We thank all three reviewers for insightful comments on our manuscript. We significantly revised the manuscript in response to these comments. Since there are some common threads in the comments, we start by explaining the overall scope of the revisions, before we proceed to detailed responses to every set of comments. The revisions include: i) significant rewriting of the introduction and conclusion sections, emphasizing new elements of the approach and new insights into microphysical impacts of the entrainment; ii) application of the mixing diagram that has been used in the past in analysis of the microphysical impacts of entrainment following our revisions of now-accepted JAS paper (Jarecka, Grabowski Morrison and Pawlowska, 2013: *Homogeneity of the subgrid-scale turbulent mixing in large-eddy simulation of shallow convection*); and iii) addition of the section that discusses results of simulations which apply standard LES model with the double-moment bulk microphysics, that is, without the delay of evaporation due to turbulent stirring and without the local prediction of the homogeneity of mixing. We believe that these revisions lead to a significantly improved manuscript that will become a part of the permanent collection of EUCAARI manuscripts. We would like to stress that, as far as we can tell, our manuscript is the only modeling paper in the collection that discusses cloud modeling of the EUCAARI-IMPACT case. We are only aware of a single modeling non-referred manuscript available in the proceedings of the EUCAARI meeting that we acknowledge in our manuscript.

Below we detail our revisions (*reviewers' comments in italics*, our response in regular).

General comments:

If this study holds some unique insight into different mixing scenario's observed in low level clouds, then a more in-depth discussion is needed to highlight this insight. What new light does it shed on the presence of (in)homogeneous mixing and the impact of a scheme that represents stirring? Related questions that should be addressed are:

(i) how do the modeling results compare to observational evidence of (in)homogeneous mixing in shallow cumulus and stratocumulus? The authors do not refer to other observational studies looking at these aspects at the EUCAARI or other campaigns.

The extended discussion is based on the mixing diagram from experimental results from EUCAARI-IMPACT campaign and mixing diagrams from other cloud campaign available in literature (Burnet and Brenguier, 2007; Lehman et al., 2009).

(ii)are other LES/advanced microphysical codes (in)capable of showing these results?

This is now addressed in the section added in the revisions.

(iii) can the discussion of the general results be discussed more concisely, so that more focus can be paid to the discussion of the mixing patterns as simulated with this LES code? In particular section 6, the conclusions of the manuscript, fails to actually provide a discussion or conclusion.

We believe that the revised discussion is more concise and it answers the questions related to the mixing pattern from the scheme. The conclusion section has been rewritten.

(iv) what part of the modeled behavior of cloud microphysics is a property of the scheme and does (not) comply with observed behavior?

This is a very general question and we do not know how to address it to the reviewer's satisfaction. We hope that the revisions provide some insights into this aspect.

Specific comments:

1. P.1492 Line 10/11: The authors write about the homogenization of a gridbox experiencing turbulent mixing. Instead of using the word gridbox, would it be better to talk about a "volume of cloudy air" in general? It is otherwise suggested that all models represent the turbulent stirring accurately.

Corrected.

2. P.1492 Line 25: In the Jarecka et al 10'3 paper, for what case is the delayed evaporation / stirring scheme applied, and can the authors say anything about its functionality and uniqueness?

The paper Jarecka, Grabowski Morrison and Pawlowska, 2013: *Homogeneity of the subgrid-scale turbulent mixing in large-eddy simulation of shallow convection* discusses results from simulations of the shallow convection case observed during the BOMEX field campaign (based on Siebesma et al. JAS 2003). Main features of the scheme are described in the section 3 and in the appendix of the submitted ACP manuscript.

3. P.1494 Line 5: Is just the height of the aircraft used to distinguish stratocumulus from shallow cumulus, or also the estimated cloud base height? Sometimes shallow cu can reach heights that are within the stratocumulus layer, for instance how do the authors know that the points near 700 m height in Figure 4 are from shallow cumulus and not stratocumulus? And do the heights on the y-axis here also represent height above cloud base, because it does not say so.

