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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Reassessment of the common concept to derive the surface cloud radiative forcing in the Arctic: Consideration of surface albedo – cloud interactions" *by* Johannes Stapf et al.

Anonymous Referee #3

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The manuscript "Reassessment of the common concept to derive the surface cloud radiative forcing in the Arctic: Consideration of surface albedo – cloud interactions" by Stapf et al. describes the calculation of cloud radiative forcing (CRF) from measurements collected from aircraft over the MIZ in the eastern Arctic near Svalbard during the ACLOUD campaign in June 2017. The authors leverage the spatial nature of their measurements over the heterogeneous surface albedo that is characteristic of the MIZ to identify and correct biases in calculations of CRF under such conditions. The targeted biases are specifically those associated with cloud-surface albedo interactions

when the estimates of the shortwave clear-sky surrogates used in the CRF calculation are derived using radiative transfer models. The authors focus on a relevant problem that is suitable for ACP and this problem has received less direct attention from previous studies than analogous problems in the infrared. However, I respectfully disagree that this as a "reassessment" of CRF calculations, as the influence of surface albedo on CRF has been acknowledged previously and managed in various ways. The focus on albedo is warranted here because the observational platform and environmental conditions discussed make the present work particularly sensitive in that regard but I think the advancement promised by the title is overstated. I would be more comfortable with the paper being presented in either of the following ways: (a) As calculations of CRF in the MIZ during ACLOUD with the albedo work being an important, though incidental component. In this case more work or more detailed explanations of the longwave calculations are needed (see comments below). (b) If the authors wish to focus on the shortwave, they should drop the longwave data altogether and pitch the study as a proposed methodology for calculations of shortwave CRF in the MIZ where treatment of clear-sky shortwave fluxes requires special attention. In either case, the study needs to be more carefully contextualized and motivated by referencing previous work. More details and comments are provided below. I also suggest a thorough copy editing for grammar, typos, missing words or letters, etc., of which there are many to be found.

Major Comments:

(1) The introduction and study motivations need substantial improvement. Some portions of the introduction actually belong in the Methods section. More troubling is that the study promises to improve upon (indeed, to "reassess") surface-based observations of CRF without actually referencing a single example of previous work on this subject, which has developed for several decades in the Arctic. I suggest rewriting the introduction to more clearly contextualize the present work in the existing literature. I have included some (not exhaustive) useful references throughout this review.

(2) The title indicates a focus on shortwave processes. In my opinion is a fair representation of main the scope of the work, yet there are sections devoted entirely to the longwave and total CRF. There are complications in CRF calculations that are specific to the longwave (Allan et al. 2003) which are analogous to the issues affecting the shortwave; e.g., lapse rate and surface temperature responses to clearing skies (Long and Turner 2008) and systematic differences in water vapor between skies that are clear and those that are cloudy ("water vapor CRF", e.g., Dong et al. 2006). These issues are ignored and consequently the longwave and total CRF parts of the manuscript are somewhat confusing and do not serve a clear purpose. There is quite a lot packed into this study already. I think the study would be much clearer if only shortwave data were included, in keeping with the advertised focus of the work.

(3) Ehrlich et al. (2019b) (P3L30) is in review and the DOI provided is unreachable. Thus, I cannot evaluate the processing of the radiometric data, which is central to this study. Indeed, I don't even know what equipment was used. I wish to know more in particular because radiometric data from airborne platforms requires additional processing, though I am aware that the authors are familiar with some of these complexities (e.g., Ehrlich and Wendisch 2015). In addition to the instrument response corrections of the aforementioned work, how did you correct for tilt in the pyranometer? How did you correct the pyrgeometer data (measured at altitude) to represent the value that would be observed at the surface? (I think the answer is you did not [P7L9]). I have a similar question about the KT-19, which does not observe thermodynamic temperature of the surface, but rather a brightness temperature relative to the FOV and dependent on the path to the target. Is there any reason to similarly correct the shortwave data for altitude given that such details are the focus of the present work?

(4) Unless I misunderstand something, I believe there are errors in the presentation of the CRF equations. This is simple to correct if it is merely a typo in the subscripts, but if the equations were applied as stated the study's results could be impacted. Specifically, be careful how you use the terms "all sky" and "cloudy sky" because they are

not equivalent in CRF nomenclature. CRF may be defined as either (all – clear) or as CF(cloudy – clear) where CF is the cloud fractional occurrence and the other terms refer to net radiative fluxes for "all" (clear and cloudy sky conditions together), "cloudy" (only times when clouds are present) and "clear" (clear skies). Ramanathan et al. discuss both definitions. Admittedly, they are confusing on the nomenclature themselves in using the term "cloudy" early on in discussing their Eqs (1) and (2), but their meaning becomes clear when they introduce Eq. (4). Your Eq. (2) is therefore incorrectly stated; it shows the maximum CRF (e.g., Intrieri et al. 2002). The "cld" subscript needs to be "all" or the entire right side of the equation needs to be multiplied by the cloud fraction. For your purposes, and for the Arctic in general, I suggest the former. This comment applies to equations throughout the text.

