**Atmospheric
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Discussions**

Interactive comment on "Clarification on the generation of absolute and potential vorticity in mesoscale convective vortices" *by* R. J. Conzemius and M. T. Montgomery

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General Comments:

COMMENT: One aspect of the analysis that could be elaborated concerns the origin of positive PV anomaly in the stratiform region, as shown in Figure 4 and discussed in section 4.3. Three positive PV centers of large magnitude that originate within the line of convection are identified and tracked as they move rearward into the stratiform region. Although the authors do not explain how these centers were tracked, their analysis suggests that the positive PV of the stratiform region is sourced from the line of convection. This is an important point that deserves a more detailed examination,

perhaps as part of a future investigation. The following are a few additional questions that might have been considered during this analysis. What proportion of the total PV in the stratiform region was generated locally in the stratiform region itself, and what proportion was generated in the line of convection and then transported rearward? Do the fluid particles moving through line undergo significant ascent/descent en route to the stratiform region? What happens to the negative PV centers that are formed in the line of convection as part of the storm-scale horizontal dipoles? Do they also arrive in the stratiform region as weaker positive anomalies?

RESPONSE: We have considered some of these points. The PV anomalies have a large absolute value as they are formed in the convective region then weaken in absolute value as they merge and move rearward relative to the advancing convective region (model diffusion may also play a role in smearing them out). In the end, they arrive in the stratiform region as weaker positive anomalies as Fig. 4 shows. The negative centers disappear, perhaps as a result of active processes in the stratiform region, or perhaps the model diffusion scheme smoothes them.

COMMENT: I note that it is stated in the introduction that some of the issues mentioned in the preceding paragraph would be treated by way of a "cursory investigation", which is an accurate characterization of what was presented via Figure 4. Furthermore, in the last three sentences of the conclusions section the authors concede that these issues deserve further consideration, and that the BAMEX dataset could be useful. I would also encourage the authors to perform additional calculations in their simulations. For example, I recommend performing a trajectory analysis in which the PV histories of fluid particles moving through the line of convection as well as those originating in the stratiform region itself are examined. Figure 4 does not offer a full 3-d picture, nor does it make it absolutely clear that most of the positive anomaly is generated as particles move through the line of convection. If most of the positive anomaly is indeed sourced from the line of convection, then the accurate simulation of the PV structure in the stratiform region would rely upon the accurate simulation of the transport of mass

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in, around, and through the line of convection. Do we really require such detail to accurately simulate the system-scale MCV?

RESPONSE: Figure 4 offers a simple, qualitative illustration of the processes in our high CAPE simulation. Figure 3 shows the quantitative analysis of the heating rate profile, from which one can see that the PV production occurs from both processes. The convection is clearly the largest contributor early in the simulation, and then the stratiform region contributes more strongly later on. We can refer to Figure 3 a bit more in the discussion of Figure 4. We can consider a trajectory analysis, but Figs. 3 and 4 make a simple but clear statement. We also think the trajectory analysis, if conducted, would be a sufficiently large endeavor for a separate paper.

Specific comments

COMMENT: - It would be helpful if the authors could comment on how representative the two MCVs examined in this paper are for the range of MCVs encountered in nature. Related to this point, the authors should probably reference Kirk's (2007, MWR) paper in which a phase-plot technique is used to visualize the different possible evolutionary paths of MCVs. The MCV examined in the present investigation would appear to be of the type characterized by Kirk's method as being led by heating. Kirk also identifies an alternative evolutionary path that is dominated by tilting. ref: Kirk, J.R., 2007: A Phase-Plot Method for Diagnosing Vorticity Concentration Mechanisms in Mesoscale Convective Vortices. Mon. Wea. Rev., 135, 801–820.

