

Interactive comment on “Tropical cyclone genesis across palaeoclimates” by J. H. Koh and C. M. Brierley

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I had some questions about the experimental design. I believe that you are using the coupled model PMIP output, rather than simulations driven by reconstructed SSTs to perform the TC activity calculations (correct me if that's wrong). As the authors are well aware, coupled models have some well documented biases in both LGM and Pliocene, and some authors (including both the authors and myself) have proposed some of the major changes in SST gradients in these paleoclimates have been produced by TC activity that is under-represented in these models. So, is there a basic self-consistency problem in this paper? If TCs are a feedback, then perhaps it is unsurprising that giving a TC model SSTs that implicitly have removed TC forcing from them yields TCs that do not change much.

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Even though the reconstructions have their own uncertainties, it would be very interesting to see if using reconstructed, potentially more 'correct' SSTs gives the same result. If the result is different, where does that leave us?

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