Clim. Past Discuss.,
doi:10.5194/cp-2017-21-RC1, 2017

Climate of the Past
Discussions
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Interactive comment on "Regional and global climate for the mid-Pliocene using CCSM4 and PlioMIP2 boundary conditions" *by* Deepak Chandan and W. Richard Peltier

Anonymous Referee #1

Received and published: 3 April 2017

Mr. Deepak Chandan and Dr. Richard Peltier (DR for short) presented a new set of PlioMIP2 simulations and comparisons with PI simulation in order to understand the large-scale mid-Piacenzian climate features (mPWP). The writing is clear, and the introduction is very well written. However, considering the large differences in the Pliocene temperature responses and the climate sensitivity derived from the results of CCSM4 reported in IPCC AR5, and Rosenbloom et al., 2013, which also uses CCSM4, a number of critical issues need to be addressed before this paper is publishable.

The results presented here exhibit appreciable differences from previously published PI and PlioMIP1 experiments with CCSM4. The claim that this is due to the extended simulation length is not convincing as the surface temperature field appears to be

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in quasi-equilibrium after about 500 years (Fig. 5); extending the simulation beyond this point only adds about 0.5°C to the global annual temperature. The authors are urged to (1) double-check their simulation setup, (2) run a PlioMIP1 simulation using the same setup as in Rosenbloom et al., (2013) to assess differences from previously published experiments with CCSM4 (data can be downloaded from the Earth System Grid: https://www.earthsystemgrid.org/dataset/ucar.cgd.ccsm4.output.html). DR's simulations (if proven valid) may be interesting for the purpose of testing CCSM4 parameterizations, but they may not be appropriate for PlioMIP2. One major goal of PlioMIP2 is to evaluate skills in simulating mPWP climate by state-of-the-art Earth-system models. DR's PI simulation shows much worse model skills (too cold and too much sea ice) compared to the published benchmark CCSM4 PI simulation. It is confusing to use these simulations to serve the intercomparison purpose for CCSM4. Finally, there is a lack of information on the spin-up and diagnostics of DR's mPWP simulations, which hinders comprehensive evaluations of validity of these simulations. I am highlighting major differences from published CCSM PI runs here, using the part of DR simulations that do not include changes to ocean diapycnal diffusivity: 1. The global surface temperatures in DR's PI simulation is between 12.5 to 13 °C (Fig. 5 and Table 2), which is 1°C colder than CCSM4 PI benchmark simulation (http://www.cesm.ucar.edu/experiments/cesm1.0/diagnostics/b40.1850.track1.1deg.006/atm_863-892-obs/); the benchmark simulation shows 13 °C for 2-m air temperature and 14 °C for surface temperature. Northern hemispheric sea ice in DR's PI case covers Labrador Sea and North Pacific at least seasonally (Fig. 15), which is way too extensive compared to the published PI benchmark simulation (Gent et al., 2011). In Gent et al., (2011), the CCSM4 PI benchmark has an AMOC strength > 24 Sv. Shown in Yeager and Danabasoglu (2012) Figure 1b, turning off the overflow parameterization does not change the AMOC strength much, which is measured as the maximum overturning of the North Atlantic. However, in DR's PI simulation, the AMOC strength is only 20 Sv (Table 4.). Finally, the entire ocean in DR's PI simulation is colder than what is reported by Gent et al (2011). 2. The climate sensitivity to a doubling of

CO2 is ∼4°C (based on simulations E400 and E280), while the estimate for CCSM4 is 3.2°C/per doubling of CO2 (Bitz et al., 2011). 3. Comparing the zonal mean temperatures in DR's E400 to E280 case (the distance between red and green lines in Fig. 8), the 120 ppm CO2 increase leads to ∼7°C warming in Arctic region. In IPCC AR5 (Fig 12.11 in Collins et al., 2013), ∼100 ppm CO2 increase between 2081-2100 and 1986-2005 based on RCP2.6 simulations shows <3-4°C Arctic warming.

More information is needed to evaluate DR's mPWP simulations: 1. Details about the pre-existing 3500 year spin up simulation should be presented. This run was used to initialize the E280 and E400 case. For example, what are the salinity trends in major ocean basins and what are the sea ice trends? how were the carbon and nitrogen pools initialized and spun up to equilibrium? Moreover, how does the simulated E400 climate state compare to present-day observations? The surface temperatures presented in Fig. 5 suggest that both E280 and E400 start from a very cold ocean state, which is an odd practice for simulating PI or present-day climate. 2. Why are the Pliocene simulations initialized from Levitus? This shows a very different provenance from the controls. Earth system sensitivity analyses should not be performed when the oceans are started so differently and not brought into any kind of equilibrium (clear ocean temperature trends are shown in Figure 4 for PlioMIP2 cases). Based on Fig. 4, difference in ocean temperature between E400 and Eoi400 could be an artificial result of different initialization. 3. The argument for vegetation mapping is weak (P8, Line 30-34). For example, for Megabiome 1, 80% PFT type 5 and 20% PFT type 7 could lead to different surface albedo from 20% PFT type 5 and 80% PFT type 7. More rigorous mapping method should be derived and applied to the PlioMIP2 simulations. 4. The significant and widespread cooling in DJF over Canada and Eurasia in the Eoi400 simulation as compared to the E400 simulation needs to be explained (Figure 7). The cooling suggests a possible problem with the initialization of the land surface in the Eoi400 simulation. Please show time series of net primary productivity and leaf area index. An example of Rosenbloom et al (2013) can be found here: http://webext.cgd.ucar.edu/B1850/Pliocene/lmwg/ccsm2/b40.plio.FV1.003ext-

