Reviewer comments in Red and response to reviewer in black

We thank the reviewer for the detailed review and will address the reviewer's comments as listed below and in the revision.

• I would like to see more information on the spin-up and equilibration of the simulations, as this can be of key importance to interpret the results.

In the revision, we have added the time series of temperature and compute temperature drift for the spin-up to discuss the adjustment of the simulations to the boundary conditions.

• Generally, the manuscript needs a thorough check for readability and language; many sentences are rather tedious to understand and contain (minor) inconsistencies or errors.

We have revised the manuscript to make it more readable and consistent.

See above

• What about seasonal responses? e.g. are changes in precipitation linked to monsoons/storm tracks/ITCZ shifts?

The focus of this paper is on the deep-sea circulation and the forcing boundary conditions creating these changes. Thus, addressing seasonality is beyond the scope of this paper and including it in the paper revision would make this paper less focused.

• Most of the figures shown are rather generic, much of the patterns and responses shown are hardly discussed while many of the processes mentioned are not supported by the fields considered (e.g. circulation, radiation, ice, clouds).

We have improved the explanation of figures in the revised manuscript .

• In much of the results section, the design of the figures does not really support the overall structure, e.g. panels a-c of figures 3/4 are discussed in sections 3.2/3.3, while panel d is discussed in 3.1.

In the revision, we addressed this concern and improved the analysis to be more consistent to the order of the figures.

• I am missing a clear role of radiative feedbacks related to clouds in much of section 3, while this is mentioned up front as a key contribution.

In the revision we have de-emphasized the cloud feedback as part of this study and refer to the already published literature.

• In the results section, I am missing an assessment of circulation changes, which are at times mentioned but not shown or referred to.

We have added an assessment of the circulation changes in response to changes in the atmospheric CO_2 radiative forcing in the revision of this manuscript.

edit: I see near surface winds in figure 9, but there is little reference to this figure and the figure needs some improvement for readability.

We have made the figure more readable by replacing curly vectors with straight vectors and removed precipitation fields since those are already shown in Figure 3. In addition, we have added P-E and wind for all scenarios and discussed this figure in more detail.

• A much more careful consideration of the 12xCO2 (and 6xCO2?) simulation is in order, particularly considering the consequences for ECS and overturning. Despite 2000 years of simulation, the final state may still be highly dependent on the initial conditions implying very warm low-latitude intermediate waters.

By adding a new figure that includes the time series of surface and deep-water pot. temperature we have addressed the adjustment of temperature in the ocean and a discussion of the causes of the drift. In the discussion, we added a statement that the state of ocean circulation at 2000 years depends on the initial conditions that were selected to be the same for all Eocene scenarios in this study.

• The conclusion/discussion section is very limited, with the majority focussing on DWF and ocean ventilation and thus not representing the main findings well. Although this is in the main title, I found limited information in the results that support the suggested collapse of the deep-sea circulation.

We have revised the discussion/conclusion sections accordingly to address the reviewer's concerns.

Specific comments:

• The abstract needs some improvement, as it is rather tedious to read and several comparison statements lack a clear reference.

We have revised the abstract to make it more readable and consistent to the main body of the paper.

• L15: what does PAL stand for?

Corrected to pre-industrial atmospheric CO_2 levels.

I see this is clarified on L72, but I would argue the term 'levels' is a bit ambiguous? This may suggest you are increasing non-CO2 components as well. If this is following general syntax used in other work, ignore this comment.

Clarified, see above.

• L16: 'equatorial warming' is a bit ill-defined here. In addition, it is unclear to interpret the 36.9C value without a reference from proxies or e.g. 1x/3xCO2.

We changed the text accordingly from "equatorial warming" to "tropical warming between 15°S and 15°N".

• L62: Is this adjustment shown somewhere? Even after 2000 years, equilibration may be highly dependent on e.g. initial conditions.

See above, time series have been included in the revision.

• L85: The initial temperature profile leads to some questions;

o If this is used for all experiments, they likely have very different levels of, and potential biases from equilibration, is this accounted for?

We agree with the reviewer that the adjustment to equilibrium depends on the scenario, so we will only discuss the final drift for the different experiments. Limited computations resources as allocated to the project allowed us only to integrate for 2,000 years which is quite typical for CESM paleo studies.

o The linear temperature profile with depth should lead to some very warm intermediate levels, which are physically unrealistic and may lead to large temperature biases lasting many 1000s years in stable regions.

e.g. temperatures at 1000-2000m depth at low latitude are generally 10-15C in similar 3/4xCO_2 Eocene simulations, while they would be ~32-36C initially.

