Reply to Referee #3

(Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-534)

We would like to thank the reviewer for her or his time and the beneficial comments, which will help to improve the manuscript. Please find the replies to the referee comments below. The page and line numbers given by the referee relate to the manuscript in discussion, the numbers in our reply to the revised manuscript.

Referee comments are highlighted in **bold**, *changes* in the manuscript in *italic*.

The manuscript "Reassessment of the common concept to derive the surface cloud radiative forcing in the Arctic: Consideration of surface albedo – cloud interactions" by Stapf et al. describes the calculation of cloud radiative forcing (CRF) from measurements collected from aircraft over the MIZ in the eastern Arctic near Svalbard during the ACLOUD campaign in June 2017. The authors leverage the spatial nature of their measurements over the heterogeneous surface albedo that is characteristic of the MIZ to identify and correct biases in calculations of CRF under such conditions. The targeted biases are specifically those associated with cloud-surface albedo interactions when the estimates of the shortwave clear-sky surrogates used in the CRF calculation are derived using radiative transfer models. The authors focus on a relevant problem that is suitable for ACP and this problem has received less direct attention from previous studies than analogous problems in the infrared. However, I respectfully disagree that this as a "reassessment" of CRF calculations, as the influence of surface albedo on CRF has been acknowledged previously and managed in various ways.
Please find the discussion after the first major comment.

The focus on albedo is warranted here because the observational platform and environmental conditions discussed make the present work particularly sensitive in that regard but I think the advancement promised by the title is overstated. I would be more comfortable with the paper being presented in either of the following ways:
(a) As calculations of CRF in the MIZ during ACLOUD with the albedo work being an important, though incidental component. In this case more work or more detailed explanations of the longwave calculations are needed (see comments below).
(b) If the authors wish to focus on the shortwave, they should drop the longwave data altogether and pitch the study as a proposed methodology for calculations of shortwave CRF in the MIZ where treatment of clear-sky shortwave fluxes requires special attention. In either case, the study needs to be more carefully contextualized and motivated by referencing previous work.
I also suggest a thorough copy editing for grammar, typos, missing words or letters, etc., of which there are many to be found.

Major Comments:
(1) The introduction and study motivations need substantial improvement. Some portions of the introduction actually belong in the Methods section. More troubling is that the study promises to improve upon (indeed, to "reassess") surface-based observations of CRF without actually referencing a single example of previous work on this subject, which has developed for several decades in the Arctic. I suggest rewriting the introduction to more clearly contextualize the present work in the existing literature. I have included some (not exhaustive) useful references throughout this review.

The reviewer is right, we should have made more clear, that we focus on the shortwave estimate of CRF. Therefore, we changed the title to:
"Reassessment of shortwave surface cloud radiative forcing in the Arctic: Consideration of surface albedo-cloud interactions"

However, after including a literature overview of the common approaches to derive the CRF from the last two decades (section 3.1), an introduction focusing on the estimate of CRF and the conclusion part from section 3.5, we come to the conclusion, that the general knowledge of the seasonal cycle of shortwave CRF in the Arctic is based on simplified assumptions.
Neither available observational, nor model studies represent the discussed process (surface albedo-cloud interaction, see section 3.5) sufficiently. Climate models will not even rudimentarily represent this effect. However, these kind of models are used to estimate the cloud radiative feedback. Therefore, we want to point out the importance of reconsidering the described processes with respect to the estimate of the solar CRF. We added also in the conclusion (p.21 l.34):

"The shortwave net irradiances depend not alone on cloud transmissivity and surface albedo, moreover the interaction between both needs to be represented. "
To draw the attention also to the modelling community.

Another also quite important aspect is the homogenization of CRF estimates from different available studies/datasets. For the shortwave CRF, we provide an approach for snow surface types, which can be easily applied to long-term and high quality ground-based observations. As an example, the three important studies from Shupe and Intrieri (2004) during SHEBA, Dong et al. (2010) for Barrow/Alaska and Miller et al. (2015) for Greenland used different approaches for their CRF estimates.
We discuss the different approaches in the new section 3.1 and why a comparison of the CRF values of specific studies might be error-prone and misleading.
In section 3.5 we give reasons to estimate the shortwave CRF using the cloud-free albedo, but we also want to make clear, why our approach should give a better estimate of shortwave CRF.

There are two approaches available deriving the shortwave CRF specifically with a cloud-free albedo estimate.
The first one is from Miller et al. (2015), where the cloud-free observations are linearly fitted as a function of SZA. In Gardner and Sharp (2010) (Fig. 10b) it is shown that the albedo is a non-linear function of SZA, which is further affected by the snow grain size. In Fig. 2 from Miller et al. (2015) this fit is shown and shows significant deviations from the applied fit, potentially induced by snow grain size on the Greenland ice sheet. Neglecting these fluctuations can easily cause deviations in the shortwave net flux, and thus, also in the shortwave CRF for the specific prevailing conditions. For example assuming a downward irradiance of 550 Wm-2 for an SZA of 60° and applying a cloud-free albedo data point of 0.79 and 0.87 in the study from Miller et al. will cause deviations of up to 44 Wm-2 in the CRF. These albedo induced fluctuations in CRF are concealed in the obtained time series and might be related to precipitation events, warm or cold periods, or the seasonal cycle.
For the cloud-free albedo or upward shortwave estimate from the climatological approach in Dong et al. (2010) it is stated:
"The clear-sky SW-up flux is estimated using the technique described in the study by Long [2005], where the clear-sky solar zenith angle dependence of the surface albedo is taken into account, and the clear-sky SW-up flux is estimated by the clear-sky albedo and SW-down flux."

