We are pleased by the very positive and constructive nature of the reviews. We have made every attempt to incorporate their suggestions and the comments of the editor and two anonymous reviewers. Please see the details of our implementation of the reviews below.

Points raised by editor and reviewers are shown in blue, Arial type, while our responses are shown in black, Times New Roman type.

Editor

Thank editor for his positive comments. We would like to present our deep appreciation for giving us very helpful suggestions.

E1: Two reviews have been received for your manuscript, which are supportive of both data analysis and interpretations. Reviewer 1 in particular comments on the value of the synthesis of your new data with existing records.

Thank editor for summarizing and listing the pertinent issues raised by the reviewers. We have addressed all points, including comments, questions, and suggestions given by reviewers. Please refer to our reply and modifications shown below.

E2: Both reviewers provide a number of constructive comments which you should consider carefully, and incorporate and/or respond to. Reviewer 1 seeks some additional detail in your data evaluation and interpretation. Reviewer 2 seeks clarification and discussion of how your new record relates to the published materials which you draw on for your interpretations. Please provide a reply to these comments, which should include notes to where these suggestions have been incorporated into a revised version where relevant.

Thank editor. We have revised our manuscript based on all issues raised by reviewers. Please see below for details. We also provide revised version with “track-changes” and a clean copy of revised final text.

Reviewer #1: anonymous

We would like to thank this reviewer for reviewing our manuscript, giving us very valuable suggestions and comments, and pointing out mistakes we made. Our response is shown as two sections: (1) general suggestions, and (2) two specific comments.

R1.1 General suggestions: “The paper "Millennial meridional dynamics of Indo-Pacific Warm Pool during the last termination" by Lo et al. present new temperature and seawater $\delta^{18}O$ estimation data from a marine sediment core collected East of Papua-New Guinea. They put their results that encompass the last deglaciation in context with other such records from the northern and southern Indo-Pacific warm pool.

They show that the anatomy of changes in temperature and regional precipitation in that region during the deglaciation were depending on the hemisphere. The article is well written, straightforward and points to coherent regional differences in the sensitivity of SST and rainfall with respect to the timing of climate events occurring at high latitudes. Instead of adding another record to the pile of other datasets published in the region, the authors have wisely opted for trying to map the likely boundary of precipitation anomalies during the H1 and YD, which may be useful for others studying the region in the future. I suggest the paper to be published after minor revisions that I list below."

We thank this reviewer for giving the positive comments on our manuscript and suggesting our paper to be published after minor revision.

R1.2 Specific comments:

R1.2.1 Specific comments: "Despite a clear N-S SST seesaw seen in the N- and S-IPWP stacks, the rainfall pattern of rainfall anomalies is clearly different (compare Fig 2 and Fig.6). This is, to me, the most interesting result, and I suggest the authors to point to this dissymetry more clearly in the discussion and the conclusion – clearly mentioning the mismatch between the geographical pattern of the N-S SST and $\delta^{18}O_{sw}$ dipoles."

Thank reviewer for expressing this issue. We have added about the geographical mismatch in our discussion (Lines 288-290 of the annotated manuscript, hereafter) and conclusion (Lines 314-316).

R1.2.2 Specific comments: "On such mismatch, would it be possible that the regional currents can dispatch high-salinity surface waters through the Indonesian through flow, contrarily to SST changes? I just though about such possibility after realizing the regions wetter for H1/YD – apart of the MD08 – are from continental archives. Also, the stalagmite from Borneo (Partin et al., 2007, Nature) show no particular dry anomalies during that time period."

Thank reviewer for proposing this very interesting mechanism. The S-IPWP (especially from the Coral and Solomon Seas) does contribute high salinity surface water to the Indonesian through flow (ITF) through the New Guinea coastal current system, however, there is still lack of direct link to estimate the salinity contribution from S-IPWP to ITF. More terrestrial and marine records should be built in the near future to further solve longitudinal or zonal connection between Pacific to Indian Ocean in the IPWP region.

R1.2.3 Specific comments: "Can the authors briefly comment on why they think other proxies may provide other stories? In particular, some records employing alkenoens point to cold anomalies during the YD/H1, in particular in South China Sea. Do the authors think we should deal with water column and/or seasons sampled by different proxies?"

