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Title: Observations and simulations of three-dimensional radiative interactions between Arctic boundary layer clouds and ice floes
Author(s): Schäfer et al.
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The purpose of this manuscript is to describe, and quantify, the transition of radiation from above a "bright" surface to adjacent "dark" surface and the impact clouds (height, geometrical thickness and optical properties) have on this transition. This combination of surface and cloud conditions occur in the Arctic, and the research effort is predominantly a modeling effort, due to the complexity of passive remote sensing in the Arctic, yet the authors support some of their results with measurements obtained from a suite of instruments during an Arctic field campaign in Spring, 2012. The results are important as our current knowledge of the time- and spatially-dependent changes in surface and cloud properties are derived from satellite observations, for which an assumption that the horizontal net flow of radiation is zero is made for practical, operational purposes. The Arctic is more sensitive to other regions of the Earth and its time- and spatially-dependent climate changes are larger than other regions. The authors' analysis suggests the boundaries for which this assumption can be made.

The manuscript presents an in-depth discussion of the various simulations approached. In general, figures are clear (exceptions noted below). I identify several important areas for improvement below that are needed, in my opinion, to strengthen the paper. In light of these areas of improvement, and since two of the areas will require a re-organization of the paper and a deeper literature review (potentially, the authors will also want to present their results at a different spectral band as a result of this literature review), I recommend accepting the paper with major revision.

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?)

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). 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 1.6 micron channel in the case of MODIS (Platnick et al., 2001; 2003; Krijger et al.,

2011), as the authors have done. 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. This comment leads to the following one.
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).

2. *Meeting the challenge of interpreting the theoretical results to those that are important in practice*.
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.
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). 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). 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).
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?
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 ΔL_{crit}, and their usage throughout the article. 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 is onerous, I think it is necessary.

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).

Articles referenced in the major comments:
Platnick et al., 2001 – A solar reflectance method for retrieving the optical thickness and droplet size of liquid water clouds over snow and ice surfaces, JGR, 106, pp. 15,185-15,199.
Platnick et al., 2003 – The MODIS cloud products: Algorithms and examples from Terra, IEEE TGRS., 41, pp. 459-473.
Platnick, S., M. D. King, B. Wind, M. Gray, and P. Hubanks, 2004, An initial analysis of pixel-level uncertainty in global MODIS cloud optical thickness and effective particle size retrievals, *Proc. of SPIE*, Vol. 5652, doi: 10.1117/12.578353.

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.
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.
3. Lindsay and Rothrock (1994) analysed albedo in 200 km^2 cells (not 20 km^2) – page 1423.
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).
5. obverse – observe (page 1426)

6. status – stratus (page 1427)
7. Remove an extra "each" (page 1430).
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).
9. relative – relatively (page 1434).
10. Suggested wording change " As a reference ~~also~~ a clear-sky scenario was *also* simulated…" (page 1434).
11. Missing word " This results from the reduction *in* contrast between the dark.." (page 1434).
12. Misplaced text? From "On the other hand, the decrease of …" through end of paragraph would be better incorporated two paragraphs preceeding. (page 1435).
13. One too many clouds? "For an increasing cloud altitude of a cloud…" (page 1436).
14. proofs – proves (page 1437).
15. Word change "To quantify the influence…we *quantified* ΔL." (page 1437).
16. Awkward sentence "For all values of *simulated* optical thickness…". Use instead, perhaps, "For simulations at all optical thicknesses, …"(page 1437).
17. Define SDs (page 1441).
18. kind of – approximately (page 1445).
19. Incorrect statement "The different patterns of overestimation …suggest that the 3-D effects can be larger at absorbing wavelengths" (page 1445).
20. weather –whether (page 1447).

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.
2. For all figures with units, please place units in open parentheses (), instead of after a slash.
3. Figure 5 – this is an incredible result!
4. Figures 8a-b – It is difficult to interpret various curves, on left hand side of each plot.
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.
6. Figure 14 – Perhaps revisit this figure if you decide on an alternative wavelength combination for your results.