We thank the reviewer for the helpful comments which we think have helped to improve the manuscript significantly. Especially, by removing the grammatical errors and misleading statements the revised manuscript will be easier to understand for the reader. The detailed replies on the reviewers comments are given below and structured as follows. Reviewer comments have bold letters, are labeled, and listed always in the beginning of each answer. The reviewer comments are followed by the author's comments including if necessary revised parts of the paper. The revised parts of the paper are written in quotation marks and italic letters.

Major Comments:

1. *The necessity for a more thorough literature review*.

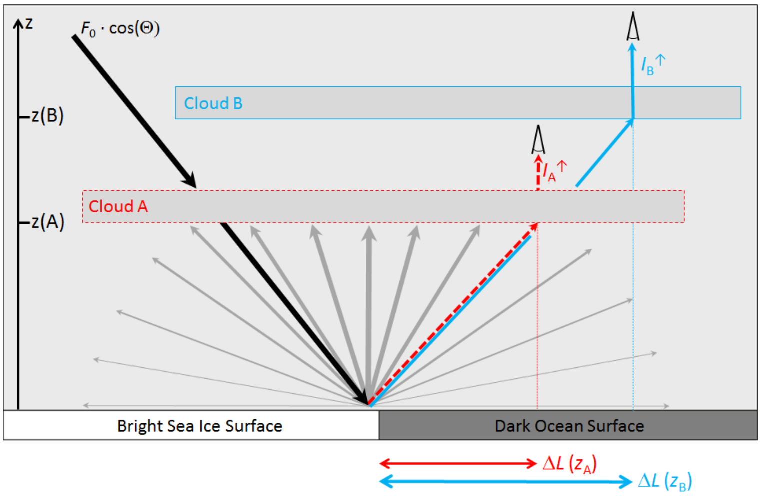
a) The authors use of the catch-all term, '3-D effect' could be better formalized in the introduction (Which are you accounting for? Which are you not accounting for?)

➔ We thank the reviewer for highlighting this lack of information. Now, we have included the following part to the introduction:

"Within the present study, the focus lies on those 3-D radiative effects that are related to the horizontal photon transport between cloud and surface due to isotropic reflection of the incident radiation on the bright sea ice. The goal is to quantify the magnitude and horizontal extent of those 3-D effects as well as their influence on cloud retrievals from the visible wavelength range with a high spatial resolution. In reality, such surface 3-D radiative effects will be combined with cloud 3-D radiative effects due to cloud inhomogeneities."

➔ Additionally, we have included a new Figure (Reply-Figure 1), illustrating the effect of the horizontal photon transport between surface and cloud layer to better describe the 3-D radiative effect we are investigating here. The manuscript is adjusted as follows:

"…In this study, only the latter case is considered, namely, the 3-D radiative effects related to the pathway of the photons between cloud and surface. Horizontal photon transport in the layer between surface and cloud smoothes the abrupt decrease of the surface albedo from large values above sea ice to low values above the open water. For measurements without clouds (Fig. 4f, green in Fig. 6) we could not find similar areas with enhanced γ_λ above the water close to the ice edge.
The theory explaining the 3-D radiative effect, which cause the enhancement of γ_λ, is illustrated in Fig. 7. The incident radiation ($F_0 \cdot \cos(\Theta)$) impinges on the cloud, where scattering and absorption processes take place. Part of the incident radiation is transmitted through the cloud and scattered into the direction of the ice edge (bold black arrow). Sea ice acts similar to a Lambertian reflector and reflects the incoming radiation almost uniformly in all directions (grey arrows). The reflected radiation penetrates the cloud at a certain altitude (red or blue arrows), from where parts of it are scattered into the observation direction. Without sea ice in the vicinity of the measurements, the reflected radiance would be influenced only by the cloud and dark ocean water. The measured nadir radiance $I^{\uparrow}$ above the cloud parcel is enhanced due to the additional radiation reflected from the sea ice into the direction of the last scattering point in the cloud. This effect is significant only for cloudy cases, because of the weak scattering efficiency of the clear atmosphere compared to that of clouds. If we compare the 3-D effect for clouds of different altitude (Fig. 7), the horizontal photon path of the reflected radiation is extended (compare for cloud A (red) and cloud B (blue)). Hence, the range of the 3-D effect increases with cloud altitude."



Reply-Figure 1: Sketch of the 3-D radiative effects between clouds at two different altitudes and the surface in the vicinity of an ice edge. The arrows illustrate the pathway of the photons between source, cloud, surface, and sensor.

b) Variability of the Arctic surface albedo: The Lindsay and Rothrock paper cited (page 1423) does not emphasize solely the large variability seasonally, but also monthly. This variability is a great consideration in how important the 3-D effects presented in the manuscript are important in practice (see major comment #2). This point is given only a brief, summary statement that is well into the paper (page 1444). …

➔ Thanks for this suggestion as it clearly motivates the investigation of the 3-D radiative surface effects in Arctic regions. In the revised manuscript the statement on the monthly variability in the Arctic surface albedo is included in the introduction:

"The mean values for the cloud-free portions of individual cells range from 0.18 to 0.91 and were found to be highly variable at monthly and annual time scales (Lindsay and Rothrock, 1994)."

"However, even when ice and ice-free areas are perfectly separated by the retrieval algorithms, 3-D radiative effects may still affect the cloud retrieval over ice-free pixels close to the ice edge. With respect to the large temporal and spatial variability of the Arctic surface albedo as described by Lindsay and Rothrock (1994), the investigation of the 3-D effects becomes even more important.…"

… I disagree with the author's statement that near-infrared snow/ice surface albedo decreases only slightly compared to the visible (see, for example, measurements shown in Platnick et al., (2001; reference(s) listed at end of review)). In fact, the reduced variability in bright snow/ice surface conditions at near-infrared channels is the reason why satellite algorithms do not use the 645 nm wavelength channel to retrieve cloud properties over snow/ice, but rather the 1.2 micron plus 1.6 micron channel in the case of MODIS (Platnick et al., 2001; 2003; Krijger et al., 2011), as the authors have done.

