Atmos. Chem. Phys. Discuss., 12, C10512–C10517, 2012
www.atmos-chem-phys-discuss.net/12/C10512/2012/

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.

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Received and published: 13 December 2012

Thank you for taking the time to review this article and the many useful suggestions for improving clarity and content. As suggested by both reviewers, comparison to results from Takahashi and Luo (2012) would be fruitful; text has been added to the article accordingly. Responses to specific suggestions are detailed below.

Please note that Dr. Sarah Tessendorf has been added as a co-author as she was mistakenly omitted from the original draft. Dr. Tessendorf conducted the dual-Doppler processing for several of the cases presented in the article.

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.

Yes, this statement was misleading and has been corrected to say "tens of minutes".

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?

I believe you are referring to line 26 on P21271. The different horizontal resolutions were what were available from the original dual-Doppler analysis, completed in previous studies (as referenced). I would not expect the horizontal resolution difference of 0.5 or 1.0 km to have a significant impact on the level of maximum detrainment (LMD), as that is primarily dependent on vertical gradients. It's conceivable that the magnitude of the vertical gradients would be different in 0.5 and 1.0 km analyses, however this is not significant for our study as 1) only the normalized magnitudes of the divergence at LMD are considered and 2) the divergence is averaged over the entire domain.

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.)

Figure 1 has been expanded to include a view of one of the squall line cases (22 June) and a visible satellite view and a vertical reflectivity cross section have been added. In addition, the plus sign indicating the "most representative" NARR sounding has been bolded for emphasis.

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?

250 m vertical resolution is somewhat arbitrary. The radar observations have 500 m vertical resolution and the mobile soundings have vertical resolution of 50 m. 250 m was a compromise, resulting in more localized estimates of LNB altitude, without unrealistically fine resolution. The interpolated resolution is given such that our methodology could be better understood, but the exact vertical resolution is not important in the overall conclusions, so no additional explanation has been added to the text.

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.

More information was requested by Reviewer 1, as well. The calculation of parcel ascent including ice processes follows the work of Bryan and Fritsch (2004). Their methodology allows for mixed phase between 0 and -40 degrees C. The lack of reference to this work was a significant oversight, which has now been corrected, and additional text has been added to this paragraph.

*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.*

We agree that further explanation is helpful here. Although no single parcel represents all parcels that enter a particular storm updraft, using the most unstable parcel allows the possibility of elevated instability and elevated inflow, whereas using just the surface

parcel alone may show very little available potential energy in profiles with elevated instability. The 29 June STEPS case was not the best case to show this particular difference, as there was sufficient instability for surface-based parcels. In fact, all but one of the cases included in the article have soundings that show surface-based LNB and most-unstable LNB that are within 0.5 km. However, for one case, 15 July CHILL, the soundings indicated surface-based LNB is 3.0 km lower than the most-unstable LNB. For this reason (and from our own experience looking at operational soundings where convective regimes can show 0 surface-based CAPE, but positive most-unstable CAPE), most-unstable LNB is chosen as “most representative”. The article text has been modified to better explain this choice for “most representative” parcel.

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?

There was an error in the figure caption for Figure 2. The LMD at 23:57 UTC is 12.1 km (dashed line), and the mean LMD is 12.3 km (solid line, and as stated in Table 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.

Your suggestions are on target for further investigation of this case. Unfortunately, there is no simple relationship between maximum vertical velocity and mass flux during the strengthening period- the maximum vertical velocity oscillates between 40 and 51 m/s during this period (starting at 50 m/s and ending at 43 m/s). Additionally, cross-sections of vertical velocity show a widening of the updraft area, but no systematic changes in depth or extent of overshooting tops. This case has several interesting characteristics- besides the severe right turn (leading to the hypothesis about the vertical pressure gradient), the tropopause structure also shows multiple stability zones, which further complicates the evolution of storm depth. This case is, in fact, being simulated to attempt to better understand this increase in detrainment depth. Text has been added to better explain the preliminary findings regarding the evolution of transport in this storm, but the full story has yet to be understood, and is beyond the scope of this article.

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.

On June 3, S-Pol was down for a period of time and CHILL was employing a scanning strategy that limited dual-Doppler coverage. On the June 11, CHILL and S-Pol varied their scan strategies, because they could not volumetrically scan the entire MCS on their own in a timely manner. Thus, at times, their sector PPIs (and RHIs) covered different portions of the storm, again limiting overall dual-Doppler coverage. A note has been added to the article text.

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'. Detrainment above the LNB is likely due primarily to overshooting tops, but could also be due to turbulence along the top of the outflow region. The text has been changed to state this more clearly.

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

The statement is confusing, as it refers to detrainment profiles like Fig. 2, which sometimes had a second maximum above the main maximum (a dual-lobe feature). As this is not being addressed with much detail in this study, the statement has been removed.

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.

Correction has been made.

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).

Good suggestions- we have added the classification information to the panels of Fig. 4, and made the lines consistent with Fig. 3. And a note on panels 4d and 4e has been added to the article text.

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