

Response to anonymous referee 1

We thank the reviewer for thorough and thoughtful comments. Our responses to each of the points are given below.

General comment

As this paper is rather process-oriented, impacts that the presented findings may have on the aerosol indirect effects (AIEs) from shipping emissions in terms of radiative forcing (RF) are not mentioned. As it is only a small step to establish the link from a change in albedo ΔA to RF, a short discussion of the implications (other than those for geoengineering) would be beneficial. As it is known that ship tracks lead to almost negligible negative RF anyway (e.g. Schreier et al. (2007)), the results shown in this study could actually lead to estimating the RF of ship tracks to about zero.

>> **Response:** The following discussion on radiative forcing has been added in the Conclusions: “The satellite data show that, on average, 75% of the ship tracks are brighter than the surrounding clouds. When taken as a whole, the polluted clouds are about 2 – 3 % more reflective than the surrounding clouds. The ship track radiative forcing at the local scale (averaged over numerous ship tracks) is on the order of ~ -10 to -20 W m^{-2} (e.g., Schreier et al., 2007; Christensen and Stephens, 2011). On the global scale, however, negligible radiative forcing from ship tracks has been observed (Schreier et al., 2007; Peters et al., 2011). In the current study, $\sim 25\%$ of the ship tracks produce a positive radiative forcing, the strength of which depends on the depth of the cloud (or decoupling) and the free-troposphere humidity. The current study employs ship track observations as means to assess the microphysics of aerosol-cloud relationships. Further studies are needed to quantify these effects on the global scale.” One can also estimate the radiative forcing from the ship track dataset. Given the goals of the present work, the change in cloud albedo is an appropriate metric.

P13554, L7: ship tracks should not generally be defined as “cloud regions impacted by ship exhaust” but are better described as “quasi-linear cloud features emerging in oceanic regions impacted by ship exhaust”. This is because by far not all cloudy regions influenced by shipping emissions show ship tracks, e.g. regions of shallow cumulus convection.

>> **Response:** We agree. The definition has been revised accordingly:” Ship tracks, quasi-linear cloud features prevalent in oceanic regions impacted by ship exhaust, are a well-known manifestation of the effect of aerosol injection on marine clouds.”

P13555, L16-25: The science presented in this paragraph should at least be accompanied by some references regarding the historical and recent advances in quantifying AIEs from shipping emissions using observations (such as for example Coakley et al.(1987), Platnick and Twomey (1994), Coakley and Walsh (2002), Segrin et al. (2007), Schreier et al. (2007), Campmany et al. (2009), Christensen and Stephens (2011), Peters et al. (2011), Christensen et al (2012).)

>> **Response:** The following sentence has been added in this paragraph: “There have been a number of observational studies of ship tracks, including in-situ airborne measurements (e.g., Radke et al., 1989; Ferek et al., 1998; Durkee et al., 2000; Twohy et al., 2005; Lu et al., 2007, 2009) and remote sensing satellite observations (e.g., Coakley and Walsh, 2002; Schreier et al., 2007; Segrin et al., 2007; Lebsock et al., 2008; Christensen and Stephens, 2011, 2012).”

P13556, L10: This formulation should be modified. It should be clear that it is not the ships but their emission plumes and their effect on cloud micro- and macrophysical properties which are probed.

>> **Response:** The sentence has been revised as follows: “Over 30 flights, approximately 45 cargo/tanker ship emissions and their effect on cloud microphysical and macrophysical properties were probed.”

P13556, L17-19: I am not an expert on in situ measurements of clouds, but are these thresholds for defining the “cloud” (N_d and LWC) generally accepted values? Generally, how is measurement uncertainty accounted for? Supplying uncertainty estimates of the retrieved micro- and macrophysical cloud properties and discussion thereof would be of great benefit.

