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Atmospheric Chemistry and Physics 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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Summary remarks:

The present article attempts to clarify the basic mechanism that produces MCVs through examination of an idealized simulation using the MM5 model, with nesting sufficient to use explicit representation of convection. The study notes that the horizontal flux of absolute vorticity is the dominant term, consistent with previous studies. Positive aspects of this study are that the vorticity budget is phrased in the flux form of the vorticity equation, and the authors compare the left-hand-side integrated time tendency to the sum of the terms on the right-hand-side. Thus we can see that the

budget balances. Far too few studies in the literature go through the full budget computations. Finally, the paper is concise and readable and the figures are generally clear and readable.

General comments:

1. In the Introduction, I feel that the authors are making a bit too much of an issue regarding apparent confusion on the part of previous studies and the relative importance of different mechanisms of producing a mesoscale circulation. One point involves the issue of the ultimate source of vorticity versus the processes responsible for the mature circulation. There is little contradiction in the literature regarding the statement that tilting initiates the vortex, but stretching intensifies it. Yes, there are a few studies that implicate tilting in the formation of the mesoscale gyre, but these are either from observations alone (e.g. Brandes 1990), or based on very coarse-resolution simulations, perhaps averaged over domains too small (e.g. Kirk 2007).

A second point is that the quoted studies were conducted in very different flow situations, ranging from highly idealized (Hertenstein and Schubert 1991), to idealized full-physics simulations (e.g. Skamarock et al. 2004) to simulations of a real case (Davis and Trier 2002). It is not apparent that these results are in conflict with each other, or even ambiguous. Davis and Weisman (1994) conducted simulations with and without background rotation. Without background rotation, a counter-rotating vortex pair developed. With no initial vertical component of voriticty, stretching could not be the ultimate source. It had to be tilting. With rotation it was clear that a similar process of tilting occurred within the first 3-4 hours, but that the background rotation became the dominant factor after that (e.g. on a time scale of 1/f). The issue is not one of tilting VS. stretching in that case, but one of tilting followed by stretching. I believe the Cram et al. study found the same result. The present study provides an additional datum in a different simulation setup.

Finally, the authors may wish to compare results to the recently published Davis and

Galarneau (2009, JAS) article, which uses a similar budget approach to diagnose the evolution of MCVs in simulations of two BAMEX cases. This could easily be done in the conclusions.

2. The separation of heating regions into convective and stratiform is potentially useful and informative, but the basis for the separation is arbitrary and there are other issues. First of all, it is not clear that this kind of "convection permitting" simulation produces a realistic stratiform region because most of the simulations run at this resolution that are published in the literature do not. This fact could be important in that a greater fraction of the precipitation would appear as "convective" than in nature. Of course, there is no "nature" comparison in the present case, but I suspect that there is a model bias in the physical representation of convection.

Second, a vertical velocity of 1 m/s may not indicate buoyant convection. This speed is of the order expected in, for instance, strong frontal circulations and probably less than occurs in the sloping front-to-rear updraft of an MCS. This front-to-rear circulation is largely hydrostatic and consists of relatively small vertical accelerations. Is this circulation convective or is it stratiform? I think this is difficult to answer. I would like to see some calculations of the sensitivity of the results to the arbitrary threshold of 1 m/s. I am perfectly willing to believe the overall conclusion about the importance of the convective heating, but there should be more care to appropriately define how heating is partitioned because it is such an important part of the paper.

Third, the authors themselves seem to waffle in the last two paragraphs of the Introduction about whether there is a physical distinction between the convective and stratiform regions. I think this is evidence that the issue is not quite clear in their minds, either.

3. The authors include a temperature gradient to balance the vertical shear. How important is the effect of allowing meridional gradients of temperature and moisture prior apart from contributing to the large-scale baroclinic development? Although the CAPE in the center of the channel is 2000 J/kg, what is it to the south, and how does

the CAPE of the inflow air evolve during the simulation? Is 2000 J/kg a representative value of CAPE throughout the simulation?

4. Some other model details are not mentioned. They probably appear in the Conzemius et al. (2007) paper, but some could be added here. Was there a diurnal cycle? Was there a stratosphere (e.g. departure from a true "Eady" basic state)? What were the top, bottom and lateral boundary conditions? True, this is redundant with the previous paper, and the reader can look it up, but adding a paragraph would not compromise the readability of the present article and could make it more self-contained.

5. Was the box over which the budget was computed fixed in location and size during each averaging period? It appears so, but I cannot find where it is stated. A statement about this would be good to add. Are the boxes different sizes in different averaging periods? If so, how can we compare tendencies from one time to the next, because it appears that what is presented is the vorticity tendency averaged over the box?

It would be helpful to show the locations of the boxes relative to the convective system.

Also, I believe that (2) and (3) should read "dot hat(n) dl", not "cross hat(n) dl".

6. The section on tracking PV features does not seem to add much to the paper. First of all, these features are not conserved because PV is not conserved. Second, there is no evidence provided that their circulation is an important contribution to the overall circulation of the MCV. It would seem that this section should be expanded to allow quantification of the findings, or it should be dropped altogether.

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