

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

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This is a review of Koh and Brierley, "Tropical cyclone genesis across palaeoclimates". The issue of how tropical cyclones (TCs) have changed in the past and future is an important scientific problem, with important implications. To my mind, the most interesting part of this problem is the potential role tropical cyclones play as feedbacks in the climate system, but that is not something really addressable in this paper as currently framed. So while this vitiates some of the potential interest and importance of the results, the paper still has merits as a step in the direction of using complex models to answer first order questions about how TC activity may have changed in the past.

I think I am not the only one who will find the presentation somewhat confusing, but this is easily remedied by altering the title and some of the verbiage in the paper. Since

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Climate of the Past may not have an audience that is very knowledgeable about TC activity and the theory and metrics which surround it, I think that some more plain language about what is and what is not being studied and constrained is in order. For example, the results do not provide information on how many storms will reach maturity, how long they will last, how strong they will be, or where they will go. Those are some pretty large limitations and they should be put right up front near the beginning of the paper in a language that the broad readership of CoP can understand.

I would clear things up in the beginning better, as well as a more detailed explanation of the methods for readers who may want to duplicate and extend the results. I think that adding a more clear section about the limitations and assumptions is important. I have raised three of those in the short comments and I find the brief responses to be a good basis for a more formal response and I encourage the authors to include a section describing some of these limitations better.

I am also particularly concerned about highlighting for readers that the use of 'correct' SSTs might substantially alter the results. The approach that the authors have taken is fine with me, but it will be good that people can understand how these results might be an example of garbage in=gabrage out. I do not believe in a literal interpretation of proxy data and that is not what I am arguing for here, but merely to acknowledge that the models have large and well quantified biases for these time intervals and it is eminently plausible that with different SSTs the result might be different.

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