We thank reviewer 2 for the very useful comments and suggestions. As addressed further below, a key point raised by the reviewer is the need to compare our results presented in the manuscript with an additional simulation to isolate the effect of different forcings. We agree and believe that the addition is useful for the manuscript, and we outline a potential revision below, in which we add additional model results from the 13k simulation data, as well as another sensitivity experiment of 13k with an ocean state from 12k, i.e., increased freshwater forcing. This sensitivity experiment allows us to isolate the effect of AMOC-only changes vs. orbital forcing changes of 13k vs. 12k. We address each comment below, and have included the reviewer's comment in *grey italics* to aid the readability of this document.

Reviewer #2: Overall, I like the concept of the manuscript: describing seasonal changes in western tropical precipitation during the Younger Dryas climate event. However, I cannot accept the manuscript in its current form, and I suggest major revisions to the manuscript. The primary cause for the revision is to clearly separate which forcings are responsible for differences between the Younger Dryas state and Pre-Industrial (PI).

Reviewer #2: In the intro (paragraph on lines 49-55): the jury is still out as to how ENSO changed during the glacial times, including during the deglaciation. This paragraph and the paper should depict the current state of ENSO research. Reviewer #1 provides many important references.

Reply: As we responded to reviewer 1, we agree that this section needs to be revised, and will rewrite it to better reflect the current state of the debate.

Reviewer #2: The major issue: by comparing 12 ka to PI, the authors are conflating several different forcings that could drive the precipitation changes, such that it is not clear if the conclusions hold as presented. The summary of the difference in forcings between 12 ka (Younger Dryas) and 0 ka are:
Precession of the equinoxes.
Sea Level changes and the presence of large ice sheets on land
Freshwater forcing reducing AMOC

To separate the forcings, and hence attribute what is causing the differences between the paleo and PI runs, the authors should compare different time slices, in addition to the 12 ka and 0 ka.
*Usually the early Holocene (~10 ka) or the mid-Holocene (~6 ka) compared to 0 ka is used to identify changes forced by precession of the equinoxes, like in the PMIP protocol. This is *especially important when considering changes in the seasonal cycle, as precession has little effect on mean annual changes in insolation, but instead causes very large impacts on the seasonal distribution of insolation (e.g. Huybers, Science, 2006 or Huybers, QSR, 2007). The insolation changes in turn lead to changes in precipitation. Therefore, the authors need to make major changes to the manuscript, as the seasonal cycle is a major point of focus for this manuscript.*

Comparing the time interval just before the Younger Dryas, i.e. the Bølling–Allerød interstadial (BA) at 13-14 ka, with 0 ka will address both orbital changes and sea level/ice sheets. By then using the 10 ka run, which addresses precession only, and the ~13 ka run, one can isolate the climate effects due to sea level and ice sheets.

Comparing the run during the BA with the YD will isolate the climate effects due to AMOC changes.

I am not suggesting to do any additional runs, such as 10 ka. Rather, present what runs have been completed, and present the output accordingly. Schenk, et al., Nature Comm., 2018, ran the BA interval. So that should be readily available to the authors. The 0, 12, and 13 ka runs should be used to better describe which forcings are driving the seasonal changes in precipitation of the western tropical Pacific.

Reply: The main focus of this manuscript was to explore the differences in climate between the Holocene and deglacial periods, in context of absence of peat formation and more open vegetation during the glacial. As such, it is outside the scope of this paper to fully isolate which changes in forcing are responsible for the large scale deglacial-Holocene transition. With that said, we agree with the reviewer that the comparison with 13 ka BP simulation would be useful as an addition to the analyses already presented, even though we will not be able to fully isolate all forcings and changes in boundary conditions. We thus propose to add a section to the manuscript with the comparison suggested by the reviewer for 13k vs. 12k, with the figures and main points as discussed below.

