**Answers to comments of Review #2**

We gratefully thank the reviewers for the positive feedback on our submitted manuscript. We appreciate the time they took to extensively read and comment on the given manuscript. The constructive comments are very helpful for the improvement of the manuscript. Our replies to the referees' comments are structured as follows:
*Referee's comments in italic – line numbers according to initially submitted manuscript*
Authors' responses in roman – line numbers according to adjusted manuscript. **Citations from the initial and the adjusted manuscript are given in bold.**

*2.1 Major Comments*

*Consider changing the terminology of "solar" and "thermal-infrared" to shortwave and longwave (and later abbreviated as "SW" and "LW", even the spectral ranges of the broadband radiometers only partially cover the SW and LW) since the SW and LW are more broadly used by the CRE community.*

Although the terms "shortwave" and "longwave" are more widely used, we prefer to use terms that, in our point of view, more precisely define the measured quantity. Whether a wavelength is shortwave or longwave always depends on the perspective, these terms are somehow relative (Bohren and Clothiaux, 2006). For people working with microwaves, also the so-called "longwave" radiation has short wavelengths. Thus, we use the term "solar" instead of "shortwave", which clearly indicates that almost the entire radiation emitted in this wavelength range originates from the Sun. In contrast to Bohren and Clothiaux (2006), we use the term "thermal-infrared" instead of "terrestrial" or "longwave" to indicate that this wavelength range, which is part of the infrared, mostly consists of thermal radiation emitted by objects with temperatures typical for the Earth's atmosphere. The term "terrestrial" might be misleading, since this radiation is not only emitted by the Earth, but also by atmospheric objects.

In the end, the usage of the terms is a rather philosophic question without true and false answers. It is, however, important that the wavelength ranges are specified, which we do in our manuscript.

We added a footnote to the manuscript: "**The terms "solar" and "thermal-infrared" are often referred to as "shortwave" and "longwave". However, since the latter terms might be relative, we use the former terms throughout this manuscript (Bohren and Clothiaux, 2006, page 22f.).**"

However, we abbreviate the terms in the remainder by adding the subscripts "sol", "TIR" (and "tot") to the variables.

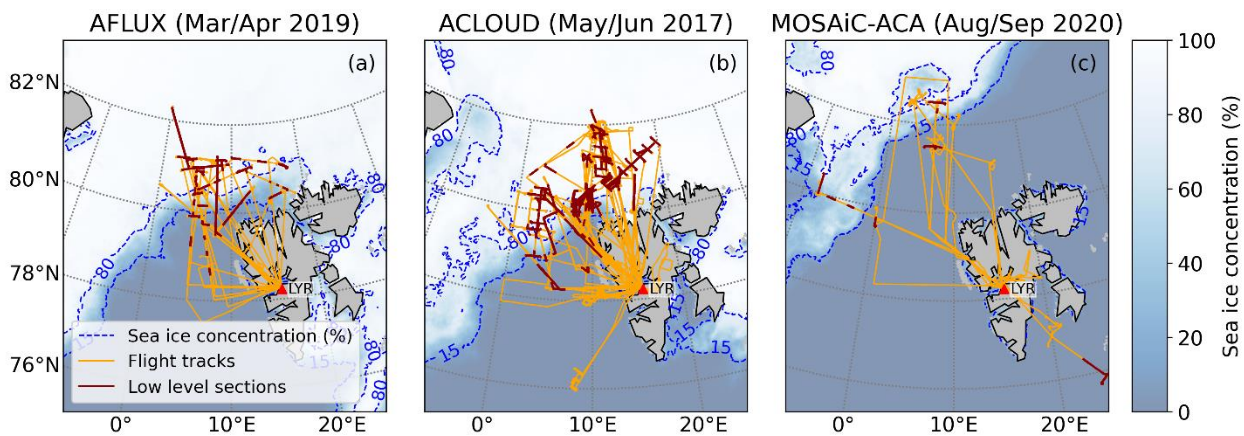
*L80: "However, both results included … observations." I didn't quite get what are the limitations of others studies. Please clarify.*

Here, we wanted to mention that the similarity of the CRE found by the satellite observations (Kay and L'Ecuyer, 2013) and the study of Ebell et al. (2020) at Ny-Ålesund could result from the charactieristic surface types of these observations. In contrast to the other studies, which performed measurements only over bright sea ice or snow, also snow-free surfaces were observed by Kay and L'Ecuyer (2013) and Ebell et al. (2020). To clarify this, we changed the sentence to: "**This similarity likely results from the**