However, if we look at Long (2005) it is noted that only the observed albedo (cloudy) can reproduce significant changes in the surface albedo (like precipitation events) during longer cloudy periods (nicely shown in Fig. 1 in Long (2005)).
Although an albedo change like the presented one in the Southern Great Plains will not occur in the Arctic snow grain size can quickly change the surface albedo. That demonstrates that during longer cloudy periods, which are common in the Arctic, the cloud-free surface albedo estimate by this approach will induce significant uncertainties.
That is why we state in section 3.1 (p.6 l.27):
"An application of the climatological approach is primarily limited by the high cloud fraction commonly observed in the Arctic (Shupe et al., 2011). It causes large uncertainties in the estimated cloud-free irradiance, as reported by Intrieri et al. (2002), preventing an application to long-term observations with reported high cloud fractions (e.g., Sedlar et al., 2011)."
Also in the following sentence:
"Although the climatological approach will produce a more realistic estimate of CRF (especially longwave) with reduced uncertainties and representation of humidity changes (Dong et al., 2006), it remains unclear how representative a monthly average of cloud-free irradiance with a monthly averaged cloud fractions often well above 90% can be."

For the longwave range, we state now in the introduction (p.2 l.28):
"As demonstrated by Walsh and Chapman (1998), the surface temperature change accompanied by the transitions from cloudy to clear skies is not an instantaneous effect; it rather occurs in the range of hours to days and potentially only advanced boundary layer models might be able to predict the transition between the two states after a given time."

While the terrestrial instantaneous CRF studies from Shupe and Intrieri (2004), Sedlar et al. (2011) and Miller et al. (2015) can be nicely compared for saturation effects with increasing LWP, the estimate from Dong et al. (2010) appears unique as compiled by Miller et al. (2015). However, we know that the climatological approach with a colder surface temperature in cloud-free conditions should cause a weaker warming effect in the longwave CRF compared to the instantaneous approach. On the other hand, the cloud-free surface temperature response to changes in the cloud cover is a function of time as demonstrate by Walsh and Chapman (1998) and other surface fluxes. This circumstance raises the question, which time span after a dissipation of clouds is representative for longwave cloud-free fluxes estimated by the climatological approach, 5 minutes or 1 day? In the shortwave instead, we see an instantaneous response of surface albedo and shortwave net fluxes.
Another issue is added by the study from de Boer et al. (2011).

de Boer, G., Collins, W. D., Menon, S., and Long, C. N.: Using surface remote sensors to derive radiative characteristics of Mixed-Phase Clouds: an example from M-PACE, Atmos. Chem. Phys., 11, 11937-11949, https://doi.org/10.5194/acp-11-11937-2011, 2011.

There the temperature inversion caused by cloud top cooling is removed for the radiative transfer simulations by linearly interpolating between the surface and atmosphere above the cloud induced inversion, while keeping the surface temperature the same, and thus, mixing both approaches.

In the end, we have a lot of different estimates of CRF in the Arctic, and we conclude in section 3.1 (p. 6 l. 34):
"By comparing the available studies, using different approaches to estimate the CRF, it becomes evident that the variety of strategies and the handling of physical processes involved in the CRF in the Arctic limits the comparability of the individual studies and our understanding of CRF in the Arctic."

(2) The title indicates a focus on shortwave processes. In my opinion is a fair representation of main the scope of the work, yet there are sections devoted entirely to the longwave and total CRF. There are complications in CRF calculations that are specific to the longwave (Allan et al. 2003) which are analogous to the issues affecting the shortwave; e.g., lapse rate and surface temperature responses to clearing skies (Long and Turner 2008) and systematic differences in water vapor between skies that are clear and those that are cloudy ("water vapor CRF", e.g., Dong et al. 2006). These issues are ignored and consequently the longwave and total CRF parts of the manuscript are somewhat confusing and do not serve a clear purpose. There is quite a lot packed into this study already. I think the study would be much clearer if only shortwave data were included, in keeping with the advertised focus of the work.
Obviously, the original title was misleading. We adjusted it to make the overall focus more clear.
"Reassessment of shortwave surface cloud radiative forcing in the Arctic: Consideration of surface albedo – cloud interactions"

"…**there are sections devoted entirely to the longwave and total CRF…**":
In section 3.3 we also show the shortwave CRF. To quantify the total CRF in the Arctic and answer the question if a cloud is warming or cooling the surface, the longwave contributions needs to be included in this study, even when only the instantaneous effect is considered here.
We revised the manuscript in a couple of sections to make clear, that we simply derive the instantaneous longwave CRF, so the discussion about the longwave effects are not relevant at this point (besides the conclusion).
From section 4.2 and the conclusion we see a shift from a mainly warming effect of clouds during ACLOUD to a cooling one in the end of the campaign simply by accounting for surface albedo- cloud interactions. Furthermore we added in the conclusion (p.21 l.19):
"In addition, the instantaneous longwave CRF approach might additionally induce an overestimate of the warming effect potentially shifting the total CRF further to cooling."

