

Clim. Past Discuss., referee comment RC1
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Comment on cp-2021-58

Chris Brierley (Referee)

Referee comment on "Reduced El Niño variability in the mid-Pliocene according to the PlioMIP2 ensemble" by Arthur M. Oldeman et al., Clim. Past Discuss.,
<https://doi.org/10.5194/cp-2021-58-RC1>, 2021

This manuscript looks at the ENSO in PlioMIP2. It follows the structure and methods of my 2015 paper on PlioMIP1 and finds somewhat similar results. I see no reason why this manuscript should not be published by Climate of the Past, albeit in a slightly revised form.

My main issue with the research as written is that it does not incorporate the recent literature on the subject of modelling ENSO. Recent advances relating to the paleoclimate component of CMIP6 have been included, but there is a raft of publications looking at ENSO in the historical and future scenarios that are not considered. This is particularly noticeable with the discussion of Yeh et al (2009) - there are certainly more recent works looking at future changes in ENSO flavour. These are reviewed in the upcoming IPCC AR6 and I strongly advise waiting until that is published before completing your revisions.

Specific comments:

L22. You give the date for the whole mPWP here, but never explicitly mention that PlioMIP is aimed at an interglacial within this period. KM5c is mentioned in passing later on, but without dates.

L37. You need to describe what the bracket with "(1.7-5.2oC)" indicates. Does it include HadGEM3-GC31-LL?

L60. Did either of Fedorov et al and Barriero et al actually use coupled models? If not, how could be expected to any variability?

L68. (or longer periods) -> (so longer periods)

L85. Brown et al (2020) did not look at future scenarios. Rather they used the idealised warming scenarios of the CMIP DECK.

L123. Interpolating variables onto a common grid prior to analysis is not best practice. This would act to smooth out spatial variations and lop-off extremes. I do not expect you to re-perform all of your analysis, as I suspect that it will make little difference to your conclusions. You may want to mention why this is the case in your methods section. Try to avoid this approach in future – it should be performed at the last possible moment, as part of the ensemble averaging.

Table 1. Why are you not using the model acronyms that are part of the CMIP controlled vocabulary? Will this not prevent your study coming up on Google Scholar searches and the like?

Table 1. Are you sure that HadCM3 was a CMIP5 model? I thought it was CMIP3.

L155. Factor 3.0 -> factor of 3.0

L165. I believe that "standardised" should be "normalised" here.

L162. How does your statement about the internal variation compare with the conclusions of Tindall et al?

L182. Please mention if the monthly SST anomalies are detrended prior to the PCA.

L230. GISS-E2.1-G was not in PlioMIP1 – rather that was GISS-E2-R. Please justify why you consider these to be iterations of the same model, rather than different generations as other studies often do.

Sect 3.1.2 How many of the 17 model show an lengthening of the periods – refer to Fig. S2 for this.

L274. Please provide more explanation about the word "normalised" - is the information about the ENSO amplitude (in oC) contained within the EOF or the PC?

L282. Cite Fig. S3 to support this.

L289. Please rephrase to only use word "region" to have a single meaning.

L309. Please compare this HadCM3 result with that shown in Brierley (2015).

L325. This sentence reads as if it encompasses the warmer E. Pac coastal temperatures. These are instead a feature of insufficient ocean model resolution to capture the coastal upwelling.

L333. Choosing a red-green color scheme is unhelpful to readers who are colorblind.

L340. The alphabetic indicators have not been introduced earlier. Why do you start at P? Please add letters to Fig 7.

L352. Warming trends (up to average year of 1970) are less than 1oC globally, let alone tropical pacific. Put nuance in your expectation.

L358. Please be consistent with your longitude names. Fig 7, showing these boxes, goes 0--360 not -180--180.

L358. Is there a reference to choose these regions? You later discuss how these regions are inappropriate for 2 models. Maybe using max and min in two larger region would be more helpful?

Fig 8. prints badly in black & white, I've discovered. Also would be poor color choice for those suffering from deuteranomaly.

L375. I feel that it is worth stressing that Brown et al include many of the models used here.

Sect 3.2.3 Any lines of best fit would not pass through the origin in either Fig 10a or 10b. What are the implications for that on your interpretation? Are you expecting an external condition to cause a roughly 25% amplitude reduction and then the zonal gradient to control the deviations from that?

L394. Pre-industrial -> E280?

L399. Fig 11d does not show 'reduction' in ENSO amplitude, as that implicitly is between E280 and Eoi400. Please rephrase sentence.

Sect 4.1.1 You do not mention the observational uncertainty in the earliest portion of the record (e.g. Ilyas et al, 2017: <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017GL074596>). I wonder if we really can resolve ENSO flavour accurately back in the 1920s.

L440. Is "clear agreement" the best choice of words. I know what you mean – but isn't more like "cannot rule out disagreement"

L447. This sentence needs a citation.

L450-2. I do not see how both these sentence can be true. Isn't a reduction in SST gradient the defining feature of an El Nino-like change? Please reconcile these facts.

L456. Personally, I feel that Watanabe et al.'s conclusions on this point overstepped their data.

L505. Difference -> different

L545. Spell out how the longer, yet inconstant, record lengths I used in 2015 could impact the results. Also, are they detrimental impacts?

L560. The analysis of the 500 year long records is a really nice addition to the paper. I was left wondering what the impact of internal variability could be on Fig. 10. You could readily assess this, and that might explain why the overlapping models in a & b differ (although note GISS is not overlapping).

L572. The Indonesia throughflow impacts were also investigated by myself (Brierley & Fedorov, 2016) and Zhongshi Zhang (certainly Zhang et al 2016, but maybe others).

L605. Is your 24% reduction distinguishable from my 20% reduction? Can you use your analysis of the 500 year simulations to estimate a significance?

REF:

Brierley, C.M. and Fedorov, A.V., 2016. Comparing the impacts of Miocene–Pliocene changes in inter-ocean gateways on climate: Central American Seaway, Bering Strait, and Indonesia. *Earth and Planetary Science Letters*, 444, pp.116-130.

Ilyas, M., Brierley, C. M., & Guillas, S. (2017). Uncertainty in regional temperatures inferred from sparse global observations: Application to a probabilistic classification of El Niño. *Geophysical Research Letters*, 44(17), 9068-9074: <http://onlinelibrary.wiley.com/doi/10.1002/2017GL074596/full>.

Zhang Zhongshi, Li Xiangyu, Yan Qing, Zhang Ran. Impact of changes in seaways on Chinese climate during Pliocene[J]. *Quaternary Sciences*, 2016, 36(3): 768-774.