**observation of snow-covered and snow-free surfaces in both studies, while most other aforementioned studies only investigated snow-covered surfaces.**"

*For Figure 1, considering changing the color of red (or orange) to another color (green maybe?) and make the lines for low level sections slightly thinner (or add a little bit transparency in color) so the flight tracks can be better distinguished.*

To increase the contrast between all flight tracks and the low-level sections, we changed the light red color indicating all flight tracks to a brighter orange. The dark red lines for the low-level sections were thinned, as suggested. Additionally, we added LYR (Longyearbyen) as campaign base to the map. See the revised figure below. The first sentence of the figure caption now reads: "**Flight tracks (orange) and low-level sections (dark red) performed during (a) AFLUX, (b) ACLOUD, and (c) MOSAiC-ACA based at Longyearbyen (LYR).**"




*For Figure 2, consider changing the dashed line (or dotted line) to solid line, as dashed lines and dotted lines are difficult to distinguish. Also, the temperature variation of profile near surface is almost invisible, can you experiment with log y axis to see whether the temperature variation near surface stands out more (e.g., temperature inversion)?*

We changed the dashed lines to solid lines. However, rather than changing to a log y axis, we removed the values below 90 m from the profiles of both temperature and absolute humidity. Since only a small number of aircraft ascents/descents reached such low altitudes, the sparse sampling statistics artificially shape the temperature and absolute humidity profiles there. Thus, the interpretation of these values is difficult and not valuable. Additionally, the exact knowledge of the surface temperature is not important, because we assume the surface temperature and, consequently, the surface emission to not change between cloudy and cloud-free conditions. The revised Figure 2 is similar to Fig. 3 shown in these replies, except that the relative humidity is not shown in the manuscript.

Because of the exclusion of the lowest altitudes, we needed to change the sentence "**Although the temperature at the open ocean surface was close to the freezing point during all campaigns, it strongly decreased within the lowermost 80 m during AFLUX.**" to "**The near-surface temperature over open ocean was close to the freezing point during ACLOUD and MOSAiC-ACA, and below -10 °C during AFLUX.**"

*L181-185: Which do you think is the more plausible cause for the much more frequent thin clouds occurrence observed during MOSAiC-ACA? Limitation in sampling statistics (e.g., with more data we will see similar distribution like ACLOUD) or the cloud type associated with Arctic season (e.g., even with more data we will still see predominant thin clouds)?*

We think that especially the low sampling statistics is the reason for the distribution of the equivalent LWP over sea ice during MOSAiC-ACA. The respective data consists of three sets of maximum two-minute samples, two of which were performed within 10 minutes, the third one approximately 45 minutes later. Within this time, we assume the cloud conditions to not have changed significantly, leading to this narrow distribution. Statistically, this is not representative for the sea ice conditions during that season. For a larger sample, we would at least expect a broader distribution and a larger median LWP. Whether this LWP really approaches the ACLOUD distribution is speculative. Climatologies of Arctic cloud properties are sparse. According to Wang and Key (2005, https://doi.org/10.1029/2004JD005720, their Fig. 5d), the mean cloud optical thickness in the Arctic ocean region in September is only slightly lower than in March. In the North Pole regions, the values are similar in September and March/April, but significantly lower than in summer. We added to the text: "**These observations are statistically not representative and very likely don't reflect typical conditions present over sea ice during this season.**"

*Figure 5 is very interesting. I would like to see more explanation about why the albedo change in such way (e.g., the "dip" of albedo at SZA of 75° when transitioning from clear-sky to optically very thin clouds, there must be some counteracting factors) rather than the descriptions of how the albedo change along LWP.*

