Reply to Referee #2

(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*.

General comments In summary, I think this is interesting work, but it needs a more solid foundation and a discisson about the why's and the how's, and less details on ACLOUD; maybe do a separate but more exten-sive paper on CREs during ACLOUD, referencing this paper.
See third comment.

Maybe I'm nitpicking, but the terminology has been changed by the climate community, from Cloud Radiative Forcing, or CRF, to Cloud Radiative Effect, or CRE, quite a while ago.

As was noted by reviewer 3, there are inconsistences in the definition of CRF and CRE. These parameters represent different definitions, as described in Cox et al. (2015). We derive the instantaneous CRF and, therefore, kept CRF in the revised manuscript. To make the definition of CRF more precise, we added this sentence in section 3.2 (p.7l.10):

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

The title is also in my personal opinion too long and clunky; try something shorter. Maybe "Interactions be-tween surface albedo and clouds for Arctic cloud radiative effect estimation".

We changed the title to:

"Reassessment of shortwave surface cloud radiative forcing in the Arctic: Consideration of surface albedo-cloud interactions"

The content in the study balances along many borders and as a consequence it doesn't quite fulfill any of the topics it crosses well enough. It is unclear if this is a theoretical study that uses ACLOUD data just because its good and convenient or if it is a contribution to ACLOUD as such. I already hear the authors say "can't it be both?" and my response is it would be a better radiation interaction paper if ACLOUD was tuned down and a better ACLOUD paper if the radiation stuff was more background and the actual results where more detailed. Typically to be a good paper on both aspects it would have to be longer – which is not good. There is also a lot taken for granted on the readers; not everyone is a radiative transfer modeling expert. So choices must be made.

We tried to reduce the ACLOUD topic as much as possible, but the study needs to be based on observations to show the impact of the investigated physical processes in real conditions. A study completely based on theoretically constructed scenarios would raise the question how relevant these results are in reality. The results shown are not meant to specifically characterize the clouds observed during ACLOUD. ACLOUD data is more a test base to apply the new approach and contributes in the end with (see abstract):

“Applying ACLOUD data it is shown that the estimated average shortwave cooling effect by clouds almost doubles over snow and ice covered surfaces (-62Wm-2 instead of -32Wm-2), if surface albedo-cloud interactions are considered.”

However, these observations require an introduction, especially because it is a new dataset with challenges discussed in this paper. We totally agree that unfortunately these sections partly distract from the main aspect of the paper. On the other hand, observations like the one from ACLOUD enables to quantify the dependence of surface albedo with cloud LWP (Fig. 9) from observations, a process that represents the major motivation for this study. Regarding **“can’t it be both?”**, we have the opinion it must be both. By using synthetic radiative transfer simulation the manuscript shows, why surface albedo-cloud interactions matter for the estimate of CRF. The solution of this problem depends on the application on real data when the cloud-free albedo needs to be estimated from cloudy sky observations, a problem reported by different studies in literature. Only with help of the ACLOUD data, we can show that the proposed approach does lead to an improvement of the CRF estimates.

Furthermore, we keep the ACLOUD specific interpretation of the cloud properties as short as possible. We do not interpret the obtained CRF in detail, simply the relevant sign of the distributions and magnitude of changes related to surface albedo-cloud interactions (main aspect of this manuscript) are highlighted. The interpretation of CRF during ACLOUD will be treated in an upcoming paper.

Besides the use of an old terminology (CRF instead of CRE) there needs to be a much more in-depth and philosophical background.

See reply to comment 2.

Why are we interested in CRE (or CRF) and how does that impact how we do these calculations? The question “How does the clouds affect the surface energy budget” is not the same as the question “How would the surface energy budget look if the clouds were not present?”. As an example, the authors argue both that changes in surface broadband albedo between cloudy and clear states must be considered, and that the details in thermodynamic are important. However, we know that the thermodynamic profiles for clear and cloudy cases are very dif-ferent. The presence of clouds depends on the vertical profiles, but the clouds themselves also modi-fy the profiles by their presence. Yet the authors argue that we change the surface albedo to how would be without the clouds, but keep the thermodynamic profiles as they are; only remove the cloud water. I think that the an- swers depend on what we want to use this metric for. Ideally the an-swer to my second question above would have us examine the conditions in clear and cloudy condi-tions separately, not modifying cloudy cases by removing the condensed water. However, the Arctic is a very cloudy place and there are not enough clear cases to make this possible. Therefore, I think this paper needs a much more detailed introduction and background to what we are trying to do and why.

