

Interactive
Comment

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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Review 1 (André Ehrlich)

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 used which limits

Full Screen / Esc

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Interactive Discussion

Discussion Paper



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?

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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 authors will know how to weight in such cases.

First, we would like to thank the reviewer for a thoughtful and thorough review of the manuscript. We believe that the points raised have aided tremendously in improving the quality of the paper.

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. We understand the reviewers concern and have updated the manuscript to more directly focus on the retrieval of surface radiative flux densities. We have included a sensitivity analysis to better illustrate the impact of measurement uncertainties on the derivation of surface flux densities, and have attempted to shift our discussion on cloud radiative forcing to a more secondary role.

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

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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?

We have included discussion on the sensitivity of calculated surface downwelling radiation to the individual retrievals used (LWP, IWC, Reliquid and ice crystal habit), as well as a combined sensitivity including all combinations of retrieval techniques to better characterize the retrieval of cloud properties. Sensitivity of upwelling flux densities was not evaluated since, as the reviewer points out, these are simply a factor of albedo (sw) and surface temperature (lw). This discussion is included in the newly constructed section 3.3.

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 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.

We have included references to the Shupe paper – we are certainly aware of its existence, and agree that the methods used are similar. The similarities between our retrieval methods and those used by Shupe were already clearly stated in section 2.1 (although we had mentioned that retrievals were based upon the Shupe 2008 BAMS paper, rather than the JAS one). Regarding the leading coefficient for the IWC rela-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

tionship, in personal communication with Matt Shupe, he has conveyed to me that he believes the 0.07 value to be more generally applicable since it is based on a full year's measurements, while the 0.04 value is geared specifically at the clouds observed at M-PACE. While we acknowledge that the M-PACE clouds are precisely what we are looking at in this instance, we wanted to provide the variation that would be the most universally applicable, since it is hoped that this technique could be employed for more locations and times of year. In order to assure ourselves that this coefficient does not have a large impact on the results, we evaluated the surface flux densities derived using the 0.04 value against those derived with 0.07. That comparison is available in the new Figure 5 as IWC_C . As the reviewer can see, variability in IWC due to this coefficient was relatively small.

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.

The evaluation of this method in the first place is due in part to exactly the circumstance that the reviewer describes above, where radar, lidar and microwave radiometer measurements are readily available, but surface radiation measurements are not. While this specific site (Eureka) does have radiation measurements, they are challenging to obtain, and even when obtained include numerous missing data points and quality issues. Therefore, we disagree with the reviewer that this can not be the motivation for starting this effort. We do agree with the reviewer that there are additional arguments to be made for using this set of measurements, and have made modifications to the last paragraph of the introduction to more strongly reflect these justifications.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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 asses the uncertainties of the retrieval method. In P12490, 27 the authors claim to "quantify uncertainties associates with using this technique". This is the last time the word "uncertainty" is used in the manuscript!

As discussed above, we have included a section (3.3) and figure (5) describing the sensitivity of the retrieved surface radiation to errors in the retrieved quantities. This should shed light on the uncertainty associated with this method, and provide an idea of the potential issues with the results plotted in Figure 3.

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 inconsequent. 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, 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.

We don't quite follow what the reviewer is referring to with this statement. In section 2, the only introduction provided to the cases is a reference to Table 1, which outlines the mean properties of the 16 cases. As pointed out be the reviewer, the remainder of the discussion, as well all of the Figures (other than Figure 1) demonstrate the results from all of the individual profiles. At no point are "single measurements" shown, as implied by the reviewer. As stated in the text, within each of the 16 cases, measurements

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

are averaged into 2 minute periods (the temporal resolution of the lidar and radar are much higher than this). This is done to reduce noise, but the measurements shown (Figure 2) and the radiative transfer calculations (results in Figure 3-6) are those from the 2-minute averaged data, not the raw resolution of the instruments. The averages provided in Table 1 are simply the case-average properties (i.e. the average value for all of the 2-minute intervals).

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.

