

Interactive comment on “Evaluation of PMIP2 and PMIP3 simulations of mid-Holocene climate in the Indo-Pacific, Australasian and Southern Ocean regions” by Duncan Ackerley et al.

A. Lorrey (Editor)

a.lorrey@niwa.co.nz

Received and published: 28 February 2017

I openly declare ahead of this review that I know almost all of the authors of this paper personally. I've worked directly with many of them in the past on other published work and with some of them on previously funded projects. I have no personal interest conflicts with any of them.

An additional review was requested of me by CPD because we have been unfortunate to have only one solicited review for this paper. As such I submit this review here, but would like the authors to understand that final adjudication will be undertaken in consultation with the editors of CPD.

[Printer-friendly version](#)

[Discussion paper](#)



Overall, I thought this was a useful manuscript to put forward and it will be a welcome addition to the regional literature when completed. Attempts to reconcile Australian palaeoclimate data with simulations are not numerous. The region has a great number of complexities of climate drivers and processes that could be evaluated using model-proxy comparisons.

However, I have some concerns about how this paper has been presented though, or at least how a few things are couched, and with some of the visual content that is presented (or data not shown). I suggest it could be publishable in CP, but only if many minor and some major revisions were undertaken. Please see specific comments in the attached PDF.

First, the balance of the paper strengths lean heavily toward the analysis and descriptions of the climate model simulations. I'm elated that some basic physics of the climate system has been brought to bear with the incorporation of the discussion on the latitudinal gradient and geostrophic wind equations. There are some very interesting findings here for the model results, but I also cannot determine if any bias corrections were actually undertaken for the circulation patterns or if there is simply reference to identifying them using the post 1750CE data sets. Please make this more clear. Ahead of the following comments, I would also suggest the authors simply recast the use of the palaeo proxy data network (from Reeves et al., 2013a) as supporting field-based information that the models can be compared against (details why are stated below). Section 2.1 is also poorly written; it leads off with a description of INTIMATE and then Reeves et al. 2013 - and it seems very odd to me that for a SHAPE issue that there is no mention of that initiative anywhere, which has superseded INTIMATE in the Southern Hemisphere. I think it would be more contemporary to refine the aims of the model-proxy intercomparison in light of the stated goals of the SHAPE IFG - which are similarly stated on the SHAPE project website.

Second, there appear to be no real surprises to me in terms of the findings - the proxy data-model comparisons are elementary (mostly descriptive, but still very useful

[Printer-friendly version](#)[Discussion paper](#)

and clearly-written). They are divided into sections that essentially show where the proxy-model comparisons work, and where they don't. I would greatly appreciate if the listing of proxy data derived from Reeves et al., 2013 (fundamental to supporting this work) was tabulated, including all metadata about location, type of archive, dating controls, seasonal sensitivity and signature for climate during the 6k interval are stated. The Reeves et al., 2013 paper is also mentioned as providing 'a method' but it does not do that in terms of integrating the data or providing a dynamical understanding of past variability or change with reference to a mean climate state. That particular work collected climate proxy records under certain criterion, and binned them into different geographic regions for Australia. If you adopt the spatial division of Reeves et al., 2013, and the data series used there in, it would be best to simply say so. There in, those geographic regions are somewhat arbitrarily ascribed; but I temper this comment by saying in reality there is good reason to have made those divisions. Just a bit more support and justification from modern climate studies that indicate there is a strong reason for the geographic divisions would go a long way to informing the readership. I believe that information can be easily obtained, and cited in the revised work. And better recognition that the real strength of Reeves et al., 2013 is the pre-selected proxy data that are 'regionally-representative'.

Third, the Reeves et al., 2013 depictions did not compare the past climate change signals to a common modern interval, but rather assessed the direction of change from one time step to the next. This limits a meaningful comparisons of the past patterns that are shown in Reeves et al., 2013 to the climate model simulations shown in this study. I realise there were probably data limitations in Reeves et al., 2013 that sent those authors down such a path, but it was identified as problematic early on (in discussions in Aus-INTIMATE). In this paper, it (and the pictures showing signals for different time slices) is advocated as 'presenting a new opportunity to integrate models with data'. At the risk of repeating myself, it does not: What it does is supply a series of pre-screened data and climate signals for the mid Holocene where assessing PMIP2 and 3 model signals may be undertaken. The authors have largely done this in a point-by-point

[Printer-friendly version](#)[Discussion paper](#)

fashion; if the data from Reeves et al. has been further transformed, it is not clear how it was done. deeper understanding from a data integration would have been more meaningful; so I feel justified in mentioning this specific point here.

Fourth, I would also strongly encourage the authors to submit the data from Reeves et al., 2013 along with this paper, or provide a supplement with stable URLs where the data may be obtained. Sub-issues related to the points of viewing and assessing those data are: a. mapping of proxy signals onto the PMIP simulation outputs shown in Figures 3, 4, and 5. b. being able to observe the time series for each c. seeing how the 6k signatures compare to modern or pre industrial times.

Fifth, the scaling of the proxy signals so that they are compatible with the GCM signals is still unclear to me. this relates to point number 3. In using a tercile-based evaluation system of the proxy data, one needs to create a distribution for the data, with reference to a common interval (also the same interval used in the control run for the model simulations), then establish what the thresholds are for the terciles to obtain meaningful signals (warm, wet, cold, dry etc). that has not been clearly shown anywhere here ... and it cannot rely on antecedent work. Seeing the data and the new analysis are required for the descriptions of the proxies to be understood as factual.

Addressing the above comments, the more minor grammatical issues in the text, and recasting the paper toward the main strengths (modelling results and forcing mechanisms, supported by point data, rather than proxy-model intercomparison) would see this through. I'd also like to encourage the authors to evaluate their future work section and to try to be more broad with regard to proxy development, chronology evaluation and integrative approaches that could help future efforts bring models and proxies together - please see if that can also be done in a more refined manuscript.

I am happy if the authors would like to discuss any of this business directly with me. Best wishes, Drew

[Printer-friendly version](#)[Discussion paper](#)

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/cp-2016-136/cp-2016-136-EC1-supplement.pdf>

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-136, 2016.

CPD

Interactive
comment

Printer-friendly version

Discussion paper

