Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-344-RC2, 2019

Atmospheric Measurement Techniques Discussions
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Interactive comment on "Shortwave Radiative Effect of Arctic Low-Level Clouds: Evaluation of Imagery-Derived Irradiance with Aircraft Observations" *by* Hong Chen et al.

Anonymous Referee #2

Received and published: 12 December 2019

1 General remarks

The manuscript analyses airborne radiation observations and satellite observations of Arctic clouds. A surface albedo parametrization is derived to account for the inhomogeneous Arctic sea ice surface. Spectral and broadband are compared to radiative transfer simulations which are based on satellite observations. The spectral irradiance is analyzed to untangle uncertainties resulting from the surface albedo and the cloud optical properties.

In general, the analysis of airborne observations in remote Arctic areas is of high value and provides one rare tool to validate satellite observations. Therefore, the study has high potential and is within the scope of AMT. It could have a wide scientific interest and might contribute to improve our understanding of Arctic clouds. However, the manuscript lacks in several major issues and therefore, does not exhaust its full potential. These issues have to be reassessed in detail before publishing the manuscript.

First, the objective of the study is not well presented and outlined. Based on the title and introduction, the readers expectations and the presented analysis may strongly differ. This deficiency might results from a non-existing description of a general approach and methodology how the measurements can and will be used to validate and improve satellite observations. Such a general strategy is an important part of the manuscript in order to promote future application of the methods. Based on the unclear objectives also the conclusions are weak and leave many questions unanswered putting off the reader with promises for future studies. Finally, a throughout uncertainty estimation is missing, which is mandatory if observations are used for validation purposes. I'm sure, there are options to restructure and improve the manuscript in a way that it presents the full potential of the study.

Below, I compiled a list of comments which have to be considered in a revised version of the paper. There might be some contradictory statements which result from my misinterpretation of the text when first reading the manuscript. I am sure the authors will know how to weight in such cases and how to improve the text to avoid misinterpretations by other readers.

2 Major comments

2.1 Unclear objective of the study

After reading the title and the introduction it is somehow unclear, what the manuscript aims to obtain: irradiance or the cloud radiative effects. The introduction does not match the title. The analysis and methods presented in the study also show not what was promised in the title:

"Shortwave radiative effect" was calculated and discussed only briefly. Most of the analysis concentrates on irradiances. The introduction does not give an overview on how cloud shortwave radiative effects are commonly derived. In the analysis CRE is only discussed in two sentences. Neither the method and uncertainties are introduced nor are the values discussed. This does not justifies the title of the manuscript.

"Imagery-derived irradiance": To me, this implies, that camera images are used and integrated into an irradiance. Or at least, that the irradiance is directly derived from images. That's not done in the manuscript and also not at all covered by the introduction. What the authors did is a parametrization of the surface albedo based on the sea ice fraction, which was observed from a camera. So I suggest to remove the word "imagery-derived" from the title.

This misleading title leaves the reader searching for the actual objectives of the study. Unfortunately, also the motivation given in begin of the manuscript does not fit to what finally was achieved. E.g.:

"Validation of CERES-MODIS derived irradiance": In section 2, the authors state that one objective is to validate CERES-MODIS derived irradiance. This is confusing after reading the title and introduction. CERES irradiance is a different story compared to estimating the CRE. And I also do not see a CERES product in the study. The authors theirself state, that the design of the measurement strategy failed to compare to the CERES product. MODIS retrieval and own radiative transfer simulations are applied. However, on Page 13 line 36 the authors conclude, that CERES observations are used to constrain the observations. This was not done. To avoid confusion, I suggest to remove CERES form the argumentation. Still, comparing irradiances is not the same as estimating the CRE.

"longwave radiative effects": There is one section in the introduction on the long-wave effect of water vapor on the surface radiation budget. But the title of the manuscript suggest, that the study is on solar effects only. So longwave radiative effects by water vapor is kind of irrelevant. The manuscript also does not include a study on the radiative effects of the water vapor profiles. Only the pure profiles are discussed.

