

In this paper, Zhang et al present results from IPSL-CM5A2 configured for the Eocene, and compare them with results of the modern. The focus is on ocean circulation, including regions of deep water formation, and the partitioning of heat transports into separate terms. Finally, sensitivity results to modified CO₂ are presented. The paper is very well written and clear in general, and in my opinion it is very appropriate for Climate Dynamics, with minor-moderate revisions.

General Comments

Line 100-127. The model description and experimental design needs considerably more detail. Given that Sepulchre et al is just “in prep”, we need many more details of the difference between IPSL-CM5A2 and IPSL-CM5A. For the experimental design, we need to know how soils, vegetation, subgridscale topography, and what frame of reference was used for the Herold reconstruction. The “bar” here is that the simulations should be approximately repeatable. The Lunt et al experimental design paper gives several options for many of the boundary conditions, so we need to know what choices have been made here.

The model-data comparison needs some more work. In particular, the annual mean SSTs should be compared with the data in the main paper, and the seasonal in Supp Info, rather than the other way round. More explanation is needed of what the “uncertainty” in the proxies (Table 2) represents. Also, it is claimed that the model-data agreement is “overall consistent”, but this should be quantified more. For example, what RMS score would be obtained if it was assumed that the Eocene warmed uniformly, or with a $\cos(\text{latitude})$ response. This can give some quantification to the question “how good is good”. Also, the choice of 3x CO₂ is rather arbitrary in the context of model-data comparison, so the GCM warming patterns could be scaled uniformly to best fit the data, and the RMS recalculated.

Specific Comments

Abstract - please mention the results of the CO₂ sensitivity here. Otherwise “different levels of atmospheric CO₂” on line 16 is hard to understand when reading for the first time.

Abstract – please add something about the model-data comparison that you have carried out.

Line 19: Explain where (depth and latitude) that the 40Sv occurs.

Line 32: Check whether 55 Ma is really the time period that DeepMIP focuses on; see e.g. Hollis et al (2019) and Lunt et al (2017).

Line 40: I don’t think it’s correct that CO₂ can’t explain the decreased meridional gradient at the EECO. The high CO₂ can lead to enhanced feedbacks at high latitudes.

Figure SI1: For the timeseries of temperature in the simulations, the evolution seems inconsistent with the statement that the 1.5x run is branched off from the 3x run after 1500 years. Please align the simulations correctly so that the 1.5x run starts after 1500 years.

As well as the timeseries of temperature (Figure SI1), it is important to show the timeseries of some metric of overturning, e.g. maximum overturning, or averaged mixed-layer depth, so we can assess to what extent the ocean circulation is in equilibrium, and the interannual variability of circulation.

Figure 4: maybe use a log scale rather than using different scales for each panel, or use the same scale for each panel.

Line 223: Not clear what “intermittent” means here – it implies temporal variability.

Line 234: I would have expected the vertical gradient in density, rather than surface density, to be the key control.

Line 237: For the salinity and temperature values, please also give the percentage change that this induces in density (e.g. 80% and 20%)

Line 243-254: I am not totally convinced by this mechanistic link to atmospheric circulation. Please either add some more quantitative analysis, or caveat this section with “possible” or “maybe”.

Line 294: This additional simulation should be introduced in the methods section.

Line 335-336: Please reference/add a Figure for “visible on sea surface salinity”

Line 356-366: This section may link more clearly to ocean circulation if you explore the wind stress curl rather than the wind stress.

Line 498-499: This non-linearity in climate sensitivity is interesting and could be explored in a bit more detail. See e.g. discussion of why this may be in Farnsworth et al, in press, GRL.

Line 552: It is not clear what you mean by “robust”.

Line 560-570: Make clearer at the outset of this paragraph that it is a summary of the previous paradigm, not a summary of your results.

Technical Comments

Line 37: “Ma ago” should be “Ma”.

Line 44: “continental configuration” as well as “bathymetry”

Line 68: “latitude” should be “latitudes”

Line 82: remove “briefly”

Line 89: remove “IPSL in the following” and stick with the full name.

Equation 1: need to define E, W, x, z.

Line 172: “clockwise” needs definition of which way we are “facing”.

Line 221: “deep convection” instead of “convection”.

Line 242: net surface freshwater gain.

Line 447: label the two terms in the equation, e.g. with curly brackets, OHTmoc and OHTgyre.

Line 530: HadCM3L.

Line 560: Warmest period in the Cenozoic.

Figure 2: Add labels for different lines.

Figure 3: Add units for isopycnal contours in caption.

Dan Lunt