Clim. Past Discuss., 11, C75–C80, 2015
www.clim-past-discuss.net/11/C75/2015/

Climate of the Past
Discussions
Open Access

CPD
11, C75–C80, 2015

Interactive Comment

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

R. Korty (Referee)

korty@tamu.edu

Received and published: 9 March 2015

Koh and Brierley present an analysis of large-scale environmental conditions in simulations of the LGM, mid-Holocene, and Pliocene prepared for PMIP3 and PlioMIP. They find that most of the features reported by Korty et al. (2012), who performed a similar analysis on the older PMIP2 for the LGM and mid-Holocene, are present in the newer, higher-resolution versions in PMIP3. I'm delighted to see the results they have presented, and believe there is great value in the work they've done. But one issue that affects the entire paper is a misapplication of the term "genesis" when what this paper actually presents is an analysis of variables that are known to be important for genesis (wind shear, vorticity, various thermodynamic quantities). This is a critical problem in need of urgent repair. Even the title of the paper is affected! The work here does not

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

examine genesis across paleoclimates; rather, it examines how the conditions necessary for genesis to occur change across paleoclimate simulations. (It would be proper to call these genesis factors, or large-scale environmental conditions, or something of that sort.)

Relatedly, one critical detail was not clearly presented in the manuscript: were these analyses done with monthly averaged data? With daily data? With climatologically averaged data (i.e., where data from each "January" is averaged with all other "Januaries" to form an averaged annual cycle)? As the comment posted by Matt Huber earlier this week notes, these are nonlinear quantities, and they will differ depending on the order of averaging. (We found in Korty et al. that the qualitative results were not sensitive, however, to these differences.)

What does matter about the temporal frequency is what types of analyses are possible with the data. I suspect that these were likely from monthly mean climatologies or monthly averaged data (the phrasing in part of Section 2 seems to suggest this), in which case there are no tropical cyclones actually present or tracked in any of the data. So the language that permeates the paper about "cyclone frequency" or "cyclone genesis" is extraordinarily misleading. It gives the false impression that individual model storms have been defined, tracked, and aggregated and presented. In fact, that would only be possible if the frequency were 6 hourly or finer, and a particular tracking algorithm (e.g., Camargo and Zebiak 2002, among many others) were defined and employed.

If I am correct that what is presented here is solely genesis factors and the genesis potential index, care must be given to presenting what such an index actually is. It is not measuring any actual genesis, only identifying where the combination of large-scale factors conducive for it are present. It is an empirical formula that in effect regressed large-scale environmental conditions against a dataset of contemporary record of actual storms. With no evidence of how many storms occurred during the LGM or Pliocene, there is no way to calibrate such an index for a prior climate. In this

CPD
11, C75–C80, 2015

case, the index is merely one tool for summarizing how the environment changes. (And it is just one possible way to summarize it, which is why I advocate for the individual factors ought to be presented and discussed, as they are here.)

The problem with the paper as it stands is the use of terminology—beginning with the title and line 1 of the abstract—about changes in cyclone “genesis”. What this paper is actually presenting are changes to large-scale environmental conditions that are known to be important to genesis. This is a critical distinction that needs to be corrected. I have tried to catalog examples of this below, though the list is by no means exhaustive; a careful editing of the entire manuscript to purge these phrases is necessary.

Specific comments: The principal problem begins with the title, and then on line 1 of the abstract: this paper does not investigate tropical cyclone genesis, but rather conditions that are necessary for genesis.

Abstract lines 9-10: Given the data used in this study, one cannot assess cyclone frequency or spatial patterns of cyclogenesis; you can only assess changes to the conditions that are necessary for it.

Abstract, lines 13-20: Same as before; you are not assessing changes in genesis, but rather how the large-scale conditions that allow it differ.

Line 2, page 185: you are not assessing TC activity in these simulations unless you track specific features or simulate them using various downscaling methods. What is presented here are analyses of how environmental conditions differ.

