Response to Reviewer#1

We thank the Reviewer for her/his comments.

General remark. Retrieval method well described and illustrated, however the discussion is incomplete. The main issue concerns the temporal resolution: 0.5 min retrievals seem extremely variable. In my opinion 0.5 min averaging of the signal is far from optimum. What is the effect of averaging? In which temporal scales assumption of /almost/ adiabatic cloud with deflection makes sense? The following, more specific questions should help to address the issue. How does the signal averaged over longer time compares to the averaged retrievals? What is the optimum? Authors do not have to fully answer these questions, I believe that the problem extends far beyond this paper. However, the problem should be addressed and at least shortly discussed.

We agree with the Reviewer that this is an important aspect of our analysis that has not been discussed enough. As the Reviewer has certainly noticed all variables have been retrieved and showed in graphs at different temporal resolutions, each layer-averaged variable is shown at the bottom of each graph with 0.5-min and 7.5-min temporal resolution. The reason of this has been to study the effect of averaging on the indirectly retrieved cloud dynamics. I elaborate this more here: for the ~0.5-km and ~1-km cloud depth of the continental and marine cases, respectively, the updraft and downdraft velocity is on the order of ±0.5 m/s and ±1 m/s (Doppler velocity from the cloud RADAR). Over the cloud depth this leads to ~15 min for both cases to have the full ascent/descent of an air parcel. The 7.5-min temporal resolution allows then to observe (where the process can be detected) the cloud dynamics while reducing significantly the noise. A discussion about the effect of temporal averaging has been added to the revised manuscript.

DETAILED REMARKS.

1. Measurements are performed in Mace Head, they concern marine stratus/ stratocumulus advecting over land. Marine in a sense thatclouds are formed over the ocean (limited diurnal cycle at the surface) and are advected over Mace Head coastal observation site. Continental and marine microphysics in the title is fine, but “continental and and marine stratocumulus formed over...(P4826, 8-9) is somewhat misleading..

The abstract has been rephrased and the expression changed.

2. P4827, 4 “more significant mixing” - is significance result of more intensive mixing (more mass exchange) or result of more contrasting humidity and temperature air mixed into cloud? I suggest more caution here.

The final statement in the abstract has been rephrased.

3. P.4830,18. Explain why your condition for excluding precipitation cases from the data is LWP(rain) > 2000gm-2. Citation?

Explanation and reference added.

4. P4838, 26 – as 2. Subadiabaticity is a deflection from adiabatic profile of LWC, most likely due to entrainment, but is NOT a measure of entrainment. Effects of entrainment depend of WHAT was entrained, which is partly, but not consequently discussed in P4839, 12-26.

Also accordingly to the other reviewers' comments, the discussion presented on P4839, 12-26 has been omitted in the revised manuscript.

Subadiabaticity can be also effect of solar heating at the cloud top. Discussion of continental (nighttime) and marine (daytime) cases completely neglects this aspect.

We agree with the Reviewer on the importance of solar heating at cloud top. There are also other parameters and physical processes inducing the mixing at the cloud boundaries and then affecting the subadiabaticity. For this reason this study prefers to deal with the final effect (entrainment) rather than with the independent processes leading to it.

5. Section 4.2. It seems that the authors are aware of the averaging /spatial/ temporal variability issue. Is higher variability of marine case related really to continentality? Is thicker cloud more variable than a thin one? Is assumption of constant CNDC in the region of full attenuation valid in a thick cloud?

All these aspects are now discussed in the revised version of the manuscript. Concerning your last comment my answer is yes, constant CDNC is a good approximation at 100-200 m above the cloud base and up to the top, especially for stratocumulus cloud. Morales and colleagues (JGR, 2011) discuss the correctness of assuming this also in deep convective cumulus.