Replies to comments of Review #1

We would like to thank the reviewers for their constructive feedback and appreciate their time for extensively reading and commenting on the submitted 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.

Beyond edits related to the reviews, we applied additional revisions to some text passages after carefully going through the manuscript. For these changes, we refer to the track changes file.

"Quantifying the impact of solar zenith angle, cloud optical thickness, and surface albedo on the solar radiative effect of Arctic low-level clouds over open ocean and sea ice" by Becker et al. investigates the relative contributions of cloud properties (summarized as optical thickness) and surface albedo to solar cloud radiative effects using observations made from aircraft during the ACLOUD and AFLUX campaigns. The study concludes that surface albedo overwhelmingly dominates over cloud properties in the difference in observed CRE between ocean and sea ice domains. The study presents some interesting concepts and results. I think it will suitable for ACP with a revision that addresses concerns that are both technical in nature and relate to the overall scoping of the study's motivation and interpretation.

General Concerns:

1. *The introduction needs work. It provides a review of the cloud radiative forcing in the infrared, with discussion of solar effects being absent or incidental. It then says the present study doesn't analyze the infrared, only solar. Seems like the intro should focus on the current state of, and outstanding gaps in knowledge of solar radiation.*

   Thanks for identifying this rather one-sided discussion in the introduction. Usually, the studies cited in the literature review of the CRE investigated both the solar and the TIR CRE, such that there are no larger gaps in knowledge of the solar compared to the TIR CRE. Therefore, we tried to balance the discussion by including the solar CRE in the introduction:

   "In previous research, the CRE at the surface has been characterized as a complex function of cloud properties, such as optical thickness or height, as well as the concurrent solar zenith angle (SZA) and surface and thermodynamic conditions (e. g., Shupe and Intrieri, 2004). In addition, several studies have investigated the seasonal cycle of the surface CRE using ground-based observations over mostly snow- and ice-covered surfaces across the Arctic (e. g., Intrieri et al., 2002; Miller et al., 2015; Ebell et al., 2020). In contrast to lower latitudes, all Arctic studies identified a total warming effect of clouds on annual average, because the solar cooling effect is limited due to the low Sun and the bright surfaces present in the Arctic. Only during summer, a total cooling effect was observed in most cases, when the magnitude of the solar cooling effect surpassed the TIR CRE due to decreasing SZA and surface albedo. Because of this surface albedo dependence of the solar CRE, low-level airborne observations performed during three, seasonally distinct campaigns and analyzed by Becker et al. (2023) revealed a strong total cooling effect of

clouds over open ocean as opposed to the adjacent sea ice surfaces. In contrast to the strongly variable solar CRE, the TIR CRE is less affected by seasonal variability, which results from a frequent compensation of increased emission by clouds for warmer temperatures and stronger water vapour absorption below the cloud (Cox et al., 2015; Becker et al, 2023).

Among the rather qualitative studies assessing the CRE, quantitative analyses focussing on the impact of important drivers on the CRE variability are largely lacking. Solely Shupe and Intrieri (2004) estimated sensitivities of the surface CRE with respect to cloud, surface, and thermodynamic properties by applying a simple CRE parameterization and measurements obtained during the shipborne Surface Heat Budget of the Arctic Ocean drift expedition (SHEBA, Uttal et al., 2002). However, these sensitivity estimates do not account for interactions between the considered properties. Yet, a full separation of the relative contributions of different drivers has only been performed for the surface REB." (lines 44–62)

2. *The study is motivated by the need to constrain cloud feedback estimates, but it does not investigate cloud feedbacks at all, despite what the study sometimes suggests, such as at the top of section 4. Fundamental to the feedback concept is a change in cloud properties as a response to some climate forcing, which may, for example, be measured by cloud forcing. Here the concept is conflated with the difference in cloud forcing between when a cloud is over ocean and when it is over ice. Therefore, the "distinct states" referred to throughout the text are not analogous to Taylor et al., Soden et al. etc. Sometimes, these are referred to as "climate states" (e.g., L200), which is more consistent with the referenced literature, and other times as "locations", "seasons" (L75), or just "states" (throughout), which is less consistent. Regardless, these are not interchangeable concepts. I like the objective here of trying to understand how varying surface and cloud properties result in the CRE that is observed, but the study should not overstate its connection to the problem of cloud feedbacks.*

