Response to Reviewer #1

We would like to thank reviewer #1 for his helpful and very constructive comments. We have accepted most of them and introduced the comments and suggestions in the text (new text is highlighted in blue).

We have also introduced the word "Intermediate" in the title as suggested by reviewer # 1.

The point-by-point response follows:

Major comments:

1) The study is not discussed in the context of previous work. The results for surface, thermocline and deep water are not new here. The authors seem to be unaware of substantial previous works on the oceanic response at LGM and MH for both surface and subsurface (especially on thermocline and deep/bottom water). The annual SST and SSS response and thermocline response in the MH has been discussed extensively in Liu et al. (2003). For example, regarding Fig.1 (MH), the overall feature is a cooling in the tropics and warming at high latitude, because of the increased tilting. The SSS is a direct response to the SST response. In the thermocline, the NH (SH) thermocline water is colder (warmer), because of the intensified (weakened) winter cooling and in turn the subduction (Liu et al., 2000; Liu et al., 2003). (The paper should also show the difference of subsurface temp between MH and PI, which have to be consistent with these previous studies). For LGM, the shallower NADW and greatly intensified AABW is first simulated in Shin et al. (2003) and explained in Shin et al. (2003). It is caused by the increased sea ice formation and brine injection around the Antarctic, causing the saltiest and coldest water of AABW. The TS characteristics were first discussed in Liu et al. (2005), consistent with the proxy of Adkins.

We have introduced references to previous work (Shin et al. 2003, Liu et al. 2000, 2003, 2005 and others) in the text as suggested by the reviewer. The reviewer is correct, it is important to be clear and contextualize about what was already discussed (and known) such as the annual configuration of the surface fields in the MH and LGM (e.g. Figure 1).

We included Liu et al., 2005 for the validation of LGM simulation results with proxy data as follows:
"Previous model studies have already shown theta-S relationships for the LGM and MH [shin2003, liu2005,ottobliesner2006], and their comparison to the theta-S obtained from proxy data [adkins2003] suggests reasonable agreement with observations.

We have increased our discussion about the differences between MH-PI and LGM-PI from the surface to the bottom of the water column including new panels to Figures 3 and 4.

2) The discussion of intermediate water, which is the major new result of the paper, is poorly presented. For example, L210 states: "the PI and MH show.... with similar formation in all basins". This contradicts with L268- :" reduced subduction in

intermediate levels by 2 Sv. ..But increased AAIT formation by 2 Sv., which is consistent with a seasaw ...in the Atlantic". Actually, I can't see the difference between MH and PI formation rate in Fig.6. Also, the definition of intermediate water and the calculation of the formation rate for each source is unclear from Fig.6. It gives an impression that the authors are pretty arbitrary in determining the formation rate for each source. It is best to mark the density range for each intermediate water source calculated in Fig.6 directly. Perhaps, a schematic diagram will be helpful to summarize the change of intermediate water. It will also be useful to calculate the buoyance flux as did in Shin et al. (2002), to be complementary to the formation rate. Overall, the discussion on the intermediate water needs to be much improved.

The reviewer makes good points. The discussion of the subduction section was much improved, and dubious sentences as the one cited above were excluded. We used the T-S plots to infer the range of water formation in the sigma space. For this we calculated the T-S plots first interpolating to isopycnal coordinates and then averaging in each basin, following the Downes et al. [2010] methodology. The definition of density ranges for the intermediate and mode layers is much more clear now. Also we included in figure 6 the ranges of the intermediate/modal water masses. We believe it is not necessary to add a schematic diagram, since this would be similar to previously published ones [c.f. Duplessy et al. 1988, Oppo and Lehman 1993, Ramstorf 2002, among others.

We also believe that including buoyancy fluxes calculations would increase too much the length of the paper, and probably would not include much additional information. Shin et al., (2002) did not compare the two (kinematic and buoyancy fluxes) estimates, so we do not believe it is necessary to perform this exercise. We do believe, however, that this could be something to focus in a future publication.
The discussion of intermediate waters, and the overall paper writing was much improved from the previous version.

3) Since the major highlight of this paper is on intermediate water, I suggest the title explicitly include the water intermediate water.

We agree with the reviewer. The word INTERMEDIATE was introduced in the title of the paper.

<u>Minor comments:</u>

L98: "global temperature" should be "global annual surface temperature". .... Similiar for SSS.

This was changed in the manuscript according to the reviewer's suggestions.

L107: "A bipolar temp....in the North Pacific...". I don't see it as a bipolar response, instead, it is a symmetric response: colder tropics and warmer extratropics, with a polar amplificiation in the NH due to sea ice feedback. See major comment 1.