Additionally, we have access to an additional simulation experiment with 13 ka BP climate state, but with the cold ocean state from 12 ka BP (which we refer to as 13kYD). This allows to isolate the changes in climate between 13k and 12k that came only from ocean SST's (i.e., AMOC/meltwater) changes, and what changes arose from other forcing (orbital, GHG, sea level). The comparisons we propose in this new section are thus:

1) 12k and 13k versus PI: large scale changes deglacial to modern forced by GHGs, orbital, ice sheets and sea level. Essentially here we compare deglacial to modern conditions, as already done in the manuscript but with one more simulation.

2) 12k vs 13k: full changes between YD and Allerød
3) 13k vs 13kYD: isolates AMOC change
4) 12k vs 13kYD: isolates the residual of changes arising from non-AMOC related forcing, i.e. mainly orbital + nonlinearities

Results 13k simulations:

1)

The new comparison shows that the main differences between 12k-PI are the same as 13k-PI (fig RC2_1 a-d), with drastically reduced ISEA precipitation and shift of convection centers during both periods. That the difference between 12k and 13k is relatively small compared to PI suggests that our original approach is valid for our goal to compare late glacial and PI; it does not matter much which specific time period during the late glacial is used if the focus is on a comparison with PI. This was also the reason why we did not include 13k in the current manuscript (results for 13k also show a prohibitive climate for peatland formation as for 12k).

2)

As the reviewer suggests, the 12k-13k comparison is informative, and we see changes in Pacific circulation and seasonal cycle that are related both to the changes in AMOC and precession (Fig RC2_1 e and f). The increased freshwater forcing at 12k cools the north Atlantic and to a lesser extent the entire northern hemisphere.

The (annually averaged) precipitation centers are shifted further south caused by the southward shift of ITCZ during NH cooling – which is a well-known phenomenon as discussed in the manuscript on line 357. There are only minor changes over ISEA land areas between the periods. There is apparent drying in the western equatorial Pacific, and wetting in central/east Pacific, suggesting a partly altered Walker Circulation. In terms of seasonality of this simulated annual change in precipitation between 12k and 13k, there is little effect over ISEA, but the substantial drying in the western equatorial Pacific occurs during June-October, while the November-May period mainly shows a southward shifted ITCZ.

