Summary

This paper presents the contribution to PlioMIP from the CCSM4-Utrecht (CESM1.0.5) model. The broad-scale features of the Pliocene simulation are presented, and in addition there is a model-data comparison, a factorisation analysis of the CO2 versus non-CO2 boundary conditions, and the modes of variability are explored. Overall, I think that this is a nicely written and presented paper, and will likely be of benefit to other group in PlioMIP who will find it useful when interpreting other results from the wider PlioMIP ensemble. However, it is somewhat descriptive, and at times it is a little speculative as to the mechanism involved, but this is the nature of a paper such as this, so I think this is OK.

Main comments

(M1) In the abstract and in Section 3.2, it is proposed that the relative warmth of the Pliocene simulation compared with other PlioMIP models is the initialisation and long spinup. This may be true, but it would be good if this could be verified more robustly, for example by explicitly presenting and comparing the integration lengths and initial conditions of all models in PlioMIP, and/or showing the Utrecht global mean temp after a similar amount of spinup as other models, for a direct comparison.

(M2) In Section 4.6 it would be good to have more of a direct comparison with the results of Oldemann et al (in press), - try to build on their results in this section.

(M3) Similarly in the section on ocean circulation (4.3) I would expect to see here an in-depth comparison with Zhang et al (2021), and here to bring additional insights, and to note how this model fits in with the larger ensemble.

(M4) Line 91-99 – if the vertical diffusivity makes little or no difference to the model results, as is claimed, then why did you modify them in the Pliocene? This needs to be better explained and justified. I would expect to maps of the temperature difference between these two different model versions, at least in Supp info.

(M5) Section 4.5 - Here, I think the paper would benefit from use/discussion of the factorisation framework presented in Lunt et al (2021), for analysing these simulations. For example, the mean of Figure 10 (top left and top right) could be presented.

(M6) Section 4.4 – I would recommend using the McClymont et al SSTs instead of Foley and Dowsett, because McClymont et al have been peer-reviewed.

(M7) Line 263 – 272 – careful here. I am not sure that I agree with this interpretation of the changes in fluxes. If both simulations are in equilibrium, then both simulations will have a net zero energy balance at the surface and TOA. Interpreting a change in shortwave net flux is not necessarily an indicator of changes in feedbacks. A full energy balance analysis (e.g. Heinemann et al, 2009; Hill et al, 2014) or even better, a APRP analysis would be more appropriate here.

(M8) section 4.3.2 - Rather than just presenting SST and surface temperature (which are very similar), why not show the same analysis but for e.g. precipitation, or seaice, which may be more interesting?

Specific Comments

(S1) Figure 1 – for the modern ice sheet, it seems odd to me that there are large parts of Antarctica that are not ice covered (see light blue contour) but are above sea level (see colour scale). I would have expected the whole Antarctic continent to be covered in an ice sheet (which it is, according to figure S1).

(S2) Figure 2 – what happens at ~1000 years? The model appears to be taking in energy before this time, and then releases heat. Any idea why?

(S3) Line 180 – It is not just slow feedbacks that can give a non-linearity, it is simply the intrinsic non-linear nature of all feedbacks, especially clouds; see e.g. Bloch-Johnson et al., (2015) or Knutti et al. (2015).

(S4) Line 229-231 – "The globally averaged sea surface temperature (SST) only increases by 2.1 $_{o}$C per CO_2 doubling, as a result of the inhomogeneous distribution of land/sea surface" – This is perhaps more to do with lack of snow-cover and icesheet (and seaice to a certain extent) feedbacks for the SSTs, and lack of evaporation over land; i.e. it is a result of the well-known land-sea contrast in warming.

(S5) Line 235 – 241 – This section could benefit from some literature around the non-linearity of forcings/feedbacks. Could also give a feedback parameter (units W/m2 K-1)

(S6) I am not sure that the discussion of surface versus deep ocean temperature is robust given the different mixing coefficients in the simulations (see comment M1).

(S7) Line 287 – "This is in agreement with a larger ice volume over parts of East Antarctica" . I am not sure I follow the mechanism here – why is this in agreement?

(S8) Line 305 – there does seem to be a coincidence with maximum warming and mslp/500mbar geopotential height, but the reason for this coupling is not clear- one might expect a longitudinal shift in the temperature response so that it coincided with the anomalous north/south winds, rather than the centre of the geopotential anomaly?

(S9) Section 4.2.3, Figure 6. For the seaice observations, if the model were perfect then which fraction of seaice would lie on the observed contour line? 100%, 0%, or 50%?

Technical Comments

(T1) Figure 4,5 – show absolute of both E280 and Eoi400, and the difference – there is room for 3 plots side-by-side if the full page-width is used.

(T2) Figure 7 – be consistent throughout whether Eoi400 is on the left or right (left here, right in figure 6)

(T3) Line 24 – relatively stable

(T4) Line 29 – foe -> for

(T5) Line 37 – cite Haywood et al (2020) large scale features of PlioMIP2.

(T6) Line 52 – is it really equivalent to the latest version? This implies you are using the latest CMIP6 version, which is not the case I believe.

(T7) Line 65 – "switching to an adjusted Pliocene climatology"

(T8) Line 165 – "Within the PlioMIP2" – database?

(T9) Line 285 – besides *being* warmer

Review by: Dan Lunt

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