>> **Response:** Various studies have used different LWC thresholds to define cloud regions. The threshold of LWC 0.01 g m^{-3} has been generally applied in a number of studies (e.g., Wang et al., 2010; Chen et al., 2011; Shingler et al., 2012). In Lu et al. (2007), a cloud layer is determined using the threshold of $N_d > 5 \text{ cm}^{-3}$ and $\text{LWC} > 0.001 \text{ g m}^{-3}$. To ensure that the aircraft was in a cloudy region, a more strict threshold of $N_d > 10 \text{ cm}^{-3}$ and $\text{LWC} > 0.01 \text{ g m}^{-3}$ was applied in this study to provide a degree of conservatism. Concerning the uncertainty, the following sentence has been added: “In situ measurements are subject to a variety of uncertainties and limitations. The measurement uncertainty of the probes is documented in several studies (e.g., Baumgardner et al., 2001; Conant et al., 2004; Lance et al., 2010). The probes were repeatedly calibrated during the E-PEACE field mission.”

P13556, L21: From this equation alone it is not clear how the LWP is obtained in detail, e.g. how many cloud levels are used for the integral? It is just later in the manuscript where the reader is informed that the whole depth of the cloud layers is sampled. This information should be included here, otherwise this does not make sense.

>> **Response:** The paragraph describing the four cases has been moved forward (before the equations for different parameters). Also, the following sentence has been added: “Based on the entire cloud layer profile sampled in these four flights, LWP can be estimated using $\text{LWP} = \dots$.”

P13557, L7-15: The assumptions leading to the derivation of Eq. 3 must be explained in more detail.

>> **Response:** The following has been added: “The assumption of adiabatic conditions in Eq. (2) may not be appropriate for heavy drizzling clouds and/or partly cloudy (i.e., open cell cloud) conditions (e.g., Hayes et al., 2010). The comparison of cloud albedo susceptibility using both Eq. (1) and Eq. (2) will be investigated to evaluate the effect of the adiabatic assumption (Sect. 3.1).”

The effect of adiabatic assumption has been examined and added into Figure 4. The following has also been added in Section 3.1 last paragraph: “Comparing the cloud albedo susceptibility estimated from analytical formulation based on adiabatic assumption (Eq. (3)) and derived using droplet size spectrum (Eq. (1)), the two derived albedo susceptibilities exhibit only small difference for non-precipitating and light-drizzling clouds (RF18, 19, and 24, as shown in Fig. 4). However, for heavy drizzling cloud with open cell structure (RF20), the cloud albedo susceptibility derived using Eq. (1) is about twice as large as that from Eq. (3), indicating that the assumption of adiabatic condition is not applicable for heavy drizzling clouds (see also Hayes et al., 2010).”

P13557, L19-22: The description of the cloud thickness response is confusing and should be split into at least two sentences. It should be clear that precipitation suppression does not lead to marine boundary layer cooling (due to the lack of evaporative cooling), but rather enhances entrainment and thus cloud thinning (Wood, 2007).

>> **Response:** The sentences have been revised as follows: “The cloud thickness response, which can be either positive or negative, is determined by the balance between (a) the moistening/cooling of the marine

boundary layer resulting from precipitation suppression, and (b) drying/warming resulting from enhanced entrainment due to increased turbulence (Ackerman et al., 2004; Wood, 2007). Precipitation suppression does not always lead to moistening of the MSc; under certain conditions, it can enhance entrainment and lead to cloud thinning.”

P13558, L2: Replace “cloud optical depth” with “cloud albedo” for consistency

>> **Response:** This has been changed accordingly.

P13558, L1-6: The impact of shipping emissions, or anthropogenic emission of aerosols and aerosol precursors in general, on clouds is not as straight forward as it seems from the text. The manuscript suggests that an increase in aerosol number directly translates to an increase of N_d . This is however not the case as factors such as the number of emitted aerosol particles acting as CCN, the background aerosol concentration and the cloud susceptibility must be accounted for. This should be commented on.

>> **Response:** The sentences in question have been changed as follows: “The three effects represented in Eq. (3) are the major ones governing the response of cloud albedo to a perturbation in cloud droplet number concentration. As an increase in emitted aerosol particles can lead to an increase in N_d (the strength of which depends on background aerosol number concentration, particles acting as cloud condensation nuclei, etc), Eq. (3) can be applied to the ship exhaust observations, expressing the change between the unperturbed clouds, subject only to the marine background N_a (thus background N_d), and those perturbed by ship exhaust.”

