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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

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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 and 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

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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 observations. 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 evidence 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 reformulating the objectives in the introduction and maintaining these methods in Section 3.

Specific Comments:

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(Page and line numbers refer to the printed format)

Abstract:

P3699/L3: Correct 'advanced stages of melt' to singular.

P3699/L1-21: Is the main result you are highlighting the regional albedo estimate of 0.40?

1-Introduction:

P3699/L24: 'ocean-sea ice-atmosphere' used here, 'atmosphere-sea ice-ocean' used later. Choose one and abbreviate it if necessary.

P3700/L14: "..up to 70% of the surface". Is would be more appropriate to refer to level (shorefast) FYI here if you also include value(s) for drift ice.

P3701/L19-27: The seasonal evolution of sea ice albedo and its relationship to ice topography and heat balance have been well studied, e.g. during the SHEBA study, prior the references dated 2012 and 2013 here.

2-Data and Methods

P3702/L12: How was aerial photography used to assess the representativeness of ice thickness?

P3702/L13-14: The latter stage of melt is interesting. It looks like you may be observing the point at which there is an increase in pond fraction due to flooding by seawater. It would be nice to have this expressed within the context of the expected temporal evolution of pond fraction for this ice type.

P3703/L19-21: Describe the technique or appropriate reference instead of the software used.

P3704/L7: Again, appropriate to describe method but no need for software used.

P3706/L17: "...underestimates melt pond coverage...". Here and elsewhere in the

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paper – adopt either the symbology or full text and stick with it throughout the paper.

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?

P3709/L16-19: Sentence "The results of in situ measurements ...". Re-write for clarity.

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) ai > as and ai $\approx$ as, 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?

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P3715/L20: change “melt pond coverage and open water fraction” to “melt pond and open water fractions”

P3716/L22-P3717/L14: See general comment #2 above.

P3717/L24: delete “Further”

P3717/L15-16: replace “the regional albedo estimate as it was defined in our framework” with “our regional albedo estimate”

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:

Eicken, H., H. R. Krouse, D. Kadko, and D. K. Perovich (2002), Tracer studies of pathways and rates of meltwater transport through Arctic summer sea ice, J. Geophys. Res., 107(C10), 8046, doi:10.1029/2000JC000583.

Hanesiak, J.M., J. J. Yackel, and D. G. Barber (2001a), Effect of melt ponds on first-year sea ice ablation-integration of RADARSAT-1 and thermodynamic modelling, Can. J. Remote Sens., 27(5), 433-442.

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