Comments on *Collapse of Deep-Sea Circulation during an Eocene Hyperthermal Hothouse – A DeepMIP Study with CESM1.2*
By *Winguth et al*.

General comments:

- 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.
- Generally, the manuscript needs a thorough check for readability and language; many sentences are rather tedious to understand and contain (minor) inconsistencies or errors.
- What about seasonal responses? e.g. are changes in precipitation linked to monsoons/storm tracks/ITCZ shifts?
- 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).
- 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.
- 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 results section, I am missing an assessment of circulation changes, which are at times mentioned but not shown or referred to.
  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.
- 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.
- 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.

Specific comments:

- The abstract needs some improvement, as it is rather tedious to read and several comparison statements lack a clear reference.
- L15: what does PAL stand for?
  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.
- 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.
- L62: Is this adjustment shown somewhere? Even after 2000 years, equilibration may be highly dependent on e.g. initial conditions.

- L85: The initial temperature profile leads to some questions;
    - If this is used for all experiments, they likely have very different levels of, and potential biases from equilibration, is this accounted for?
    - 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 3/4xCO2 Eocene simulations, while they would be ~32-36C initially. I would not expect such a large deviation to dissipate in these simulations.
    - Is there a reason to have a discontinuity at z=5000m and scale with 6000m instead?
- 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 helpful here.
- L103: Why would this effect be uniquely important to West-Antarctica, rather than all high-latitude regions?
- 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.
- 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?
- 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).
- L129: This is mostly a repetition of an earlier paragraph. Also, 200Sv is mentioned here versus >180 above?
- L145: I do not see SST in Table 1? Is the value the same by coincidence, or a typo?
- 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.
- L168: I assume this is about the global average?
- 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?
- L172: This is again land only?

- 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.
- L176: Where/how can I see the enhanced moisture transport mentioned here?
- 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;
    - Overall tropical rains are increasing towards higher T/CO2, but the rain bands are contracting towards the equator.
    - Tropical rainbands cover a significantly wider latitude range in the PB versus PI, but contract again towards higher CO2. Can you explain this?
    - While evaporation is higher due to lower saturation in a warmer atmosphere, precipitation does not decrease in the subtropics for higher CO2.
    - Is there a significant role of monsoonal rains in understanding changes in precipitation?
    - Extratropical rains increase significantly and expand polewards for higher CO2, while the meridional temperature gradients reduce. What does this mean for midlatitude storm tracks?
- 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.
- 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.
- 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.
- 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. 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. 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?
- 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.

- L195: Is this value adjusted for possible transient forcing? With 2000 years of simulation, this should be feasible (see e.g. Baatsen et al 2020; Figures 2 and 10). The reported value is lower than the one we found for our 38Ma simulations (3.2C) using CAM4, which is expected to be lower compared to CAM5. While the Eocene simulations in Baatsen et al. (2020) were reasonably well equilibrated after ~4000 years (see e.g. Figures 2 and S2), a more significant deviation was found in our shorter (1000-2000year) Pliocene simulations (Baatsen et al 2022), but these values could be easily corrected using the Gregory et al extrapolation method (Figures 2,3 and S2-S4).
- L199: It is unclear to me which inferred value is considered here.
- L205: It is new to me at this point that both the CAM4/CAM5 configuration was used for this study, please clarify! In the previous results, which configuration is considered?
- L220: Although intuitive, a direct comparison between proxy and model temperatures is not very informative (but I still appreciate the scatter plots shown). For example, a simulation overestimating tropical temperatures by 10C and underestimating polar ones by the same amount could still yield a perfect correlation. In addition, the meridional temperature gradient is such a basic pattern that any model should show a reasonable correlation. If anything, consider RMS error values or correlate temperature anomalies relative to PD/PI values.
- L225: What are the actual findings here?
- L236: There is a disclamer here on the scale used for error margins in the model results, but I am missing a clear motivation of the latter. Also, when using simple grid cells (which should be fine), it is good to notice the differences in scales due to the model grid.
- L244: This is confusing: is the model precipitation too low, or too high compared to proxies? Are we not considering CESM1.2 here?
- L245: Can anything be said about precipitation changes between LP and PETM? This is argued to be the main motivation for the different CO2 forcings.
- L250: Although this is a valid point, there is no clear sign (as far as I am aware) that a double ITCZ in the Early Eocene would not have been present.
- L253-267: As this is purely a discussion of prior knowledge, this part should be either in the introduction or discussion? The same holds for L270 onwards, making this mostly a discussion in addition to 2-3 lines presenting results.
- L270: Extreme care is needed here, considering the likely limited adjustment of deep ocean water masses in these simulation.
- L291: I do not fully understand how enhanced WV saturation would limit precipitation
- L293: I am not convinced about this, based on the results shown

Technical remarks:

- L13 't' redundant?
- L15 and following: use {\times} for 3x and similar? Throughout, syntax is not consistent; e.g. lines 175-185 are very inconsistent in usage of spacing.
- L20: mid-latitude regions?
- L60: To my knowledge 'Finite volume' refers to the dynamical core, which is connected to the horizontal grid but does not specifically define it. Is the ocean grid the 'standard' ~382x320 curvilinear bipolar configuration? Does it also incorporate equatorial stretching?
- L84: Is there a specific use to the underlining of phi?
- L85: 6000z/6000: typo?
- L113: 'the precipitation increases … is linked to'
- L129: capitalise Southern Hemisphere?
- L148: 8C-13C is a bit inconsistent with the use of differences before.
- L153: Colombia or Columbia?
- L157: in the in
- L168: respectively at the end of the sentence?
- L169: CO_{2}
- L205: this version of the CESM1.2
- L215: TEX_{86}
- L223: up to and more?
- L228: lt = late?
- L288: I would always make a clear distinction between observations and proxy estimates.

Tables/figures

- Figure 1: surface or near-surface/sm air temperature?
- Figure 2: As the absolute (zonal average) temperatures are already shown in Figure 1, I feel like it would be more informative to show the response per CO2 doubling here, or otherwise between successive simulations?
- Figure 3: I was confused by the use of the same colormap for anomalies and absolute values, assuming panel d was showing the difference relative to PI.
- Figure 4: This could be combined with figure 1?
  Especially for precipitation, consider using cos(lat) on the horizontal axis such that shifts in precipitation are conserving surface area.
- Figure 5: The contour lines are difficult to interpret here.
- Figure 8: Despite one brief mentioning of reduced upwelling, the relevance of this figure is limited to me. In addition, the use of colour map and scale is far from optimal
- Figure 9: While I appreciate the information shown, this figure is very tough to read due to the overlay of contours and quivers. Using the same colourmap for both absolute values and anomalies is again confusing, particularly as the background field is already symmetric about 0. Additionally, I hardly see any anomalies due to the scaling in panels a,b. Clarification of the zonal averages on the side seems to be missing
- Figures 10-12: the formatting of these figures is very different from other figures, please improve for consistency especially in terms of colours, fonts, and markers used.
- Figure 12: Is there a clear motivation for the use of cm/yr rather than mm/yr or mm/day such as in other figures? A simple map showing these proxy sites overlaid with model precipitation would be a useful addition here.
- Figure 13: I cannot find any mentioning of the depth that is considered here. A clear reference to the precise simulation length of the different experiments is also quite relevant to assess this figure.