The reviewer is right and the phrase in line 107 was deleted. The discussion suggested in major comment 1 was introduced in the text, and we specifically include

the reviewer's statement:
“Overall, there is a symmetric response that reveals a tropical cooling and high latitude warming”

L124: " no visible shift is seen in the SH.". This is consistent with Shin et al. (2003).

The reference Shin et al., 2003 was added.

L129: " Since MH results are very similar to PI they are discussed jointly". The difference of the two periods will show the systematic difference discussed in major comment 1.

We have added a more detailed discussion on the MH-PI differences (new Figure 1).

L143: -148: the mechanism for LGM AABW has been discussed extensively in Shin et al., (2002).

References were included (Shin et al., 2003 and Liu 2006)

L152-174: The TS diagram and its mechanism have been discussed in Liu et al (2005).

This reference is also mentioned in the text.

L208: Fig.6: what are the dashed lines in Fig.6? It will be useful to separate the two parts of formation rate, the lateral subduction and vertical subduction in eqn. (1) in Fig.6. Also, useful to calculate surface buoyance flux as in Shin et al (2002). Also, better to mark each intermediate water range so one can see clearly how its formation transport is changed.

From the three reviewer's suggestions we have included two. In figure 6 the dashed lines are the vertical subduction, and this is now explained in the text. We also marked the intermediate waters ranges in figure 6. About surface fluxes we answered in the question 2 above.

L254:" A .... increase in SHZW....intensifies the AAIW". Why an intensified SHZW intensifies AAIW? For example, in the formation rate eqn. (1), the increased formation can be caused both lateral and vertical subduction and the lateral subduction depends on the slope of mixed layer depth, which may not be related to the SHZW, but to surface buoyance changes. So can the change of AAIW also be affected by surface buoyance flux?

We have added a stronger discussion of the effect of winds in the the following discussion to the text following line 254

Stronger SHZW is associated with enhanced northward Ekman transport which intensifies the AAIW. Previous work shows through several numerical experiments in an ocean general circulation model, that the production of AAIW is dependent on the strength of the Southern Hemisphere winds Ribbe [2001].

AAIW changes in ventilation have been associated with Ekman heat and freshwater transport changes caused by wind stress variability in the Southern Ocean (e.g., Rintoul2002, sallee2006, naveira2009).

Ribbe, J. (2001), Intermediate water mass production controlled by southern hemisphere winds, Geophys. Res. Lett., 28(3), 535–538.

Rintoul, S. R., and M. H. England, 2002: Ekman transport dominates local air–sea fluxes in driving variability of Subantarctic Mode Water. J. Phys. Oceanogr., 32, 1308–1321

Sallee, J.-B., N. Wienders, K. Speer, and R. Morrow, 2006: Formation of subantarctic mode water in the southeastern Indian Ocean. Ocean Dyn., 56, 525–542.

L268: "During MH,...seasaw". In Fig.6, I see MH and PI almost the same. Where is the seasaw?
This is also inconsistent with previous statements that MH and PI almost the same (see major comment 2, too).

The reviewer is correct and this was stricken from the text.

L274: " A seasaw mechanism...YD". The mechanism for YD is likely related to meltwater forcing. This is very different from LGM or MH. How can it be applied?

The reviewer is correct and we have deleted the YD statement proposed by Pahnke et al. [2008]

L281: " AABW and PDW are much denser, presumably due to excess ice formation.." This has been discussed in Shin et al. (2002) and Liu et al (2005).

References were included in the discussion.

Response to Reviewer # 2

We would like to thank reviewer #2 for his helpful and very constructive comments. We have accepted all of them and introduced the comments and suggestions in the text (new text is highlighted in red).

The point-by-point response follows:

Major comments:

Fig. 6 prominently features mode waters, but these water masses are not mentioned at all in the introduction. In fact, most people would associate a "thick (low potential vorticity), outcropping mixed layer just north of the Sub-antarctic Front" (ll. 31-32) with mode waters rather than AAIW. So it seems appropriate to include a full discussion of mode waters in the paper.

The reviewer is correct. We included mode waters description extensively in the text, in the introduction and in the subduction sections:

Minor comments:

l. 75: I assume that you are analyzing version 3 of the CCSM?

Yes, The version analyzed is version 3. This was added to the text.

Section 2: How long were the LGM, MH, and PI runs integrated for? Are the results presented in this paper averages over the entire period? What about model drifts?

A detailed description of the model and its forcing is given in otto-bliesner et al 2006: the numerical simulations of the MH and LGM (simulation except for the ocean) are initialized from the PI run. The LGM ocean is initialized with a previous LGM simulation. Both are run for 300 years. At this time, as discussed in otto-bliesner et al. 2006, the simulations reach quasi-equilibrium, with small trends present, particularly at Southern Hemisphere high latitudes and the deep ocean. The mean climate results analyzed are averages for the last 150 years of the LGM and MH runs.

l.109: Please be more specific what you mean with mass transports.