It is correct that our study does not investigate the cloud feedback and we also did not intend to do so. Our motivation of the cloud feedback constraint was rather meant as an example for application of the CRE. However, the fact that the CRE, due to its dependence on non-cloud properties, is an inaccurate measure for cloud feedback should motivate the need to disentangle the various drivers of the CRE. To make our intention clearer, we rewrote the cloud feedback part of the motivation and put a stronger emphasis on the CRE and its drivers, but still using the cloud feedback example:

"A widespread application of the CRE concerns the constraint of the cloud feedback, which is often approximated by the CRE change between two climate states (e. g., Cess et al., 1990; Cesana et al., 2019; Lutsko et al., 2021). However, Soden et al. (2004) demonstrated that this approach does not yield an accurate cloud feedback estimate, because non-cloud properties, such as surface albedo, aerosol particles, or water vapour, can non-linearly affect the CRE even if the cloud characteristics are unchanged. In fact, negative CRE changes, i. e., decreasing CRE, often coincide with positive cloud feedback. These interactions and the opposing effects of solar and TIR CRE complicate accurate estimates of the cloud feedback, which, thus, represents a key source of uncertainty in climate projections (Kay et al., 2016; Choi et al., 2020; Forster et al., 2021). Therefore, it is

crucial to precisely investigate the CRE and separate the impacts of different cloud and non-cloud properties on the CRE.” (lines 36–43)

Regarding Section 4: We understand that CRE and cloud feedback are different concepts and we never intended to provide cloud feedback estimates with this study. However, in our opinion, it is still feasible to apply a decomposition method similar to the approximated partial radiative perturbation (APRP) technique (Taylor et al., 2007) to our problem, although the original method was developed to disentangle the shortwave (solar) components of different (not only cloud) feedbacks. The common goal of the APRP and our application is to partition a change in radiation budget between two states into the contributions of different drivers. The radiation budget is given as a quantifiable function of the drivers and the contribution of a driver is quantified by simultaneously only changing the value of this single driver between state 1 and state 2, while keeping the others constant. The analogies between both applications are summarized in the following table:

	Original (Taylor et al., 2007)	**This study**
radiation budget difference	net shortwave radiative forcing and response at the TOA → together simply the difference in TOA net shortwave radiation	difference in solar surface CRE
drivers of radiation budget difference	shortwave feedbacks due to changing - surface albedo - cloud properties - atmosphere (e. g., water vapour)	changing - surface albedo - cloud optical thickness - solar zenith angle
states	control vs. perturbed climate or two different times ...	sea ice vs. open ocean or two different seasons, locations, ...

The only purpose of introducing the APRP method in the introduction to Section 4 is highlight the similarities and differences between the APRP and our application. To make this intention more obvious, we shortened the description of the APRP method and put more emphasis on our application in the revised version of this paragraph:

“Based on the parameterization of Eq. 9, the contribution of a driver is quantified by the partial CRE difference resulting from a sole change of the associated variable between the two considered points, while the other variables are kept constant. In parts, this approach is similar to the approximated partial radiative perturbation (APRP) technique applied in climate dynamics, where a parameterization of the solar REB at the TOA is used to decompose the solar REB difference between two climate states into the contributions of various feedback mechanisms (Taylor et al., 2007). However, due to the different quantities and fields of application, the contributions calculated here are not comparable to the results from Taylor et al. (2007).” (lines 211–217)

3. *How many total hours of observations are included here? How many unique clouds are sampled? I suspect the samples are limited. While there are some nice methods and analyses here, we are still working with a few case studies. Therefore, while I agree that similar analyses in the infrared is a good suggestion (L274), the main recommendation for the community should be more studies focused on how cloud and environmental properties combine to produce cloud forcing without the implicit suggestion this is the final word on the solar component. Be careful not to overstate.*

It is correct that the sample analyzed in the manuscript is limited. In total, 6.1 and 13.6 hours of low-level observations were performed during AFLUX and ACLOUD, respectively, including measurements over the marginal sea ice zone that were not taken into account here. We agree that additional, statistically representative data sets could help to solidify the conclusions. Therefore, we added the following sentences to the manuscript:

"Nevertheless, the conclusions are based on limited airborne samples, which might be biased by flight strategy. It would be useful to apply the described method to further, statistically more robust data sets to extensively investigate the impact of changing cloud and environmental properties on the solar CRE." (line 297–299)

