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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.

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We thank the reviewer for recommending publishing. His/her comments are very detailed and helpful. We believe the comments and advices had led to improved quality of the manuscript. The following is our reply to queries and comments raised. The reviewer's comments are in black and ours are in blue.

Reply to reviewer 2
Reply to specific comments:

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-Title:

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.
» As we follow the comments by both reviewers, we will change the title as below. “Bottom water variability in the subtropical northwestern Pacific from 26 ka to present based on Mg/Ca and stable carbon and oxygen isotopes of benthic foraminifera”

-Abstract:

1.To understand variations in intermediate and deep ocean circulation in the North Pacific, bottom water temperatures (BWT), carbon isotopes (d13C) of benthic foraminifera, and oxygen isotopes (d18O) 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.
>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

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temperatures (BWT), carbon isotopes (d13C) of benthic foraminifera, and oxygen isotopes (d18O) 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

»Will be revised as suggested. We will omit the sentence "In addition, core GH08-2004, retrieved from 1166 m water depth east of Okinawa Island, was used to reconstruct water properties from 26 ka to present."

2. 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.

» We apologize for the confusing figure. We will check the tick labels in Fig. 9b and move the label to the correct position. Detail explanation will be written later.

3. 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 d18O 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.

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>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). 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.

» Concerning conversion of foraminiferal Mg/Ca to temperature, analytical error of Mg/Ca is lower ($\pm$0.2–0.3 $^\circ$C) than an error of the calibration line ($\pm$0.96 $^\circ$C). Eventually $\pm$0.34‰ of $\Delta\delta$18Ow error is derived by $\pm$0.1‰ for error of the ice volume offset that adds to $\pm$0.24‰ (1σ) of the propagated error of δ18Ow from BWT reconstruction when the BWT error of $\pm$0.96 $^\circ$C is applied (0.25‰ per 1 $^\circ$C). Error bars will be added in the figure.

4. 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 analytical error of carbon isotope is $\pm$0.05 per mil, and the decrease from 17 to 15 ka is 0.2 per mil. This means that the decrease is significant. However, we agree that 0.2 per mil change is rather small and the time resolution is not very high. Thus, we follow the advice and revise the sentence to tone down as suggested.

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5. 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).
» As a critical interval for CO2 degassing from ocean is 17-15 ka when atmospheric δ13C and 14C is depleted (Schmitt et al., 2012), we would like to focus on this interval. However, the reviewer 1 also pointed out that these sentences are an over interpretation. We agree with these comments and reconsider our interpretation concerning δ13C and its link to CO2 degassing.

-Introduction:
Line 5 pag. 1269: The formation of NPIW was expanded or shifted to the Bering Sea during H1?
»Rather shifted (Rella et al., 2012). Then we specify this.

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 of the AMOC, although there was a difference between the two simulations in how much deep water formed.
»Thank you for your suggesting. However, as suggested by the reviewer 1, we will omit the explanation for the modeling results in the introduction and focus more on proxy evidence in the North Pacific here.

-Oceanographic settings and water mass traces:

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1. 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.
» Thank you for letting us know the reference concerning AAIW advection. We agree with your advice. As you mentioned, we will remove the statement and replace it to statements below referred to Qu and Lindstrom (2004). "AAIW is a distinctive water mass with high oxygen concentrations (200–250 μmol/kg) and relatively low salinity (34.3–34.5) (Bostock et al., 2010). AAIW is formed in the southeast Pacific off southern Chile (Talley, 1996; Tsuchiya and Talley, 1998; Hanawa and Talley, 2001) and flows west into the Coral Sea. The distribution and flow pass of AAIW is still under debate (Reid, 1997; Yasuda et al., 2001; Yasuda, 2004; Qu and Lindstrom, 2004)." "Relatively high oxygen water does exist in the Okinawa Trough and south of Japan (Komatsu et al., 2004), which had been thought to be possible influence of AAIW (Reid, 1997; Yasuda et al., 2001; Yasuda, 2004). Subsequently, Qu and Lindstrom (2004) on the contrary showed evidence that AAIW does not reach northward of 15°N based on traceable salinity minima."

