We thank this anonymous reviewer for a thoughtful reading of our manuscript and helpful comments to further improve the presentation of our results. For convenience, the reviewer's comments are here repeated in blue, our respective responses are given in black.

Response to reviewer #3

Relatively major comments:

1. In the idealized experiments in Riemer et al. (2010), the shear direction is in the same direction as the storm motion. Is this setup the same for the new experiments added in this paper? In real hurricanes, there is generally an angle (~60 – 90 degree) between the shear and storm motion direction as shown by previous studies by John Molinari and Gary Barnes. The authors' new framework will be more convinced if additional numerical experiment can be done with the wind shear direction different from the storm motion. One more set of experiment should be enough to answer this question.

The TC tracks are very similar in all experiments presented in this manuscript. Storm motion in our experimental setup is approx. 30^o – 45^o left of the environmental-shear vector, i.e. storm motion exhibits a southward component while the environmental shear profile provides an easterly steering flow. This southward component is likely due to a combination of the downshear displacement of the outflow anticyclone and the "β-drift" on the gradient of potential vorticity associated with the environmental shear flow (RMN10, Sec. 3.2.1).
We agree that gaining a better understanding of the role of storm motion in the vertical-shear problem poses an interesting and important research topic. In our experimental setup, the approximate alignment of the storm motion and the shear vector is a consequence of the uni-directional vertical-shear profile. We have chosen a uni-directional shear profile (with a wave number 1 structure) for our experiments because there is good theoretical understanding of the excitation and propagation of vortex Rossby waves (VRWs) for such a forcing profile (Reasor et al., 2004).
To obtain a storm motion that differs more significantly from the shear vector, vertical-shear profiles with a more complex vertical structure need to be considered. These complex shear profiles need to feature a change of the wind direction with height, introducing a new degree of freedom to the problem. Experiments with multi-directional shear profiles, however, would not isolate the role of storm motion. It can be expected that a much more complex set of quasi-stationary and propagating VRWs is excited and thus the tilt behavior of the TC may be modified. The excitation and propagation of VRWs forced by multi-directional shear are, to date, not well understand for idealized, dry, TC-like vortices. We therefore refrain, at this time, from performing full-physics numerical experiments of TCs in complex vertical-shear flows. Furthermore, we believe that examination of multi-directional shear profiles and associated motion effects are research projects in their own right and are well beyond the scope of the this study.

To clarify the scope of the current study we have renamed Sec. 1.2 in the introduction ("Summary of numerical experiments" → "Purpose of additional experiments") and added the following paragraphs at the beginning of this section:

"The experimental setup in RMN10 features a simplified cloud microphysics scheme, a likely overestimation of the surface exchange coefficients of momentum and enthalpy, and a very high TC intensity at the time when shear is imposed, representative for a minority of TCs in the real atmosphere only. The particular relevance of these points for RMN10's framework is discussed in more detail below. One goal of this study is to assess the robustness of RMN10's results in a more realistic and representative experimental setup.
Several environmental factors likely play a role for the evolution of TCs in vertical wind shear also. Besides the obvious importance of the shear magnitude, such factors include the vertical profiles of environmental wind speed and direction (Zeng et al., 2010, Wang 2012), and the environmental moisture and temperature profiles (cf. discussion in RM11).
Careful examination of the importance of these environmental profiles is beyond the scope of this study but constitutes an important topic for future research."
The following footnote is added at the end of the above paragraphs: "A brief discussion of the potential consequences of the environmental wind profile can be found in the authors' response to the anonymous reviewer (item 1 and 2) on the ACPD webpage."