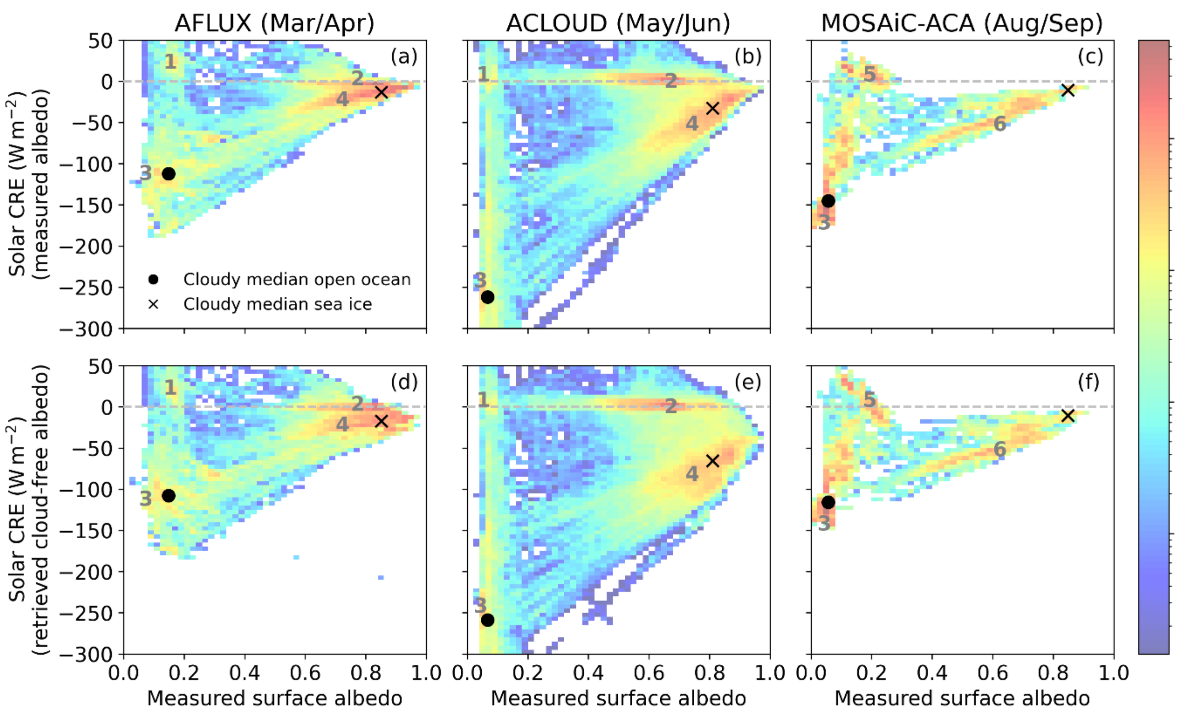
As explained in subsection 3.2.1, two effects (geometry effect and spectral weighting effect) contribute to the change of the broadband surface albedo, i. a., depending on SZA and cloudiness.

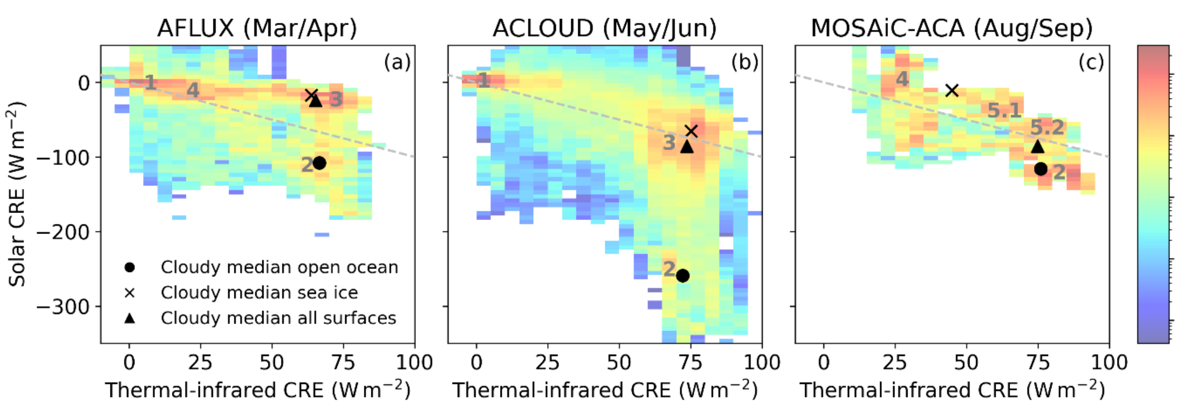
It is shown that the geometry effect dominates over the spectral weighting effect for open ocean, which leads to an albedo decrease with increasing cloudiness (LWP). This is already explained in the original text: "**The broadband open ocean albedo (Fig. 5a) decreases with increasing LWP, which indicates that the geometry effect dominates over the spectral weighting effect. This is due to the relatively low spectral differences of the spectral open ocean albedo (Fig. 4a).**" The surface albedo differences between the different SZAs for cloud-free conditions and optically thin clouds are also explained: "**Similar to the spectral albedo, … the albedo in cloud-free conditions increases for increasing SZA.**" The reason for a higher albedo and higher SZA is explained in Sect. 3.2.1: "**This difference is due to the enhanced specular reflection at the air–water interface for larger incident angles (i. e., SZA), according to Fresnel's equations.**"

