Response to Reviewer#2 Jeff Sneider

We thank the Reviewer for his comments and suggestions, we hope with this to address most of the Reviewer's concern.

Major Criticism –

1) Section 3.4 - Your explanation of the retrieval of N (Equation 6) is confusing. While it does seem that the least-squares minimization described by Boers et al. (2000) is employed, your mention of a "curve-fit" suggests something different. I recommend that this explanation be clarified.

We used the relation worked out by Boers and Mitchell in 1994 and by Boers and colleagues in subsequent studies, i.e. the link between the number concentration of droplets, the LIDAR extinction and the LWC in the cloud. The use of least squared minimization technique is applied to a power-law model to fit to Eq. (6). More detailed explanation of the procedure has been added to the text in Sect. 3.4.

In addition, the reader needs to be told, in this section, that alpha is not known.

Clear statement about that is provided few lines after about the chosen value of α in polluted and clean condition (Miles et al., 2000; Goncalves et al., 2008)

Also, since your Equation 6 is a repetition of an equation presented in Boers and Mitchell (1994) and in Boers et al. (2000) this needs to be explicitly acknowledged.

It is now clearly acknowledged.

Finally, it seems that Equation 6 translates all of the subadiabaticity into a decrease in droplet concentration, while leaving droplet size unaffected mixing (e.g., Equation 24 in Boers and Mitchell, 1994). Since entrainment and the resulting subadiabaticity can affect both number and size, your assumption needs to be acknowledged and tested in the error analysis.

Indeed, Eq.(6) does not take into account directly the effect of entrainment on the size of droplets. However, SYRSOC propagates f(z) through all equations, so that also r_{eff} and the LWC are calculated taking into account the effect of mixing. The error analysis has been improved in the revised manuscript taking into account the error o the LWP propagatiting to the subadiabaticity and all variables.

2) P4831-L22 - The lidar is 1 um, so you need to be at a diameter of ~10 um for the Q=2 assumption to be reasonable. I see that you examine sensitivity to that assumption in the error analysis. Related to this, Figure 3 in the O'Conner et al. reference indicates that for D<10 um the assumption of constant extinction/backscatter introduces bias. In your work, how substantial is bias coming from the extinction/backscatter=constant assumption?

O'Connor et al. (2003), shows indeed departure from S=18.8 ±0.8 sr for D<10 μm. Despite the fact that we are not using O'Connor's determination of S (we use instead S= 18.2 sr worked out by Pinnick et al, 1983 for the exact wavelength at 1064 nm,), the range of size D > 10 μm perfectly suits our cloud cases (see r_{eff} frequency distribution in Fig.9). 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 worked out 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. Since the adopted scheme to retrieve the extinction (Ferguson and Stephens, 1983) in our manuscript is normalized by the LIDAR ratio S, its related 10%-error is now considered when assessing the total uncertainty in Sect. 5.

3) P4833-L12-14 – In most (all?) shallow cloud applications, the Asat is taken to be a constant set by estimate of temperature and pressure at the cloud boundary. I am not aware of a need to account for the temperature/pressure dependence of Asat through the cloud. This needs to be justified, especially in light of the fact that the cloud depths are relatively small (100 to 500 m).

We agree that a constant A_{SAT} is good enough approximation in many microphysics studies, especially when the cloud depth does not exceed 1 km. However, why to use approximated constant value of temperature and humidity when the MWR is available for co-located measurements of T and Q through the cloud? The deployed MWR is the HATPRO with 50-m vertical resolution in the first 1200 m a.g.l. and +/- 0.7 K-accuracy which allows the retrieval of at least 2-to-10 T and Q points between cloud base and top.

4) Equations 2, 3, 4 and 5 - Because Equation (2) is developed in an atmospheric thermodynamics textbook and in the peer-reviewed literature - e.g., Albrecht et al. (JGR, 17, 89-92, 1990) - I see no reason to present these equations. There are several other factual errors in this section, and I would be glad to elaborate, but I feel that this part of the paper needs to be condensed by referencing to the original work, specifically Boers and Mitchell (1994), Albrecht et al. (1990) and Boers et al. (2000).

The need of an explicit representation of equations 2-5 is to allow the reader to reconstruct the complete analysis used in SYRSOC to determine the presented variables and results. However, we appreciate that citing references to the main studies would suffice the busy reader to reconstruct the analysis individual steps. In order to make the description more concise we then only give references to past literature and drop equations 2-4. Eq. 5 (now Eq.2) remains as integrated expression of Eq.1-b.

<u>Other Comments and Criticism –</u>

Abstract - "The large reff of the marine case was determined by the contribution of drizzle drops (large radii and few occurrances) and in fact the modal radius was reff = 12 um (smaller radius and large occurrances)."

Comment - It seems that the _average_ reff is large because the frequency distribution of reff is positively skewed (average > mode), but I am uncertain. I suggest that you not go into so much detail in the abstract.

Details have been removed from the Abstract.

Looking ahead at Figure 9, I have a question. Are not the middle two panels the normalize frequency distribution of reff? If yes, the Y-axis should be labeled accordingly. If no, the explanation in Section 4.3.2 needs to be improved.

Thanks for noticing it, it is mislabeling indeed. It is now corrected.

Abstract - Suggest that you round the N uncertainty to +-1 per cubic centimeter.

Done

Abstract - Does it make sense that the 10-90% LWC range is much larger for marine but the LWC standard deviation is the same for continental and marine?