As mentioned above, we do always provide each of the two minute averages (again, except for Figure 1, which is provided as an example case). Figures 2 and 3 show the distribution of values within each of the 16 cases. Wide distributions are indicative of a large amount of variability within a case. Never are the radiative transfer calculations performed for the 30 minute averages presented in Table 1. Figures 7 and 8 demonstrate the impacts of changes to the cloud droplet effective radius and surface albedo on the MEAN flux densities (derived by averaging the 2-minute radiative transfer calculations). We believe that Figures 7 and 8 are the cleanest way to plot this information. In other words, once again the radiative transfer calculations are performed at 2 minute

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

intervals (the resolution displayed in Figure 1, and the results of which are shown in Figure 3). We believe that this is a fair and efficient way to portray this. Finally, we do not understand why the reviewer would say that Tables 2 and 3 should be removed. We believe that they provide valuable insight into both the errors occurring through the use of this method (table 2) and the cloud radiative forcing for the mixed-phase clouds observed during M-PACE (table 3). While derivation of CRF statistics is not the central goal of this study, certainly those results are original and of interest to the community. Therefore at this point we have not removed either table.

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.

We have provided a normalized version of the shortwave radiation to better bring out the relationships that may be masked by SZA. This information is presented in the top row of Figure 7 and discussion covering this portion has been added to the latter portion of section 3.4.

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 basic assumptions, which lead to different results as discussed

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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 Reff when keeping the optical thickness constant will change the WP.

In this work, the water path is kept constant, and as pointed out by the reviewer, this results in varying optical depths. While framing the question in terms of the optical depth is interesting, we believe that it's redundant to include information for both optical depth and effective particle size, particularly since a relationship is provided (eq. 3) that relates the two. Therefore, we have not provided information on the range of optical depth values resulting from the change in droplet size, but we have added some additional discussion on how the change in radius was performed (lines 275-276).

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 $r_{e,liq}$, 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 $r_{e,liq}$, surface albedo and surface temperature are unknown. Is it necessary to know the $r_{e,liq}$ 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.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

We have updated the manuscript to focus on the retrieval of flux density, since this is the quantity that eventually is used to calculate the radiative forcing. The updated paper now focuses on the calculation of flux density, including an analysis of the sensitivity of the downwelling components to remotely-sensed cloud properties. We do include the discussion on cloud radiative forcing at the end of the paper as an example of an end-product that can be obtained using this technique. In terms of the individual parameters, here we have focused on those tied directly to the remotely sensed measurements. Therefore, surface temperature and albedo have not been included in the sensitivity analysis, and are only brought up in discussion.

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? 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.

This is a good question – in order to address it, we ran extra simulations that removed the influence of all ice and compared them to the base simulation. This comparison is included in Figure 5. Discussion of the result has been integrated in section 3.3. In general, it is found that the ice crystal contribution is small relative to the absolute magnitude of the flux density. This implies that characterization of the liquid layer is more important than characterization of the ice properties, but that the ice component can influence the overall calculation.

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.

We appreciate this recommendation and have made edits to the manuscript at the passages suggested below.

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 Wm^{-2} . Radiative flux has the unit W.

We agree that either irradiance or flux density are a more accurate description of the quantities provided and have updated the manuscript to reflect this. Having said this, we would like to point out to the reviewer that the American Meteorological Society Glossary of Meteorology indicates that the terms “radiative flux density” and “radiative flux” can be used interchangeably.

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.

We agree that the short title can be reworded in a better way. We have updated it to “Mixed-Phase Cloud Radiation Retrieval”.

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

We have included a list of the instruments in the abstract.

P12488, 7: “coincidence”, better use overlap.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

We disagree that “overlap” is better than coincidence and have left it as is. Coincidence is defined as: “the condition or fact of coinciding”, where coinciding is defined as: “to occupy the same place in space or time”.

P12488, 20: temperature sensitivity?

We have included a sentence on surface temperature sensitivity in the abstract.

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

“Multiple phases of liquid” has been updated to “Multiple phases of water”. Thanks!

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.

We have modified the text to make the association of lines 14-25 to Curry and Ebert more clear. We have not added an additional reference because we felt it was unwarranted.

P12489, 20: Which estimates are meant here?

The Curry and Ebert paper did not include “low” IWP for clouds, but rather only for ice crystal precipitation (ICP). Since this statement was not relevant to the current work, it has been removed.