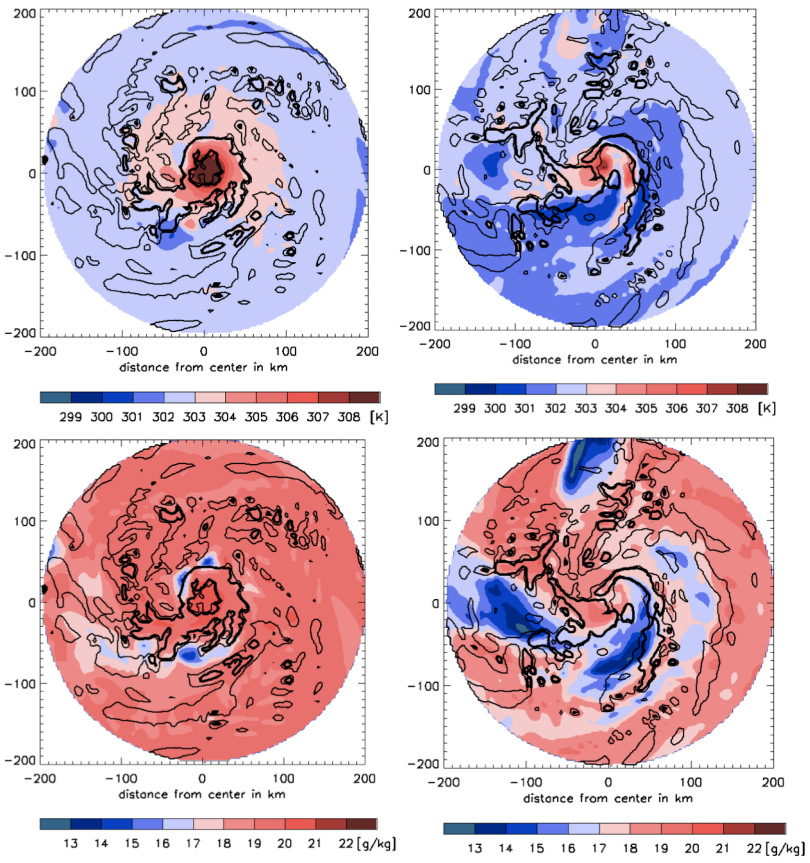
To further emphasize the likely importance of environmental factors and storm motion, the following paragraph is added after the second paragraph in the conclusions:
"Several environmental factors, such as the vertical profiles of moist entropy and the environmental winds, likely play a role for the evolution of TCs in vertical wind shear also. Furthermore, the motion vector of the TCs in all of our experiments is approx. 30^{o} – 45^{o} left of the shear vector (not shown) while TCs in the real atmosphere generally feature a more pronounced and more variable angle. The role of the environmental factors and of storm motion constitutes an important topic for future research. It is hoped that the framework presented in RMN10, RM11 and in the current study provides a helpful conceptual basis for such research."

2. The vertical shear profile the authors' used is a cosine shape shear. Recent studies by Yuqing Wang have shown that the simulated TC structure is sensitivity to the shapes of the vertical shear profiles. The authors my need to test the effect of different shapes of shear on the robustness of their framework. At least, they should mention this effect.

We now refer to work by Prof. Wang and collaborators and emphasize the potential importance of the vertical shear profile for TC evolution (see response to comment 1) above).

Apart from the excitation of a more complex set of VRW modes (see above), we note here that the ideas of flow boundaries as discussed in RM11 appear to be relevant to interpret the impact of the vertical structure of uni-directional shear.
An important kinematic aspect of vertical wind shear is that it imposes *storm-relative* flow on the TC. In the simple framework of quasi-steady and 2-dimensional flow, the interaction of the TC's vortical circulation with the storm-relative flow gives rise to a "dividing streamline" that approximates the boundary up to which environmental low-θ_e air can approach the TC (for details see RM11).
The conceptual framework presented in the current manuscript (and in RMN10) considers the regime of resilient TCs. In this regime, the dividing streamlines lies outside of the inner-core convection at all levels, protecting it from direct intrusion of environmental low-θ_e air (RM11, Fig. 4; consistent with the results from an analogue model by Smith et al. 2000).
For a different vertical structure of the wind shear, the location of the dividing streamline at different vertical levels will likely be modified. For example, for a "top-heavy" shear profile in which vertical shear is concentrated at upper-levels, it can be expected that upper-level relative flow increases and a regime change may occur at the point at which the "dividing streamline" no longer encompasses the inner core: the TC is then no longer resilient.
We regard this regime transition as a worthwhile topic for future research. Within the scope of the current study, however, we do not believe that this topic can be addressed in sufficient detail.

