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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observational estimates of detrainment and entrainment in non-precipitating shallow cumulus" *by* M. S. Norgren et al.

Anonymous Referee #2

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Review on "Observational estimates of detrainment and entrainment in non-precipitating shallow cumulus clouds" by M. S. Norgren, J. D. Small, H. H. Jonsson and P. Y. Chuang

General Comments:

This manuscript is about airborne observations in shallow cumuli and deals with estimates of mass fraction of entrained and detrained air. In general this is an interesting and important topic, however, in its current form the manuscript needs some serious improvements before it can be accepted in ACP. The method is proposed as a novel

one (see beginning of conclusion section) but it is not well described. I strongly suggest illustrating this method by one example (one single cloud penetration). Figures 3 to 8 could be merged in a clever way but showing six different profiles of the same parameters does not provide a conclusive picture. Many statements and conclusions are quite generic (I will be more precise in the following specific comments) and qualitative. The latter is not a general problem but in this manuscript it is not clear if it is due to observational uncertainties, the choice of the model, which is the basis of the methods or other reasons. At several places other possible analysis techniques are suggested for further analysis of the entrainment/detrainment problem but not used in this work. This is particular true for the last sentence of the conclusion section. If you mention that there are "a number of approaches that could be used to address this important problem" why don't you apply these and compare with your new method? Another general suggestion is to compare your experimental results with LES. I think that there are many LES cases including fields of shallow cumuli available and you could apply your technique to these data in order to compare your results and see how robust your results are. I know that this would need some more time but it would really strengthen your results significantly.

A few figure captions/labels/legends are of poor quality; for example I am not able to read the legends of Fig 3. In general the font size of the figure labels should be increased.

Specific Comments:

Abstract: page 21786, line 14: please be more specific, what means "small" cumulus clouds? Can you provide at least a mean diameter? If you close your abstract with a statement like "findings are consistent with previous studies" one could easily argue: "so what is new? and why is your data worth to be published?" I suggest finishing the abstract with something more positive like "this new method allows for. . ."

Introduction: page 21789, line 4:if you don't discuss the budget equation in detail most

ACPD
14, C10197–C10201, 2014

people will not understand what you mean with “accumulation term”, furthermore, what do you mean with “qualitative” picture? I think in particular the detrainment part of the introduction could be improved because - as you mention – most studies are about entrainment.

On page 21790 , line 7 ff: I don’t understand the sentence starting with “Because this method. . .” Furthermore, the last part of the last sentence in the introduction is difficult to interpret. If you want to introduce a new method than it is not really important if previous studies are based on “bigger clouds”. Same as with the abstract, I would finish this introduction with something more positive – you want to convince the reader to continue with reading your paper! I think your statement in the last sentence is not really important because you don’t compare your results with previous studies.

Method section 2.1:

There are not too many details about the sensor used in your analysis, however, there are some critical concerns in particular about temperature readings in clouds and the time response of a dew-point mirror. The dew-point mirror has typically a time response of a few Kelvin per second. There is no information about the difference of the dew-point inside cloud and the environment but the spatial resolution of the humidity observations is probably in the order of hundred meter or so. It is most critical to discuss this in terms of your analysis. The same with temperature: it is well known that typical airborne temperature measurements in clouds have serious problems with sensor wetting (see papers of Lawson/Rodi, 1992 & Lawson/Cooper, 1990 & Lenschow/Pannell, 1974). This is still an actual problem and the error in temperature readings can be as large as a few Kelvin, even with housings such as Rosemount or so.

That is, total water and static energy estimates are subject to big errors, which will or might influence the results. A thorough discussion is mandatory for this paper!

page 21794, line 11, please provide a value/range of used heat capacities.

ACPD
14, C10197–C10201, 2014

Same page line 19ff: please avoid repetitions: If you start a sentence with “We note again. . .” you can probably avoid this statement.

Page 21795, line 12: one of your assumptions is that detrainment occurs mainly for adiabatic conditions. It is well known that adiabatic conditions are only found in the cloud core region of actively growing clouds. This assumption seems to be inconsistent with the conclusions (line 25) where you state that detrainment increases with height and in the same sentence you mention that adiabaticity decreases with height. This is at least confusing and should be better explained.

You mention – at several places in the entire text but particular in Sec 3.2.1 – that there might be a possible bias in your observations due to the sampling strategy; mainly it is questionable that one single penetration cannot really represent a single cloud and the edge region might be oversampled. I suggest discussing this in more detail, please have a look at :

F. Hoffmann, H. Siebert, J. Schumacher, T. Riechelmann, J. Katzwinkel, B. Kumar, P. Götzfried, and S. Raasch. Entrainment and mixing at the interface of shallow cumulus clouds: Results from a combination of observations ans simulations. Meteorologische Zeitschrift, 23(DOI: 10.1127/0941- 2948/2014/0597):349 – 368, 2014.

In this paper such sampling problems are simulated and discussed.

Page 21801 line 1 to 25: this part is highly speculative and should be carefully re-written

Page 21802,line 14ff: A more thorough analysis of this issue would make your paper much stronger. Why not testing other methods? Why not trying a comparison with LES data? Without such a comparison the results are somewhat weak and the conclusions are not really convincing.

Page 21083 line 14ff: this conclusion is not convincing. Phrases such as “ easily understood reason.” In line 16 should be avoided or much more specific but this three

ACPD

14, C10197–C10201, 2014

lines are really generic.

Lines 21 to 24: This statement is also not convincing. It sound like the observed effect is small so there should not be a big difference if we change the under laying model?

Page 21804, line 12 ff: Why do you consider your model to be more realistic. Please give a detailed argument. If you close this section with a statement like: The overall picture. . .detrainment is a weak process." the reader might ask why you think this paper contributes to an important topic. Here it's more the wording, which makes your argument weak.

The conclusion of section 4 remains unclear to me. What is the physical picture behind this observation? You only mention that your qualitative picture agrees with one publication. You should also define the buoyancy by an equation, than you can avoid ambiguous explanations such as around line 18 or 11/12.

Conclusion section: you spent a lot of time arguing why your results do not agree with other observations ("These low values. . .." Starting at line 13). Following your arguments starting in line 17 one might conclude that natural variability is dominating and the effect od detrainment is not very clear.

Page 21807, line 1. ". . .it is well-known..:" than please provide a reference

Line 8: ". . .positively buoyant relative to the environment" Buoyancy is always defined as a temperature difference and the reference temperature is the temperature of the environment. See previous comment, define buoyancy and you can avoid phrase like "..relative to the environment" which are repetitive.

Line 15: if it is possible to develop more complex model. . .why don't you do it? It would really strengthen your conclusions, which are a little bit weak so far. The same with the last sentence of the conclusion .

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 21785, 2014.

ACPD

14, C10197–C10201, 2014