Most of the uncertainty in the polluted case comes from the large variability of σ_{LWC} during the cloud lifetime. With reference to Fig. 11, the comparison between the most homogeneous parts in the continental (22:45-23:45) and marine cloud reveals a much larger departure between the continental (~10%) and the marine σ_{LWC} (~18%) than the overall mean value. In the revised manuscript the error analysis has been improved by adding two terms of uncertainty to the total one. As a consequence also the final uncertainty of each variable has changed.

Super-saturation or supersaturation?

Supersaturation.

P4827-L8 – Don’t you mean to say “…15% of thermal radiation emitted back to the Earth’s surface.” Also, I would not define “cloud forcing” in this sentence. Rather, I would make that definition in a subsequent sentence. Also, please check the 15%.

The paragraph has been completely rephrased.

P4827-L17 – I don’t concur with your assertion that because the “cloud” GH effect is large relative to CO2, microphysics has received emphasis. As far as I know, thermal emission is primarily sensitive to water path, not to how the condensate is distributed as a function of hydrometeor size. Because of this, I assert that the sensitivity of cloud albedo to aerosol was a significant motivation. Of course, the possibility of precipitation modification was another one.

Infrared radiation at ~10 µm is certainly extremely sensitive to condensed water path irrespective of how the condensate is distributed, but the retrieval of LWP is indeed part of the microphysics study. It is also true that the size of hydrometeors in the top 2 cloud optical depths is paramount when determining the albedo and the amount of reflected radiation. We then agree that it was the indirect effect described extensively by Twomey

that gave the burst to study cloud microphysics, but I wouldn't discard the motivations provided by the GH effect by clouds either. Finally, we have reworded the sentence also accordingly to your suggestion.

P4827-L26/27 – Sentence should be reworded for clarity.
Sentence has been reworded.

P4828-L8 – This set is not independent…LWC(z) and reff(z) imply knowledge of the second moment as function of z, the extinction coefficient as function of z, the optical depth and hence the albedo.

We agree.

P4828-L9 – Albedo controls the amount absorbed? Perhaps, but I can imagine a situation (broken marine cloud) where the fraction not reflected by cloud is not absorbed.

The cloud albedo provides the relative fraction of reflected vs absorbed radiation, in case of no cloud there would be no albedo.

P4828-L17/20 – CCN increase with what? The activation is generally thought to be independent of the cloud subadiabaticity, although Morales and Nenes (JGR, 2011) have a different take on that. Also, CCN activity spectrum (aerosol size distribution and aerosol composition) and cloud updraft are important determinants, but you don't mention either.

The sign of the rate of evaporation directly influences the activation of a droplet inhibiting or enhancing the rate of condensational growth. The dry entrainment affects the evaporation rate but we agree that this hardly happens at the forming cloud base of a stratocumulus cloud. A better explanation is now provided in the text referring also to the recent publication by Morales et al (2011).

Isn't there a reference describing the Mace Head site?

We added the reference.

E-band radar?

E-band radars represent the lower-band limit for cloud and storm measurements (e.g. Medium Power Radar (MPR)).

Regarding the Hudson et al. (2010) reference. This is standard cloud physics, there are extensive discussions in Pruppacher/Klett and in the Rogers/Yau textbooks.

References have been adjusted accordingly.

"atmospheric-attenuated backscatter" – Can't this be shortened to "attenuated backscatter", provided we accept that clouds are a component of the atmosphere? It seems the lidar literature typically refers to this as attenuated backscatter.

Shortened to "attenuated backscatter".

Related to this, there is a range correction, an overlap correction, and the removal of the molecular scattering that needs to be applied to the lidar detection to derive the attenuated backscatter. Are all three of these corrections discussed in THT?

All corrections are applied to the raw LIDAR signal before inverting it and retrieving the extinction. While it would be recommended to use the attenuated β every time when retrieving the aerosol information from the LIDAR return, it is not strictly necessary when retrieving boundaries (boundary-layer height or cloud base). THT is not sensitive to the applied corrections (apart from the correction for the overlap which is mandatory) and it is sufficient to work with the received power or the range corrected power.

P4831-L12 – You seem to be implying that the MWR reports temperature at cloud base. This needs to be explained in the methods section.

Not exactly: the temperature is retrieved all along the cloud thickness not only at the cloud base (see my previous comment on derivation of A_{SAT}).

Section 3.2 – Lidar Calibration – The basis for the calibration is return from molecular backscatter. Why discuss the calibration with a sun photometer?

The calibration with the sun photometer has been performed in clear-sky to calibrate the LIDAR signal. The so-calibrated signal has then been inverted through cloud to retrieve the extinction. The paragraph has been rephrased for clarity.

P4832-L13 – The lidar and ceilometer are the same thing, right? Why use "lidar/ceilometer" here, and why repeat CHM15K?

Replaced by only LIDAR.

P4833-L1 – LWC is proportional to the _concentration_ of cloud droplets.

Corrected.

P4839 – Where do you present the RH or absolute humidity? I don't see this data presented, yet it gets a page of discussion. I recommend that this part be omitted.

RH data are not shown in order not to overload the graphical part. However, we understand that the discussion would suffer the missing RH data and have decided to omit this part.

Section 4.2.1 – Here you need to acknowledge the fact that the derived ss is influenced by droplet removal due to entrainment and coalescence scavenging (drizzle formation). As a consequence, the ss you derive will be an underestimate of the value of the maximum(ss) reached during activation. Jim Hudson has written extensively on this and I recommend his work.

The entire Section 4.2.1 has been rephrased. The Reviewer's comment and the reference have been added.

P4842-L5 – Since you are dealing with active sensing of reflection, both in the near-IR and in the microwave, you should specify which reflectivity.

Done.

P4842-L16 – Be careful here….it is condensational growth, possibly some coalescence, which leads to large droplet radius. Not hygroscopic growth.

Corrected.