For sea ice, the spectral weighting effect mostly surpasses the counteracting geometry effect, leading to an albedo increase with increasing cloudiness (for details, we referred to Stapf et al. (2020) who already did similar analysis for the sea ice albedo): "**Thus, the spectral weighting effect becomes more dominant and leads to an increase of the broadband albedo with increasing LWP (Fig. 5b).**" We agree that the reason of the "dip" occurring for a SZA of 75° is missing. We add the following sentence: "**This feature arises from the geometry effect surpassing the spectral weighting effect for optically thin clouds when the Sun is low enough.**"

*Consider changing the color of the markers (crosses and dots, also in the legend) in Figure 7 and 9 as they are hardly distinguishable between the numbers and add descriptions in the figure caption.*

Instead of changing the color of the markers, we changed the color of the numbers to grey (see below). Hopefully, markers and numbers are now better distinguishable. In the figure captions, we changed the last sentence to “**The symbols represent the median of $CRE_{\mathrm{sol}}$ and the measured surface albedo** (Fig. 7) / **$CRE_{\mathrm{sol}}$ and $CRE_{\mathrm{TIR}}$** (Fig. 9) **over the surface types given in the legend in (a) and only considering cloudy observations (equivalent LWP > 5 g $m^{-2}$).**” to better refer to the markers. Additionally, we added a sentence explaining the numbers of the modes: “**The numbered modes represent (1) cloud-free open ocean, (2) cloud-free sea ice, (3) cloudy open ocean, (4) cloudy sea ice, (5) thin/broken clouds, and (6) cloudy MIZ conditions.**” (Fig. 7) and “**The numbered modes represent (1) cloud-free, (2) cloudy open ocean, (3) cloudy sea ice, (4) thin/broken clouds, and (5.1/5.2) cloudy MIZ conditions.**” (Fig. 9).



