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Interactive Comment

Interactive comment on "Interannual climate variability seen in the Pliocene Model Intercomparison Project" *by* C. M. Brierley

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Reply to reviewers comments

I would like to thank both the reviewers for their positive and well considered comments. They raise some valid points and questions. In a revised manuscript, I would address these comments in the following manner.

Reviewer 2

1. I had chosen to show the variance of the Niño 3.4 region rather than the standard deviation in figure 1 simply because it is the conventional definition of the second moment of a distribution. I therefore felt it fitted more appropriately in with the rest of the figure. I am not particularly attached to presenting the variance, and would happily use

the standard deviation in a revised version of the manuscript.

2. The reviewer rightly notes the existence of long preindustrial control simulations for some of the models presented here. In the discussion manuscript, I do not mention them and have not yet analysed them. I had felt that even a thousand year long control simulation would only provide five segments of 200 years, which would not be sufficient to calculate a statistical significance. Should a 2000 year long simulation exist though, that could provide sufficient samples if intermediate sub-sampling is used. I will investigate the length and features of the models' various control simulations before submitting a revised manuscript.

3. The Pacific Decadal Oscillation is calculated as the EOF of the sea surface temperatures in the North Pacific; the poleward portion of which would be covered in sea ice during the winter months (at least in the preindustrial). The ocean covered with sea ice records a surface temperature of -1.8 °C, and therefore the variability in the winter months will be suppressed. I do not think this is an issue for diagnosing the PDO in the simulations presented here. The sea ice retreats in the Pliocene simulations leading to less SSTs suffering from this suppression. Inspection of the PDO patterns shows that all their centres of actions are of the coast of Japan and compare well with the observed pattern.

Reviewer 1

A. The reviewer estimates the probability of the signal occurring by chance. I have also calculated this later in the manuscript, but arrived at a somewhat lower value. Firstly, I should have pointed out in my introduction the section where this issue will be discussed. Secondly, I don't understand the number given by the reviewer. I believe that he meant $1/2^8$ as there are 8 simulations giving a similar sign. Personally I would also include the possibility of no change [giving a probability of $1/3^8$]. Either of these two numbers would give a probability of occurring by chance as statistically insignificant. I acknowledge in the manuscript that my calculation of this chance contains

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uncertainties. Nonetheless, I agree that there is a finite chance that the result could have occurred randomly and without an underlying physical mechanism. In a revised manuscript, I would made this finite chance more clear and add in a sentence to the conclusions elucidating the possible role of randomness in creating the results shown.

B. There is an ongoing discussion in the community about the validity of the some of Mg/Ca SST records. I did not want to descend into this geochemistry debate in a purely modelling manuscript, but rather motivate the following analysis. Personally, I feel that Zhang et al. (2014) was written with a strange emphasis since their TEX86 records do show a weakened gradient in the Pliocene. O'Brien et al. (2014) do not discuss the equatorial gradient in their work (apart from hidden in the supplement). The global nature of the Mg/Ca seawater correction they apply means that it does not alter the SST gradient, unless one takes the difference between different proxies. Incidentally, I feel that O'Brien et al. (2014) over-estimate the Mg/Ca seawater correction and have submitted a piece of correspondence to make this point. Whilst I would be happy to mention this discussion, I do not see it as being necessary here nor would it aid the readability of the manuscript. I would appreciate some guidance by the Editor on this point.

C. As regards authorship, I was not aware of this agreement - perhaps I need to attend PlioMIP workshops. Nonetheless some of the modelling groups have already published work on their SST records (as discussed in section 2.1). I had initially planned this study as a collaboration with both Zhongshi Zhang and Weipeng Zheng. Once I realised the amount of results I had generated alone, I consulted with both of them as to whether they would object with me submitting this a sole author paper. Neither raised the agreement alluded to by the reviewer. Since the work has been now been submitted by myself alone, I would prefer it to stay that way. I do acknowledge the important assistance of the PlioMIP participants and hope this will suffice.