Thank reviewer for pointing out this inter-proxy comparison issue. It is difficult to well quantify the potential differences between different proxies. For conservative consideration, we compared our records with published studies using the same single species (*G. ruber*, *s.s.*, white) and tracers (δ^{18}O-Mg/Ca) to build solid stack records. The example that reviewer mentioned is also very good: specific region with strong monsoon may bias the application of sea surface temperature proxies. We think it would be great to go into the details of seasonality and water column in every specific site. It would be an in-depth study; but it is beyond the current scope of this work.

R1.2.4 Specific comments: "Figure 3: there seem to be a lower resolution in the $\delta^{18}O_{sw}$ compared to *G. ruber* $\delta^{18}O$ and Mg/Ca between 14 and 16 ka. Is that because the samples of *G. ruber* $\delta^{18}O$ and Mg/Ca do not perfectly correspond to the same depth horizons?"

Thank this reviewer for pointing out this resolution issue. This reviewer was correct. Some $\delta^{18}O$ and Mg/Ca data do not correspond to the same depth horizons. Please refer to the corrected version of this figure (new Figure 3). It clear catches the H1 $\delta^{18}O_{sw}$ increasing period. Figures 4B is also corrected with the new dataset.

Reviewer #2: anonymous

We would like to thank this reviewer for reviewing our manuscript, giving us very valuable suggestions and comments, and pointing out mistakes we made. Our response is shown as two sections: (1) general suggestions, and (2) two specific comments.

R2.1 General suggestions: "Lo et al. presented here geochemical datasets on planktonic foraminifera from a new marine sediment core, MD05-2925, retrieved from the Solomon Sea. Using coupled Mg/Ca ratio and $\delta^{18}O$ from the foram tests, they successfully established records of both thermal (SST) and hydrological ($\delta^{18}O_{sw\text{-}ivc}$) changes through the last deglaciation at the core site. They found that the SST record resembles the temperature change over Antarctica, whereas the $\delta^{18}O_{sw\text{-}ivc}$ profile follows approximately the temperature variation in Greenland. The observed asynchronous changes between SST and $\delta^{18}O_{sw\text{-}ivc}$ suggest different control mechanisms on variations of IPWP SST and precipitation during the deglaciation. The authors then grouped 6 marine cores from the North and South of the eastern IPWP, respectively, and came out stacked SST and $\delta^{18}O_{sw\text{-}ivc}$ profiles for both sides of the Equator. They found that, surprisingly, the N- and S- stacked SST records are different in timing of initial warming as well as degree of warming during the abrupt events, such as H1 and YD. On the other hand, both the N- and S- stacked $\delta^{18}O_{sw\text{-}ivc}$ records share the same trend of isotopic depletion after ~18 ka, indicating a broad rainfall increasing in the whole region. The most exciting piece of information resulted from this practice is that Lo and his co-authors were able to use the divergences between the N- and S- stacked records, i.e. the N-S gradients of SST and $\delta^{18}O_{sw\text{-}ivc}$, to tease out the mechanisms that possibly control IPWP SST and rainfall patterns. We found that the approach presented in the manuscript is novel, and results are interesting, and we would be happy to recommend publishing the manuscript if the authors can consider the following comments in their revision."

We thank this reviewer for giving the positive comments on our manuscript and recommending it for publication after revision.

R2.2 Specific comments:

R2.2.1 Specific comments: "First of all, our main concern is that the reconstructed $\delta^{18}O_{sw\text{-}ivc}$ from the studied marine core MD05-2925 (Fig. 4B) does not look quite similar to the S- stacked profile (Fig. 5C), in either absolute value or trend. For example, Fig. 4B presents the largest magnitude of $\delta^{18}O_{sw\text{-}ivc}$ rising in YD, suggesting a dramatic rainfall decrease. However, Fig. 5C shows a sharp $\delta^{18}O_{sw\text{-}ivc}$ decreasing in YD, suggesting a likely rainfall increase. In fact, the wet YD shown in the stacked $\delta^{18}O_{sw\text{-}ivc}$ curve is not consistent either with the dry H1/YD pattern in Fig. 6."

Thank reviewer for pointing out this issue. The data used in Figure 5C are "$\delta^{18}O_{sw}$" without sea level corrections (both N- and S-IPWP stacked). It is why the pattern and absolute value are different between regional stack and MD05-2925. Please refer to the revised figure. H1/YD clear show increasing trends of oxygen isotope values. We also revised our description of both stack $\delta^{18}O_{sw\text{-}ivc}$ records (Lines 231-240).

