Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-57-RC2, 2017

Interactive comment on "Emulation of long-term changes in global climate: Application to the late Pliocene and future" *by* Natalie S. Lord et al.

Anonymous Referee #2

Received and published: 21 June 2017

The authors of this manuscript are investigating long-term past and future climatic changes under the forcing of orbital parameters and prescribed CO2 scenarios. For this purpose they have developed an interesting emulation technique calibrated on GCM experiments. Overall, the manuscript is quite interesting, well written and represents a useful contribution. I believe that after some modifications following my comments below, it would be suitable for publication in Climate of the Past.

1 – My main concern is about the limitations of the emulation strategy. They are not sufficiently stressed in the manuscript. Indeed, the authors have performed a very good job in developing and implementing the emulator technique, and the manuscript explains in details the methodology. To some extent, this is "the best that can be done" based on GCM tools. But, obviously this is also probably not entirely sufficient. . . Over-

all, the fundamental hypothesis is that "climate" responds very smoothly (as explained in the paper) to external forcing. This also makes the even stronger assumption that long-term components of the Earth system, in particular the deep ocean, the carbon cycle and ice-sheets, have no dynamic role. Though this is indeed a fairly usual assumption when studying century-scale changes, this is unlikely to be adequate for 100-kyr to million-year studies. I think the authors should clearly state that their strategy cannot account for : (for instance) deep ocean changes (as experienced during the Quaternary during cold and but also warm periods), CO2 dynamics, ice sheet dynamics. The authors make the hypothesis that it might be suitable for warmer climates (thus the Pliocene and the future) while it is clearly inadequate for the Pleistocene. This might be true, but it is also likely a perspective problem: we know quite well that the Pleistocene climate results from complex interactions between ice-sheets, deep ocean and CO2; with much fewer data, we may (or may not) assume that the Pliocene is simpler. . .

2 – On Pliocene results. In line with the above comment, the hypothesis of rather small ice-sheet changes in the late Pliocene is not very well founded. The authors mention that their chosen time window does not include the M2 glaciation at 3300 kyrBP (line 614). This is not quite correct since they investigate the 3300-2800 kyrBP time window, which starts precisely with the M2 glaciation, as can be clearly seen on the data of Fig.10. The M2 glaciation is estimated to correspond to a sea-level fall between 40 and 65 m (Miller et al. 2012; Dwyer & Chandler, 2009). The following cold events (KM2 at G20) are not so well characterized, but should correspond to roughly half the size of M2 (20 to 40 m of sea level drop). On the other side, the G17, K1 or KM3 time periods experienced significantly reductions in ice volume with sea level rise estimated to be +25$\pm$10 m (Miller et al. 2012). Overall, ice-sheet changes are certainly much larger than assumed in the manuscript, and not bounded by the lowice/modice configurations.

3 – The corresponding calculation of pCO2 (§6.3) probably illustrates the failure of these assumptions. In any case, the four "reconstructions" shown on Fig.12 have little

in common, which certainly deserves some comments. The much higher variability seen in high-latitude data points to "polar" climatic processes not being accounted for by the emulator (like ice-sheets, incorrect sea-ice, ...). Instead of presenting these curves as possible pCO2 reconstructions (something difficult to buy), I would rather use them to discuss the limitation of the overall strategy : if the model were perfect, the four curves should be identical... Most probably, the model-data strategy is furthermore inadequate: For instance, is it reasonable to use annual mean SAT to be compared with alkenone-based SST reconstructions ?

4 – On the future 200 ka results. I also have problems with the rather "conservative" assumption of small ice sheet changes. According to Pollard & DeConto (2016), the disappearance of WAIS (somewhat equivalent to lowice?) correspond to the rather mild RCP4.5 scenario, while an extended RCP8.5 results in more than 20 m of sea level rise for Antarctica alone. These ice-sheet changes might also impact the deep ocean circulation, something difficult to account with the emulator strategy.

5 – Lines 808 + following are discussing the limitations of the overall strategy for the next glacial inception, since there is no ice-sheet model component. I would also add that the carbon cycle is prescribed here, not interactive. In other words, the long-term smooth decrease of CO2 is based on the assumption that nothing unexpected will happen in the Earth carbon cycle, and that the "silicate weathering" mechanism (or hypothesis) is a robust one, something far from being fully understood.

6 – On the experimental design, it could be useful to explain why the ice-sheet size (lowice/modice) has not been included in the emulation procedure.

7 – The simulation of sea ice at high latitudes under high CO2 might be a problem, as explained in the text (lines 575-580). It could be useful to discuss rapidly how HadCM3 compares to other GCMs in terms of sea ice.

8 – Line 871. The comparison of model results with paleodata, or the projection of future impacts, is not so much a question of resolution. 1 - The GCM resolution is often

not sufficient. 2 - Very often, this requires additional modelling (proxy modeling, impact models, ...)

9 – Fig.2: Simulations over 2000 ppm have been discarded (§3.4.1): the corresponding points should either be removed, or should be plotted with a different color. These plots are not "slices" but "projections".

10 – Fig.10: the comparison to data is poor. I believe just computing a correlation coefficient and/or explained variance ratio could be useful. See above comments on discussing the overall limitations.

References:

Miller KG et al. 2012 High tide of the warm Pliocene: implications of global sea level for Antarctic deglaciation. Geology 40, 407–410. (doi:10.1130/G32869.1) Dwyer, G.S., Chandler, M.A., 2009. Mid-Pliocene sea level and continental ice volume based on coupled benthic Mg/Ca palaeotemperatures and oxygen isotopes. Phil. Trans. R. Soc. A 367, 157–168.âĂÍ Pollard et DeConto. Contribution of Antarctica to past and future sea-level rise. Nature (2016) vol. 531 (7596) pp. 591-597

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