➔ The reviewer is completely right. By mistake, we switched the words "slightly" and "significantly" in the original manuscript. Thank you for pointing at this. We revised this sentence and included also quantitative albedo values for the wavelength 1.6 µm.

"These differences significantly decrease in the near-infrared wavelength range (α_{water} = 0.01 and α_{snow} = 0.04 at λ = 1.6 µm wavelength; Bowker et al., 1985), but still slightly alter the radiative transfer."

… I also note that the authors cited the Krijger results, from which I also draw my finding that the literature review needs more thorough treatment.

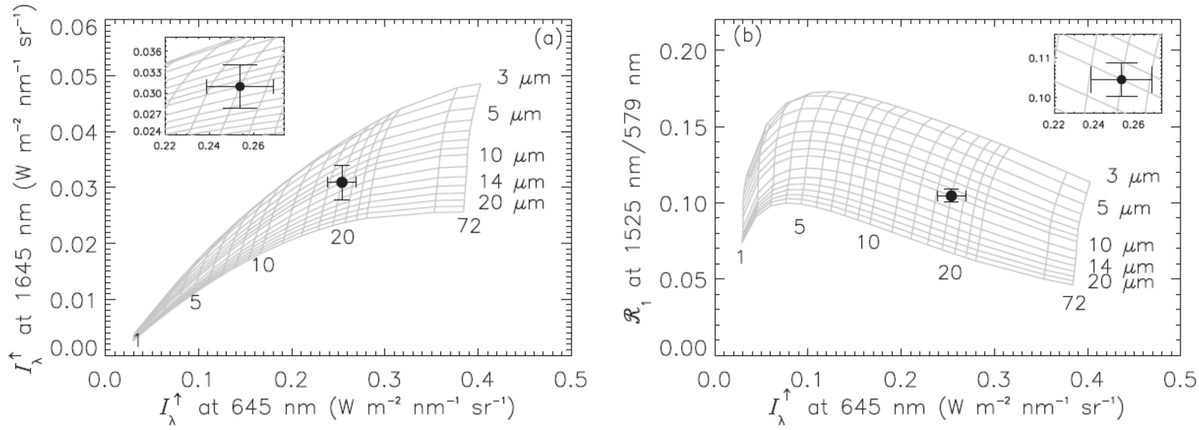
➔ We totally agree with the reviewer. A couple of references, e.g. necessary to discuss the problems of cloud retrieval in arctic regions, have not been addressed in the original manuscript. This is changed in the revised version. In particular, based on the references suggested by the reviewers, it is clear that we overemphasized the difficulties of cloud retrievals over bright surfaces and were wrong with the statement that cloud retrievals are not possible over ice surfaces. The reason for our misleading statement was that we focused only on the measurements with the imaging spectrometer AisaEAGLE, which covers only wavelength in the range from 400 nm to 1000 nm. For this spectral range, cloud retrievals over ice surfaces in fact are not possible without additional information (as it is stated by Krijger et al., 2011). But of course it has to be mentioned that this is only valid for the visible wavelength range and can be overcome by introducing near-infrared wavelength channels. We thank the reviewer for highlighting this lack of information, which necessarily must confuse the reader. We revised the relevant parts in the manuscript (also with respect to your later comments on MODIS) and introduced a series of new references including Platnick et al. (2001, 2004), Platnick and King (2003), and Krijger et al. (2011).

> *"A highly variable Arctic surface albedo as observed during the VERDI campaign complicates the cloud retrieval introduced by Bierwirth et al. (2013). In fact, retrievals of cloud microphysical and optical properties using only visible wavelengths are strongly biased by a bright surface (Platnick et al., 2001, 2004; Platnick and King, 2003; Krijger et al., 2011). To overcome this limitation, near-infrared channels are introduced in the retrieval algorithms instead of the visible channel used over dark surfaces. E.g., for MODIS the 1.6 µm band reflectance is applied as a surrogate for the traditional non-absorbing band in conjunction with a stronger absorbing 2.1 or 3.7 µm band (Platnick et al., 2001, 2004; Platnick and King, 2003). However, an accurate separation between sea ice and open water needs to be performed before the retrieval algorithms are applied. Operational algorithms such as that for MODIS use NOAA's (**N**ational **O**ceanic and **A**tmospheric **A**dministration) microwave-derived daily 0.25° **N**ear Real-Time **I**ce and **S**now **E**xtent (NISE) dataset (Armstrong and Brodzik, 2001; Platnick and King, 2003) to identify snow- or ice-covered scenes."*

c) The applicability of the selected cloud retrieval algorithm to Arctic conditions: The authors apply the method of Werner et al. (2013) to Arctic conditions. I think their point here is that the Werner cloud retrieval (developed for trade cumuli over an ocean surface impacted by thin, overlying cirrus) is also applicable to Arctic conditions, given good cloud clearing. I would like to see more discussion of the support for their retrieval band combination (in line with comments of 1b as well).

