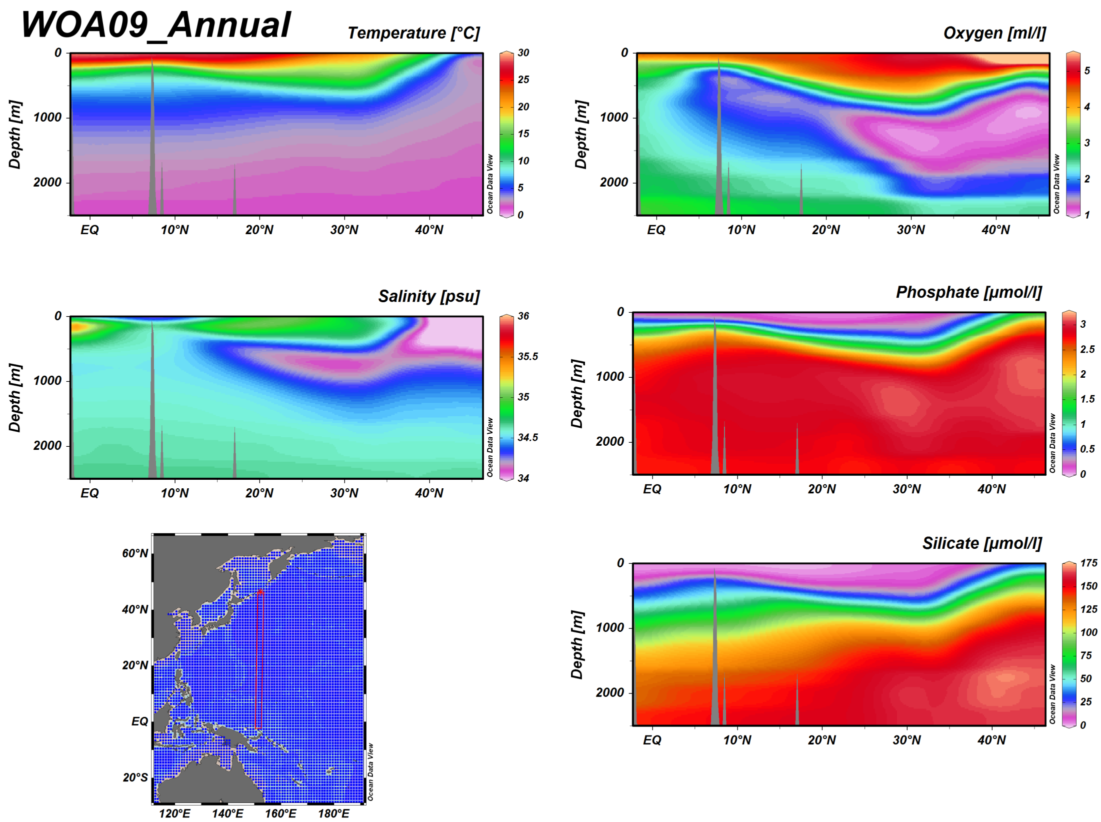

Fig. 3.

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