

Interactive comment on “Using surface remote sensors to derive mixed-phase cloud radiative forcing: an example from M-PACE” by G. de Boer et al.

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1 General remarks

The manuscript provides estimates of the radiative forcing of mixed-phase clouds using a combination of remote sensing measurements and radiative transfer simulations. The lidar and radar remote sensing measurements of cloud properties including cloud liquid and ice water path, ice particle size, cloud geometrical depth are used as input of the radiative transfer model to derive the solar short wave and terrestrial longwave radiative forcing. To accurately simulate the longwave simulations radio soundings are

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used which limits the retrieval to the times of the radio sonde launch. The calculated irradiances have been compared to measurements provided by the ARM site. The radiative forcing was analyzed in dependence on different cloud properties.

An investigation of the radiative forcing of mixed-phase clouds and the development of new methods to derive the cloud radiative forcing is highly welcome and worth to be published. The approach presented by the authors to derive the radiative forcing by a combination of remote sensing measurements and radiative transfer simulations is within the scope of ACP and may help to improve the uncertainties in the radiative forcing estimates for mixed-phase clouds. However, I do not see a clear focus of the methods and results presented here. Though the authors claim to test a new retrieval method and quantify the uncertainties of the retrieval, rather than giving a general answer on the radiative forcing of such clouds, they practically do it the other way around. The cloud radiative forcing is the main quantity discussed, while the aim to quantify the uncertainties in the method is not touched at all.

The problem I see with this shifted focus is, that the limited cases of measurements, will hinder to give any useful general conclusion on the radiative forcing of mixed-phase clouds. This is what the authors already agreed with in their paper.

Before the paper can be considered for publication, the authors have to show clearly in which direction the paper may accomplish a new valuable contribution for the science community. This direction has to be targeted with a stronger focus and discussed thoroughly. I see this chance only by significantly extending the characterization of the new retrieval. How the retrieval differs from known methods? How large are the uncertainties of the retrieval? How the results of the cloud radiative forcing differs compared to known methods applied for the same cloud cases?

Below, I compiled a list of comments which have to be considered in a revised version of the paper. When writing the comments I did not consider, which direction the revised paper may have. This may result in some contradictory statements. I am sure the

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authors will know how to weight in such cases.

2 Major comments

Focus: The paper suffers from failing the aim claimed by the authors. E.g., in **P12489, 6** the authors state that the central goal of the study is not to provide estimates on the cloud radiative forcing. This differs from the paper content, where the cloud radiative forcing is the main quantity discussed. In **Section 3.2**, the authors try to find relations between cloud parameters and the cloud forcing and discuss in **Section 3.3**) how the radiative transfer is affected by several parameters. Shupe and Intrieri (2004) have already discussed the effects of different parameters on the radiative forcing of mixed-phase clouds. The data set used by Shupe and Intrieri (2004) is much larger than the data provided within this paper and thus allows more general conclusions on these effects. Contrarily, the study presented by the authors is limited to 16 cloud cases only, which additionally are not distributed over the entire year. In this regard I doubt, that the results presented here give any new contribution to the science community.

The original aim of the study, to present a new method to derive the cloud radiative forcing, was introduced, the methodology described but the results not discussed with regard to measurement uncertainties. Only the variance within the cases was calculated, which gives an indication of the cloud variability but does not characterize the retrieval method itself. But how uncertainties of the measurements do propagate into the radiative transfer simulations and the cloud forcing?

I am also missing a discussion of how the measurements differ from Shupe et al. (2008, Vertical Motions in Arctic Mixed-Phase Stratiform Clouds, J. Atmos. Sci., 65, 1304-1322). Shupe et al. (2008) use the same instrumentation and partly the same measurements presented by the authors. They provide similar cloud properties and a complete characterization of the clouds observed during M-Pace. Surprisingly, the

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Shupe et al. paper is not even cited here. The empirical relationship to calculate the IWC (**P12492, 10**) also differs from Shupe et al. (2008). Here the authors refer to an older equation tuned to measurements during SHEBA (Shupe et al. 2006). I wonder if the authors did know about the Shupe et al. (2008) paper. In a revised version the findings of Shupe et al. (2008) have to be considered and the innovative part of the methods presented here pointed out more clearly.

