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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Vertical transport of pollutants by shallow cumuli from large eddy simulations" *by* G. Chen et al.

T. Heus (Referee)

thijs.heus@zmaw.de

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The authors use Large Eddy Simulations to study how passive scalars can be transported in a cumulus topped boundary layer. The topic is definietly interesting, and while cumulus transport is quite commonly studied, and dispersion is done very often for the dry convective boundary layer, there is definitely room for improvement in our understanding of dispersion in a cumulus layer. Unfortunately, this paper has a number of fundamental flaws that make it impossible to recommend acceptance of this paper. I will list my biggest concerns here.

1) The LES simulation is run at a 100x100x40m resolution. While the eventual outcome that the subsiding shell is responsible for significant downward transport is in line with my personal view, Heus et al (QJ, 2009) showed that we need at least 25m in the

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horizontal to even begin to resolve this shell. 100m resolution is in my opinion not enough to resolve the clouds reliably (see, e.g. Matheou et al, MWR 2011) but when we're talking about the detailed structure of the cloud, like here, I believe 25m is the maximum acceptable resolution.

2) The comparison with the dry boundary layer does not make sense at all to me. Of course, when the dry inversion never reaches the pollutants, the pollutants will not be entrained into the layer. But this cannot be a reason to conclude that cumulus clouds are much more efficient in transport: Such a conclusion can only be arrived by a different dry setup, that is well scaled, both in boundary layer depth, surface flux as well as with the resultant typical time scale. This however would be quite close to what has already been done by Verzijlbergh et al, including a discussion of autocorrelation time scales etc. A more extensive discussion of the dry cbl can be found in the works of Dosio (e.g. JAS 2005). I am not sure that the current work can add to that base of knowledge, and I therefore suggest to leave this out altogether.

3) The figures 2 and 5 only show the resolved transport, not the unresolved, which is large by definition at the surface, and probably significant elsewhere as well given the resolution of the simulations.

4) Without an accurate description of the case, and especially the maximum cloud top height evolving over time, it is hard to interpret the results. If the data would be non-dimensionalized, the results would be even clearer.

– On a brighter note, I do believe that the authors have an interesting topic with the notion that much of the downward transport is related to clouds. I also do like the elegance of the decomposition between near-cloud and remote air in figures 2 and 5. A related notion that only seems to be implicit in this study becomes clear from figures 4 and 5 at 12 hours: Despite the lack of tracer in the sub cloud layer, the clouds already have a significant upward transport of tracer. This suggests that a) the cloud layer has been mixed reasonably well within 12hrs, b) a significant amount of air is laterally mixed

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into the clouds at lower levels and c) in cloud downdrafts do not show. If the authors would be able to further our knowledge in this respect, I would be very interested and enthousiastic about it.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 11391, 2012.

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