After all, I had the feeling that the manuscript shows a potpourri of separate analysis, without a clear major goal. This probably is not true, but the manuscript requires a more clear objective. From what I read, the study aims for a closure study, which validates the MODIS cloud product by airborne observations of irradiance. Could this be the major aim? Or do you aim identifying radiative processes in the Arctic atmosphere related to surface-cloud interaction?

I think it is important to clarify the main objective of the manuscript and concentrate on the major aspects needed to achieve these goals. If the aim is a closure study, then I suggest to remove the estimation of cloud radiative effects, which currently is misleading. Or at least shift the calculation of CRE to the end of the study, after the irradiances have been compared. This would allow to extend the validation for CRE based on the uncertainties/conclusions which have been found already before when comparing the irradiances.

2.2 Methodology needs to be outlined

Several comparisons of different quantities (albedo, irradiance above, below clouds) are shown in the manuscript. However, it is not always clear what the purpose of each individual step is. The general and also the specific methodology of the analysis should be outlined. In the conclusion the authors write about "developing a validation approach". I don't see a clear validation approach in the study. If there is a strategy, then this needs to be outlined precisely in the begin of the manuscript. Maybe any schematic showing the different comparisons broadband, spectral, flight one, flight two might help.

It is also confusing, how and when the data of both flights is used. It took long until I understood, that the two flights provide different observations (high vs. low flight altitude). I suggest, that the authors clearly report, what is different between both flights. Why two cases are needed and how the observations are mixed/combined in the study?

Similarly, the motivation of section 3.1 was missing and leaves many open questions when reading the section. These questions should be addressed before starting the analysis:

What is the purpose of this analysis and of the parametrization of surface albedo with snow fraction? I only can guess. Wasn't surface albedo measured directly with BBR and SSFR? Where is the need to parameterize surface albedo if albedo is measured anyway?

As the surface albedo properly is an input to the radiative transfer model, section 3.1 should be presented before explaining the radiative transfer simulations.

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2.3 Only limited conclusions

The conclusion section does contain a lot of "may"s and "if"s. More questions are raised than answered. The authors theirself are hesitant to draw conclusion: "sheds some light on these questions", "the actual surface albedo may deviate from commonly used climatologies", which is more than obvious. Also the limitations of the limited data set for conclusions is acknowledged. Based on these little new results, the entire section, especially the last part of the conclusion read more like an outlook, indicating, that the study did not improve much. It is not mandatory to make big improvements, but I also do not see any method, approach on how to improve all the issues that are summarized in the conclusion section. To improve the manuscript it would be helpful to present and discuss a method or approach of how to perform validation studies based on similar measurements as shown in the study. As mentioned by the authors, there is potential to process more data from the airborne campaign. To do so, a clear approach with step A, B, C,... should be presented in the manuscript.

2.4 Uncertainty Analysis

The study aims comparing measured irradiance with simulations. As the data is intended for use in a comparison study, a discussion of the the measurement uncertainties is fundamental. No uncertainty ranges are indicated in the plots.

BBR: What about the accuracy of the data? What is considered in the data processing? Are the BBR instruments actively levelled? The SSFR is levelled. What makes this for a difference comparing SSFR and BBR?

The same holds for SSFR. What are the final uncertainties? How the radiometric calibration contributes to the uncertainties? What is more important, correcting the angular response or tracking changes of the radiometric sensitivity over time?

P10 L13: There are several sources of uncertainty in the determination of the surface albedo. How the estimation of the surface albedo affects the uncertainty of the final results/study?

Also extend the discussion of uncertainties by the MODIS cloud retrieval. Only undetected clouds have been considered so far. What about cloud phase, the second thin cloud layer, surface albedo assumed in the retrieval?

3 List of specific comments

P2 L9: From the abstract it is not clear, why two independent estimates of the surface albedo (from SSFR/BBR and from the camera imagery) are needed?