P13558, L12-14: What kind of situations were sampled in the other research flights of the campaign? From what I know, E-PEACE was specifically designed to investigate the impact of shipping emissions on clouds. So why are only four of the total 30 flights used in this study?

>> **Response:** The following sentences have been added to clarify the experimental design of the E-PEACE campaign: “In the E-PEACE campaign, three types of particle sources were used as cloud perturbations: (a) combustion exhaust from large container/tanker vessels (dry diameters 50 – 100 nm); (b) organic smoke generated onboard the Research Vessel Point Sur (dry diameters 100 nm – 1 μ m); and (c) aircraft-emitted milled salt particles (dry diameters 3 – 5 μ m). The present work focuses on the effect of combustion exhaust from large container/tanker ships. Over the 30 flights carried out, approximately 45 container/tanker ship emissions and their effect on cloud microphysical and macrophysical properties were probed. Several flight strategies were applied. In most flights, the aircraft executed a zigzag pattern in and out of the plume, with below cloud, in-cloud (cloud base, mid-cloud, cloud top), and above cloud legs. In four of the flights, spiral soundings and/or slanted ascents (Fig. 2) were conducted in areas clearly influenced by the ship exhaust and in adjacent areas relatively free of ship exhaust, from which the cloud base/top heights, LWP, and cloud albedo, using the vertical boundary layer profile, were obtained. This strategy of spiral sounding and slanted ascents proved ideal to probe the response of cloud properties with respect to ship-emitted particles: ship exhaust and background marine aerosol below cloud, in cloud, and above cloud were probed, with the perturbed clouds subject to the same background meteorological conditions as those outside the region of exhaust impact. We focus here on these four research flights during E-PEACE.”

P13558, Section 2.2: It should be more clear from the beginning of this section that data produced in the framework of the Christensen et al (2012) study is used. Some comments should be made on the data products used and on issues of data quality screening (especially for MODIS data).

>> **Response:** The following sentence has been added in the beginning of this section: “The A-Train satellite observations stemmed from the framework of Christensen and Stephens (2012).”

Concerning the data product and data quality screening, the following has been added: “Droplet effective radius and cloud optical thickness were derived from the 3.7- μm reflectances and obtained using the MODIS cloud product (MYD06, King et al., 1998). One-kilometer pixels were screened to include only those with full cloud coverage and fitting the requirement of a single layer, low-level (cloud top pressure greater than 600 hPa), and warm phase cloud. The screening criteria are similar to those applied in Christensen and Stephens (2012).”

P13559, L3-4: Is this a standard method for deriving r_e and LWP? Is it supplied with the data or did the authors use their own retrieval algorithm? Please give a reference for this.

>> **Response:** r_e was derived from the 3.7- μm reflectances and obtained using the MODIS cloud product, as described in previous comment (P13558, Section 2.2). The method for deriving LWP has been added: “LWP was derived from the effective radius and optical depth through $LWP = (2/3)\rho_w r_e \tau$ (Stephens, 1978), assuming that the cloud contains spherical droplets and that liquid water content follows an adiabatic vertical profile. These assumptions lead to ~30 % errors at the pixel scale as derived from Bennartz et al. (2007). Therefore, numerous pixels, a minimum of 30 for a ship track, were grouped together into segments to reduce the uncertainty, thereby producing a more representative average of the cloud optical properties derived from MODIS.”

P13559, L5: radiative -> radiative

>> **Response:** The word has been corrected.

P13559, L16: Please specify “ideal conditions”

>> **Response:** In these four research flights, the spiral soundings and slanted ascents provided ideal conditions to investigate the changes in cloud base/top heights, as explained in the previous comment on P13558, L12-14. To avoid confusion, the sentence has been revised: “During four of the research flights (RF18, 19, 20, and 24) with spiral soundings and/or slanted ascents, the responses of cloud properties with respect to ship-emitted particles were probed.”

P13559, L20: ship plumes -> ship emission plumes

>> **Response:** This has been revised accordingly.

P13560, L7: “significant” is a statistical term, replace it with something like “substantial, large, ...”

>> **Response:** The word has been changed to “substantial”.