R2.2.2 Specific comments: "The authors summarized in Fig. 6 a map of proxy-inferred precipitation in H1/YD, and proposed a precipitation boundary outlined between sites in brown and blue. Out of the three "wet" sites, however, only one (MD28) is a marine record. And, the interpretation of a wetter H1/YD at MD28 was concluded from "sediment thorium isotopic proxy" (page 3405, lines 11-13). But in all the other sites (brown dots), the dry condition was derived from $\delta^{18}O_{sw}$. Climate signatures of the two different proxies are possibly comparable. But, the original publication on MD28 core (Shiau et al., 2011, GRL) did report $\delta^{18}O_{sw}$ data. Then, why not the authors cite MD28 $\delta^{18}O_{sw}$ data here instead? And, should the record also be included in the S-stack?"

Thank reviewer for raising this inter-proxy comparison problem. In our material and method, we built up identical proxy-based stack records to prevent potential problems between different proxies. We used SST and $\delta^{18}O_{sw}$ records inferred from only *G. ruber* (*s.s.,* white) $\delta^{18}O$ and Mg/Ca data. This consideration limits the number of proxy records used; however, this methodology provided solid stack records. As this reviewer stated, Shiau et al. (2011) built very important records using different proxies of alkenones for SST and sediments thorium for runoff. The records were used for comparison in this study.

R2.2.2 Specific comments: "A few other comments, mostly cosmetic:

1. Page 3398, line 13. Meridional SST gradient is actually around 1°C during YD.

Corrected (Line 39).

2. Page 3398, line 14. “events” and “snapshots” are redundant. Delete one of them.
Corrected (Line 42).

3. Page 3398, line 15. “... the southern hemispheric branch ...” instead of “... the southern hemispheric convection branch . . .”.
Corrected accordingly (Line 42).

4. Page 3400, line 16. “. . .cleaning. . .” instead of “. . .clean. . .”.
Done (Line 103).

5. Page 3400, line 17. which equation was used here to derive SST from Mg/Ca ratio?

Thank this reviewer for pointing out this mistake. We did not offer this information in the original manuscript. We used a composite equation by Anand et al. (2003). In the revised manuscript, we wrote “We used a composite Mg/Ca-SST equation by Anand et al. (2003) to calculate SSTs.” (Lines 105-107). Anand et al. (2003) was also added in the reference list.

6. Page 3400, line 23. “. . ., and data are reported with respect to. . . (VPDB).” Instead of “. . ., with respect to . . .(V-PDB).”
Corrected (Line 114).

7. Page 3401, line 13. “... the published age models for ...” instead of “...an age model for . . .”.
Corrected (Line 134-135).

8. Page 3401, line 23. in Fig. 3, the numbers of data points for $\delta^{18}O$, Mg/Ca and $\delta^{18}O_{sw}$ are different. Why?

Thank reviewer for pointing out this problem. Reviewer #1 also gave this comment. We have corrected and revised Figure 3. Please refer to **R1.2.4** for details.

9. Page 3402, line 13. “. . . a strong climatic. . .” instead of “. . .a strong synchronously climatic. . .”.
Corrected (Line 165).

10. Page 3402, line 13. “. . . change of greenhouse gas concentrations . . .” instead of “. . . greenhouse gases . . .”
Corrected (Line 166-167).

11. Page 3402, line 19. “. . . the east equatorial Pacific (EEP) . . .” instead of “. . .the east equatorial Pacific. . .”.

Thank this reviewer for pointing out this mistake. We have corrected accordingly (Line 174).

12. Page 3402, lines 15-20. This mechanism is very intriguing. However, if it works, how does this current advection (“ocean tunneling”) affect ^{14}C age reservoir correction and water salinity at eastern IPWP sites?