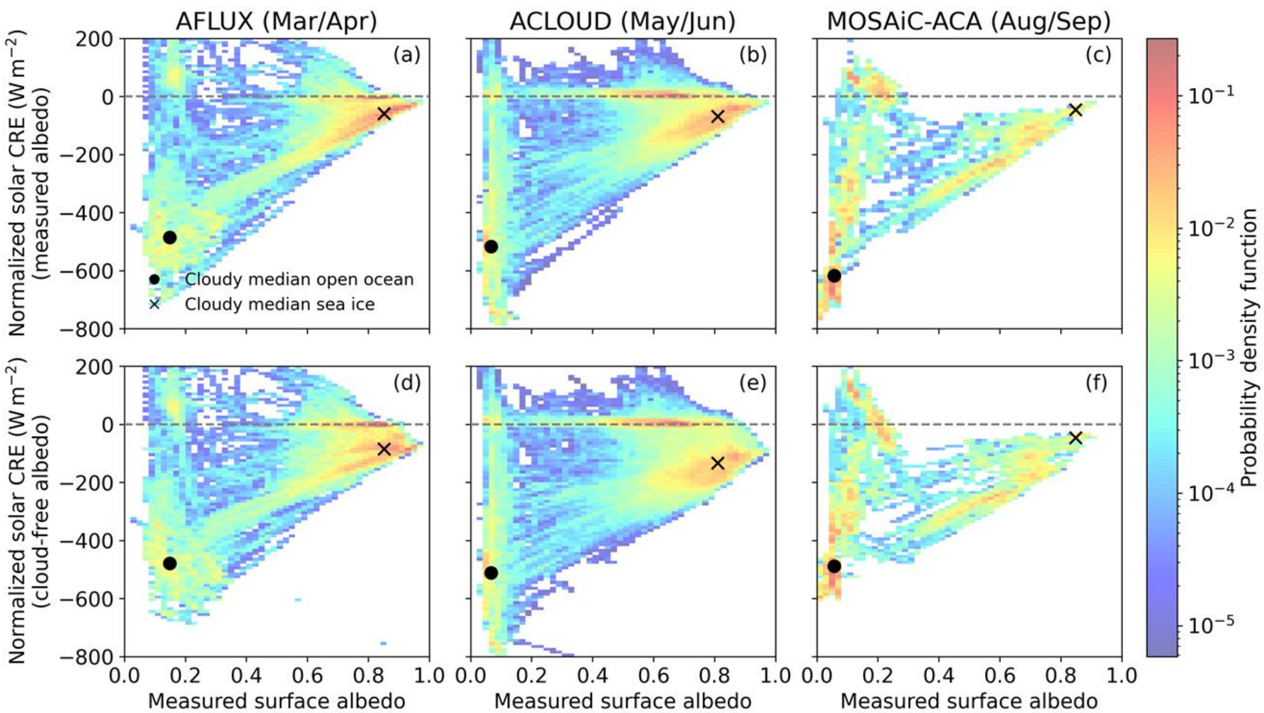



*Figure 1: Same as Fig. 7 in the manuscript, but for $CRE_{sol}$ normalized with the cosine of the SZA on the y-axis. (Figure not included in the manuscript)*

*From L316-322, the author argues the SZA causes the different distributions in CRE among different campaigns. I have an idea of normalizing the CRE with the cosine of SZA (it should not be difficult to do). If SZA is the culprit, the distribution difference should disappear once the CRE is normalized.*

We also normalized the $CRE_{\mathrm{sol}}$ with the cosine of the SZA during our analysis. The result is shown in Fig. 1, which indicates that the modes of all campaigns are similar when normalized $CRE_{\mathrm{sol}}$ values are analyzed. Thus, the SZA is clearly responsible for the $CRE_{\mathrm{sol}}$ differences between the campaigns. Merging the observations of all campaigns (accounting for the different number of data points such that all campaigns are weighted equally) leads to a distribution with four clearly separated modes (Fig. 2). In the manuscript, we decided to keep the original plot with the non-normalized $CRE_{\mathrm{sol}}$ as this provides an absolute value of $CRE_{\mathrm{sol}}$, which finally contributes to the surface energy budget. We modified the respective sentence in the text: "**Although the lower surface albedo contributed to the lower $\boldsymbol{CRE_{\mathrm{sol}}}$ during ACLOUD, a normalization of $\boldsymbol{CRE_{\mathrm{sol}}}$ with the cosine of the SZA (not shown) reveals that the major contribution to the $\boldsymbol{CRE_{\mathrm{sol}}}$ differences between the two campaigns resulted from the different solar illumination as a consequence of the clearly distinct SZA ranges (Table 1).**"

*I quite like the places where you brought up "broken clouds" seen during the MOSAiC-ACA campaign, which have caught my attention in wondering how much 3D cloud radiative effects are there in the Arctic. Technically, the 3D effects should be predominant in the Arctic when broken clouds present as the surface is bright and sun is low (more scattering events). Even though I understand the 3D effects are not the focus of this paper, I would recommend adding some brief discussion about it could potentially favor the radiation closure development in the Arctic.*

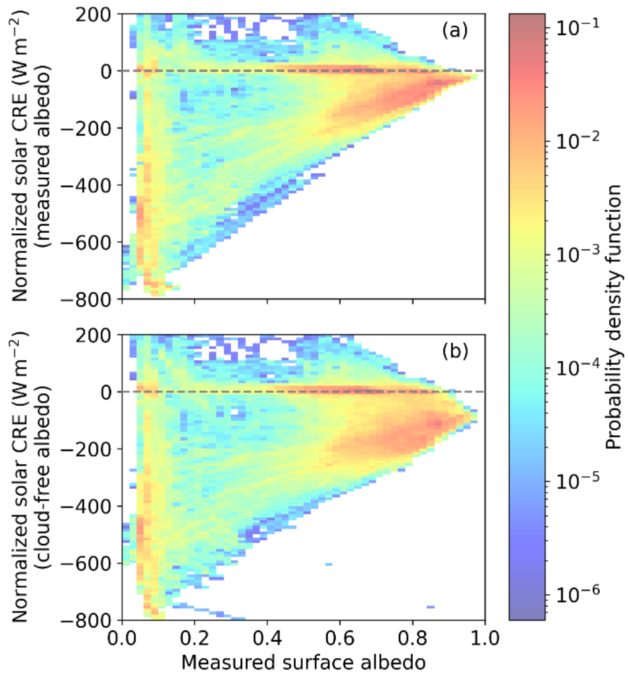



*Figure 2: Same as Fig. 5, but the observations of all campaigns are merged such that all campaigns have equal weighting. (Figure not included in the manuscript)*