Furthermore, I can not agree to the argumentation on **P12490, 28**. If you are able to run lidar, radar and radio sonde launches at a certain measurement site, it should be possible to run relatively simple radiation measurements as well. This can not be a motivation to start this huge effort. There are other arguments like the additional information on cloud properties gained by radar and lidar measurements. These can be used to provide parameterizations of the cloud forcing. If only the radiative forcing would matter, I would not use this complex method involving radar, lidar and radiative transfer simulations.

P12495, 9, Figure 2 and 3: The percentiles shown in Figure 2 and 3 result only from the statistics of the measurements within each of the 16 cases. But, what is about the measurement uncertainties or the uncertainties in the radiative transfer simulations caused by uncertainties in the cloud retrieval. These have not been plotted or specified anywhere in the manuscript (see Shupe et al. 2008). The discussion of uncertainties, measurement biases, etc. is absolutely crucial for the manuscript as the authors intend to assess the uncertainties of the retrieval method. In **P12490, 27** the authors claim to "quantify uncertainties associated with using this technique". This is the last time the word "uncertainty" is used in the manuscript!

Section 3: In Section 2 the authors introduce 16 cases of observations. However, in the following analysis often all measured data points are presented and discussed. This is inconsequential. Furthermore, I don't understand, why the authors have to average the measurements for the 16 cases if later they present and discuss all measurement points. If the averaging is needed to overcome noise in the measurements,

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than single measurements should not be shown. On the other side, if each single measurement is a proper data point, than there is no need to average.

If the second holds, I suggest to remove the averaging into 16 cases and always show all measurements. This also would be the best choice from a radiative transfer point of view to avoid any systematical error in the calculation of cloud radiative forcing caused by averaging. As shown in Figure 1 and later in Figure 4 and 6, the variability of cloud parameters is quite high within one case. However, when averaging the measurements, nonlinear effects may occur as radiative transfer is nonlinear. E.g. picking two single measurements with different cloud optical thickness will give a different mean radiative forcing for a) first averaging the cloud properties and then calculating the radiative forcing and b) first calculating the radiative forcing and then afterwards averaging the forcing. For this reason, all single measurement should be considered without calculating mean values for the 16 cases. Table 2 and 3 should be removed. Figure 2, 3, 7 and 8 should show all single measurements.

P12500, 20: Figure 6 has shown that the solar zenith angle is the most significant factor determining the absolute value of the cloud radiative forcing. This is obvious and well known. However, the authors continue to discuss the impact of other cloud parameters without considering this fact a priori. Often it is shown that the dependence of the cloud forcing on other parameters does not show up due to the domination effect of the solar zenith angle (**P12500, 27** or **P12501, 20**). In doing so, partly obvious findings are discussed which is not necessary because they are well known. In order to remove the impact of the solar zenith angle, I suggest to normalize the cloud radiative forcing and all shown irradiances by the incoming solar radiation or the cosine of the solar zenith angle. In this way the discussion can be focussed more clearly on the impact of cloud properties. At some passages (**P12500, 20**) this have already be done, but not consequently.

P12501, 11: How the radius was changed for the sensitivity study? Was it by keeping the WP constant or did the authors fixed the optical thickness. These are two different

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basic assumptions, which lead to different results as discussed by Wendisch et al. (2005) and Ehrlich et al. (2008). From a measurement point of view, it is the WP which is given. So I suggest to keep this constant. But then the resulting new optical thickness of the clouds has to be calculated and presented in the paper. To understand the impact on the radiative transfer, the changes in the optical thickness are essential. The same would hold for the second option. Changing R_{eff} when keeping the optical thickness constant will change the WP.