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

This statement was referring to the Curry and Herman work, but we have removed it all together.

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

The peak was demonstrated to occur in fall. This has been added to the text.

P12490, 20: Which kind of measurements was utilized? This is still unclear at this part of the manuscript. Radar? Microwave? Lidar?[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

A list of utilized instruments has been included.

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

This has been edited to say “remote sensor 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".

RRTMG comes packaged with two different versions, a “column model” and a “gcm model” (terminology provided in the package, not by us). For this study we used the column version. Therefore, we have left this as is.

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

This profile is distributed as part of RRTM. The text has been updated to reflect this. Unfortunately, no references are provided for these profiles.

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

The cloud base temperature is in fact derived from a combination of remote sensor and radiosonde measurements. The radiosonde-derived temperature at the altitude of cloud base (as detected by lidar) was used as cloud base temperature. This has been added to section 2.

P12496, 18: What QCRAD stands for?

This is stated in the text: “a quality-controlled surface radiation estimate product available through the ARM program database (Long and Shi, 2008)”. No edits have been made.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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

The text has been updated to clarify further.

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

Edited to state "greatest for surface shortwave..."

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

As stated later in the sentence, this included SZA greater than or equal to 90 degrees.

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.

This partitioning has been included in the new version.

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.

Equations 5 and 6 and following text has been updated to match the abbreviations used in Figure 3. Figures 5 and 6 seem ok to me, since we are only looking at cloudy cases (i.e. $CF_{LW} = LW_{NET}(\text{cloud})$ and $CF_{SW} = CF_{NET}(\text{cloud})$). Figures 7 and 8 have been updated.

P12498, 23: The cloud fraction AC was not considered in the paper assuming fully overcast cases only as I suppose. I suggest to change F(AC) into Fcloud, F(0) into Fclear. The same holds for the longwave irradiance.

These changes have been made.

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

irradiance.

This is correct and the text has been modified.

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.”

This text has been modified.

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.

This has been added.

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.

This sentence has been modified. We were not presenting it as a new finding, but were merely explaining the noticeable relationship between SZA and longwave forcing in Figure 6 (which is due to seasonal shift in surface temperature).

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.

Text illustrating this has been added to section 2. Additionally, this information is located in Table 1. We feel that this is sufficient notice.

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.

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Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



This means, that the cloud does not emit as a black body. The emitted radiation is lower or corresponds to black body radiation of lower temperatures.

The text has been updated.

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.

Yes, we agree.

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

The text has been updated to reflect the flux density discussed (downwelling short-wave).

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

Based on some rough calculations, these values encompass the range of critical radii of droplet formation for aerosol particles with radii between 0.2 and 0.7 microns at supersaturation of 0.1% (0.5-1.05 microns at $S_w=1\%$). The lower limit for droplet radii allowable in RRTMG is 3.5 microns. The calculations above demonstrate that droplets with radii larger than 10.5 microns would require the presence of large aerosol particles.

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

While the reviewer may perceive this as “basic knowledge” we believe that it is worth mentioning to support the previous statement. We are not claiming this to be a new finding, but are simply using it to explain the derived results. Therefore we have left this statement in the text.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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."

This change has been made.

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. ..."

We appreciate the suggestion but have decided to leave the order alone in this case.

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.

Figures 7 and 8 have been removed completely.

P12502, 16: Please clearly describe/repeat, what was the original surface albedo and do give absolute changes rather than relative changes.

The changes provided were absolute changes (with albedo presented as a percentage). We have updated the text to use the decimal form utilized in the figure, rather than percentages.

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.

We have removed this discussion, as suggested.

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.

Figures 7 and 8 have been removed completely.

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.

We are not certain what the reviewer means by volumetrical and optical. We are assuming that he meant column-integrated and height-resolved. We believe that the column-integrated value is easy enough to estimate from the values provided in the table (though it will not be exact due to the values being averaged). It is not practical to provide height-resolved ice fraction for each case in table form.

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.

Thank you for the suggestion. We have modified the spacing to (hopefully) improve the readability of the figure.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/11/C6993/2011/acpd-11-C6993-2011-supplement.pdf>

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

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