This ocean tunneling mechanism was first proposed by Pena et al. (2008), agreeing with modern observations (Qu et al., 2013). The effect on ^{14}C reservoir correction and salinity is not clear; however, Sarnthein et al. (2011) did report several "^{14}C plateaus" during the last termination in Timor Sea. Fortunately, the observed "^{14}C plateaus" could bias ±100-500 years, which is not significant in this study. For the effect on salinity, it could be more difficult to evaluate. According to the Qu et al. (2013) estimation, ~70% of the high salinity South Pacific Tropical Water (SPTW) enters equatorial Pacific and resurfaces and mixes with the surface water mass. The average SPTW salinity is around 36.0-36.5 PSU, and the sea surface salinity (SSS) around the Solomon Sea is 34.5-35.0 PSU. However, there is no direct evidence of the SPTW salinity changes, nor the subduction/resurfacing rates during the last termination. Here we use the constant rate of SPTW resurfacing through the past 30 kyrs.

13. Page 3402, line 23. Fig. 4a also shows a warming during B/A, despite that it is relatively subtle, ~1 ^{o}C. Why?

Thank reviewer for giving this comment. First we eliminate the greenhouse gas concentrations effect because they stayed at the similar range during B/A period. The local (15^{o} S) mean annual insolation decreased during B/A. Thus, we argue that the slightly warming in the S-IPWP may due to the mixing between warmer N-IPWP during the Northern hemisphere warming B/A period (Line 178-180).

14. Page 3402, line 25. "A relatively stable . . ." instead of "A relative stable . . .".
Corrected (Line 181).

15. Page 3402, line 26. " . . . from 23.0 to 16.0 ka." instead of " . . . from 23.0-16.0 ka.".
Corrected (Line 182).

16. Page 3403, lines 3-5. The $\delta^{18}O_{sw-ivc}$ increase in the Solomon Sea might be partially attributed to a strong evaporation, during to a higher temp. during H1 and YD. But of course, it could be argued that the evaporation effect should be negligible, as a lower salinity was registered in B/A while temp. could increase for about 1 ^{o}C. Nevertheless, the evaporation effect should be acknowledged in the context.

We have corrected accordingly. We wrote "The dramatic $\delta^{18}O_{SW-IVC}$ increases during H1 and the YD likely resulted from a weakening and/or southward shift of the ITCZ (Chiang and Bitz, 2005; Broccoli et al., 2006), and local evaporation could also be involved." (Line 188).

17. Page 3403, line 6. ". . . from high-latitude Northern Hemisphere . . ." instead of ". . . from high Northern Hemisphere . . .".
Corrected (Line 190).

18. Page 3403, line 18. "This timing is synchronous with . . ." instead of "This timing is synchronously with . . .".

Corrected (Line 205).

19. Page3403, line 21. "Instead, . . .represents. . ." rather than "Instead of that, . . .represent. . ."
Corrected (Line 209).

20. Page 3404, line 25. "Observations over . . ." instead of "Modern observatory data over. . ."
Corrected (Line 245).

21. Page 3406, lines 13-15. This is a rather vague statement. What are the hemispheric climate events? Did the authors refer to temperature changes over Greenland or Antarctica? Authors also mentioned a few times in the text "greenhouse gas concentration". Better to state it clearly as the radiative forcing of atmospheric greenhouse gases.

Thank this reviewer for giving this comment. We wrote "Here we propose a new process of the thermal evolution of IPWP region, which meridional differences of its thermal gradient could amplify the signal from high latitude Northern hemisphere climate events and radiative forcing from greenhouse gases." (Lines 307-308).

22. Page 3406, lines 18. ". . . HC anomalies." instead of ". . . HC circulation anomalies." 23. Figure 6. "MD65" was labeled twice.

Thank this reviewer for pointing out this mislabeling. We have corrected on the figure 6. One is MD65 and the other is MD78.

References:

Pena, L. D., Cacho, I., Ferretti, P., and Hall, M. A.: El Niño-Southern Oscillation-like variability during glacial terminations and interlatitudinal teleconnections. Paleoceanography, 23, PA3101, doi: 10.1029/2008PA001620, 2008.

Qu, T., Gao, S., and Fine, R. A.: Subduction of South Pacific tropical water and its equatorward pathways as shown by a simulated passive tracer. J. Phys. Oceanogr., 43, 1551-1565, 2013.

Sarnthein, M., Grootes, P. M., Holbourn, A., Kihnt, W., and Kühn, H.: Tropical warming in the Timor Sea led deglacial Antarctic warming and atmospheric CO_2 rise by more than 500 yr. Earth Planet. Sci. Lett., 302, 337-348, 2011.