Specific Comments:

1. *L95: Can you provide more information on the profiles? How far away are the soundings? Where they found to be comparable to the aircraft data? How did you merge these data?*

The thermodynamic observations during the aircraft ascents and descents form the basis of the thermodynamic profiles, as they were performed locally, confining the respective low-level leg. Only for levels above the maximum flight altitude of the Polar 5 aircraft (usually around 3 km), the thermodynamic state is obtained from the measurements of the radiosonde launched at Ny-Ålesund. The maximum spatial and temporal distance to these radiosoundings is 430 km and 6 hours, respectively. To clarify this strategy, the last sentence of this paragraph is modified to:

"These profiles resulted from thermodynamic measurements during local aircraft ascents and descents adjacent to the low-level flight sections that were complemented by the radiosonde observations at Ny-Ålesund (max. 430 km away) for atmospheric layers above the maximum flight altitude." (lines 101–103)

We did not compare the thermodynamic aircraft and radiosonde measurements. On the one hand, we already applied a local thermodynamic profile for the lower atmosphere, making a comparison to the remote radiosonde observations redundant. On the other hand, the lack of local thermodynamic measurements for higher altitudes makes a comparison impossible. Anyway, despite the impact of water vapour on solar radiation, we expect a potential water vapour difference between aircraft and radiosonde location, especially at higher altitudes, to affect the solar CRE at the surface only weakly.

2. *Can you show comparisons between the simulations and observations when the sky was clear to validate the CF estimates? This will help ensure there is not a mean bias in the CRE.*

Figure 1 compares the measured and simulated solar downward irradiance in cloud-free conditions during AFLUX and ACLOUD as a two-dimensional histogram. As the majority of the data points are distributed around the one-to-one line and the coefficient of determination is close to 1, there is a good agreement between the simulation and observation without any mean bias.

We added one sentence to the text: **"For cloud-free conditions, a comparison between measured and simulated $F^{\downarrow}$ yields a coefficient of determination R^2 of 0.9971, indicating the accuracy of the simulations."** (lines 103–104)

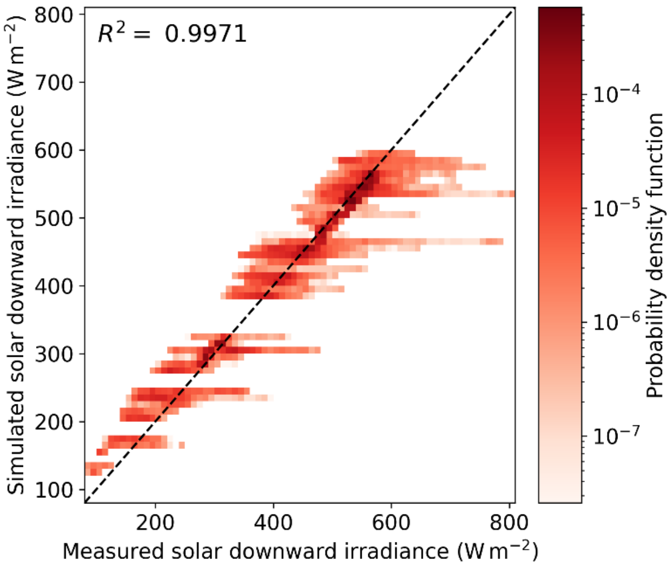

Figure 1: Two-dimensional probability density function depending on simulated and observed downward solar irradiance considering all cloud-free observations (τ less than 1.125) of AFLUX and ACLOUD. The dashed line marks the 1:1-line.

3. *You state the CRE fluxes are w.r.t. the surface (e.g., line 90), but the observations were made at flight level. As you say at L59-60, water vapor is impactful on the solar, and later at L248 that you flew above some cloud layers. You should use the model to assess whether an atmospheric correction is needed to transfer the observations to the surface, and apply it if it is determined to be significant. Otherwise, you should refer to the calculations as what they are, observations from flight level (and calculate accordingly).*

The statement of the importance of the water vapour mentioned in Lines 59-60 holds for the solar downward irradiance in cloud-free conditions excluding the impact of the usually dominating SZA. For the variability of the solar CRE, water vapour only plays a minor role. Nevertheless, it is correct that the water vapour amount above and below the measurement altitude affects the solar downward and upward irradiances. To assess the impact of the flight altitude on the solar CRE, radiative transfer simulations depending on SZA, surface albedo, and cloud optical thickness were performed for both the flight altitude and the surface. For the two analyzed campaigns, the resulting differences are shown as histograms in

Figure 2. In all cases, the underestimation of the CRE at flight altitude compared to the surface is weak and does not exceed 2 W m^{-2}. The larger underestimation during ACLOUD results from the lower SZA and the higher flight altitude (up to 250 m) compared to AFLUX (up to 100 m). Due to the weak impact, we did not apply an atmospheric correction.