P2 L16: How large is the radiative effect of the non-detected clouds? This is an important value when MODIS misses a significant faction of clouds.

P3 L14: The study by Hartmann and Ceppi (2014) does not fit to the topic of cloud radiative effects. Direct radiative effects by sea ice loss has nothing to do with clouds unless you argue, that the expected increased cloud cover over increased area of open water is not able to compensate the reduced reflection of solar radiation by the surface.

P3 L17: Explain acronyms CERES-EBAF, 2BFLXHR-LIDAR.

P4 L2: This presented state of the art on spectral albedo of Arctic surface types is very pessimistic and does not consider recent publications which cover a lot more data also derived from airborne observations (areal and temporal variability):

Perovich, D. K., Tucker, W. B., and Ligett, K. A.: Aerial observations of the evolution of ice surface conditions during summer, J. Geophys. Res., 107, SHE24-1–SHE24-14, https://doi.org/10.1029/2000JC000449, 2002.

Malinka, A., Zege, E., Heygster, G., and Istomina, L.: Reflective properties of white sea ice and snow, The Cryosphere, 10, 2541–2557, https://doi.org/10.5194/tc-10-2541-2016, 2016.

Malinka, A., Zege, E., Istomina, L., Heygster, G., Spreen, G., Perovich, D., and Polashenski, C.: Reflective properties of melt ponds on sea ice, The Cryosphere, 12, 1921–1937, https://doi.org/10.5194/tc-12-1921-2018, 2018.

P4 L29: Fairbanks is in the center of Alaska. Where did you fly over Arctic sea ice?

Figure 1: Add longitude and latitude.

P6 L22: Were cloud properties derived from the SSFR measurements? If not, I suggest to remove this statement here.

P6 L26: What is the resolution (number of pixel) of the camera? What type of lens is used (distortion-free?)?

P7 L4: Instead of using such an interpolation technique, could you determine the vignetting effect by measuring over a white almost lambertain surface? I could imagine, that a snow covered surface could provide this as a first approximation. For your application this should be sufficient. Or use a certified diffuse reflector.

P7 L5: "Black" means probably "dark" like the dark signal of a non-illuminated camera sensor? In terms of radiation I would prefer "dark". Black is a color and limited to visible wavelength.

P7 L5: How the 2D matrix was determined? Each camera and lens system must have an individual matrix.

P7 L15: Can you discuss the retrieved snow fraction of the example and the uncertainties/quality of the method in this section? Only referring to the figure is not sufficient.

P7 L16, Figure 2: Figure 2a shows the presence of thin gray ice, which is not detected as sea ice in Figure 2b. This means, that from a physical view, the sea ice fraction is underestimated. Although, optically these areas are less bright, they have a higher reflectivity and might bias your results.

Can you give an uncertainty estimate, how the sea ice fraction will change with adjusting the threshold between bright and dark pixel?

As the camera provides RGB images, it should also be possible to classify different ice types following the methods describes by Perovich et al., 2002. Did you thought about this?

Perovich, D. K., Tucker, W. B., and Ligett, K. A.: Aerial observations of the evolution of ice surface conditions during summer, J. Geophys. Res., 107, SHE24-1–SHE24-14, https://doi.org/10.1029/2000JC000449, 2002.

P7 L30: For altitudes above 6.5 km, a standard atmospheric profile is used. Aren't there any radio soundings available? Barrow? What about dropsonde releases from the aircraft?

P8 L3: It would be helpful to include a figure showing a time series or similar plots of the MODIS COPs which are extracted along the flight path. Just to know, what range of COPs have been present and how variable the cloud field was. What about temporal offsets between MODIS and airborne observations?

P8 L4: All clouds are assumed to be liquid. Is there any prove for this? In situ observations? The temperature profiles are well below 0 °C where mixed-phase clouds typically are often present.

