Response to Reviewer#3

We thank the Reviewer for her/his comments.

This paper describes a multi-instrument retrieval of cloud microphysical parameters in the framework of CLOUDNET and beyond, with the stated goal of improving climatologies of cloud (shortwave) albedo (if I understand the motivation/introduction correctly).

The aim of this manuscript is to retrieve the microphysics of two liquid stratocumulus clouds, one continental and one marine, by means of synergistic co-located measurements from a Doppler cloud RADAR, a single-wavelength 1064-nm ceilometer and a microwave-radiometer. The three microphysical variables, namely the cloud droplet number concentration (CDNC), the droplet effective radius (r_{eff}) and the liquid water content (LWC) are retrieved by the Synergistic Remote Sensing Of Cloud – SYRSOC – technique.
Although the Albedo is mentioned in the Introduction as well as other microphysical parameters, it is not the focus of our study. Moreover, nowhere in the manuscript the cloud albedo is retrieved or shown in any plots. SYRSOC only retrieves the three above-mentioned variables which are shown in Figures 5 to 10.

I agree with the authors that it is important to study cloud albedo and its dependence on various cloud microphysical parameters, which in turn are affected by various aerosol effects. But I doubt that the paper offers a solution to better global observations of this parameter, for two reasons: (1) While ground stations do offer long time series of important parameters, there are not enough of them to be globally representative;
I would argue that a better use of ground stations would be to validate and improve existing satellite retrievals, or to establish local climatologies; (2) Since the main motivation for this work seems to be the radiative effect of clouds (albedo), it is astonishing that the authors chose this particular combination of instruments.

As it is explained above the focus of this manuscript is neither to retrieve nor to provide an assessment of the cloud albedo. The RADAR-LIDAR-MWR is, as suggested by the recent literature (e.g. Boers et al., 2000; Illingworth et al., 2007; Brandau et al, 2010), an optimal combination of remote sensors to retrieve CDNC-LWC-r_{eff} from liquid clouds.

Shortwave cloud albedo depends primarily on cloud optical thickness (not cloud thickness, as stated by the authors) and effective drop radius.

Here (P4828 L13-14), the authors refer to the dependence of the cloud albedo on cloud (geometric) thickness, CDNC and LWC (Seinfeld J. H. and Pandis S. N. (2006) Atmospheric Chemistry and Physics: From Air Pollution to Climate Change, 2nd edition, P1082 Figure 24.17).

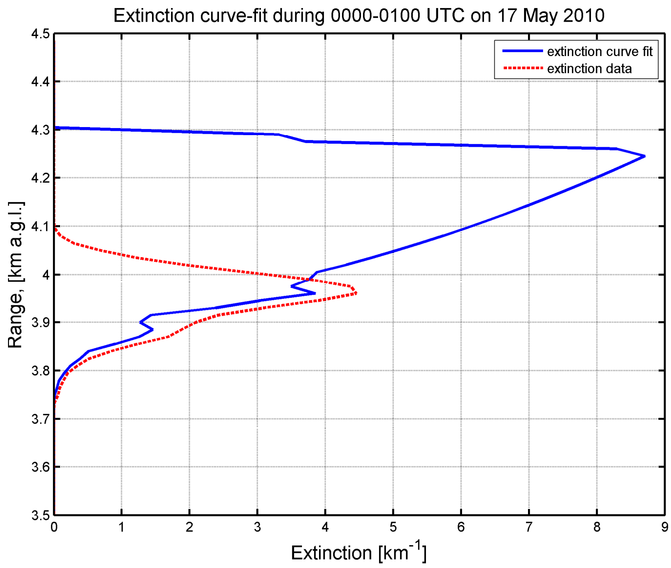
In the near-UV, it also depends on cloud top height. (In gas absorption bands, e.g., water vapor bands, it also depends on gas concentration.) The most important parameter: cloud optical thickness (column-integrated extinction), is related to the second moment of the drop size distribution, or <rˆ2>; the second most important parameter, the effective radius, is the third divided by the second moment, <rˆ3>/<rˆ2>,

which is also explained in the paper. With the instrumentation used by the authors, the available moments of the cloud drop size distribution are <rˆ6> (radar); <rˆ3> (microwave). The second (and most important!) moment, <rˆ2>, is supposedly provided by the lidar/ceilometer

We agree with the Reviewer's comment.