We added the following sentences to the manuscript: **"During low-level flight sections, the CRE was retrieved at flight altitude from a combination of airborne radiation measurements and radiative transfer simulations. The resulting CRE values are considered representative for the surface since the flight altitude was consistently lower than 250 m and, according to radiative transfer simulations, the corresponding atmospheric impact low-biased the CRE by less than 2 W m^{-2} with respect to the surface. The irradiances $F^{\downarrow}_{\mathrm{cld}}$ and $F^{\uparrow}_{\mathrm{cld}}$ in cloudy conditions were measured …"** (lines 92–96)

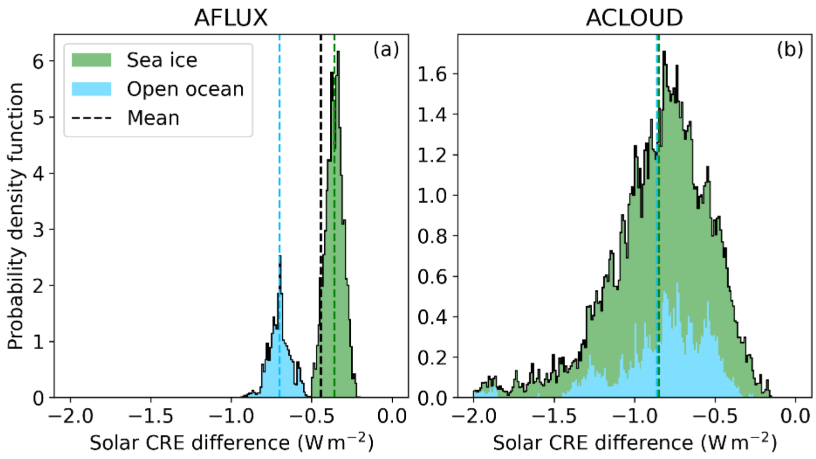

Figure 2: Probability density function of the difference between the solar CRE simulated at flight altitude and at the surface for (a) AFLUX and (b) ACLOUD.

Correcting for the cloud layers (sea smoke) referred to in the comment would be a more difficult task. As this sea smoke revealed a complicated structure and we don't have any information about its microphysical properties, we were not able to assess the impact on the surface CRE. However, since the downward solar irradiance is primarily determined by the cloud optical thickness and these structures were optically thin, we assume that the additional impact of the sea smoke on the surface REB would have been weak compared to the thicker clouds above flight level. In contrast, the albedo and, thus, the upward component are biased significantly by the sea smoke. To account for this effect, we tried to estimate, how the resulting relative contributions would have changed if the sea smoke had been absent. For this purpose, we added the following sentence to the manuscript:

"If the sea smoke had not been present and the open ocean albedo had revealed a typical value of 0.06, the relative contribution of α would have reverted to -2.0 % in favour of the SZA contribution." (lines 267–269)

4. *L111-112: Cloud optical depth is not a property independent from transmissivity. They are different ways of saying the same thing.*
5. *L112: I don't think it is correct to say multiple reflections actually increase cloud transmissivity, though it would confound your ability to calculate an accurate value using the broadband measurements with e.g., Eqs (4,5).*

This reply concerns the previous two comments. Probably, confusion arose, because the cloud transmissivity is usually associated with Lambert–Beer's law. However, this law only describes the transmissivity of the direct solar irradiance. In our study, we are interested in the combined transmissivity of both direct and diffuse solar radiation, which is defined according to Eq. 6 of the manuscript.