(3) Ehrlich et al. (2019b) (P3L30) is in review and the DOI provided is unreachable.
We apologize for that, but in our submitted manuscript the DOI works, unfortunately after the processing of ACP (discussion version) it does not work anymore. However it is an open access journal (ESSD) and can also be found without https://doi.
Thus, I cannot evaluate the processing of the radiometric data, which is central to this study. Indeed, I don't even know what equipment was used.
In section 3.2 we referenced two papers, which fully describe the campaign, the used instrumentation, the processing, also in Wendisch et al. (2019).
I wish to know more in particular because radiometric data from airborne platforms requires additional processing, though I am aware that the authors are familiar with some of these complexities (e.g., Ehrlich and Wendisch 2015). In addition to the instrument response corrections of the aforementioned work, how did you correct for tilt in the pyranometer?
The inertia correction is applied by the approach from Ehrlich and Wendisch (2015). In cloud-free conditions we applied the approach from Bannehr and Schwiesow (1993) and Boers et al. (1998) as described in Ehrlich et al. (2019b) to correct for the tilt/attitude of the sensor. In cloudy conditions an attitude correction cannot be applied as the irradiance is mainly diffuse, also the upward solar fluxes (assumed mainly diffuse) were not corrected out of this reason. Further details are given in Ehrlich et al. (2019b).

How did you correct the pyrgeometer data (measured at altitude) to represent the value that would be observed at the surface? (I think the answer is you did not [P7L9]).

Yes we did not correct the CRF to represent values at the surface, because the impact is of minor importance.

In the next sentence we made clear that we are not analyzing single irradiance quantities, which are definitely influenced by the flight altitude. In the sentence the reviewer refers to, we argue that the vertical gradient of longwave irradiance remains the same, if we remove the cloud in radiative transfer simulation from the atmosphere (instantaneous approach). We clarified this sentence (p.10 l.8):

"Due to the fact that the vertical gradient dF_lw/dz and dF_lw/dz below clouds remains almost the same with or without a cloud <u>in the radiative transfer simulations (for atmospheric profiles as observed during ACLOUD),</u> the observed CRF in flight altitude can be related to surface CRF values <u>causing uncertainties below +-5Wm-2</u>."

To show that the average flight altitude of 80 m has only a minor impact on the CRF estimate, we simulated two observed thermodynamic states during the ACLOUD campaign and implemented simplified vertical homogenous clouds. In Fig. 1 (in this document) it can be clearly seen that the vertical gradient between cloudy and cloud-free single flux directions changes only slightly. Consequently, also the vertical profile of CRF (right panels) changes only slightly (see values for flight altitude of 80m and the surface embedded). We have the opinion that *"…can be related to the surface CRF…"* is correct regarding the other potential uncertainties of radiative transfer modelling and observations. But we added an uncertainty of +- 5 Wm-2 (conservative estimate) to account for this effect. The surface based inversion was a clear-sky profile observed by a dropsonde over water, of course the assumed cloud is kind of sketchy, but only serves as a test case for a stable atmosphere.

I have a similar question about the KT-19, which does not observe thermodynamic temperature of the surface, but rather a brightness temperature relative to the FOV and dependent on the path to the target. Is there any reason to similarly correct the shortwave data for altitude given that such details are the focus of the present work?

The flight altitude (average 80 m) does hardly affect the KT19 observations. We are aware of the study from Haggerty et al. (2003), which is cited in Ehrlich et al. (2019b). The applied corrections for flight altitude are necessary due to higher flight altitudes compared to the low-level observations during ACLOUD. Regarding the assumed surface emissivity for the KT19 wavelength range we use the results from Hori et al. (2006), which indicate an emissivity in this wavelength range and for the nadir viewing angle close to unity. As the values are only used for a linear interpolation of the temperature profile (required for the radiative transfer simulations) from in average 80 m to the surface, an influence on the simulated downward irradiance can be excluded. We added in the specific sentence (p.5 l.9):

"The atmospheric levels below flight altitude were linearly interpolated to the surface temperature observed by the KT-19 <u>assuming an emissivity of unity. The assumption of the black-body emissivity is justified by the high spectral emissivity for nadir observations in this wavelength range (Hori et al., 2006).</u>"

The impact of flight altitude on the estimate of shortwave CRF can also be seen in the specific panels of Fig. 1 (in this document) and is minor important.