Table 2: Concerning the measurements for RF20. The aerosol particle number concentrations N_a are substantially lower compared to the other research flights. While this seems reasonable for the clean conditions (it may just be an exceptionally clean boundary layer with depleted aerosol due to precipitation), N_a is up to a factor of 5 lower in the polluted parts compared to the other flights. Why is this the case? Was the aerosol particle size distribution also measured? If so, then the aerosol particles should be considerably larger in RF20 compared to the other flights (if the emitted particle size distribution was approximately the same for all flights).

>> **Response:** In RF20, the heavy drizzle in the unperturbed cloud led to substantially lower aerosol number concentration. Under the impact of ship exhaust, the increase in aerosol led to slightly lower

precipitation, but it did not totally suppress the drizzle. It is shown in Table 2 that the cloud base rain rate is 11.40 mm d^{-1} in the perturbed cloud (compared to 12.53 mm d^{-1} in the relatively clean cloud), with a large mean cloud effective radius ($16.01 \mu\text{m}$). Due to the heavy precipitation in the polluted condition, N_a was depleted and thus was much lower compared to other polluted cases.

P13560, L10: Table 3 shows an albedo increase of 82% and not 83% as in the text. Please correct one or the other for consistency.

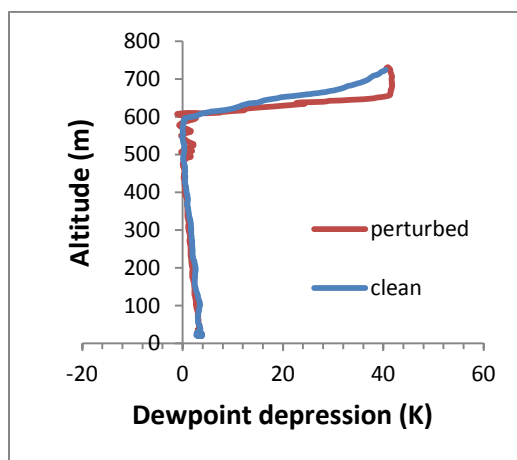
>> **Response:** This has been corrected to 82% both in Table 3 and in the text. Thank you for pointing this out.

P13560, L20: Please be clear that this sedimentation effect holds for clouds exhibiting smaller cloud droplets than those present in a reference cloud.

>> **Response:** The sentence has been revised as follows: “As the perturbed cloud droplet size near cloud top becomes smaller (Fig. 2), reduced sedimentation near the cloud top entrainment zone tends to cause more efficient cloud top evaporation, enhancing turbulent kinetic energy and entrainment, and leading to smaller LWP and a thinner cloud (Bretherton et al., 2007).”

P13561, L1-4: The dewpoint depression for RF 18 seems extremely high to me (40 K). This must be an exceptionally dry free troposphere. I would be interested in seeing a plot of the atmospheric thermodynamic profiles for this particular situation. Why isn't a more familiar expression for free tropospheric moisture, like relative humidity, used? Personally, I find a particular dewpoint depression hard to put into context.

>> **Response:** Please see the figure below for the dewpoint depression for RF18 (vertical profile based on the slant sounding for clean and perturbed condition, respectively). We use the dewpoint depression to express free tropospheric moisture for in-situ measurement, allowing a direct comparison with satellite data. Relative humidity is measured on the aircraft, but is not available from the satellite data.



P13561, L7-9: From these two sentences, it is not entirely clear to me what mechanism for cloud thinning is suggested. The scientific reasoning should be presented in more detail here.

>> **Response:** Cloud thinning is a result of entrainment drying/warming as described in earlier sentences. Since the two sentences in question lead to confusion, they have been rephrased as the follows: “As the boundary layer dried, the cloud became thinner, with higher cloud base and lower cloud top.”

P13561, L9-12: The use of lower tropospheric stability LTS is VERY confusing here (and throughout the rest of the paper). Traditionally, LTS is defined as $LTS = \theta_{700} - \theta_0$ (Slingo 1987; Klein and Hartmann 1993; Klein 1997; Wood and Hartmann 2006).