2. 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.
» We agree with your comment. According to Kaneko et al. (2001), waters at 1500 m water depths in the Philippine Basin are PDW, which comes from southeastern part of the basin. In Bostock et al., 2010, they define NEqPIW as a mixture of PDW and NPIW, and we follow them to define the water mass in the previous manuscript. Considering your comment, we think that we should better to use PDW instead of NEqPIW because it is simpler to describe the variations in water mass. In the revised manuscript, thus, we regard the bottom water of our site as a mixture of PDW and NPIW. As explanation

for PDW was little in the previous manuscript, we will add definition of PDW as below in Line 21 page 1270. “PDW lies beneath the NPIW (< 1000 m) in the North Pacific. The deep Pacific is ventilated from the south by the densest waters such as Antarctic Bottom Water and lower Circumpolar Deep Water that upwell to middle-depth in the North Pacific and return south as PDW (Schmitz, 1996).”

3. 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.
»This is a matter related to the reply 2. We agree with you on the AAIW influence. According to Qu and Lindstrom (2004), indeed, AAIW has little influence on the Philippine Basin at present. Then, we revise the sentence as below. “The bottom of this site would be mainly under the influence of a mixture of NPIW and PDW. Below 800 m, salinity increases at depth, indicating the decreasing contribution of the NPIW and the increasing contribution of relatively more saline PDW. We interpret the downcore data as changes in mixing ratio of the two water mass or variations in properties of the each water mass itself.“

-Surface sediment samples and core GH08-2004:
1.Line 23 pag. 1272: Was there any reason for not including the core top of GH08-2004 in the calibration?
»Thank you for pointing out. No reason, so we will add the core top data of GH08-2004 in the calibration.

2.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?

» The core material was subsampled at 2.2 cm intervals, but we don't use all the samples for the measurement due to lack of foraminifera. On average, we measured the samples at 3.6 cm intervals for the stable isotopes and 4.3 cm for the trace elements.

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

» We revise the sentence as you suggested.

4. 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.

»We revise the sentence as below as you suggested. "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:

1. 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.

»Yes, we keep this order. In Boyle (1995), he noted that a change in procedure. He revised the order of treatments to have the reductive cleaning before the oxidative

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cleaning in order to eliminate an artifact due to CdS precipitates (Rosenthal, 1994).

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

» We will omit this sentence as you suggested.

3.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?
»We apologize for the confusing sentences. These are in the wrong order. In lines 26-28, and line 1 in the next page, we would like to revise the text as below. "For twenty surface sediment samples, the foraminiferal tests were split into several sample cups after the cleaning steps and measured the trace element for the each sample cup. Obtained Mg/Ca ranges for each horizon are expressed as error bars in Fig. 6. For two (331 and 207) of the surface sediment samples, the foraminiferal testes were repacked, cleaned, and measured the trace element analysis."

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

» Line 2 pag. 1275: We use the SPEX Claritas PPT certified solutions as Standard elemental solutions not only for Sc but also for other elements. To all samples and standard solutions Sc was added to make 1 ppb Sc solutions.

5. 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.

» Please see the reply for the reviewer 1 for discussion of carbonate ion effect. We compare Mg/Ca value with carbonate saturation state and found a strong positive correlation with $\Delta[CO32-]$ ($R2$ =0.97) at BWTs between 1.7 °C and 16.3 °C. However, each effect is hardly quantified because there is a robust relationship between BWT and $\Delta[CO32-]$ in the wider range. Then we use the modern BWT vs $\Delta[CO32-]$

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diagram in order to discuss more about the each effects of BWT and $\Delta[CO_3^{2-}]$ on Mg/Ca. In a lower BWT range (< 5°C) the modern $\Delta[CO_3^{2-}]$s are relatively constant (10 umol/kg), where BWT effect can be solely evaluated. Then, the correlation becomes weaker in the lower BWT range (< 5°C) but is still statistically significant (R^2=0.57, p <0.0001).

-Age model:

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

» The time resolution achieved was 340 yr and 430 yr for the isotopes and trace element analyses, respectively.

2. Ages were 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?).

»This is an English mistake. We interpolate ages between 14C date. Then, we revise the sentence as below. "Ages of GH08-2004 are interpolated between 14C dates."

-Downcore results:

1. 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

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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.

