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.

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

- *The introduction does not state any real goal for the paper, other than doing a simulation of a particular cloud deck at a particular time and place. What could the paper mean beyond that? For instance, it could serve as a paper to (further) validate the mixing model, or emphasize the necessity of it. But for that you would need to show the impact of the model on the mean quantities of your simulation. Or the focus of the paper could be on the physics of the cloud edge mixing. Then, some more explanation of why the results are the way they are would be in order (why is the evaporation time scale almost equal to the mixing scale? What happens at the cloud top?).*

We believe that the revision directly addresses the criticism of the reviewer. To reiterate, we add a new section demonstrating that the traditional model is not capable in predicting observed microphysical characteristics near the cloud top (where the mixing takes place) and it better puts model results in the context of observations. We stress that the unique cloud features observed during the examined flight (cumulus under stratocumulus) are successfully simulated by the models considered, but the microphysical characteristics of the cloud top region requires sophisticated subgrid-scale scheme.

- *There are many minor grammatical errors in the text that reveal that this manuscript is not written by native speakers. Specifically, many articles are missing. The first one is in the title: I would say the EUCAARI field campaign. These errors do not prohibit the* *understanding, so for now it is fine, but I would recommend proof reading by a native speaker.*

We make an effort to improve English of the revised manuscript. In particular, the manuscript will be read by a native speaker.

- *I cannot access JGMP13 (is Hugh Morrison one of the authors? if so, he is missing in the references), but I would appreciate to know what is already done in that paper. There is a potential for overlap with this paper, and I'd like to know how well the subgrid model is validated in that paper. I assume that JGMP13 focused on conventional shallow cumulus?*

The paper Jarecka, Grabowski, Morrison and Pawlowska, 2013: *Homogeneity of the subgrid-scale turbulent mixing in large-eddy simulation of shallow convection* was accepted for publication in the Journal of the Atmospheric Sciences in late April and should be available on-line in early June (DOI: 10.1175/JAS-D-13-042.1). The paper focusses on the description of the new subgrid-scale mixing scheme and its application to the model intercomparison case based on the BOMEX field campaign data as described in Siebesma et al. (2003). We put the model results in the context of field observations (similarly as in the current paper), but we would not say that we "validate" the model. If needed, we can make the JAS paper available to the editor.

- *As far as I know, the method is based on filaments of cloudy air being ripped away from the cloud, and after that slowly evaporating. This is a process that I much more heavily associate with cumulus than with stratocumulus, and I've always read the Andrejczuk in the cumulus context. How well is the model validated for stratocumulus?*

The subgrid-scale mixing parameterization discussed in the JAS paper applies to the case of turbulent cloud-environment mixing, of which ripping cloudy filaments out of the cloud is just one example. The same happens inside a cloud, where cloud-free filaments are "ripped away" from the unsaturated environment. Our model treats these two processes. It happens both in the cumulus (near its lateral edges and near the top) as well as in the stratocumulus (mostly near its top).

- *Does the resolution of your simulations suffice? Sandu and Stevens (JAS, 2011) use a 35/5 m grid. Especially at the stratocumulus top I would expect that to be necessary. Also, a horizontal resolution of 50m means that the 3 smallest cumulus clouds in figure 7 have a width of 2Δx or less. Given that this paper is not just about being able to simulate a certain field, but about gaining understanding of the mixing around the cloud edge, I suspect a significant resolution dependency here. Also, for these cloud sizes and resolutions, does the assumption of Λ = Δ still hold? Given all of this, I would recommend redoing the simulations preferably with a 25/10m resolution. This should still be very well doable on most present days machines.*

We agree with this point. However, since the model applies the sophisticated subgrid-scale mixing scheme, we believe that the effects of low spatial resolution are to some extend mitigated. We agree that it would be the best to perform a suite of simulations with increasing spatial resolution. Unfortunately, this is not possible for variety of reasons. As for the Sandu and Stevens paper (JAS, 2011), we would like to point out that increasing heterogeneity of the model grid (i.e., drastically different horizontal and vertical gridlength) introduces additional problems, not appreciated by many authors. Specifically, LES subgrid-scale schemes are not designed for strongly heterogeneous grids and results from such models need to be treated with much caution.