➔ The choice of the method by Werner et al. (2013) is justified by the following points. We refer to Werner et al (2013), because the general approach using ratios instead of absolute radiances was applied here as well. Second, the method is not restricted to cases when cirrus is above the aircraft (we have chosen data with clear sky conditions above the aircraft) but also improves retrieval uncertainties in this cases. It further improves the retrieval technique from Bierwirth et al. (2013) by using ratios of radiances instead of total radiance only. In comparison to the retrieval grid, derived by the two-wavelength retrieval from Bierwirth et al. (2013), the ratio method further results in a better orthogonality of the τ and r_{eff} solution space (please notice Reply-Figure 2). This leads to a better separation of the τ and r_{eff} solution space. For airborne investigations of τ and r_{eff} with large spatial coverage and high spatial resolution (as we want to perform it in future studies), this will result in a better accuracy of the retrieved values. To make our decision using the ratio method by Werner et al. (2013) more clear, we included the following part in the revised manuscript:

"The retrieval grid is constructed from the simulated γ_λ at 645 nm wavelength on the abscissa and the ratio of γ_λ at 1525 and 579 nm wavelength on the ordinate. This wavelength and the wavelength ratio was chosen in order to improve the retrieval method by Bierwirth et al. (2013). The choice of wavelength follows the method presented by Werner et al. (2013). This method creates a retrieval grid with a more separated solution space for τ and r_{eff} than the classic two-wavelength method by Nakajima and King (1990) or Bierwirth et al. (2013). Furthermore, it effectively corrects the retrieval results for the influence of overlying cirrus and reduces the retrieval error for τ and r_{eff} caused by calibration uncertainties (Werner et al., 2013). For airborne investigations of τ and r_{eff} with large spatial coverage and high spatial resolution, this will result in a higher accuracy of the retrieved cloud properties."



Reply-Figure 2: Comparison of classical two-wavelength retrial method by Nakajima and King (1990) and ratio method by Werner et al. (2013). Graphs adapted from Werner et al. (2013), not included in the manuscript.

2. <u>*Meeting the challenge of interpreting the theoretical results to those that are important in practice.*</u>

As mentioned in preamble, the authors have presented very detailed simulations. However, it is difficult to draw the practical implications from the simulations. In my opinion, this is due to the following reasons: uncertainty analysis, spatial averaging, and organization of paper (see comments 1a and 1b above, and comment 3 below). In particular, while I find Fig 15 interesting, I don't agree that it could be used (as is) to correct the retrieved cloud optical thickness and particle size, due to the many assumptions, different scale factor, and the choice of your retrieval wavelengths.

➔ We have revised the manuscript with regard to your suggestion. Please find our revisions below in subsections a-c.

➔ With regard to Figure 15, we agree with the reviewer that our statement about a possible "correction" is too ambitious. In fact, due to the large number of parameters changing the 3D-effect (shape, size, distribution of ice flows, cloud properties) and appropriate assumptions to be made, a corrections seems only reasonable when all parameters are known. In that case, a correction is not necessary anymore as the entire scene will have been accurately modeled with radiative transfer simulations anyway. Therefore, we removed this statement.

a) Uncertainty analysis and interpretations – This comment derives from what I feel is missing from the article, or hypothesis/findings which could be better set up (in introduction) and summarized (in conclusion). …

➔ With regard to this comment and in line with your later comments on the length of the paper, we revised the introduction and summary as well. The hypothesis and findings should now be better clarified. With regard to this topic, the main changes for the introduction are:

"… Within the present study, the focus lies on those 3-D radiative effects, which are related to the horizontal photon transport between cloud and surface that occurs due to isotropic reflection of the incident radiation on the bright sea ice. The goal is to quantify the magnitude and horizontal extent of those 3-D effects, as well as their influence on cloud retrievals from the visible wavelength range with a high spatial resolution. In reality, such surface 3-D radiative effects …"

➔ With respect to the revision of the summary, please see the information given under 2c.

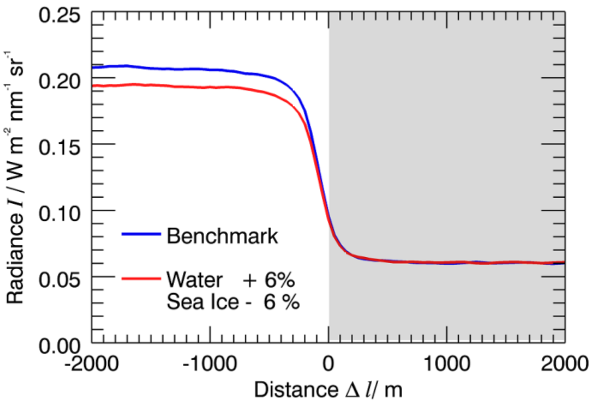
… For example, in comment 1b, I noted the relative importance of incorrect surface albedo assumption (or unaccounted for natural variability in the surface albedo) on the modeled radiance fields to the 3-D effects. It would not require numerous, detailed calculations to provide, for instance a value for upwelling irradiance over your assumed dark ocean value (plus a reasonable 5% for a measurement uncertainty) and compare it to the measured and modeled (average) values shown in Figure 6. Similarly, uncertainty bars (or even, better, retrieval values derived from your measurements) would be beneficial to interpreting Figure 15 (in addition to spatial averaging that I comment on below). …

➔ We hope we got the point right that this comment addresses uncertainties with respect to the accurate value of sea-ice albedo. Actually, this is what we already tried to discuss at Page 1435 Line 28 – Page 1436 Line 6 in the old manuscript. To make this point more clear, we revised this part and elaborate it in more detail.

"Furthermore, simulations with varied values of the surface albedo were performed (not shown). Based on the measurement uncertainty of AisaEAGLE, the surface albedo of the dark ocean water and bright sea ice was varied by ± 6 %. Over the dark ocean area, the simulations show almost identical results with differences far below 1 % in γ_λ. Compared to the measurement uncertainties, those differences in the surface albedo are of less significance for ΔL. Indeed, the albedo has a larger effect over the sea-ice surface (up to 10 %) due to changing the albedo value relative with 6 %, which corresponds to an absolute change of ± 0.05 compared to 0.002 absolute change for the water surface. For the investigations presented here, the effect over the dark ocean area is relevant only."

