Summary

In “The Response of Tropical Cyclone Intensity to Temperature Profile Change”, Done et al evaluate how tropical cyclone intensity responds to changes in vertical temperature profile in idealized simulations. The changes in temperature profile are derived from the RAOBCORE data and from ERA5 reanalysis and are used as an initial condition to CM1 axisymmetric simulations. Resulting changes in TC intensity are then explained by invoking the concept of potential intensity, and it is found that the sign of intensity changes corresponds to the sign of the change in thermodynamic efficiency, one of the components of potential intensity. As the climate warms, TCs become more intense.

I think the paper was quite interesting to read, and that its goal represents an important endeavour in bridging the gap between theory and observations. However, there are issues in the comparison between the modelling results and the theory that need to be addressed before it can be considered for publication. For these reasons, I recommend major revisions.

Major comments

1. Comparison to PI theory

1.1. Efficiency vs disequilibrium

Throughout this paper, changes in intensity are discussed in the context of changes in E-PI qualitatively only via the changes in thermodynamic efficiency, which is problematic. The square of E-PI is proportional to the product of the thermodynamic efficiency and the thermodynamic disequilibrium, not only to the thermodynamic efficiency. While the role of the thermodynamic disequilibrium is acknowledged in the introduction, it is not discussed further in the paper. This is an issue because changes in thermodynamic disequilibrium, not efficiency, dominate PI variations in multiple contexts, from seasonal variations [Gilford et al., 2017] to interannual and decadal variations [Rousseau-Rizzi and Emanuel, 2021].

Table 2 clearly shows substantial variations of SST between experiments, which reinforce the idea that changes in thermodynamic disequilibrium might be dominant here too. Further, removing upper tropospheric warming or stratospheric cooling from the end-of-century experiment results in much smaller changes in E-PI or in TC minimum pressure than between present-day and end-of-century, which suggests that disequilibrium, not efficiency, is responsible for the intensity increase from present-day to end-of-century. In other words, changes in saturation enthalpy at sea-surface temperature relative to near-surface air enthalpy may be more important to changes in TC intensity than the changes in efficiency are. Hence, I think that, to interpret the results of the TC simulations presented in this study by comparing them to E-PI, it is necessary to address how disequilibrium changes by comparison to efficiency.

As a possibly useful aside: the E-PI algorithm used here is formulated following a CAPE-based definition of E-PI, which does not depend explicitly on efficiency and disequilibrium like the "Carnot" form of E-PI does. Rousseau-Rizzi et al. [2022] provides a comparison of the CAPE and Carnot forms of PI which may be useful here to assess why E-PI is changing.

1.2. Temperature profile changes

This study generally interprets changes in TC intensity due to changes in temperature profile in light of E-PI theory. I think this calls for a small but important clarification. Since E-PI depends on the difference between environmental CAPE and saturation CAPE at SST, most of the effect of the environmental profile on E-PI simply cancels out [e.g., Garner, 2015, Rousseau-Rizzi et al., 2022]. The only locations where the environmental profile matters for E-PI in the algorithm (unless the CIN is larger than the CAPE), are near the surface and near the storm outflow. In other words, in most cases, perturbing the temperature profile in the mid-troposphere will result in zero change in E-PI.

Hence, I think it would be useful to be just a bit more precise throughout the paper. Instead of talking about changes to the "temperature profile" at large influencing E-PI, it could be better to talk about changes in "outflow layer temperature" or "upper levels stratification" or something similar. I do not suggest that changes in mid-level temperature would have no effect on simulated TC intensity itself, but simply that any response in simulated TC intensity would be inconsistent with PI theory (which is, in itself, interesting).

2. Numerical domain size

At L199, it is stated that the radial domain length is 768 km which, I strongly believe, is insufficient. This is particularly true with cloud-radiation interactions, which you have (I think), and which tends to greatly increase the radial extent of TCs [Bu et al., 2014]. I think a radial extent of 3000 km is closer to what you might need to completely avoid having the secondary circulation impede on the outer boundary, which would influence the solution. I would have no problem with a stretched horizontal grid to help you avoid increasing your current computing cost too much if that is an issue. Hence, I think it is important to verify whether the domain is large enough, and if not, to expand it.

Minor comments

Comment 1.

