

Interactive comment on “Occurrence of lower cloud albedo in ship tracks” by Y.-C. Chen et al.

Anonymous Referee #1

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In this paper, an analysis of the cloud albedo response in ship tracks, i.e. one of the most compelling pieces of evidence of aerosol indirect effects, using in-situ and satellite observations is presented. Normally, ship tracks are associated with enhanced cloud albedo compared to the surrounding unperturbed clouds – a response expected from first and second aerosol indirect effect considerations. Here, compelling evidence of the possibility of an albedo reduction in ship tracks is presented.

The analysis is mainly based on an equation which approximates the cloud albedo response per change in cloud droplet number concentration N_d . This equation includes micro and macrophysical effects on cloud albedo. Microphysical effects are the change in N_d (Twomey-effect) and droplet size distribution breadth. The macrophysical effect of changing cloud liquid water path LWP is approximated using the change in cloud geometrical thickness. The equation is derived using the approximations for cloud

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albedo presented in Lacis & Hansen (1974) and for cloud optical depth presented in Brenguier et al. (2000). Generally, the change in cloud albedo is dominated by the macrophysical response (which is not a surprise by itself), i.e. a decrease in cloud thickness for scenes showing a reduction of cloud albedo in ship tracks. It is argued that this macrophysical response results from a combination of cloud droplet sedimentation effects (Ackerman et al. (2004), Bretherton et al. (2007)), leading to enhanced entrainment and thus cloud thinning, and a particularly dry air layer above the cloud.

In light of the ongoing debates on geo-engineering, e.g. increasing marine stratocumulus cloud albedo through injection of sea-spray aerosols, the results presented in this study highlight the subtleties of aerosol-cloud interaction, using the prime-example of ship tracks, and merit publication in ACP.

The paper is well structured, the scientific basis is adequately described and the figures are well chosen and produced. The text is very concisely written and although being concise is often of advantage, the authors sometimes miss to mention important information which then leaves the reader baffled and longing for more elaborated text. Specifically, the applied methods are not always thoroughly enough explained and the scientific explanation of observed cloud responses needs a more thorough consideration and/or rethinking in some instances.

I recommend the manuscript to be published in ACP after the following remarks/comments have been adequately addressed.

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

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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.

Specific comments

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.

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).)

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.

P13556, L17-19: I am not an expert on in situ measurements of clouds, but are
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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.

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 the i's do not make sense.

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

Assuming adiabatic conditions may be appropriate for closed decks of stratocumulus. However, it is known that this assumption breaks down for broken cloud fields (e.g. Hayes et al. (2010)). Furthermore, the adiabatic assumption does not hold for drizzling (or even heavy-drizzling as in RF20) clouds. Evidence for the validity of the assumptions leading to the use of Eq. 3 throughout the paper should be given.

The approximation used for the albedo (Lacis & Hansen (1974)) holds for horizontally homogenous scattering layers. Although appropriate for closed decks of stratocumulus clouds, especially the case of ship tracks in open cell regimes may not represent homogenous scattering cloud layers. This should be commented on.

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).

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

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.

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 ?

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).

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.

P13559, L5: raidative -> radiative

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P13559, L16: Please specify “ideal conditions”

P13559, L20: ship plumes -> ship emission plumes

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

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).

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.

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

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

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humidity, used ? Personally, I find a particular dewpoint depression hard to put into context.

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.

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 " $\theta_{925mb} - \theta_{sfc}$ " 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.

P13561, L19: is consistent with -> can be derived from

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

P13562, L2: susceptibility of cloud albedo

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.

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

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.

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

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

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.

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...”

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

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

P13563, L24: influenced -> increased

P13563, L25: loss -> reduction

P13564, L3: can be seen (Fig. 7) -> are depicted in Fig. 7

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

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

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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.

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.

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

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.

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P13564, L21: delete “, and ”

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)

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)).

P13565, L4: deeper -> higher

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 13553, 2012.

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