Climate of the Past Discussions

Clim. Past Discuss., author comment AC1
https://doi.org/10.5194/cp-2021-140-AC1, 2022

Reply on RC1

Michiel L. J. Baatsen et al.

Author comment on "Warm mid-Pliocene conditions without high climate sensitivity: the CCSM4-Utrecht (CESM 1.0.5) contribution to the PlioMIP2" by Michiel L. J. Baatsen et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-140-AC1, 2022

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.

AC: The authors would like to thank Dr. Dan Lunt for the detailed feedback and comments. We mostly agree with the main remark that some of the analyses may be too qualitative and suggest to make a few improvements there. We also want to point out that the main goal of this manuscript is to look at the results of our model results, albeit within the PlioMIP2 ensemble. We therefore prefer to not present any new analyses of model data beyond our specific set of simulations, but rather refer to other relevant PlioMIP2 studies as much as possible.

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.
AC: The main point made here is that the model was initialised with an average ocean temperature from a PlioMIP1 CCSM4 simulation, which has a very similar model set-up. Our Eoi400 simulation still warms up considerably, indicating the importance of an extended (>1kyr) model spin-up. This is why we also show the complete time series of our model spin-up phase. We do not see much added value comparing the spin-up procedures of all of the PlioMIP simulations within the scope of this manuscript. We will, however, explain the above in the text while referring to the spin-up temperatures already shown. We will also compare our results to those of Chandan et al. (2017), who have a detailed discussion on their model spin-up.

(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.
AC: We will improve the connection to Oldeman et al. (2021) here (Figure 2a: standard deviation, Figure 4b: spectral shift, Figure 5: pattern shift), as they show that our simulation has the largest reduction in ENSO amplitude between E280 and Eoi400 cases within the PlioMIP2 ensemble.

(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.
AC: We can make a more extensive (but mostly) qualitative comparison to the results of Zhang et al (2021) here, as the deepening and/or strengthening of the AMOC in the Pliocene seems to be robust within the PlioMIP ensemble. A more in-depth analysis of the underlying mechanisms and contribution of the AMOC to meridional heat transports will be presented by Weiffenbach et al. (in prep.)

(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.
AC: A direct comparison between a pre-industrial simulation with/without mixing adjustment is made in sup. Figures 4 and 8. The former shows how the vertical distribution of heat in the ocean is altered, but surface temperatures are left mostly unchanged. The latter repeats the pre-industrial to mid-Pliocene comparison of Figure 5, but using the reference with mixing adjustment instead. We will add motivation and reference here to clarify the choices made. The original Eoi400 simulation, which was uploaded to the PlioMIP2 database had the modified vertical mixing parameters. We therefore keep using this simulation as the standard and have added the sensitivity simulations with other vertical mixing configurations to the supplement.

(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.
AC: We agree that there is a missed opportunity to make use of this factorisation framework with the set of simulations that we present here. We will thus replace the current table and discussion of direct fluxes by the results of the suggested analysis using the framework of Lunt et al (2021) and Heinemann et al. (2009). The results of the EBM analysis are shown in Figure C1 (see supplement), which will replace Table S2.

(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.
AC: We will add the McClymont et al. SST dataset in the comparison, which should also make the results more comparable to the other PlioMIP studies.

(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.
AC: As suggested above, we will leave out this analysis and replace it with a full energy balance analysis. We have already found that this leaves the main findings mostly unchanged, but the results are more straightforward to interpret (see Figure C1).

(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?

AC: Our main objective here is to show how and why the mean structure of the AMOC is different in the mid-Pliocene simulations. We therefore make a comparison of the potential density structure of the upper ocean and mixed layer depths. As the boundary conditions are probably a key factor, we also look into the changes of the freshwater budget over the Arctic Ocean. In the discussion, we refer back to the changes in temperature/salinity/sea ice shown earlier.
Remark: if this refers to Figures 10/11 (Section 4.5), the main reason to show both near surface air temperature and SST differences is that the latter are considerably smaller (Figure 11 can be moved to the supplement). The sea ice edge is already shown using contours in Figure 11. We will simplify the colour scales and introduce e.g. precipitation differences into Figure 10, see Figure C2.