L92-L94: These seem to be presented as two distinct effects, but my impression is that, perhaps, the question is whether the TC outflow temperature occurs in the lower stratosphere or in the upper troposphere. If the former, thermodynamic efficiency increases, and if the latter, it decreases, which might explain the bimodal changes in intensity with warming.

Comment 2.

L97-L100: This disparity between PI changes and simulated changes could be due to the outflow temperature as computed in a PI algorithm being lower than the actual outflow temperature of the storm, so that SSTs decrease in a region that could in theory impact TC intensity, but in practice does not. This is just a thought, I am not very familiar with the paper by Vecchi et al. (2013).

Comment 3.

Table 2: It would be nice to clarify how the variations in SST between experiments are obtained.

Comment 4.

L181-L186: This is an interesting idea but an odd formulation. In essence, I feel like you mean that changes in the temperature stratification near the tropopause may enhance or decrease the sensitivity of the outflow temperature to the other parameters controlling the intensity of the TC. For example $dT_{out}/dSST$ may increase if stratification decreases. Is that correct?

Comment 5.

L209-L210: Does lowering 1000hPa down to 1015 hPa make much of a difference? Could it be simpler to say "an sst of 28 C, close to the near-surface air temperature" if that is the case?

Comment 6.

L216-L217: Here and elsewhere in the paper, I think you might have switched up steady-state definitions. Did you mean "we focus on core steady-state rather than on the equilibrium state." ? In Rousseau-Rizzi et al. [2021], equilibrium state refers to periods occurring tens of days after peak intensity, while core steady-state refers to time surrounding or immediately following peak intensity (100hrs to 200hrs would fall in the core steady-state category).

Comment 7.

Figure 2: I think it would be very useful, and important to the interpretation, to have the corresponding SST changes and trends plotted at the bottom of each of these panels.

Comment 8.

L302-L303: The faster warming of strong TC environments may be due to increase in the SST heterogeneity in the tropics, leading to PI increases that are larger than the average of the tropics. The fact that this is occurring mostly under 850 hPa may be due to radiation by gravity waves homogenizing temperature above that level as explained by the WTG approximation. I think it would again be interesting to have SST changes for the corresponding profiles on plots, as I would expect that the enhanced increase in sub-850 hPa temperature is due to an locally enhanced increase in SST. If SST was not increasing faster than the tropical average in strong TC environments, the association with such a profile would lead to a decrease in thermodynamic disequilibrium and hence in PI, and probably weaker TCs.

Comment 9.

Figure 4b: Based on L362, I would expect the red-dashed E-PI trend to be at 0.09 m/s/year here, not 0.12 m/s/year at is appears to be

Comment 10.

Figure 4: I think it could be interesting to see quantile changes vs E-PI, but I leave it up to the authors to decide if they think it adds to the paper and if they want to add it.

Comment 11.

L433-L434: It is worth specifying in the methods whether PI in the simulations is computed a priori (e.g., using pyPI at the initial time) or in situ (e.g., in Bryan and Rotunno 2009). If, as I seem to understand, the computation of PI occurs at the initial time based on the temperature profile, this interesting discrepancy may be due to a mismatch between the E-PI-calculated outflow temperature and the realized outflow temperature of the storm.

Comment 12.

L441-L453: I don't know whether saying that the differences between the experiments are "small" is very useful, given the fact that the ensembles perturb numerical and physical parameters to which the TC intensity is highly sensitive. Since it is physics that is perturbed in the ensemble, not initial conditions, and we don't really expect physics to change with climate, wouldn't it make sense to simply take the central pressure difference between each corresponding ensemble member in different warming scenarios (i.e. same physics in present day and end of century)? Then the distribution of pressure differences could be used to produce a box plot of central pressure change and to verify whether change is statistically different from zero. I think this may yield similar results to the Wilcoxon test you are performing here, so please only add this if you feel that it would improve the paper. Disregard otherwise.

Comment 13.

L505-L508 and elsewhere: This is one instance of Major comment 1.1. For an imaginary completely fixed temperature profile, if SST increases, thermodynamic efficiency will also increase, but the biggest contribution to PI by far will come from thermodynamic disequilibrium, not efficiency. Hence this interpretation that changes in intensity are explained by the changes in efficiency is incomplete.

* **Final note:** I really appreciated reading this paper and I hope it can be published! I cite a few papers in this revision, including some of my own, which was meant to provide examples. Please do not feel the need to cite them.
Raphaël Rousseau-Rizzi

References

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