According to the footnote of Tab. 3 in the manuscript, LTS is defined as “ $\theta_{925\text{mb}} - \theta_{\text{sfc}}$ in this study. This must also be mentioned in the main text. Why is this definition used in this study? I suppose this is because according to the US standard atmosphere, a pressure of 925hPa roughly corresponds to a height of 766m above sea level, i.e. more than 100m above the clouds in this case. Would the conclusions be different if the original formulation of LTS were used?

Assuming the use of this definition of LTS is appropriate, the arguments explaining the influence of high LTS values on the boundary layer moisture are wrong. In the paper it is suggested that high values of LTS “led to a diminished moisture supply from the ocean surface, and thus a drier boundary layer”. However, it is well known that it is exactly the other way around, namely that high LTS values act as a lid on the marine boundary layer, thereby supporting it to be rather well-mixed and moist (e.g. Wood and Bretherton (2006)). This should be commented on and corrected in the manuscript.

>> **Response:** In this work, LTS was defined as $\theta_{925\text{mb}} - \theta_{\text{sfc}}$ based on in-situ measurements because the aircraft did not reach an altitude of 700 mb, and therefore the standard definition of LTS ($\theta_{700} - \theta_0$) could not be used (e.g., Klein and Hartmann 1993; Klein 1997; Wood and Hartmann 2006). To avoid confusion on this point, we have removed the analysis and description of LTS as it is not essential to the work.

P13561, L19: is consistent with -> can be derived from / P13562, L2: susceptibility of cloud albedo

>> **Response:** These words have been changed accordingly.

P13561, L28: What would be the uncertainty associated with these relative albedo changes?

>> **Response:** The cloud albedo is calculated based on $\tau = \int \int 2 \pi r^2 n(r) dr dz$, as noted in the paper. As τ is calculated based on the droplet size spectrum and the integral over altitude, a single value of τ is obtained from each spiral/slanted sounding. Therefore, albedo uncertainty cannot be estimated (as well as the uncertainty associated with the relative albedo changes).

Fig. 4: change the legend for the black dot from “cloud susceptibility” to “cloud albedo susceptibility” or similar. The term albedo should be mentioned in the legend in any case.

>> **Response:** The legend has been changed accordingly.

P13562, L6-8: The reduction in drizzle rate also is a factor contributing to an increase in LWP, right?

>> **Response:** The sentence has been revised as follows: “In these two cases (RF19 and RF20), the relatively moist overlying air led to less efficient entrainment drying, together with reduction in drizzle, resulting in higher LWP and albedo (cloud brightening).”

Section 3.2 (P13562): Throughout this section, it should be made clear that the dataset used stems from the Christensen et al (2012) study. Data issues and quality control must be accounted for. See also my previous comment regarding the description of used data.

>> **Response:** Please see the response to comment P13558, Section 2.2 for the dataset, data issues and quality control. These comments have been addressed in Section 2.2 where the satellite data are described.

P13562, L18: Please provide a reference for the re-analysis dataset. How does this dataset compare to observations in the lowermost troposphere?

>> **Response:** The reference to CloudSat ECMWF-AUX product documentation has been added: Partain, P. 2004, Cloudsat ECMWF-AUX Auxiliary Data Process Description and Interface Control Document, Cooperative Institute for Research in the Atmosphere, Colorado State University, Fort Collins, CO 80523. A description of the ECMWF-AUX data product:

Benedetti A., 2005, CloudSat AN-ECMWF Ancillary Data Interface Control Document, Cooperative Institute for Research in the Atmosphere, Colorado State University, Fort Collins, CO 80523.

Also, in regards to the lower troposphere:

In general, there is good agreement in the estimation of LTS and free-tropospheric moisture between the Atmospheric Infrared Sounder (AIRS) and ECMWF re-analysis (Yue et al., 2011). LTS represents the bulk state of the atmosphere between the surface and roughly 3 km. Due to the coarse vertical resolution (250 m) of the ECMWF re-analysis product, the height and strength of a temperature inversion generally found above stratocumulus are poorly resolved. The average uncertainty in the model temperature and humidity fields near the surface and in the free-troposphere is approximately 15% compared to the ERA-40 analysis (Benedetti, 2005). As the analysis of LTS has been removed, an LTS comparison is not added.