But section 3.2 and Fig. 3 are (at least in this version of the manuscript) inadequate to explain how the full vertical profile of extinction can be derived even in regions where the lidar is completely attenuated.

The explanation of the procedure to calculate the extinction in Sect 3.2 and Sect. 3.4 has been improved in the revised manuscript.
Indeed, the retrieval scheme SYRSOC has amongst its best and most innovative skills the efficient reconstruction of the extinction profile through the cloud. As mentioned in Sec. 3.2 and explained in more detailed way in Sect. 3.4, a curve-fit is used to fit in least-square sense the extinction through the first couple of hundreds meters above the cloud base. The figure below shows an example of derivation of the extinction profile through the cloud layer. In red the measured extinction profile and in blue the curve-fit extrapolated profile up to the cloud top averaged over one hour.



It is also not clearly described how the assumed lidar ratio is justified; literature where extinction is derived from the backscatter ratio without assuming a backscatter ratio should be consulted and cited (check numerous papers by Eloranta), and a better justification should be given why it can simply be assumed.

The LIDAR ratio S= 18.2 sr used in our calculations to retrieve the extinction was originally worked out by Pinnick et al, 1983 for the LIDAR wavelength at 1064 nm. The zero-order solution of the relation between backscatter and extinction used in Pinnick et al and leading to S= 18.2 sr is independent of droplet size distribution. Higher-order terms in

Pinnick's solution take the size distribution into account. These terms are calculated explicitly for Gamma-type size distributions characteristic of cloud and are found to contribute on the order of 10% of the leading term which then becomes S= 18.2±1.82 sr. Because the numerical procedure used in this study to invert the LIDAR return (Ferguson and Stephens, 1983) is normalized by S, the retrieved microphysical variables based on the value of the extinction are physically independent of S. However, as the LIDAR return is mathematically divided by S, the S-error propagates to the extinction and to the other determinations and must be considered when assessing the total uncertainty. This is now done in Sect. 5.

Also: How was the lidar/ceilometer calibrated by a sunphotometer, and which parameter was calibrated? This calibration certainly does not apply for cloudy conditions, because sunphometers determine aerosol optical thickness if the direct beam is not attenuated - they are therefore inadequate for the retrieval of cloud parameters (except in the so-called zenith-pointing mode introduced for AERONET: see Chiu, J. C., Huang, C., Marshak, A., Slutsker, I., Giles, D. M., Holben, B. N., Knyazikhin, Y., and Wiscombe, W. J.: Cloud optical depth retrievals from the Aerosol Robotic Network (AERONET) cloud mode observations, J. Geophys. Res., 115, D14202, 2010.).

Calibration is performed in clear-sky conditions with the purpose to calibrate the lidar signal versus a known reference. This is now clearly stated in the text.

The most confusing statement is that about the extrapolation of lidar-derived extinction profiles from the lower regions of the cloud into the higher regions where the lidar signal is attenuated. In sum: Although the most relevant parameters for cloud albedo are optical thickness and effective radius, the second, and most important, moment of the size distribution is problematic for the aforementioned reasons.

Please, refer to our answers provided above.

A more appropriate method for determining cloud optical thickness would be the method by Chiu et al., JGR, 2010, or by McBride et al., ACP, 2011. Both methods use ground-based shortwave radiance measurements, which by definition are more appropriate for climate-relevant cloud observations because they determine second (optical thickness) and third over second moment (effective radius) directly

The cloud optical thickness has not been retrieved in this study.

Despite all of these severe issues, I do think that this paper has substantial merit, albeit not for studies of the cloud radiative effect. It warrants publication after major changes. Most importantly, I highly encourage the authors to change the motivation of the paper. Rather than claiming to improve climatologies of, e.g., albedo, I would suggest to focus on cloud process studies and satellite validation work (a predecessor study would be that of Brandau et al.,2010).