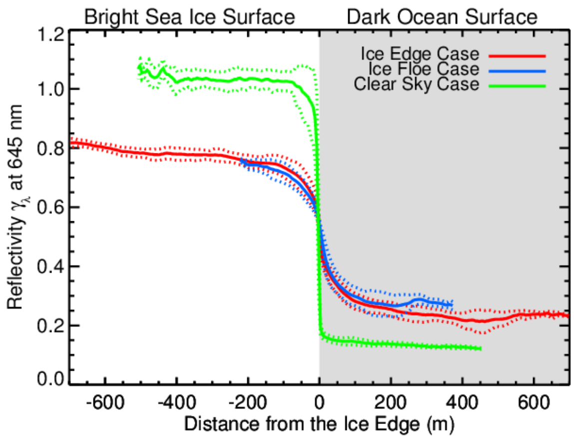
➔ Additionally, we present the results of the sensitivity study with respect to uncertainties in surface albedo here: See Reply-Figure 3. To confirm that a measurement error in the albedo is of less importance for ΔL, please see Reply-Figure 3. The blue line represents simulations with an ice albedo of α_{ice} = 0.91 and a water albedo of α_{water} = 0.042. The red line represents the same simulation, except changes in the albedo of minus 6 % (α_{ice} = 0.8554) over the ice surface and plus 6 % (α_{water} = 0.04452) over the dark ocean surface. The 6 % error was chosen with respect to the measurement uncertainty of AisaEAGLE. Compared to the measurement uncertainties from the cases presented in the manuscript (Reply-Figure 4), over the dark ocean water, differences due to uncertainties in the surface albedo covered area are of less significance. Over the sea-ice surfaces, the difference of 6 % has a larger effect due to the larger value of the sea-ice albedo. However, for our investigations only the effect over the dark ocean covered area is of interest.

Considering the number of Figures included in the manuscript, we do not present Reply-Figure 3 in the revised manuscript, but give the numbers of the sensitivity study.

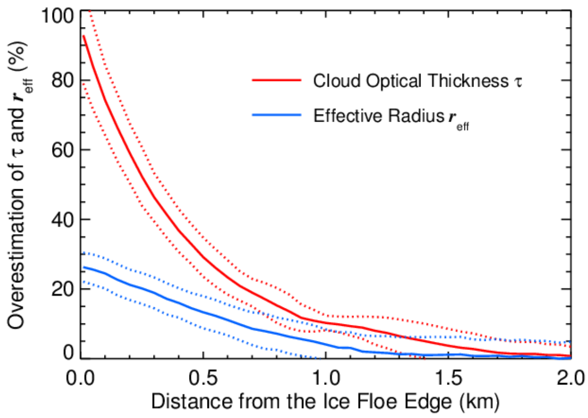


Reply-Figure 3: (not included in the resubmitted manuscript)

➔ We have revised Figures 6 and 15 including measurement uncertainties. The uncertainty range is illustrated by dotted lines, which represent the standard deviation from the measurements and simulations, calculated for each distance to the ice edge.



Reply-Figure 4: Revised Figure 6

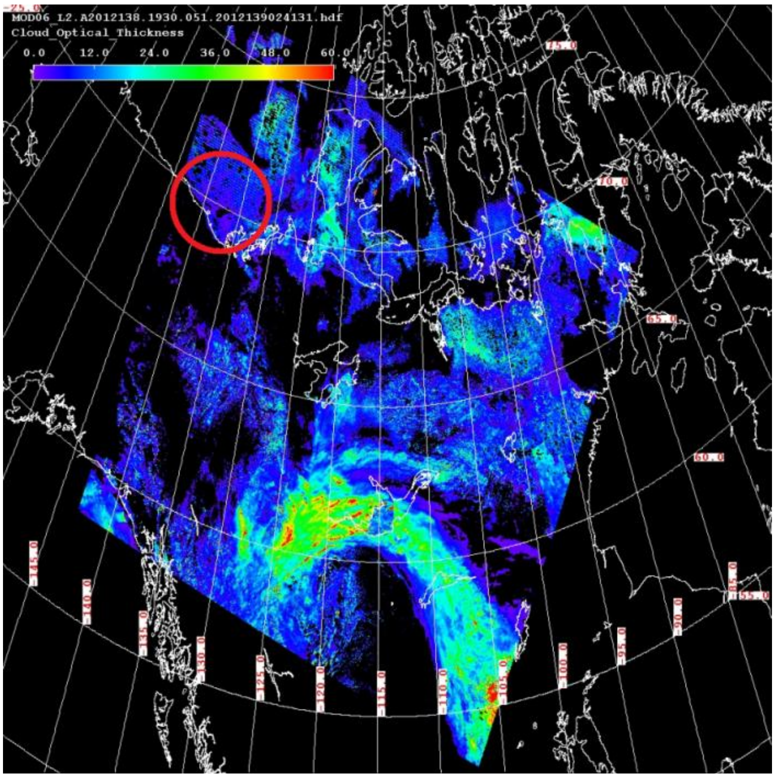


Reply-Figure 5: Revised Figure 15 (now 16)

… Again, only because you remark on MODIS in your article, I mention that the MODIS operational cloud retrieval has associated uncertainties, which include those due to spectral surface albedo (implemented since collection 5; current version is collection 6), which could accompany Figure 1 and support the valid point that retrievals of clouds over snow/ice are challenging. (see Platnick et al., 2004). This could be used to strengthen the statement, "We estimate the cloud optical thickness from the MODIS image to be in the range…" (page 1428).