3. The authors only showed θ_e and θ_e depression fields when presenting their most important results to confirm the thermodynamic modification by wind shear. It will be complete to add plots of temperature and humidity. Is the θ_e depression caused



by temperature or humidity modification by the wind shear? How do the convective downdrafts influence the temperature or humidity? This type of discussion will clearly connect the authors' framework with surface flux transfer processes.

It is of interest to examine the temperature and humidity fields for completeness. In Fig. 1, potential temperature and water vapor mixing ratio is presented at 5h after shear is imposed, i.e. at the same time as Fig. 7 in the manuscript. For comparison, the respective distributions are shown just before shear is imposed, at 0h, also. The fields are shown for the ICE_{68} experiment. Similar figures for the RMN_{68} experiment can be found in our response to the anonymous reviewer of RMN10 (item 9) on the ACPD webpage: www.atmos-chem-phys-discuss.net/9/10711/2009/acpd-9-10711-2009-discussion.html.

The depression in potential temperature is approx. 2-3 K and approx. 4-5 g/kg in the mixing ration. These values have also been found in the RMN_{68} experiment. Within the framework of Emanuel's Carnot cycle theory, and recent extensions by Tang and Emanuel (2010), it is really just the moist entropy of the air masses that matters. We thus prefer to minimize the emphasis on the individual contributions to the θ_e depression. Information on the individual contributions is added to the manuscript by the following footnote (after "cyclonically inward" on page 7011):

"Approximately, the θ_e depression is associated with a depression in the water vapor mixing ratio by 4-5 gkg^{-1} and a depression in the potential temperature by 2-3 K. The spatial distribution of these fields at the same time as depicted in Fig. 7 is presented for ICE_{68} and RMN_{68} in the authors' response to the anonymous reviewer (item 3) on the ACPD webpage."

4. It is a really nice idea to calculate the timescale for vortex spindown in section 4.2. Equation (6) includes an important parmater h. The authors should specific how h is defined. Recent study by Jun Zhang et al. (2011MWR) showed that boundary layer height in hurricanes can be defined differently. Different definitions of boundary layer top would give different height scales that affect timescale calculation in Eq. (6).

Thank you, and we agree that the definition of the TC boundary layer height is an important issue.

In Eliassen and Lystad's theory, however, the important parameter is the vortex depth above the boundary layer, (H – h) in Eq. (6). Evidently from this equation, the spin-down time-scale depends linearly on this parameter. We have estimated (H – h) to be 10 km in our experimental setup. Uncertainties in the determination of the boundary layer height may be of order 500 m, or 5% of the vortex depth above the boundary layer. This translates to uncertainties of 5% of the spin-down time-scale (from the estimate of the depth of the boundary layer only). Such a small potential variation of the spin-down time-scale does not impact the interpretation of our results.

5. The authors claimed that the decrease of intensity is mainly due to the decrease of low-level θ_e after shear is introduced. In Figs. 7 and 8, it is shown that the decrease of θ_e is only in a relatively small area compared to the whole inner core region. I am wondering if this small area low θ_e air is able to shut down the convection. It is likely the integrated downward θ_e flux is an important parameter to look at as well.

Inspecting Fig. 8, we cannot concur with the reviewer that the reduction of inflow layer θ_e is limited to a "relatively small" area. Furthermore, and probably more importantly, it is explicitly argued in the discussion of Fig. 8. (Sec. 3.6) that it is the reduction of the θ_e values of air parcel rising in the inner-core (eyewall) convection that leads to the observed intensity decrease. In RMN10, we have documented reduction of eyewall θ_e values by several degrees *in the azimuthal mean*. In the current study, we present as an additional diagnostic the horizontal distribution of vertically averaged θ_e values underneath the inner-core convection (the open, asymmetric eyewall). The reduction of eyewall θ_e constitutes a frustration of the thermodynamic (Carnot) cycle of the TC and, based on steady-state, axisymmetric theory, leads to a reduction of storm intensity (Tang and Emanuel, 2010).

In that sense, the reduction of θ_e values in the inner-core convection is a result of the "integrated" downward flux minus the replenishment of θ_e values while the air is spiraling towards the inner-core updrafts. The replenishment itself is not considered in detail in this study.