This sentence was meant to be volume transport instead. It was subtracted from the text.

l.117: Are these changes "not significant" in a statistical sense? It looks like there are places with values between -0.15 and -0.2, which I wouldn't call insignificant. The patterns of wind stress changes resembles a strengthening of the Southern Annular Mode.

The reviewer is correct. We did not perform a t-test here, simply analyzed the diferences. The text was altered to avoid confusion.
Otto-bliesner [2006] examined the changes of the atmospheric modes of variability

during the LGM. While they found large changes in the Arctic Oscillation due to expanded ice coverage they found negligible change in the SAM.

ll.133-134: I don't see this statement supported by the plots.

The reviewer is correct. This sentence was excluded from the manuscript.

ll.152-161: Confusing discussion, going back and forth between upper layer and AAIW? I don't see the statement "The upper layer...fresher" reflected in the plots, so please be more clear what you mean.

We want to thank the reviewer for pointing this out. This section was rewritten, and our statements are clear now.

ll.172-174: Please elaborate.

We excluded this sentence and added a better discussion in this section.

l.179, 188: So the "annual maximum depth of the mixed layer" is the average of monthly maximum mixed layer depths, rather than the maximum of monthly maximum depths?

We clarified this statement in the text, and the latter statement is the correct one. The text is now written as follows: "The mixed layer depth is calculated following the criterion of Large et al.[1997], and H is the maximum winter time mixed layer depth, averaged over the last 20 years of model integration for each run."

l.178-189: So there are only contributions where the maximum mixed layer depth breaches through an isopycnal?

The reviewer is correct. The subduction is defined at the outcropping of an isopycnal, and the outcropping region is where the mixed layer encounters a certain isopycnal, in that water subducted can pass through the base of the mixed layer.

ll.220: This statement does not hold for the South Indian Ocean and the northern basins.

Reviewer is correct and this was added to the text.

ll.208-251: I found this discussion all but incomprehensible.
Please rewrite. In particular, it is not clear at all where the AAIW and AABW feature in figure 6.

Thanks for the suggestion. We redid Figure 6 showing the range of intermediate water and improved the discussion. Please refer to the new text.

Caption Figure 1: Please mention on what levels these variables are shown. Are these multi-year averages? Please be a bit more specific about the velocity vectors shown. What do they represent (barotropic velocity? surface level velocity?), what magnitudes do they represent,did you use a threshold, etc.

Figure 1 caption and legends were changed. What is shown are changes relative to PI of surface temperature (SSTA) and salinity (SSSA). The vectors are the average of the upper 80m of velocity anomalies, using the magnitude of 1 cm/s as a threshold.

Caption Figure 2: Please mention that these are wind STRESSES.

We added the word <u>stresses</u> to the caption of Figure 2.

Figure 6: What are the dashed lines? Tick marks at 27 and 29 would be helpful as well. And maybe indications of the water masses discussed in the text.

Figure 6 was re-done according to suggestions.

Response to Reviewer #3

We would like to thank reviewer #1 for his helpful and very constructive comments. We have accepted all of them and introduced the comments and suggestions in the text (new text is highlighted in green).

Following the suggestions and comments, the discussion and comparison of the results was re-written .

Major comments:

- The description of the results from p. 12 on does not seem to correspond to the figures and is difficult to follow. Part of the problem certainly comes from the fact that no definition is given of the dashed curves shown in Figure 6. It is unclear whether some of the discussion relates to these dashed curves or not. The entire paragraph preceding section 4 (l. 233-251) is particularly strangely discordant with respect to the figures. Also, the numbers given in Table 1 for the Indian Ocean do not correspond to what can be seen on Fig. 6C. Other examples of unclear sentences include line 221-224: I do not see any "jump" between PI and MH on the figures.

Thank you for pointing this out. This was a concern of all reviewers. This section was re-written and the figure 6 is now better explained and also shows the region of intermediate water formation.

- The statement on line 229-230 deserves an explanation: why would vigorous AAIW formation compensate for "the excess AABW production"? what does mean the term excess here? The reference to Oppo & Fairbanks, 1987 is certainly inappropriate since this article mainly describes increased northward extent of AABW and increased westward extent of the Mediterranean Overflow Water during the LGM with respect to Holocene.

The reviewer is correct. The idea was to show that the AAIW is displaced upward by a broader layer of AABW . This sentence together with the ill-chosen reference was deleted from the text.

- l. 246-248: Cleroux et al. (2011) and Galbraith et al. (2007) concern different water depths: 0-700 m and ~3500 m, respectively, so the sentence is completely contradictory and unclear.

The reviewer is correct and the sentence was removed.

- l. 205-207: there are studies showing STF shifts between cold and warm stages in the South Atlantic similarly to what is observed in the South Indian Ocean (e.g. [Dickson et al., 2009 ; Vazquez Riveiros et al., 2010].