➔ A discussion about the uncertainties of MODIS retrieval is given in the revised manuscript. See reply above. Furthermore, "We estimate the cloud optical thickness from the MODIS image to be in the range…" was a rather poor choice of wording. Since we had a detailed look at the level-2 MODIS products (see Reply-Figure 6), which gave us quantitative numbers of τ in the surroundings of the measurement area, we changed it to the following:

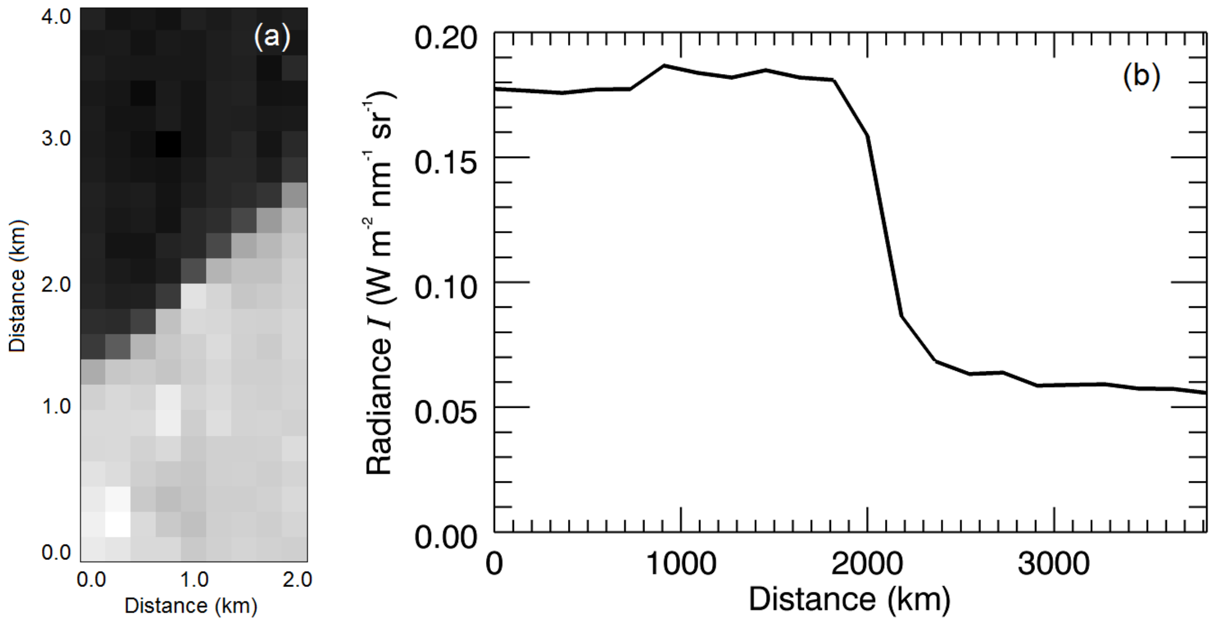
"τ was obtained from AisaEAGLE measurements above open water far from any ice edge using the retrieval method presented by Bierwirth et al. (2013). An average value of τ = 5.3 ± 0.5 was derived, which agrees with the MODIS level-2 product showing values for τ between 0.02 and 15.5 (τ = 3.6 ± 2.5) in the investigated area."



Reply-Figure 6: Level-2 MODIS product. Cloud optical thickness.

b) Spatial averaging – In general, the authors conclude the horizontal transfer of radiation is detectable within a distance of ~ 2km or less from ice edge, with various dependencies on cloud properties, and ice floe size/shape/area and proximity of individual ice floes to adjacent ice floes. Have you considered spatially averaging your results from 50 m pixels to 1 km pixels, to more closely align with the pixel size of operational imagers, such as MODIS, which you reference in your manuscript?

➔ The reviewers suggestion points at a topic worth for detailed discussion. However, even if it might not be pointed out clear enough in the original manuscript, our intention of the study was not to relate the observed effects to satellite observations of different scales. Therefore, we did not vary the spatial scaling but focused on the full resolution obtained with AisaEAGLE. To transfer those investigations to satellite retrievals, several crucial changes would have to be applied to our current work, e. g. changing our wavelength choice, which is not possible for the limited measurements of AisaEAGLE. In contrast, at the moment we rather want to use the full capacity of the spatial resolution of the imaging spectrometer AisaEAGLE to investigate the 3-D radiative effects on small horizontal scales. During future projects, when also the AisaHAWK (see comment 1c) is available, we will be able to perform the retrieval also with near-infrared wavelength. For those measurements and reason of comparisons, it would be valuable to investigate the scaling of observations to the pixel size of operational satellite imagers. However in a first attempt, we scaled the AisaEagle observations to a 50 times larger grid. For the case with the elongated ice edge, presented in Fig. 4a of the original manuscript, this results in a pixel size of 180 m into the flight direction by 220 m across the flight direction. Please see Reply-Figure 7. The pixels next to the ice edge show still enhanced and reduced radiances. Furthermore, the smooth decrease can still be observed from the cross section presented in Reply-Figure 7b.



Reply-Figure 7: a) Scaled image of the measurement case from Fig. 4a in the original manuscript. b) Cross section into the direction of flight for the center pixel of the image. Not included in the resubmitted manuscript.

c) Organization of paper – The paper is long, but the most significant challenge to reading the paper comprehensively come from a lack of organization, which, by necessity, then results in multiple instances of redundant prose. In section 4 (model studies) could you, instead, present the material by the physical dependency you are trying to quantify versus the current approach of model case studies organized from basic to more complicated? I feel this will reduce the length, and also make clearer the distinction between ΔL and ΔLcrit, and their usage throughout the article. …

➔ We started to replace all variables (cloud optical thickness, effective radius, upwelling radiance radiance, downwelling irradiance, …) by its symbols (τ, r_{eff}, $I_{\lambda}^{\uparrow}$, $F_{\lambda}^{\downarrow}$, …). Furthermore, we agree that Section 4 was way too long. We revised this section and significantly shortened it, especially by removing most of the repetitions or summarizing them in Section 4.2 (repetition of input parameters such as τ, cloud altitude or geometrical thickness) and 4.2.1 (general findings such as the description of the enhanced or reduced reflectivity in the vicinity of ice edges). Furthermore, we have resorted single paragraphs, which makes this section even shorter and avoids unnecessary back and forth switching between the single parameters. Now, we complete the investigations of a single parameter, before discussing the next one (ice edge length, sea-ice area,…). By revising the Section, we also hope that it is more clear to the reader and that the original order of the single investigations (straight edge, single circular flow, group of flows, real scenario) is from basic to complex scenarios.