The azimuthally integrated evolution of DFX has been presented in a radial-time plot in RMN10 (Fig. 13). In the current study, however, it is argued in the last paragraph of Sec. 3.6. that the azimuthal location of the inflow layer θ_e depression may play an important role also because of the pronounced wave-number 1 asymmetry of the inflow (depicted in Fig. 8 of the current manuscript). This point is exemplified and further discussed in Sec. 4.1 in the manuscript.

6. Is the depression of inflow layer θ_e (in Figs. 7 and 8) sensitive to the time window you chose in the analyses? How about the θ_e depression calculated from 1 to 3 h after the shear is introduced or time window between 4 and 7 h? When did the θ_e depression start to happen? Since θ_e in the boundary layer varies with intensity, it would be interesting to scale the θ_e change by the intensity change.

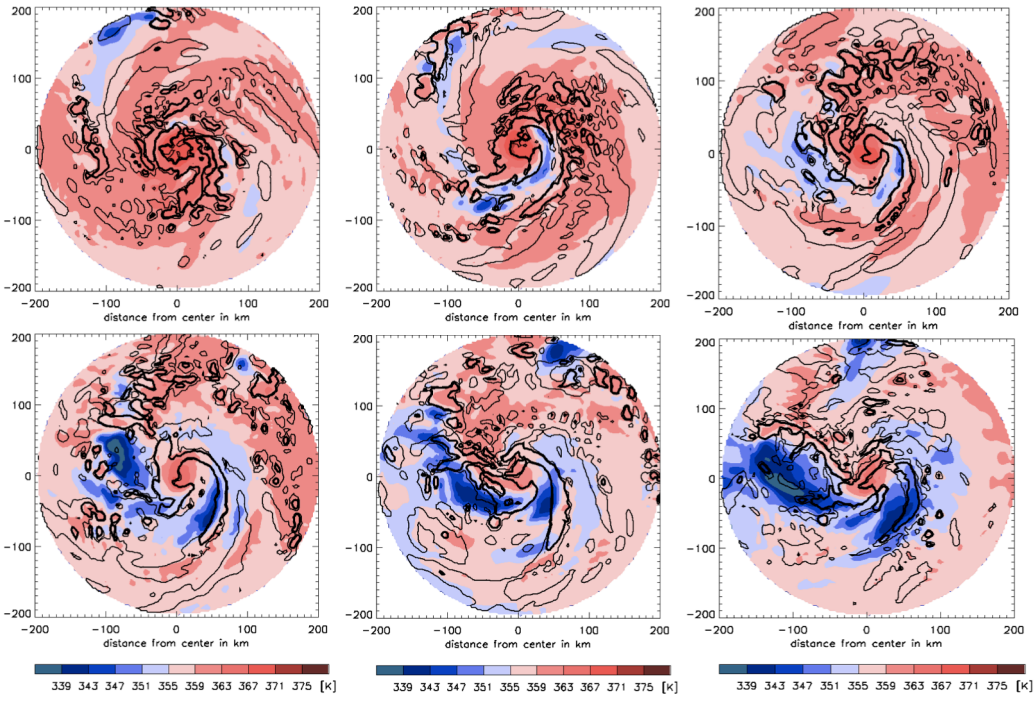


Fig. 2: As Fig. 7 in the manuscript but for ICE_{68} at, from left to right, 0h, 2h, 4h (upper panel)