Please refer to our answer to your first comment. The work by Christine Brandau and colleagues represents indeed the basis of our work to which they contributed by providing their scientific support as acknowledged in the Acknowledgments section.

Major Comments

(1) The description of MODIS retrievals is erroneous (p4843,l17-20): It is not calculated from the emission at two wavelengths, at least not in the daytime retrieval. In the daytime retrieval, MODIS obtains optical thickness and effective radius from the combination of *reflectances* in two channels in the very-near infrared, and in the near-infrared. Also, the sentence on line l19-20 (region that is 'responsible' for emission) should be revised. The dependence of the MODIS cloud retrievals on the vertical profile is described by Platnick, S.: Vertical photon transport in cloud remote sensing problems, J. Geophys. Res., 105, 22919–22935, 2000

The description has been improved accordingly to the Reviewer's comment and suggested reference.

(2) In the introduction (most importantly on page 4827, l9-17), terms "uncertainty" (of which parameter?), forcing, albedo, absorption, and emission, are used extensively, but it is unclear which of these parameters the authors are intending to improve. At one point, they talk about the balance between shortwave cloud absorption and longwave absorption; at another, the point out the importance of cloud albedo. Cloud albedo, cloud absorption, and cloud emission, *all* play a role in the cloud energy budget, and the wording of the introductory paragraphs are confusing in this regard. Cloud forcing is yet another parameter which is not directly related to cloud albedo; while cloud albedo is F_up/F_dn, cloud forcing is (F_dn-F_up)[cloudy] - (F_dn-F_up)[clear]. I am not sure that the authors are fully aware of these 'ingredients' of the clouds' energy budget.

The Introduction has been substantially reworded in order to make the description more clear.

(3) Two clouds were singled out in the analysis. Is there a reason for choosing two particular clouds rather than a larger data set?

The reason is in the aim of this study. In fact this is not a statistic of microphysical parameters but the comparison of a continental and a marine case retrieved by the new technique SYRSOC. After the presented technique will be assessed by peer-review process, future statistical studies will certainly be produced (a 4-year database is available for statistical analysis of cloud products).

(4) There are many assumptions and parameterizations used throughout this paper. And yet, none of these is questioned in the sensitivity analysis of the paper. For example, the ramifications of fixing the lidar ratio, or formula (6) with various underlying assumptions (such as that of a Gamma cloud drop size distribution) are not considered in the error analysis. How is it possible, that, e.g., equation (6) still holds even for a bi-modal distribution (when drizzle is present)?

We answer point by point here:
i) The effect of assuming a LIDAR ratio can be calculated when assessing the total uncertainty. This is now done in Sect. 5.

ii) The effect of assuming the cloud droplet size distribution was already discussed and quantified in Sect. 5 (point (ii) term k_2***).***

iii) The effect of assuming a mono-modal Gamma distribution can be easily assessed a posteriori making use of the r_{eff} ***frequency distribution in Figure 9. The drizzle affects only the marine case (for the continental case no drizzle is observed). The drizzle has nevertheless only***

negligible impact on the size distribution since N is on the order of ~1 cm^{-3} for the drizzle. The reviewer comment is indeed significant for mixed-phase clouds in which a bi-modal size distribution must be considered.

(5) Water vapor super-saturation (p4828, l15) does not play an important role in determining cloud albedo. Rather, it is water vapor concentration that determines cloud albedo, and that only in water vapor absorption bands.

The paragraph has been rephrased.

(6) Since albedo is one of the motivations of the paper, what would the resulting uncertainty be in this parameter, or in any other relevant parameter (forcing, absorption,: : :)?

Albedo has not been retrieved in this study nor has been assed the cloud radiative forcing. No uncertainty is then calculated for either the cloud albedo or the radiative forcing.

Minor comments:
p4827,l24/25: aerosols –>aerosol

Done

p4827,l25-27: Revise sentence, it is unclear

Done

p4828,l6: droplets –> droplet

Done

p4828,l14: cloud thickness –> cloud optical thickness; how about effective radius (see comments above)?

Here is cloud geometric thickness not optical. See answer above.