We agree, that the motivation and definition of the CRF is not well given in the manuscript. Therefore, in the revised manuscript, a new section is added. In the new section 3.1 the available approaches applied in literature to derive CRF are discussed. We adjusted the introduction to explain what is changing between the cloudy and cloud-free atmospheric state in the Arctic and how it affects the estimate of CRF by the approaches listed in Section 3.1.

For the longwave CRF we are totally aware of changing thermodynamics between the cloudy and clear state. However, we aim to quantify an instantaneous effect, switching cloud on/off without changing the atmospheric profile. This is justified by putting the main focus on radiative effects caused by the shortwave CRF (see also changes in the title). To highlight which approach we use, we added "instantaneous" CRF at couple of sections, what should make clear, that we neglect the impact of changes of temperature and humidity.

1) Effects of proper thermody-namic profiles:

1) This is pretty obvious; of course one must use the profile from the same location as the CRE is considered. In this paper this is discussed in the context of aircraft observations covering an area, but the conclusion is also important for fixed-point observations. One question is, if a proper sounding at the location is not available, from how far away can it be used? This question of course has no an-swer other than "it depends." But here one could also raise the issue of cloudy profiles being differ-ent from the corresponding clear case; in other words, if we could magically remove the cloud water so that the clouds vanish, what would that do to the thermodynamic profiles? Or, are the profiles found in clear conditions systematically different from those in cloudy conditions? From both mod-eling and observations in subtropical stratocumulus regions we know that the moist PBL is deeper and warmer when clouds are present; for the Arctic we don't really know, but I would wager a bet that they are different!

We did not intend to discuss in general how the atmospheric thermodynamic profiles influence the estimate of CRF and that a local profiles are required, which is of course obvious. In this section the focus is mainly on the impact of air mass transformation, such as warm air intrusions or cold air outbreaks, which changes the thermodynamic state within relatively small horizontal scales. By citing the literature from Tjernström et al. (2015,2019) and Pithan et al. (2018) in this section our message should get more clear now. Regarding the thermodynamic states, see also new section 3.1.

"…from how far away can it be used?"

Yes, it depends on the scenario and where exactly a different air mass is located. We, therefore, do not try to give a general answer to this question and try to estimate the effect for our observations where we use the in situ profiles directly before and after each low level leg to replace the layers of the remote radiosoundings from Polarstern or Ny-Alesund.

"…, if we could magically remove the cloud water so that the clouds vanish, what would that do to the thermodynamic profiles?"

See comment above. We aim to quantify an instantaneous effect neglecting changes of the thermodynamic state and their impact on the terrestrial CRF. To clarify this we added the new section 3.1.

2) Effects of heterogeneous surfaces

2) That heterogeneous surfaces poses a problem for upwelling shortwave radiation is also pretty obvious. This is a main factor in the MIZ but also in the pack ice mainly due to melt ponds.

In this section we focus on the impact of surface heterogeneity on shortwave <u>downward</u> radiation by horizontal photon transport, not the obvious upwelling shortwave radiation, which is roughly a linear function of cosine weighted sea ice concentration.

This pa-per doesn't even mention the effect of melt ponds, presumably because there weren't any during ACLOUD.

We mention melt ponds and discuss their effects in several sections in the paper, although we had only a low percentage during the ACLOUD campaign as was shown by Jäkel et al. (2019). Of course we are aware of their radiative effects in the summer melting season, which are part of the hypothesis Fig. 8 and discussed in section 3.5/3.51 as well as the conclusion.

So is this an ACLOUD paper usuing advanced radiation methods or a radiation paper using ACLOUD observations?
See our reply to the general comment above.

Moreover, to this reviewer it is not obvious that the downwelling radiation is dependent on sur-face albedo; in most NWP or climate models that I know this is not considered, but may be ignorant.
The downward radiation is affected by the surface albedo due to multiple scattering effects. With increasing surface albedo, the upward irradiance will increase. This increased upward irradiance is then scattered back by aerosol particles and atmospheric gases in cloud-free conditions (or clouds during cloudy conditions) back to the surface. This multiple scattering contributes to an increase of downward irradiance over highly reflective surface types like snow, compared to absorbing surfaces like water (see also reply next comment). This effect of course is considered in NWP models as it is simulated by any radiative transfer scheme.
What is not considered in NWP models is:

- Horizontal photon transport due to multiple scattering between neighboring grid cells (3D radiative transfer required, or the areal average albedo) (see section 3.4)
- A change of the surface albedo due to different illumination conditions (cloudy vs. cloud-free) and cloud optical thickness (see section 3.5/3.51). This effect is a major subject of the manuscript.