» Here, we mean the average value for the LGM. We will revise the text.
»Thank you for the correction. Your calculation is correct. We used a wrong equation (BWT =(Mg/Ca-0.64)/0.14), which was happened by mistyping. We sincerely apologize for this careless mistake and we will check the calculation equations all the way thorough the revised manuscript. As you pointed out, 0.8 mmol/mol (0.83 mmol/mol is more accurate if it is rounded off to two decimal places) is an average value for the LGM (18-24 ka). In the revised manuscript, as it is written in the comment for the reviewer 1, we use an equation, Mg/Ca=0.10(±0.02) BWT+0.79 (1). 0.83 mmol/mol yields a BWT of 0.4 °C. The tick label in Fig. 9b is placed in mismatched positions though making PDF format figures. We recalculate BWT and d18Ow with Equation (1).

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.

»Thank you for the correction. The wording was wrong. We change negative/positive to high/low.

3. 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. Here it appears disconnected.

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» Thank you for your suggesting. In the revised manuscript, we reorder sections concerning downcore records as below.

6. Downcore results

6.1.1 Benthic Mg/Ca

6.1.2 Evaluation of carbonate saturation effect on Mg/Ca

6.1.3. Conversion of foraminiferal Mg/Ca to temperature

In section 6.1.2 of the revised manuscript, in order to estimate the carbonate ion effect on the temporal Mg/Ca changes, we present another proxy, an index using Globorotalia menardii fragmentation, which could reflect the carbonate saturation state though it is qualitative, and discuss the effect on our Mg/Ca record (See also our reply on the reviewer 1). We think we can move the above sentence to section 6.1.2.

4. 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):

»Thank you for your suggestion. We will revise the sentence as you suggested.

5. 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.

»We apologize for our mistake. Yes, this is d18Ow (PDB) not d18Oc (PDB). We will revise it to the correct term. We will change the term "residual d18Ow" to "local d18Ow"

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as it is suggested.

6. 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.
» Unfortunately, we cannot conduct a third measurement due to lack of foraminifera samples for the core top. The core top yields $\Delta\delta$18Ow of 0.57‰ for C. wuellerstorfi type B, which is about 0.9‰ higher than that expected in equilibrium with ambient water (-0.3 to -0.4‰ Suzuki et al., 2010). Even when we take a modern δ18Ow value of -0.14 $\pm$0.03‰ (1σ, n=17), which is an average value for 1000-2000 m water depth in the northwestern Pacific (120-180 °E, 20-40 °N), the difference is still large (0.7‰). Considering that δ18Oc of C. wuellerstorfi type B increases by 0.5‰ in the youngest horizon and shows large difference from δ18Oc value of C. wuellerstorfi type A, we would rather not rely on the core top value. Middle to late Holocene average $\Delta\delta$18Ow including the core top shows -0.09 $\pm$0.39‰ (1σ, n=8), which becomes close to the modern δ18Ow value of ambient seawater.

7. 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

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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)?

»We agree with your comment. As reported by Qu and Lindstrom (2004), AAIW does not seem to have a large influence on our study area. As you pointed out, high oxy water can be interpreted as water coming out from the South China Sea (Qu and Lindstrom, 2004).

»Concerning the interpretation of our late-Holocene d13C values, thank you for providing useful figures for the modern d13C distribution. We also plot d13C and 14C distributions together with temperature, salinity, and phosphate along WOCE line P10 (Fig. 1). However, d13C minima lie below salinity minima (core of NPIW) and above 14C minima (core of PDW). That is, d13C minima are found at 1000-1500 m water depths, mixing zone between PDW and NPIW. Cibicidoides reflect δ13C of DIC in ambient water with a defined genera- or species-dependent offset in δ13C (Graham et al., 1981; McCorkle et al., 1990, 1997). δ13C offset is zero for C. wuellersrorfi, and difference between type A and type B is $\Delta\delta$13C (typeB-A) =0.03±0.10‰ (n=25). Although this is not significant compared to analytical error, adjustment factor of -0.03‰ is applied for δ13C record of C. wuellersrorfi type B. The core top δ13C of C. wuellerstorfi type B is 0.06‰ with -0.03‰ adjustment, which is rather higher than a modern NPIW value (Bostock et al., 2010) but within the modern range of δ13C of DIC of 1000 to 2000 m water depth in 0-20°N (-0.25 to +0.25‰ Fig. 1). This is consistent with a water flow, at 1500-3000 m water depth, suggested by Kaneko et al. (2001) that argued that a main inflow at 9-15°N carries PDW probably from the eastern tropical Pacific along 10°N, forming a large clockwise gyre. Therefore, we conclude that the bottom of the site is mainly influenced by PDW mixed with NPIW.