I would not expect such a large deviation to dissipate in these simulations.

o Is there a reason to have a discontinuity at z=5000m and scale with 6000m instead?

We corrected the formula for the initial condition of the potential temperature

"The model has been initialized from a depth(z) –dependent temperature profile (with a vertically declining temperature with T=25°C (5000-z)/5000 for temperature $z \leq 5000$ m and 15 °C for $z > 5000$

Thus, surface temperature-at surface is 25°C and in the abyss 15°C and vertical sections of T do indicate a warm pool at intermediate depth. By adding TS profiles in the revision readers of the manuscript will better understand water mass distributions.

• L89: In contrast to the very warm initialisation, there is a positive net TOA radiative balance (I assume it is defined positive downward?). While still quite large, I would expect negative values considering the initialisation. Some further analysis of the time evolution of e.g. depth-dependent temperature and radiative balance would be

For the revision, we have added time series of temperature at the surface and at 4000 m and discuss the adjustment for the different climate simulations.

• L103: Why would this effect be uniquely important to West-Antarctica, rather than all high-latitude regions?

We have changed the sentence accordingly, The higher-than-pre-industrial surface air temperature in the 1xCO2 Paleocene Baseline experiments leads to a reduced snow height and snow-free areas in polar regions which in turn reduce the reflectivity and amplify the warming, particularly over ice sheet-free West Antarctica.

• L109: This is confusing; global precipitation is linked to polar temperatures rather than warmer SSTs overall? It is also not straightforward to me how land albedo is linked directly to precipitation.

We have deleted "absence of ice sheets, and changes in the land surface albedo"

• L113: Where do we see precipitation differences over land relative to the PI scenario? The argumentation is a bit weak here; why would a strengthened ITCZ have a different effect over land versus ocean?

In this paragraph we have changed the text accordingly by focusing on global pattern and not on difference land vs ocean:

"The precipitation increases in the 1xCO2 experiment relative to the preindustrial conditions is linked to heavy rainfall over the tropics, due to the intensification of moisture transport along the lower branch of the Hadley Cell into the intertropical convergence zone."

• L120: I am missing an explanation on the drying patterns close to the equator over much of the ocean. These do not align with the downward branches of the Hadley cells, so are seemingly related with a migration and/or contraction of the ITCZ which further intensifies at higher CO2? (edit: I see this is the topic of sections 3.2/3.3, but still miss a clear explanation there).

We have clarified the text accordingly by explaining the downward pattern close the ITCZ: "Lower SST at the equator, particularly in the Pacific Ocean are linked due to Ekman-induced upwelling contribute to band of low precipitation."

• L129: This is mostly a repetition of an earlier paragraph. Also, 200Sv is mentioned here versus >18^0 above?

We have deleted the sentence accordingly to avoid repetition.

• L145: I do not see SST in Table 1? Is the value the same by coincidence, or a typo?
SST values have been included in Table 1 in the revised version of the paper,

• L149: Is the ice albedo feedback shown somewhere? As sea ice is said to only be present in winter, this would unlikely have a major radiative effect.

We have de-emphasized the ice albedo effect in the revision and mentioned that this will be only a seasonal climate feedback.

• L168: I assume this is about the global average?

Yes, we have clarified the sentence accordingly.

• L171: The decline in latent heat flux is contradictory to what was said earlier (and further down; L180) when considering rainfall, if this is land-only versus global some clarification is needed. More generally, a discussion on the radiative balance would be helpful (which I believe is shown in the general DeepMIP paper including these simulations?). What about the potential role of albedo and clouds? Are the land temperature changes highly seasonally dependant?

We have revised this sentence and included a more detailed analysis.

• L172: This is again land only?

Yes. We have reorganized sentences to discuss global averages first and then changes over land.

• L173: It is a bit counterintuitive in this section to generally treat the 3xCO2 case as a reference, while this is not shown as such in the figures. While it does make sense following up on the previous section, this is not consistent with the figures and makes the interpretation more difficult. For example: further strengthening of equatorial rains is seen for 6x/12x CO2, consistent with the 3xCO2 anomaly pattern, but there is a clear double ITCZ at 1xCO2. It is therefore difficult to assess whether the former show only a further strengthening of the anomaly pattern, or the background as well.

