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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.

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Summary In this study the authors argue for corrections in the surface albedo they suggest are necessary for the calculation of the Cloud Radiative Effect in the Arctic, where the surface albedo is very variable on all time and space scales. It is an interesting study but it needs some rethinking before it is pub-lished. Therefore, I recommend that this manuscript is accepted after major revision.

General comments In summary, I think this is interesting work, but it needs a more solid foundation and a discisson about the why's and the how's, and less details on ACLOUD; maybe do a separate but more exten-sive paper on CREs during ACLOUD,

referencing this paper.

Maybe I'm nitpicking, but the terminology has been changed by the climate community, from Cloud Radiative Forcing, or CRF, to Cloud Radiative Effect, or CRE, quite a while ago. The title is also in my personal opinion too long and clunky; try something shorter. Maybe "Interactions be-tween surface albedo and clouds for Arctic cloud radiative effect estimation".

The content in the study balances along many borders and as a consequence it doesn't quite fulfill any of the topics it crosses well enough. It is unclear if this is a theoretical study that uses ACLOUD data just because its good and convenient or if it is a contribution to ACLOUD as such. I already hear the authors say "can't it be both?" and my response is it would be a better radiation interaction paper if ACLOUD was tuned down and a better ACLOUD paper if the radiation stuff was more background and the actual results where more detailed. Typically to be a good paper on both aspects it would have to be longer – which is not good. There is also a lot taken for granted on the readers; not everyone is a radiative transfer modeling expert. So choices must be made.

Besides the use of an old terminology (CRF instead of CRE) there needs to be a much more in-depth and philosophical background. Why are we interested in CRE (or CRF) and how does that impact how we do these calculations? The question "How does the clouds affect the surface energy budget" is not the same as the question "How would the surface energy budget look if the clouds were not present?". As an example, the authors argue both that changes in surface broadband albedo between cloudy and clear states must be considered, and that the details in thermodynamic are important. However, we know that the thermodynamic profiles for clear and cloudy cases are very dif-ferent. The presence of clouds depends on the vertical profiles, but the clouds themselves also modi-fy the profiles by their presence. Yet the authors argue that we change the surface albedo to how would be without the clouds, but keep the thermodynamic profiles as they are; only remove the cloud water. I think that the an-

swers depend on what we want to use this metric for. Ideally the an-swer to my second question above would have us examine the conditions in clear and cloudy condi-tions separately, not modifying cloudy cases by removing the condensed water. However, the Arctic is a very cloudy place and there are not enough clear cases to make this possible. Therefore, I think this paper needs a much more detailed introduction and background to what we are trying to do and why.

There are three corrections that the authors argue are necessary: 1) Effects of proper thermody-namic profiles; 2) Effects of heterogeneous surfaces, and; 3) Effects of the clouds on the characteris-tics of the solar radiation.

1) This is pretty obvious; of course one must use the profile from the same location as the CRE is considered. In this paper this is discussed in the context of aircraft observations covering an area, but the conclusion is also important for fixed-point observations. One question is, if a proper sounding at the location is not available, from how far away can it be used? This question of course has no an-swer other than "it depends." But here one could also raise the issue of cloudy profiles being differ-ent from the corresponding clear case; in other words, if we could magically remove the cloud water so that the clouds vanish, what would that do to the thermodynamic profiles? Or, are the profiles found in clear conditions systematically different from those in cloudy conditions? From both mod-eling and observations in subtropical stratocumulus regions we know that the moist PBL is deeper and warmer when clouds are present; for the Arctic we don't really know, but I would wager a bet that they are different!

2) That heterogeneous surfaces poses a problem for upwelling shortwave radiation is also pretty obvious. This is a main factor in the MIZ but also in the pack ice mainly due to melt ponds. This pa-per doesn't even mention the effect of melt ponds, presumably because there weren't any during ACLOUD. So is this an ACLOUD paper usuing advanced radiation methods or a radiation paper using ACLOUD observations?

Moreover, to this reviewer it is not obvious that the downwelling radiation is dependent

on sur-face albedo; in most NWP or climate models that I know this is not considered, but may be ignorant. Either way, for a reader like me, this needs to discussed in more detail. Commenting about "horizon-tal photon transport" is not sufficient. If this is a factor, how large is it? What is it caused by? Are there differences between say the MIZ, with alternating ice and open water, pack ice with melt ponds, pack ice with open leads, or pack ice with many substantial pressure ridges? Or all of the above?

3) Changes in the spectral composition from absorption in clouds is a real tangible effect that one can discuss if it is necessary to compensate for or not; see my discussion above. Changes due to the different distribution between direct and diffuse radiation is trickier. Also the cloud albedo is sensi-tive to solar zenith angle.

Finally, the language is mostly OK, but occasionally I stumble on unnecessarily difficult wording, for example "exemplarily" in the context it is used is an existing word but even the dictionary indi-cates it isn't much used in modern English. There are also past/present inconsistencies; what is done and presented in this paper is sometimes described in past tense and sometimes in present. Either is fine with me; just be consistent. Finally, final: among the data made available here, only a subset is actually really made available; the rest is just referenced.