… In section 6 (summary and conclusions), I also feel tightening the prose (perhaps even by half!) and summarizing the results by general impact, versus re-iterating specific results would be much more effective. As another example, a prime motivation for your approach (that a simplified albedo field is necessary in a general characterization of the individual influences), is not presented until the last page of the article. Overall, while I am sensitive to the fact that this request I onerous, I think it is necessary.

➔ We agree with the reviewer that the summary in many instances was not written efficiently. We tried to follow the suggestions by the reviewer, revised this section and shortened it by almost the half summarizing only the most important results from the main part.

3. *What is your source of near-ir measurements*?

Section 2 discusses the instruments, and spectral range of AisaEAGLE (400-970 nm). What is your source of near-ir measurements? Section 5 discusses simulations at near-ir wavelength where liquid water absorbs (hence sensitivity to particle size), necessary for the cloud retrievals. While the authors mention the further work expanding the implications of this study to retrievals of cloud properties in the Arctic region, the results of this paper would be improved through a couple of your own results (adding a few derived points to the simulated curve in Figure 15, for example).

➔ This comment by the reviewer may have evolved from a misunderstanding due to an insufficient introduction of this section. The reviewer is right that AisaEAGLE only covers the visible wavelength range of up to almost 1000 nm. However, in Section 5 no measurements were applied at all. The whole study is based on radiative transfer simulations as our measurements do not cover the wavelength needed to apply the retrieval method by Werner et al. (2013). We still have done this study as outlook with regard to future studies, when a near-infrared imaging spectrometer (AisaHAWK, 1000-2500 nm wavelength) might be available. Intelligible, this is a legitimate question, since this information was not included in the manuscript yet. We have revised the manuscript and added a few more words at the point in the manuscript where we introduce the retrieval method by Werner et al. (2013).

"To quantify the magnitude of this overestimation, a synthetic cloud retrieval is investigated. The retrieval is based on simulations only in order to investigate also the uncertainties of retrieved r_{eff}, which cannot be derived from the current setup of AisaEAGLE measurements during VERDI. The limitation of AisaEAGLE to visible wavelengths restricts the retrieval to τ (Bierwirth et al., 2013). However, near-infrared measurements might be available by use of additional imaging spectrometers such as the AisaHAWK. Therefore, this study addresses both quantities τ and r_{eff}. To do so, the retrieval based on forward simulations is applied to the γ_λ field of a 3-D simulation where the cloud optical properties are known exactly."

Minor Comments:

1. Multiple instances of "ground overlaying cloud", in text and in figure captions, is confusing terminology. Replace instead with "overlying cloud", or simply "cloud" (or some variation of these) given that we know clouds are above the surface.

➔ The reviewer is right. "Ground overlaying" is a bad choice to characterize low-level clouds, which are touching the ground. However, we could not find an appropriate word, so we decided to replace "ground overlaying" by "low-level" and to add the altitude in quantitative numbers, from which it should become clear that the cloud is touching the ground. We changed it at each point where it occurred in the manuscript.

*"For a low-level cloud at 0–200 m altitude, as observed during the Arctic field campaign **VER**tical **D**istribution of **I**ce in Arctic clouds (VERDI) in 2012, an increase of the cloud optical thickness τ from 1 to 10 leads to a decrease of ΔL from 600 to 250 m."*

"From the two measurement cases presented here (τ = 5, h_{cloud} = 0–200 m), a distance ΔL of 400 m was observed."

***"Figure 8.** Simulated mean γ_λ across an ice edge for clear-sky conditions as well as for low-level clouds between 0 and 200 m altitude, τ = 1/5/10, and r_{eff} = 15 µm. …"*

***"Figure 10.** (a) Distance ΔL as a function of the cloud base altitude h_{cloud} for a cloud with a geometrical thickness of Δh_{cloud} = 500 m and different τ. (b) Distance ΔL as a function of the cloud geometrical thickness Δh_{cloud} for a low-level cloud with cloud base at h_{cloud} = 0 m and different τ."*

2. The sentence "the low Sun in summer and its absence in winter combined with usually high surface albedo…" could lead to confusion. All clouds warm in the absence of sunlight, irrespective of cloud altitude or surface albedo. I think what you are trying to say is that for conditions of low Sun and high surface albedo, the terrestrial warming dominates the reflective cooling. Could fix by re-formulating sentence, or removing the "absence in winter" part. It's just semantics.

➔ That is true. The wording we have used in the former manuscript could be misleading. We followed your suggestion and removed the part "absence in winter".

"However, the low Sun in summer combined with a usually high surface albedo lead to a dominance of the terrestrial (infrared) radiative warming of low clouds (Intrieri et al., 2002b; Wendisch et al., 2013)."

3. Lindsay and Rothrock (1994) analysed albedo in 200 km^2 cells (not 20 km^2) – page 1423.

➔ We corrected this mistake.

"Using Advanced Very High Resolution Radiometer (AVHRR) data from the polar-orbiting satellites NOAA-IO and NOAA-11, Lindsay and Rothrock (1994) analyzed the albedos of 145 different 200 km^2 cells in the Arctic."

4. The ending sentence to one paragraph ("The individual 3-D effect of heterogeneous surfaces in cloud free situations…"), should be moved to the starting sentence of the following paragraph – (page 1424).

➔ The last sentence belongs to the next paragraph. We changed this according to the reviewers suggestion.