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).
AC: This seems to be an error in the way the ice sheet edge is visualised, we will correct this.

(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?
AC: During the first part of the spin-up, there is only a shallow and sluggish AMOC. A much stronger and deeper northern overturning cell only materialises after those first ~1000 years, greatly impacting the heat distribution in the ocean and global heat budget. The evolution can be found in sup. Figure 5, which we will refer to here.

(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).
AC: That is indeed the case, we will adjust this. With the additional analysis of the different components in the radiative balance, a better quantitative indication of the different feedbacks will be provided.

(S4) Line 229-231 – "The globally averaged sea surface temperature (SST) only increases by 2.1 oC per CO2 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.
AC: This is indeed the first-order effect, which we did not state clearly here. The main point was that, on top of this effect, the land-sea distribution further enhances the contrast between globally averaged temperatures over land versus sea. We will clarify this. (e.g. 'In addition to different surface temperature feedbacks over land versus ocean, …').

(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)
AC: we will add references here (e.g. Caballero & Huber 2013, Baatsen et al. 2021, Lunt et al. 2021) and link to the added EBM analysis.

(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).
AC: The effect of the mixing coefficients is taken into account here, looking at the crosses in Figure 3. This actually explains some seemingly inconsistent differences in deep ocean temperature between the pre-industrial and mid-Pliocene cases. We will clarify how this figure is influenced by the mixing parameters and refer to Figure S4.

(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?
AC: 'Volume' is maybe not the correct word; the AIS reconstruction used in the prescribed PlioMIP2 boundary conditions has a higher elevation over parts of East Antarctica and thus explain the lower temperature by a simple lapse rate feedback. We will rephrase, using 'higher ice sheet elevation' instead

(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?
AC: The patterns of Z500/MSLP and temperature/precipitation both show a zonal shift at middle latitudes. We typically see higher precipitation at the western flank of high pressure anomalies, lower at the eastern flank. The temperature anomalies are shifted more towards the centre of pressure anomalies, mostly due to the effects of vertical motion, precipitation and radiative feedbacks (on top of meridional advection). We will add this to the explanation.

(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%?
AC: The late 20th century sea ice edge is indicated by a 15% sea ice concentration, which is indicated in the colour bar. It may be unclear that the colours refer to model data and contours to observations, only, so we will clarify this in the Figure caption.

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.
AC: We suggest to not add more panels to this figure, to make it more readable in the final version. The E280 fields in our view do not add much information here, so we suggest to add a side-by-side comparison of E280/Eoi280 to the supplement.

(T2) Figure 7 – be consistent throughout whether Eoi400 is on the left or right (left here, right in figure 6)
AC: Thanks for pointing this out, as we try to have the Eoi400 on the left side as much as possible we will adjust Figure 6.

(T3) Line 24 – relatively stable
AC: Will be adjusted

(T4) Line 29 – foe -> for
AC: will be corrected

(T5) Line 37 – cite Haywood et al (2020) large scale features of PlioMIP2.
AC: will be referred to here

(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.
AC: 'latest' is probably not the right word here, as we want to point out that the earliest versions of CESM1 are identical (with the settings used here) in terms of model components to the 'last' version of CCSM, i.e. CCSM4 and therefore can be referred to as either CESM1 or CCSM4. We will rephrase this.

(T7) Line 65 – "switching to an adjusted Pliocene climatology"
AC: We can split this part into 2 sentences to improve readability.

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

AC: We will change this to 'PlioMIP2 database, model fields …' and clarify which of the model data is available within the database (i.e. last 100 years of the E280, E560, Eoi280, Eoi400 and Eoi560 cases).

(T9) Line 285 – besides *being* warmer
AC: we will add this

Review by: Dan Lunt

Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2021-140/cp-2021-140-AC1-supplement.pdf