We thank the reviewer for the thoughtful comments and, in particular, the careful examination of the equations. Below, the blue text addresses the reviewer's comments, which are repeated here for convenience in black.

Review of ACP-2010-653

Summary: This paper examines the extent to which low-entropy environmental air (in the free troposphere) invades the core of a tropical cyclone that is exposed to vertical shear. The storm- relative flow at a given altitude is conceptualized as the superposition of a point-vortex, an (off-center) mass-sink, and a uniform velocity field. The conceptual model is shown to capture the basic flow topology that is seen in a realistic hurricane simulation. The penetration of environmental air into the vortex core is examined over a wide range of vortex intensities and relative flow velocities. The potentially adverse influence of such penetration on tropical cyclone intensity is discussed.

General Comments: In my view, this paper is interesting and well written. The contents should prove useful to meteorologists who specialize in hurricane dynamics. Moreover, the limitations of the conceptual model are adequately addressed. I have only a few "specific" and "technical" comments, most of which should be viewed as requests for clarification.

Specific Comments:

S1. Discussion below Eq. (28): The authors remark that the opening between separatrices depends on U and D only, but not on Γ. Might one expect D to increase with Γ in a tropical cyclone?

Relpy to S1: No, not necessarily. If the vortex has attained maturity with no further increase in maximum intensity or size, the inflow above the boundary layer must vanish (Smith et al. 2009, QJRMS). Thus, maximum Γ does not imply maximum D.

S2. Fig. 7, caption: I am not sure that I would call a non-attractive streamline an "emerging limit cycle."

Reply to S2: Agreed. In the revised caption we now use the same wording as in the corresponding text describing this figure (section 3.4, page 28082, last paragraph). The last sentence of the revised caption now reads:
"The emerging closed streamline, reminiscent of the limit cycle found in the idealized numerical experiment (cf. Fig. 3a), is highlighted."

S3. The introduction lists 3 important mechanisms by which vertical shear can allow environmental air to weaken a tropical cyclone: ventilation of eyewall convection, erosion of the upper level warm core, and depression of inflow layer θe. It might be helpful to explain how intensity change caused by these

3 mechanisms compares to that directly caused by vertical misalignment of potential vorticity.

Reply to S3: In the introduction of RMN we have discussed the impact of different processes on intensity change in some detail. Quoting from page 3164 of that article:

"While these resiliency studies did not focus on the intensity change of a resilient vortex, it was noted that the excitation of the tilt mode weakens the azimuthal mean circulation of the vortex. During the alignment of the vortex, i.e. the decay of the tilt mode, much of the kinetic energy feeds back into the mean circulation. These changes in the strength of the mean vortex, however, are small (e.g. Reasor and Montgomery, 2001). DeMaria (1996) followed the same idea using PV superposition arguments. Using a dry 2-layer model of the troposphere he found an intensity decrease of about 10% for a very large vortex tilt (O(100km)). In contrast, Wong and Chan (2004) found, in an idealised numerical experiment with a representation of moist processes, a significant intensity decrease with very small tilt. These results suggest that the kinematic effect is not the primary intensity change mechanism."

To address the reviewer's comment, we have now added a brief mention of the kinematic effect in a footnote in line 12 of page 28061 in the current study:

"One kinematic impact on TC intensity for a sheared storm arises through vortex tilt, i.e. the vertical misalignment of the TC's potential vorticity tower. However, as discussed in the introduction of RMN, there is strong indication that this kinematic effect is not a primary intensity change mechanism."

We prefer to convey this information in a footnote over a brief discussion in the text, because the focus of the current paper is on environmental interaction and the kinematic effect is arguably of the second order for TC intensity evolution.

Technical Comments: T1. Equation 4: The notation is potentially confusing: "div" usually represents the divergence *operator*, such that div(x) = 2 (in 2D).

T1: We have changed "div" to \mathcal{D}.

T2. Equation 6: Is there a minus sign missing? If ψ is a streamfunction and v = $\partial_r \psi$, then u = $-\partial_\phi \psi / r$ in a conventional cylindrical coordinate system. If the sign is wrong here, then Eq. (10) for ψ should also be modified. If my concern is legitimate, please double check all results that are derived from Eq. (10).

T3. Equation 9: If the background flow is westerly for positive U, shouldn't ubg = U sin ϕ? That is, shouldn't the radial component of the background velocity (ubg) have its maximum positive value at ϕ = π/2 (east on the compass)?

T4. Equation 22: The authors state that if $D < 0$ (and $\Gamma > 0$), the stagnation point will shift anticyclonically from its $D = 0$ position. This seems consistent with Fig. 7. However, the azimuthal perturbation given by $\Delta = -D/\Gamma$ is positive.

T2-T4:
We greatly thank the reviewer for checking our equations carefully. The reviewer's comment have pointed out that our presentation opens up some potential for confusion. The potential confusion arises primarily because we used a rather unconventional "left-handed" polar coordinate system (azimuth increases in the clockwise direction) and the definitions $v = - \partial_r\psi$ and $u = \partial_\phi\psi/r$. Because of the left-handed coordinate convention and our desire to define cyclonic flow to be positive, we dropped the minus sign in the relation between the streamfunction and v. The explanation pertaining to the left-handed system and the drop of the minus sign was not given in the original text.

In the revised version of the manuscript we now adopt the common convention of a right-handed polar coordinate system and $v = \partial_r\psi$ and $u = -\partial_\phi\psi/r$. This change in convention leads to a change of sign in Eq. (5) and (6). The sign of the term including divergence D then changes in Eq. (10) and (25). The $U\sin\phi$-term changes sign in Eqs. (9), (19), (31), and (32); the D/Γ-term changes sign in Eqs. (20) and (22), and in the text below Eq. (20) and below Eq. (21). These changes address the reviewer's comments T2-T4. None of the results are affected.

We have also deleted "cylindrical" on page 28071, line 18.

Additional references cited:

Smith, R. K., M. T. Montgomery, and V. S. Nguyen, 2009: Tropical cyclone spin up revisited, *Q. J. R. Meteorol. Soc., 135 (642), pp. 1321-1335*