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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Relationship between level of neutral buoyancy and dual-Doppler observed mass detrainment levels in deep convection" *by* G. L. Mullendore et al.

Anonymous Referee #2

Received and published: 10 October 2012

GENERAL COMMENTS:

This study investigates and quantifies the variability associated with predicting where the maximum storm outflow level occurs from environmental soundings. In doing so, it extends the single case study by the two lead authors in their 2009 paper. Here, the same methodology is used to examine the temporal evolution of outflow from 9 different mid-latitude cases (using dual-Doppler radar data) and compares them with the corresponding level of neutral buoyancy (LNB) as calculated via various methods and sources.

Their results for mid-latitude storms certainly complement those found by Takahashi

and Luo (2012) - TL2012 - and I certainly invite the authors to refer to their GRL paper and discuss. The TL2012 paper used CloudSat cloud-profiling radar data to conduct a near-global survey of of tropical storms (30S-30N) over a 2.5-year period. One of the more important findings is that there are land-ocean contrasts in the level of maximum convective outflow, with observations from land storms being similar in nature to what is found in this paper.

Overall, I find the results of this study to be interesting given the different storm classifications and certainly useful enough to warrant publication. The results should help guide convective parameterization schemes in terms of identifying the level where deep convective storms are expected to mostly detrain, depending on storm morphology. I suggest minor revisions given by the specific comments below.

SPECIFIC COMMENTS:

P21269, Lines 1-2: Try be more precise and include an approximate timescale (e.g. 30-40mins) as only the most intense storms are able to do transport BL mass into the UTLS in such a short time frame.

P21270, Line 26: Why were different horizontal resolutions used in the mapping? How would this affect the calculation of the total vertical mass divergence profiles and hence the final LMD values used for comparisons between different storm types?

P21272, Lines 25-27: It is hard to tell in Fig. 1 which of the four plus signs is taken to be the most representative sounding for the 29 June 2000 case, unless Table 2 is referred to. I suggest color coding the most representative sounding plus sign. Also, would a plot of maximum reflectivity be better to get an overall sense of how large the storm system is?

I agree with Comment 4 from Referee 1 (Z J Luo) that Fig. 1 is not enough. It would be useful to also include vertical cross-sections of this storm (see later comment) and perhaps a few more of the cases being studied (e.g., contrast this supercell storm with a convective squall line case.)

P21273, Line 2: Is there a reason why 250 m is chosen instead of 0.5 km for the vertical grid spacing (to match radar data) when doing cubic interpolation for the NARR soundings?

P21274, Lines 2-4: When and at what level did you assume that ice forms? Supercooled-liquid water droplets can exist down to about -38 or -39C before homogeneous nucleation occurs in the absence of ice nuclei.

P21275, Line 6-8: While it is true that this calculation method would produce the highest LNB values, can you state further why this is considered the 'most representative LNB'? I note that this method produces the highest LNB and CAPE values, and therefore the highest vertical velocities as given by the max W = 2*sqrt(CAPE) relation. However, it seems to me (judging from most of the NARR-based calculations in Table 2 for the STEPS 6/29 case) that using the surface parcel with ice processes to obtain the most representative LNB is also valid.

P21277, Line 6: Shouldn't the LMD be 12.3 km (not 12.1) as stated in Table 2 and in the caption of Fig. 2?

P21277, Lines 24-28: Having the LMD above the LNB implies, at least to me, that the storm is undergoing a sustained period of convective overshooting (thus largely detraining above the calculated LNB). It would be useful to include either a sequence of vertical cross-sections of radar data because it is hard for the reader to gain a sense of the thunderstorm structure and updraft tilt at this stage.

In addition, it would also be useful to plot the time evolution of the maximum vertical velocity as well to get a sense of how updraft characteristics correlate temporally with the observed LMD for each case.

P21278, Lines 7-10: Please state why there are missing portions for the STEPS 6/3 and STEPS 6/11 cases shown in Fig. 4d & 4e.

P21278, Lines 17-19: "The regions of the detrainment envelopes from the LMD to the upper edge of the detrainment envelope include transport occurring in the overshooting tops". Are there other transport mechanisms besides persistent overshooting updrafts that can cause the blue line (upper edge of detrainment envelope) to be consistently above the LNB (black dashed line) for these storms?

Mixing with stratospheric air would cause the outflow from the overshoots to attain neutral buoyancy at a higher level than the LNB (as explained by the authors) but overshooting seems to be the only way that mass from below can get there. If so, replace the word 'include' with 'are due to'.

P21279, Lines 11-13: State the panels in Fig. 4 that are you referring to (e.g., Fig. 4f).

TECHNICAL CORRECTIONS:

P21274, Line 15: Shouldn't the difference be 1.7 km for 17:57UTC; a 1.6km difference based on Table 2 is for time 20:22UTC.

P21289: For Fig. 4 here, it would be useful to add the storm type/classification on each panel shown. Also, the lower edge of the detrainment envelope in Fig. 3 is a solid gray line, but the lower edges in Fig. 4 are in dashed lines. Please alter to one style for consistency. Also state why there are missing portions in panels 4d and 4e (either here or in the text).

Reference:

Takahashi, H. and Luo, Z.: Where is the level of neutral buoyancy for deep convection?, Geophys. Res. Lett., 39, L15809, doi:10.1029/2012GL052638, 2012

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 21267, 2012.