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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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In this paper the authors analyze simulations of the development of mesoscale convective vortices in the context of a mesoscale convective system embedded in a larger-scale baroclinic system. The main point is that the flux form of the vorticity equation is a much better diagnostic tool than the advective form, the latter of which has been used extensively in the past. The main point is that the so-called tilting and vertical advection terms in the advective form undergo major cancellation, causing serious problems with diagnostic calculations and their interpretation. I strongly agree with this conclusion; the use of the advective form of the vorticity equation when the much simpler flux form

is available (and has been in the literature at least since the papers of Haynes and McIntyre in 1987 and 1990) is nothing short of perverse. The advective form in particular encourages the misconception that the vertical component of vorticity can be advected vertically, a conclusion that careful examination of this equation refutes.

The authors also attempt to distinguish the vorticity balances in convective and stratiform rain areas, and it is in this effort that they run into trouble. They attempt to distinguish convective and stratiform regions by the 600 hPa vertical wind, whereas the more common method to identify stratiform rain areas is to look for moderate rainfall rate areas with little convective-scale spatial variability (see many papers by Bob Houze and colleagues). The vertical mass flux profiles they derive for convective and stratiform regions do not resemble those thought to be characteristic of these situations, suggesting that their distinguishing criterion is flawed. One could imagine, for example, strong but shallow convective regions producing a lot of rain, but with weak vertical velocity at 600 hPa. As the distinction between convective and stratiform rain is somewhat artificial (these simply represent two different phases of the convective process), it is risky to invoke a criterion distinguishing the two that is different from the normally used criteria.

The authors are motivated to distinguish convective from stratiform areas because numerous papers attribute the development of mesoscale convective vortices to stratiform precipitation only, ignoring the contribution of convective precipitation. A much simpler way to refute this assertion is to note simply that the contributions to precipitation from convective and stratiform regions are roughly equal (again, see many papers by Houze and colleagues), implying roughly equal amounts of vorticity convergence, albeit at different levels in the atmosphere. Perhaps the division between convective and stratiform regions can be avoided completely in this paper – it a way, it just distracts from the main point about the advantages of the flux form of the vorticity equation and the associated conclusions.

As the authors emphasize, the flux form of the vorticity equation demonstrates that only two effects can change the vertical component of the absolute vorticity at a point, horizontal vorticity convergence and the "tilting-like" term (in reality, momentum forcing due to vertical advection of horizontal wind in a region of shear). The latter produces positive-negative vortex pairs, usually on a small scale. (See Raymond, 1992: QJ, 118, 987-1015 for a potential vorticity perspective on this.) As a result, the circulation integral method is quite sensitive to small changes in the circulation path if it cuts through convectively active regions; bisecting a sheared updraft, for instance, puts one of the vortex pairs inside the loop and the other outside, resulting in significantly different results from those obtained by shifting the circulation path to one side or the other of the updraft. The authors note that this sensitivity causes problems in their analysis.

There are two possible solutions to this problem: (1) Shift the circulation loop further out so that it encompasses all significant convection; (2) apply smoothing to the vorticity field before computing the circulation, so that the little vortex pairs resulting from localized updrafts are annihilated. I suggest that the authors try one or both of these techniques. If the loop cannot be expanded enough to encompass all convection, then at least the smoothing will eliminate the small-scale noise in a physically defensible way and allow the calculation of the true mesoscale effects of tilting.

Specific comments:

1. Page 7537, just above section 3: How is the "isolated vortex" different from the other case? The authors shouldn't assume that the reader has read the earlier paper by Conzemius et al.

2. Page 7538, equation (2): The second cross product should be a dot product instead.

3. Page 7539, 3 lines up from section 3.2: "low-levels" –> "low levels" (this is a noun, not an adjective, in the present context).

4. Page 7540, second line of section 3.3: "Since the potential vorticity time tendency..." The terminology can get awkward here; what is really meant is the time tendency of "potential vorticity substance density" (ugh!) or the time tendency of potential vorticity times density (marginally better!).

5. Page 7544, second paragraph: "...convective contributions are relatively large at first but acquiesce to stratiform contributions..." – "acquiesce" doesn't sound right – how about "evolve" or "change" or "convert"?

6. Page 7546, paragraph 3 of Conclusions: "Some significant difference" –> "Some significant differences".

7. Page 7547, last line of text: The mention of "BAMEX" without a reference is not appropriate. Either provide a reference and explain the acronym or omit this sentence completely.