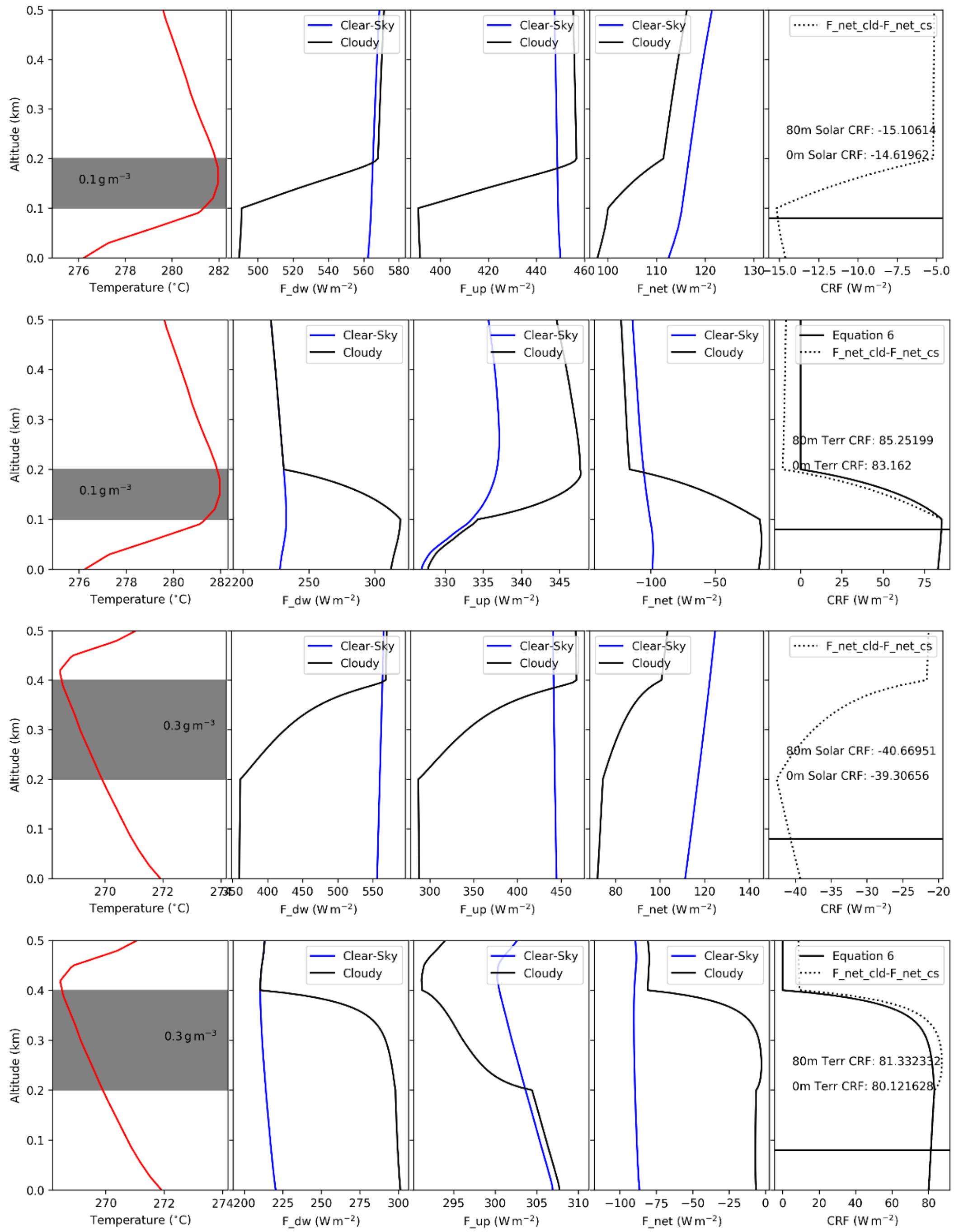

Figure 1 Simulated thermodynamic profiles during ACLOUD with a rarely found surface based inversion and a typical profile over sea ice with a lifted inversion at 400m. Cloud extent and assumed homogeneous LWC is shown in the grey box together with the temperature profile (left panel). First row: Shortwave fluxes and CRF. Second row: Longwave fluxes and forcing. Third row: Shortwave fluxes. Forth row: Longwave fluxes. Fluxes are always given for cloudy and cloud-free (removed cloud) case. The surface emissivity is set to 0.99, the surface albedo 0.8 and the SZA 60°.

(4) Unless I misunderstand something, I believe there are errors in the presentation of the CRF equations. This is simple to correct if it is merely a typo in the subscripts, but if the equations were applied as stated the study's results could be impacted. Specifically, be careful how you use the terms "all sky" and "cloudy sky" because they are not equivalent in CRF nomenclature. CRF may be defined as either (all – clear) or as CF(cloudy – clear) where CF is the cloud fractional occurrence and the other terms refer to net radiative fluxes for "all" (clear and cloudy sky conditions together), "cloudy" (only times when clouds are present) and "clear" (clear skies). Ramanathan et al. discuss both definitions. Admittedly, they are confusing on the nomenclature themselves in using the term "cloudy" early on in discussing their Eqs (1) and (2), but their meaning becomes clear when they introduce Eq. (4). Your Eq. (2) is therefore incorrectly stated; it shows the maximum CRF (e.g., Intrieri et al. 2002). The "cld" subscript needs to be "all" or the entire right side of the equation needs to be multiplied by the cloud fraction. For your purposes, and for the Arctic in general, I suggest the former. This commentapplies to equations throughout the text.