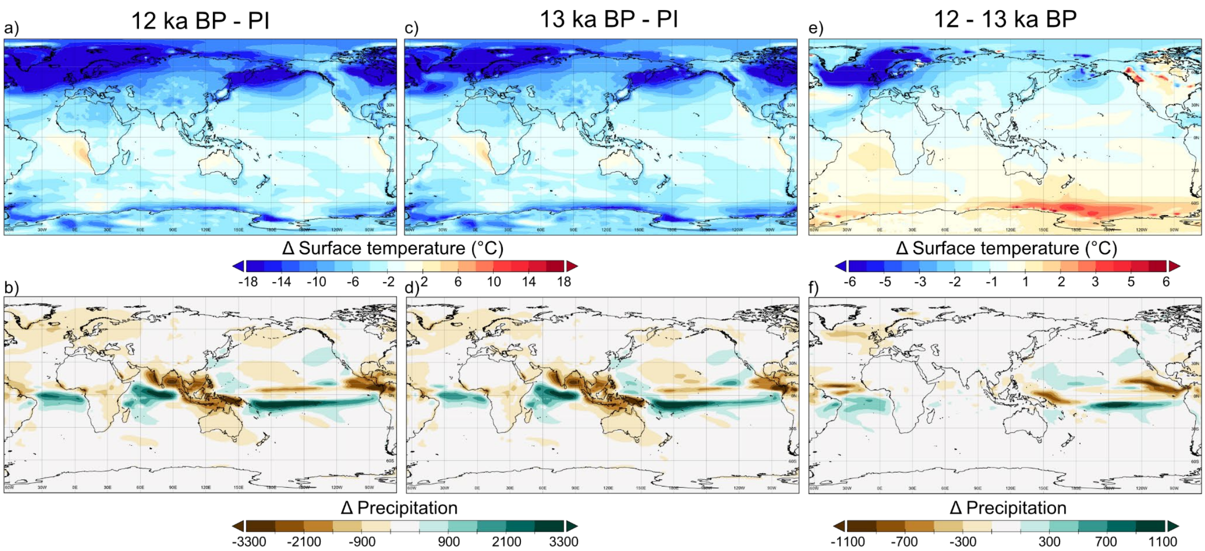

Figure R2_1. The change in TS and P between 12k, 13k and PI. a and b) 12k-PI, c and d) 13k-PI, e and f) 12k-13k.

3)

The differences between 13k and the 13kYD simulations reveal that almost all changes between Allerød and Younger Dryas were caused by AMOC changes, since nearly the whole difference between 13k and 12k (Fig. RC2_2 a and b) is also captured in 13k-13kYD (Fig. RC2_1 c and d). Changes in AMOC (and ensuing cold high latitude northern hemisphere) are thus causing a 1) southward ITCZ shift over the Pacific Ocean and 2) rearrangement of the Walker Circulation with drying in the West Pacific Warm Pool and wetting in the central/east Pacific. This finding supports the conclusions made in the original manuscript, concerning the role of a cold NH and large thermal meridional gradient, but also suggests that short term AMOC changes can have an important effect on Walker Circulation changes.

4) The residual difference between 12k and 13kYD (i.e., same ocean state but slightly different orbital, GHG and ice sheets) show very small differences (Fig. RC2_2 e and f). These differences appear to mainly affect the ISEA climate in winter and spring, following the insolation change at each season. September has slightly increasing insolation from 13k to 12k,

leading to slightly warmer and wetter autumn conditions at 12k, while the opposite is the case for march. Overall, this has a very minor effect on the seasonality of ISEA climate.

Conclusions:
From the results we outlined above we conclude that AMOC is the dominant forcing on short (millennial) time scales, such as between 12 and 13 ka BP. AMOC can thus be an important modulator of both ITCZ and Pacific Walker Circulation. Little change is observed over ISEA and Indian Ocean due to AMOC. On short (millennial) time scales, orbital forcing plays a minor role. On longer timescales between 12 or 13 ka and PI, AMOC changes (as reflected by model freshwater forcing and resulting ocean SSTs) play a minor role. On these longer time scales, the orbital, ice sheet and sea level forcings dominate.

