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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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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COMMENT 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

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simulations, perhaps averaged over domains too small (e.g. Kirk 2007).

RESPONSE: We have made some revisions to the introduction so as not to overdo the issue. In particular, we cannot take the Weisman and Davis results to be a blanket statement for all MCVs. In particular, our MM5 setup was aimed at simulating both synoptic and mesoscale processes. That, in addition to a larger analysis area (although in our initial analysis periods, the box is smaller because the vortex is smaller), can explain the dominance of stretching in our case. Our main point is that the tilting does not explain the formation in every case. At the very least, the analyses represent an additional confirmation that the MCV vorticity is produced primarily by flux convergence of planetary and relative vorticity. Our tilting term is also positive at low levels during most of the analysis periods chosen. Perhaps a more significant corollary of our work is that the convective heating can be thought of as a significant contributor to the PV in the MCV. This is an important result that has not been highlighted in previous studies.

COMMENT: 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.

RESPONSE: Indeed, the present study provides an additional datum but does not have to explain every case. The points of view could easily be reconciled by noting that the tilting dominates on the small scales as in Cram et al. (it could be individual convective cells or just a small cluster of cells that make up an MCS), so tilting may dominate early in the MCS lifecycle. Note, however, that we also observe a positive tilting contribution to the vorticity budget during most of our analysis periods.

It is therefore possible that the analyses herein do not prove “the tilting THEN stretching argument” because any dominance by the tilting process may occur over a time interval shorter than our integration time. A point that we would like to make is that the results show that the stretching process can be dominant during the first few hours of the MCS life cycle in addition to later times.

There are many differences among the studies cited in the introduction, so a blanket statement cannot be made for all of them. In our simulations (compared to idealized ones such as Weisman and Davis), baroclinic processes are active. The integration over a box that is a bit larger in the east-west direction than the MCV itself (in order to encompass the area of positive relative vorticity over the entire integration time) may include some flux convergence of absolute vorticity at synoptic scales as well as at the MCV scale.

COMMENT: 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.

RESPONSE: Thanks for pointing out this article. We have referenced it in the revised paper.

COMMENT: 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.

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RESPONSE: Clearly, there is not always a particularly sharp distinction between what part of an MCS can be considered convective and what can be considered stratiform. The distinction can be somewhat subjective, although there are a number of techniques to distinguish them that are well-accepted. The statements in the last two paragraphs were meant to convey the thought that both regions are part of the same process, not to indicate confusion. We will strive to make this clearer.

COMMENT 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?

RESPONSE: CAPE reaches up to 4000 J/kg in the southernmost portion of the domain, but those values never reach the bulk of the MCS. 2000 J/kg is representative of the CAPE during the "CAPE" simulation. The value of CAPE during the control simulation ranges from near zero to 700 J/kg, and the larger CAPE values are associated with meridional transport of moisture and heat. Thus, the meridional gradients, as well as the values of CAPE, do play a role in the dynamics of the MCS.

COMMENT 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.

RESPONSE: There was no diurnal cycle, so the effects of diurnal heating and cooling do not reveal themselves in these simulations. There was a stratosphere in the basic

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state. We will repeat the relevant details in the methods section because there is enough room in the paper to include the information. Thanks for this suggestion.

COMMENT 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?

RESPONSE: The box was fixed in size during any single averaging period shown in the budget analyses. The boxes were chosen to encompass the area of cyclonic vorticity in the MCS. As the MCS grew upscale during the simulation, the boxes, correspondingly, were larger in the later time periods of analysis. To the extent possible, we chose the box to surround the cyclonic part of the MCS, so that we could calculate the area average vorticity of the MCV. We can add a figure to indicate the placement of the boxes.

COMMENT: 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".

RESPONSE: We will make sure that the typographical errors do not appear in the equations in the final version of the paper. We did not catch them during the draft stage.

COMMENT 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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RESPONSE: The ideas we point out are better revealed in animations, but we wanted to show figures in the article in order to illustrate the process behind the calculations that appear in earlier sections. The positive PV anomalies are produced at the leading convective line and are carried back by the front to rear flow (relative to the MCS). The figure was meant to simply provide a qualitative description of the process in the simulations.

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