The Cryosphere Discuss.,
doi:10.5194/tc-2016-204-RC1, 2016

The Cryosphere Discussions
Open Access
EGU

Interactive comment on "Attribution of Greenland's ablating ice surfaces on ice sheet albedo using unmanned aerial systems" *by* Jonathan C. Ryan et al.

A. Pope (Referee)

apope00@gmail.com

Received and published: 30 September 2016

Overall Comments: In "Attribution of Greenland's ablating ice surfaces on ice sheet albedo using unmanned aerial systems," Ryan et al. describe an interesting study where digital camera data, pyranometer measurements, and MODIS data are combined to better understand the range of surfaces present in one section of the Greenland Ice Sheet. Overall, the paper provides a new method for getting quantitative data using UAS as well as a thorough description of results, which will likely be of interest to the Greenland surface mass balance community. However, at times it feels like the paper over-reaches in significance without backing it up with a strong motivation. In addition, more detailed description is needed of the methods to be fully understood by

Printer-friendly version

Discussion paper

the reader. With some revision, this will be a solid contribution to The Cryosphere.

Broad Comments: Some overstatements: At a few points, namely in the introduction and the conclusion (and slightly in the title), it feels to me like the impact and applicability of this paper is overstated. The conclusions are very much limited to one section of the ablation region of the Greenland ice sheet – not the accumulation area and certainly not all of Greenland. Indeed, repeated mention of the "dark patch" reminds the reader of this (as that is confined to a section of West Greenland), but the text often discusses or draws conclusions to all of Greenland. These should be limited so as not to overstate the paper. In addition, the paper ties the spatial results here as being important to surface mass balance modeling. However, as the authors admit, nothing is known about the temporal changes of these fractions over time. As there are only 3 days of data, we cannot draw any conclusions from this paper either. Rather than providing such modeling applications as motivation/significance for this paper, I would suggest more simply stating that this paper gives some insight into higher resolution spatial variability and now temporal variability and trends need to be studied. I think being more direct will benefit the paper overall.

Describing Methods: Simply put, the methods need to be more fully described for the reader to be able to fully understand what the authors have done, particularly in the classification process. It is not clear what order things were done in, what was done manually vs. not manually, and while success is demonstrated with a few metrics, very few examples of other ways to assess the classification are given. While I very much appreciate a succinct paper, I do not believe there is quite enough for the reader to go on here. Specific suggestions would be a clearer description of classification seeding, processing, and validation, as well as more examples (figures – like 4C', but more of them!) showing where the classifier succeeds/fails. (On a related note – it is my understanding from k-means classifying of multispectral data that the classes are not necessarily distinct. A figure showing distinct clusters would very much boost my confidence in the results.)

Data & Code Sharing: “Copernicus Publications recommends depositing data that correspond to journal articles in reliable (public) data repositories, assigning digital object identifiers, and properly citing data sets as individual contributions.” Currently this paper does not make it clear how data are shared, stored, or cited. It would benefit the paper and the community if both DATA AND CODE were shared following Copernicus recommendations and those of projects like “Geoscience Paper of the Future”: http://www.scientificpaperofthefuture.org/gpf/node/1

Specific Comments: Title: “Attribution . . . on” doesn’t quite make grammatical sense to me. Should there be an “effect” in there?

Title: I believe that it should be made clear this study is only done in one study area on the Russell Glacier / Kangerlussuaq area rather than implicating all of Greenland.

Authors: Is there really a need to call 2 more people the “Dark Snow Project Team” It seems unnecessarily complicated. Just add the two co-authors and add Dark Snow as affiliation if you like.

P1 L3: use of “10ˆ2 to 10ˆ3 m” comes up MANY times in the paper, but it seems like a confusing term to me. Do you mean to indicate a length scale or an area? If the former, then why not “100m – 1km”? If the latter, then it should be m2, and surely 10ˆ3 is not intuitive? Also, surely it is the combination of high spatial resolution of the imagery AND the scales that you are studying at that are important. Basically, use of this range has me a little lost. Please clarify / fix at many points in the paper.

Abstract: In general, sentence structure can be simplified to make this easier to read.

P1 L8: Suggest editing the sentence to “The highest correlation with mesoscale albedo was the fraction area of distributed impurities, which although not the darkest surface type, explains 65% of the albedo variability across the surface transect.”

P1 L8: you haven’t defined “distributed impurities” as a surface class yet.

P1 L14: Just my personal preference perhaps, but I do like the Oxford comma.

Printer-friendly version

Discussion paper

P1 L15: Example of what sounds like overreaching as I describe above.

P1 L20: There is high variability both before and after the snow disappears. See Ed Pope et al RSE 2016.

∼P1 L20: The ablation region in Greenland is a relatively small area. Why is it so important to study it? Motivate this for itself and not just possibly later conclusions!

P2 L2: I have never heard the term “fracture cornices” – is it what it sounds like? Snow builds up in fractures more than the surrounding area and so persists?

P2 L6: To keep this paper up to date, it is important to note WorldView 3 (and WV4 which, depending on when you read this, will be launched soon or will have recently launched), which both have even higher spatial resolutions.

P2 L12: This range is essentially the same as satellites now. Which is fine, but that can’t be the only motivation for UAS imagery.

P2 L15: No space after “lovenbreen”. Oxford comma. Okay, I’ll stop mentioning them now.

P2 L33: “approximate” rather than “consistent”?

P2 L33: I would also like to hear more here about the footprint size here, with a possible cross-reference to Section 2.3

P3 L1: What is the view angle/sensitivity of this pyranometer? How does that compare with the camera? How sure are you about this overlap?

