

Jakel et al.: Validation of the sea ice surface albedo scheme of the regional climate model HIRHAM-NAOSIM using aircraft measurements during the ACLOUD/PASCAL campaigns

The authors use data from the ACLOUD/PASCAL field campaigns in May/June to force a surface albedo parameterization from HIRHAM/NAOSIM and compare to observations. The major findings are that early in the season the parameterized albedos are too low but later they are too high; the parameterized distribution of albedo is too narrow; and the cloud cover affects the observed albedos. I have a few major concerns related to the focus on cloud cover impacts, snow heterogeneity, and surface type.

Major concerns:

- **Cloud focus:** There needs to be more discussion of why cloud cover should impact albedo. Is it due to more scattered light? Additionally, there should be more discussion of how the modifications to albedo for cloud cover would work in a coupled model. Typically, just surface fluxes (SW, LW) are passed to an ice model from the atmosphere model. Could SW or LW be used instead of cloud cover because modeled cloud fraction is notoriously poor.
- **Snow on the surface:** Nearly all the observations compared are over snow covered ice, but there is little discussion of snow heterogeneity and how this might impact the results. At Pg.8/Ln.2 you mention “snow type” and also later and at Pg.12/Ln.6 you mention “more structured snow” and “increased roughness”, all of which allude to the heterogeneity in the snow cover. In fact, Fig.4 (and Pg.8/Ln.15-17) shows that there doesn't seem to be good correlation between changes in temperature, which is relatively constant between early and late June, and albedo, which drops off during this period. Since during this time the ice remains snow covered, this suggests to me that perhaps changes in the snow rather than temperature should be the impetus for the albedo change. Finally, on Pg.14/Ln.2 you mention grain size or snow thickness as being important for temporal evolution on albedo. Why don't you focus on better understanding these snow effects on albedo rather than clouds? Could these snow differences be important for the larger variability in observations than the parameterization? It's known that the snow type is important, it seems worth more mention in this manuscript.
- **Other surface types:** I am concerned that all the comparisons are done over ice with nearly 100% snow cover. You are clear that the results of the work are valid for covered ice, but later in the season does that mean these results are unimportant? Do clouds have any impact when there are more melt ponds, and is it worth the effort to include cloud cover?

Minor concerns. There are just a number of small clarifications or suggestions for figures.

- Fig.1a – Add a colorbar.
- Fig.4 – is there a better way to show this? I can hardly see the whiskers or differentiate between polar 5 and 6 flights. The other thing to point out is that in

panel b there is a large range of observed albedos on each day. This is worth pointing out. Even in early May there are albedos of 0.7 within one standard deviation.

- Fig.6 – can you clarify on the figure which part is a and b of the components for assessment?
- Fig.7 – the dashed lines are very hard to see.
- Fig. 9 – Do red or black correspond to measurements? The caption and legend conflict.
- Table 2 – it looks like for a number of days the parameterized and observed albedos are similar. Is it worth mentioning this?
- Throughout – Does SIS just mean “sea ice surface” or it is the name of the model albedo parameterization.
- Equation 6 – Why is the maximum fraction for melt ponds (22%) so low? Is there justification?
- Pg. 5 Line 17-18: It looks like for $h_s > 0.1$ then the fraction is solely snow-covered ice while for smaller snow depth melt ponds or bare ice become more relevant. I didn't follow the text here.
- Pg.6 Line 20: The winds cause the southwesterly ice drift but your wording is confusing: “due to northerly winds coupled with a southerly to southwesterly sea ice drift.”
- Pg.6 Line 30-31: What are the increase of 9% and 32% compared against.
- Pg.9 Line 14: No bars in Fig.5 have 70% open water.
- Pg.10 Line 15: do you mean “coming from directly below the aircraft for (i) only.” ... “Therefore approach (ii) might lead...”.
- Pg.11 Line 19: Why did you use 50 as a threshold?
- Pg.12 Line 14: what is snow grain size differences less important relative to?
- Pg.16 Line 1: the values of albedo given for min and max don't match those in Table 4. I'm confused. Also the precision for RMSE values is probably too great.