(5) P7L9: I do not agree that this is a good assumption. About 70% of the downwelling longwave at the surface originates from atmosphere below the altitude of your aircraft (Ohmura 2001). Your assumption is plausibly (not certainly) valid if the atmosphere is isothermal between surface and the base of the cloud. While this condition could be met, in your case studies (Fig. 2), it is not. Your observations of flux at the altitude of the aircraft should be corrected to represent the surface.

(6) P8L11-P9L7: The approach you suggest to achieve a downwelling clear-sky shortwave is intriguing, but more information is needed for future studies to adopt your method. As written, it is not reproducible and there is no information on the sensitivities of the estimation; for example, I would expect that a filter of constant width assumes that leads are randomly distributed and roughly of the same size. I would also like to know more about the justification for your choice of a Laplace distribution as the most appropriate filter for this application.

Specific Comments

(1) Given that your upwelling shortwave is observed from an aircraft platform, the FOV covers and enormous area. I therefore do not understand why your albedo measure-

ments (e.g., Fig 3a) are not implicitly area-averaged, even if observed from a relatively low altitude (e.g., Podgorny et al. 2018).

(2) P2L18: Consider using "L" for longwave instead of "t" for terrestrial to avoid confusion with the terrestrial surface. "S" would then represent "shortwave" rather than "solar".

(3) P2L18: It is true that RT simulations are a common approach, but there are other approaches as well. Long and Ackerman (2000) present a method for estimating clear-sky fluxes that implicitly accounts for the albedo dependencies on sky conditions. (Refer also to Dong et al. (2010) and Long (2005)). More in line with your study, Miller et al. (2015) parameterized clear-sky albedo for their RT simulations, though your situation is considerably more complex with regard to surface cover. Other studies have analyzed the dependencies. These studies do not necessarily detract from your work here and in some ways maybe motivate it, but either way really need to be referenced.

(4) P4L15 - P5L4: (1) I don't understand how (or when) you combined the dropsondes and NYA radiosoundings. (2) You do not say how you represent the atmosphere above the height of the soundings; this is necessary (even if estimated using a standard atmosphere) to a reasonable effective TOA (say, 60 km). (3) You do not say how you represent atmospheric gases that are radiatively active in the infrared, but were not measured by the sounding (not notably, CO2, but also O3, methane, etc.).

(5) P5L14-16: I do not agree that the upward longwave between clear and cloudy conditions is equal, but I doubt that this is what you actually mean to say. I think you mean you defined them to be so because (a) you do not account for the response of the atmospheric lapse rate (and surface skin temperature) to changes in sky cover and your calculations for the longwave are therefore "instantaneous" CRF (e.g., Miller et al. 2015), and (b) that you also neglect the influence of differences in the amount of longwave reflected from the surface between clear and cloudy skies. It is acceptable to make the first assumption (see Allan et al. 2003), but you should include the emissivity

term and then proceed with your CRF calculation. See my next comment.

(6) P4 Eq. (4): This variable is more frequently referred to as (longwave) "cloud radiative effect" (CRE) (e.g, McFarlane et al. 2013; Viudez-Mora 2015; Cox et al. 2015, 2016) and should be distinguished somehow from CRF. At the surface CRF and CRE are different, the former being the difference in the net fluxes and the latter being the difference in the incident fluxes. Confusion sometimes arises because the terms are frequently used interchangeably in satellite studies, being that the terms are equal against the backdrop of space.

(7) P5L28: Multiple scattering also depends on the albedo of the sky. How do you account for this?

(8) P6L30: Is 60deg SZA representative of the flight conditions?

(9) P9L20: You might also consider that your pyranometer is at best a 2% instrument and thus you might expect uncertainty of around 10 Wm-2 in the measurement. Thus, Figure 4 looks quite good. I am however curious about the source of the bimodality of the solar CRF in Figure 4. My first thought is that one of these peaks is associated with ice-covered areas and the other with open water, pointing to some residual bias in the method.

(10) Section 3.4: It would substantially increase the value of this section if you contextualized your simulated biases with your observations. For example, it would be interesting to see the biases from Figure 3c plotted over Figure 7 in the phase space of the figure panel that is most appropriate.

(11) P18L9: You have mentioned SHEBA a couple times, but have not referenced it (Uttal et al., 2002), nor have you defined the acronym.

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