Responses to the Reviewers of the Revised Manuscript

**CYCLOPs: A Unified Framework for Surface Flux-Driven Cyclones Outside the Tropics**

We thank the reviewers for their second round of reviews and here respond to their suggested minor revisions.

**Response to Reviewer 1:**

First, we apologize for not explicitly stating line numbers that we revised; we thought that the revisions would be clear in the tracked-changes version of the paper. Also, while we stated that we had included a new reference to the point about the simultaneous operation of both advection and surface enthalpy fluxes, we in fact forgot to include that reference. The reviewer correctly surmised that the reference is Fantini (1990). It is now included in the present version.

As to the reviewer's Point 1: The reviewer argues that cyclone amplification cannot occur simultaneously by surface enthalpy fluxes and advection, but this clearly does happen in the model described by Fantini (see, e.g., his Figure 15). The model itself is not highly idealized (it is a nonlinear, nonhydrostatic, convection-permitting model) but it is run in idealized environments, which permits him to, for example, unambiguously specify the initial air-sea thermodynamic disequilibrium. Later work, summarized to some extent in the Wernli-Gray 2024 paper, does not very clearly distinguish whether any added amplification by surface enthalpy fluxes occurs simultaneously with or before or after the baroclinic phase.

The point of contention here is not central to our current paper, but we have slightly revised the paragraph on lines 64-72 to try to compromise between the reviewer's view and ours. If the reviewer is so inclined, we would be happy to engage him or her in an offline discussion of this point if he or she is so inclined.

On Point 2: We share the reviewer's view that the there is a gray zone between tropical transition and CYCLOP development, and have modified the language near line 626 to reflect this. We would point out that if there is no antecedent potential intensity whatsoever, then surface flux-driven cyclogenesis must be the result of local PI generation and thus could be considered a "pure" CYCLOP. Likewise, if the enhancement of antecedent PI by the upper-level trough is trivial, as seems to have been the case, for example, in Hurricane Daniel, then the development is much closer to pure tropical transition. In between these two limiting cases is probably a continuum. In our view, the existence of such a continuum in no way negates the usefulness of the CYCLOP concept, anymore than the continuous spectrum of the strengths of upper-level systems that trigger tropical transition lessens the usefulness of that concept.

On the reviewer's detailed comment: We feel that Figure 1 benefits from its comparative simplicity and think that adding in a potential pre-existing surface low (or high for that matter), while it would add generality to this conceptual figure, would also add complexity that would make the sequence potentially more difficult to follow, so we elect not to modify that figure or the existing discussion around it. About lines 243-245: We have added a suitable reference.

**Response to Reviewer 2:**

We appreciate that we caused some confusion in switching from a modification of the potential intensity based on the upper-level geopotential perturbation to one based on the near-surface geopotential perturbation. The first author takes full responsibility for this change. The original equation stemmed from his earlier work on medicanes (Emanuel, 2005) and sought to answer a different question: Given some climatological mean state, how would potential intensity be affected by the insertion of an upper-level, cold-core cutoff cyclone? But here the objective is very different. The upper-level, synoptic-scale cyclone is likely to be well represented in the ERA-5 reanalyses, so no modification of the potential intensity calculated using the reanalysis data near the upper cyclone center is necessary. On the other hand, once a warm core, surface flux-driven cyclone begins to develop under the cutoff low aloft, the warming of the column modifies the pre-existing potential intensity under that upper-level cyclone. We want to know what the potential intensity was *before* the warm core cyclone developed. That is what the new correction accomplishes.

The same exact problem occurs for normal tropical cyclones. Potential intensity calculated from operational analyses usually shows a diminishment of PI near the TC center. In the case of a very strong TC, this may cause the PI to approach zero near its center. For this reason, when we need PI as an input to a TC model, we usually use the PI several days before the analysis in question. Given the rapid evolution and local nature of PI in the case of a CYCLOP, we do not have this luxury and thus use equation (1) of the paper to correct for the presence of a CYCLOP in the calculation of PI.

Just before equation (1) in the new version of the paper, we state that "*One practical challenge is calculating the potential intensity. This should be calculated using the temperatures of the sea surface and the free troposphere under the PV anomaly aloft, but before the troposphere has appreciably warmed from surface fluxes. In practice, because the warming occurs either as the PV anomaly develops over water, or as it moves over water from land, we have no access to the sounding of the free troposphere before it has warmed up. The true potential intensity for a TC developing within the cold column under the PV anomaly is hence impossible to obtain. Nevertheless, we can estimate how cold the troposphere was before surface fluxes warmed it by using the surface pressure perturbation as a proxy for the CYCLOPs-induced warming, and assuming that the troposphere has an approximately moist adiabatic temperature profile. This is derived in the Appendix*". The first paragraph of the Appendix itself reads "*CYCLOPs form when dynamical processes create synoptic-scale cold columns marked by cutoff cyclones in the upper troposphere. The objective here is to calculate tropical cyclone potential intensity in these cold columns. The problem is that the CYCLOPs themselves, and surface heat fluxes in general, warm the columns, sometimes rapidly, diminishing the potential intensity. We want to know what the potential intensity was before this warming occurs. Here we develop a modified potential intensity using the surface pressure depression as proxy for the column heating*." We hope this is now sufficiently clear.

As to this reviewer's minor comments: Footnote 2 references a research paper by Emanuel et al., not the Emanuel textbook. Both were published in 1994, thus the confusion. We have modified the language around line 744 (now 748) to avoid the confusion noted by the reviewer.