Regarding comment 4: Using Lambert–Beer's law, the cloud transmissivity may serve as a quantity equivalent to the cloud optical thickness τ if the SZA is constant. Nevertheless, also the direct cloud transmissivity calculated in this way is formally a function of both τ and SZA. The direct+diffuse cloud transmissivity (Eq. 6 of the manuscript) is far from being equivalent to τ, as it depends on SZA and surface albedo in addition to τ. In this way, the cloud transmissivity is not independent of τ, but τ, in turn, is independent of the cloud transmissivity. This becomes obvious from the definition of τ as the vertical integral of the volumetric extinction coefficient b_{ext} between cloud base and cloud top, where b_{ext} is obtained by integrating the single-particle extinction properties over the entire particle range:

$$b_{\mathrm{ext}}(\lambda) = \int_0^\infty Q_{\mathrm{ext}}(\lambda, r) \cdot A_{\mathrm{proj}}(r) \cdot n(r)\, \mathrm{d}r.$$

The extinction efficiency $Q_{\mathrm{ext}}(\lambda, r)$, with λ being the wavelength, can be approximated by 2 in the solar spectral range. The number concentration $n(r)$ and the projected area $A_{\mathrm{proj}}(r)$ of the cloud droplets solely depend on droplet size r. Consequently, b_{ext}, and, thus, τ, is only a function of the microphysical cloud properties.

Regarding comment 5: Since scattered photos constitute the diffuse radiation component, they do not affect the direct component anymore. Thus, multiple scattering cannot affect the direct cloud transmissivity (Lambert–Beer's law). However, diffuse solar radiation transmitted through a cloud can still undergo multiple scattering events and modify the direct+diffuse solar downward irradiance below the cloud. Since the intensity of this multiple scattering largely depends on the brightness of the surface (darker surfaces increase the probability of absorption), the direct+diffuse cloud transmissivity (Eq. 6 of the manuscript) additionally depends on the surface albedo.

Consider a case characterized by a SZA of 60° and a low-level cloud in 400–600 m altitude with a LWP of 30 g m^{-2}. For these conditions, Figure 3 shows the results of radiative transfer simulations as a function of the surface albedo. The dashed lines correspond to cloud-free conditions, while the solid lines correspond to situations below a cloud. The solar downward irradiance below the cloud strongly increases with increasing surface albedo, while the increase in cloud-free conditions is weak (Figure 3a). As a result, the cloud transmissivity is higher for larger albedo values (Figure 3b). Despite the weak impact of the surface albedo on the solar downward irradiance in cloud-free conditions, Figure 3c clearly shows a decrease of its direct fraction for higher albedo values. The enhanced diffuse component can, thus, only result from multiple reflections between surface and atmosphere, which are favoured in the case of a brighter surface. Although, the radiation is completely diffuse below clouds, these enhanced multiple reflections are responsible for the larger cloud transmissivity.

To stress that we refer to the direct+diffuse cloud transmissivity, we added: **"The cloud transmissivity $\mathcal{T}_{\mathrm{cld}}$ accounts for both the direct and diffuse component of the solar downward irradiance. As $\mathcal{T}_{\mathrm{cld}}$ depends on α and μ in addition to the independent cloud property τ, it is not an ideally suitable quantity to describe the impact of clouds on the CRE."** (lines 119–121)

To make clear that the surface albedo directly affects the strength of these multiple reflections, we change the phrase **"The intensified multiple reflection over brighter surfaces cause ..."** to **"The intensified surface albedo-induced multiple reflections over brighter surfaces cause ..."** (line 121).