P3 L6: Why 6 degrees?

P3 ∼L25: What errors are there from flight issues, which aren’t discussed here?

P4 L3: Change section title to “From Camera DN to Albedo” or something like that? Just trying to get away from calling the result “albedo” directly.

P4 L11: “EXIF” not defined.

P4 L14: Although you say you get another white reference panel every 10 minutes, lighting conditions can change MUCH faster than that. How is this accounted for? What errors might be expected?

P4 L16: This is the first of many examples, but where you provide a linear fit statistic, I think it is generally good practice to show us the fit. Things can have high r-squared value which not being truly linear!

P4 L12-22: This is a really important section but it is hard to read. Simplify sentence structure. Reduce the use of pronouns. Consider a flow chart.

P4 L25: And also a different bandwidth than the other sensors, too?

P4 L31&32: How was “resampling” done? Nearest neighbor?

P5 Section 2.6: I wrote this above, but I think it is crucial that you detail every step clearly and chronologically to make the classification process entirely clear. Description should include what is subjective/user-specific and what is not/repeatable. I would also like to see some figures which demonstrate the efficacy of your classification strategy.

P5 L16: For these segments, it would be helpful to see an illustration. Or are the bits in Figures 4 and 5 examples? Basically, I’m trying to figure out spatially how these are broken down. Maybe be more specific/explicit in Figures 1 and 2?

P5 Section 2.7: Rather than bilinearly interpolating MODIS (which is okay, but I’m not sure it is completely valid given that you are looking at smaller scale variability), have you tried to go the other direction? Do you have UAS imagery which completely covers the MODIS pixels?

P6 L5: Since you mention 7% uncertainty – have you thought about adding shading or something to the Figure to indicate the uncertainty?

P6 L7: Why only use August 8 data here? Could you consider “averaging” the data from the different transects to get a clearer signal? Just a thought – usually better to

Printer-friendly version

Discussion paper

use more data, right?

P6 L10: Figure 6 is out of order here.

P6 L14: I'm confused here. I though "water" was one distinct class (as listed earlier and in Fig 6), but here and in Fig 4 shallow and deep water are classified differently.

P6 L25: PCA should have a reference. Either something very basic or more applied to glacier facies (e.g. Pope & Rees 2014 RSE).

P6 L26: Why "r" here and "R2" everywhere else?

P6 L28: Why do you immediately equate PC3 and the presence of surface water? This gets re-written on P7 L28 and I'm sorry if I missed the motivation for this.

P8 L2: What about this sampling area in particular?

P8 L3: "absorption" instead of "absorbance properties"

P8 L5: Is there expected to be a daily variation in albedo? Does the daily product accurately capture this / compare to local noon best?

P8 L21: I understand that lakes are forming at higher elevations, but that isn't the subject of this paper. The ablation area is the subject here. And we don't have tons of evidence for consistent change in lakes in the ablation area (e.g. Pope 2016; Earth and Space Science), although there is certainly interannual and intraannual variability

P8 L32: Are the crevasses dry our water-filled? Does it matter?

P9 L15-20: The section seems a bit too speculative and disconnected from the ablation region. I think it needs to be a bit more tied to the subject of the paper.

P10 L1: Given the angles involved, do you think are missing small cryoconites with your imagery and also underestimating cryoconite that way? How does this matter in relation to MODIS?

P10 L4: The energy balance regime in the McMurdo Dry Valleys is completely differ-

ent than the Greenland Ice Sheet. Would this not impact number/size/distribution of cryoconite?

Section 4.5 See earlier comments above.

P11 L3: You aren't studying the whole "Greenland ablation area and its dark region" – don't overreach!

P11 L18: "where" instead of "that"?

P11 Conclusion: I would try to condense this entire section if possible to be a little more concise. As written above, section 6 in particular is as simple as: "we have characterized the spatial, we don't know about the temporal variability. That temporal variability could be important for modeling." Don't overstretch.

Table 1: Have you considered making this a figure instead of a table? A box plot, for example? Or histograms? Provide evidence that you do have normal distributions if you're using std dev. Using a box plot would also show how distinct (or not) the populations are graphically. Annotate the figure if you want to explicitly provide the numbers, too.

Table 2. For the R2 values - if you're showing linear goodness of fit values, it would be good to show the scatterplot, too.

Figure 1: The colors printed out a little dark here. Replace "on" with "from"? "Fig." to "Figs."

Figure 2: I really liked some aspects of this figure and some of it confused me. I thought it was good to combine that you compare the MODIS and UAS imagery at different resolutions. What was less clear was the width of the UAS imagery and how that actually compares to MODIS pixels – and that you bilinearly interpolate MODIS data rather than averaging UAS data, which is what the figure makes It look like. I would also point out that MODIS is 500 m2; how does this compare with the "500 m2" chunks you describe in the paper? Should these actually be .25 km2 chunks? Just

checking. Also – there are 5 surface types listed here, but 6 in the paper.

Figure 3: I would suggest adding scatterplots (on the righthand aide) for each comparison to illustrate the linear relationship? See comments for Table 2.

Figure 4: Photos print a little dark. RGB histograms should be noted in caption. Give more examples of classifications like this! Consider making a 9-panel figure so each shows albedo and classification, for example?

Figure 5: Same goes here – show me the classifications to prove the do a good job!

Figure 6: Why are the lines different colors? It doesn't add anything to the figure. I would also suggest making a division of some kind (extra white space) between the top albedo part and the fractions. Consider labeling the two parts. When I first looked at it my immediate reaction was that the fractions added up to more than one . . . before I realized they weren't all fractions!

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-204, 2016.