-Discussion:

1. 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).
» We rewrite the discussion section and devote descriptions that are led by our results. We add references, such as Jaccard and Glabraith, (2013), Siani et al. (2013). As you said, we also interpret Mg/Ca variations as a reflection of changes in BWT, which is supported by the proxy of G. menardii fragmentation index that will be newly presented in the revised manuscript. Temporal changes in the index does not match Mg/Ca variations.

2. 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). Incontrast,
>there is not contrast because you did not speak about H1 of the previous records.
» You are right. We will remove " In contrast".

3. 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, sim-

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ulated 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 1 °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.
» For the LGM, in Matsumoto et al. (2002), they report lower d13C values at 2000 m water depth in the North Pacific during the LGM and suggest presence of 13C depleted water mass. This water mass could be the glacial PDW because NPIW did not seem to penetrate such deep depth according to Jaccard and Glabraith, (2013).

>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.
»We reorder sections concerning discussions as below.
7.1. Ocean condition in the North Pacific during the LGM
7.2. Interpretation of the variations during the deglaciation
7.3. Pacific Ocean circulation during the deglaciation
7.4. Possible large changes in ocean circulation during the Holocene
In section 7.2, we discuss interpretations of our downcore results for the deglaciation.

4. 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

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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.
>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.
»We rewrite this section and move these sentences after line 4 pag. 1282.

5. 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?
» We apologize for the confusing figure again. We will revise the figure that compares our result with GH02-1030 result (we will check the position of the tick labels). We interpret that the BWT record of GH02-1030 reflect the rather end-member of NPIW because the location is close to the production area of NPIW. On the other hand, our BWT record reflects temperature of mixture of NPIW and PDW. BWT of GH02-1030 shows 3-4 °C during BA while our record shows 0-2 °C. This might be interpreted as a larger influence of PDW that has a colder temperature on our site than on GH02-1030 site, or simply due to lack of time resolution in our BWT record because a warm interval during BA in GH02-1030 is short (less than 1 ky) and only three data point cover this interval in our record. In the former case, ocean circulation does not seem equivalent to the present one. As is inferred by a uranium record at 682 m water depth by Jaccard

and Galbraith (2013), intermediate depths in the Pacific has more depleted oxygen that might be due to more contribution of PDW than today.

6. 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: : :"
»We apologize for the mistake. We will correct "decrease" to "increase". The text will be corrected as suggested.

7. 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-west gradient with warming in the east, cooling in the west (their figs. 3c, Okazaki, 4b Chimamoto).
»Thank you for pointing out. We agree with your comment. We will remove the sentence "The large-scale surface cooling in the subarctic North Pacific. . .".

8. 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 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.
» Thank you for your advice. We will move to the discussion about GH02-1030 to this part.

9. 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.
»As suggested, we will revise this part.

10. 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 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.,

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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 â′Lij 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
»We will add Jaccard and Glabraith, 2013 (GRL) in this part.

11. On the other hand, the an increased meridional export of AAIW at 18–17 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 16 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"?
»Yes, we think a stronger penetration of the glacial NPIW could cause the displacement of PDW.

12. 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 reorganization of Pacific circulation that was related to reduction

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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?)
» As far as we know, there is no reference. In Pena et al. (2013), they interpret their Nd isotope data as changes relative contribution between AAIW and PDW and fraction of Southern Ocean Intermediate water is 5-15 per cent and the rest is PDW during the deglaciation. Then, reduction of AAIW could be interpreted as increased PDW as well from the data of Pena et al. Thus, it would be more accurate to say that increased fraction of PDW is also supported by Pena et al., instead to say that intermediate cooling and low d13C is related to reduction of AAIW.

13. Our interpretation is consistent with Okazaki et al. (2010),
>this should be Okazaki et al. 2012
» Yes, it is. Thank you for pointing that out.

14. 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.
» We correct as it is.

15. 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.
» Yes, YD looks more like H1. We will revise this sentence.

16. 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 fol-

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lowing 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 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 16–15 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)
» Will be corrected as suggested.

17. Please, include Siani et al. 2014, Nature communications, in this discussion; they do see old waters off Chile after correcting the reservoir ages.
» Will be added, and we think this upwelling seems to connect stronger return flow of PDW, in the North Pacific, that is inferred from lower d13C and lower BWT in our record.