The reviewer is right that definition was kind of confusion, although cloudy is not equal to overcast (Ramanathan et al.). To avoid any issues with the cloud fraction definition, we replaced "cld" with "all" in the whole manuscript.

(5) P7L9: I do not agree that this is a good assumption. About 70% of the downwelling longwave at the surface originates from atmosphere below the altitude of your aircraft (Ohmura 2001). Your assumption is plausibly (not certainly) valid if the atmosphere is isothermal between surface and the base of the cloud. While this condition could be met, in your case studies (Fig. 2), it is not. Your observations of flux at the altitude of the aircraft should be corrected to represent the surface.

From our personal point of view the statement "**70% of the downwelling longwave at the surface originates from atmosphere below the altitude of your aircraft (Ohmura 2001)**" is kind of misleading and gives the impression that everything above does not matter, which is definitely not true. Please have a look on Fig. 1 (in this document). Even for single terrestrial flux directions the average flight altitude will cause a deviation well below 10 Wm-2 (quite conservative estimate again), in the net fluxes even less, because of the same vertical gradient.

The atmosphere during ACLOUD was in the most cases not isothermal between surface and cloud base (during the low-level sections a least), which can be seen in Fig. 14 from Wendisch et al. (2019), where the distribution of the observed longwave net fluxes in the cloudy state shows always negative numbers indicating a colder "effective cloud base temperature" compared to the surface. The profile from the surface based inversion was during clear-sky conditions over open ocean and did not "affect" or observation during that day.

Nevertheless, the estimate of CRF is hardly influenced by the flight altitude. Please see also the answer to comment 3 together with Fig. 1 (in this document).

(6) P8L11-P9L7: The approach you suggest to achieve a downwelling clear-sky shortwave is intriguing, but more information is needed for future studies to adopt your method. As written, it is not reproducible and there is no information on the sensitivities of the estimation; for example, I would expect that a filter of constant width assumes that leads are randomly distributed and roughly of the same size. I would also like to know more about the justification for your choice of a Laplace distribution as the most appropriate filter for this application.

We added in section 3.4 the equation and settings of the estimated smoothing kernel to make it more reproducible. In addition we added (p.12 l.2):
"This enables a more reliable estimate of the CRF in the heterogeneous MIZ and over the specific surface types, taking into account that the complexity of surface albedo fields in the MIZ can only be insufficiently represented by this simplified approach to estimate the areal averaged albedo."

Please see the next comment for further information about the simulations/estimate of filters.

Specific Comments

(1) Given that your upwelling shortwave is observed from an aircraft platform, the FOV covers and enormous area. I therefore do not understand why your albedo measure-ments (e.g., Fig 3a) are not implicitly area-averaged, even if observed from a relatively low altitude (e.g., Podgorny et al. 2018).

Thanks for bringing up this point. Yes, the albedo observed in the average flight altitude of 80 m is "already smoothed". We did a mistake in estimating the smoothing kernel in the 3D simulation for the surface (0 m) and unfortunately neglected this altitude induced effect. So we revised the whole procedure and estimate the kernels for a representative flight altitude of 80 m.
In Fig. 2 (in this document) one of the clear-sky simulations is shown for a lead with 1 km width embedded in homogeneous sea ice similar to the study from Podgorny et al. (2018). In the upper panel the albedo is color coded as a function of scale and altitude. Also the comparison between surface albedo (0 m) and flight altitude (80 m) is shown, where the altitude induced smoothing effect can be seen, which is rather weak. The upward irradiance is cosine weighted so the alleged FOV is not really representative for the upward fluxes. In an average flight altitude of 80 m, 80 % of the signal is represented within a radius of 102 m below the aircraft. Even when we did not resolve the ground level, the smoothing effect is still in a small range, but of course the estimated filter width and shape changed accordingly (embedded in the lower panel).