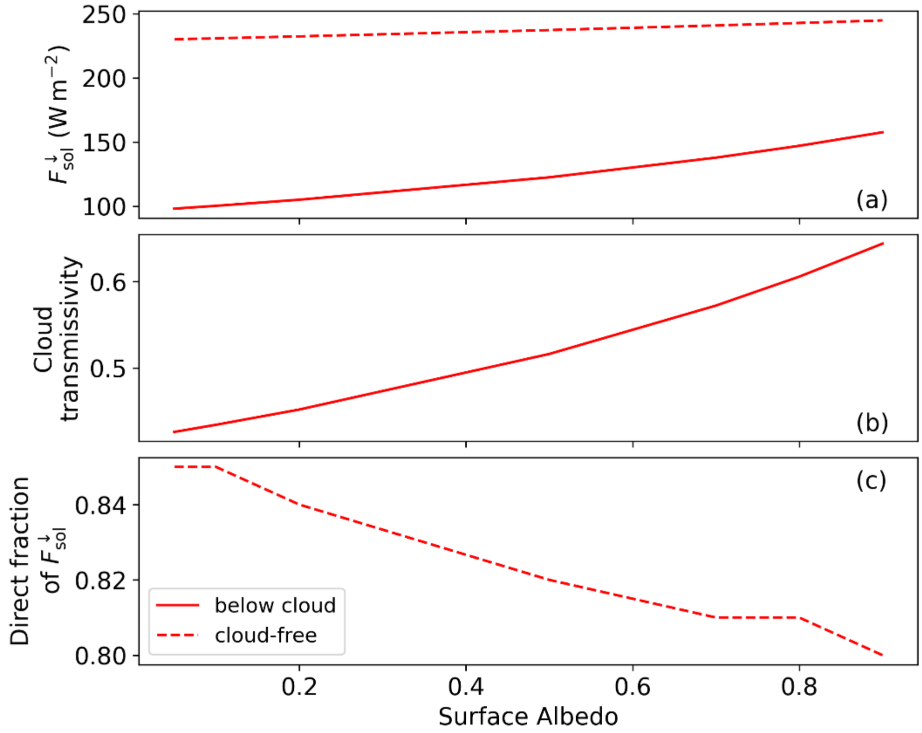

Figure 3: (a) Solar downward irradiance, (b) cloud transmissivity, and (c) direct fraction of the solar downward irradiance as a function of surface albedo.

6. *L30: This point is somewhat stylistic. Ramanathan et al. (1989) defined "cloud-radiative forcing" (CRF), as the net effect, as your equation states. While the literature is not very consistent about this (partly because at TOA, CRE=CRF), at the surface it is useful to reserve the term CRE for the difference in the downwelling components. Consider changing to "CRF" terminology.*

We agree that some studies distinguish between CRF (downward and upward components) and CRE (only downward components). However, as mentioned by the reviewer, this is not very consistent in the literature and other studies refer to the CRE including both components. Since Eq. 2 of our manuscript clearly defined what we mean by CRE, we decided to not change the terminology. Instead, we extended the phrase **"cloud radiative effect (CRE)"** to **"cloud radiative effect (CRE; also referred to as cloud radiative forcing, CRF)"** (lines 27–28).

7. *L30: I think it would be helpful to the reader to include subscripts denoting the fluxes and CRE (CRF) are solar, even though infrared is not analyzed here. In that way, no one will misinterpret the figures if they aren't paying close attention to the text.*

Among the authors, we agreed to skip the subscript because of conciseness of the equations. We clearly mentioned in the beginning that only the solar component is analyzed. Furthermore, the title of the work explicitly refers to the **"solar radiative effect"** and the relevant captions of and within all figures still contain the undoubted term **"solar"**.

8. *L32: "quantify the REB in cloud and cloud-free conditions" is not correct. The statement should be "quantify the REB difference between all-sky and cloud-free conditions". Correspondingly, the "cld" subscript in Eq. 1 and 2 is also not correct: it should be "all-sky". For example, CRF = ALL – CLR could alternatively be defined as CRF = [CLD – CLR]*FCC, where FCC is cloud fraction. I realize it may be a matter of semantics for your application.*

Yes, the statement is correct. The sentence refers to the mentioned (cloudy and cloud-free) net irradiances separately and not to the CRE as a whole. Hopefully, this becomes clearer in the updated version of the sentence: **"The net irradiances $F_{\mathrm{net,cld}}$ and $F_{\mathrm{net,cf}}$ represent the REB in cloudy and cloud-free conditions, respectively, …"** (line 31).

Furthermore, we decided to stick to the term "cloudy" and the subscript "cld". This term is not to be confused with the term "overcast". By "cloudy", we simply mean that any cloud is present in the field of view of the instrument regardless of the cloud fraction. Since we do not analyze completely cloud-free scenes, this is equivalent to "all-sky" conditions. We added the following sentence: **"Here, cloudy conditions refer to situations where a cloud of any fraction is present."** (lines 32–33)

9. *L50: Not all studies find a cooling effect in summer…Miller et al.*

Yes, that is correct. To consider this exception, the text adapted in response of the reviewer's first general comment accounts for this exception: **"Only during summer, a total cooling effect was observed in <u>most</u> cases, when the magnitude of the solar cooling effect surpassed the TIR CRE due to decreasing SZA and surface albedo."** (lines 49–51).