P13562, L20-22: This is not evident from the observations. Please add a reference supporting this statement.

>> **Response:** The following sentence has been added: “This result is similar to that of Wood (2007), in which increasing droplet number concentration leads to cloud thinning in clouds with higher cloud base height (particularly those higher than 400 m).”

P13562, L22-24: In my view, this statement, although scientifically plausible, lacks sound evidence from the data shown in Fig. 5 and Fig. 7. In Fig. 5 it is shown that clouds with reduced albedo exhibit substantially higher cloud tops (Fig. 5c), but the response in r_e is only marginal and it shows that clouds with reduced albedo seem to be more susceptible to aerosol perturbations, i.e. the mean r_e is smaller. The results shown in Fig. 7 also do not support this statement as it is found that the relative change in r_e in polluted vs. unpolluted clouds is quite insensitive to changes in cloud top height. This should be commented on and corrected in the manuscript.

>> **Response:** In Figure 5 we do not show responses between polluted and unpolluted clouds. These parameters are the average properties of the ambient (unpolluted) clouds for cloud brightening/dimming regimes (except dewpoint depression; it is averaged over the entire domain of the ship track). As the data in the plot were not clearly explained in the manuscript, the following has been added in the caption of Figure 5 to avoid confusion: “The cloud top height, effective radius, and optical depth are averaged over the unpolluted cloudy sections of each ship track.”

As noted, the difference in r_e for the background clouds is marginal between two regimes (in Fig 5). However, in Figure 7, it is shown that the response caused by the ship plume is large (~20% reduction in cloud droplet size) regardless of the cloud top height.

Figure 7 shows that the effective radius response is roughly constant with height (polluted clouds have smaller droplets regardless of the depth of the boundary layer) with a slightly weaker response for the higher clouds. This might simply be the result of greater aerosol plume dispersion in a deeper boundary layer as was found in Durkee et al. (2000). The following sentence has been added: “Despite the higher cloud tops in the cloud dimming regime, smaller droplets in the ambient clouds were observed, suggesting that droplet growth was suppressed in an environment of drier air above cloud tops.”

P13563, L4: Beginning this sentence with “Based on satellite data” yields some confusion as this suggests some direct link to the previous sentence. A better way to start the sentence would be “Contrary to our results from in-situ observations, the effect...”

>> **Response:** The sentence on LTS has been removed, as the LTS comparison between in-situ and satellite data can lead to confusion due to the inconsistent LTS definition.

P13563, L4: The use of two different definitions for LTS is confusing. Please use a consistent definition throughout.

>> **Response:** As the two different LTS definitions are confusing, the analysis on LTS has been removed; see response on P13561, L9-12.

P13563, L15: Please provide a reference for this equation, e.g. Twomey (1991) or similar

>> **Response:** Equation (4) is obtained from Eq. (3) with the neglect of the dispersion and cloud thickness effects: $\frac{dA}{dN_d} = \frac{A(1-A)}{3N_d}$. This can be written in the form of: $\frac{\Delta A}{A(1-A)} = \frac{\Delta N_d}{3N_d} = \frac{1}{3}\Delta(\ln N_d)$. Therefore no reference for this equation is needed.

P13563, L24: influenced -> increased / P13563, L25: loss -> reduction / P13564, L3: can be seen (Fig. 7) -> are depicted in Fig. 7

>> **Response:** These have been changed accordingly.

Fig. 7: How are the error bars for the albedo change (black curve) defined? Please also give uncertainty estimates for the other functional dependencies.

>> **Response:** The following sentences have been added in the caption of Figure 7: “Error bars were determined from the standard deviation of average cloud albedos taken from the population of ship tracks in each bin. The length of the error bars extends over two standard deviations; i.e., the bar extends one standard deviation below and one above the mean for each bin.”

P13564, L4: insert “on the change in cloud droplet effective radius”

>> **Response:** It has been added accordingly.

P13564, L7: It would be interesting to look at the change in cloud top height with free tropospheric moisture. According to the results shown, the two should be anti-correlated, i.e. a drier free troposphere results in higher but thinner clouds. This follows from the response in LWP shown in Fig. 7. Why is this the case? This should be commented on.