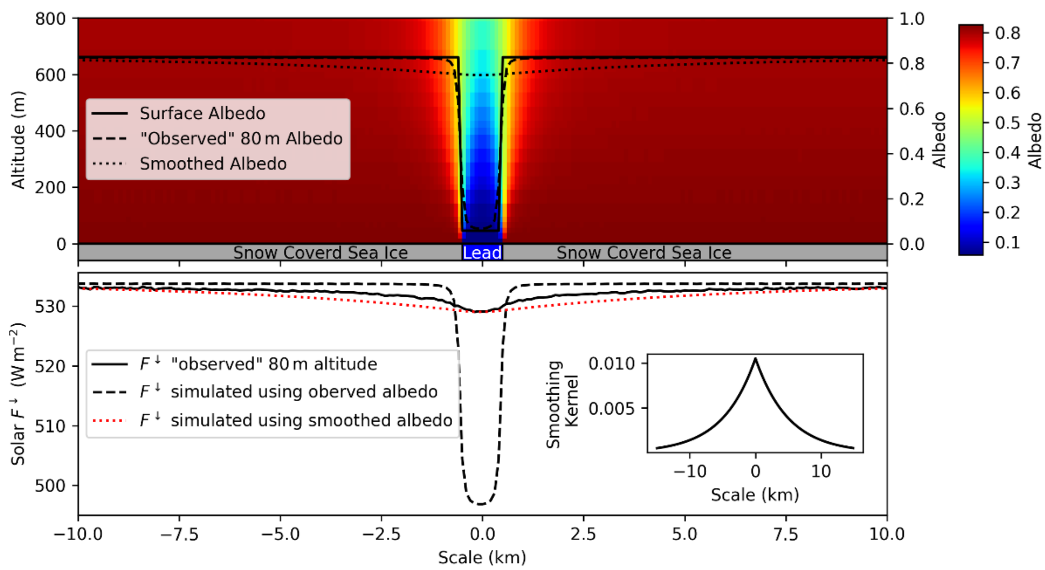

Figure 2 Broadband shortwave 3D radiative transfer simulations in clear-sky conditions of a 1 km lead embedded in homogeneous sea as shown in the lower bound of the upper panel. Upper panel: Vertical distribution of albedo. Comparison between surface albedo, albedo as observed by an aircraft in 80 m flight altitude and the smoothed albedo using the filter embedded in the lower panel. Lower panel: Comparison

between 3D downward irradiance perpendicular to the lead as it would be observed in 80 m altitude (solid), 1D simulations using the observed albedo as input and the final product 1D simulations using the smoothed albedo. SZA is set to 60°.

Also for the transmissivity based LWP retrieval, we recalculated the kernel and give the settings in the appendix to represent average flight altitude, as shown in Fig. 3 (this document).

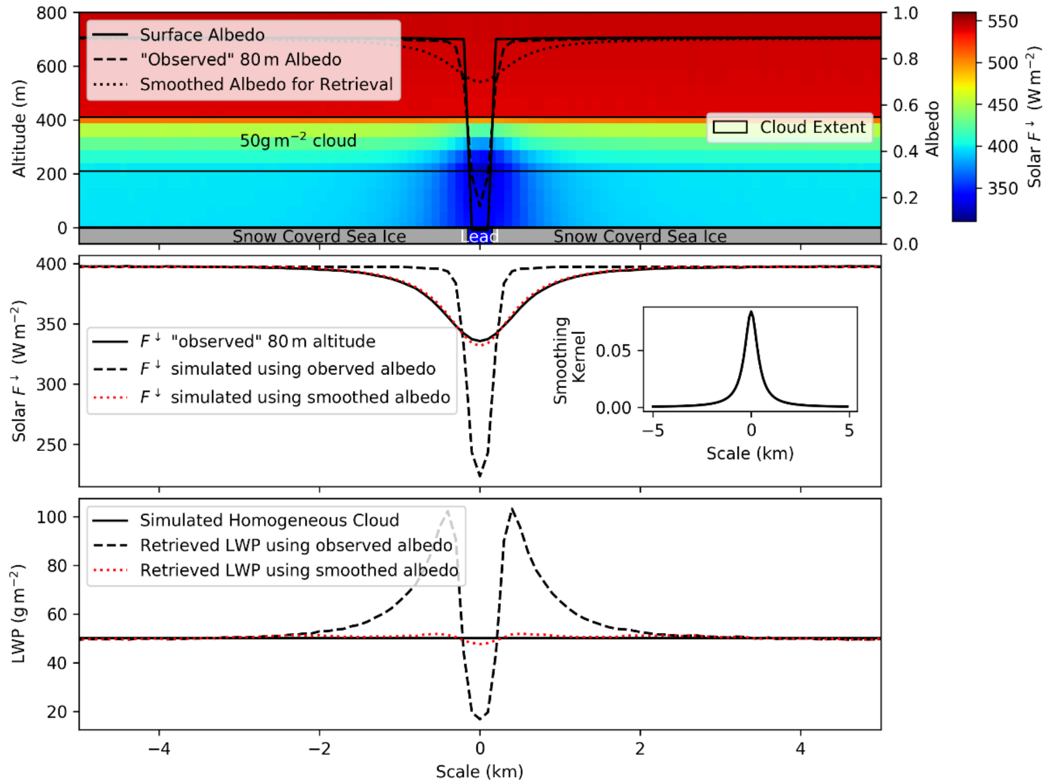

Figure 3 Broadband shortwave 3D radiative transfer simulations in homogeneous cloudy conditions with a constant LWP of 50 gm-2 and a 300 m lead embedded in homogeneous sea, as shown in the lower bound of the upper panel. Upper panel: Vertical distribution of shortwave downward irradiance in the color code and comparison between surface albedo, albedo as observed by an aircraft in 80 m flight altitude and the smoothed albedo using the filter embedded in the middle panel. Middle panel: Comparison between 3D downward irradiance perpendicular to the lead as it would be observed in 80 m altitude (solid), 1D simulations using the observed albedo as input and the final product 1D simulations using the smoothed albedo. Lower panel: Impact of smoothed albedo on the LWP retrieval for the homogeneous 50 gm-2 cloud. SZA is set to 60°.

As we state in the appendix the variety of potential surface heterogeneity prevents a specific solution. These results should work out for ACLOUD, where we exactly observed these simulated surface / cloud scenes, which brings us back to "**I would also like to know more about the justification for your choice of a Laplace distribution as the most appropriate filter for this application.**"

It simply fits to our observations and simulated cases and enables us to make it applicable to our study. A general solution like the one from Pirazzini and Raisanen (2008) requires unfortunately surface albedo maps, which we do not have.