Either way, for a reader like me, this needs to discussed in more detail. Commenting about "horizon-tal photon transport" is not sufficient.
In the revised manuscript, a short explanation of the horizontal photon transport in case of inhomogeneous surface conditions is given (p.8 l.2).

"<u>For highly reflective surface types like snow the upward irradiance is significantly higher compared to over mostly absorbing surfaces like ocean water. A part of this upward irradiance is scattered back towards the surface (often referred to as multiple scattering), and thus, contributes to the downward irradiance. Consequently the multiple scattering between surface and atmosphere causes an increase of downward irradiance over snow and ice compared to open ocean. Photons reflected from a bright surfaces like an ice flow might scatter back to the surface increasing the downward radiation over dark areas like surrounding ocean water.</u> For airborne observations in the MIZ, characterized by strong variability in surface albedo due to the variable sea ice cover, as well as ground based measurements in heterogeneous terrain<u>, this, often referred to as, horizontal photon transport due to multiple scattering from the surrounding area to the actual point of observation</u> is not negligible for the estimate of F_down_sw (Ricchiazzi and Gautier, 1998; Kreuter et al., 2014)."

In addition we cite five papers distributed in the manuscript (Ricchiazzi and Gautier, 1998; Kreuter et al., 2014; Weihs et al., 2001; Wendisch et al., 2004; Pirazzini and Raisanen, 2008), which specifically discuss this topic.

If this is a factor, how large is it?
The magnitude of the horizontal photon transport is quantified for the case study in Fig. 3. To make this more obvious, we added/reworded this section by (p.10 l. 22):
"However, due to horizontal photon transport from surrounding ice fields in reality the changes in F_sw,cf are less pronounced. The quantitative impact of multiple scattering on F_sw,cf is indicated by the gray shaded area in Fig. 3b with a maximum contribution of almost 40Wm-2 (relative to open ocean)."

What is it caused by? Are there differences between say the MIZ, with alternating ice and open water, pack ice with melt ponds, pack ice with open leads, or pack ice with many substantial pressure ridges? Or all of the above?
The 3D radiative effects due to horizontal photon transport in the MIZ are complex and depend on the given scenario (surface albedo map) as indicated by the reviewer.
The presented approach (smoothing the surface albedo using an appropriate filter shape, simulated using simplified scenes (see replies to the other reviewers)) can only give a rough estimate of the conditions during ACLOUD, but on the other hand, it is still simple enough to be applied to observations. However, to address this issue, we added a short discussion in section 3.4 (p.12 l.2):

"This enables a more reliable estimate of the CRF in the heterogeneous MIZ and over the specific surface types, <u>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.</u>"

3) Effects of the clouds on the characteris-tics of the solar radiation.
3) Changes in the spectral composition from absorption in clouds is a real tangible effect that one can discuss if it is necessary to compensate for or not; see my discussion above.
The change of the spectral composition is not only due to absorption. The largest difference is the almost wavelength independent scattering by clouds (Mie regime) compared to the preferred scattering of short (blue) wavelength of atmospheric gases (Rayleigh regime). Clouds are white, the cloud-free sky is blue. In section 3.5/3.51 of the manuscript, we demonstrate why these spectral differences are of relevance and how a compensation affect the estimated CRF.
Changes due to the different distribution between direct and diffuse radiation is trickier. Also the cloud albedo is sensi-tive to solar zenith angle.
All these effects are considered and analyzed in the radiative transfer based study in section 3.5/3.51.

Finally, the language is mostly OK, but occasionally I stumble on unnecessarily difficult wording, for example "exemplarily" in the context it is used is an existing word but even the dictionary indi-cates it isn't much used in modern English.
Thanks for this remark, we reworded the sections.

There are also past/present inconsistencies; what is done and presented in this paper is sometimes described in past tense and sometimes in present. Either is fine with me; just be consistent.
We apologize for these inconsistences and corrected it.

Finally, final: among the data made available here, only a subset is actually really made available; the rest is just referenced.

During the submission of the manuscript, the publication process of the data in PANGAEA was not finalized and parts of the data might have been inaccessible. All basic data from the broadband radiometers, dropsondes, radiosondes, aircraft temperature and humidity measurements, and the camera images are available (Please see data availability). The publication of derived quantities such as LWP, cloud-free albedo and CRF is currently in progress. We wanted to wait for the reviews before publishing the data. The references will be added in the manuscript before publication. The just referenced data are dataset with a doi made fully available on the PANGEA database.

Title is unnecessarily clunky
We changed the title, see reply third comment.

also here and throughout the paper, Cloud Radiative Forcing (CRF) should be replaced by Cloud Radiative Effect (CRE); even maybe surface CRE.
See first reply in this document.