10. *L111: I'm not certain the best place in the text to do this, but somewhere it would be helpful to state that that transmissivity (and tau) are broadband values.*

We agree and added the term **"broadband"** to the place, where the transmissivity is mentioned the first time (line 111). The same was done for the surface albedo (line 97). However, as discussed earlier, the cloud optical thickness has a weak spectral dependence in the solar range. Therefore, we did not explicitly mention that the cloud optical thickness is a broadband quantity.

11. *L155/Figure 3: can you add the symbols (alpha, tau) to the appropriate axis labels? Perhaps also make it clear that the color-coding from (a) is used also in the other panels.*

To address this comment, we produced an updated version of the figure, which is shown in Figure 4 of these replies. We added the symbols μ, τ, and α to the axis labels and the legend. In this way, we get a better connection between the figure and the caption. To clarify the colour coding, we replaced **"(right y-axes)"** by **"(consistent colour coding used throughout the study)"** in line 160 in the text and extended the now third-last sentence of the figure caption by **"…, applying the same colour coding as in (a)."**. An additional change to the figure concerns different line styles of the yellow, green, and blue lines to be independent of the colour coding.

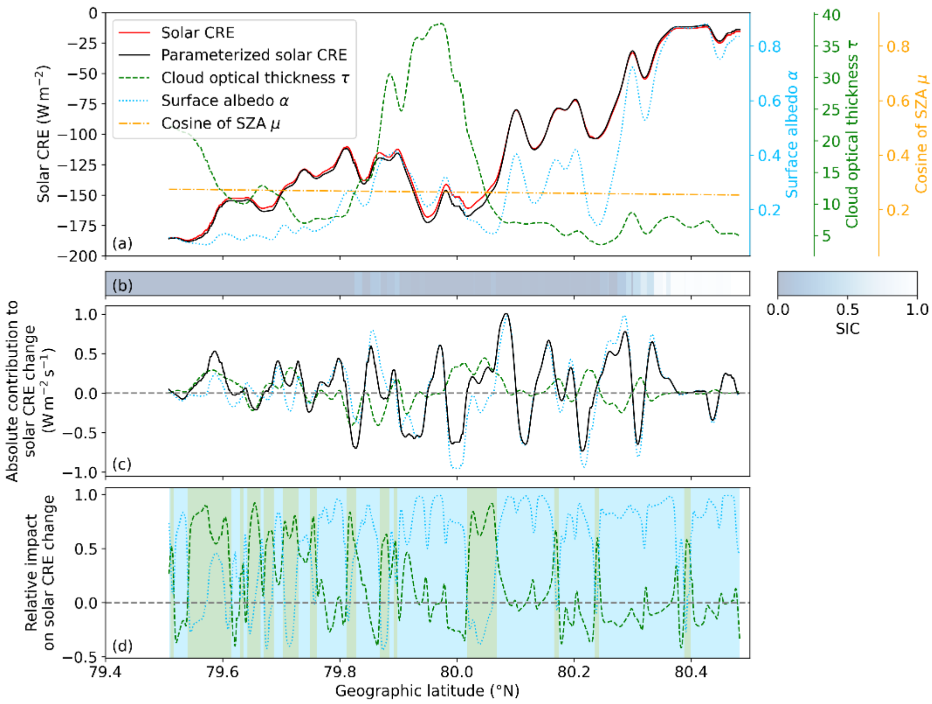

Figure 4: Updated version of the manuscript's Fig. 3: We added the symbols μ, τ, and α to the axis labels and the legend in panel (a) and applied a colour coding to panel (d) to indicate the dominant driver of the CRE (green: cloud optical thickness dominant, blue: surface albedo dominant).

12. *L160: SZA still has an impact on CRE, so maybe replace "impact" with "dependency". I'm not certain what you mean by this sentence because you state that mu will be neglected and then in the very next sentence you set it to a constant. I understand why you made it a constant, but the second statement seems to contradict the first.*

We agree that the SZA still affects the CRE. We actually meant that the almost absent variability of the SZA during the analyzed flight leg did not really affect the variability of the solar CRE during this section. That's why we set the SZA to a constant value, corresponding to the mean SZA during the flight section. So, we actually neglect the variability of the SZA. To express this in the text, we changed **"... the impact of the SZA on the CRE can be neglected ..."** to **"... the CRE change is not significantly driven by the SZA ..."** (line 168).

