We sincerely thank the reviewer for taking the time and effort to read and comment on the manuscript. We think we can revise the manuscript according to the suggestions. We address each comment below, and have included the reviewer's comment in *grey italics* to aid the readability of this document.

*Reviewer #1: The article by Hällberg et al. presents an interesting and well thought study of seasonal variability of rainfall and climate over the last deglaciation in the Island South-East Asia (ISEA) region. The authors focus on how rainfall has changed regionally above the main landmass, that have themselves considerably been reshaped and shrinked along the deglacial eustatic sea level rise, and compare their transient model output with landmark paleoclimate (speleothem) reconstructions from different localities in the Indonesian archipelago. They detail, in particular, the seasonality which seems to have been greater at 12 ka as compared to nowadays, and manifest as the development of a dry austral summer dry period in the southernmost part of the ISEA. They discuss their results in light of peat deposits that started accumulating during the Holocene, leading to a potential savanna corridor connecting the main islands of Indonesia and plains in the Sunda and Sahul shelves prior to their exundation.*

*The analysis is nice, and I believe it should be published after some corrections that may be easy to deal with but could improve the manuscript, as detailed below.*

Replies to Reviewer #1

*Reviewer #1: First, the PI temp and precip patterns should be compared to modern data in Figure 3 or elsewhere, to highlight the ability of the model to reproduce local-scale climate variability, and even perhaps at seasonal and interannual timescales. The model seems to perform well, but rainfall seasonal and interannual variability at equatorial latitudes is, I think, notoriously challenging to reproduce with fully coupled GCMs. The authors should do better to convince the reader that the model is able to reproduce with a decent precision and accuracy at least the modern climate variability prior to inferring the transient evolution of past regional rainfall in such a complex environmental setting, with coastlines constantly varying.*

Reply: The CESM1 version used here is one of the scientifically validated releases by NCAR and is widely used in different studies including a validation of SE-Asian monsoon characteristics. Shown in Fig. R1_1 is the annual difference between the simulated and observed precipitation (presented at https://www.cesm.ucar.edu/experiments/cesm1.0/diagnostics/b40.1850.track1.1deg.006/atm_863-892-obs/ accessed 26-04-2022). These differences are also further discussed in the context of monsoons and ENSO by Meehl et al. (2020). The main discrepancies in the CESM1 simulations of modern climate compared to observations are 1) a westward displacement of precipitation maximum over the Indian Ocean, 2) the double ITCZ over the Pacific (see Zhang et al., 2019 for more details), and 3) a too strong Pacific Cold tongue (Meehl et al., 2020). The Maritime Continent precipitation is well reproduced by CESM1; small discrepancies consist of a slightly too dry Borneo, and slightly wet southern Indonesia. Since the model validation has been published previously in detail, we do not see a benefit of including a comparison in this manuscript. The remaining question is how realistic the response to late glacial conditions simulated in our 12k and 13k runs are, and the only way to validate the paleoclimate runs is to compare with proxy evidence. We show from the proxy compilation in our manuscript that the CESM1 in general is consistent with proxies from 12 ka BP.

We propose to refer to the CESM1 diagnostics webpage and the Meehl's paper in the methods section of our manuscript, to refer the reader to CESM comparisons previously done by others.

References:
Zhang, G. J., Song, X., Wang, Y. (2019) The double ITCZ syndrome in GCMs: A coupled feedback problem among convection, clouds, atmospheric and ocean circulations. Atmospheric Research, 229, 255-268, https://doi.org/10.1016/j.atmosres.2019.06.023.