Page 1, line 23: Comma after amplification.
Corrected.

Page 2, line 1-2: If this is the prime question, one can not use the cloud profile minus the cloud as representative for clear conditions; one must do the clear and the cloudy cases completely separately.

Thanks for this comment, which indicates, that our text might lead to a mix-up of the cloud feedback in the Arctic and the cloud radiative forcing. The former is obtained from climate model studies, the latter is a measure of how cloud influence the energy budget at a certain location and time. The manuscript only deals with the cloud radiative forcing. We changed the sentence to more clearly separate feedback and CRF (p.2 l.2).

"One prominent example is the cloud radiative feedback, which includes the effects of an increasing cloud amount in the Arctic, balancing between potential increase of both longwave downward radiation (positive) and cloud top reflectivity (negative)."

Page 2, line 3: It is not at all clear that clouds are cooling the surface in the Arctic in summer, so I would drop "dominates". It depends on a lot of factors, some of which this paper deal with. A clear case when this statement is correct, perhaps the only one observed case, is for SHEBA; the only annual observations that exist and BTW where the CRE was calculated without any of the corrections discussed here. Suffice it to say that I've seen summer conditions with a lot of snow and almost no melt ponds at very high latitudes where surface temperatures plummets when the clouds dissipate.
The reviewer is totally right. See the changes in the previous comment. We furthermore added a more specific discussion of the CRE of Arctic clouds (in the introduction) (p.2 l.14):

"Long-term ground-based observations of CRF in the Arctic (Walsh and Chapman, 1998; Shupe and Intrieri, 2004; Dong et al., 2010; Miller et al., 2015) showed that in the longwave wavelength range clouds tend to warm the surface. The magnitude of the warming is influenced by macrophysical and microphysical cloud properties (e.g., Shupe and Intrieri, 2004) and by regional characteristics (Miller et al., 2015) and climate change (Cox et al., 2015). In the solar spectral range, clouds rather cool, whereby the strength and timing over the year is determined, besides cloud microphysical properties, by the solar zenith angle (SZA) and the seasonal cycle of surface albedo (e.g., Intrieri et al., 2002; Dong et al., 2010; Miller et al., 2015)."

Page 2, line 11: The cases are "clear" or "all-sky", so I would swap places between "all-sky" and clear here. You can still define that as "cloudy" from her on, but it should be stated that the normal case is the existing clouds; not just when it is completely overcast.
Ramanathan et al. (1989) defines cloudy as not necessarily overcast, but to clarify the definition throughout the whole manuscript we changed it to "all-sky".

Page 2, line 20: Here it is stipulated that surface albedo affects the incoming (downwelling) solar radiation for clear skies. To me that is not obvious, and even if it is obvious from multiple refelctions in

clouds it is not obvious that it is important for clear-sky radiation. Do spend some more time on this please.
We discuss here multiple scattering in cloud-free conditions, not multiple scattering related to clouds.
This part was moved to section 3.2, where we state now (p. 8 l.1):
"The downward shortwave irradiance at the surface in cloud-free conditions (F_dw_sw_cf) is modulated by the atmospheric profile parameters, but also by the surface albedo. For highly reflective surface types like snow the upward irradiance is significantly higher compared to over mostly absorbing surfaces like ocean water. A part of this upward irradiance is scattered back towards the surface (often referred to as multiple scattering), and thus, contributes to the downward irradiance. Consequently the multiple scattering between surface and atmosphere causes an increase of downward irradiance over snow and ice compared to open ocean. Photons reflected from a bright surfaces like an ice flow might scatter back to the surface increasing the downward radiation over dark areas like surrounding ocean water. For airborne observations in the MIZ, characterized by strong variability in surface albedo due to the variable sea ice cover, as well as ground based measurements in heterogeneous terrain, this, often referred to as, horizontal photon transport due to multiple scattering from the surrounding area to the actual point of observation is not negligible for the estimate of F_dw_sw (Ricchiazzi and Gautier, 1998; Kreuter et al., 2014)."

Page 2, line 14-15: ". . . observations of . . . conditions and of atmospheric thermodynamic state."
This sentence was removed from the introduction.

Page 2, line 26: Is shape the right word here? Isn't it the magnitudes; not just the shape?
Yes the reviewer is right, we intended to say spectral albedo type not shape. This sentence was remove from the introduction and the topics is covered now in section 3.5.1.

Page 2, line 28: Comma after "albedo".
This sentence was remove from the introduction.