Section 3.3: The intention of Section 3.3. was to test the retrieval method presented in the paper with regard to uncertainties in the unknown parameters $r_{\text{e,liq}}$, surface albedo and surface temperature. First, I don't understand why the surface irradiances are discussed here and not the radiative forcing, which is the final output of the method. Second, a discussion is missing which tells how severe the assumptions on these three unknown parameters are for the retrieval of cloud radiative forcing. What are the consequences for the forcing retrieval if $r_{\text{e,liq}}$, surface albedo and surface temperature are unknown. Is it necessary to know the $r_{\text{e,liq}}$ exactly or not? How accurate surface albedo and surface temperature have to be assumed to not rise the error in the cloud radiative forcing above a certain level.

After reading this section it looks as if the authors again try to give some general conclusion on the behavior radiative transfer (upwelling, downwelling irradiance) in such kind of clouds. By doing so, the main focus of the paper was forgotten. This was the retrieval of cloud radiative forcing by a new method and its uncertainties.

Section 4: The paper investigates mixed-phase clouds. However, the analysis of the cloud radiative forcing presented by the Authors does not deeply consider the potential differences between pure liquid water clouds and mixed-phase clouds. What is missing from my point of view, is a discussion on how the cloud radiative forcing depends on the mixed-phase character of the clouds. Are there any differences between liquid water clouds and mixed-phase clouds at all? Do ice crystals matter at all or for what ice fraction we have to consider ice crystals in the cloud radiative forcing calculations?

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Considering the remote sensing measurements I could ask, if there is any need to characterize the ice crystals to derive the cloud radiative forcing. Or would it be sufficient to quantify the liquid part of the clouds.

Wording Some passages of the paper show a nonscientific wording. Also often well known relations are discussed in a figurative language. Examples are commented below. This way of writing may let the reader assume, that the authors are not totally familiar with the topic of radiative transfer presented in the paper. As I suppose, that this is not the case, I suggest, to revise the paper with focus on a more plain scientific wording.

3 Minor comments

P12488, 11: "Flux" is not the exact word for the quantity discussed in the manuscript. Correct is radiative flux density or irradiance. This is given by the units $W m^{-2}$. Radiative flux has the unit W .

short title: The short title "Remotely-sensed cloud radiative forcing" is not appropriate. The radiative forcing is not remotely sensed by radar and lidar. The authors calculate the forcing with a radiative transfer model using cloud properties from the retrieval. In my opinion these are two different steps, remote sensing and radiative transfer simulations.

P12488, 2: Specify which ground based measurements, instruments have been used. This is essential and should be given in the abstract as well.

P12488, 7: "coincidence", better use overlap.

P12488, 20: temperature sensitivity?

P12489, 4: Change "multiple phases of liquid" into "multiple phases of water".

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P12489, 14-25: Is this all part of Curry and Ebert (1992)? During reading I was confused due to the sentence structure. If so, please once more refer to Curry and Ebert (1992) at an appropriate location.

P12489, 20: Which estimates are meant here?

P12489, 21: Which early work has been referred here?

P12490, 13: During what time of year the forcing shows a peak? Summer/Winter?

P12490, 20: Which kind of measurements was utilized? This is still unclear at this part of the manuscript. Radar? Microwave? Lidar?

P12490, 25: The measurements are not remotely sensed. The clouds have been remotely sensed by remote sensing measurements.

P12494, 19: Does "column version" mean, RRTMG is able to be run as a Monte Carlo model too? I don't think so. What the authors mean, probably is "plan parallel".

P12494, 22: Sub-Arctic atmosphere: Please give a reference where this atmospheric profile comes from.

P12495, 13: The cloud base temperature is certainly derived from the radio sonde profiles. If so, please add this information here.

P12496, 18: What QCRAD stands for?

P12496, 20: What is compared here? This is not clear from the text. The radiative transfer model with measurements?

P12496, 28: Please add "... radiation compared to longwave radiation"

P12498, 4: Please specify which SZA value is very low.

P12498, 18, Section 3.2: Section 3.2 is too long and can easily be cut into two parts. The first part discussing the irradiances, the second one discussing the radiative forcing. The new section may start with **P12498, 18**.