Meehl, G. A., Shields, C., Arblaster, J. M., Annamalai, H., & Neale, R. (2020). Intraseasonal, seasonal, and interannual characteristics of regional monsoon simulations in CESM2. Journal of Advances in Modeling Earth Systems, 12, e2019MS001962. https:// doi.org/10.1029/2019MS001962

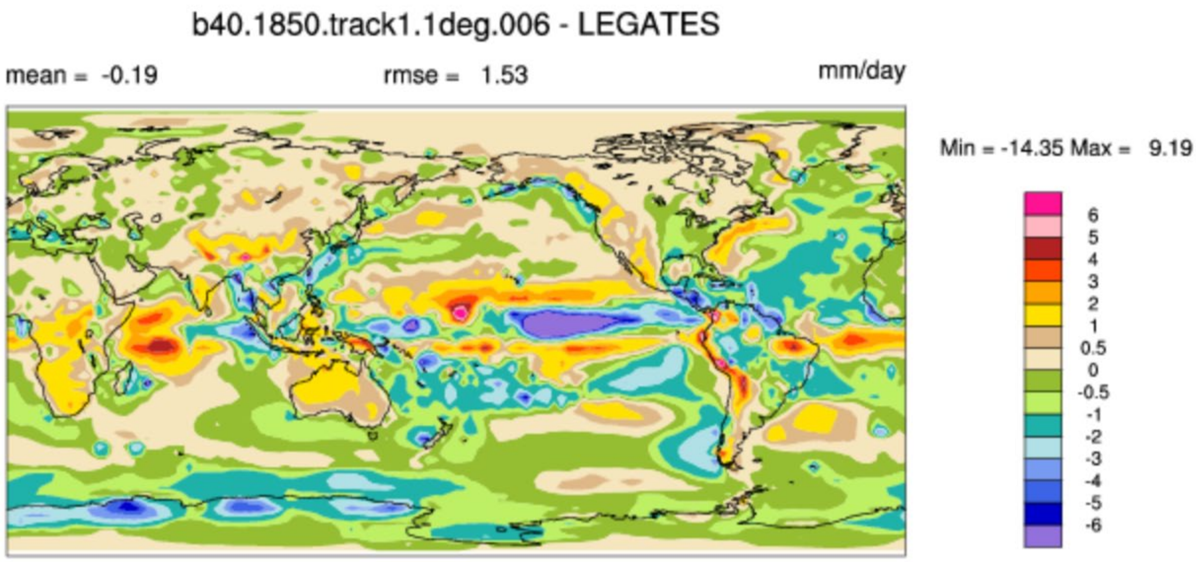


Figure R1_1: Precipitation bias in CESM1 for the modern climate. Sourced from the diagnostics of CESM1 (sourced from https://www.cesm.ucar.edu/experiments/cesm1.0/diagnostics/b40.1850.track1.1deg.006/atm_863-892-obs/ , accessed 26-04-2022).

*Reviewer #1: I think you may also want to discuss in greater details the large-scale outputs of PMIP4 outputs to highlight such point: those recent results indicate extreme variability in LGM rainfall model output scattering in the ISEA region (Kageyama et al., 2021, their figure 6 is particularly instructive).*

Reply: We're well aware of these quite mixed results for the LGM. The large disagreement even on the sign of precipitation changes between models is rather worrisome although there is a somewhat better agreement that at least parts of the ISEA region were definitely drier in most simulations. Highlighting the spatial complexity and disagreement between models for the glacial period in this region is a good suggestion, and we would like to add this in the introduction in line 70, where we already discuss some diverging results in model-model and model-proxies. The high-resolution simulation figures in our study clearly show large spatial changes on short distances between very wet and very dry region – which could explain the large disagreement in the PMIP4 results shown by Kageyama et al. 2021.

*Reviewer #1: I also think that some important data available should be cited and discussed as follows:*
*- On ENSO, you cite Clement, Koutavas and Sadekov to suggest that ENSO was enhanced during the LGM, without discussing the contrasting results of Leduc et al., 2009, Ford et al., 2015 and Liu et al., 2017 that point to a reduced ENSO variability during the LGM. Those results that demonstrate that the LGM ENSO state is still debated should be explicitly discussed along with the Clement, Koutavas and Sadekov articles.*

Reply: We agree that this section needs to be revised and will rewrite it to better reflect the current state of the debate.