The changes due to the new smoothing kernels caused small changes in Fig. 3, 4, 9 and 10, and the related statistics (text section 4.2 and 3.4.1). Due to the shorter scale of the clear-sky kernel in Fig. 3 changes in the areal averaged albedo occurred accordingly, which do not change the general picture. Changes in the kernel for the LWP retrieval caused only small changes in strongly fluctuating albedo sections, but did not affect the obtained statistics or retrieval of cloud-free albedo.
However, we found that occasionally surface albedo values exceeded the range of Gardner and Sharp (2010) parameterization and filtered the specific values out and added (p. 18 l.23):
"Rarely occurring surface albedo values above/below the range of the parameterization from Gardner and Sharp (2010) have been filtered out."
Hence, the average values of CRF and Albedo in Fig. 10 and the distribution in Fig 10a and 9 changed slightly accordingly, see also changes in the values given in the text, abstract (p.1 l.12):
"Applying ACLOUD data it is shown that the estimated average shortwave cooling effect by clouds almost doubles over snow and ice covered surfaces (~~-63~~ -62Wm-2 instead of ~~-33~~ -32Wm-2), if surface albedo-cloud interactions are considered."
And conclusion (p.21 l.14):
"For the ACLOUD campaign, characterized by snow on sea ice in the beginning melting season, the averaged shortwave CRF estimate over homogeneous sea ice of -32Wm-2 (cooling) almost doubles to -62Wm-2, when surface albedo-cloud interactions are taken into account."

(2) P2L18: Consider using "L" for longwave instead of "t" for terrestrial to avoid confusion with the terrestrial surface. "S" would then represent "shortwave" rather than "solar".
We adapted the subscripts and wording in the whole manuscript.

(3) P2L18: It is true that RT simulations are a common approach, but there are other approaches as well. Long and Ackerman (2000) present a method for estimating clearsky fluxes that implicitly accounts for the albedo dependencies on sky conditions. (Refer also to Dong et al. (2010) and Long (2005)). More in line with your study, Miller et al. (2015) parameterized clear-sky albedo for their RT simulations, though your situation is considerably more complex with regard to surface cover. Other studies have analyzed the dependencies. These studies do not necessarily detract from your work here and in some ways maybe motivate it, but either way really need to be referenced.
We included the different approaches in the new literature overview in section 3.1. (See also reply to the first comment in this document)

(4) P4L15 - P5L4: (1) I don't understand how (or when) you combined the dropsondes and NYA radiosoundings.
Before or after most of the low-level section the aircraft descended or ascended from/to higher altitudes, and thus, in situ profiles of thermodynamic state were observed. In addition dropsonde data from P5 could be used. For each low-level section we need a representative local thermodynamic profile for the calculations of F_{dw} (impact was shown for example in Fig. 2 in the manuscript). Therefore, we replaced the layers from the radiosoundings (either from Ny-Alesund or Polarstern (the temporal and spatial closer one)) with the local profiles to obtain a merged representative profile.
(2) You do not say how you represent the atmosphere above the height of the soundings; this is necessary (even if estimated using a standard atmosphere) to a reasonable effective TOA (say, 60 km). (3) You do not say how you represent atmospheric gases that are radiatively active in the infrared, but were not measured by the sounding (not notably, CO_2, but also O_3, methane, etc.).
Thanks for this remark. We agree with the reviewer, that these are important information to make the study/ RT simulations reproducible. We included all information in section 2.3.

(5) P5L14-16: I do not agree that the upward longwave between clear and cloudy conditions is equal, but I doubt that this is what you actually mean to say. I think you mean you defined them to be so because (a) you do not account for the response of the atmospheric lapse rate (and surface skin temperature) to changes in sky cover and your calculations for the longwave are therefore "instantaneous" CRF (e.g., Miller et al. 2015),

We apologize for this unclear definition. After the literature overview and the definitions section this should be clear now.

and (b) that you also neglect the influence of differences in the amount of longwave reflected from the surface between clear and cloudy skies. It is acceptable to make the first assumption (see Allan et al. 2003), but you should include the emissivity term and then proceed with your CRF calculation. See my next comment.

We slightly adjusted the longwave CRF definition (Eq. 3 - 6) and account now also for the reflected downward longwave irradiance. (See changes in the definition section). Accordingly also the average longwave CRF value in Fig. 10b,c and section 4.2 changed slightly. See also next reply.

(6) P4 Eq. (4): This variable is more frequently referred to as (longwave) "cloud radiative effect" (CRE) (e.g, McFarlane et al. 2013; Viudez-Mora 2015; Cox et al. 2015, 2016) and should be distinguished somehow from CRF. At the surface CRF and CRE are different, the former being the difference in the net fluxes and the latter being the difference in the incident fluxes. Confusion sometimes arises because the terms are frequently used interchangeably in satellite studies, being that the terms are equal against the backdrop of space.