Some detailed comments Title is unnecessarily clunky – also here and throughout the paper, Cloud Radiative Forcing (CRF) should be replaced by Cloud Radiative Effect (CRE); even maybe surface CRE.

Page 1, line 23: Comma after amplification.

Page 2, line 1-2: If this is the prime question, one can not use the cloud profile minus the cloud as representative for clear conditions; one must do the clear and the cloudy cases completely separately.

Page 2, line 3: It is not at all clear that clouds are cooling the surface in the Arctic in summer, so I would drop "dominates". It depends on a lot of factors, some of which

this paper deal with. A clear case when this statement is correct, perhaps the only one observed case, is for SHEBA; the only annual observations that exist and BTW where the CRE was calculated without any of the corrections discussed here. Suffice it to say that I've seen summer conditions with a lot of snow and almost no melt ponds at very high latitudes where surface temperatures plummets when the clouds dissipate.

Page 2, line 11: The cases are "clear" or "all-sky", so I would swap places between "all-sky" and clear here. You can still define that as "cloudy" from her on, but it should be stated that the normal case is the existing clouds; not just when it is completely overcast.

Page 2, line 20: Here it is stipulated that surface albedo affects the incoming (downwelling) solar radiation for clear skies. To me that is not obvious, and even if it is obvious from multiple refelctions in clouds it is not obvious that it is important for clear-sky radiation. Do spend some more time on this please.

Page 2, line 14-15: "... observations of ... conditions and of atmospheric thermodynamic state."

Page 2, line 26: Is shape the right word here? Isn't it the magnitudes; not just the shape?

Page 2, line 28: Comma after "albedo".

Page 3, line 17-18: Here's an example of tense mismatch: "... were investigated in this paper" and "... aircraft is displayed ...". Later on same page the dataset "is" merged and on line 3-4 on the next page "... concentration was calculated ...".

Paragraph staring on Page 6, line 29:How do you handle the observed surface albedo when calcu-lating CRE (or surface ïĄĎF)? The text says that the clear-sky albedo is set to 80% and the zenith an-gle to 80°; presumably those were not the observed conditions?

Section 3.3: Spend some time explaining why albedo affects downward solar radiation.

Also, ex-plain the choice of albedo filter; is there any theoretical consideration here or was it "trial and error"? Paragraph starting at Page 9, line 14: It seems to me this is a test comparing calculated results to observed results from a clear day; correct? If so, maybe this should not be reported under this head-ing? And maybe the term CRF (or rather CRE) should not be used when there are no clouds and the CRE is expected to be zero?

Page 10, line 6: Explain SSA.

Page 10, line 9: I don't think you could find a case with 1 cm snow thickness in reality. That would be > 1cm at some location and no snow at other.

Paragraph starting at Page 10, last line: This should come before the calculation specific. First ex-plain why and then how.

Page 11, line 3-4: Why is diffuse radiation coming in at a zenith angle of $\sim$50°? When the cloud is thick enough that where the sun is in the sky can no longer be determined, is there a zenith angle at all? I thought, but may be wrong, that diffuse mean precisely that the radiation was equally strong in all directions.

Page 11, line 18: Drop "the".

Page 12, line 17: "... with increasing LWP is not, or only poorly, parameterized..."

Page 12, line 18: Unclear past tense in "have been used". Previously, or did you do this work now. In the previous case, give reference; in the latter, present tense should be used.

Figure 8: The SHEBA albedo line includes melt ponds and eventually even a lead. The drop in albedo starting at the beginning of June is due to this; as there were no melt ponds I ACLOUD(?), your comparing apples and pears here. For the SZA calculations, did you take into account that SHEBA moved northward during the year? Another idea would be to redo the SHEBA Intrieri et al. CRE study with this new information. Maybe a bit more work than anticipated for now, but it would be interesting.

Page 13, line 2 & 3: Again, two examples of past-tense confusion. When was this done; for this paper or by an earlier investigator.

Page 13, line 10: Using both “indicate” and “might” in the same sentence almost obliterates the conclusion.

Page 14, line 4: Can’t find any red line in Figure 8.

Page 17, line 5: “indispensable” is a strong word. Since it is impossible to know the cloud-free state with any accuracy, I would mellow the language here. If something indispensable is also im-possible, then why even try?

Page 17, line 11: If by “local” you mean in one single specified point, then I’m confused. The lo-cal albedo is what it is; it is different at a different locale when the sea ice is variable; I still get hung up on this concept. If you are referring to the effects on the cloud free downwelling radiation, that I wanted to have elaborated on, the at least write “local cloud-free albedo”.

Page 18: lines 27-31: Only part of the data is available, a large chunk is only cited. Why? Appendix A: OK; but, why don’t show that this works, using aircraft passages over Polarstern, where you have both transmissivity and LWP?

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