Interactive comment on “Regional albedo of Arctic first-year drift ice in advanced stages of melt from the combination of in situ measurements and aerial imagery” by D. V. Divine et al.

Anonymous Referee #2

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Review

Regional albedo of Arctic first-year drift ice in advanced stages of melt from the combination of in situ measurements and aerial imagery

D.V. Divine et al.: The Cryosphere Discuss., 8, 3699-3732, 2014

General Comments:

The authors present a case study of surface feature fractions and albedo estimates derived from low-altitude aerial images of melt pond covered first-year sea ice floes north of Svalbard. The paper is within the scope of The Cryosphere as it addresses a timely topic important to several disciplines: the seasonal and spatial variability of sea ice melt pond fraction and summer ice albedo. Observations such as these are crucial to understanding melt pond/albedo evolution and governing processes, as well as for improving model parameterizations.

In general the methods section of the paper is well structured and written in a fluent and concise manner. The methods are scientifically sound, with experiments, calculations, and uncertainty estimates provided as needed. The authors’ approach to addressing the issue of upscaling is particularly noteworthy. However despite the considerable effort in processing, analysing, and reporting the data and methods, the overall organization of the paper and its impact are insufficient for full publication without a major revision. The following issues need to be addressed.

1. The introduction states that the paper shows an analysis of regional morphological properties of the ice surface, as inferred from aerial images, followed by estimates of regional albedo. Are these the primary objective of the paper? If yes, the use of flight tracks made over such a short time period (31-Jul to 03-Aug), and the decision to disregard data from the flight over the MIZ (but part of the same region), mean that these objectives are not met. Similarly, the authors’ conclude that the relatively short time scale precludes comparison to other studies which suggests that an adequate regional estimate is not been obtained. Instead emphasis should be placed on ice type rather than region – in this case pack ice which, as it appears, is observed late in the advanced melt stage. Or, is the primary objective to present a new tool for extracting pond fractions and making albedo estimates? If yes to this, I defer to the comment made by reviewer #1 regarding the algorithm of Renner et al. 2013. What is the advantage of this approach and how does it compare to other techniques such as Renner et al. 2013? In either case, the objectives of the paper need to be more clearly formulated, the structure of the paper re-organized accordingly, and relevant conclusions made.

2. In some instances the citations are lacking, which makes it difficult to ascertain how...
this contribution fits within the context of the literature in general. While references
to classic papers dealing with sea ice albedo and heat balance are good, there is a
notable gap in literature dealing with melt pond fraction and melt pond albedo obser-
vations. Instead the authors rely heavily on (i) Polashenski et al. 2012 and (ii) Perovich
and Polashenski 2012. Though (i) is relevant, this paper appears to be referring to the
review section of (i) instead of the original contributions which are referenced therein.
Paper (ii) is cited several times, though it deals with very smooth, shorefast ice (fast
ice); a clear distinction needs to be made with the pack ice investigated here, or ev-
idence of consistency between types provided (i.e. was the drift ice level?). In the
conclusions a new set of references are introduced by the authors in an attempt to
compare their findings here to albedo estimates from similar studies. The comparisons
are weak, which is acknowledged by the authors, and points to the need for a better
synthesis of these results relative to the literature. Again, more emphasis could be
placed on ice type and/or topography and season, rather than region and/or latitude.
Are there surface observations from ICE12 that could help in this regard?

3. The use of the term “advanced stages of melt”, in the title and text of the paper is
misleading. Its usage suggests in plural form suggests there are sub-stages within
the advanced melt stage of the sea ice evolutionary cycle which are being examined
(e.g. thermodynamic/ablation states of Hanesiak et al., 2001 stages related to surface
hydrology by Eicken et al., 2002).

4. Several sub-sections in Section 3 (Results and Discussion) focus on methods. See
comment #1 above: if the primary objective of the paper is to analyze morphological
properties then methods such as in 3.2.1 and 3.2.2 should be in Section 2 to set the
stage for presentation and discussion of results in Section 3. I realize it may be the
authors’ intention is to present the technique, in which case it is more a matter of refor-
mulating the objectives in the introduction and maintaining these methods in Section
3.

Specific Comments:

C1873
3-Results and Discussion

The section headings for 3.1, 3.2, 3.21, and 3.2.3 should be shortened.

P3708, L21: Methods regarding EM-bird calibration are not needed.

P3708/L25: Why have you chosen flight 2 out of the 5 pack ice flights?

P3709/L2: “...the results are similar...”. Based on the authors’ experience or using any supportive data, can it be said that conditions were similar as well?


P3710/L3-5: “This suggests... negatively biased.” I don’t see evidence of this from the boxplots in Figures 5 & 6 which show (mean values) \( a_i > a_s \) and \( a_i \approx a_s \), respectively.

P3710/L21-P3711/L9: Methods out of place in results and discussion section.

P3711/L21-23: “This suggests... regional-scale estimate of the surface albedo.” This statement is not well justified, i.e. how does the between-flight similarity in swath-based aggregate albedo values improve their use in providing a regional estimate? Also you have purposely left out flight 6 due to a different ice cover state, but is that not part of the region? Again I would suggest the focus is placed on ice type/condition rather than region.

Sections 3.2.2 and 3.2.3 are well written, though better explanation/justification for 3.2.2 is needed for readers not familiar with issues of autocorrelation in spatial analysis.

4-Conclusions

P3715/L1: delete “small scale features such as” and “entirely”

P3715/L11-17: Are you implying that the observations in this study are unique? It would appear so based on Section 2.1. So why not mention this earlier and be more explicit?

Technical Corrections:

P3700/L8: ‘adequate representations’

P3701/L20-21: ‘geographical setting’; delete ‘used in the study’

P3705/L11: ‘sea-ice’ is used here, ‘sea ice’ elsewhere; be consistent

P3706/L9: delete ‘or leads’ since it is implied open water

P3710/L9: “available” not “avaialbe”

P3717/L25: “a detailed analysis”

Table 1: shorten description

Figure 1: figure is too small, especially the text.

Figures 3-4: percentages are used here for melt pond fraction but not in Figures 5-6. Maps of flight tracks are redundant. What is the purpose of ‘c’ in these figures if along-track data is not discussed in the results?

Literature:


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