P8 L11: What quantities are included in the atmosphere profile? I usually understand also temperature and humidity to be part of the atmospheric profile, but there are provided separately.

P8 L16: What albedo is assumed here?

P8 L17: Specify or provide the slit function in the instrument description.

P8 L19: Does MODIS provide cloud base?

Section 2.5: The description of the radiative transfer simulations should be separated from this MODIS section. The title of the section does not suggest that it will include the methodology of how the solar irradiance is derived. I suggest to add a separate section "methodology". See general comments.

P10 L8: Why the albedo for 11 September needs to be calculated/constructed? I'm lost ... If you have the ice fraction, why there is now surface albedo measurements?

P10 L9: I was wondering, why you use "snow fraction" instead of the more common "sea ice fraction" or "cover". Likely because there is dark snow-free sea ice. Can you elaborate the term "snow fraction" more clearly in section. 2.3. This would help the readers to understand immediately, that there is a difference to sea ice fraction and why this is relevant.

P10 L17: How the cirrus is considered in the analysis? How strong does it influence the final results? Especially with respect to the proposed CRE of clouds?

P10 L23. Figure 7b: There is a large mismatch for areas where no clouds are detected. How strong, the undetected clouds can change the irradiance? I guess, the difference in the "cloud-free" areas is more due to the surface albedo (sea ice fraction) than due to the clouds. $\tau = 0.5$ would not make much difference over bright snow surface.
In optically thick areas, the agreement is better. How strong e.g. 10% uncertainty of the sea ice fraction would influence the results here? Can you rule out any change of the sea ice fraction along the flight track?

P10 L33-37: These general details of the cloud conditions that have been present during the two flights is needed much earlier. I also suggest to add a comparison of the differences between both observed cloud cases. Otherwise, it is hard to follow the analysis.

P10 L40: So far it was not clear, that for the first case, the albedo was fixed. This needs to be made clear at the begin of the analysis. What are the differences between both cases?

P11 L2: I don't understand, why also for this case the parametrization is used. Isn't the parametrization based on the same data? What are the advantages of this approach?

P11 L14: As I understood, the two days are different in a way, that once the aircraft flew above the cloud layer and once below. This means, that the CRE is defined differently for both cases. So how this is accounted here?

Section 3.2. What is the benefit of having both BBR and SSFR broadband irradiance here? Why SSFR was integrated to broadband values and compared to BBR? This makes only sense, if e.g. surface albedo is once considered spectrally and once with a fixed broadband value? Or is there any other purpose?

P11 L18: Where the "model-measurement biases in the broadband shortwave CRE" are discussed? The different values in Table 1 and 2 have not been explained. How the different CRE are calculated? How CRE is derived based on measurements and how it is derived based on the simulations? If there are estimates based only measurement, you would need cloud free flight sections, which I do not see in the data.

How the cirrus layer is considered in the estimation of the CRE and the radiative transfer simulations? You can not neglect the cirrus.

P11 L21: Upward or downward irradiance?

P11 L25: I don't understand, why you need a climatological surface albedo, when you measured and parameterized the albedo.

P11 L28: Was the surface albedo fixed or varied in the simulations or do I misunderstood this sentence? That a fixed albedo will not represent reality is more than obvious. That's why I don't fully understand the approach to use a fixed albedo here. How is this motivated?

P11 L29: Specify. Range of what?

P11 L31: Climate models? How you can draw this conclusion? No climate model is applied, analyzed or discussed in the manuscript. Even when a climate model would have been considered, the "underestimation" may only hold for your specific case, where the albedo is assumed to be to high. This must not hold for the climate models. Further, there are climate models available, which use sophisticated snow albedo parametrization accounting for different sea ice types, melt ponds, snow etc. E.g.:

Dorn, W., Rinke, A., Köberle, C., Dethloff, K., and Gerdes, R.: HIRHAM–NAOSIM 2.0: The upgraded version of the coupled regional atmosphere-ocean-sea ice model for Arctic climate studies, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-278, 2018.