We have discussed the double ITCZ in the revision and increase in precipitation linked to an increase in convective precipitation with a rise in CO_2 radiative forcing.

• L176: Where/how can I see the enhanced moisture transport mentioned here?

We have improved this sentence by including the assumption that under steady state conditions, <TR> = <E>- <P> where TR denotes the horizontal moisture transport of water vapor.

• L179: An increased downward branch of the Hadley cell is mentioned (but not shown?) explaining drier conditions in the subtropics. Although reasonable, I think the assessment of precipitation patterns is too limited here;

We have revised the sentence accordingly with a more detailed assessment and literature references.

o Overall tropical rains are increasing towards higher T/CO2, but the rain bands are contracting towards the equator.

More intense equatorial rainbands are linked to an increase in convective precipitation (see above)

o Tropical rainbands cover a significantly wider latitude range in the PB versus PI, but contract again towards higher CO2. Can you explain this?

See above – This appears to be linked to an increase in convective precipitation towards higher CO_2 radiative forcing.

o While evaporation is higher due to lower saturation in a warmer atmosphere, precipitation does not decrease in the subtropics for higher CO2.

We have improved this paragraph in the revision, discussing changes in global patterns followed by regional changes.

o Is there a significant role of monsoonal rains in understanding changes in

precipitation?

Monsoonal precipitation affects seasonal rainfall patterns that can also affect annual patterns. However, this paper focuses on mean climate and changes in the ocean circulation and not seasonal variations.

However,

o Extratropical rains increase significantly and expand polewards for higher

CO_2, while the meridional temperature gradients reduce. What does this

mean for midlatitude storm tracks?

The atmospheric resolution in CESM1.2 is finite volume ~1.9°x2.5° resolution which very likely underrepresents the strength of the storm tracks. A higher resolution simulation of ~0.25° would be more adequate to resolve storm tracks, and we refer to Shields et al. (2021, Palaeogeography, Palaeoclimatology, Paleoecology). We have included this reference in the revision.

• L181: I assume we are looking at figure 5 here? In that case, jumping from zonal wind stress on the ocean to trade winds is a bit steep. In its current form, the wind stresses are also quite tough to assess from the figure.

We have cited Figure 5 in L181 and increased the number of labels for zonal wind stress.

• L182: As a reader, it is tough to assess the role of the mentioned albed-related feedbacks as they are not shown or referred to.

We have clarified the assessment of feedbacks in the revised version.

• L183: Looking at Figure 8, Ekman upwelling seems to be displaced as much as being reduced? This is, however, hard to see clearly from the figure.

We have improved Figure 8 by smoothing vertical velocity and using non-linear contour labels and revised the text of L183 to better represent changes shown in Figure 8.

• L187: conclusions are made here that deserve more careful consideration; without a thorough assessment of deep ocean equilibration it is near impossible to have a clear conclusion on the overturning circulation state.

This has been addressed by including a temperature time series for the surface and 4000m (see above)

Looking at the age tracers in

figure 13 (which are very relevant, but not mentioned up to this point?), my suspicion

is that much of the deep ocean is completely stagnant and therefore dominated by

the initial conditions.

Although the CESM1.2 may have some "memory" of initial conditions the new time series reveal that the various scenarios adjusted to the forcing boundary condition. While the drift appears to be low for scenarios 3x-12x CO_2 the drift for the 1xCO2 is highest due to adjustment to boundary conditions that predict a much cooler climate than given initial conditions. The revised manuscript has discussed this issue in more detail.

As the latter are very warm, this run likely has a TOA imbalance

which appears acceptable, but may not at all mean that the ocean is adjusted to the

applied forcing. Despite relatively weak and shallow, the northern overturning cell at

12xCO2 still indicates sinking mostly at 40-60N and down to 1-1.5km. I do not see

how this represents a subtropical haline mode?

In the revision, we have de-emphasized deepwater formation in the subtropics but highlighted a deepening of the mixed layer-depth and an increase in subtropical density by increase in evaporation from the surface for the PETM 6x and 12x CO_2 scenarios.

• L191: Again, I miss a clear assessment of the actual ice-related feedbacks here. I

assume the 3xCO2 case only has ice in the wintertime, so any ice albedo-related

feedback should be minimal.

We have clarified the assessment of feedbacks in the revision of the manuscript.