We agree that quantifying the 3D effects is not the scope of our study. However, we provide some brief discussion about the topic here: Of course, if we would only be interested in $F_{\mathrm{sol}}^{\downarrow}$, multiple scattering occurring between surface and cloud will be stronger over bright sea ice than over dark open ocean surfaces. When interpreting the 3D cloud radiative effects on $CRE_{\mathrm{sol}}$, the aforementioned effect is counteracted by the surface albedo. Over bright sea ice, an enhanced $F_{\mathrm{sol}}^{\downarrow}$ is counterbalanced by an enhanced reflection at the surface. Thus, the magnitude of $CRE_{\mathrm{sol}}$ over sea ice is always lower than over open ocean. For this reason, 3D effect will appear more dominant over open ocean and are less imprinted in $CRE_{\mathrm{sol}}$ over sea ice. Besides that, cloud types might be different over the different surfaces. We observed broken clouds and the associated effects only over open ocean, mostly during cold air outbreaks (CAOs). When the cold air is advected over the relatively warm open ocean, the stability is significantly reduced, leading to roll convection and forming cloud streets. These cloud streets consist of broken clouds with gaps in between. We discuss the broken clouds for the first time in Sect. 4.1 (Solar CRE) for AFLUX. There we add: "**This solar warming effect is due to broken cumulus clouds, which often enhance $F_{\mathrm{sol}}^{\downarrow}$ compared to a cloud-free situation for several minutes by scattering additional solar radiation towards the surface (cloud enhancement, Mol et al., 2023**[, https://doi.org/10.1029/2022JD037894]**). Broken clouds frequently occur during cold air outbreaks over open ocean, when the cold air advected over the warm ocean reduces the thermodynamic stability and leads to the formation of cloud streets (e.g., Brümmer, 1996**[, https://doi.org/10.1007/BF00119014]**).**"

*Consider adding some thoughts about lesson learned from the three Arctic aircraft campaigns and improvement one can make for future aircraft campaigns in order to progress cloud radiation science in the Arctic (e.g. ARCSIX) in the conclusion.*

We added: "**The results might be biased by the flight strategy and the spatial and temporal selection of low-level sections to satisfy different campaign goals. Furthermore, not all synoptic conditions could be captured due to weather-caused flight limitations. To overcome these limitations and to improve the statistics of the surface CRE, extensive low-level sections are necessary regardless of the weather conditions.**" and later "**The validation of satellite CRE retrievals with airborne measurements might be the key for long-term observations of the CRE over open ocean. This study and the published datasets of the CRE in the Fram Strait (Stapf et al. 2021c, Becker et al., 2023) could provide a basis for such investigations and for further research of cloud-related processes and feedback mechanisms in numerical models.**"

*2.2 Minor Comments*

*L1: <during airborne> to <from>; <three> to <three airborne>*

Thanks for the suggestion, we changed it accordingly.

*L2: suggest to added <– AFLUX (2019 March to April), ACLOUD (2017 May to June), and MOSAiC-ACA (2020 August to September)> after <Svalbard>*

We added this information: "**…: AFLUX (March/April 2019), ACLOUD (May/June 2017), and MOSAiC-ACA (August/September 2020)**"

*L3: <of the surface> to <at the surface>*

Ok.

*L8: <component> to <components>*

Changed.

*L8: <and in combination> to <as well as combined for the study of total CRE.>*

Changed accordingly.

*L90: <airborne measurements of … were performed during three seasonally distinct campaigns in the vicinity of Svalbard> to <three airborne campaigns were deployed to collect measurements of cloud, surface, and thermodynamic properties during different seasons near Svalbard.>*

Changed accordingly.

