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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A numerical study of convection in rainbands of Typhoon Morakot (2009) with extreme rainfall: roles of pressure perturbations with low-level wind maxima" *by* C.-C. Wang et al.

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This study uses dynamical diagnosis of numerical model simulations to understand convective processes in the rainbands of Typhoon Morakot (2009). In particular, the authors focus on the behavior of updrafts in the strong low-level vertical wind shear in the rainbands. They conclude that rather than being forced by lift along a cold pool, the location of ascent associated with the updraft/shear configuration is responsible for the back-building of convection in the rainbands.

The topic of the manuscript and the methods used are generally sound, and the paper is clearly written. The study did raise several questions for me that I think should be more thoroughly addressed in the manuscript, but I believe these bigger-picture issues still only require minor revisions. Therefore, I recommend that the manuscript be accepted if these minor revisions are sufficiently considered by the authors. I don't need to see the manuscript again, but would be willing to review a revised version of the editor prefers it.

"Big picture" comments:

1) This study uses diagnostics originally developed for understanding supercell thunderstorms to look at convection in the outer rainbands of a tropical cyclone. In fact, some past studies have shown that this convection is indeed supercellular, e.g., Eastin and Link (2009); Morin and Parker (2011); see also the references therein. I think it would be useful to establish whether the convection in Morakot was consistent with supercell dynamics, or whether these convective cells were non-supercellular but potentially still explained by these diagnostic methods.

2) One thing that wasn't totally clear to me in the manuscript was whether the "low-level jet" (LLJ) being discussed here was something external to the tropical cyclone, or the flow associated with the TC itself (or perhaps a bit of both). Perhaps the findings here could be compared to the conceptual model of Hence and Houze (2008) for rainbands in the "secondary horizontal wind maximum"?

3) Although from my reading of the manuscript, the use of the pressure perturbation diagnostics is interesting and applicable to this case, the manuscript may be more convincing if at least one more time is shown, such as 0645 or 0650 UTC when we can see how the detailed diagnostics shown for 0630 UTC related to the subsequent back-building convection. I don't think it's necessary to show 4 or 5 complete figures for the later time, maybe just one figure illustrating how things evolved after this time.

4) There is some interesting 3-dimensional structure to the low-level vertical velocity

field to the west of the updraft, as seen in Figs. 11 and 12, but this isn't really addressed in the subsequent discussion. (I'm referring to the "line" of upward motion that extends to the south-southwest of the main updraft.) Is this ascent explained also by the pressure perturbation diagnostics, or is something else going on there? And is it important to the back-building convection?

References

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