D. Obviously, it would be nicer to have a physical mechanism to explain the results, which is why I covered several in the discussion section. I was not aware that ENSO sensitivity to different boundary conditions had been investigated by Sarah Bonham. As I did not investigate teleconnections (see later), I did not discuss the elements of her work published in Phil Trans (2009). I will endeavour to secure a copy of her thesis to see if there are any useful insights included in it. It does not appear to be accessible via either the White Rose eThesis repository or the British Library - hopefully the Editor will be able to provide me with a copy.

P3789, L20. I will rephrase this sentence.

P3791, L10. The lengths shown in table 1 is the record length I have analysed, not the simulation length. I shall clarify this in the text, and attempt to fit an additional column in the table.

P3791, L19. This shall be addressed in a revised manuscript.

P3791, L25. Trenberth (1997) really does define an El Niño using the 5-month mean anomalies, rather than five year ones. I feel that the reference to the software routine is sufficient instead of an equation. It is my intention to release my software repository used to write this paper publicly, which should also help people understand exactly what method has been followed.

P3792, L22. I can see this has confused the reviewer and will remove this aside in a revised manuscript. A conventional approach when showing two different Niño time-series is to only normalise by the control simulation amplitude, such that the amplitude changes are visible in the timeseries (e.g. in Zhang et al., 2012). I have already investigated the amplitude changes in isolation, so normalise each simulation by it's own respective standard deviation.

E. I actually feel that the wordy description, combined with a publicly accessible software analysis code, is more understandable and easier to digest than a series of equations. I would therefore prefer not to implement these changes in a revised manuscript.

P3793, L16. The purpose of the analysis approach I have adopted is to isolate individual properties of the ENSO behaviour in a similar manner to those by which Power et al. (2013) isolate the changes in El Niño structure. It is otherwise easy to convolve changes in period with those of amplitude. I will clarify this is an revised manuscript.

P3793, L18. Figures will have their subpanels labeled in a revised manuscript and I will refer to Figure 1a here.

P3793. The moments of a distribution are fairly standard and I don't see the benefit of defining by equations here. Wikipedia does a fine job of it.

P3794, L21-26. I had included this paragraph in light of discussions with Julia Tindall on an earlier poster version of this work. She had found a increase in intense El Niños in HadCM3. This is not in contradiction of the results presented here, as the skew of HadCM3 increases whilst the variance [standard deviation] drops. I wanted to point this fact out, but feel I must stop my analysis of ENSO at some point, so had chosen not to present a similar metric as that used by Cai et al. (2013). I will attempt to clarify this paragraph.

P3795 – L10. Figure 2 shows the average of the nine different spectra for each time period. I appreciate that it may be hard to interpret, but An Choi (2013) have shown its utility. I also created a figure showing the different spectra of each model individually. It was even harder to interpret due to the quantity of overlapping lines. I could provide this image as a supplement if required.

E. Section 3.1.3 discusses only the ensemble average change in structure and the reviewer asks about individual members. Whilst each individual member shows deviations from the ensemble mean structure, I did not see anything worth highlighting.

F. I will clarify the direction of the difference in the caption for Figure 4.

P3796, L26. I agree that the PlioMIP simulations may not be in true equilibrium and will clarify this point. However, they are substantially closer to equilibrium than any of the regular CMIP5 scenarios [95 years of transience the 21st Century runs and 150 years

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of response in the 4x CO_2 simulations], which was the point I was trying to make.

P3797, L22. I have looked at all the EOF patterns in the models and subjectively assessed whether they look more like a Basin or Dipole pattern. This is necessary as some models see changes in the relative dominance of the two patterns (e.g. CCSM4). This means the assumption that the Indian Ocean Dipole is the second EOF is not appropriate. The approach is completely subjective, rather than objective. The effort required to develop an objective pattern-matching algorithm better than the human brain for this task could not be justified for this component of the manuscript however.

P3800, L4. I have not investigated the ENSO teleconnections in this manuscript. I agree with the reviewer that it would be interesting to do so and did look in the feasibility of doing it. The primary reason for not tackling this task is the lack of either monthly air temperature or precipitation data in the PlioMIP archive spanning the same period as the SSTs for most of the models. I did look into the teleconnections in NorESM-L, for which this data exists, and found surprisingly little change in the spatial correlation structures.

Interactive comment on Clim. Past Discuss., 10, 3787, 2014.