13. *L188: I don't think this is obvious at all.*

Maybe, the interpretation of the corresponding figure (Fig. 3d in the manuscript) is not accurate enough. Basically, the quantity with the higher relative impact dominates the evolution of the solar CRE at every given point. To better illustrate that, we colour-coded the background of panel (d) according to the dominant driver of the CRE. Please see the updated figure (Figure 4). To account for this change in the figure caption, we extend the second last sentence by **"... and the background colour indicates the dominant CRE driver"**. In the text, **"From Fig. 3d, it is obvious that the CRE change was mostly controlled by τ over open ocean ..."** was replaced by **"The quantity with the largest relative impact indicates the dominant driver of the CRE evolution, which is highlighted by the background colour in Fig. 3d. The frequent green background for latitudes less than 79.7° demonstrates that the CRE change was mostly controlled by τ over open ocean ..."** (lines 196–198).

Editorial Comments:

L21: "on the one hand" L23;

We do not see any problem with this formulation and kept it.

L25: I think you mean "is expected to" not "will"

Yes, thanks for this comment. We corrected it accordingly (line 25).

L36: "antagonism" is an odd choice of word. "Due to these opposing effects..."?

Due to a reorganization of the cloud feedback motivation, the concerned sentence was deleted. However, within the updated text, the phrase **"... the opposing effects of solar and TIR CRE ..."** occurs (line 41).

L111, elsewhere: "suitable"?

We changed **"suited"** to **"suitable"** (lines 120 and 210).

L222-223: I don't understand this sentence. I might be a run-on or something.

We tried to formulate this sentence in easier words. Based on a larger revision of this section, further information is now included around the sentence concerned:

"Since Eq. 13 is underconstrained with the two unknown variables α and τ, the possible solutions to it are distributed along the black dashed line in Fig. 4 and include both (α_1, τ_2) and (α_2, τ_1). However, the fraction of the partial CRE differences with respect to the total CRE difference (i. e., the relative contributions) are not identical for these solutions. To obtain a unique pair of relative contributions, an additional criterion is introduced, which requires (α_e, τ_e) to lie on the straight connection line between the two states (black solid line in Fig. 4), parameterized as

$$\begin{pmatrix} \tau \\ \alpha \end{pmatrix} = \begin{pmatrix} \tau_1 \\ \alpha_1 \end{pmatrix} + s \cdot \begin{pmatrix} \tau_2 - \tau_1 \\ \alpha_2 - \alpha_1 \end{pmatrix}. \tag{14}$$

By inserting Eq. 14 into Eq. 13, this requirement yields a solution for the parameter s that is used to calculate the final evaluation point (α_e, τ_e) = (0.49, 7.7). For these values, the partial CRE differences eventually quantify the absolute contributions of cloud and surface, which amount to 4.3 W m^{-2} and 127.9 W m^{-2} (red numbers in Fig. 4) and correspond to relative contributions of 3.3 % and 96.7 %, respectively." (lines 238–247)

L266: This sentence lacks clarity.

We added some more information for clarity and replace the sentence by:

"Since the method using the total differential can lead to significant uncertainties for too large changes of the drivers, an alternative approach to disentangle their contributions was introduced. This decomposition method is similar to the approximate partial radiative perturbation technique (Taylor et al., 2007) and also applicable to partition the CRE difference between two distinct states into the contributions of the drivers." (lines 287–290)

L274-275: "an as" to "a"?

We changed **"an as simple method"** to **"a similarly simple method"** (line 305).

Added literature

- Cesana, G., Del Genio, A. D., Ackerman, A. S., Kelley, M., Elsaesser, G., Fridlind, A. M., Cheng, Y., and Yao, M.-S.: Evaluating models' response of tropical low clouds to SST forcings using CALIPSO observations, Atmos. Chem. Phys., 19, 2813–2832, https://doi.org/10.5194/acp-19-2813-2019, 2019.
- Lutsko, N. J., Popp, M., Nazarian, R. H., & Albright, A. L.: Emergent constraints on regional cloud feedbacks. Geophys. Res. Lett., 48, e2021GL092934. https://doi.org/10.1029/2021GL092934, 2021.