Review of “The Response of Tropical Cyclone Intensity to Temperature Profile Change” by J.M. Done. G.M. Lackmann, and A.F. Prein

Summary: This study uses historical data and idealized modeling to understand how tropical cyclone (TC) intensity changes with varying profiles of atmospheric warming. While historical temperature changes coincide with stronger TC intensities, stronger TCs generally have a greater sensitivity to atmospheric profile changes than weaker TCs. Additionally, as the atmospheric profile warms, TCs interestingly tend to become more efficient heat engines. While these differences over time are robust, results from a large ensemble of simulations show that the changes due to varying the atmospheric temperature profile are small compared to observed TC intensity change trends, meaning changes to the atmospheric temperature profile cannot fully explain the observed recent increase in TC intensity.

Overall, the study was well-written and well-presented. I found two main issues which I think will be easy to address. First, the colors used in the figures, particularly Figs.1b and 2b, were incredibly hard to distinguish. Please change the colors or add symbols to the lines for them to be more distinctive. Secondly, throughout the manuscript, the authors continually refer to the results derived from axisymmetric simulations as generalized results for TC intensity. For example, at the beginning of the Concluding Discussion in L513-514, the authors state: “To do so we worked to isolate and quantify the response of TC intensity to observed trends in environmental temperature using a combination of historical data analysis and idealized numerical modelling.” With very well-documented limitations of axisymmetric simulations, as well as the plethora of work about how thermodynamic and kinematic asymmetries influence TC intensity (See comment on L96-102), these generalizations about TC intensity are inappropriate. The authors do appropriately state their weaknesses in L475-480 and L573-575 to put the work in proper context, but this needs to be more clearly articulated throughout the manuscript. To be clear, I am not criticizing the choice to use axisymmetric simulations. My concern is how the significance of the results derived from the simulations are portrayed. Given that these concerns can be addressed without additional analysis, I am recommending **Acceptance after minor revisions**.

Points by line number:

L96-102: It is not surprising that the intensity evolution of TCs does not closely follow that of PI. There are many internal processes, particularly asymmetries in the thermodynamic (e.g., in the distribution of moist entropy, Riemer et al. 2010; Alland et al.2021a,b; Wadler et al. 2021) and dynamics (e.g., in the convective or precipitation distributions, Rogers et al. 2013; Zawislak et al. 2016; Alvey et al. 2020) which would limit the strength of a TC.

L155-156: I don’t understand why the authors would not just use ERA5.1 for their entire analysis. Some clarification here would be beneficial.

L194-196: I disagree with this sentence. As alluded to in comment to L96-102, asymmetric thermodynamic processes (e.g., downdraft and radial ventilation), which occur as a response to

TC-environment interactions, would certainly "vary substantially in direct response to changes in the environmental temperature profile". This is a more significant limitation than the authors mention. Now, I do not believe this invalidates the study, but it should be emphasized further.

L224: Why did the authors change PI to E-PI while still citing the Emanuel (1988) paper for both. Some clarification is needed here.

Figure 1b: It is very difficult to distinguish the line colors, even when zoomed in 200%! Please use a more divergent colorbar. Also, for all these simulations, it is unclear how humidity was treated? Was the specific humidity or relative humidity maintained constant from the Dunion moist-tropical sounding? This would be helpful to know.

Figure 2 caption: Missing a space between "to2017" (L286)

Figure2b: Same comment in color as in Figure1b. I can't distinguish the color between ERA5 00s and ERA5 90s.

L304-306: In my experience, soundings around TCs vary substantially with radius, storm-relative location, etc (see Zawislak et al. 2016 as an example). I understand this is the best methodology for climate studies, but I wonder how representative these soundings can be of TC environments? Some comments about this in the text would be nice.

L418-419: It doesn't look to me like the stead-state intensity of the simple radiation members is that much weaker than the ensemble mean. Certainly, those members intensify at a slower rate, but by hour 150, it looks like a number of those members are stronger than the ensemble mean.

L486-488: Very small detail, but these sentences can be combined to read: "The similarity in outflow temperatures is consistent with the Fixed Anvil Temperature (FAT) hypothesis (Hartmann and Larson, 2002) which argues that the environmental cooling rate is largely governed by temperature."

L587-589: Thank you for including this contingency. While many use the Dunion moist tropical sounding, it is not realistic in many TC environments. The older Jordan mean sounding (Jordan 1958) can produce more realistic simulations.

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