Page 3, line 17-18: Here's an example of tense mismatch: ". . . were investigated in this paper" and ". . . aircraft is displayed . . .". Later on same page the dataset "is" merged and on line 3-4 on the next page ". . . concentration was calculated . . .".
We checked the whole manuscript for tense mismatch. Thanks for this eye-opener.

Paragraph staring on Page 6, line 29:How do you handle the observed surface albedo when calcu-lating CRE (or surface ïA˛Dˇ F)? The text says that the clear-sky albedo is set to 80% and the zenith an-gle to 80_; presumably those were not the observed conditions?
Only for this sensitivity study in section 3.3 the albedo and SZA was fixed. For all simulations used to derive the CRF based on observations (section 4), the cloud-free albedo estimated from the measured are applied. To clarify that it is only applied for this sensitivity study, and why we did this, we added (p.9 l.7):
"The surface albedo and SZA <u>is fixed for this sensitivity study</u> to 0.8 and 60° <u>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.</u>"

Section 3.3: Spend some time explaining why albedo affects downward solar radiation.
See reply on comment on Page 2, line 20.
Also, ex-plain the choice of albedo filter; is there any theoretical consideration here or was it "trial and error"?

There is no analytic theoretical basis for the filter. However, we based our choice on 3D radiative transfer simulations of the downward irradiance in case of a typical lead, which are presented here in Fig. 1 (this document). The simulations indicate, that the shape of a La-Place distribution (with the shape parameter as defined now on in the section) is required/suitable to obtain the observed weighting of albedo information in the near-field of the aircraft. Yes it is “trial and error”.

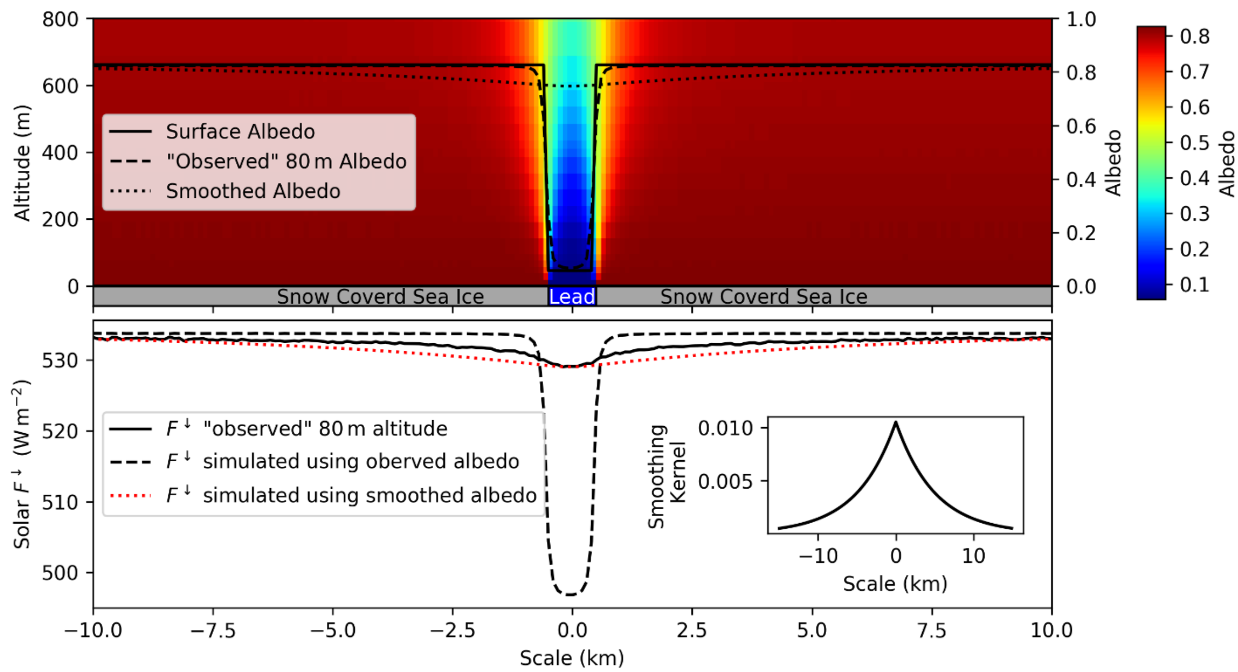

Figure 1 Broadband shortwave 3D radiative transfer simulations in clear-sky conditions of a 1 km lead embedded in homogeneous sea as shown in the lower bound of the upper panel. Upper panel: Vertical distribution of albedo. Comparison between surface albedo, albedo as observed by an aircraft in 80 m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

Although the match (red dashed to solid black) is not perfect, by comparing it with the approach using simply the observed albedo (dashed black) it becomes clear that a certain smoothing is required. We tried different filter shapes and found that the La Place filter gives the appropriate weighting of the near and far-field albedo for this specific scene also for different lead sizes. It should be stated as well, that this smoothing depends on the atmospheric profile (here simply subarctic summer standard profile), albedo distribution etc. and can only give a rough estimate of the observed conditions during ACLOUD.