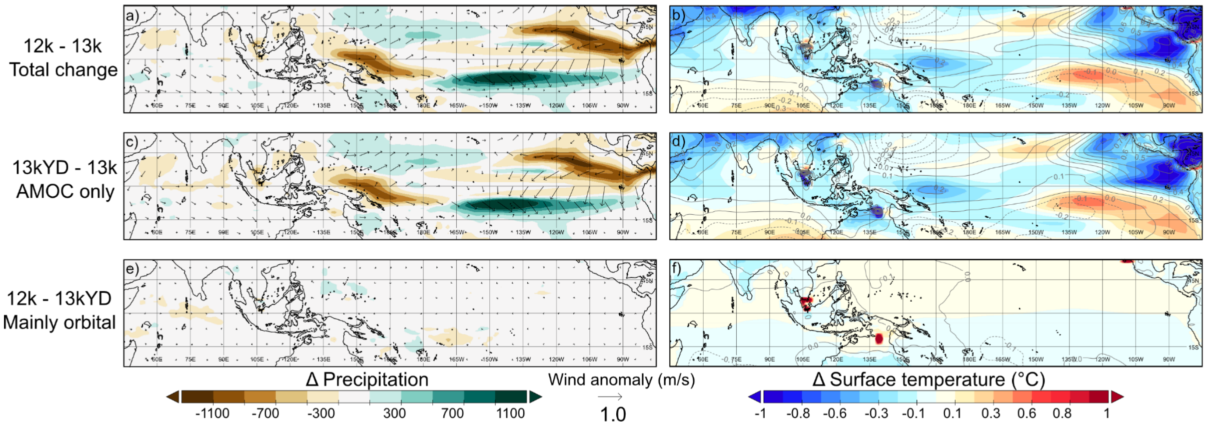

Figure R2_2. Precipitation and surface temperature changes between the 12k and 13k simulations, and the sensitivity experiment. a and b) 12-13 ka BP, representing the total change between the simulations. c and d) 13kYD-13 ka BP, representing AMOC only. And e and f) 12 ka BP-13kYD, showing the residual changes, arising from small changes in GHG, orbital, sea level. Also shown are the anomalies in surface winds as vectors in a, c and e and tropical (±23.5°N) sea level pressure difference as isobars in b, d and f where dotted isobars denote negative values and solid isobars denote positive values. Note that the strong temperature changes in Sunda and Sahul shelves are artefacts arising from comparing land to ocean grid points resulting from the altered sea level between simulations.

Reviewer #2: Plot the coast lines in Figure 3-7 using the boundary conditions in the model, not modern observations of coastlines or bathymetry. This greatly facilitates the interpretation of the precipitation signals. One should look at where the model has land – not the actual planet.

Reply: The model land-sea distribution for 12ka BP in the model was shown in figure 7c in the original manuscript, which is pointed out in the methods. We left the model coastlines out of figure 3-6 to improve readability, and not make the figures excessively busy, in particular for figures with several panels combined. The bathymetric coastlines that we currently use in the figures also reflect the model coastline relatively well. We thus suggest adding the coastlines to figure 3 only, which both allows for better readability while still clearly showing the coastlines for both PI and YD to the reader. However, it is relatively easy to plot the model coastlines in all figures if the editor or reviewer disagree with this suggestion.

Reviewer #2: If you want to use the term "El Niño like" to describe the mean state, one should include changes in the thermocline and sea surface salinity (Di Nezio, Paleo., 2011). Sea surface temperatures alone are not sufficient to describe the mean state when discussing changes in ENSO. Furthermore, what is the ENSO signal in the runs? 150 years is not adequate enough to converge on the median of the ENSO response (Lawman, et al., Science Adv., 2022), but it would be nice to see how the Niño 3.4 SSTA at 12 and 13 ka responds in CESM1, as it has a realistic ENSO.

Reply: Since this simulation was done with a prescribed ocean state from TRACE, i.e., is fixed for the duration of the snapshot simulations, the short term (2-7 year or similar) ENSO variability in not meaningful to discuss in this paper. We therefore focus on changes in the mean state and seasonal cycle - and use the term "El Niño like" to describe the patterns in SST, sea level pressure, winds and large-scale atmospheric circulation. We agree however that this should be clarified in the manuscript text better, and will be more specific in what we mean when we write "El Niño-like". We acknowledge that DiNezio's suggestion to use SSS and thermocline would be useful in a fully coupled simulation, but in our case that does not make sense since the ocean does not respond dynamically to changes in the atmosphere.

We thus believe that the term "El Niño-like" in relation to SST patterns, SLP and atmospheric circulation is a valid and useful term to describe the mean state simulated by the model, as long as we specify clearly what we mean. We note that this is an established usage of the term, and seen in other recent papers (such as Brown et al., 2020, and references therein).

Brown et al., 2020 reference: Climate of the Past, 16, 1777–1805, https://doi.org/10.5194/cp-16-1777-2020

Reviewer #2: Please update the Borneo record to use the recently published version (Buckingham, et al., GRL, 2022). It has a clear YD signal. I realize that this manuscript was submitted before the GRL manuscript was published. This is not to penalize, but rather to update, this manuscript. Also, the interpretations in that manuscript may help strengthen this manuscript.

Reply: We thank the reviewer for this suggestion that we indeed had not published at time of submission. We will update the manuscript accordingly.