5. obverse – observe (page 1426)
➔ corrected

6. status – stratus (page 1427)
➔ corrected

7. Remove an extra "each" (page 1430).
➔ removed

8. Two suggested wording changes for "Furthermore, the simulations…of the mean nadir radiance *for* a certain area…or if the enhancement is*, on* average*,* counterbalanced" (page 1432-1433).

➔ We followed the reviewers suggestion.

> *"Furthermore, the simulations are used to clarify whether these 3-D radiative effects result in an enhancement of the mean γ_λ for a certain area or if the enhancement is, on average, counterbalanced by the decrease of γ_λ above the sea ice."*

9. relative – relatively (page 1434).
➔ corrected

10. Suggested wording change "As a reference also a clear-sky scenario was *also* simulated…" (page 1434).

➔ We revised the whole section (please see comments above), for which reason this sentence was removed.

11. Missing word "This results from the reduction *in* contrast between the dark.." (page 1435).
➔ Word "in" included

12. Misplaced text? From "On the other hand, the decrease of …" through end of paragraph would be better incorporated two paragraphs preceeding. (page 1435).

➔ We followed the reviewers suggestion and moved this part up. Furthermore, we changed the order of ΔL and ΔL_{crit} (now ΔL_{HPT}) to avoid an unnecessary back and forth switching, as it was before.

"To compare the results with the measurement example in Fig. 6, the distance ΔL_{HPT} defined by Eq. (3) is analyzed. $\gamma_{\lambda,water}$ is set to the IPA values above water. For the cases presented in Fig. 8, ΔL_{HPT} increases with increasing τ from 100 m at $\tau = 1$ to 250 m at $\tau = 5$ and to 300 m at $\tau = 10$. This shows that the horizontal photon transport increases with τ due to increased scattering inside the cloud layer.
In contrast to ΔL_{HPT}, the distance ΔL defined by Eq. (4) decreases from 600 m (at $\tau = 1.0$) to 400 m (at $\tau = 5.0$) and to 250 m (at $\tau = 10.0$). The decrease of ΔL suggests that the area in which γ_λ is enhanced and a cloud retrieval might be biased is smaller for optically thick clouds. This is related to the decrease in contrast between cloud covered sea ice and cloud covered ocean if τ increases. The difference Δ(IPA) between $\gamma_{\lambda,ice}$ and $\gamma_{\lambda,water}$ decreases from $\gamma_\lambda = 0.87$ for the clear-sky case to $\gamma_\lambda = 0.44$ for $\tau = 10$, mainly due to the increasing reflection of incoming radiation by the cloud. If τ increases, $\gamma_{\lambda,water}$ increases which results in a higher uncertainty range exceeding the γ_λ enhancement also in areas closer to the ice edge. Therefore, the γ_λ enhancement becomes less significant for a cloud retrieval compared to the measurement uncertainties. Since we aim to retrieve τ above water areas enclosed by ice floes, in the following ΔL is used to quantify the 3-D effects."

13. One too many clouds? "For an increasing cloud altitude of a cloud…" (page 1436).

➔ We removed one "cloud" after altitude.

"For an increasing altitude of a cloud with a geometrical thickness of 500 m, ΔL increases from…"

14. proofs – proves (page 1437).

➔ corrected

15. Word change "To quantify the influence…we *quantified* ΔL." (page 1437).

➔ The reviewer is right. "quantified" fits better than "analyzed". We changed this according to your suggestion.

16. Awkward sentence "For all values of *simulated* optical thickness…". Use instead, perhaps, "For simulations at all optical thicknesses, …"(page 1437).

➔ We revised this by the following:

"For all simulated τ, ΔL increases with an increasing radius of the ice floe, …"

17. Define SDs (page 1441).

➔ We defined SD as standard deviation.

18. kind of – approximately (page 1445).

➔ We changed this according to the reviewers suggestion.

19. Incorrect statement "The different patterns of overestimation …suggest that the 3-D effects can be larger at absorbing wavelengths" (page 1445).

➔ The reviewer is right. The statement is the wrong way round and contradicts the statements given before. Accordingly, we revised this part.

"Furthermore, Fig. 16 shows that the overestimation of τ increases approximately exponentially starting at about 1.5 km distance, while the overestimation of r_{eff} increases more slowly and only extends up to a distance of 1.0 km. This indicates that the magnitude of the 3-D effects depends on the wavelengths. In all simulations shown in Sect. 4.2, a wavelength of 645 nm was used for the retrieval of τ. However, the retrieval of r_{eff} also requires simulations at 1 525 nm in the absorption band of liquid water. Therefore, the smaller magnitude and horizontal extent of the overestimation of r_{eff} compared to the magnitude and horizontal extent of the overestimation of τ suggest that the 3-D effects will be smaller at absorbing wavelengths."