- *Figure 1: Segments instead of sedments*

Corrected.

- *Figure 3: What is the definition of the cloud base here? Local lowest level of cloudiness, global lowest level, first level with a cloud fraction over, e.g., 80%, etc? Also, the line in fig b is undefined.*

The local stratocumulus base is estimated for each cloud pass separately. LWC in stratocumulus layers is typically close to adiabatic, so the cloud base is estimated by fitting the theoretical function of adiabatic LWC to the experimental data.

- *Figure 5a: Is this the potential temperature, or the liquid water potential temperature?*

It is the liquid water potential temperature. The description is now clarified.

- *Figure 8b: I assume this is ql conditionally sampled over cloudy regions? If not, entrainment cannot be blamed of dilution using this graph. Could you plot the 1g/kg/500m line as a reference in this graph?*

Yes, it is conditionally-sampled. The suggested line is added to the figure.

- *p1499, l2: The cloud fraction for cumulus being .1 and for stratocumulus .9 is a rather trivial result.*

Yes, this is trivial, but only if the two cloud formations are observed separately, as in the paper by Siebesma et al. (2003) for cumulus and Stevens et al (MWR 2005; the DYCOMS case) for stratocumulus. We do not think this is trivial for the Cu-under-Sc formation. In addition, stratocumulus does not have to be homogeneous: Pawlowska and Brenguier (JGR, 2003) reported cloud fractions between 0.5 and 0.9.

- *p1500, l 19: Section 3, not the previous section*

Corrected.

- *p1500, l 22: "Most of the mixing occurs at the edges of cu and the top of StCu" Where else? Would you have expected more mixing further removed from the cloud, or inside the cloud after a strong entrainment event? The thing that is interesting here is the* *combination with the timescales of Fig. 13. In 2 minutes, (= τmix), I'd expect the mixing parcel to have moved a fair amount.*

We agree that this is fairly obvious, but we think that stating this explicitly is OK. For instance, if one has negative-buoyancy-driven downdrafts, one can expect some mixing away from the cloud top. In fact, it does happen in cloud holes as illustrated in the bottom panel of Fig.7.
Relatively big values of τ_{mix} do not necessarily mean that the mixing parcels move far before homogenization takes place. These are the values calculated from the instantaneous values of the model variables and they likely change from one model time step to the next one.

- *Figure 13: These timescales are interesting. For instance, the mixing timescale is close to the Brunt Vaisala timescale, although I believe no information on buoyancy is explicitly present in the calculation of it. Is there a buoyancy correction in your sub grid model? Or is it just the mixing time of a grid box, meaning that there is a resolution dependency here. Even if you don't know why the time scales are the way they are, I would enjoy some educated guesses. The evaporation time scale is a lot longer than I would have expected it, and certainly suggests that the usual all-or-nothing approach of traditional LES models is not valid. Could you comment on this? Do you know what the impact is on the mean quantities (e.g, in precipitation, or cloud top entrainment)*

The buoyancy information is only remotely involved in the mixing timescale through its effect on the TKE production. So this is just a coincidence we think. The mixing time scale depends on the predicted filament size, and since the size is limited by size of the gridbox, so there is some resolution dependency.
The evaporation time scale depends on the relative humidity of the entrained air and the size of cloud droplets. Since the entrained air comes from the cloud shell and is very humid, the evaporation time scale is indeed quite long.
We agree that traditional all-or-nothing approach is not appropriate in the case or relatively coarse resolution, as illustrated by simulation results added in the revisions.

- *Figure 15: Many of your entrainment events seem to sit at λ0. Although the length scale should come straight out of theoretical turbulence, there is significant room for alterations to this parameter. Could you comment on the sensitivities?*

We checked that there is no significant sensitivity on the selection of λ_0. This was also checked in the original Grabowski 2007 submission, although perhaps not commented upon in the published version of the paper. This is because mixing progresses rapidly when λ is close to λ_0. The fact that all mixing events have to eventually lead to $\lambda=\lambda_0$ is perhaps the reason for the observed pattern.