We agree that is impossible to distinguish clearly between the cumulus and stratocumulus. The stratocumulus base is determined for each cloud pass separately. LWC in stratocumulus layers is typically close to adiabatic, and the cloud base is estimated by fitting the theoretical function of adiabatic LWC to the experimental data points. However, some points above estimated stratocumulus base can still come from cumulus clouds. This is particularly true for the points with higher LWC than the predicted adiabatic value. New diagrams added in the revisions should clarify this aspect.

The y-axis in Fig. 4 represents height above the sea level and it is now clarified in the manuscript.

4. *P.1494 Line 25/26: Here would be a good point to introduce specific questions the authors are interested in to test using the EUCAARI case, e.g. open questions (about model features) left from the Jarecka 2013 study? Instead of saying that "these observations are compared to results", one could say, "these observations serve as a guideline for".*

We hope that the revisions address this point.

5. *P.1496 Line 1: I have trouble understanding how the mixing scale λ is diagnosed in the model. Although probably outlined in detail in Grabowski et al 2007, it may be worthwhile providing a short statement here.*

The mixing scale is one of variables predicted by the model. We stress this aspect in the revision by replacing "including" by "predicting" in the sentence beginning in line 23 of page 1495.

6. *P.1498 Line 15: How are these points that undergo turbulent mixing determined? For instance in Figure 7 many parts of the cloud field are apparently not subject to mixing, is there a threshold set on the diagnosed TKE?*

The value of the variable λ determines if a point undergoes turbulent mixing. The value of lambda is locally predicted and has to satisfy $\lambda_0<\lambda<\Lambda$.

7. *P.1498 Line 22-24: Here is a nice point to discuss and describe the structure of the mixing as observed in the Figure. Where and at what heights does one generally find certain values of α, based on existing observations/literature?*

Unfortunately, alpha cannot be deduced from aircraft observations. This is why Lehmann et al. (referred to in the revised manuscript) introduced a spatial scale for which the mixing and evaporation time scales become equal. Such a procedure introduces an alternative way to evaluate mixing characteristics from aircraft observations. We hope that the new material added in the revisions address the reviewer point to his/her satisfaction.

8. *P.1499 Line 11: Do you need the word CFAD to indicate a frequency distribution? The CFAD term is not broadly used outside of the radar community, so why not just naming it a frequency distribution, with the colors referring to the frequency.*

We prefer to use the term CFAD. Its meaning is now better explained in the revised text.

9. *P.1499/1500: The end of section 4 goes through Figure by Figure describing typical features of clouds that are commonly known. This reads fairly boring. Can the authors*

combine the description of what is known and also present in the observations, and outline those features that are particularly interesting and new?

The discussion is rewritten. We hope that the new material makes the reading more interesting to the reviewer.

10. P. 1501 Line 15-18: The authors show that the base of the stratocumulus layer behaves much like the top of cumulus clouds, in terms of α. Here would be a good point to discuss what is more surprising to the reader, the behavior at stratocumulus base or at the cumulus top? How much of that behavior is due to the specific formulation of the stirring scheme and the diagnosis of α and the mixing time scales? The important details of Appendix A may be mentioned here to remind the reader.

The discussion is rewritten and we believe it addresses these points now.

11. P.1502 Line 2-7: This section is particularly confusing and lacks at providing a clear link between the results and the features of the stirring scheme. The authors write that the droplet radii are very similar in both layers (in Figure 11), but in Figure 14/15 the differences in radii to the power of two are rather different. Line 4: "This is against a common assumption", what does "This" refer to? The diagrams in Figure 14/15 can be explained in more detail e.g. the presence of many (red) points (presumably cloud base) that have a value of 100 for (1 – RHd)−1 and large radii.

Fig. 15 shows the points near the stratocumulus top and the text is about the lower levels of the stratocumulus layer. However, we agree that this part was confusing and it is now rewritten.

12. P.1502: The conclusions are no real conclusions, but rather a summary or repetition from previously written sentences. What news does this study bring? Is it yet common knowledge that mixing in these type of clouds are inhomogeneous? Why are the mixing and evaporation time scales for stratocumulus and cumulus so similar? Is that also observed during the EUCAARI (or other) campaigns?

The conclusions are rewritten in the revised manuscript. We make an effort to address some of the questions posed by the reviewer.

Typos/grammar/graphics:

The issues have been addressed in the revisions.