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b40.1850.track1.1deg.006/set1/set1.html. 5. Global, annual surface air temperature in the Eoi400 simulation is ∼2C warmer than in the E400 simulation (Figure 5, Table 2). This is puzzling, even more so because ∼1C of this surface temperature difference occurs at tropical latitudes (Figure 8) where the relatively small coverage and changes in vegetation would not be expected to produce that much warming. If this is indeed a result of the changes in the gateways then a figure supporting this contention needs to be included. 6. As noted above, DR's PI simulation shows too much sea ice in the North Atlantic and Labrador Sea compared to the published benchmark CCSM4 PI simulation. As such, it strongly influences their surface air temperature anomalies in the North Atlantic and Labrador Sea, as shown in Figures 6a and 7a, leading to their conclusion that they better match the PRISM3 SST anomalies in these regions (Figure 12) than the simulation of Rosenbloom et al. In fact, their PlioMIP2 simulation, by itself, is colder than Rosenbloom et al., (2013).

Detailed comments:

P 1, Line 9-10: This is a different way in which the CCSM4 has been used with major changes to the ocean vertical mixing physics, so not just boundary conditions are different. P 3, Lines 22-25: Cite Haywood et al (2011) when talking about the designs and aim of PlioMIP1 P 6, lines 10-15: Please discuss the choice of the ocean convective parameterization. Was the KPP scheme used apart from turning off the tidal mixing component? Was tidal mixing also turned off when the Kv was kept constant? Perhaps show your preindustrial POP1 simulation's mixed layer depths. How is the 2-d distribution of mixed layer depth different from the standard CCSM4 preindustrial simulation as described in Danabasoglu et al., J. Climate, 25, 2012? P 7, line 8: The default 1° POP2 bathymetry field (KMT) is not from GTOPO30 as described here. The CCSM4 POP2 KMT field was generated from ETOPO2v2 as described and cited by Danabasoglu et al. 2012. P 8, 18-20: This line, describing how the global mean salinity is adjusted for glacial climates to reflect a drop in sea level, and hence ocean volume, is not needed here as there is no glacial climate simulation. Thus, to make the manuscript more concise and focused, it should be deleted. P9, line 22: By what measure have the global mean SSTs come into equilibrium? Trends are still quite apparent in Fig. 3a. P9, line 24: "...increases the rate of warming for the middle and lower and decreases the rate of warming for the upper ocean." This decrease/increase of trend is not apparent apart from the very beginning of the start of the POP1-Kv runs. The trends at the end of the POP1-Kv runs look very similar to the trends at the end of the POP2-Kv runs. Typically there is a flattening as the ocean model equilibrates, but this is not the case here. P9, line 29: "...given the deep ocean sufficient time to come into equilibrium." Again, Fig 3d shows that the deep ocean is far from equilibrium with major drifts by the end of the simulations. Comparing Figs 3b-d, there does seem to be a transfer of heat from the upper ocean at depths above 550 m which leads to cooling at the beginning of the POP1-Kv runs, while the deep ocean below 1850m starts to warm. There was probably not a net heat loss to the atmosphere at the start through venting the upper ocean, otherwise the global mean temperature (Fig. 4) would show a cooling trend at this time instead of a warming trend. P9, lines 30-32: Trends of global mean temperatures are twice as large O(.07K/century, shown in Fig. 2) as what is shown in Rosenbloom et al. 2013 for the CCSM4 PlioMIP1 simulation, which is around 0.03K/century. P13, lines 12-13: ''...run to near statistical equilibrium..." By what measure are these simulations in a statistical equilibrium? As discussed above, deep ocean temperature trends are large and the global mean ocean temperature at the end of the runs exceeds the CCSM4 PlioMIP1 simulation discussed in Rosenbloom et al. 2013. P13, lines 18-34: Please indicate over what region of the water column and latitude range the maximum Atlantic Meridional Overturning Circulation metric was calculated. One needs to exclude the upper 500m or so of the wind-driven tropical cells. Figure 5 and Table 2 temperature both say Mean Annual Surface Air Temperature, but in comparing the same runs, the values do not agree. Table 2 numbers are higher than shown in Figure 5, suggesting that Table 2 numbers are TS, the Surface Temperature (radiative) which tends to be higher than the TREFHT or 2m air temperature typically used for 'MASAT'. Figure 10: It is difficult to discern the contours in 10b-c. There appears to be possibly a very large cold bias in c and a smaller one in b, based on the minima shown in the color bars for these two panels of anomalies. There appears to be a large black swath in panel c emanating from the eastern boundary of in the N. Pacific, suggesting a very large cold anomaly with respect to the Modern control.

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