Both references were added .

- The discussion section needs to be developed. For instance, the first paragraph states that there is an increase in AABW and AAIW formation during the LGM with respect to the PI, but does not say anything on how this affects the relative importance of these two water masses.

The formation and circulation of the AAIW is an important component of the upper branch of the meridional overturning circulation that is associated with the transport of heat and salt within the southern hemisphere subtropical gyre (Schmitz, 1996; Sloyan and Rintoul, 2001a; Talley, 1996, 2003). The AABW mostly forms in the Ross and Weddell Seas, spreading below NADW, and associated with the lower branch of the MOC. It's formation is a combination, among other things, of sea-ice /ice-shelf melt and brine rejection. Therefore sea-ice changes in the Southern Ocean have a significant role in the modulation of changes in the ocean's meridional overturning (Goosse and Fichefet, 1999, Shin et al. 2003).
With the increased sea-ice formation and expansion at the LGM, there is an associated increase in the surface density flux off Antarctica in the Southern Oceans (Shin et al 2003, Shin et al 2003b, Clauzet et al 2007) causing deep circulation changes seen (i.e. enhanced AABW). As discussed by Duplessy et al. 1998, Liu et al. 2003) paleoclimate records also suggest a shallower and weaker NADW circulation and an enhanced AABW intrusion into the North Atlantic at LGM accompanied by intensification of westerly winds and AAIW production.

In fact, Mckay et al. 2012 discuss, in the context of the late Pliocene cooling, how Southern Hemisphere strengthened westerly winds are associated with a more vigorous ocean circulation and have been linked to increased production of AAIW at the LGM (Muratli et al. 2009) .

- The second paragraph is too vague and it is not possible to understand what the authors mean. For example, the authors have omitted to specify the time period to which the first sentence applies. Further down, the use of "Although" (line 261-262) is incorrect: an increase in upwelling in the Southern Ocean during northern hemisphere cold intervals is certainly in agreement with many paleodata studies (e.g. [Anderson et al., 2009 ; Skinner et al., 2010; Spero and Lea, 2002]) synthesized by Denton et al. (2010). Concerning the last sentence of that paragraph (l. 264-267), an explanation of which aspects of the LGM simulations are in agreement with AAIW data is lacking.

The discussion was amplified and parts were re-written. The use of "Although" was deleted and the comment was included in the text.

- The first half of the last paragraph (l. 268-274) should be clarified.

The reviewer is correct and the paragraph was removed from the text.

- l. 274-276: this statement can not stand alone; it should lead somewhere in the reasoning.

Discussion was increased

- l. 279-284: the authors speak of "excess ice formation in the CCSM" which presumably refers to a too large simulated sea ice extent around Antarctica during the LGM. However, the meaning of this portion of the discussion is unclear since the authors do not give any reason for which the features described (i.e. denser and more stagnant AABW and PDW, increased C concentration in the deep ocean during the LGM with respect to the PI) would be wrong.

Explanation was added (see earlier response)

minor remarks
- l. 39-41: the sentence is awkward.

The sentence was changed.

- l. 60-61 : the end of the sentence is unclear: does it mean that the observed cooling + freshening resulted in unchanged isopycnals?

We removed this sentence from the text.

- l. 66-67: it seems to me that the computed subduction rate corresponds to the rate at which water is transferred downward from the maximum depth of the mixed layer, as explained in section 3.2, rather than the rate "at which fluid is permanently transferred into the main thermocline". The latter definition is misleading and gives the impression that the fluid permanently remains in the thermocline.

The reviewer is right. We replaced this statement by"... we quantify the rate of water formation across the base of the mixed layer".

- Figure 1 caption: I assume it is "surface" temperature and salinity? This must be specified.

Figure 1 caption includes now the details of the figure.

- l. 118-122 and l. 153-155: what is the utility of these sentences in the article?

l. 118-122 reference to the AO was deleted.
l. 153-155 this sentence was included in other context, of the validation of T-S properties of water masses in the discussed paleoclimates.

- l. 138-139: unclear: AAIW is comparable in the Atlantic and Indo-Pacific plots, so I assume, the authors speak of the N. Pacific intermediate waters?

The reviewer is correct. This sentence was re-written as follows:
"In the South Indo-Pacific, as observed in the South Atlantic during the LGM, formation of AABW and Pacific Deep Water (PDW) are increased (Figure 4d). The volume increase of the PDW displaces the LGM intermediate waters in the Indo-Pacific to shallower depths (above 800 m)."

- l. 157-158: "than during the PI" should be specified.

This sentence was re-written.

- l. 160-161: I do not understand the sentence. Which observations? There are no measurements on the figures.

Reviewer is right and sentence was deleted.