Paragraph starting at Page 9, line 14: It seems to me this is a test comparing calculated results to observed results from a clear day; correct? If so, maybe this should not be reported under this heading? And maybe the term CRF (or rather CRE) should not be used when there are no clouds and the CRE is expected to be zero?

Yes, this is correct, here we look at cloud-free conditions. Ideally, the CRF should be zero. However, we still think, that we need to use CRF in this section as well. The idea of the histogram is to give an uncertainty estimate for the CRF and not for, e.g., the downward irradiance. This approach is similar to Shupe and Intrieri (2004). The calculations use the same method as described in 3.3 to estimate the CRE. Differences

from zero indicate the uncertainties related to non-cloud effects. In the revised manuscript, we separate this comparison by using a subsection 3.4.1:
"Uncertainty estimate in cloud-free conditions"
In addition, we give the average and standard deviation of downward irradiance between simulations and observations in the text.

Page 10, line 6: Explain SSA.
SAA is a measure of the snow grain size. To make this link more clear, we added *"a measure of snow grain size"* to the introduction of SSA and added a citation of Gardner and Sharp (2010), where it is nicely introduced (p.13 l.4).
"Different snow packs with a density of 300 kgm-2 are specified with various values of snow geometric thickness and specific surface area (SSA, <u>a measure of snow grain size) (Gardner and Sharp, 2010</u>), and located above a layer representing bare sea ice with a wavelength constant broadband albedo of 0.5."

Page 10, line 9: I don't think you could find a case with 1 cm snow thickness in reality. That would be > 1cm at some location and no snow at other.
The purpose of these three snow packs is to represent a typical spectral albedo not necessarily "one of the real" snow packs in the Arctic. It is more relevant to show the spectral features related with the onset of melting. Of course, on small spatial scales, the snow depth can vary and be zero. However, the albedo used in the simulations should represent the spatial average of a representative area where snow covered and bare ice are mixed. As the TARTES model is not made for slush or melt-ponds, the snow thickness is a scaling factor for the shorter wavelengths to roughly represent snow or white ice in the summertime Arctic.
An example of 2-4cm of snow above sea ice can be found in Fig. 4 in Zatko and Warren (2015), where nicely the impact on the shorter wavelength is shown in reality.

Zatko, M., & Warren, S. (2015). East Antarctic sea ice in spring: Spectral albedo of snow, nilas, frost flowers and slush, and light-absorbing impurities in snow. Annals of Glaciology, 56(69), 53-64. doi:10.3189/2015AoG69A574

Paragraph starting at Page 10, last line: This should come before the calculation specific. First ex-plain why and then how.
In the revised manuscript, these effects are already introduced in the introduction (p.2 l.31):

"Besides temperature and humidity changes, clouds modify the illumination and reflection of the surface. For highly reflecting snow surfaces, radiative transfer simulations show that two processes are crucial: (i) A cloud-induced weighting of the transmitted downward irradiance to smaller wavelengths, causing an increase of shortwave surface albedo, and (ii) a shift from mainly direct to rather diffuse irradiance in cloudy conditions, which decreases the shortwave albedo (Warren, 1982). Observations have shown that, in general, there is a tendency that the surface albedo is larger in cloudy, compared to cloud-free conditions (e.g., Grenfell and Perovich, 2008), and was demonstrated for a seasonal cycle by Walsh and Chapman (1998) for highly reflecting surface types."

Page 11, line 3-4: Why is diffuse radiation coming in at a zenith angle of _50_? When the cloud is thick enough that where the sun is in the sky can no longer be determined, is there a zenith angle at all? I thought, but may be wrong, that diffuse mean precisely that the radiation was equally strong in all directions.

It is correct, that the radiation field below clouds is diffuse and the Sun is not visible. The 50° represent an "effective" solar zenith angle for which the surface albedo of pure direct illumination is similar to diffuse illumination. The angular weighted incoming diffuse irradiance has an "effective" (average) incoming angle of 50°, for details we refer to Warren (1982) or Gardner and Sharp (2010).

In the manuscript we write (p.13 l.19):
"… clouds decrease the averaged incoming (effective) angle of the mainly diffuse irradiance to approximately 50° above snow (Warren, 1982)."