*Reviewer #1: -I wonder why you choose to not discuss leaf wax isotopes (the Niedermeier and Konecky papers), other speleothem records (Griffiths), etc. without attempting to highlight them in Figure 2, as you've done for the tree other speleothem records. Also, the original paper evidencing a dry and open vegetation during the LGM at the Konecky site is Russel et al., 2014, and should also be cited, as dD interpretation is puzzling at this site as evidenced in Konecky.*

Reply: Niedermeier et al's paper on leaf wax isotopes are discussed in line 197 in the manuscript, stating that they report large variability, but no clear trends since the LGM to the present, which they interpret to be caused by local effects. Since the nearby speleothem $d^{18}O$ record (Wurtzel et al., 2018) show a much more consistent trend to other maritime continent records and large-scale convection, we put more emphasis on Wurtzel's results. The Konecky et al. paper is included in the overview figure, but not further discussed in the text. Commenting or re-interpreting (and therefore showing) all these proxy records individually with respect to potential seasonality effects would be rather tricky, as these are also influenced by 'source' effects and 'amount' effects, and distract from our main message. Since the three speleothem records in ISEA that we show in Fig. 1 give a relatively good overview of similarities (and differences such as from southward shifts of ITCZ during the NH cold events) on a south-north transect across the equator, we think that more records are not generally useful to be added to this figure.

The Griffiths et al., (2009) speleothem d[18]O record is also included in Ayliffe's (2013) composite which we refer to, but we agree that it is probably appropriate to also refer to Griffiths' original publication. Other records that we have left out (such as Partin et al., 2015 Palawan speleothem) only covers a shorter time period and does not allow comparison between 12ka BP and the near-present.

We will add the Russel et al., 2014 reference for the Tuwoti vegetation findings.

*Reviewer #1: -I also think you should better discuss what has happened at 12 ka in more details in the data as long as you opt for focusing on that time period. The results obtained in the simulation does not always fit to the data you highlight in Figure 2 for that time period, and there is a kind of overshoot in precip seasonality after 12 ka, when then YD resumed. I understand you can't describe everything in its full complexity but your statements for the 12 ka in particular are not always met in reconstructions, and a lot happens before and after 12 ka in both data and climate simulation, as shown in Figure 2.*

Reply: Since the focus of our manuscript is the difference between YD and the modern climate, we have decided to mainly discuss the large-scale differences between those periods, and not the (also interesting) details of climate responses to northern hemispheric short term forcings of the YD event (such as 12k vs 13k). We will expand on this in the revised version (see below).

We already explain some of the differences between the records for YD (ITCZ shift south, causing wetter Java signal and drier conditions to the north). The overshoot in climate seasonality after YD according to TRACE is primarily related to an increase in summer precipitation when the early Holocene warming commenced, and winters remained cold and dry. The seasonal distribution of precipitation clearly follows the precessional cycle where the summer-winter difference peaked at ~11 ka BP, with the lowest winter insolation on the northern hemisphere. We propose to add this reasoning to the manuscript. The seasonality is then rapidly diminished to near modern values upon the opening of the ITF at 6.2 ka BP in the model, and as discussed in the manuscript, this reduction in seasonality would have been more gradual during at least the Early Holocene as the ITF opened in response to sea level rise.

Since we will add a comparison with a 13k simulation in response to reviewer 2's suggestion, we also agree that further discussion about the changes in climate before and after the YD is appropriate to add, in light of those results.

*Reviewer #1: Some other minor details:*
*-Fig 5b & d, I don't really see stronger vs weaker trade winds in the lower branch of the Hadley cell in panels a and c, but rather an eastward displacement of the convection site above the western Pacific at 12 ka WRT nowadays*

Reply: We agree that shifted convection centres is a main feature of the altered atmospheric circulation, and we write this in multiple places in the manuscript. We also agree that the weakening of trade winds is not easily seen from the wind vectors shown in figure 5a and c. However, in the text, on line 374 we state that the Nino3.4 zonal winds are 12% weaker. We also discuss zonal wind changes and in fig S6c where we show the zonal wind speed change between 12k and PI which are clearly weakened over the Pacific. We also refer to the zonal winds weakening on line 338, where we discuss figure 5.

To remedy that this may not be entirely clear to the reader, we propose to add the following sentence to the Figure 5 figure text: "For details on the weakening and/or reversal of the zonal winds, see figure S6." Fig. 5 is intended as a somewhat simplified sketch focusing on shifts in convection cells and adding horizontal wind anomalies would make the figure less readable.