18. 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.
» We apologize for this mistake. Eventually, we delete this sentence in the revised manuscript.

19. You need to link here the 14C story with the d13C minima story. It appears discon-

nected. 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 16–15 ka (core TR163-19). Pena et al. (2013) find the same d13C minima at ODP site 1240 and using "Nd linked it to AAIW and SAMW. Considering the enhanced PDW signal (decreasing "Nd) 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 AAIW, continues with increase PDW. So, upwelling of DIC rich water may not be the explanation for that core.

» As you pointed out, there seems to exist some inconsistency between d13C and Nd isotope in Site 1240 of Pena et al. (2008) and Pena et al. (2013) although the authors argued that d13C were linked to the advection of Southern Ocean Intermediate Waters. As PDW is expected to have lower d13C as well, d13C record could not distinguish each water mass. If we focus more on the North Pacific, we think we could omit discussion part concerning d13C minima in EEP because the thermocline d13C record of EEP cannot be used as a water tracer.

20. In the western side of the equatorial Pacific, boron isotope data suggests that the interval from 18 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 18–17 ka and was highest at 15 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.

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» As pointed out by the reviewer 1, we agree that these sentences seem an overimplication of the data. We will delete these sentences. Our data suggest more contribution of NPIW at 17 ka and increasing (decreasing) contribution of PDW (NPIW) from 17 to 15 ka. We also find that d13C minimum associated with lower BWT of our results at 15 ka seem to correspond to well-mixed condition in the Southern Ocean. This implies that increasing southward return flow of PDW inferred by our results in the North Pacific could connect to increasing upwelling of PDW in the Southern Ocean.

-Conclusions:

1. 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”.

»We remove “benthic 13C” here. We revise the whole text to fit the discussion above.

Figures

1. 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.

» We refer to Kaneko et al. 2001 to draw the intermediate and deep water circulation in the Philippine Basin. We will correct the figure and figure caption.

2. 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.

»Will be corrected.

3. Fig. 5: could morphotype B be C. cf wuellerstorfi, also known as C. dispars? See Hayward et al. 2010 Recent New Zealand deepwater benthic foraminifera: Taxonomy,

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ecologic distribution, biogeography, and use in paleoenvironmental assessment.

» We refer to Hayward et al. (2010) and Holbourn et al. (2013). At this moment, it is considered to be reasonable that this species is classified in C. wuellerstorfi (Schwager) from the morphological point of view. C. wuellerstorfi type B has common characteristics in terms of granular and more robust test with Planulina renzi Cushman and Steinforth but differ by its less flattered, less chambers, and more depressed sutures. C. wuellerstorfi type B differs from Cibicides dispars (d'Orbigny) that is characterized by its more bloated umbilical side and flattened umbilicus. We will add "taxonomic note" in the revised manuscript.

4. 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, salinity increases towards higher values, i.e. the other way around as displayed by the arrows.
»Will be corrected.

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):
» Thank you for advising. None of us is native English speaker and English grammar is sometimes difficult for us. We will submit the revised manuscript after a native speaker check our English.

1. e.g. 1 samples were used to calibrate benthic Mg/Ca to (rather than "with") bottom water temperature »Will be corrected.

2. e.g. 2 line 6 pag. 1268: Thus, the reconstruction of: : : »Will be corrected.

3. e.g. 3 line 24 pag. 1268: In the paleoceanographic field: : : »We omit this as we

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follow the comment by reviewer 1.

4. e.g.4 line 21 pag. 1273: overgrown surface of the type B results from secondary calcite (rather than "the secondary calcite") »We will remove "the".

5. e.g. 5 Table 1. List of surface K-grab samples: : :

»Will be corrected.

6. e.g. 6 section 3.1: Surface sediment samples (rather than surface sediments) ãĂĂ »Will be corrected.

7. e.g.7 Oceanographic setting (rather than settings) Please, get that checked throughout the text. ãĂĂ »Will be corrected. We will check that throughout the text.

8. 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). »Thank you for your advice.

9. Section 2. It reads to me more correct to say: Oceanographic setting and water mass tracers »Will be corrected.

10. 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. »We will omit PSU throughout the text.

11. 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) »We apologize for the mistakes. These references will be corrected.

12. 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. »As you pointed out, we think we should better to refer Qu et al. 2004 (this would be Qu and Lindstrom, 2004) that shows northern limit of AAIW advection.