P12 L17: Please write the equation using the symbols of the quantities which are calculated here, not x, y.

P12 L19: The behaviour shown in Fig. 11 can be explained by the change of surface albedo between cloud-free (direct Sun illumination) and cloudy conditions (diffuse illumination). A Similar behavior is reported by Gardner and Sharp (2010). This means, that you albedo is not necessarily wrong. It depends on what you want. A closure study comparing the irradiance in cloudy conditions required the cloudy-sky albedo. For estimating the CRE, the cloud-free albedo needs to be applied in the radiative transfer simulations.

Gardner, A. S., and Sharp, M. J. (2010), A review of snow and ice albedo and the development of a new physically based broadband albedo parameterization, J. Geophys. Res., 115, F01009, doi:10.1029/2009JF001444.

P12 L20: Figure 11 and the parametrization is shown for broadband quantities. Figure 12 is calculated spectrally resolved but used the broadband parametrization? Shouldn't the parametrization be computed for each wavelength?

P12 L20: "Spectrum" of what? Upward downward irradiance or albedo?

P12 L20: What is x? COT? If yes, then write COT. Also a bracket is missing in line 21.

P12 L23: "remarkable agreement". Without uncertainty estimation you can not judge about an agreement.

P12 L33: If the major aim of the study is to show how good irradiances or the CRE can be derived from the MODIS cloud product, the most general scenario should be considered as well. The general case is, that you do not have any airborne observations. Which means, you have to rely on the surface albedo product of MODIS. Atmosphere profiles from reanalysis, etc.. This would be the routine/operational approach. Based on that, you may try to improve the approach by exchanging different assumption with the airborne observations, such as measured surface albedo.

P2 L38: You did not discuss deeply the uncertainties of the MODIS retrieval. The comparison only shows, that there are differences, but could you use the observations to constrain which COPs have been retrieved wrong and to what degree?

P13 L1: The impact of water vapor profiles was not shown in the manuscript.

P13 L3: "developing a validation approach": I don't see a clear validation approach in the study. If there is a strategy, then this needs to be outlined precisely in the begin of the manuscript and summarized here.

P13 L15: It seems, that the study presented here could not answer any of the questions written in this section. I suggest to start the conclusion section with conclusions and not rise more questions than have been pointed out in the introduction.

P13 L35: Define "excellent"! Why you consider the agreement excellent? Any objective measure to judge this? Excellent compared to what? This requires an uncertainty estimation.

Appendix A: Can you briefly explain the concept behind these calculations? Is this done to derive the diffuse fraction based on the present cloud cover? What about broken clouds? Is this covered by the approach?

P15 Eq 1: Avoid the large slash.

P15 Eq 2: This equation was already given above.

P15 L24: How the boundaries of the image are treated if the subdomain is that large?

Table 1 and 2: What unit have the numbers? Is this only solar radiative forcing or total (solar + terrestrial)?

Figure 4: Can you provide the parametrization equation (linear regression?) and the regression coefficients. This might be useful for other research studies.

Figure 8: a) There are data points behind the text. c) Add a legend. And use two labels c) and d) for the left and right panel. In the caption there is a typo in "MOODIS".

Figure 10a: The label hides some of the data.

Figure 10: Indicate the wavelengths used in Fig. 9.

Figure 10b: I suggest to remove the ratio here as the absolute irradiance is almost zero in the range of water vapor absorption. It could be better to show absolute differences instead of a ratio. If the comparison should be linked to the broadband irradiance, the absolute difference will be integrated and result in the difference of the broadband irradiance. Short: 5% difference at 500 nm is more important for broadband than 5% difference at 1600 nm.

Figure 12: Why $\tau = 10000$ and not infinity? Please use τ instead of x. Also here: Do you need to include the water vapor bands wavelengths and have a y-axis down to zero?

Figure 13: Provide equation and coefficients of the fit. Not necessarily in the figure, but in the text.

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Discussion paper

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