*Reviewer #1: -Still on Figure 5, the situation shown in panel c looks quite like the LGM situation as seen in Holstein et al., 2018 (their figure S6), it is interesting to note that the LGM and YD have the same effect on the convective cell displacement during those two contrasting yet cold time periods*

We agree it is interesting that the LGM simulation using MRI-CGCM3 and our 12 ka BP CESM1 simulation show very similar effects on the Walker Circulation, despite different boundary conditions. We ascribe the reversed land-ocean circulation mainly to cooling of South East Asia during lower sea level and cold NH winters due to insolation and ice sheets. Since the sea was even lower and the NH was very cold at the LGM, it is consistent with our findings that the LGM circulation may have been similar in some respects, despite that orbital forcing was very different. However, according to the TRACE results we present in figure 2, the LGM cooling of the Maritime Continent did not result in a more seasonal climate. We would like to add a brief discussion of this in the discussion section of our manuscript.

*Reviewer #1: -You state in the discussion and the conclusion that the deglacial interval « resembles to ENSO », but you show only variability and seasonal features. Is it possible to differentiate variability in rainfall that occurred at interannual timescale? The article by Liu et al., 2014, may help.*

Reply: Since our CESM simulations were run with a prescribed ocean state from TRACE, i.e., doesn't change during each snapshot simulation, the simulated variability in precipitation or ENSO in response to SSTs will be absent compared to a fully coupled simulation, and not meaningful to discuss in the case of our simulations. We realize that the wording is unclear where we write about resemblance to "El-Niño like" states. We propose to correct this by being more specific in stating that the results resemble El-Niño like mean states "in terms of sea surface temperature and sea level pressure patterns, and atmospheric circulation", and to be clearer that our results are concerning the seasonal cycle and mean state, not the short term (~2-7 year or similar) ENSO variability, throughout the manuscript.


*References:*

*Ford, H. L., Ravelo, A. C., & Polissar, P. J. (2015). Reduced El Niño–Southern oscillation during the last glacial maximum. Science, 347(6219), 255-258.*

*Hollstein, M., Mohtadi, M., Rosenthal, Y., Prange, M., Oppo, D. W., Méndez, G. M., ... & Hebbeln, D. (2018). Variations in Western Pacific Warm Pool surface and thermocline conditions over the past 110,000 years: Forcing mechanisms and implications for the glacial Walker circulation. Quaternary Science Reviews, 201, 429-445.*

*Kageyama, M., Harrison, S. P., Kapsch, M. L., Lofverstrom, M., Lora, J. M., Mikolajewicz, U., ... & Zhu, J. (2021). The PMIP4 Last Glacial Maximum experiments: preliminary results and comparison with the PMIP3 simulations. Climate of the Past, 17(3), 1065-1089.*

*Leduc, G., Vidal, L., Cartapanis, O., & Bard, E. (2009). Modes of eastern equatorial Pacific thermocline variability: Implications for ENSO dynamics over the last glacial period. Paleoceanography, 24(3).*

*Liu, Z., Lu, Z., Wen, X., Otto-Bliesner, B. L., Timmermann, A., & Cobb, K. M. (2014). Evolution and forcing mechanisms of El Niño over the past 21,000 years. Nature, 515(7528), 550-553.*

*Russell, J. M., Vogel, H., Konecky, B. L., Bijaksana, S., Huang, Y., Melles, M., ... & King, J. W. (2014). Glacial forcing of central Indonesian hydroclimate since 60,000 y BP. Proceedings of the National Academy of Sciences, 111(14), 5100-5105.*

*Zhu, J., Liu, Z., Brady, E., Otto Bliesner, B., Zhang, J., Noone, D., ... & Tabor, C. (2017). Reduced ENSO variability at the LGM revealed by an isotope enabled Earth system model. Geophysical Research Letters, 44(13), 6984-6992.*


Reply: We will add these references suggested above by reviewer #1 to the manuscript, as indicated in the replies above.