

Interactive comment on “Observations and simulations of three-dimensional radiative interactions between Arctic boundary layer clouds and ice floes” by M. Schäfer et al.

Anonymous Referee #3

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Overall recommendation: Major revisions

Overall assessment:

This paper studies surface-cloud radiative interactions in the marginal ice zone in the Arctic, which is an interesting and relevant topic. Specifically, the study of the enhanced reflectance zone surrounding ice floes in presence of overlying clouds is useful, and I recommend to make this the central focus of the paper. Rather than focusing on the radiative cloud effects above ice (as the title suggests), the paper then seems to drift into a remote sensing direction. In this context, the repeated statement that cloud

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retrievals are impossible above snow and ice is quite irritating, given the literature in this area, which is completely ignored here. The sequential comments below provide specifics. Put bluntly, the author doesn't seem to be aware that retrievals above snow and ice are, in fact, possible based on information in the near-infrared where snow is dark, and that they are obtained operationally from MODIS (but NOT through the cited Nakajima-King method). It also appears that none of the co-authors have been given to review this paper before submission before they would surely have caught such a blatant error. Also, the manuscript is wordy, and poorly written - for example "...increases kind of exponentially..." proves again that the manuscript was probably submitted unreviewed by any of the co-authors. In light of the sequential comments below, the authors should either delete the part of the manuscript that is related to cloud retrievals, or thoroughly revise it, based on an adequate literature study. In its own right, the analysis of Delta L is very interesting, and seems sufficient material to be published by ACP.

Sequential comments:

P1423,L4: The quoted reference, Bennartz et al. (2013) is a poor choice for substantiating the statement that clouds play a major role in projections of the future Arctic climate because it is observations-based and do not include climate model runs in any way.

L11: "In this regard, surface albedo..." since this follows after statement about the dominating influence of IR, one should perhaps clarify that this is for the solar wavelength range again - how about "For the solar wavelength range, surface albedo..."

L14: "...Arctic stratus is nearly homogeneous..." This is an unsubstantiated claim if no reference is provided. Also, "from a microphysical point of view" is ambiguous. Does this mean in terms of droplet radius, thermodynamic phase, LWC/IWC? Wouldn't stratus be homogeneous in the macroscopic rather than microphysical sense?

P1424,L4: Krijer et al. (2011) cannot be used to support the statement that "retrievals

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of Arctic cloud properties over bright surfaces [is] impossible". In fact, the opposite is true: Krijer et al. do state that with visible channels alone, this is not possible, but in their paper, they specifically mention that they overcome this limitation by introducing near-infrared channel(s) from SCIAMACHY.

P1425, L16-L17: "Variations. . . will characterize. . ." unclear wording. Is the intention to say that changes in cloud altitude etc. will affect the transition? Or is the intention to describe what will be done in the paper?

P1426: This is an insufficient description of the instruments; while references can be used to "outsource" specific information, each paper needs to stand on its own, and at least the information that are crucial for understanding this paper need to be provided - for example the accuracy etc. of the instrumentation. For example, a google search reveals that AisaEAGLE covers wavelengths up to 1000 nm only - but later on in the paper, near-infrared wavelengths are used for applying the retrieval method by Werner et al. (2013).

Figure 1: What do the labels (1) and (2) mean (probably open ocean vs. ice, but it needs to be stated).

Figure 2: The authors may consider showing reflectance rather than radiance because the effects of SZA would then be removed, and the effect of optical thickness/surface albedo be isolated.

P1427, L23: Unclear what this statement means. In fact, MODIS uses 860 nm, not 650 nm, for cloud retrievals over open ocean; 650 nm is used over land (when snow-free). MODIS retrievals over land(sea) ice and snow are a different story (see comment below).

P1427, L18-19; P1428, L1-2: Statements of this kind are all over this manuscript and need to be carefully removed everywhere because the retrieval of cloud microphysics is, in fact, possible, and is done operationally by MODIS. Rather than Nakajima-King,

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it is based on near-infrared bands (1.6 and 2.1 microns) where snow is dark. This is discussed by King et al. (2004), among a few other papers. Why this is not mentioned anywhere in the manuscript is unclear. It appears that the authors are unaware of it, which seems impossible given the publication and research record of this group. Have they checked whether MODIS retrievals are, in fact, available, in addition to just looking at RGB images (Figure 1)? Attached as a supplement is a description of MODIS products, which demonstrates that MODIS does have skill to provide retrievals over snow and ice. Many more documents are publicly available. The question is what direction the manuscript would have taken, had the authors known of the existence of the MODIS algorithm. Since section 5 only refers to retrievals over the dark ocean in the vicinity of ice floes, but not over snow/ice itself, this paper is actually pertinent to the "classical" MODIS retrieval above dark surfaces. Back to the statement "A retrieval of cloud . . . properties . . . is not possible." and others of this kind: Please remove because they are incorrect.

