

Interactive comment on “Atlantic Hurricane response to Sahara greening and reduced dust emissions during the mid-Holocene” by Samuel Dandoy et al.

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This is a very nice and useful study on the interesting responses of TC activity to orbital forcing, dust, and North African land surface properties. Each of these has direct relevance to model responses in mid-Holocene simulations, and understanding how TCs respond to them are useful for broader questions of hurricanes and climate. The paper is well written and there are only a few specific comments on the text, which I post at the end of this review. I do have a few general comments that I expect can be easily addressed with minor revisions.

General comments:

I see an interesting consistency between the changes in the seasonal cycle reported here and those reported in earlier papers on environmental conditions at mid-Holocene that I think you would be well-served to highlight. Kory et al. (2012), who studied the response to PMIP forcing (i.e., orbit only), reported that the Northern Hemisphere season was less favorable during its summer months, but that the environment switched to become more favorable by October and November (see their discussion at several points in their section 4 as well as their Figure 8). As discussed in that paper, the response was largely driven by the behavior of potential intensity (V_p): the atmosphere warmed faster in response to positive summer radiation anomalies than did the ocean surface, resulting in lower V_p during early summer, but V_p was higher during autumn after the ocean surface warming caught up. Koh and Brierley didn't find the increase in October/November conditions to be robust from model to model (their page 1445), but my impression from their paper was that there was a robust decline in the earlier summer months, which is again consistent with your findings here and with those of Kory et al. Thus, your results on the timing of changes in Northern Hemisphere TC activity show important consistency with earlier work on this subject. That is, there is a repression of either modeled activity or in the environmental conditions that support them during the summer months in all of these studies, which is later offset by increases in autumn activity in many of them. The difference across these investigations has been whether this autumnal increase would be large enough to only partially offsets the summer decline, cancel out changes, or whether it overwhelms the decreases from earlier months. You note this at line 360, but I think you should highlight this comparison in the introduction, as well as in Section 3.2 where much of this detail is presented. Your work, along with that of Pausata et al. (2017), has shown that there is an important (even dominant) role for dust related to the Green Sahara, but I think these similarities with the earlier work on PMIP simulations shows there is an imprint from the orbital changes present in all of them also.

Your analysis of the AEWs and comparison with the results of Patricola et al. (2018) is important. I wonder whether this could be taken one step further by commenting

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further on differences in genesis locations between Pausata et al. (2017) and this study. With a reduced AEW production at 6ka, do you see differences in genesis locations between explicitly generated storms (this study) and statistically downscaled (Pausata et al.) that are as large as a 20th century control? If there are differences in genesis location, do these have repercussions in track densities downstream? Finally, what are the implications (if any) for paleotempestology sites in the western part of the basin?

Specific comments:

Abstract: You characterize the mid-Holocene as a warm climate state, but the warming is neither global or year-round. . . wouldn't it be better to characterize this as a change in the amplitude of the annual cycle? It is, however, a warmer state during the Northern Hemisphere summer, which is perhaps what you meant here, but if so you should clarify it explicitly. Hurricanes will respond to transient radiation anomalies differently than to warming from other sources, such as increases in CO₂—e.g., Emanuel and Sobel 2013, for example—and readers should be cautioned about that.

General comment about the opening paragraph: This is a rather general introduction to why hurricane-climate studies matter, but I think it might strengthen this paper to start off focused with why studying the effects of dust emissions from the mid-Holocene is useful. I think your work makes a strong case that dust matters (perhaps even overwhelming the large orbital changes), so I recommend leading off with that. This has relevance to modern times as interannual Saharan dust variability can have large effects on Atlantic activity, and these effects are consequential for understanding what changes may come in the decades ahead.

Line 27: replace “rebuilt” with “rebuild”. Also, “infrastructure” is more accurate than “vast quarters. . .”

Line 37: “recent decades” rather than “last decades” (the Emanuel paper cited was published 15 years ago, and it reported on storm trends primarily covering the decades

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of the late 20th century)

Line 66: As you note, neither the Korty et al. nor Koh and Brierley papers studied simulated TCs, so it is not accurate to say they suggest a decrease in “activity”. It would be better to say they found the orbit changes induce changes in Northern Hemisphere summer that—all else being equal—make environments more challenging for TCs.

Line 66: the meaning of “despite changes in summer insolation forcing” is unclear and I found the phrase is confusing. (Especially because you are talking about both Northern and Southern Hemispheres in this sentence, and insolation anomalies are opposite between them.)

Line 154: Just curious, at what specific pressure levels (or layers) do you calculate the “boundary layer” and “midtroposphere” entropies in the chi parameter?

Section 3.2: As I mentioned above, I think highlighting the similarities and differences between your results in this section with the predictions of Korty et al. and Koh and Brierley (based on orbit only) is important. There is an important similarity in that early summer months appear less conducive for TCs across all of these studies, with an increase later in the season in many of them. This suggests there is a detectable role of the orbit present in your results too, even if the Atlantic dust variations are strong enough to control the annual total.

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