RESPONSE: It's difficult to place our results in the light of Kirk (2007) because of the different analysis techniques used. In particular, our analysis area was much larger (on the order of 65,000 km2 for one example I looked at, versus 5184 km2 in Kirk), and within our analysis period of approximately four hours in length, we do not move the boxes. This appears to have caused the vorticity budget to behave somewhat differently. If we tried the phase plot technique, there are levels where the final minus initial vorticity would pass through zero, causing some erratic behavior in Kirk's Equa-

tion 12 and resulting in hard to plot profiles. Perhaps the technique works better when the analysis area is more focused on the MCV itself and moves with the MCV. Since we were looking at an interaction of scales for these simulations (see Conzemius et al. 2007), we happened to choose larger analysis areas. The MCV in the control experiment developed in an environment of initially zero background CAPE. Meridional transport of moisture and sensible heat increased the CAPE to nearly 700 J/kg. The MCS in the control simulation initiated within a developing baroclinic system, and since the CAPE was small, the MCV dynamics were dominated by low-level convergence (Kirk's HF term is strong at low-levels) as is seen in Fig. 1 of our paper. Tilting just does not seem to dominate during any phase. The background shear is consistent with the shear in Kirk's J21 and M6 cases. At upper levels, it is difficult to compare our paper with Kirk because the vorticity tendency within our fixed boxes is mostly negative in the upper troposphere, although this vorticity change is dominated by tilting processes near 500 mb. It is hard to attribute the overall intensification to tilting, and due to the less convective nature of this MCS compared to most, its phase plot would probably be dominated by synoptic scale, low-level stretching.

The synoptic scale processes may also be considered strong for the CAPE simulation because it also developed within an intensifying baroclinic wave. However, it can be thought that the CAPE-case MCV could be dominated by heating. This appears to be the case early in the CAPE simulation (see Figs. 2a and 3d) where strong mid-level heating occurs at around 500mb. However, once again, the net vorticity change crosses zero near the level of maximum heating, making the comparison difficult without a full calculation of Kirk's terms.

COMMENT: - On page 7535, lines 1-3, it is stated that "When tilting occurs in conjunction with a convective updraft, the vertical gradients of diabatic heating produce a concentration of PV substance". I don't think this statement is accurate. PV is generated when a heating occurs that has a gradient in the direction of the background absolute vorticity vector.

On the mesoscale, heating has a vertical gradient directed against the background planetary vorticity, and hence vertical dipoles are generated. On the storm scale, diabatic heating has a large horizontal gradient in the direction of the background horizontal (shear) vorticity. This is why the PV accompanying the convective storms has a horizontal dipole structure. The horizontal PV dipole is not a consequence of tilting. The subsequent generation of relative vorticity within this horizontal PV dipole is the consequence of tilting.

RESPONSE: We will revise our statement because it seems to throw two separate processes into one pot. It is not consistent with the definition of PV we used, and your description is better than the one we originally used. Thanks for suggesting this correction.

COMMENT: The MCS is initiated by inserting a warm-core, low-level vortex, with zero PV onto the background state. To what extent does this influence the evolution of vorticity later in the simulation? Although this structure has zero PV, you've effectively increased the background vertical vorticity against which diabatic heating generates anomalous PV.

RESPONSE: The discussion of the effect of the initial vortex is included in an additional paper that is currently in the review process. In essence, the effect of the initial vortex on generating MCV PV is minor because the initial vortex only ends up exciting moist baroclinic waves, which subsequently initiate an MCS. This MCS is not collocated with the initial vortex. We can add Conzemius et al. (2007) as a discussion point here, because we discussed the initial vortex impact extensively in our earlier paper.

COMMENT: How was diabatic heating computed in the simulations? Is it a trivial calculation? In my experience, it is not always so trivial.

RESPONSE The diabatic heating was computed as a residual as it was in Kirk 2007. We will make sure that it is noted in the final version.

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COMMENT: All of the analysis presented in the paper is performed on the finest grid. If data is also available in the outer coarser domains, then it would be interesting to see if the budget calculations are similar to those in the convection-permitting domain. I'd be interested in knowing to what extent the results of the present investigation are influenced by the parameterization of convection. Such an analysis might also clarify which storm scale processes are most influential to the evolution of system-scale vorticity. If we need to accurately simulate the horizontal transport of mass in the storm environment, then would simulations relying upon a convective parameterization behave very differently from those that explicitly permit convection?

RESPONSE: It is an interesting question. Because of a question from David Raymond, we will at least examine the budget on the coarser domains (time resolution is not as good on the coarser domain). However, because of the two-way nesting, in essence, the MCV on the coarser (parameterized convection) grids is also convection-permitting. The topic seems to be a rather large one and would be best addressed in a separate paper. We think it would require several different simulations to be able to arrive at general results.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 7531, 2009.

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