P1428, L24: "We estimate the cloud optical thickness. . ." What does this mean? Did the authors look at the level-2 MODIS products and get the number from there? Or did they visually estimate 5 from the RGB image? Why estimate if a retrieval is, in fact, available? Also, why did the authors not do their own retrieval of tau and reff, based on data from the instrumentation?

P1430, L20: One cannot see the effect of enhanced reflected radiance close to the sea ice from Figure 4b and d (mask results); better use Figure 6, which shows quantitative radiances.

Figure 6, Question 1: The question about Figure 6 that needs to be discussed is the significance of the local maximum of radiance at -50m.

Figure 6, Question 2: The different cases shown are probably observed at different solar zenith angles. Could this be shown as reflectance instead to normalize with respect to μ ?

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P1433,L12: No, The MODIS retrieval over water does not use this wavelength (see comment above).

P1435,L7-8: Unclear statement

P1435,L18: The purpose of Delta L and Delta L critical is misleading - why was Delta L critical (in addition to Delta L) introduced? Judging from Figures 9 and 10, it appears that the horizontal extent of the "vicinity zone" around ice floes decreases with cloud optical thickness, increases with with cloud base/top height, (and geometrical thickness? - please add a table or Figure that shows this), as well as with the radius of the ice floe. It does not appear that Delta L critical is necessary because Delta L alone gives a clear picture.

Section 4.2.3: This section is too long, and there are many problems with repetition, language/grammar. Rather than listing the issues in detail, the authors are encouraged to shorten this section AND have this proof-read by the co-authors (something that should always be done). I would like to point out that the finding on P1442,L22-24 seems important, but would "shine more" if presented in a considerably shorter section 4.2.3.

P1443,L21-23: Unclear what justifies the statement that cloud and surface heterogeneity effects "are in the same range". If that is true, please make this a quantitative statement and provide the respective ranges.

Section 5: I recommend removal of this section. In general, sections 5, 4.2.3, and 4.2.4 are of much lower quality than the rest of the paper. But regarding content, the applied retrieval technique actually does not replicate what MODIS is doing (if the goal is to improve/validate satellite retrievals). If this were the purpose of the study, the correct pairing of bands (860 nm + 2150 nm), should be used. It is unclear why Werner et al. (2013) is used here instead - why is Nakajima-King ambiguous (with respect to which retrieval parameters)? In addition, if the overestimation zone for cloud optical thickness is only 2km, this would hardly be seen by MODIS anyway because its grid size is 1km.

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If anything, this will affect ONE pixel in the vicinity of an ice floe. This would be relevant for sub-grid-resolution ice floes though, which MAY bias the MODIS cloud retrievals high if they go undetected. But this does not seem to be the intent of the current study.

P1445,L22-23: "...different patterns of ... 3D effects can be larger at absorbing wavelengths". This statement needs to be substantiated. How do the different patterns suggest this?

P1446,L11-13: See comments above. Retrievals are possible over snow surfaces. Please review the literature.

Conclusions: This section should be significantly shortened; after all, the purpose of this section should be to summarize the most important results.

P1449,L14-L26: Remove (see comment above), or frame this differently, after having reviewed the cloud retrieval literature.

Language/Spelling comments:

I am sure I did not capture all the language issues (especially in punctuation), the ones given below are representative. The manuscript needs to be revised by a native speaker and undergo ACP copy-editing.

P1422,L2: add comma after "observations"

L7: "instantaneously" -> "instantaneous"

L10: "with help" -> "with the help"

L13: "ground overlaying" sounds a bit awkward, can it be replaced with some other term or at least be hyphenated?

L13: "in 0-200m altitude" -> "at 0-200m altitude" (multiple occurrences throughout manuscript)

L13: "on both, cloud and sea" - remove comma

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L21: "infinite" -> "infinitely"
P1424,L19: "superposed" – use a more suitable word such as "combined"
P1425,L12: add comma after "(2013)"
L13: add comma after "simulations"
L23 and L27: add comma after "(VERDI)"
P1426,L3: "aimed at" -> "was aimed at"
P1426,L22: "obverse" -> "observe"
P1434,L11: "effects affect" - improve language
L16: "will be simulated" – please fix the usage of tense throughout the paper. The simulations have already been performed, so future tense is inappropriate.
L21: "have been" -> "were"?
P1435,L1: add comma after "observations"
L2: add comma after "ice"
L9: add comma after "general"
L16: add comma after "7"
L20: "That" -> "This"
P1437,L5: "That proofs" -> "This proves"
P1439,P1443: R_(3-D-IPA) is misleading, this should be relabeled R_(3D)/R_(IPA) (Currently, this looks like a difference, but it's a ratio).
P1444,L8: "roll" -> "role"
P1445,L17: Replace "kind of" - this is slang

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P1446,L19: "This causes horizontal photon transport, which ... is scattered" It is not the transport that is scattered but the radiation (fix structure).

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/15/C667/2015/acpd-15-C667-2015-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 1421, 2015.

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