Page 11, line 18: Drop "the".
Corrected.

Page 12, line 17: ". . . with increasing LWP is not, or only poorly, parameterized. . ."
Corrected.

Page 12, line 18: Unclear past tense in "have been used". Previously, or did you do this work now. In the previous case, give reference; in the latter, present tense should be used.
Corrected.

Figure 8: The SHEBA albedo line includes melt ponds and eventually even a lead. The drop in albedo starting at the beginning of June is due to this; as there were no melt ponds I ACLOUD(?), your comparing apples and pears here. ; as there were no melt ponds I ACLOUD(?)

As ACLOUD is limited in time, the aim of this section is to transfer the results estimated for the albedo-cloud interaction to the seasonal cycle in the Arctic. The SHEBA data, even when influenced by melt ponds, is the only comparable data set available. Therefore, the comparison does not aim to have a perfect match between ACLOUD and SHEBA. As was shown by Intrieri et al. 2002 with the onset of melt pond formation and the related strong drop in the albedo, the total CRF shifts to a cooling effect. ⇔ For ACLOUD we had higher albedo values, but we still find the transition to a total cooling in the end of the campaign caused by the surface albedo-cloud interaction (which is not represented in the study from Intrieri et al. (2002)). That is why we state in the last sentence of section 4.2 that the transition to the cooling might start earlier in the year, simply by accounting for surface albedo cloud interaction and represents a reassessment of shortwave CRF in the Arctic.
To make this clearer, we extended this sentence in the conclusion (p.21 l.17):
"Hence, the observed albedo trend during the campaign (Fig. 8) induces a transition in CRF from a warming to a cooling already for snow covered surface types, and thus, <u>earlier in the season as reported during SHEBA.</u>"

For the SZA calculations, did you take into account that SHEBA moved northward during the year?
As stated in the figure description: "Computed daily averaged SZA for 80° N in dashed black." it is fixed and only serves as a reference in which range of Figure 7 the underestimation/overestimation might take place. As shown in Fig. 1, 80° N is representative for ACLOUD and for the last months of SHEBA.

Another idea would be to redo the SHEBA Intrieri et al. CRE study with this new information. Maybe a bit more work than anticipated for now, but it would be interesting.
That's true but out of the scope of this study. The interpretation of the SHEBA CRF with the new knowledge is exactly why we made this hypothetical sketch. However, we do not have yet the radiative transfer model to represent the melt pond properties and, therefore, the SHEBA CRF was not revised. Additionally, we wrote in the conclusion that further effort is required to fully understand the seasonal cycle of solar CRF

in the Arctic by application of a similar approach to long-term observations like SHEBA or the upcoming MOSAIC. In general this approach using the parameterization from Gardner and Sharp (2010) is easy to apply to common ground based stations in the Arctic with cloud microphysical remote sensing instrumentation providing high quality LWP values, however a snow and ice dominated surface is required (no slush, melt ponds,… etc.). We added to the conclusion (p.21 l. 26):
"The proposed method to estimate the surface albedo in cloud-free conditions using the parameterization from Gardner and Sharp (2010) can be easily applied to common Arctic long-term observations above snow and ice surface types, especially if high quality LWP measurements are available."

Page 13, line 2 & 3: Again, two examples of past-tense confusion. When was this done; for this paper or by an earlier investigator.
Corrected.

Page 13, line 10: Using both "indicate" and "might" in the same sentence almost obliterates the conclusion.
We removed "might" from the sentence.

Page 14, line 4: Can't find any red line in Figure 8.
We changed it to: *"red scatter points"* in the figure description and in the given line.

Page 17, line 5: "indispensable" is a strong word. Since it is impossible to know the cloud-free state with any accuracy, I would mellow the language here. If something indispensable is also im-possible, then why even try?
We reworded the sentence (p.20 l. 11):
"To estimate the warming or cooling effect of clouds on the surface REB in the Arctic from observations or models, a precise characterization of the cloud-free state is required."

Page 17, line 11: If by "local" you mean in one single specified point, then I'm confused. The lo-cal albedo is what it is; it is different at a different locale when the sea ice is variable; I still get hung up on this concept. If you are referring to the effects on the cloud free downwelling radiation, that I wanted to have elaborated on, the at least write "local cloud-free albedo".
The reviewer is right, the word "local" is confusing in this sentence and was removed.