>> **Response:** This was assessed and is shown as the pink line in Figure 7. The cloud thickness response induced by the ship plume is roughly constant as the free tropospheric moisture decreases. This shows that the cloud thickness and free tropospheric moisture are not clearly correlated or anti-correlated.

P13564, L10: This sounds as if the clouds determine the free tropospheric moisture. I would intuitively assume that it is rather the free tropospheric humidity given by the large scale state which determines cloud properties.

>> **Response:** The sentence has been revised as follows: “As the cloud albedo response follows closely the LWP response, the cloud brightening diminished under drier free troposphere or higher cloud top heights.”

P13564, L11: Insert something like “Therefore” or “In conclusion” at the beginning of this sentence.

>> **Response:** The following sentence has been added at the beginning of the paragraph: “Clouds were classified as closed cell, open cell, unclassifiable, or others.”

Figure 8: Although I find this figure very informative and enlightening and acknowledge the effort put into creating it, I do not think that including it is of any especial benefit for the paper. Such a figure would be better placed in a general overview or review paper and I therefore suggest to omit it from the manuscript. The subtleties of quantifying aerosol effects on clouds and precipitation have been adequately described elsewhere and these could be referenced in the conclusions (e.g. Stevens & Feingold (2009)).

If the authors do decide to keep the figure, then the role of the meteorological environment in determining the cloud albedo should be incorporated differently. The way it is shown now is that large scale conditions only act to reduce cloud thickness, whereas it is known that this is not true for at least one variable, namely LTS (large values of LTS promote thicker clouds (e.g. Wood and Bretherton (2006))).

Please provide a reference for the last sentence of the figure caption.

>> **Response:** Figure 8 is intended to illustrate the key processes regulating the cloud albedo response. We would like to retain this figure. The figure legend has been revised as follows: “Conceptual diagram displaying the interactions among aerosol, cloud, precipitation, and meteorology. The response of each property/phenomenon to increased aerosol (N_a) is shown as a red plus (signifying positive response), and a blue minus (negative response) sign. Footnotes to figure: (1) Twomey effect (Twomey, 1991). (2) Albrecht effect (Albrecht, 1989). (3) Sedimentation-entrainment effect (Ackerman et al., 2004). (4) Drizzle-entrainment effect (Wood, 2007). (5) Significant meteorological conditions, such as free tropospheric humidity (q_{ft}), large scale divergence rate, as well as cloud top height (z_i), can control the MSc structure (Wood, 2007; Chen et al., 2011).”

Also, the following has been added in the main text in conclusions: “The so-called Twomey and Albrecht effects can lead to cloud brightening and thus cooling. On the other hand, in response to an aerosol perturbation, reduced in-cloud sedimentation leads to an increase of cloud water and evaporation in entrainment regions, resulting in stronger entrainment (Ackerman et al., 2004; Bretherton et al., 2007). Besides, less drizzle reduces below-cloud evaporative cooling and in-cloud latent heat release, resulting in higher turbulent kinetic energy and thus stronger entrainment (Wood, 2007).”

The effect of meteorological conditions on the entrainment effect has been revised. LTS has been removed from the Figure.

P13564, L21: delete “, and ”

>> **Response:** The sentence in question has been removed (the description of LTS).

P13564, L23-24: Here it should be noted that these arguments are based on the findings of Ackerman (2004) and Bretherton et al. (2007) (the sedimentation effect)

>> **Response:** This has been added as follows: “In response to an aerosol perturbation, reduced in-cloud sedimentation leads to an increase of cloud water and evaporation in entrainment regions, resulting in stronger entrainment (Ackerman et al., 2004; Bretherton et al., 2007).”

P13564, L25-26: This is wrong. High values of LTS generally promote a moist and well-mixed marine boundary layer (e.g. Wood and Bretherton (2006)).

>> **Response:** Due to the problematic nature of defining LTS based on in-situ measurement, this sentence has been removed. (See response to P13561, L9 – 12)

P13565, L4: deeper -> higher

>> **Response:** It has been changed accordingly.