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P12498, 20: In equation 5 and 6, F and Q are used as symbols for the net long- and shortwave irradiance. This does not fit to the results shown before in Figure 3 and 4. Please synchronize. Same holds for Figure 5–8. There is no consequent use of symbols for the irradiance and the radiative forcing.

P12498, 23: The cloud fraction A_C was not considered in the paper assuming fully overcast cases only as I suppose. I suggest to change $F(A_C)$ into F_{cloud} , $F(0)$ into F_{clear} . The same holds for the longwave irradiance.

P12498, 26: There is no clear sky forcing. You certainly mean the clear sky net irradiance.

P12498, 26: "Cloud effects" is misleading. Simply the cloud has been removed and the atmospheric profile had been adjusted. I suggest to write: "... case removing the cloud and adjusting the atmospheric profile in the model."

P12500, 2: It always is difficult to write about changes of a negative forcing which may confuse easily. To clarify this issue, please add here, that decreasing means, that the absolute value increases; the forcing becomes more negative.

P12500, 5: Why the longwave radiation should be affected by the solar zenith angle? Longwave radiation just depends on the temperatures. Please do not present well known relationships as if these were new findings of this study.

P12500, 6: Solar zenith angle and surface temperature also vary during a day and not only during the course of a year. Please add here and once more in the conclusion

P12504, 4:, that the day time measurements are always conducted at the same time of day.

P12500, 11: Please use a more scientific wording. The cloud transmissivity for IR radiation in the water absorption band is just not zero if the optical thickness is very low. And so is the cloud absorptivity and emissivity not equal to one. This means, that the cloud does not emit as a black body. The emitted radiation is lower or corresponds

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to black body radiation of lower temperatures.

P12500, 13: Sure, for the most cases, the liquid water fraction is dominating the total cloud optical thickness. For these cases the fractional ice optical depth will not have a large impact, even if the values are small. If the forcing is plotted against the total optical thickness, this should come out more clearly.

P12500, 20: Specify which irradiance (flux density) is discussed here; downwelling? upwelling? net? short- or longwave?

P12501, 11: What is the basis on which this range has been chosen?

P12501, 22: Remove the following sentence which describes basic knowledge. "As the pathlength..."

P12501, 25: All simulations are done for the surface. There is only the albedo which makes the difference between F_{dw} and F_{up} . This is absolutely obvious, well known and should not be presented as a new finding. So please change the wording to a more scientific way: "As the upwelling shortwave irradiance is directly linked to the downwelling irradiance by the surface albedo, the identical behavior is observed for the upwelling radiation."

P12502, 3: It is obvious, why these two cases are different. Here the optical thickness is just low. So try to argue the other way around. First tell the reader the difference of both cases and than state the effect on the radiation: "The two exceptions are the cases where the optical thickness of the cloud is low. ..."

P12517, Fig. 7: As mentioned above, the changes in the upwelling shortwave irradiance are directly linked to the changes in the downwelling irradiance by the surface albedo. The longwave upwelling irradiance depends only on surface temperature and is not affected by cloud parameters at all. Therefore, I suggest to remove the plots of the upwelling irradiances in Figure 7.

P12502, 16: Please clearly describe/repeat, what was the original surface albedo and

do give absolute changes rather than relative changes.

P12503, 1-5: This discussion is irrelevant. The aim of this section was to quantify sensitivities of the simulated radiative forcing due to uncertainties in the assumptions made. A measurement error in the surface temperature will not change the true state of the atmosphere. The surface albedo does not change in reality. So, any temporal evolution has not to be considered here and should be removed.

P12518, Fig. 8: From theory it is well known, that the upwelling longwave irradiance is not affected by surface albedo, I suggest to remove the plots showing the longwave irradiance in Figure 8.

P12508, Table 1: Add a column with the ice fraction (volumetrical and optical). This gives a hint on the state of mixing of the clouds.

P12513, Fig. 3: It is partly hard to see which pair of boxes are from one case and should be compared. It might happen that the reader compares between two different cases. Please try to improve the presentation of this plot.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 12487, 2011.

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