*L97: <performed> to <deployed>*

Ok.

*L108: add brief description of why the observations are discarded, something like <due to the contamination of radiation signals from …> after <discarded>*

Due to another reviewer's comment, the entire sentence is changed to: "**Because of remaining uncertainties in the estimation of the fraction of direct solar irradiance, the irradiance data for aircraft attitudes exceeding 5° in roll and pitch angle were discarded."**

*L121: <the cloud boundaries> to <cloud boundaries>*

Changed.

*L134: <fice> was not clarified (or I missed it?)*

You are correct, we forgot to introduce the variable $f_{\mathrm{ice}}$, representing the sea ice concentration. We included it in the sentence explaining the measurement of $f_{\mathrm{ice}}$ with the fish-eye camera: "**Additionally, the sea ice concentration $f_{\mathrm{ice}}$ was derived from measurements of a three-channel digital camera equipped with a 180° fish-eye lens (sampling frequency: one image every 6 seconds).**"

*L151: worth adding RH profiles*

We decided against the depiction of the RH profiles for several reasons: First, another panel would reduce the lucidity of the plot. Second, rather than the RH, the AH representing the total amount of water vapor is directly related to the radiative emission by water vapor. Third, the information content delivered by the RH profiles is minimal. We additionally show the RH profiles here (Fig. 3). The important points discussed in the text become obvious: In general, the RH is lower during ACLOUD compered to MOSAiC-ACA, which causes the larger AH during the latter campaign. Additionally, the AH differences between sea ice and open ocean for ACLOUD and MOSAiC-ACA (at least below 1500 m) can be explained by the differences in RH.

*L172: <meidan> to <median>*

Changed.

*L203: <illumination geometry> do you mean <solar geometry>? Just want to confirm.*

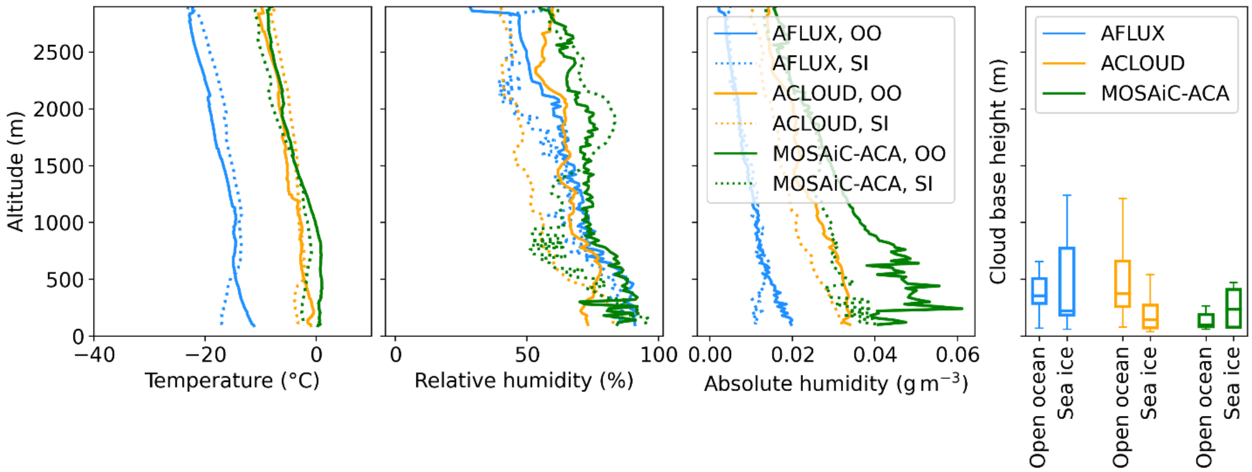



*Figure 3: Same as Fig. 2 in the manuscript, except the additional panel showing the mean profiles of relative humidity (RH). OO – open ocean, SI – sea ice. (Figure not included in the manuscript)*

Partly yes. Beside solar geometry (solar zenith and azimuth angles), we additionally mean by that whether the incident solar radiation is rather direct (as for cloud-free conditions) or diffuse (as in cloudy conditions). We changed it to "**amount of direct and diffuse solar radiation**"

*L230: why? Due to rough ocean surface from high wind speed, thus less specular reflection?*

Exactly, we add the following phrase to the beginning of the sentence: "**Due to a reduction of specular reflection on a roughened surface, …**"

*L247: where does <sea ice concentration> come from? Estimation from aircraft camera imagery? Any reference for figure for this linear combination?*

Please see the answer to the comment on L134. The sea ice concentration $f_{\mathrm{ice}}$ was derived from camera images. To clarify, we replaced "**the sea ice concentration**" by "**… $f_{\mathrm{ice}}$ obtained from the fish-eye camera (Becker et al., 2022)**" and gave a reference.