Section 2: This section needs to detail what data is analyzed. What is the frequency of the output (monthly, etc.)? What is the vertical resolution, how high into the stratosphere are temperature data available (this affects potential intensity)?

Line 4, page 187: although we modified Emanuel’s earlier index by employing a “clipped vorticity” dependence, Tippett et al. (2011) should be referenced here; that is the paper that showed vorticity was not a rate limiting factor in these genesis indices outside of latitudes very near the equator.

Line 10, page 187: A tropical storm is defined when winds exceed 17 m/s, not 35. The 35 m/s in the index is unrelated to this definition.

Lines 12-16, page 187: If you wish to include the coefficient b in your definition of the genesis potential index, why not choose a value that normalizes GPI to yield a value of 80 or 90 per annum in the preindustrial era control of each model (a model-specific value of the coefficient)? What matters here are how the factors change away from their values in the control climate of their own particular model. By using a single number across the ensemble, you give the impression that one model produces many storms and another few, when in fact you're merely comparing how representations of the large-scale conditions vary. The raw numbers are very likely to differ with model resolution, which varies from model to model. This issue feeds into my larger concern that the way this was presented left an impression that you had tracked actual model-generated storms, when that is not the case.

Line 5, page 191: No model is simulating genesis here; they are producing conditions that should support genesis.

Line 8 page 191: Same problem with wording; conditions in the southern Indian Ocean may be more favorable in JFMA with limited potential in JASO.

Line 15 page 191: Again, there is no intensity of cyclone genesis; rather, there are large-scale conditions that are more favorable.

Line 27 page 191: genesis potential, not intensity.

Line 13 page 192: You do not diagnose cylcogenesis in any of the five models; you calculate a genesis potential index for each.

Lines 27-28, page 192: this indicates a shift in the region where conditions most favor genesis, not a shift in genesis itself.

CPD
11, C75–C80, 2015

Lines 3-4, page 193: There is a decrease in the favorability of conditions in the Northern Hemisphere, not in genesis. Opposite in the Southern.

Lines 5, 6, 7, page 193: Same problem with terminology.

Line 9, page 193: In Korty et al., we did not diagnose genesis, but rather genesis potential.

There are many other examples of this throughout the remainder of Sections 4, 5, and 6; rather than document all of them here, a careful editing to purge the manuscript of all language of genesis where genesis potential index was meant is necessary.

Section 4.6: The presentation of this entire section should be rethought in light of my comments about what the index really is. This is not an analysis of frequency, but rather whether conditions are moving in a direction that are more or less favorable.

Table 2: The column titled "storms per year" must be changed. There are zero "storms per year" in all of your data! This is again very misleading as it creates the impression that you have tracked some number of storms in each model—perhaps 94 such events in CCSM4, for example. You are not tracking individual events; rather you are calculating an index of how favorable the large-scale atmosphere is. Whether or not CCSM4 generates 94, ten thousand, or zero storms in any year of simulation could only be determined by employing a definition of what a storm is in a GCM and then applying an algorithm to detect how often that definition is met. That would require at least 4 times daily data, which is not available for most fields in most paleoclimate models.

What I recommend here is that you choose a value of the coefficient b that would normalize each model's preindustrial era to either 80 or 90 per annumâĂŤthat would mean a smaller coefficient than you used for CCSM4 and MIROC4m, for example, while a larger one for FGOALS, HADGEM2, IPSL, and MIROC-ESM. You should then replace the "storms per year" column in this table with the value of b used in the formula for that model.

CPD

11, C75–C80, 2015

Figure 3: “Cyclone genesis difference”; same in Figures 5 and 7. Figure 9 uses “cyclone genesis frequency”, as does Figures 10 and 11. All of these need to be corrected here and in the text. Again, in each case these are really differences in the genesis potential index, etc.

Interactive comment on Clim. Past Discuss., 11, 181, 2015.

CPD

11, C75–C80, 2015

Interactive Comment