

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).

*The authors present a LES simulation of a stratocumulus-over-cumulus case observed during the EUCAARI-IMPACT field campaign, focusing on the mixing of the cloud with its environment. Although the topic is interesting, because we need to advance our understanding of mixing at cloud edges, the paper does not manage to bring forward what is novel about this investigation or how it improves our understanding of such processes. In its current status, the paper therefore resembles more a "deliverable" report than a scientific paper. For being worth to being published in ACP, the authors should think what are the important points they want to make, and rewrite the paper trying to bring these points forward. My main concerns join the concerns brought out by the other reviewers, namely:*

*(i) in the introduction instead of the approach : " there is this observed case and we simulate it with our LES and it kind of works", it would be far more interesting if the authors were highlighting what are the issues with mixing at cloud edges, what we know about it and what we don't, and what questions the new development in their LES can help addressing. And then construct the paper so that it responds to these questions.*

The introduction has been rewritten and we hope it now addresses the reviewer's concerns. We try to explain the focus of the paper and the key features of the new subgrid-scale mixing scheme.

(ii) *as highlighted by another reviewer, the resolution at cloud top is really too coarse for focusing on mixing at cloud edges, and particularly at the top of the stratocumulus. Indeed Sandu and Stevens (2011) went down to a resolution of 5 m at cloud top to be able to reproduce the sc to cu transition, and the grid they used is used ever since in all intercomparisons of GASS (composite transitions, ASTEX, now CONSTRAIN). Given the demonstrated sensitivity of cloud top entrainment to grid size in LES, I would not consider the results as reliable, unless the authors redo the simulation with a vertical resolution of at least 5m within the cloud layers.*

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 extent 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.

(iii) *It is well demonstrated now the role played by the shortwave radiation within the diurnal cycle of the cloud. If the authors want to quantitatively compare the cloud evolution with the observed one, both the shortwave and longwave radiation should therefore be accounted for. When wanting to look at something as sensitive as mixing at cloud edges, the more precise we can get the better it is. So why not using one of the radiative transfer codes available in the different LES models, like done nowadays by all the participants to the latest intercomparison exercises related to boundary layer clouds ? This would allow including the shortwave but also having a more precise description (rather than using tuned numbers) of the cloud to radiative cooling which is the main driver of mixing at cloud top.*

Although we do agree with the spirit of the reviewer's comments, we do not believe that including shortwave radiation is necessary. First, diurnal cycle concerns time scales orders of magnitude longer than the mixing time scales we consider in the paper. The observed case undoubtedly evolves in time, due to absorption of solar radiation as well as – perhaps more importantly – evolving large-scale advection, but this aspect we believe is beyond the fidelity of observational and modeling techniques at the moment. We are sure the reviewer agrees with us that the model manages to predict the unique characteristics of the observed cloud field.

*(iv) it would be also good to make the distinction between what is done in the submitted JAS paper and this one, especially that the JAS paper is not available for the readers at present.*

The paper by 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 focuses on the description of the new subgrid-scale mixing scheme and its application to the model intercomparison case based 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.