Page 18: lines 27-31: Only part of the data is available, a large chunk is only cited. Why? Appendix A: OK; but, why don't show that this works, using aircraft passages over Polarstern, where you have both transmissivity and LWP?
All primarily measured data are made available on the PANGEA database. DOI-links, where the specific dataset can be downloaded are given in the references. The publication of derived quantities such as LWP, cloud-free albedo and CRF is currently in progress. We wanted to wait for the reviews before publishing the data. The references will be added in the manuscript before publication.
We would have very much liked to validate the method using PASCAL data. Unfortunately this was not possible for different reasons. For the PASCAL campaign unfortunately no broadband albedo measurements are available from the ship. Without the information of broadband albedo, transmissivity cannot be interpreted or linked to cloud microphysics, due to multiple scattering.
In order to validate this retrieval we compared different observations during the ACLOUD/PASCAL campaign with our estimate of LWP. In Fig. 2 (this document) a comparison of LWP for the 2 June 2017

flight is shown. In situ observations of the Nevzorov probe (dataset on PANGAEA: https://doi.pangaea.de/10.1594/PANGAEA.906658) during ascents and descents before and after low-level section (orange scatter in Fig. 2) are used to derive the LWC profiles, which have been vertically integrated to obtain the LWP. During that flight only low-level clouds have been present. In addition to our transmissivity based retrieval (blue scatter) the MODIS overpass (0945 UTC) is shown in red scatter points collocated to the aircraft low-level flight sections. For the flight section close to Polarstern (last flight section), the HATPRO microwave retrieval of LWP on Polarstern is shown for the times where the aircraft was close by (dataset on PANGAEA: https://doi.pangaea.de/10.1594/PANGAEA.899898 or "Cloudnet" LWC data https://doi.pangaea.de/10.1594/PANGAEA.900106).
In general, the two first flight section have been over open ocean close to the MIZ. The third section is over the MIZ, while the last long section is in the vicinity of Polarstern, where a "staircase pattern" was flow.
In general, the transmissivity based retrieval shows a good agreement with the MODIS observations as well as the cloud microphysical in situ observations on the same aircraft. Unfortunately, the microwave radiometer retrieval shows significantly lower LWP values, which cannot be explain easily. From the good agreement with in situ and satellite observations we conclude, that our LWP retrieval fulfills the required accuracy to estimate the surface albedo in cloud-free conditions.

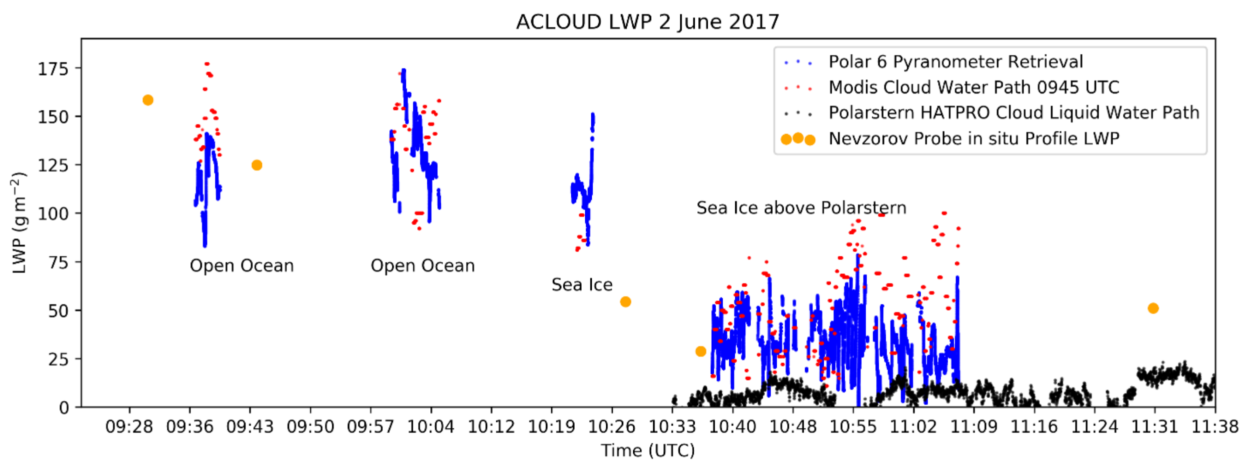

Figure 2 Time series of different LWP retrieval during the 2 June 2017 ACLOUD flight. The blue scatter represent the transmissivity based retrieval presented in this study, the scatter points collocated MODIS cloud water path retrieval. In orange scatter points the vertically integrated in situ profiles from the Nevzorov probe (measuring LWC) on Polar 6 is shown during descents and ascents before and after the low-level flight sections through the clouds. In black scatter the "Cloudnet" retrieval of LWP from the Polarstern research vessel for the time period where the aircraft was close by.