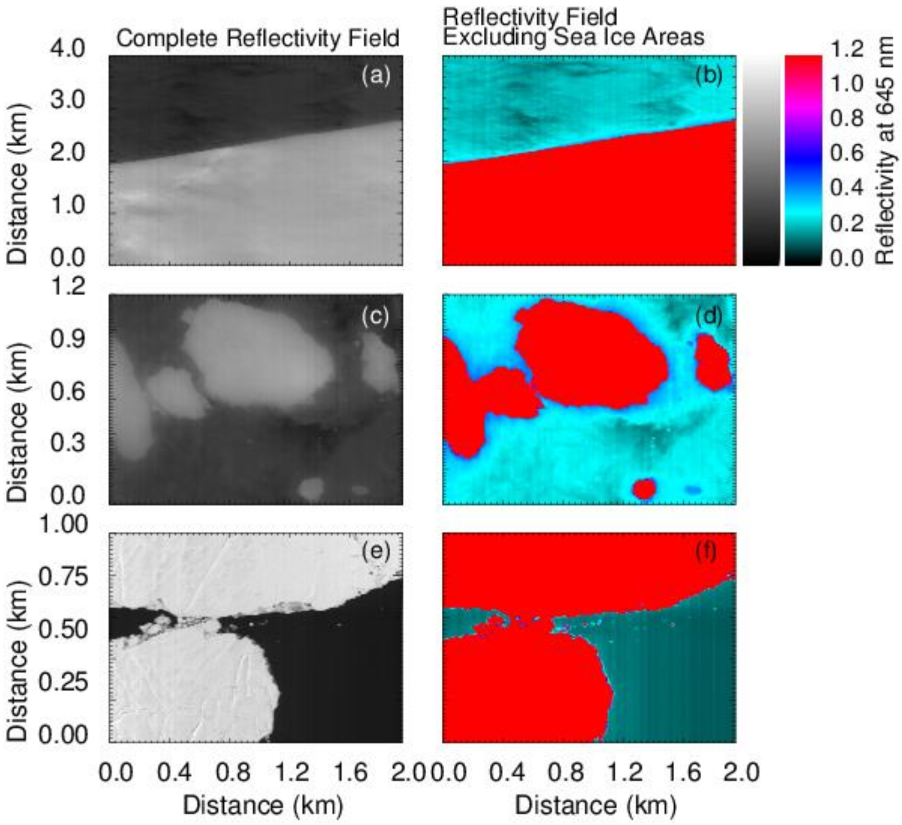
20. weather –whether (page 1447).

➔ Corrected

Figure Comments:

1. Suggest replacing the color bar in Figure 4 with a more dynamic scale range, or (even though I don't usually suggest doing this!), utilize different scale ranges for Figure 4e-f, than 4a-d.

➔ We revised this Figure (see below Reply-Fig. 8) and color-coded the images, which contain the ice masks. Due to the use of reflectivities instead of radiances, the span between extreme values became closer, which supports the use of the same legend for each image. The mentioned narrow bright bands around the sea-ice edges should now be easier to identify.



Reply-Figure 8: Revised Figure 4.

2. For all figures with units, please place units in open parentheses (), instead of after a slash.

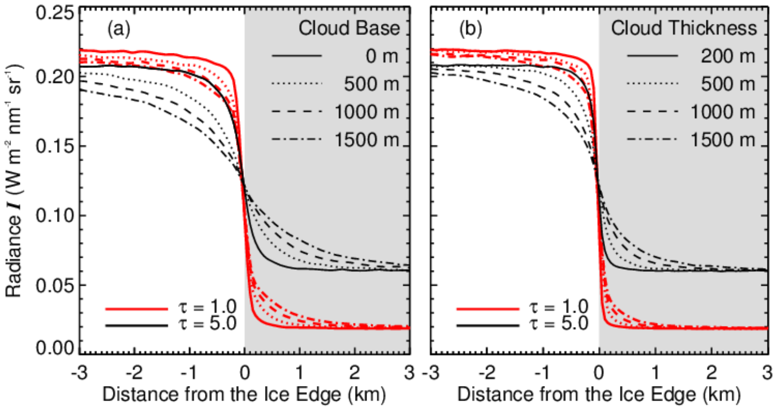
➔ According to this comment, we revised all Figures and placed all units in open parentheses now.

3. Figure 5 – this is an incredible result!

➔ Thank you very much. We are encouraged to read this comment.

4. Figures 8a-b – It is difficult to interpret various curves, on left hand side of each plot.

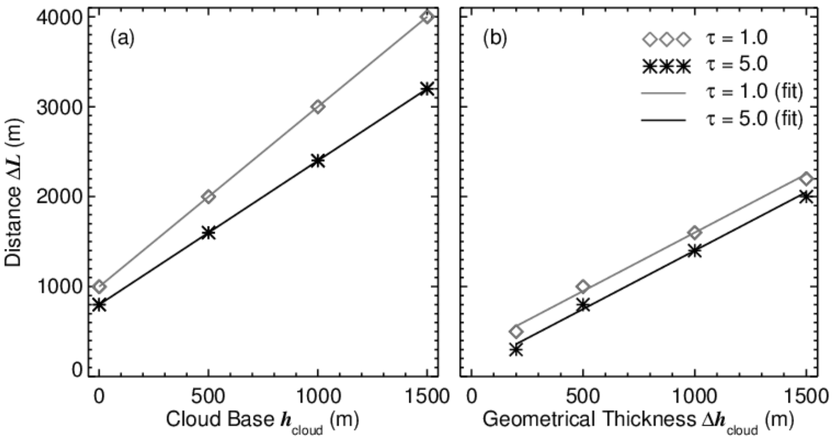
➔ That is true. In the former graphs it was difficult to distinguish between the single curves, especially on the left side of each panel. We tried to fix this, using different colors for curves of different cloud optical thickness. The separation between both should be better now. Please view Reply-Fig. 9.



Reply-Figure 9: Revised Figure 8 (now Fig. 9)

5. Figure 10 – check your symbols, especially the curve for tau=1 and tau=10, as currently this plot contradicts your results in Figure 9.

➔ We thank the reviewer for highlighting this mistake. Indeed, not Figure 10 (now Fig. 11) was wrong, but Figure 9 (now Fig 10). We revised this Figure, See Reply-Fig. 10a. Additionally, we included the simulations for clouds with different geometrical thickness in a second panel (b).



Reply-Figure 10: Revised Figure 9 (now Figure 10): *"(a) Distance ΔL as a function of the cloud base altitude h_{cloud} for a cloud with a geometrical thickness of Δh_{cloud} = 500 m and different τ. (b) Distance ΔL as a function of the cloud geometrical thickness Δh_{cloud} for a low-level cloud with cloud base at h_{cloud} = 0 m and different τ."*

6. Figure 14 – Perhaps revisit this figure if you decide on an alternative wavelength combination for your results.

➔ As we kept the wavelength choice as it was (please see our comments above), we have not revised this figure.