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13. 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. » Duplessy et al., 1988, Kroopnick, 1985, and Mook et al., 1974 will be added.

14. Lines 1-2 pag. 1274: washed through a 63 ïAËŻ m mesh sieve and dried in the oven at 50°C. ("for one day" is not needed). » "for one day" will be omitted.

15. Lines 17 pag. 1274: The d18O and d13C were measured: : : (idem line 19) »Will be corrected.

16. Line 11 pag. 1275: Radiocarbon analyses were carried out by accelerator mass spectrometry (AMS) on approximately 6-12 mg of the planktonic foraminifera: : :

»Will be corrected.

17. Line 15 pag. 1275: according to Table 2 you extracted foraminifera for 14C from 6 "horizons" (since 4 14C dates are from Itaki). »Yes, we did. Thank you for pointing out. We will correct the text in the age model section.

18. Line 24 pag. 1276: a Mg/ca (instead of "an Mg/Ca"). Line 26 pag. 1277: Refer here to Fig. 8. » Will be corrected.

19. Line 12 pag. 1281:INCREASE in d18Osw » Will be corrected.

20. Line 10 pag. 1283: Okazaki et al. 2012 not 2010 Line 26 pag.1282: Huang et al. 2013 » Will be corrected.

21. Line 18 pag. 1288: Capital letter for "Hydrographic". » Will be corrected.

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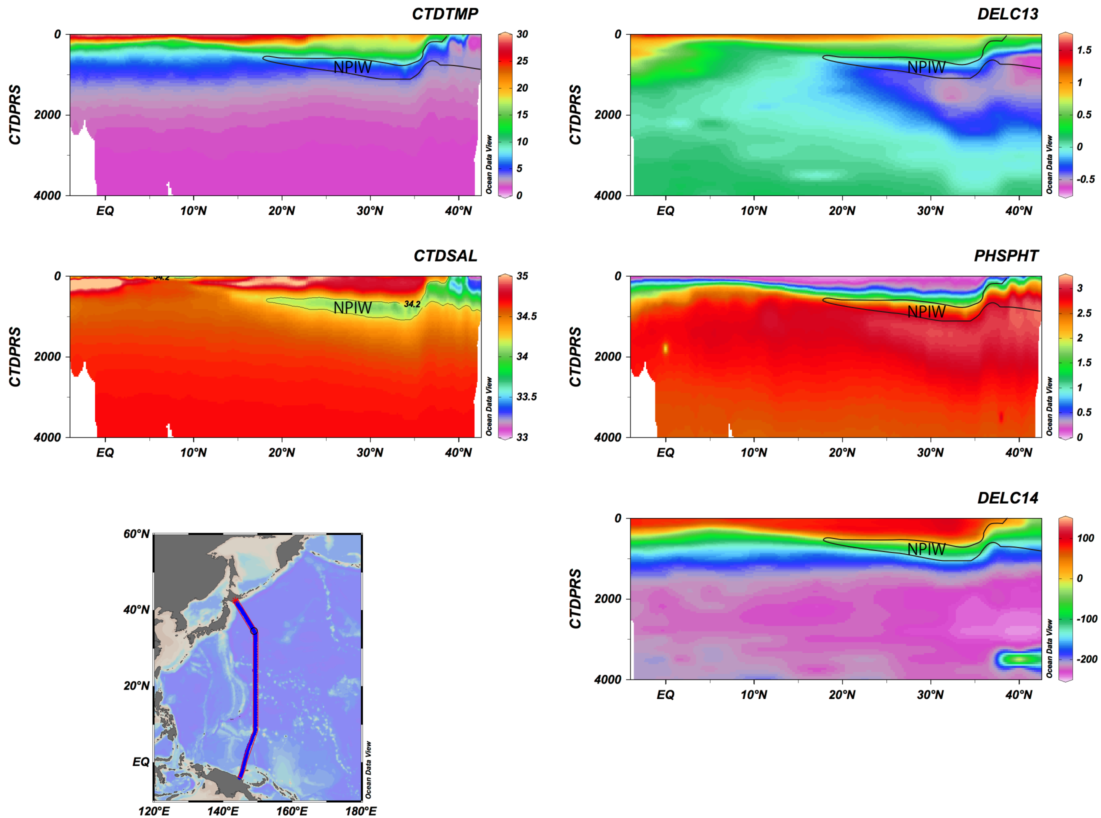

Fig. 1.

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