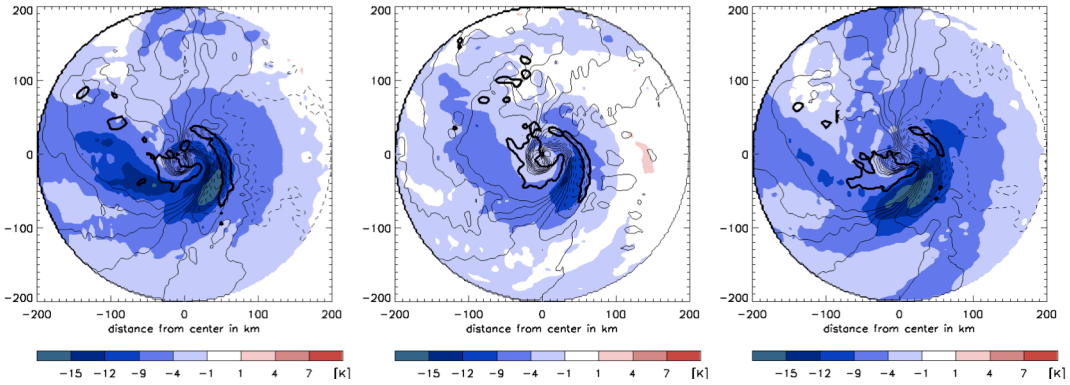


Fig. 3: As Fig. 8 in the manuscript, but for ICE_{68} only and averaged over the 6 h time period from 7-12 h (left), 1-6 h (middle), and 10-15 h (right panel). In the manuscript, the time period from 4-9 h is depicted.

Thank you. It has been our oversight to inform the reader that the snap shots depicted in Fig. 7 in the manuscript are representative for the early time period after shear is imposed. In all experiments, depressed θ_e values occur 2-3 h after shear is imposed. For illustration, we document here the evolution of θ_e for ICE68 from 0-10 h every other hour (Fig. 2 above). The θ_e field at times 5 h and 7 h can be found in the manuscript (Figs. 7e) and 11a)). While the amplitude of the θ_e depression varies with time, the general pattern of the depression is very similar at all times depicted.
After the first sentence in Sec. 3.6.1 we have added:
"This time is representative for the early part of the interaction after the θ_e depression has formed 2 - 3 h after shear is imposed."

For Fig. 8 in the manuscript, we have chosen to average over the time period of 4-9 h to capture the general pattern of the θ_e depression early during shear interaction and *after* the depression is established in all experiments. Averaging over a time period from 7-12 h (Fig. 3a) above) yields a very similar result (cf. with Fig. 7e) in the manuscript). As can be expected from Fig. 2 above, averaging over an earlier time period (1-6 h, Fig, 3b)) yields a less pronounced θ_e depression. Averaging from 10-15 h (Fig. 3c)) yields a similar pattern as in Fig. 3a) and Fig. 7e) in the manuscript, namely a maximum of θ_e depression (with very similar values) to the downshear-left and upshear, and the cyclonic wrapping of lower θ_e values around the center. At this later time, however, the secondary maximum in the downshear quadrant is less pronounced, indicating that this feature is not a persistent one in this experiment (c.f. discussion in Sec. 4.1 in the manuscript).

We are not sure if we interpret the reviewer's statement in the last sentence correctly. Discussing Fig. 8 in Sec. 3.6 of the manuscript, we do show that the θ_e depression underneath the eyewall and the intensity decrease are related and that, in particular, the intensity change scales with the decrease of θ_e underneath the eyewall.

Minor comments:

1. Fig. 10 caption, should be CBLAST54/CBLAST68?

Thank you for noting this typo. As indicated in the figure itself, the caption should read RMN54/CBLASR54. Caption corrected.

2. First line in section 3.7, low - θ_e ?

Thank you for noting this typo. Corrected.

3. Eq. 4. The definition of the θ_e flux needs to be defined more clearly. The authors should distinguish this flux with the standard turbulence flux.

The deviations from the azimuthal mean, θ_e' and w', are obviously quantities that are resolved by the numerical model. In that sense, the quantity DFX does not represent a turbulent flux. We have discussed the relation between DFX and the more commonly used flux term θ_e'w' in some detail in RMN10. We have added the following footnote to clarify this relation in the current manuscript also:
"Our downward-flux metric DFX is virtually identical to that part of the flux term θ_e'w' for which w' is negative (see RMN10, Sec. 4.2.1 for details)."

4. Since the model the authors used is not coupled with ocean model, ocean feedback to the asymmetry of surface fluxes may influence the authors' result. It is worthwhile to mention the limitation of the authors' results with lack of ocean response induced sea surface temperature cooling effect.

We now note the limitation of the experiments with respect to ocean feedback in the model section (Sec. 2.1, after the first sentence in the second paragraph we add:
"The model does not include ocean feedback. The time-invariant SST is set to 28.5^{o}C."

5. The authors should also mention the storm motion effect may contribute to the boundary layer thermodynamics asymmetry.

The potentially important role of storm motion is now emphasized in the conclusions (see response to "Relatively major comment" 1 above).

References:

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