Thanks a lot for this remark. We totally agree that a standardized nomenclature (and definition) is required in the literature and a difference should be made between CRF and CRE. We added (p. 7 l.10):

"As was stated by Cox et al. (2015) the CRF definition refers to net fluxes, while the cloud radiative effect (CRE) characterizes only changes in the downward irradiance."

As we stated and show now in the definitions section (p.7 l.11), the upward terms cancel for the instantaneous CRF estimate (e.g. Shupe and Intrieri, 2004; Miller et al., 2015) accounting for the reflected residual.

But the reviewer is right, the reflection term needs to be represented, why we changed the formulation (Eq. 3 - 6). Assuming an emissivity of 0.99 (Warren, 1982) the reflected residual is 1 % of the CRE, and thus, will cause a difference in the derived long CRF below 1 Wm-2. (Average values in the Fig. 10, section 4.2 changed slightly)

(7) P5L28: Multiple scattering also depends on the albedo of the sky. How do you account for this?

The albedo of the sky (scattering processes of the atmosphere) is implemented by the radiative transfer simulation of the local atmospheric profile. We continuously update/run the simulation along the flight track using the closest thermodynamic profile and the local estimated areal averaged albedo.

For the smoothing kernel of the areal average albedo estimated by the idealized 3D radiative transfer study we had to fix the atmosphere (represented by the subarctic summer standard profile), which will influence only the smoothing filter, and thus, will induce only a minor impact on the resulting 1D online simulations using the smoothed albedo.

(8) P6L30: Is 60deg SZA representative of the flight conditions?

These are rounded values but representative for the conditions over sea ice during that flight. To clarify that we only used this fixed values for this sensitivity study to avoid an impact of changing SZA and surface albedos during the specific profiles and only focus on the thermodynamic impact in this section 3.3 we added (p.9 l.7):

"The surface albedo and SZA is fixed for this sensitivity study to 0.8 and 60 respectively, similar to the observed conditions over sea ice during that flight, in order to avoid any effects induced by changing SZA or surface albedo."

(9) P9L20: You might also consider that your pyranometer is at best a 2% instrument and thus you might expect uncertainty of around 10 Wm-2 in the measurement. Thus, Figure 4 looks quite good. I am however curious about the source of the bimodality of the solar CRF in Figure 4. My first thought is that one of these peaks is associated with ice-covered areas and the other with open water, pointing to some residual bias in the method.

To rule that out, we show in Fig. 4 (in this document) the 2D histogram of this shortwave CRF distribution of Fig. 4 (in the manuscript) as a function of sea ice concentration. The occurrence of leads do not induce a bias in the derived downward irradiance as the bimodal distribution can be seen also in the 2D distribution. Only the data of the Polar 5 aircraft produce this bimodality. We found a slight correlation to aircraft heading for this aircraft, what might indicate an issue related to the observations not the radiative transfer simulations. However, we have double checked the processing and could not find any issues regarding the offsets for the correction applied by the approach from Bannehr and Schwiesow (1993) and Boers et al. (1998) as described in Ehrlich et al. 2019b.

In Ehrlich et al. 2019b also a comparison between the two aircraft is shown indicating a good agreement. Unfortunately, we cannot resolve this bimodality during this flight, it could also be related to slightly different local conditions not captured by the in situ profiles or aerosol/haze layers as a huge area was covered. But we should be aware that these differences are well below the measurement uncertainty (<3 %) of these instruments and the albedo smoothing method is not the cause of this deviation.

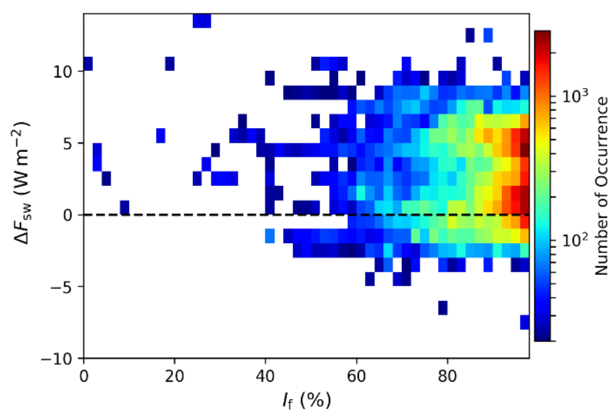

Figure 4 2D histogram of shortwave CRF derived during the clear-sky flight (as shown in the manuscript Fig. 4) as a function of observed sea ice fraction (I_f).

(10) Section 3.4: It would substantially increase the value of this section if you contextualized your simulated biases with your observations. For example, it would be interesting to see the biases from Figure 3c plotted over Figure 7 in the phase space of the figure panel that is most appropriate.

We do not fully understand the point of the reviewer here. The bias from figure 3c is attributed to the changing downward shortwave irradiance due to multiple scattering, while Figure 7 shows a completely different process and a quantitative estimate of surface albedo-cloud interactions.

In Fig. 10 we show the impact of the surface albedo-cloud interaction (Fig. 7) on the ACLOUD observations by comparing the different approaches.

(11) P18L9: You have mentioned SHEBA a couple times, but have not referenced it (Uttal et al., 2002), nor have you defined the acronym.

We give now an appropriate citation and the introduced the acronym.