We noticed that "sea ice concentration" and "$f_{\mathrm{ice}}$" were used inconsistently. We thus replaced all but the first occurrence of "sea ice concentration" by "$f_{\mathrm{ice}}$".

*L251: <amounts to> to <converges at>*

Ok.

*L258: see my earlier comments, why a decrease is observed for SZA of 75 but not 60 under clear-sky?*

See my earlier answer on that comment. ;)

*Figure 6: consider adding "retrieved" and "observed" to the y axis labels for (d) to (f), as well as for the legend labels in (b)*

We do not see the benefit of changing "measured" to "observed" and kept it. In contrast, to make clear that the shown "**cloud-free**" albedo was retrieved, we decided to combine both terms to "**retrieved cloud-free**" conditions in both figures. We also changed it in the text, where appropriate.

*L284: <their mode structures> to <the mode structures over the parameter spaces of surface albedo and CRE>*

With your suggestion and keeping the following sentence, this information would be doubled. Thus, we decided to directly combine the sentence with the following one: "**… the frequency distributions of $CRE_{\mathrm{sol}}$ as a function of the strongly influential measured surface albedo, which are shown in Fig. 7.**"

*L287: add <cloud-induced> before <surface albedo change>*

Added.

*Figure 7: last sentence is unclear, please clarify which symbols are which. Consider referencing back to the text or adding an example to explain. Consider change the color of dot and cross markers to distinguish them from numbers.*

See my answer to your major comment on that. The symbols are clearly attributed to the different surface types using the legend in (a). We clarified that by changing the last sentence as specified in the earlier answer.

*L290: since you are showing broadband irradiance, suggest change <solar spectral range> to <solar range>.*

The terms were used inconsistently throughout the manuscript. We decided to omit the term "**spectral range**" entirely and replaced it by simply "**range**".

*L291: <Figs. 7d, 7f> should be <Figs. 7c, 7f>*

Thank you, we changed it.

*L292: can the mode change/shift due to 3D cloud radiative effects (from thin or broken clouds)?*

Please see my answer to your major comment on the broken cloud effect.

*L300: consider providing the actual values of solar CRE of mode 4 in Fig. 7e*

The actual value of mode 4 in Fig. 7e is given later in the text (**-65 W m$^{-2}$**). Thus, we think that the information "**almost doubled**" in combination with the of -33 W m$^{-2}$ for mode 4 in Fig. 7b is sufficient here.

*L301: <reduction> can be unclear about which way CRE goes, whether more cooling (values become more negative) or more warming (values become more positive). Consider <mitigation>.*

We agree. Thus, we replaced this and similar occurrences by the terms "**weakening**" and "**strengthening**" of the solar cooling effect. Larger/lower solar cooling is changed to "**stronger**" and "**weaker**" (or similar) solar cooling effect. Hopefully, the new terms will be associated less with numbers than the old ones.

*L303: <was increased by 29 Wm-2> to <imposed an artifact of 29 Wm-2 cooling due to the neglect of cloud-induced surface albedo change>*

Ok.

*L304: <slight> to <negligible>*

Ok.

*L309: see my earlier comments, consider mentioning the 3D cloud radiative effects*

See my earlier answer.

*L323: <blurry> to <unclear>*

Ok.

*L341: the cross marker in Figure 7 seems unexplained.*

Actually, none of the markers is unexplained. The legend in Fig. 7a explains the markers. As mentioned earlier, we refer to this legend in the figure caption, such that, hopefully, it becomes clear what is meant by the markers.

*L456: reference shown up as <?>*

*L470: please fix <?, …>*

These two comments have a joint answer.

This “?” is the placeholder for a reference of a data set, which has been submitted recently during the review process. As the reference for this data set is now available, the question marks are replaced by the actual reference (Becker et al., 2023, https://doi.pangaea.de/10.1594/PANGAEA.95775).